Pain and the Pursuit of Objectivity: Pain-Measuring Technologies in the United States, c.1890-1975

Noémi R. Tousignant

Department of History, Faculty of Arts
McGill University, Montreal
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Abstract

Since the late 19th century, scientists and clinicians have generated an astonishing array of meters, scales, experimental designs, and questionnaires to quantify pain with more precision, accuracy, and objectivity. In this thesis, I follow the development and implementation of pain-measuring technologies in the United States until the mid-1970s. Focussing on how these technologies work, I analyse the relationship between practices of objectification; the social, material and technical resources on which these practices depend; and changing conceptions of pain, subjectivity and objectivity.

Surprisingly, as efforts to objectify pain were intensified, pain was increasingly conceptualised as a subjective experience, that is, as a phenomenon inextricably tied to the unique emotional, psychological, and social condition of the experiencing self. I argue that this transformation was not solely due to the development of new theoretical models of pain, but also, importantly, enabled by the implementation of new technologies that could measure pain as an individual and psychological phenomenon. I also argue that the successful implementation of these technologies depended on the availability of specific social, material, and technical resources, and examine the social settings in which these resources were made available.

The main motivation for the direct investment of new resources towards pain-measuring technologies was a desire to make analgesic drug testing more objective. Beginning in the late 1930s, professional, industrial and public health interests in drug addiction, opiate pharmacology, new drug development and therapeutic testing converged on the goal of better pain-measurement. By the 1950s, the organisation and funding of analgesic testing made it possible to implement and validate the analgesic clinical trial, a technology that determined analgesic efficacy by measuring collective pain and its relief. The validity of the clinical was based on procedural and statistical control of data collection and analysis, rather than on the standardisation of individual experiences and evaluations of pain. It became possible to think of pain relief as an inevitably

idiosyncratic experience, open to multiple sources of psychological variation, and yet still measure it consistently and objectively on a collective level.

Keywords: pain; measurement; objectivity; subjectivity; clinical trials; analgesics; psychophysics; psychosomatics; history of medicine; history of science.

Résumé

Depuis la fin du 19^{e} siècle, une gamme surprenante d'outils et de techniques ont été créées dans le but de quantifier la douleur de façon plus précise, plus juste et plus objective. Dans cette thèse, j'examine la conception et l'application des technologies à mesurer la douleur aux Etats-Unis jusqu'à la première moitié des années 1970. En portant mon attention sur le fonctionnement de ces technologies, j'analyse la relation entre les pratiques de l'objectification de la douleur, les ressources—à la fois techniques, matérielles et sociales—qui alimentent ces pratiques, et la transformation des représentations de l'objectivité, de la subjectivité et de la douleur.

Etonnamment, j'ai constaté que plus on a multiplié les efforts pour objectiver la douleur, plus cette dernière a été conçue comme étant une expérience subjective, c'est-à-dire, en tant que phénomène indissociablement lié au soi dans sa condition affective, psychologique et sociale. Je soutiens que cette transformation n'était pas uniquement le résultat de l'application de nouveaux modèles théoriques, mais s'est aussi réalisée grâce a l'application de nouvelles technologies capables de mesurer la douleur en tant qu'expérience individuelle et psychologique. Je demontre également qu'une application valable de ces technologies repose sur des ressources materielles, sociales et techniques specifiques, et qu'il faut donc examiner les conditions sociales donnant lieu a l'investissement de ces ressources.

C'est le désir de rendre plus objective l'évaluation des thérapies analgésiques qui a été le principal moteur de la mise en oeuvre de technologies a mesurer la douleur. A partir de la fin des années 1930, des intérêts professionnels, industriels et de santé publique, s'étant orientés vers les problèmes de toxicomanie, la pharmacologie des opiacées, le développement de nouveaux médicaments et les méthodes d'évaluation thérapeutique, ont partagé le but de mieux mesurer la douleur. Dans les années 1950, l'organisation et le financement des tests analgésiques ont permi l'application et la validation de l'essai clinique analgésique, une technologie qui calculait l'efficacité analgésique a partir de la

quantification de la douleur, et de son soulagement, au niveau collectif. La validité de l'essai clinique se fondait sur le contrôle procédural et statistique de la collecte et de l'analyse des données plutôt que sur la standardisation de l'expérience et de l'évaluation individuelles de la douleur. Cette nouvelle forme d'objectivité permettait une conception de la douleur analgésiée comme étant inévitablement variable et personnelle, tout en permettant de la mesurer de façon constante et certaine à un niveau collectif.

Mots-clé : douleur; objectivité; subjectivité; quantification; essais cliniques; analgésiques; psychophysique; psychosomatique; histoire de la médecine; histoire des sciences.

Dedication

To Moussa, *sama xol*, and in memory of A. V. Simon (1913-2003)

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My thanks must first go to my family. I am fortunate that my parents were able to offer not only emotional support but also invaluable practical advice. Moussa gave me a good reason to finish; he is also my comfort and my reward. Always curious, Eleanor and Gammy challenged me to think about what I was really working on, and also helped take my mind off it. Cynthia, Josh, Allison, Richard, Lorraine, and Lianne generously offered their hospitality. Tobie, Joss, David, and Nanook provided distraction and encouragement. My grandfather, Dr. Albert V. Simon, passed away as I was beginning research for this thesis. He would have read it cover to cover.

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1. Introduction: Pain and the Pursuit of Objectivity

In February of 2006, a BBC[1] News article predicted an imminent victory in the fight against pain. Finally, a technology had been developed to measure pain objectively using functional magnetic resonance imaging (fMRI) of the brain. "Doctors hope they will soon be able to assess how much pain their patients are suffering," proclaimed the article's by-line. Following the headline "Studying the Brain to Relieve Pain," this announcement took for granted that there existed a connection between the alleviation of pain and its measurement. Also assumed in the article was the superiority of visible, objective images of pain obtained by fMRI over patients' own descriptions of their experience. These two assumptions gave sense to the otherwise non-sequitur opening sentences of the article: "As many as one in four people suffer from chronic pain—people like Malcolm Pankhurst who was plagued for years by chronic pain. Until now, doctors have had to rely on patients' descriptions of their pain." The power of this technology's ability to excavate images of pain from a subject's body was thus pitted against a high prevalence of persistent suffering and the inadequacy of verbal communication—unmediated by technology—for obtaining information about pain.[2]

A few years earlier, Americans had announced another kind of triumph in the twin battle for the better evaluation and alleviation of pain. In 2001, the Joint Commission on Accreditation of Healthcare Organizations (JCAHO), the largest healthcare accrediting body in the U.S., instituted new "pain standards." The text of these standards proclaimed that every patient had the right to have their pain "appropriately" treated *and assessed.* To remain competitive in the American healthcare market, hospitals, clinics and care institutions have, since then, had to make the evaluation of their patients' pain routine.[3]

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[1]British Broadcasting Corporation
[2] Jane Elliott, "Studying the Brain to Relieve Pain," *BBC News*, February 5, 2006 <http://news.bbc.co.uk/go/pr/fr/-/1/hi/health/4674136.stm> (Accessed February 4, 2006)
[3] June Dahl, "New JCAHO Pain Standards are Approved," *1998-1999 APS (American Pain Society) Annual Report* (American Pain Society)

Though the text of the JCAHO standards does not specify exactly how this assessment should be carried out, the requirements indicate clearly that pain should be assessed, recorded, and reassessed, thus indirectly encouraging clinicians to choose from a wide range of available scales and questionnaires. These measuring instruments can now easily be found in manuals and on websites run by numerous organisations concerned with pain research and management.[4] The great majority of these tools depend on patients' own subjective judgments of the intensity and character of the pain they have or had. They do not "objectify" in the usual way we have come to expect from the use of blood pressure gauges or the analysis of blood samples. Many of my friends who have been presented with such scales—asked to give their pain a number, a pre-selected word, or a metered length—have found the task absurd and confusing. From what I have heard, many healthcare practitioners also take this exercise with a grain of salt. Pain specialists and health workers are aware that there is no common reference point, a universal understanding of what a "5" pain is, or exactly how much worse a "6" pain would be, and that the numbers themselves are somewhat arbitrary.

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http://www.ampainsoc.org/about/annual/1999/annual16.htm (Accessed March 14, 2006); "Pain Assessment and Management Standards—Hospitals," *Joint Commission Resources* http://www.jcrinc.com/subscribers/perspectives.asp?durki=3243&site=10&return=2897 (Accessed March 14, 2006).

[4] For example, The American Academy of Pain Management sells a Pain Outcomes Profile (POP), a "23-item questionnaire that utilizes 11-point, 0 to 10, numerical rating scales (NRS) to assess a number of relevant dimensions in the pain patient's experience." It also promises to "assist you in preparing to comply with standards put in place by the Joint Commission on Accreditation of Healthcare Organizations (JCAHO) in 2001," as well as the standards of the Pain Program Accreditation and the Commission on Accreditation of Rehabilitation Facilities, see: "Pain outcomes profile," *American Academy of Pain Management* http://www.aapainmanage.org/programs/NPDBMain.php (Accessed March 14, 2006).
Various hospitals and academic medical research centres offer a variety of scales for adults and children. For example, see: "UCLA Pain Assessment Tools," *UCLA Pain Management Resource Guide* (UCLA Department of Anesthesiology) http://www.anes.ucla.edu/pain/assessment_tools.html (Accessed March 14, 2006); "III. Pain and Symptom Management: A. Pain Assessment Tools," *City of Hope Pain/Palliative Care Resource Center Website* (City of Hope Beckman Research Institute) http://cityofhope.org/prc/pain_assessment.asp (Accessed March 14, 2006), which offers a list of other institutions offering pain assessment tools.
The NIH Pain Consortium also offers a selection of scales: "Pain Intensity Scales," *NIH Pain Consortium* http://painconsortium.nih.gov/pain_scales/ (Accessed March 14, 2006).

Yet, for the pain activists who have created and publicised these tools — campaigning for the creation of the JCAHO standards[5] or for pain to be considered as a "5th Vital Sign"[6]—they are weapons in a fight against the invisibility of pain. They may depend on the patient's imperfect and idiosyncratic judgment, but they standardise the evaluation process itself, putting each voice on the same level of eloquence and insistence. Thus, these measurement instruments offer some protection against insufficient and inequitable access to relief, against the fickleness of healthcare professionals in their attentiveness to pain, and against the ignorance of patients who, not knowing they are entitled to relief, fail to complain.

To understand the significance of the JCAHO standards and the concomitant growing use of pain assessment tools, one must be aware of the emergence and convergence of multiple strands of activism for the reform of attitudes, practices, and policies in matters of pain treatment. Denunciations of variable and insufficient responses to pain as a form of inequity have been articulated since the 1970s, but have become more frequent in the past two decades. A range of actors—including pain specialists, professional pain societies, pain policy groups, journal editors, patient groups, and journalists—have moralised social and medical responsibility towards pain sufferers in new ways.[7] Articles, lectures, and editorials have pointed to the undertreatment of pain in healthcare institutions; the needless suffering of children and the elderly;

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[5] June Dahl, heading a group from the University of Wisconsin-Madison Medical School was at the forefront of this initiative. She received support from the American Pain Society and the Robert Johnson Wood Foundation, among others. See Dahl, "New JCAHO Pain Standards."

[6] James Campbell, "Pain: the Fifth Vital Sign™" *American Pain Society* http://www.ampainsoc.org/advocacy/fifth.htm (Accessed March 14, 2006): In his presidential address to the American Pain Society in 1995, James Campbell explained: "APS has created the phrase "Pain: The 5th Vital Sign" ™ to elevate awareness of pain treatment among health care professionals...Quality care means that pain is measured and treated." Among the tools of this education campaign were buttons and counter cards.

[7] There are now a large number of organisations who do education and advocacy work for better pain management in the U. S. run by various patient and professional groups. They include: the American Pain Society, the American Academy of Pain Management, the American Pain Foundation, the American Chronic Pain Association, the American Society for Pain Management Nursing, the Pain & Policy Studies Group, the American Alliance of Cancer Pain Initiatives (an umbrella organisation for various state initiatives), Partners Against Pain, as well as many groups formed around specific painful diseases, such as the American Fibromyalgia Syndrome Association and the Sickle Cell Disease Association of America.

discrimination against African Americans, minority groups, and AIDS patients in the provision of pain relief; irrational fears of addiction; and the lack of compassion and validation encountered by sufferers of mysterious ailments.[8] Pain societies, both lay and professional, have created courses and materials to educate ignorant doctors, nurses, and even patients as well as their families about pain relief.[9] These societies have also lobbied governments and organisations for funding for pain research,[10] the reform of narcotics legislation,[11] and the institution of standards of pain treatment.[12]

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[8] There is a voluminous literature on the undertreatment of pain in the professional literature, which has received fairly wide coverage in the news and popular media. Many articles identify lack of professional education on pain, especially of nurses, and fears of causing addiction, or of narcotics regulation, as the main barriers to adequate treatment. Myths and prejudices about pain in certain cultural or ethnic groups, children, AIDS patients, the elderly, etc. have also been emphasized. A cross-section of these articles includes: R. M. Marks, and E. J. Sachar, "Undertreatment of Medical Inpatients with Narcotic Analgesics," *Annals of Internal Medicine* 78, no. 2 (1973): 173-81; D. E. Joranson, "Fear of Addiction is an Impediment to Cancer Pain Relief: A Proposal to the World Health Organization Programme on Substance Abuse," *Symptom Control in Cancer Patients* 5 (1993):52-58; Ronald Melzack, "The Tragedy of Needless Pain," *Scientific American* 262 (1990): 27-33; Marcia Angell, "The Quality of Mercy," *New England Journal of Medicine* 306 (1982): 98-9; B. R. Ferell, et al., "Pain and Addiction: an Urgent Need for Change in Nursing Education," *Journal of Pain and Symptom Management* 7 (1992):117-124; N. L. Schechter, "The Undertreatment of Pain in children: an Overview," *Pediatric Clinics of North America* 36 (1989): 781-94; M. McCaffery and L. L. Hart, "Undertreatment of Acute Pain with Narcotics," *American Journal of Nursing* 76 (1976):1586-91. W. O. Evans, "The Undertreatment of Pain," *Indiana Medicine* 81 (1988):842-3; J. Streltzer and T. C. Wade, "The influence of cultural group on the undertreatment of postoperative pain," *Psychosomatic Medicine* 43 (1981):397-403; W. Breitbart, et al. "The undertreatment of pain in ambulatory AIDS patients," *Pain* 65 (1996):243-9; C. S. Cleeland, "Undertreatment of cancer pain in elderly patients," *Journal of the American Medical Association* 279 (1998):1914-5; B. J. Primm, "Managing Pain : the Challenge in Underserved Populations : Appropriate Use versus Abuse and Diversion," *Journal of the National Medical Association* 96 (2004):1152-61.

[9] The websites of the groups listed in n.6 include descriptions of their education and advocacy initiatives. Two particularly visible and well-organised initiatives have been the American Pain Society's "Pain: the Fifth Vital Sign™" campaign, see: http://www.ampainsoc.org/advocacy/fifth.htm and the Power Over Pain Campaign, a joint initiative of the American Pain Foundation and divisions of the American Alliance Cancer Pain Initiatives, with support of the American Cancer Society and the American Society of Pain Management Nursing, which is described as a "grassroots effort" to provide "tools to implement action-oriented public awareness campaigns on the state or community level." See: "Power over Pain Campaign," http://www.poweroverpaincampaign.org/ (Accessed March 14, 2006).

[10] As a result, an NIH Pain Consortium was established in 1996 to "enhance pain research" by developing a research agenda, identifying opportunities and increasing the visibility of pain research, see http://painconsortium.nih.gov/ (Accessed March 14, 2006). Nevertheless, pain researchers have continued to bemoan the lack of balance between the magnitude of the problem of pain and the amount of funding for research on it, see for example: " News Release : Study Assesses NIH Support for Pain Research: Only 1 Percent of 2003 Grants were Dedicated to Pain," *American Pain Society* May 25, 2005 http://www.ampainsoc.org/decadeofpain/news/053105.htm (Accessed March 14, 2006).

Pain-measuring instruments have taken on a particular significance within this broader political climate. Such instruments have not always been associated with the same kinds of debates about entitlement to pain relief and the imposition of common healthcare standards. In the past, different interests and resistances—both social and material—have framed the creation and use of technologies for eliciting, exchanging, and interpreting information about pain. My fascination with these technologies stems from an interest in the changing configurations of goals and values, and of authority and trust, that have motivated and guided efforts to reform the evaluation and communication of pain.[13]

Since the late 19th century, scientists and clinicians have generated an astonishing array of meters, scales, and questionnaires in their quest to better objectify pain. Using these technologies, these actors have attempted to reconfigure human (and non-human) interactions with tools and rules in order to make the evaluation of pain more precise, accurate, consistent, or unbiased. From sets of horsehairs to balloons that were inflated in the stomach, lie-detecting machines to machines measuring heat auras, some of them instrumentally sophisticated, such as fMRI images of the brain in pain, others relying on little

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[11] The Pain & Policy Studies Group, based at the University of Wisconsin Comprehensive Cancer Center as a collaborating centre of the World Health Organisation sponsors research on the regulatory barriers to the adequate relief of pain, and cancer pain in particular, with narcotic analgesics. See: *Pain & Policy Studies Group* http://www.medsch.wisc.edu/painpolicy/ (Accessed March 14, 2006).
The same centre houses the American Alliance of Cancer Pain Initiatives, an umbrella organisation for state initiatives that "work to remove the barriers that impede pain relief through education, advocacy and institutional improvement," of which regulatory barriers are an important focus. See: *American Alliance of Cancer Pain Initiatives* www.aacpi.org/about.htm (Accessed March 14, 2006); Project on Legal Constraints on Access to Effective Pain Relief, "The Pain Relief Act"
Journal of Law, Medicine & Ethics, 24, no. 4 (1996): 317-18.

[12] Dahl, "New JCAHO Pain Standards."

[13] In this respect, I build on the work of several historians who have shown that historically-specific codes of behaviour, artistic conventions and processes of adjudication have operated to make pain publicly intelligible under particular conditions: Esther Cohen, "The Animated Pain of the Body," *American Historical Review* 105 (2000): 36-68; Mitchell B. Merback, *The Thief, the Cross and the Wheel: Pain and the Spectacle of Punishment in Medieval and Renaissance Europe* (London: Reaktion, 1999); Greg Eghigian, *Making Security Social: Disability, Insurance and the Birth of the Social Entitlement State in Germany* (Ann Arbor: University of Michigan Press, 2000); Silvia De Renzi, "Witnesses of the Body: Medico-Legal Cases in Seventeenth-Century Rome," *Studies in the History and Philosophy of Science* 33 (2002): 219-42. For more comments and references, see my bibliographical essay in chapter 2.

more material than bowls of ice-cold water and a timer, these technologies constitute a long list of ever-more imaginative means for eliciting and quantifying various dimensions of the experience of pain.

By following the trajectory of these technologies—from design, through implementation, validation, and diffusion—my dissertation examines how and why specific actors have attempted to make the evaluation of pain quantitative and objective in the United States from the turn of the 20th Century to the mid-1970s. Given that pain is often defined as *inherently* subjective, or, at the very least, as particularly difficult to objectify, these technologies make me curious: In what contexts do they make sense? What, exactly, do they measure? How do they work? What kind of objectivity do they produce?

On a general level, technologies of pain-measurement are useful because they make practices of pain evaluation more explicit, open to collective scrutiny, and thus to collective standardisation and validation. Assessments of pain that are carried out with the use of measuring technologies are meant to be more precise and less vulnerable to irrelevant personal and incidental factors than unmediated verbal or visual evaluations of pain. Because they convert information about pain into numerical form, measuring technologies make pain recordable, communicable and comparable. But when, for whom, and for what reasons did these qualities—precision, consistency, communicability, and impersonality—become valuable in pain assessment?

Pain-measuring technologies were not always considered important to the same extent, or for the same reasons. Before the mid-1970s and to a large extent afterwards, the main force driving the objectification of pain was a desire to better evaluate the efficacy of pain-relieving therapies. This was not the first use for which pain-measuring technologies were developed, nor the only context in which they have been considered useful. It was the only reason, however, that motivated significant investments into the development of pain-measuring technologies and supported their diffusion on a larger scale. As we will see, successfully implementing such technologies—making them collectively valid—requires a collective effort to mobilise and coordinate the necessary labour and

resources. Making pain measurable has to be a collective goal, a goal that is important for those who have access to the right kinds of resources, for it to become possible to create effective pain-measuring technologies.

It was not until the mid-1970s that pain-measuring instruments became important in clinical contexts. Though we can assume that pain was frequently evaluated in the clinic before this, technologies were seldom used. It was only with the expansion of specialised pain clinics in the 1970s, when persistent pain was redefined as a pathology—chronic pain—requiring special treatment approaches, and subsequently with the articulation of a new kind of "right" to adequate pain management, that pain-measuring technologies entered the clinic. Surprisingly, even the area of medical examinations for disability insurance, where one might expect to find a demand for the quantification and standardisation of pain evaluation, seems to have been relatively untouched by such technologies until the 1980s and 1990s. Before this, official evaluation guidelines deemed pain inadmissible as evidence of disability and rejected as subjective. Clinical pain evaluation did not become a collective issue before the 1970s.

Much earlier, however, from the mid-1930s onwards, the desire to improve the accuracy and precision of analgesic-testing motivated an important investment of resources—time, money, work, as well as systems of communication and of coordination—towards making pain into an objectively measurable quantity. To understand the history of pain-measurement in the United States, it is crucial to know why analgesic testing became important, and to whom, from the late 1930s to the 1960s. Different actors were interested in analgesics—and analgesic testing—for different reasons. Public health researchers and regulatory authorities were concerned about the addictive properties of analgesic drugs; pharmaceutical companies were interested in potentially lucrative new analgesics; pharmacologists had ambitions for opiate research; the medical profession wanted a non-addictive analgesic while the military wanted a synthetic one; clinical researchers were interested in reforming, and taking over, therapeutic evaluation, and so on. These interests converged in

different configurations around the goal of better analgesic testing during the middle decades of the century, both generating a demand for pain-measuring technologies and creating the conditions under which they could be implemented effectively.

To understand how pain-measuring technologies work, it is necessary to examine how specific resources are translated into measurement practices. Objectivity is expensive in time, power, work and money. Different kinds of practices produce different kinds of objectivity. Like other historians of objectivity, I believe that both the forms and functions of objectivity vary in time and space, and that they are specific to particular social arrangements. However, unlike studies that trace shifts in the values ascribed to objectivity over time, my approach to the history of objectivity focuses on changing practices of objectification. In other words, my attention is on the social, material, and technical conditions that enable, or constrain, the pursuit of objectivity *in practice.*

Changing practices of pain-measurement created new ways of objectifying pain, but also opened up new possibilities for measuring pain as a subjective phenomenon. Pain-measurers have not been attempting to measure the same thing since the end of the 19th century; they have identified new dimensions of pain to render measurable, while they have also worked with changing models of pain. How they have considered what they measured as being subjective—that is, the way in which pain was seen to be tied to the personal or the self—and what that subjectivity implied for practices of objectification, has also shifted. Indeed, over the course of the 20th century, pain has *increasingly* been thought of as a subjective phenomenon, that is, as an *experience* rather than an *event*, primarily located in thinking, feeling, interacting selves rather than in external forces and objective matter. This process of subjectification has two main implications for my study.

To pain-measurers, subjectivity was not only something to *eliminate* or *control* in order to objectify pain, but also something to *measure* and *define* through these use of pain-measuring technologies. This is important, and calls for

an approach towards the history of objectification that allows a close examination of changing conceptualisations of subjectivity. Subjectivity has usually been defined by historians of objectivity as that which objectification seeks to eliminate, or at least control. But to study the objectification of pain, we need a more dynamic vision of the interaction between objectification and subjectivity that is not only oppositional but potentially collaborative. To some extent, this has been recognised by historians of sciences of the subjective that have expanded since the 19th century. Drawing on their studies, I emphasise the need to pay attention to the role given to subjects, and to their judgment and experience, in the operation of technologies of objectification of the subjective. Such an approach is particularly important for defining the nature of those forms of objectivity that have been associated with the expansion of the "psy sciences" in Western societies in the 20th century as it has been described by Nikolas Rose.[14] But even the objectification of non-psychological phenomena may have been affected by changing conceptions of the personal, the self, and of interindividual variation—and of the ways in which these factors can influence judgment—as a result of this process of "psychologisation."

This second implication of this process of "subjectification" is that the conceptualisation of pain has changed during the 20th century. Pain-measurers have not simply been attempting to measure the same thing in new ways, but have actually been measuring new kinds of events, experiences, and persons. Indeed, it is possible to use these technologies as windows into changing conceptions of pain. Throughout my thesis, I examine how designers and users of pain-measuring technologies defined what they were measuring; how they identified which dimensions should and could be measured; and how they developed appropriate strategies to do so. I conclude that pain-measurers did not simply work with definitions of pain that were transformed outside the sphere of their activities, but actively transformed pain through their measurement practices.

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[14] Nikolas Rose, *Inventing Our Selves: Psychology, Power, and Personhood.* Cambridge Studies in the History of Psychology (New York and Cambridge: Cambridge University Press, 1996).

Pain-measurers have been well-aware that they had to create a "measurable pain," which would only be one dimension or approximation of some abstract total, indivisible experience of pain. The creation and use of new techniques thus enabled new "versions" of pain—ones which increasingly involved affective, cognitive and experiential dimensions of the self—to become measurable, and to some extent, thinkable. I do not argue that new conceptions of pain were exclusively or dominantly produced by measurement practices. New definitions of pain were the product of an interaction between various theoretical and practical fields of activity, including measurement. I do insist, however, that measuring-practices played an important role in making increasingly subjective definitions of pain operational or usable in social processes—such as therapeutic evaluation—in which collective agreement about the validity of judgments about pain was considered to be necessary.

So far, I have identified three central dimensions of my topic: the history of pain-measuring technologies first as a history of practices of objectification; second as a history of the dynamic interaction between objectification and subjectivity; and finally as a history of changing notions of pain. In the following sections, I will elaborate on the first two, situating my approach within two bodies of historical literature: on objectivity and on the sciences of the subjective. I will discuss the social study and history of pain in a separate bibliographical essay in the following chapter.

1.1 The History of Objectivity as a History of Practices

Using measuring-technologies to make the evaluation of pain more objective has meant different things over time. The history of these technologies is not a progressive story of increasingly successful objectification, but rather a history of the (sometimes unsuccessful) pursuit of changing types of objectivity, for different reasons and with different means. My study therefore shares some of the methods and preoccupations of social historians of objectivity who have usefully demonstrated that objectivity, as a term and as a concept, is neither fixed nor unitary. Encouraging us to look closely at the terminology of objectivity, these

scholars have argued that apparent confusion in the use of the adjective "objective" as a qualifier for different things—knowledge, persons, or processes—and to mean different things—such as impartial, consistent, detached, or true—in fact holds clues about the historically distinct strands of a plural, layered concept.

Lorraine Daston and Peter Galison have discussed this issue in depth, and have added some historical precision to the vocabulary of objectivity. They have shown that the ontological notion of objectivity, which refers to the fit between theory and reality, was joined by two new types of objectivity in the 19th century: mechanical and aperspectival. Those who pursued mechanical objectivity sought to eliminate human interpretation or judgment from the process of representing nature by mechanising, literally or figuratively, the production of representations.[15] Aperspectival objectivity, on the other hand, was about eliminating individual viewpoints or perspectives, and privileging types and forms of information that could be easily be communicated, to make possible the production and exchange of knowledge on a larger scale.[16] Even before the emergence of these objectivities, as Peter Dear has pointed out for the 17th century, the characterisation of objectivity shifted away from references to truth and became defined negatively as being *not* subjective, or more precisely, as being characterized by disinterestedness.[17] Alberto Cambrosio and his colleagues have identified a new form of objectivity in post-World War II medicine: regulatory objectivity. Regulatory objectivity refers to the way in which biomedical judgment is informed by the collective production of evidence, which depends on various forms of social (and material) regulation, both tacit and explicit. For example, the use of entities such as cell-surface markers in the clinic is made possible by complex networks of regulation for the production of standards applying to substances, instruments, practices, and terminologies, which

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[15] Lorraine Daston and Peter Galison, "The Image of Objectivity," *Representations* 40 (1992): 81-128.
[16] Lorraine Daston, "Objectivity and the Escape from Perspective," *Social Studies of Science* 22 (1992): 597-618.
[17] Peter Dear, "From Truth to Disinterestedness in the Seventeenth Century," *Social Studies of Science* 22 (1992): 619-31.

give these markers meaning and validity as diagnostic tests. Thus, regulatory objectivity depends on the establishment of conventions for making diverse practices and entities compatible, and for articulating biology and medicine in novel ways, rather than with reference to absolute accuracy.[18]

These studies show, in various ways, that objectivity has often not been defined by its relationship to truth or nature, but instead with reference to processes and qualities that, within specific social (and material) settings, have been identified as liable to make knowledge collectively useful and usable. Being aware of the dissociability of truth and objectivity, and of the existence of different forms of objectivity, is particularly useful for understanding quantification, as well as standardisation, as technologies of objectivity that have been highly valued in the 19th and 20th centuries. Theodore Porter has argued that we have placed our trust in numbers, in science as in government and business, because they do not require intimate knowledge and personal trust.[19] Quantification, even though it may poorly translate our notions of reality, is valuable, according to Porter, because it allows knowledge to travel across social and geographical distances, and represents the kind of impersonality that is considered to be authoritative in pluralistic democratic societies. In one example, Porter presents an extreme case—that of calculating the cost human life—to demonstrate how quantification could produce objectivity through a process of standardisation and de-personalisation, despite a widely shared opinion that there was no truth, rationality, or meaning in the concept of a life measurable in dollars and cents.[20] Measuring pain can be seen as a different type of extreme case, in which neither absolute truth nor removal of the private self have generally been attained, and yet it has still been considered possible, within a certain milieu, to

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[18] Alberto Cambrosio, Peter Keating, Thomas Schlich, and George Weisz, "Regulatory Objectivity and the Generation and Management of Evidence in Medicine," *Social Science and Medicine* (Forthcoming).

[19] Theodore M. Porter, *Trust in Numbers: the Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1995).

[20] Theodore M. Porter, "Objectivity as Standardization: The Rhetoric of Impersonality in Measurement, Statistics and Cost-Benefit Analysis," *Annals of Scholarship* 9 (1992): 19-59.

produce collectively valid quantitative data about pain. My goal is to understand how this has been accepted as a form of objectivity.

It is also useful, in analysing objectivity, to be attentive to changing distributions of trustworthiness. Historians of science and objectivity have usefully focused on notions of trust, and more specifically on the way in which trust has been selectively attributed to varying actors, stances, procedures, and instruments in the production of valid or reliable knowledge.[21] The emergence of new forms of objectivity has often entailed a shift of trust from certain types of persons, judgments, or procedures to others. For example, mechanical objectivity placed new trust in the ideal of the machine as a means of producing images that were free of interpretation and aestheticism.[22] Conversely, the promotion of new processes of objectification has often been motivated by, or strategically depended on, the identification of specific targets of mistrust. Harry Marks has shown how advocates of the randomized clinical trial depicted specific figures, such as physicians, nurses, and pharmaceutical firms, as untrustworthy in order to persuade their colleagues to adopt the techniques of the randomized clinical trial in the 1950s.[23] Porter argues that a suspiciousness of personal authority and trust led to the valorisation of impersonality and numbers as trustworthy.[24] In designing and using pain-measuring technologies, it mattered whether pain-measurers were mistrustful of dishonesty, unconscious influences, personal quirks, or other forms of interested bias.

Why have new forms of objectivity been adopted? It is possible to approach this question from at least two directions within the perspective of social history. One can identify the advantages that are conferred to those who have laid claim to being objective or who have benefited from the implementation of objective methods within specific social orders. This is one way of explaining how new social arrangements promote new types of objectivity. Porter has argued

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[21] A good study of historically-specific means of attributing trustworthiness in scientific knowledge-making is Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago, 1994).
[22] Daston and Galison, "Image of Objectivity,"119-120.
[23] Harry Marks, "Trust and Mistrust in the Marketplace," *History of Science* 28 (2000):343-355.
[24] Porter, *Trust in Numbers.*

that quantification can be seen as a response to suspicion of the personal and arbitrary, and has been especially appealing to groups who have been vulnerable to outside criticism and did not have access to other forms of authority and personal trust. Thus, quantitative objectivity has been compatible with the political order of suspicious, open, pluralist democratic societies.[25] Similarly, Daston has shown that aperspectival objectivity was necessary for the circulation of knowledge along increasingly large and heterogeneous networks of scientists, whose labour was evaluated and organized in new ways.[26]

While these authors have paid attention to the types of tools and procedures underlying these forms of objectivity, their primary focus has been on the ideals of objectivity to which people have aspired rather than on the details of the actual practices by which objective methods, claims and status are pursued. However, we can also reverse this strategy by focusing first on these practices and what makes them possible. Changing social and material conditions can either enable or constrain practices of objectification, leading to the rejection of some forms of objectivity and the adoption of others. Of course, these conditions are not isolated from the actions of those who value certain ideals of objectivity, and who may be willing (though not always able) to mobilise resources in order to pursue specific ideals of objectivity. It is important to investigate not only what these actors have hoped to gain from being objective but also what kinds of resources they have contributed towards the production of objectivity.

The two different economies of values—of values necessary *for* the production of objectivity and of values generated *by* objectivity—that underlie the historical emergence of particular types of objectivity are well-described in the edited volume *The Values of Precision*. By emphasising two types of historical processes, its contributors suggest that the title has a double meaning. Precision has become valuable at specific times in history because it was useful to emerging states, industrial economies, and international trade. Producing precision,

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[25] Theodore Porter, "Quantification and the Accounting Ideal in Science," *Social Studies of Science* 22 (1992): 633-52.
[26] Daston, "Objectivity and the Escape," 608.

however, is also costly in many ways; it depends on the exercise of power, the mobilisation of labour, the investment of funding, and the formation of systems of organisation and communication. As M. Norton Wise pointed out in a commentary on the essays in this volume, precision has not only helped to achieve standardisation, but has also required the standardisation of quantities and measurement processes. It has not only facilitated profitable commerce, but has also been expensive. Precision has helped to generate agreement but also depended on agreement. Being precise gave certain actors authority, but becoming precise has also necessitated the use of authority.[27] I similarly explore the double meaning of the value of objectively measured pain; I ask why, and to whom, pain-measuring technologies have become valuable, but also examine, in as much detail as possible, what it took—in material and social terms—to make these technologies work.

When I speak of resources invested towards the objectification of pain, I do not only mean money, though money is important in many ways, including for the production of social resources such as labour, agreement and collaboration through salaries, meetings, and publications. Other social goods include authority, expertise, coordination, and communication, while technical resources include instruments but also models, rules and definitions. I find it useful not to distinguish too sharply between material, social and technical resources, but rather to see them as interacting with, and sometimes turning into, each other within a continuous system.

I particularly focus on the relationship between resources, practices and objectivity in chapters 5, 6 and 7. In Chapter 5, I compare the practices of two widely used pain-measuring/analgesic testing technologies on the basis of what kind of work and materials they required to *control* variability and subjectivity in the measurement process. Though both technologies worked, they did so under different conditions, and with different types of resources. I show that the

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[27] M. Norton Wise, "Introduction," "Precision: Agent of Unity and Product of Agreement Part I- Traveling," "Precision: Agent of Unity and Product of Agreement Part II- The Age of Steam and Telegraphy," *The Values of Precision*, ed. M. Norton Wise (Princeton, N.J: Princeton University Press, 1995), 3-13, 92-100, 222-236.

investment of more, and increasingly concentrated resources for analgesic testing in the late 1940s and early 1950s made it possible to implement a more expensive, time-consuming, and labour-intensive technology: the analgesic clinical trial. In Chapter 6, I examine in detail how resources such as money, skills, people, labour, authority were mobilised and translated into pain-measuring practices, while in Chapter 7, I show how a lack of such resources impeded the effective implementation of such practices. Throughout these chapters, I show that shifts in the type of control exercised by technologies of pain-measurement brought about a new form of objectification, and was also associated with new conceptions of pain.

Pain-measuring technologies did not only benefit from a direct investment of resources towards the objectification of pain for the purposes of analgesic testing. As I show in chapters 3 and 4, pain-measurers also drew, in designing their instruments and experimental designs, on techniques and definitions developed in other areas of research: psychophysics, psychosomatics, the physiology of the emotions, personality, anthropology, etc. Though it falls outside the scope of my study to give a full account of the origins of these techniques, I do point out some of the reasons why their development was supported socially and financially. My point, however, is that measuring pain in new ways—whether as a psycho-physiological event, as an emotional response correlated with personality characteristics and past experience, or as an indivisible, psychological experience unique to each person—depended on the possibility of implementing certain practices, and that such conditions of possibility were historically-specific.

If practicing certain forms of objectivity depends on having access to certain types of resources, it is clear that not everyone at a given time has the means to be objective in the same way. Historians of objectivity have generally been concerned with the emergence of radically new forms of objectivity (mechanical, aperspectival, regulatory) in association with broad social transformations in the *longue durée*. These authors nevertheless recognise that new types of objectivity have not swept away older ones, but that these continue to coexist. They also suggest that yet unnamed types of objectivity may have been

formed, perhaps, we can suppose, through processes of adaptation or hybridisation.[28] Specific actors, pursuing specific goals of objectification, may thus draw on multiple co-existing types of objectivity, shifting from one to another, or combining them, depending on the means available to them. Focusing on practices of objectification may thus highlight frequent variations in forms of objectivity over a relatively small time-frame.

The case of pain, which has very often been defined as resistant to objectification and measurement, may particularly highlight difficult, shifting, and failed attempts at objectification. With respect to some issues, however, I have tried *not* to make a special case out of pain. Many phenomena have resisted objectification and measurement. Take electricity, for example, for which considerable effort and investment were necessary in developing both technical and social strategies for the realisation of reliable, standard, and objective measurements.[29] The measurability of pain has likewise been sensitive to the investment of work, techniques and, money, as well as to the formation of agreements and networks.

On the other hand, it is important for a historian to take seriously how actors have defined pain as subjective, making it different, in important ways, from electricity or other material phenomena. This reveals that the conceptualisation of pain as subjective has changed over the course of the 20th century. Most notably, from the 1940s, pain-measuring instruments were designed to produce and collect information that were seen to originate in subjects' psychological selves—selves that were shaped by broader social and temporal spaces that came to include families, cultures, pasts, and futures—rather than in their bodies. Pain-measurers also conceptualised sources of variation, which they

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[28] Daston and Galison, "The Image of Objectivity," 123, "...the emergence of mechanical objectivity in the latter half of the nineteenth century by no means exhausts the history of modern objectivity as a whole." They also suggest that mechanical objectivity became "fused with other varieties of objectivity," but do not explore this aspect. Cambrosio et al., "Regulatory Objectivity," note that this new form of objectivity "now co-exists," with previous forms, and also suggest ways in which older medical technologies such as blood tests, that could be described as producing mechanical objectivity, had become embedded within networks of rules and evidence that they call regulatory objectivity.

[29] For example: Simon Schaffer, "Accurate Measurement is an English Science," in *Values of Precision*, 135-172.

tried to neutralise, but also to measure, as forces that acted on individuals' experiences or expressions of pain through psychological processes. Thus, the subjectivity of pain and its evaluation were not only seen as bothersome variables but also became the very thing that needed to be measured. Understanding how pain-measurers tried to eliminate and control some aspects of subjectivity in pain evaluation, and tried to measure and use other dimensions of that subjectivity, can also be useful for developing finer distinctions between different practices of objectification over time.

1.2 Objectification and the Place of Subjectivity

We have seen that subjectivity was not only considered to be an obstacle in the application of pain-measuring technologies, but has also been conceptualised as a positive source of information for the production of useful and meaningful numbers about pain. What did this mean for the pursuit of objectivity? Historians of objectivity have generally defined subjectivity as that which objectification has aimed to eliminate, at least in the modern period. Their work has not generally been very helpful understanding how subjectivity could participate in processes of objectification, not as a negative value or as a residue, but potentially as a positive source of information and even as a product of objectification. For Dear and Porter, modern objectivity has been defined negatively by virtue of not being subjective, while Porter has given a puzzlingly general definition of subjectivity as "the fundamentally personal."[30] Daston and Galison have been more attentive to the changing nature of subjectivity against which objectivity was defined, suggesting that "the history of the various forms of objectivity might be told as how, why and when various forms of subjectivity came to be seen as *dangerously* subjective."[31] Picking up on this suggestion, Harry Marks has shown that advocates of a particular technology of objectification, the randomized clinical trial, rhetorically constructed "figures of mistrust" whose (bad) judgment would

[30] Dear, "From Truth to Disinterestedness," 619-620; Porter, "Objectivity as Standardisation,"19.
[31] Daston and Galison, "The Image of Objectivity," 82.

be eliminated by the adoption of these new practices.[32] These are important points in tracing the history of pain-measuring technologies, and indeed some of the ways in which pain was seen as subjective were considered to pose threats to the measurement of pain. Examining only the threatening aspects of pain's subjectivity would tell only part of the story, and would neglect those forms of subjectivity that were seen as constitutive of any pain defined as worth measuring. It is necessary to situate the objectification of pain within a positive process of "subjectification" by focusing, as Nikolas Rose has put it, on those "processes and practices by which humans relate to themselves as selves."[33]

Can we simply say that psychological processes were objectified? One might argue that a definition of objectivity as a negation of the subjective might still be applicable to the ideal configuration pursued by scientists between experimenter, technology, and phenomenon, even if that phenomenon was defined as psychological. Surely these technologies were meant to eliminate the subjectivity of the scientist, while the subjectivity of the subject was irrelevant, or transformed into an object. But the subjectivity of the subject was not irrelevant or objectified because subjects were usually recognised as being active participants, even collaborators, in the operation of experimental technologies for the study of subjective phenomena. Certainly, in the case of pain-measuring technologies, subjects' judgments, experiences, and selves were very often seen as crucial sources of information that rendered their pain measurable.

Similar cases can be found in the scientific study of emotions, sensations, cognition, and other phenomena defined as subjective or psychological. The work of historians who have studied these sciences is helpful in formulating an integrative approach that pays attention to the interplay of notions of objectivity and subjectivity in shaping methods of evaluation and investigation, as well as conceptions of the self. In particular, they describe some of the ways in which investigators have dealt with the participation of observing subjects in the production of knowledge about the self. Practices of objectification have sought to

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[32] Marks, "Trust and Mistrust," 343-355.
[33] Rose, *Inventing Our Selves*, 24.

eliminate some forms of subjectivity. Other forms of subjectivity have instead been managed, controlled, purified, framed, or channelled by techniques of objectification.

In *Techniques of the Observer*, Jonathan Crary described how vision was relocated from the realm of images transmitted in reality, as modelled by the camera obscura, to that of images produced by the subjective observer in the early 19th century. This shift was accompanied by a new scientific study of the observing subject, a science of vision focusing on the physiological apparatus of perception rather than on the physics of what was perceptible. Crary sees this increasingly objectified observer as intimately bound up with the romantic notion of the observer as "active, autonomous producer of his or her own visual experience," and thus as simultaneously subjectified.[34] This twin process of objectification and subjectification of the observer was, according to Crary, part of the "strategic appropriation of subjectivity" described by Michel Foucault as the work of the "psy" sciences, which dissolved the boundaries that had kept the subject separated from the exterior, knowable world.[35] For Foucault, this process of subjectification was the product of the exercise of new forms of power predicated on obtaining individualised knowledge and control of personal abilities and proclivities.[36] This line of analysis was continued by Rose into the 20th century.[37]

Othniel Dror has also written about a process of "appropriation of the subjective" in the use of technologies of inscription to study emotions in early 20th century physiology. While these technologies represented an ideal of mechanical objectivity in that they supposedly produced representations of emotions independently of the subject's will, consciousness, and verbal expression, the operation of these technologies in fact "depended on the subject's ability to

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[34] Jonathan Crary, *Techniques of the Observer: on Vision and Modernity in the Nineteenth Century* (Cambridge, Mass. MIT Press, 1990), 69.
[35] Crary, *Techniques of the Observer*, 148.
[36] Michel Foucault, *Surveiller Et Punir: Naissance De La Prison* (Paris: Gallimard, 1975).
[37] Rose, *Inventing Our Selves.*

manipulate the self."[38] Those subjects who were not sufficiently neutral to create a sharp delineation between "backdrop" and the "emotion" under study were excluded.[39] Dror also suggested that "emotion-gauging" technologies should not be seen as replacing the interaction between experimenter and subject, but instead as mediating this interaction in a "trialogue."[40] This notion of "trialogue," or of mediated interactions in which the machine and the subject are part of the "discussion," is useful for better understanding the role of subjects in operating other technologies of objectification, especially ones in which their participation was seen as crucial.[41]

Historians of psychology such as Kurt Danziger, Deborah Coon, as well as Ruth Benschop and Douwe Draaisma have emphasized the function of subjects' training in early psychological experimentation. Training was seen as a means of making the subject capable of channelling his (he was usually male, as well as white) subjective judgment to make it more precise and consistent, thus, to some extent, calibrating, standardising, and mechanising his own mind as a tool of investigation. The subject was thought of as a scientist, and indeed was often a faculty member or graduate student in psychology who was called an "observer," and was also thought of as a kind of instrument. This enabled him to represent a "universal" or "generalized" mind, rather than his own idiosyncratic judgment.[42] Another means of making psychology more "objective" was to select mental phenomena and re-define them in quantitative terms, while rejecting others from the domain of legitimate psychological enquiry.[43]

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[38] Othniel E. Dror, "The Scientific Image of Emotion: Experience and Technologies of Inscription," *Configurations* 7 (1999), 368 and 382.
[39] Dror, "Scientific Image," 383-4.
[40] Dror, "Scientific Image," 388-391.
[41] This is not exactly the way in which Dror uses the concept, but seems to be compatible with it.
[42] Deborah J. Coon, "Standardizing the Subject: Experimental Psychologists, Introspection, and the Quest for a Technoscientific Ideal," *Technology and Culture* 34 (1993): 757-83; Kurt Danziger, *Constructing the Subject* (New York: Cambridge University Press, 1990); Ruth Benschop and Douwe Draaisma, "In Pursuit of Precision: the Calibration of Minds and Machines in Late Nineteenth Century Psychology," *Annals of Science* 57 (2000), 1-25.
[43] Gail A. Hornstein, "Quantifying Psychological Phenomena: Debates, Dilemmas, and Implications," *The Rise of Experimentation in American Psychology,* ed. J. G. Morawski (New Haven and London: Yale University Press, 1988), 1-34.

These scientists of the subjective clearly strove for forms of objectivity that were in some ways analogous to those in the physical and physiological sciences, but they also developed novel ways of managing subjectivity without eliminating it from the production of knowledge. The objectification of subjective phenomena is different from that of bodily processes, and the measurement of pain different from the measurement of matter, in that the first require the participation—and are open to the interference of— subjects' consciousness. To objectify the subjective, scientists have had to find ways to involve subjects, eliciting and isolating certain aspects of their subjectivity, without allowing them to influence the measurement or experimental process in irrelevant ways.

In the 19th and early 20th centuries, making the study of the mind scientific seemed to involve some form of "mechanisation" of the subject, both literally through the use of instruments and figuratively by transforming the subject into a machine, and to depend on the subjects' own ability to manipulate their minds in order to isolate and purify particular forms of judgment. As untrained and naïve subjects replaced expert observers in psychological experimentation and new models of the mind were developed in the later 20th century, practices of objectification increasingly depended on new techniques for illuminating and containing subjectivity.[44] Psychological instruments made out of brass and glass were replaced or joined by paper-based questionnaires and computers able to manipulate information and practices in new ways.[45] Much more work is needed on the history of 20th century practices of the "objectification of the subjective," in psychology or in other fields.

More generally, historians of objectivity have not gone far enough in distinguishing between different forms of subjectivity, and especially the dangerous from the valuable ones, and in identifying the practices through which they have been managed. Considering the development of psychology and its

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[44] Danzinger, *Constructing the Subject*; Gerd Gigerenzer, "From Tools to Theories: Discovery in Cognitive Psychology," in *Historical Dimensions of Psychological Discourse* ed. Carl F. Graumann and Kenneth J. Gergen, (New York: Cambridge University Press, 1996): 36-59

[45] Ryan D. Tweeny, "Whatever Happened to the Brass and Glass? The Rise of Statistical "Instruments" in Psychology," in *Thick Description and Fine Texture: Studies in the History of Psychology,* ed. David B. Baker (Akron, OH: University of Akron Press, 2003) 123-142.

widespread influence on conceptions of the self in the 20th century, it has become increasingly clear that subjectivity, even as it is defined within the sphere of scientific practice, does not only map onto negative values—bias, prejudice, corruption, vested interest, (poor) judgment, arbitrary opinion, fickleness, injustice, suspicion—but also positive ones—the richness of full experience, (good) judgment, identity, selfhood, personal meaning, personality.

I aim to contribute to this history of the objectification of the subjective by paying close attention to the ways in which investigators have characterised as subjective, in positive and negative ways, both pain itself and the process of its evaluation. What obstacles stood in the way of the reliable communication of information about pain? How did the use of technologies of measurement propose to make that communication more reliable (what would it eliminate, control, stabilize, isolate)? Though what channels (minds, bodies, personalities, cultures) was the experience and evaluation of pain influenced by the personal, the social and the idiosyncratic? Was the "personal" and "social" a constitutive or corrupting influence on the experience of pain? What exactly was the "personal" aspect of pain—was it a physical constitution, the delicacy of nerves, a personality type, a particular meaning, an ability to express oneself, a susceptibility to emotion, a set of memories, a cultural identity—and how was it formed by minds, bodies, souls, the "social" and the past?

This examination reveals important shifts in thinking about pain's subjectivity that occurred around the middle of the 20th century. At the turn of the century, technologies of pain-measurement had been meant to eliminate the emotional and idiosyncratic dimensions of pain responses, thereby isolating a psycho-physiological transmission of impulses that began in the external stimulus, travelled through the body and manifested itself in a mental perception. Variations in responses to pain were conceptualised as physiological variations in *sensitivity*, that is, in the delicacy or receptiveness of the nerves that transmitted pain impulses through the body. Around 1940, attention was refocused on the ways in which emotions, personal psychological characteristics and past experiences modulated responses to pain. By 1950, some pain-measurers suggested that pain

should not be measured as an *event* produced by bodily stimulation nor a *response* to stimulation, but rather as an *experience* produced by mental processes in which sensation, perception, response and interpretation were integrated, and shaped by social and psychological processes.

These were conceptual shifts but also shifts in practices, enabled by new models of pain but also by new technologies of measurement. Nikolas Rose has argued that the psychologisation of Western societies should be studied as a history of practices, that is, of the "ways in which persons are understood and acted upon in particular activities of life."[46] Thus, Rose showed that changing concepts of selfhood are not only the product of broad cultural influences, but of the development and deployment of specific techniques of measurement and differentiation within specific realms. I similarly argue that the psychologisation of pain required techniques for attaching pain to the personal and emotional in new ways. Pain was subjectified through the application of new techniques of investigation that have enabled researchers to outline, stabilise and quantify categories and variables such as personality, anxiety, stress, or upbringing. These were techniques to measure physiological emotional responses and personality characteristics, and experimental designs for modelling personality types and past experience, that were generated and circulated thanks to new philanthropic and military funding for psychosomatic research in the 1930s and 1940s. In the late 1940s and early 1950s, pain was also subjectified through the application of the analgesic clinical trial. The techniques of the clinical trial, which were adapted for the evaluation of analgesics, entailed new forms of procedural and statistical control that enabled highly individual and variable experiences of pain to manifest themselves in regular and reliable collective patterns. That is, the clinical trial measured the pain and pain relief of groups rather than individuals. It was no longer necessary to standardise individual experiences or evaluations of pain, and thus possible to measure analgesia reliably while still conceptualising every person's pain as unique.

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[46] Rose, *Inventing Our Selves*, 23.

These changing conceptions of pain's positive subjectivity—that which needed to be measured—were associated with shifts in ideas about what kinds of subjectivity needed to be eliminated from the measurement process. In the late 19th and 20th centuries, technologies of measurement were aimed towards the elimination of the will and emotionality of subjects whose pain was being measured, ideally bypassing their verbal and conscious participation but in reality attempting to isolate sensory judgment from emotionality and expressivity. Beginning in the 1940s, some researchers began turning their efforts towards the standardisation and comparison of emotionality and personal characteristics in pain responses by using autonomic measurements, personality questionnaires, or choosing certain categories of subjects in their experiments. The implementation of the analgesic clinical trial brought new ways of controlling subjectivity and variation through the standardisation of the interrogation process, the neutralisation of subjects' and investigators' expectations of future changes in pain levels, and the collection, and statistical analysis, of large quantities of data. Along with the use of these new controls, pain-measurers described new ways in which pain could be influenced by subjective processes: trust in medicines, an excessive desire to please, personality characteristics, political indoctrination, the influence of the media, cultural conditioning, etc. With the clinial trial, it was no longer necessary to directly target and eliminate the factors that influenced pain-measurement. The hypothetical sources of subjective influence on pain-measurement were multiplied.

1.3 Methodology

I define technologies of pain-measurement broadly, encompassing not only material technologies but also organisational or procedural ones. That is, I define these technologies as the combination of tools and rules that are used to formalise and mediate interactions (between both people and tools) in order to produce information about pain that is expressed in numbers. Certain technologies are often embedded within others, in which case their use is usually considered to be meaningless without the operation of a broader set of rules about how they should

be used. [47] Two examples will help to illustrate these points. Take the simple pain-scales I described earlier. In the triage procedure of a hospital emergency room, the number produced by this scale instrument may mean something immediate: give the person an analgesic or priority to consult a doctor. This meaning may be given by another technology, such as a protocol or guideline, which provide rules that guide the production and use of this number. In a clinical trial, however, both the rules for using this instrument and its meaning are quite different. The use of the scale means little if it is not used within the specific conditions prescribed by the design of the trial, which dictates how subjects should be recruited, what drugs they should receive, and what information they should be given. In addition, the number produced by the scale becomes valid only as part of the calculation of a change in pain experienced by a group of subjects. Individual measurements were not considered to be reliable. Thus, the real technology for measuring pain was not only the scale itself, but consisted of the whole clinical trial method that measured pain—and its change over time—collectively.

I have looked for these pain-measuring technologies in various places. Published journal articles make up the majority of my sources. I searched for these articles quite broadly, identifying as potentially relevant articles on topics such as the diagnosis of psychogenic pain, experiments on differences (racial, gender, age) in pain or other psychological and physiological aspects of pain using human subjects, the treatment of ill-defined painful conditions such as low back pain, pain in disability evaluation, etc. Analgesic evaluation appeared early on as one of the areas in which pain-measuring technologies were used the most extensively, while other areas, such as clinical diagnosis, seemed hardly to involve their use at all. I therefore collected articles on the topic of analgesic

[47] Cambrosio et al., "Regulatory Objectivity," explain, for example, that traditional measures such as blood pressure, which can be seen as embodying the ideal of mechanical objectivity, "now function as part of an expanding set of embedded regulations that set the parameters of use," such as clinical guidelines. Thus their actual use in the current socio-epistemic climate of medicine is characteristic of regulatory objectivity. They suggest that, in order to figure out what kind of objectivity is at work, we must pay attention to how the meaning and function of particular measurements are determined, which may be the product of multiple technologies.

evaluation more systematically and extensively, and selected my case studies within this area.

These articles served as a departure point to map out the "territory" of my topic and to identify potentially useful archival sources. They also contained useful information in themselves, such as: descriptions of specific technologies and their use, discussions of their advantages and disadvantages, information about the identity and quantity of subjects used in specific experiments, mentions of difficulties encountered in running experiments, and the identification of sources of funding.

To obtain additional information about why and how specific technologies were designed and implemented, archival sources are invaluable. I was aware from the work of Marcia Meldrum that a Committee on Drug Addiction had formed in the late 1920s, and had been active in sponsoring the development and use of pain-measuring technologies for the purpose of analgesic evaluation.[48] Documents pertaining to this committee and its successors have been conserved in the archives of the National Academies of Science. They include correspondence between members and researchers, transcripts of meetings at which pain-measuring technologies were discussed, and information about funding for researchers who developed or used these technologies to evaluate analgesics. In addition, minutes of the meetings, research proposals, reports, and budgets were published for limited circulation and are now available at the National Library of Medicine, while an institutional history written by an active member of the committee has also been published.[49] I used information obtained from these sources extensively in chapters 5 and 6.

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[48] Marcia Meldrum, "Each Patient His Own Control: James Hardy and Henry Beecher on the Problem of Pain Measurement," *American Pain Society Bulletin* 9, no. January/February, n. 1 (1999): http://www.ampainsoc.org/pub/bulletin/jan99/history.htm (Accessed September 29, 2003). On the work of this committee, see also Caroline J. Acker, "Addiction and the Laboratory: The Work of the National Research Council's Committee on Drug Addiction, 1928-1939," *Isis* 86 (1995): 167-93; Caroline J. Acker, *Creating the American Junkie: Addiction Research in the Classic Era of Narcotic Control* (Baltimore: Johns Hopkins University Press, 2002).

[49] Nathan B. Eddy, *The National Research Council Involvement in the Opiate Problem, 1928-1971.* (Washington, DC: National Academies of Science, 1973).

The National Academies of Science also formed a committee for the study of acupuncture anaesthesia in the 1970s. The evaluation of the efficacy of acupuncture on the relief of pain was a contentious issue during these years. Drawing on the records of this committee and those of other committees on acupuncture collected in the papers of John J. Bonica and conserved in the John C. Liebeskind History of Pain Collection at UCLA, I was able to examine the use of pain-measuring technologies to resolve these issues. Bonica's papers also included correspondence, newspaper clippings, and other useful information. The issues was also extensively covered in the press, and I was able to supplement archived clippings with articles obtained from the systematic search of JAMA, the use of medline, and the digitized archives of the New York Times and the Wall Street Journal through ProQuest.

Finally, I consulted the collected papers of individual researchers whom I knew to have used or designed particular pain-measuring technologies: Harold G. Wolff, co-creator of the Hardy-Wolff-Goodell dolorimeter;[50] Janet Travell[51] and William K Livingston[52], who used the dolorimeter; Henry K. Beecher, pioneer of the analgesic clinical trial;[53] John Adriani, who ran analgesic clinical trials;[54] John J. Bonica, who also ran analgesic clinical trials, as well as a pain clinic and who was active in the debate on the evaluation of acupuncture;[55] and Emmanuel Libman, inventor of the Libman pain test.[56] I was also fortunate to have access to oral histories collected by the John C. Liebeskind History of Pain Collection at UCLA. The oral histories of Louis Lasagna, Ada Rogers, and Raymond Houde

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[50] Harold G. Wolff Papers, Medical Center Archives of NewYork-Presbyterian/Weill Cornell, New York, NY.
[51] Janet Travell Papers, Special Colletions and University Archives, The George Washington University, Washington, DC.
[52] William K. Livingston Papers, Manuscript Collection 136, History & Special Collections Division, Louise M. Darling Biomedical Library, University of California, Los Angeles, CA.
[53] Henry K. Beecher Papers, Harvard Manuscript Collection 64, Harvard Medical Library in the Francis A. Countway Library of Medicine, Boston, MA.
[54] John Adriani Papers, Modern Manuscripts Collection 453, History of Medicine Division, National Library of Medicine, Washington, DC.
[55] John J. Bonica Papers, Manuscript Collection 118, Louise M. Darling Biomedical Library, History & Special Collections Division, University of California, Los Angeles.
[56] Emanuel Libman Papers, Modern Manuscripts Collection 406, History of Medicine Division, National Library of Medicine, Washington, DC.

were especially helpful for understanding the appeal and operation of analgesic clinical trials.

These sources enabled me to follow certain lines of analysis by asking particular sets of questions. I summarise them here:

1. Changing lobbies to transform pain into a measurable entity:
Who invested resources to create, promote and implement particular pain-measuring technologies? Were these investments made within the context of larger projects, and if so which ones? It is in answering these questions that my study tells an American story: it is the story of American campaigns against drug addiction; of the interest in analgesics of the American pharmaceutical industry, American military, and public health institutions; of the professional ambitions of American psychologists, physiologists, anaesthesiologists, and pharmacologists; of the goals of American therapeutic reformers... I also ask: What kinds of demands did they make for quantitative data about pain, or for certain kinds of pain-measuring technologies and how might these demands have shaped the design and use of these technologies?

2. Changing availability of resources to create and implement pain-measuring technologies:
What resources were available to make pain-measuring technologies work? In answering this question, I have included both those resources—including social, material, and technical ones—that were specifically mobilized towards the use of pain-measuring technologies, and those which were available for other reasons. I also invert the question to ask: What kinds of resources did it take to make specific pain-measuring technologies work?

3. Changing notions of objectivity and subjectivity:
How were pain-measuring technologies supposed to improve the evaluation of pain? What types of variables were they meant to eliminate, control or manage?

With what expectations about interactions and abilities were these technologies designed? On what abilities, materials, and practices did their actual use depend? How did they propose to reconfigure interactions and transform judgment? I have paid particular attention to the issue of variation by asking: What information about inter- and intra-individual variations in the experience and expression of pain entered into the design of pain-measuring technologies? How was the use of technologies of pain-measurement supposed to deal with this variation? How did they manage variation in practice? Also key in my analysis of these questions are notions of trustworthiness: What kinds of impulses, idiosyncrasies, prejudices, bias, or variables were thought likely to corrupt the validity of measurement? What kinds of people, instruments and practices were thought to be able to protect against such corruption? Who was entrusted with the operation of pain-measuring technologies: who made good subjects, good observers, good experimenters and what qualities of trustworthiness did they possess? What kinds of people were excluded from their operation?

4. Changing notions of pain, communicability and difference:

What, exactly, did pain-measuring technologies measure? For what reasons was pain seen to be difficult to communicate accurately, precisely, or reliably? How was it made more measurable or communicable through the use of technologies? What variables were thought to influence pain and through what channels or mechanisms did these variables operate? How did the use of pain-measuring technologies help to define and explain differences in the experience or expression of pain?

5. The utility of pain-measuring technologies

Were pain-measuring technologies useful for resolving particular debates? On what did the validity of the numbers produced by pain-measuring technologies depend? What did these numbers mean? How widely did their meaning and validity extend? What kinds of mechanisms existed to generate agreement about

the meaning and validity of pain-measurement? How were these instruments, and information about how to use them, diffused?

I chose analgesic testing technologies and debates about them as my case studies. This was a practical choice, since these were the technologies and debates about which the most written sources were generated, and the richest information could be drawn. But it was also justified since they were the technologies in which the most resources were invested, and which were central to investigating why, when, and by whom, the quantification of pain was defined as important and how it was achieved in practice.

As a consequence of these choices, much of my thesis deals with the measurement of pain as part of the evaluation of analgesic therapies. This is not because I wanted to write a history of analgesic evaluation, but because those who invested the most time, effort and money into making pain measurable were also ultimately trying to make the evaluation of analgesic therapies more accurate and impersonal. As a result, I have had to situate the history of pain-measuring technologies within the history of therapeutic evaluation. I have also, however, paid some attention to the function of pain-measurement in the development of psychophysics, psychology, psychosomatic research, anaesthesiology, and clinical pharmacology, as well as in the measurement of individual and collective differences.

1.4 Summary of Chapters

The first pain-measuring instruments were created in the last decade of the 19th century. In chapter 3, I examine their emergence in experimental psychophysics, and follow attempts to extend their use to applied psychophysics, clinical diagnosis, and the evaluation of analgesic drugs. Though these instruments worked well in limited experimental settings, they did not become widely used. Criminal and psychological anthropologists obtained some support in the United States and elsewhere for the psychophysical study of human differences in correlation with sociological and biological data, but failed to realise any large-

scale projects involving the measurement of pain. There was also some interest in using pain-measuring techniques for clinical diagnosis—to enhance the diagnostic judgment of the clinician rather than replace it—but there is little evidence that these techniques were widely used. Finally, some attempts were made to measure analgesic drug effects more objectively but they received little material support.

Laboratory psycho-physiologists, criminal and psychological anthropologists, clinicians, and pharmacologists used pain-measuring technologies for very different reasons. However, these groups shared a conception of sensitivity to pain as a nervous and physiological phenomenon and as the primary mechanism modulating individual experiences of pain, which could be differentiated according to gender, race, class, etc. They also shared a distrust of patients' or subjects' spontaneous verbal accounts of pain, and saw algometers as a means of digging beyond superficial differences in emotionality and expressiveness to reveal a "true" inner sensitivity. While the appeal of algometers seems to have been generally associated with a desire for mechanical objectivity (except perhaps in the clinic), algometers themselves did not fully mechanise the evaluation of pain because they still depended on the subjects' conscious judgment of painfulness. Instead, they rendered that judgment as easy, emotionally-neutral, and automatic as possible.

During the 1930s, two trends that would influence the future of pain evaluation began to take shape. One was a new investment into psychosomatic research and its institutionalisation by specific actors and American philanthropies, which would be bolstered by the interest in neurotic conditions provoked by World War II. This led to the creation and implementation of new techniques to define and measure psychological influences on physical experience and vice versa. The second was the launch of a systematic search for a non-addictive analgesic by the Committee on Drug Addiction of the National Academies of Science, which created a demand for precise methods to isolate and quantify the pain-relieving potency of new drugs. This demand was stimulated by the American discovery of fully synthetic analgesics developed by the German pharmaceutical industry just before and after the war.

The reception of a new algometer, which was introduced in 1940, can be seen to reflect the influence of both these trends. The Hardy-Wolff-Goodell dolorimeter was acclaimed as the most precise algometer yet, and its precision was welcomed for two different types of uses. In Chapter 4, I describe how the algometer was adopted to measure the psychological dimensions of individuals' *reactions* to pain. An individual's reactivity to pain was conceptualised as an emotional and cognitive phenomenon, which was different from the sensory sensitivity that had previously been measured with algometers. The measurement of pain reactions was made possible not so much by the dolorimeter itself but by the development of new techniques to measure psychological influences on pain reactions, though it was also based on a theoretical distinction between reaction and sensation made by Hardy, Wolff and Goodell. The techniques used to standardise psychosomatic variables were the product of the development of psychosomatic research in the 1930s and 40s, as well as of the development of new methods in psychiatry, anthropology, and psychology. They included: the measurement of autonomic responses (the psychogalvanic reflex, heart rate, blood pressure, finger tremor, etc.) and their correlation with emotional fluctuations; the development of personality tests and questionnaires; the creation of new categories of subjects through psychiatric diagnostic and therapeutic techniques (psychoneurotics, lobotomized patients), which were also correlated with personality measures; and a new way of conceptualising culture and personality and of investigating it through ethnographic methods. Drawing on these new techniques, researchers were able to measure new dimensions of pain in the 1940s, 50s and 60s. These measurement practices thus contributed towards defining pain as a psychological reaction that was susceptible to the influence of personality, early experience, culture, meaning, and emotion.

In chapter 5, I turn back to 1940 when the Hardy-Wolff-Goodell dolorimeter was introduced to examine how it was received as an instrument for evaluating the potency of analgesics. It turns out that the dolorimeter responded to the demands formulated by members of the Committee on Drug Addiction, who desired a pain-measuring technology that would enable them, along with

addiction-measuring techniques, to calculate precise ratios between the analgesic potency and addictive liability of opiate drugs. While World War II interrupted the activities of the Committee, it brought news of promising fully-synthetic strong analgesics from Germany. Pharmaceutical firms' interests in Demerol (early forties) and Methadone (post-war) made them eager for a method such as the Hardy-Wolff-Goodell dolorimeter to test the efficacy of these new drugs and pursue research on similar synthetics. Soon, however, sponsors of analgesic tests would begin favouring the analgesic clinical trial instead of the dolorimeter. By 1950, it seems that the dolorimeter, as an analgesic-testing method, was being abandoned. The aim of chapter 5 is to explain why this shift took place. I suggest that the involvement of new actors in analgesic testing at the end of the decade, and the investment of new resources for operating analgesic-measuring technologies, were important factors in the displacement of the dolorimeter by the analgesic clinical trial.

In Chapter 6, I examine in detail the design and operation of analgesic clinical trials under the sponsorship of the Committee on Drug Addiction and Narcotics. While the basic design of the trial was first described in 1949, researchers spent the next decade, and even longer, making it into a reliable and useful analgesic-measuring technology. I argue that producing effective practices of analgesic-measurement was largely due to the Committee's activities of sponsorship and coordination, which enabled four sets of relationships that were crucial to making analgesic clinical trials work. The first was the collaboration between the Committee and its grantees (the principal investigators), whose professional aspirations made them interested in workable analgesic clinical trials. The second was the relationship between investigators and their collaborators, including observers and consultants who were paid by Committee grants, and patients whose behaviours were investigated as part of Committee-funded research. The third was the alliance between the Committee and its sponsors. Finally, the diffusion of the analgesic clinical trial was facilitated by the communication, coordinated by the Committee, among different research teams,

as well as the development of techniques of multi-trial standardisation that were partly sponsored by the Committee.

While Committee-sponsored researchers succeeded in providing sponsors and regulators with definitive information about the efficacy of analgesic drugs, it proved to be more difficult to resolve debates about the efficacy of acupuncture analgesia in the early 1970s. Some commentators suggested that the efficacy of acupuncture was controversial because of the "inherently subjective" nature of pain. In chapter 7, I instead examine why it was so difficult to make technologies of pain-measurement work under the social and material conditions within which this particular debate took place in the early 1970s. Elite medical researchers pushed for the use of technologies of objectification, mainly clinical trials, to determine the efficacy of acupuncture analgesia. They portrayed pain as a phenomenon that was vulnerable to multiple, intangible influences that travelled through social interactions and acted upon the mind, thus invalidating—for therapeutic evaluation purposes—any experience of pain or relief that took place under uncontrolled conditions. They encountered several forms of resistance, however, in attempting to implement technologies of analgesic-measurement that would ensure the appropriate control and quantification of experiences of pain. These were a lack of agreement about methodological details, including disagreement about how to measure pain and what pain was; a lack of mechanisms to coordinate the use of technologies of measurement in multiple settings; a lack of material resources to implement the use of technologies of measurement on a scale considered to be adequate for resolving the issue; and a conflict for authority over the determination of the efficacy of acupuncture. This conflict took place within the medical community, with different groups competing to control the definition, evaluation and treatment of chronic pain, and outside the medical establishment, between elite medical researchers and lay people who argued for the validity of individual experiences in determining the pain-relieving efficacy of acupuncture.

Before beginning this history of pain measurement, I first review the historical literature on pain and situate my own approach in relation to other studies of pain and social change.

2. Bibliographical Essay: Pain in History

Following the publication of pioneering studies in the 1970s and 1980s, a growing number of historians, as well as other scholars, have been using pain as a focus of analysis for understanding social change. Perhaps stimulated by efforts within healthcare to redefine pain as an experience that affects the whole person within its social and psychological environment, scholars have begun exploring the ways in which ideas of pain, as well as practices of pain infliction, representation, and management, can help explain social relations and vice versa. Historians, as well as anthropologists, sociologists, literary, and art theorists, have begun referring to each other's work on pain, thus constituting the social and historical study of pain as an interdisciplinary and dynamic field of study. Now, in 2006, it is possible to define and review their work as a coherent literature.

I will not encapsulate the history of pain in the Western world, but rather will begin with a review of the ways in which historians have analysed significant shifts in the meaning of pain. I will then focus on a theme that has especially preoccupied historians and other social scientists of pain, and which is also at the heart of my own concerns: the communication of pain. Though my focus here is on the work of historians, I also include the work of anthropologists, sociologists, literary scholars, and art historians.

2.1 Trends: Modernising Pain

Undeniably, pain has undergone major transformations in Western societies during the 18th, 19th, and 20th centuries. The broad outlines of these transformations are well-known. A religious valorisation of pain as redemptive was eroded. Instead, a new sensibility, representing pain as useless, cruel, and unjust, entered into artistic discourses and movements of humanitarian reform. Various forms of legal and public practices of painful infliction were abolished. New therapeutic tools were discovered and as they were made widely available,

they transformed various kinds of physical and mental suffering into relievable, and therefore unnecessary, pain.

Since the 1970s, many historical examinations of these shifts have rejected narratives that explain modern societies' supposed growing intolerance of pain as the natural product of the march of technological, moral, and evolutionary progress. Instead, many authors have revisited landmarks in the history of pain—the abolition of torture, the appearance of various movements against cruelty, or the discovery of anaesthetics and analgesics—by paying attention to the shifting function of pain and its representations within particular sets of social relations. Historians of torture have explained the disappearance of painful legal practices not as evidence of the rationalisation of the law, but as the result of changing strategies of power and knowledge that rendered the public punitive infliction of pain obsolete. Historians who have analysed the discourses of humanitarian reformers have shown that the linkage between higher civilisation and sensibility to pain was a tool of reform, rather than its driving cause. Thus, this association, which ascribed to reformers and their followers the quality of being civilized because they were sensitive, was produced or at least reproduced by these discourses. Historians of anaesthesia and analgesia have examined the professional politics and concerns that motivated or restricted medical recourse to painkillers, while also pointing to commercial strategies and consumer demand, rather than a process of medicalisation led by healthcare professionals, as factors driving the expansion of the market for pain remedies.

Perhaps the best-known reinterpretation of the modern "disappearance" of pain is Michel Foucault's work on the history of criminal punishment.[57] Foucault sought to understand a double shift that occurred with the generalisation of incarceration in the early 19th century—the disappearance of the public spectacle of punishment, and the displacement of the physical body as the target of painful punitive practices—by placing these shifts within the framework of a history of the body and of power. That is, he looked at how the body was targeted by

57 Michel Foucault, *Surveiller Et Punir: Naissance De La Prison* (Paris: Gallimard, 1975). Translated and published in 1977 under the title *Discipline and Punish.*

strategies or technologies of power [*technologie politique du corps*][58] and thus saw changing ways of using the body as evidence of transformations in the exercise of power.

Foucault argued that the function of reforms for less painful punishments was not to "soften" power but to make it more fine-grained, more extensively distributed, and better adapted to a new society and its crimes. Old Regime crimes were committed against the sovereign, and the function of spectacular and painful punishment was to produce the truth of the crime publicly, thus re-establishing the right balance of power between the criminal and the sovereign. Both the nature of crimes and the society in which they were committed changed; the new crimes were committed against property, as well as against the whole society. Reformers were not so much concerned with cruelty involved in painful punishment, but with its inefficiency and poor distribution as a form of power. Their goal was not to punish less, but to punish better; by targeting the soul rather than the body, and by depriving the criminal of goods and liberty rather than life and bodily integrity. Having lost its function and collective intelligibility, painful punishment became repulsive.

While Foucault's study explained the function of bodily pain in punitive practices, Lisa Silverman studied torture as a means of extracting confessions in early modern France. She argued that torture had made both cultural and legal sense because there was a consensus—created in the religious arena—about the value of pain. In religion as in law, pain was seen as a means of crushing or bypassing the will to elicit truth from the body.[59] Indeed, Talal Asad has shown that the origins of inquisitorial torture depended on the emergence of religious rituals of sacramental penance linking the infliction of pain to the production of

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58 The concept of technology of power of the body is quite broad, and encompasses the various means by which bodies are disciplined and made productive within a political and economic order. Technologies of the body require knowledge about the body, not necessarily of its material composition and function, but of its capacities and behaviours, which it aims to reform in order to make it more productive.

59 Lisa Silverman, *Tortured Subjects: Pain, Truth and the Body in Early Modern France* (Chicago: University of Chicago Press, 2001).

truth in the middle Ages.[60] It was when these associations broke down in the 18th century, Silverman argued, that torture lost its consensual foundations and became criticised as indefensible by the *philosophes.* For them, personal agency was essential in the production of truth, and pain was to be rejected not only as useless, but as being opposed to personhood. Thus, for both Foucault and Silverman, as the meaning of bodily pain shifted, it lost its utility in legal and punitive practices. Pain was not rejected because of concerns about cruel and arbitrary power. Others have also argued against a progressive narrative of the disappearance of corporal punishment by pointing out that torture only disappeared for certain people, continuing to be frequently practised on enslaved and colonized peoples, dissidents, soldiers, and prisoners of war.[61]

The pains inflicted not only on criminals but also on various other groups—animals, slaves, the insane, schoolchildren—were redefined as cruel by 18th and 19th century reformers. Several historians have analysed the central role of representations of pain and suffering in these campaigns of reform, mainly in the Anglo-American context. Karen Halttunen and Elizabeth Clarke, for example, have argued that the strategic use of images of suffering in these campaigns made sense because it drew on and consolidated the emergence of a new sensibility. Where did this sensibility come from? While both place the intellectual and cultural roots of this sensibility in religious and literary movements of the 18th century, they also point out that some scholarship has instead emphasised social origins in industrialisation, consumerism, and a growing social distance between the increasingly comfortable middle classes and the suffering working classes.

Halttunen also points out that, while humanitarian representations of suffering claimed to demolish social distance, for example, as Clark shows, by newly incorporating slaves as sentient beings into a universalising "web of sympathy,"[62] they also rested on and created social distance. First, these images

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[60] Talal Asad, "Notes on Body Pain and Truth in Medieval Christian Ritual," *Economy and Society* 12 (1983): 287-327.
[61] Marcus Wood, *Blind Memory: Visual Representations of Slavery in England and America,* 1780-1865. (New York, NY: Routledge, 2000), 228-230.
[62] Elizabeth R. Clark, "'The Sacred Rights of the Weak': Pain, Sympathy and the Origins of

moved real pain into textual and visual representations, away from the immediate environment of white middle class people. They became a "spectatorial exercise of sympathy."[63] These representations also emphasised social difference by defining appropriate responses to suffering according to white middle-class values of sympathy, virtue, and civilisation, and in line with their desire for prosperity and social order, rejecting brutality and insensibility as savage.[64]

The dissolution of the religious definition of pain as redemptive, some historians have suggested, led to a more narrow, rational, and individualistic view of pain as something to be avoided or eliminated. Analyses of reformist discourses on pain, however, show that the religious implications, collective meanings, and valorisation of suffering did not disappear but instead shifted. The shift towards evangelical and liberal forms of Protestantism in Anglo-American societies produced new notions of pain that overtook pre-existing Catholic and Calvinist ones. The suffering self, expiating human sins in the image of the Crucifixion, was replaced with an emphasis on a Jesus-like sympathy for the pain of others. Suffering was re-valorised within the exercise of "spectatorial sympathy"—a strategy much used by reformers—for the affective response it evoked in the virtuous Christian, at the same time forming and confirming this virtue. In addition, as Karen Halttunen has argued, as pain was made obscene by reformist discourses, it also became titillating in pornographic representations.[65] Suffering took on value in other realms as well. Rebecca Herzig has shown that, towards the end of the 19th century, science became characterized as an activity both worthy of, and made worthy by, painful self-sacrifice. Painful sacrifice for

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Humanitarian Sensibility," *Journal of American History* 82, no. 2 (1995): 463-93.

[63] Karen Halttunen, "Humanitarianism and the Pornography of Pain in Anglo-American Culture," *American Historical Review* 100 (1995): 303-334. Referring to J. S. Mill's essay "On Civilisation," Halttunen argued that the effect of humanitarian campaigns was to distance the middle classes from actual scenes of suffering, which were relegated to the indirect representations of its literature. She also notes, as other historians have, that there seemed to be a great concern in humanitarian movements about the effects of observing pain rather than those of suffering it.

[64] Halttunen and Clark suggest this. See also James Turner, *Reckoning With the Beast: Animals, Pain and Humanity in the Victorian Mind* (Baltimore: Johns Hopkins University Press, 1980); Lucy Bending, *The Representation of Bodily Pain in Late Nineteenth-Century English Culture* (Oxford, New York: Clarendon Press. Oxford University Press, 2000).

[65] Halttunen, , "Humanitarianism," 303-334.

science was both literal, in the heroic exploits of arctic explorers and of researchers who self-experimented with yellow fever and x-ray, and figurative, in appeals to a general aesthetic of asceticism. While characterising science and scientists as rational, the valorisation of self-sacrifice rested on moral values.[66]

If the 19th century did not bring a radical secularisation and de-valorisation of pain, it was nevertheless a period of "great discoveries" in physiology and pharmacology, associated with a growing tendency to think of pain in terms of nervous transmission and psychological processes, and leading to the creation of increasingly effective painkillers: nitrous oxide, ether, chloroform, morphine, aspirin, and heroin.[67] Popular histories of these discoveries were published in the 1930s and 1940s under evocative and self-explanatory titles such as *The Conquest of Pain*, *Victory over Pain,* and *Triumph over Pain.*[68]

The first critiques of such triumphant narratives of "man's" increasing power to prevent and annihilate pain were formulated by critics of medicalisation. In the 1970s, Ivan Illich complained that the medicalisation of pain had not only failed to relieve human suffering, but had also stripped individuals of the spiritual and communitarian resources they might have used to deal with this suffering.[69] Using similar terms, Philippe Aries criticised the medicalisation of death.[70] Illich and Aries participated in a broader climate of discontent with medical authority, which also included various other "lay" health movements, in particular the women's health movement, which criticised both the limitations and unjustified dominance of medical expertise. These were also contemporary with nascent movements from *within* medicine to better medicalise, by humanising, broadening, and improving, the care of chronic pain sufferers and the dying. Reformers, such as the American anaesthesiologist John Bonica and the British

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[66] Rebecca M. Herzig, *Suffering for Science: Reason and Sacrifice in Modern America* (New Brunswick, NJ and London: Rutgers University Press, 2005).
[67] Roselyne Rey, *The History of Pain* (Cambrige, Mass: Harvard University Press, 1995).
[68] René Füllöp-Miller, *Triumph over Pain* (Indianapolis and New York: Bobbs-Merrill, 1938), translated by Eden and Cedar Paul; Victor Robinson, *Victory over Pain: A History of Anesthesia* (New York: Schuman, 1946); George Sava, *The Conquest of Pain: The Story of Anaesthesia* (London: Macdonald, 1946).
[69] Ivan Illich, *Medical Nemesis: The Expropriation of Health* (London: Calder & Boyars, 1975), 93-108.
[70] Philippe Ariès, *L'Homme Devant La Mort* (Paris: Éditions du Seuil, 1977).

physician Cicely Saunders, published extensively in the 1960s and 70s on the extensive amounts of pain that remained unrelieved by modern biomedicine.[71] Despite their different agendas, Illich, Aries, Bonica, and Saunders similarly questioned the ability of medicine and science—despite its "great discoveries"—to deal with the human problem of pain.

Echoing Illich, David Morris has denounced the way in which medical discourse has dominated the definition of pain, and has argued for the recovery of alternative ways of giving meaning to pain that were swept away by its medicalisation. Morris presented history as a tool capable of providing post-modern sufferers with new strategies to deal with their pain. Morris' approach, however, seems to caricature the modern medically-dominated view of pain as a powerful "myth," in which pain is defined as nothing more than "a matter of nerves," that is, of the physiological transmission of impulses. Morris also seems to exaggerate the medicalisation of pain as a powerful process that stripped pain of its broader cultural associations. [72] Recent scholarship and my own findings suggest that Morris may have overestimated the power exercised by the medical establishment over the definition and management of pain, oversimplified the medical view of pain, as well as ignored the broader social and cultural meanings given to "nerves" from the 18th to the 20th centuries.[73]

The discovery of anaesthesia in the mid-19th century has often been taken as a defining moment in the medicalisation of pain. Some histories have suggested that a shift in the characterisation of pain from inevitable to avoidable

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[71] Isabelle Baszanger, *Inventing Pain Medicine From the Laboratory to the Clinic* (New Brunswick, N.J: Rutgers University Press, 1998); Noémi Tousignant, "Exposing Relief: Place, Cancer Pain and Appropriate Care for the Dying in Britain 1950-1980," (Msc dissertation, Centre for the History of Science Technology and Medicine, University of Manchester, 2001). See also : John J. Bonica, *The Management of Pain With Special Emphasis on the Use of Analgesic Block in Diagnosis, Prognosis, and Therapy* (Philadelphia: Lea & Febiger, 1953); Cicely Saunders, *The Management of Terminal Illness* (London: Hospital Medicine Publishers, 1967).

[72] David B. Morris *The Culture of Pain* (Berkeley: University of California Press, 1991), 2-5.

[73] For example: G. J. Barker-Benfield, *The Culture of Sensibility: Sex and Society in Eighteenth-Century Britain* (Chicago: University of Chicago Press, 1992), explains how the concept of "nerves" was popularised in 18th century literature, becoming associated with the definition of gender roles and constitutions. Lucy Bending, in *The Representation of Bodily Pain,* explains how nervous sensitivity was mapped onto hierarchical models of society. I also review these notions in Chapter 3.

enabled the discovery of anaesthetics, while others claim that anaesthetics themselves brought about this transformation.[74] Others have instead emphasised, by paying close attention to professional politics, the way in which doctors and surgeons had to redefine their own relationship to pain in order to incorporate anaesthetics in their practices and values. American doctors, according to Martin Pernick, saw both benefits and drawbacks to painful and anaesthetic surgery, and, though they used anaesthetics, they did so selectively. For Pernick, the utility of both pain and anaesthesia was contested within a larger debate about the professional values of American physicians with respect to the risk and value of intervening on patients' lives and bodies. Settling this debate in favour of selective anaesthesia depended on adopting a new way of calculating medical risk.[75] In her study of mesmerism, Alison Winter explained how ether "won" over mesmeric anaesthesia within the context of movements for medical reform in 19th century Britain. Mesmerists tried to claim an orthodox status for mesmeric anaesthesia by making painless surgery, rather than speed and skill, a symbol of surgeons' mastery. While mesmeric anaesthesia obtained limited success on these grounds, it was soon replaced by ether, which, for various reasons, was seen as more easily incorporated into the "social relations that surgeons wished to establish with patients."[76] Mary Poovey placed pain and anaesthesia within the debate amongst obstetricians about how to best establish their positions as experts over the female body and as specialists within the medical profession. The adoption of obstetrical anaesthesia was dependent upon redefining pain as well as

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[74] For example : Clark, "'The Sacred Rights'," 473, writes: "In the early nineteenth century, doctors began characterizing pain as a treatable pathology rather than as divine punishment or common biological medium; this was most dramatically marked by the first use of anesthesia at Massachusetts General Hospital in 1846," and describes this as a "reassignment of physical agony as a medical or therapeutic problem"; while Morris, *The Culture of Pain*, 61, writes of the official date of the discovery of anaesthesia: "The year 1846 effectively divided human history into periods so different that we really cannot recapture what life was like before that date."

[75] M. Pernick, *A Calculus of Suffering: Pain, Professionalism, and Anesthesia in Nineteenth-Century America* (NewYork: Columbia University Press, 1985).

[76] Allison Winter, "Ethereal Epidemic: Mesmerism and the Introduction of Inhalation Anaesthesia to Early Victorian London," *Social History of Medicine* 4 (1991): 1-27, see also, Allison Winter, *Mesmerized: Powers of Mind in Victorian Britain* (Chicago &London: University of Chicago Press, 1998), 180.

painlessness in relation to the obstetricians' proper realm of intervention over women's bodies and the birthing process.[77]

Analgesics have attracted less attention than the more dramatic anaesthetics, but a few studies have questioned the "medicalisation" of pain led by doctors and have instead emphasized the role of lay people who marketed and bought analgesic remedies in a process of "commercialisation." Jan R. McTavish has presented the history headache remedies as a "kind of counterexample to the general notion that with the advent of modern medicine, American doctors became all-powerful."[78] American doctors did *not* seize on the new synthetic pain remedies such as aspirin to "enhance their image as compassionate" and raise their status despite the fact that these remedies were both effective and appealing to American sufferers.[79] McTavish explained this as a result of physicians'—both orthodox and sectarian—preference for treating the root causes of disease rather than symptoms such as headache, viewing the latter as a form of empiricism. Working together, consumer demand and industrial marketing strategies transformed the headache into "the very model of an ailment suitable for self-treatment" in the early 20th century.[80] Thus, as the headache lost its importance as a medical problem, it increasingly became a marketing issue. Patricia Stokes has similarly shown that labour pain was transformed in Germany by consumerist responses to patent medicines, as well as by women's desire for self-determination.[81]

Self-medication for pain was not new in the 20th century, of course, but followed 19th century practices of recourse to opium and patent medicines.[82]

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[77] Mary Poovey, "'Scenes of an Indelicate Character': The Medical 'Treatment' of Victorian Women," *Representations* 14 (1986): 137-168.
[78] Jan R. McTavish, *Pain and Profits: the History of the Headache and Its Remedies in America* (New Brunswick, N. J. Rutgers University Press, 2004), 7.
[79] McTavish, *Pain and Profits*, 81.
[80] Janice Rae McTavish, "The Role of the Headache and Its Treatment in American Medical Practice Prior to World War II," (PhD Thesis, York University, 1996), 15.
[81] Patricia R. Stokes "Purchasing Comfort: Patent Remedies and the Alleviation of Labor Pain in Germany between 1914 and 1933," in *Pain and Prosperity: Reconsidering 20th-Century German History* ed. Paul Betts and Greg Eghigian (Stanford: Stanford University Press, 2003), 61-87.
[82] Virginia Berridge and Griffith Edwards, *Opium and the People: Opiate Use in Nineteenth-Century England* (London and New York: A. Lane and St-Martin's Press, 1981), 21-48; James

While the medical profession fought for control over the dispensation of opium-based remedies and against the patent medicine industry in the19th century, the medical use of narcotics was also policed in the early 20th century because of concerns about iatrogenic (medically-induced) addiction. Caroline Acker shows that the American Medical Association engaged in a campaign of self-regulation to limit the use of opiates in response to accusations that medical treatment was contributing to growing rates of addiction.[83] Though Acker does not investigate the impact of this trend on medical definition of pain, it seems likely that doctors concerned about their liability to produce addiction would have become more conservative in their judgment of whose pain was deserving of relief.

This type of statement became commonplace in the last decades of the century. Studies denouncing the "undertreatment" of pain in the 1970s onwards have continued to point to fears of addiction—among both patients and healthcare professionals—as the cause of under-evaluations of sufferers' need and deservingness of relief.[84] Such studies and accusations have been generated as part of a broader critique of medicine's neglect of pain, and a renewed emphasis on society's moral and medical duty to alleviate suffering. This critique was associated with several interrelated trends. One of these was a new institutionalisation of pain in medicine. In the 1970s, a significant number of interdisciplinary pain clinics were founded in the U. S. and elsewhere to deal with the problem of chronic pain. This condition was newly defined as a disease, rather than a symptom, characterized by its duration and resistance to curative treatment.[85] Hospices and palliative care units were also established during this period, particularly in Britain but also elsewhere, and were largely presented as

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Harvey Young, *American Self-Dosage Medicines: An Historical Perspective* (Lawrence, KA: Coronado Press, 1974).

[83] Caroline Acker, "From All Purpose Anodyne to Marker of Deviance: Physicians' Attitudes Towads Opiates in the U. S. From 1890 to 1940," in *Drugs and Narcotics in History,* ed. Roy Porter and Mikulas Teich, (Cambridge: Cambridge University Press, 1995), 114-32.

[84] For example, R. M. Marks and E. J. Sachar, "Undertreatment of Medical Inpatients With Narcotic Analgesics," *Annals of Internal Medicine* 78, no. 2 (1973): 173-81: Margo McCaffery and L. L. Hart, "Undertreatment of Acute Pain With Narcotics," *American Journal of Nursing* 76 (1976): 1586-91; Ronald Melzack, "The Tragedy of Needless Pain," *Scientific American* 262, no. 27-33 (1990).

[85] Baszanger, *Inventing Pain Medicine.*

places to better relieve the pain of the dying.[86] Pain and palliative care were redefined as specialised topics of research and practice, occupying a growing number of societies and journals. Isabelle Baszanger has called this institutionalisation the "invention of pain medicine."[87]

The institutions of pain medicine also served to launch broader campaigns to reform medical practices and attitudes towards pain. Efforts to identify and remove barriers to adequate pain treatment, education campaigns to raise awareness of pain among healthcare professionals, lobbies for less restrictive laws on the prescription of opiates and for the institution of national standards for pain treatment are all part of what Baszanger has called the "culture of pain," which, as she noted, emerged in France in the 1990s.[88] My own research confirms a similar trend in the U. S.

Within these movements, pain was redefined as a multi-dimensional, individual, subjectively-defined experience with broad psycho-social (as well as ethical and cultural) implications. This more "social" definition of pain was welcomed by some social scientists, who saw it as an opening for collaboration with medical scientists in studying the causes and consequences of pain experiences.[89] The new attention given to pain by healthcare practitioners may have stimulated the development of a historical and anthropological literature on pain in the 1980s and 90s. The work of Elaine Scarry and David Morris, for example, seems to have been inspired by this 'revolution' in medical thinking about pain, while anthropologists such as Arthur Kleinman began to turn their attention towards chronic pain. Other researchers have instead begun to analyse the historical and sociological development of these movements per se. These studies have attempted to identify the social and professional goals that motivated

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[86] Noémi Tousignant, "Exposing Relief."
[87] Baszanger, *Inventing Pain Medicine.*
[88] Isabelle Baszanger, "Douleur," *Dictionnaire De La Pensée Médicale.* Dir. Dominique Lecourt (Paris: Presses Universitaires de France, 2004), 356-361.
[89] Morris, *The Culture of Pain;* Scarry, *The Body in Pain*; Mary-Jo Del Vecchio- Good, et al., Eds. *Pain As Human Experience: An Anthropological Perspective* (Berkeley: University of California Press, 1992).

them, the processes by which they were formed, and their consequences for the definition and treatment of pain.

In her history of the anaesthesia department in Oxford, Jennifer Beinart pointed out that the development of specialised clinical services for pain management created new professional opportunities for anaesthetists who were attracted by the prospect of "escap[ing] from the operating theatre and... [to] use diagnostic and therapeutic skills" and "returning to the general practitioner type role."[90]

Isabelle Baszanger has provided a useful sociological history of pain medicine, showing that the concepts both of the pain clinic and of chronic pain as a diagnostic and therapeutic entity were stabilised with the creation of a "world of pain," a group of individuals ready to adopt these concepts who became connected within institutional networks. However, the results of Baszanger's fieldwork suggest that two different types of pain clinics, and two approaches to treatment, came out of this "world of pain" even though pain physicians shared a common theoretical foundation and presented themselves as a unitary group. The first model was consonant with the traditional biomedical approach in that it located pain primarily within the body, and treated pain with therapeutic techniques aimed at "curing" it. The second model was focused on the person in pain, and sought to "manage" pain by modifying the person's behaviour.[91]

Jean Jackson also studied the operation of a pain clinic through ethnographic observation. The treatment approach of this clinic was similar to Baszanger's second model, in which chronic pain was defined as a set of behaviours needing to be modified or managed. Jackson shows that patients were often surprised and

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[90] Jennifer Beinart, "Pain Relief- A New Sub-Specialty ?" *Bulletin for the History of Medicine* 36 (1985): 13. See also J. Beinart, *A History of the Nuffield Department of Anaesthetics Oxford 1937-1987* (Oxford: Oxford Medical Publications, 1987), 111-132, in which Beinart described the establishment of a pain clinic in Oxford in which anaesthetists played a prominent role. She also remarked on the opportunity for anaesthetists to transform their professional roles through this development: "This is perhaps the field, arguably a sub-specialty, in which anaesthetists have the best chance of returning to the general practitioner type role, of direct interface with their own patients..."

[91] Isabelle Baszanger, "Pain Physicians: All Alike, All Different," in *Differences in Medicine: Unraveling Practices, Techniques, and Bodies.* eds Marc Berg, and Annemarie Mol (Durham and London: Duke University Press, 1998), 119-43. See also Baszanger, *Inventing Pain Medicine.*

confused by the "tough love" aspects of this therapy—the staff's discouragement of "pain behaviours" such as complaining, resting, or stopping an activity because of pain—and expressed resistance to the centre's ideology. Despite staff's claims that all pain was "real," patients felt that the interpretation of chronic pain as maladaptive behaviour delegitimised their own experience and interpretation of their pain. [92]

Participants in the emergence of pain medicine created a new object, chronic pain, as a target of research and therapy. Similarly, activists for the creation of hospices for the dying also manipulated the meaning of pain to construct a new field of intervention. In Britain, Cicely Saunders and her allies represented the hospice as a pain-free space, in contrast with the large amounts of unrelieved terminal pain found in general wards and other spaces of care, to legitimate both a new model of care and a new form of expertise in relieving the pain of the dying.[93] David Clark has analysed the function of the concept of "total pain" elaborated by Cicely Saunders. For Saunders, "total pain" was a nexus connecting the multiple dimensions of the pain of the dying—spiritual, psychological, physical, social and emotional—as well as the multiple types of interventions offered in hospice care. This concept, however, had a paradoxical consequence, unintended by Saunders, in that it could be seen as a strategy of power that extended the medical or caring gaze more widely and deeply into the social and psychological life of the patient.[94]

Together, these studies show that understandings of the nature and function of pain change because society changes. Reformers—humanitarian or medical—invested pain with new meanings, representing and treating it according to their vision of how pain should be dealt with within specific social arrangements. Their success in transforming pain depended on their power to rally other people to their vision. Practices of infliction and treatment of pain were also changed by the formation of new ways of exercising judiciary power, changing conditions of social

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[92] Jean E. Jackson, "'After a While No One Believes You': Real and Unreal Pain," in *Pain As Human Experience.* ed. Mary-Jo Del Vecchio Good, et al. (Berkeley, Los Angeles and Oxford: University of California Press, 1992)138-68. See also: Jean Jackson, *Camp Pain: Talking With Chronic Pain Patients* (Philadelphia, PA: University of Pennsylvania Press, 2000).
[93] Tousignant, "Exposing Relief."
[94] David Clark, "'Total Pain,' Disciplinary Power and the Body in the Work of Cicely Saunders, 1958-1967," *Social Science and Medicine* 49 (1999): 727-36.

organisation and interaction, new forms of professional authority, and processes of social differentiation.

There is more work to be done on the shifts and movements these authors have begun to explore and on the social history of pain more generally. I believe that the changing function of, and responses to, pain in the 20th century, before and beyond the late-century movements that have made pain their specific concern, remains particularly ignored. The role of the pharmaceutical industry, the media, and the evolution of systems of insurance and compensation; of employers, workers, soldiers, psychologists, physiotherapists, and patients' groups in addressing and defining pain are some of the many topics that have yet to be fully explored in different national contexts.

By studying practices of pain measurement, rather than attempts to treat, theorise, compensate, represent, or inflict pain, during this this period, I hope to offer new perspectives on the relationship between pain and social change during a period—the late 19th century to the post-war decades—that has largely been neglected by historians of pain.[95] I argue that the changing social, material, and technical conditions under which technologies of pain-measurement were created and implemented influenced how pain was defined and understood. In particular, I highlight a transformation in conceptions of pain that resulted from a convergence of professional, military, commercial, and public health interests in the measurement of pain for analgesic testing in the 1940s and 1950s. I show that the social and material reorganisation of analgesic testing enabled new pain-measuring practices that influenced the definition of a "measurable" pain.

I am also arguing here for the importance of studying the history of pain as a history of practices. That is, to look at pain not only as a concept fashioned by theoretical frameworks, religious and medical discourses, or cultural *mentalités*, but also by those processes aiming to make pain more intelligible, manageable, or usable for specific purposes, such as evaluating the pain-relieving potency of new substances, providing adequate healthcare, making populations more productive, or allocating disability benefits.

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[95] An exception is Betts and Eghigian, eds, *Pain and Prosperity*.

My initial concern in researching this dissertation, however, was not to explain a specific shift or trend, but rather to historicise the processes of communication that are used in attempts to make experiences of pain collectively intelligible. Many historians and social scientists have shared this concern, making it one of the major themes in the social study of pain. The following section of my review thus examines the way in which this theme has been raised and addressed in different studies.

2.2 Themes: Communicating Pain

How is it possible to know another's pain? Most researchers who have brought up this question since 1985 have referred to Elaine Scarry's landmark study titled *The Body in Pain: the Making and Unmaking of the World.* Scarry begins her analysis of the nature of human creation and destruction with the assertion that pain is inherently resistant to language, and thus to verbalisation and expression. One of Scarry's often quoted statements is: "To have pain is to have *certainty*; to hear about pain is to have *doubt*"[96] In Scarry's analysis, the quality of expressibility is closely connected with the capacity to create: pain's inexpressibility makes it a tool of destruction: of imagination, of the self, of social bonds, even of the "world"".[97]

Scarry does not deny the possibility of expressing pain, and applauds modern efforts to objectify pain as a means of "neutralizing" it and eliminating its destructive potential.[98] However, she uses a definition of pain as essentially inexpressible and "world-destroying" to analyse the structure of torture, war, and creation. For example, she explains that the function of pain in modern torture is to render its victims voiceless; pushing them beyond common bonds of words and expression, and is used by unstable regimes to appropriate the voice of the people in consolidating their legitimacy.[99]

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[96] Scarry, *The Body in* Pain, 13.
[97] Scarry, *The Body in Pain*, 19.
[98] Scarry, *The Body in Pain*, 5-11.
[99] Scarry, *The Body in Pain*, 27-59.

It must be emphasized that Scarry's approach to pain is literary, not historical. Indeed, her analyses of both pain and torture have been criticized for their ahistoricism. Lisa Silverman, for example, has argued that Scarry's view of modern illegal torture, which is hidden and renders its victims inarticulate, is not applicable to pre-modern legal torture that was based on a cultural consensus about the value of pain in making its victims speak the truth.[100] Mitchell Merback's study of medieval visual representations of pain is more specifically critical of Scarry's ahistorical definition of pain as private and incommunicable. Merback argues that the experience of public spectacles of punishment was part of a web of viewing experiences, in particular of depictions of the Crucifixion, which gave medieval viewers shared beliefs and feelings about pain as a means of expiating human sins. He concludes that "medieval people did not perceive the pain of the body as an alienating, isolating and stigmatising power" but rather as a "powerful emblem of intersubjective experience... Pain in the penitential spectacle is therefore not 'world-destroying' in the sense theorised in Elaine Scarry's brilliant essay on the structure of torture, but rather 'world-making.'"[101] My own analysis suggests that the late 20th century tendency to define pain as "inherently" private and subjective is tied to historically-specific views of the person, and was the product of a process of "subjectification" of pain that began around 1940.

Others, without necessarily rejecting some "inherent" quality of privacy and incommunicability in pain, have instead focused on the historically and culturally specific strategies by which individuals and collectivities have tried to make pain publicly intelligible. Historians and anthropologists have also found useful, and expanded on, Scarry's focus on pain and power. They have focused on two interrelated problems: that those with less power tend to have less access to the means of making their pain visible or recognisable, and that their pain is more likely to be misrepresented by others.[102] More generally, many have accepted the

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[100] Silverman, *Tortured Subjects,* 20-22.
[101] Merback, *The Thief, the Cross and the Wheel,* 20.
[102] For example, Keith Wailoo, *Dying in the City of the Blues : Sickle Cell Anemia and the Politics of Race and Health* (Chapel Hill: University of North Carolina Press, 2001), 204 : "The crucial

notion that making pain expressible is a means of creating social bonds, while situations and responses that silence expressions of pain dissolve these bonds.[103] The theme of pain's social transmission has been explored in a wide variety of contexts and time periods, focusing on two general issues: the operation of artistic and social conventions for expressing pain, and the standards of evidence guiding the judgment of pain within specific normative processes.

As we have already seen, Merback argued that there existed, in the medieval period, a religious visual culture of pain that gave representations of suffering collective meanings.[104] Focusing on the late medieval period, Esther Cohen has instead attempted to identify codes of behaviour governing the expression of pain. For Cohen, like Scarry, making pain intelligible is a means of "socialising" it. These codes, which varied depending on the situation and on sufferers' social position, thus facilitated the integration of expressions of pain into social relations. When pain was expressed outside these codes of behaviour, the contact between sufferer and audience was broken, making pain misunderstood (for example in labelling the sufferer insane).[105] Cohen's analysis raises the possibility that those who are either not familiar with, or refuse to follow, such codes for the expression of pain will be more liable to be excluded from the bonds that make pain intelligible.

Lucy Bending, in a wide-ranging study of representations of pain in Victorian culture, has argued against Scarry's denial that pain has shared referential qualities by pointing to the existence of literary and social conventions for expressing suffering. Bending, however, shares Scarry's concern about the openness of pain, because of its subjectivity, to misrepresentation by others,

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problem of pain, of course, was that it was invisible within the culture of clinical measurement...Thus pain continued to exist in the realm of the subjective—and because of it, medical professionals regarded self-reporting pain with suspicion... Medical pracitioners' very inability to speak 'objectively' on the topic merely accentuated the power of cultural assumptions in their clinical practices."

[103] For example: Eghigian "Pain, Entitlement and Social Citizenship in Modern Germany," 20: "Thus, the ability of pain to help forge and cement (or by the same merit, loosen and unravel) community also has a history."

[104] Merback, *The Thief, the Cross and the Wheel.*

[105] Esther Cohen, "The Animated Pain of the Body," *American Historical Review* 105 (2000): 36-68.

especially when these others have more power than sufferers. "To be without power is dangerous where pain is concerned," Bending explained, because pain is "liable to be represented or misrepresented by powerful groups aiming to manipulate those with less power."[106] Bending is particularly concerned with the labelling of some categories of people, such as the "primitive," as insensible. She thus suggests that we need to pay attention to the social distance between sufferers and those who are in a position to judge, interpret, or speak for their pain in studying processes of communication.

This problem of social distance complicated the representation of the pain of slaves. As Marcus Wood and Elizabeth Clarke have pointed out, the perception of slaves' bodies as fundamentally different—by their inability to feel pain and their inexpressiveness—from free, white bodies, made it difficult for abolitionists to use representations of slaves' suffering in their cause.[107] Yet, such representations played a central role in abolitionist campaigns. Clarke explains this by showing how abolitionists used the conventions of religious revivalism as a model for the use of narratives of suffering in the cultivation of sympathy.[108] The conventions of sentimental art also served as a model for reformist discourses, as Karen Halttunen has pointed out, while representations of suffering were meant to "instruct" readers and viewers on the appropriate response to pain.[109] Nevertheless, Wood claims that the racial "othering" of experiences of suffering continues to pose a problem for the use of visual representations of torture in contemporary commemorations of slavery.[110]

Individuals, in their roles as patients, claimants, subjects, or witnesses of various sorts, have also expressed their pain as a form of evidence to be examined, interpreted, and judged within specific legal, bureaucratic, medical, or experimental processes. While the techniques of art history and literary theory have enriched our understanding of past ways of representing pain, it seems that

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[106] Bending, *Representations of Bodily Pain*, 81.
[107] Wood, *Blind Memory*, 231-233; Clark, "The Sacred Rights," 474.
[108] Clark, "The Sacred Rights,"473-487.
[109] Halttunen, "The Pornography of Pain," 330.
[110] Wood, *Blind Memory*, 281.

the questions that preoccupy historians and sociologists of science might be particularly fruitful for examining the relationship between truth, trust, evidence, objectivity, and pain. Surprisingly, there has been relatively little effort to examine pain from this angle. It has often been taken for granted that expressions of pain are inevitably subjective and mistrusted by experts and judges, without examining the origins of this mistrust or investigating attempts to objectify pain. There are, however, a few exceptions.

Participating in a recent surge of interest in the history of witnessing and the construction of truth, Silvia De Renzi has examined the function of medico-legal testimony in the courts of counter-reformation Rome. In one of her case studies, she analysed attempts to prove the existence of an incapacitating headache. De Renzi explained that the alleged rise of faked illnesses put pressure on judges to impose higher standards of evidence for pain and to reject the testimony of "hearsay" witnesses, demanding impossible conditions of "visibility" to prove its existence. Some physicians, however, argued that they possessed special expertise that made them capable of verifying the existence of pain on the basis of patient testimonies.[111]

In more detail, Greg Eghigian has analysed the construction of evidence in the process of adjudicating disability claims in early 20th century Germany. Officially, subjective experience and moral considerations were excluded from consideration by insurers in calculating entitlement. Insurers instead adopted a "thermodynamic" model of the impact of disability on work, that is, they envisioned the body as a machine that could only be functionally incapacitated by changes in structure, that is, by "objective" signs of injury or disease. However, claimants persisted in expressing their pain and suffering when they engaged with the welfare system, and, as is shown by the high number of appeals, were ready to dispute rejections of their suffering. In resolving these appeals, the Reich Insurance Office was more willing than insurers to take pain into account, but did so by integrating pain into the "thermodynamic" calculation of entitlement—as a

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[111] Silvia De Renzi, "Witnesses of the Body: Medico-Legal Cases in Seventeenth-Century Rome," *Studies in the History and Philosophy of Science* 33 (2002): 233-237.

functional impairment—rather than in the moral and subjective terms expressed by claimants. Eghigian concluded that claimants used their experiences of pain to engage with the social security system, and to the nation, an engagement that created a sense of entitlement and of social citizenship. "In effect," he wrote, "Germany's national security system transformed somatic pain into a medium of national belonging."[112]

Deborah Stone's study of disability insurance in the U. S. suggests that similar "unofficial" negotiations of pain as legitimate grounds for disability claims took place in American courts, despite its exclusion from codified standards of evidence.[113] Thus, while historians will find little about pain in the manuals and schedules for disability rating or evaluation of the Social Security Administration, as well as those of the Veterans Administration, state industrial accident boards, or the American Medical Association, at least until the 1990s, Stone and Eghigian's studies suggest that records of claims and appeals may be much more informative about how pain was expressed and negotiated within these institutions.

De Renzi, Eghigian, and Stone each pointed out that the evaluation of pain in bureaucratic and legal settings is quite different from its interpretation within doctor-patient relationships. Despite its obvious importance in medical encounters, the communication of experiences of pain between patients and their carers has received fairly little attention. This is partly a problem of sources. Records of medical consultations are often incomplete, while recent ones are inaccessible because of laws protecting patient confidentiality. In addition, pain in these and other sources may seem to be both everywhere and nowhere. Discussions about pain may not have been noted in medical records. Until recently, it often did not receive attention as a separate category or theme in various types of medical textbooks and manuals, nor was it the object of a particular type of care, but could instead be found under many headings and in many types of institution. Despite these difficulties, a few studies have offered

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[112] Eghigian, "Pain, Entitlement, and Social Citizenship," 21.
[113] Deborah A. Stone, *The Disabled State* (Philadelphia: Temple University Press, 1984).

intriguing glimpses of how pain was communicated in medical settings, while others have outlined major shifts in the epistemological relation between doctors, patients, and disease that have had an impact on the medical judgment of pain.

One of these shifts seems to have occurred in the early 19th century, with the constitution of the "clinical gaze," as described by Michel Foucault in *The Birth of the Clinic*. The "clinical gaze" interpreted pain as a specific symptom that pointed, most importantly by its location, to a pathological lesion within the body.[114] This new interpretive style, which has been called hospital or anatamo-pathological medicine because of its associations with hospital practice and its focus on body tissues as revealed by autopsies, represented a shift away from a pre-existing style of "biographical medicine," focused instead on the patient's narrative.[115] A good description of the function of pain in "biographical" medical encounters has been given by Barbara Duden in her study of the records of Johannes Storch, an 18th century doctor practising in the Prussian town of Eisenach. Storch's patients, Duden remarked, used a rich vocabulary to express their inner experience of pain, the wealth of which is, she claimed, "now lost to us [moderns]."[116] It was on the basis of this expression of suffering that the doctor worked. Pain was not considered to be a symptom—in the sense that it was not a message either from body to consciousness or from disease to doctor—but rather to be the very means by which the body, indistinguishable from the soul, expressed itself.[117]

In contrast, anatamo-pathological medicine sought to use pain as a means to locate lesions within the body. While it placed less emphasis on patient

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[114] Michel Foucault, *Naissance De La Clinique; Une Archéologie Du Regard Médical* (Paris: Presses universitaires de France, 1963).
See also: David B. Morris, "An Invisible History of Pain: Early 19th-Century Britain and America," *The Clinical Journal of Pain* 14 (1998): 192.
[115] John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Chicago: University of Chicago Press, 2001); Erwin H. Ackerknecht, *Medicine at the Paris Hospital, 1794-1848* (Baltimore: Johns Hopkins University Press, 1967); Nicholas D. Jewson, "The Disapppearance of the Sick-Man from Medical Cosmology, 1770-1870," *Sociology* 10 (1976): 225-244.
[116] Barbara Duden, *The Woman Beneath the Skin: a Doctor's Patients in Eighteenth-Century Germany* [Geschichte under ter Haut] Trans. Thomas Dunlap (Cambridge, MA: Harvard University Press, 1991), 87.
[117] Duden, *Woman Beneath the Skin,* 88.

narrative, it still relied on patients' descriptions of pain. However, the information that was elicited was more limited and precise, and interpreted according to physicians' knowledge of the inner structure of tissues and organs. Historians of pain have pointed out that this new way of interpreting the body "demoted" pain from *being* the illness to being a symptom of disease, and constrained explanations of pain to the frame of reference of the physical lesion.[118] By the early 20th century, patients' complaints were increasingly being verified using laboratory tests and other diagnostic technologies, giving rise to "laboratory medicine" and apparently demoting the epistemological status of pain even further.[119]

Such a typology may give a mistaken impression that pain has progressively been eliminated from the doctor-patient encounter. It has been pointed out, however, that new ways of eliciting and evaluating information about illness have not replaced older styles, but that they have co-existed.[120] Thus, in some settings and situations, doctors have probably continued to listen carefully to their patients' detailed descriptions of pain. In addition, "biographical medicine" has reappeared in new forms in practices of pain evaluation. Andrew Hodgkiss has shown that, while pain was defined as a function of lesions—even if these lesions were invisible—for most of the 19th century, by the end of the century the inception of a psychoanalytic paradigm provided new perceptual tools that allowed "lesionless pain" to be interpreted in its metaphorical relations to biographical events.[121] Isabelle Baszanger similarly describes a return to a "biographical" style in deciphering chronic pain in a French pain clinic in the 1980s (which we can assume was similar in some American clinics).[122]

What Baszanger does not discuss extensively is the creation of a large number of instruments to standardise and quantify the assessment of clinical

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[118] Francis Schiller, "The History of Algology, Algotherapy and the Role of Inhibition," *History and Philosophy of the Life Sciences* 12 (1990): 28-29; Andrew Hodgkiss, *From Lesion to Metaphor: Chronic Pain in British, French and German Medical Writings, 1800-1914* (Amsterdam: Rodopi, 2000).
[119] Jewson, also called analytical medicine or science by Pickstone, *Ways of Knowing*
[120] Pickstone, *Ways of Knowing.*
[121] A. Hodgkiss, *From Lesion to Metaphor.*
[122] Baszanger, *Inventing Pain Medicine*, 186-188.

pain—particularly chronic pain—in the 1970s and 80s. Though it is difficult to tell, from published sources, how and where these instruments were actually used, they seem to be associated with new concerns about, and practices for, the clinical judgment of pain.

The chronological scope of my research on pain-measuring technologies leaves out, for the most part, the clinical evaluation of pain. Such technologies were not widely used in the clinic to evaluate individual patients' pains (outside of experimental contexts) until late in the century. To understand how pain was evaluated in clinical practice before this, it will be be necessary to adopt some other focus of analysis. Before pain became a meaningful diagnostic category in the 1970s, there were few explicit published discussions of how physicians should approach complex complaints of pain. It might make sense to follow a specific set of diagnostic categories involving pain, such as headache or low back pain, to draw together the web of techniques, criteria, and considerations that have determined how patients in pain have been sorted out. And, of course, as patient records become available they will prove extremely useful for this type of study.

There is a need to look not only for shifts in the general means of evaluating pain, but also at the ways in which specific types of patients' pains have been differentiated. The judgment of pain experiences may reflect trust or mistrust for certain types of individuals—potential malingerers and addicts, women, people qualified as sensitive, respectable or neurotic—or certain styles of expression—over-emotional, vague, reserved, articulate. For example, the patient records and correspondence of William Livingston, a medical examiner of the State of Oregon, indicate that he sometimes referred to his patients' honesty as evidence of their genuine suffering when he communicated with insurance officials.[123]

Two historians have drawn attention to the operation of a "politics of pain" in the differential evaluation of patients' pains and their need for medical

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[123] William K. Livingston Papers, Manuscript Collection 136, History & Special Collections Division, Louise M. Darling Biomedical Library, University of California, Los Angeles, CA. Adjust footnote to text.

resources. Martin Pernick has shown that 19th century American doctors drew on cultural assumptions about social groups' vulnerability to pain in calculating the value and risk of anaesthesia for particular patients.[124] For example, women were generally seen to be more sensitive, and thus to "need" anaesthesia more than men, but this judgment was not usually extended to black women. Keith Wailoo's study of sickle cell anaemia in Memphis has drawn links between changing responses to pain and the politics of race and health in the 20th century. Formerly neglected, the distinctive pain of sickle cell disease was used to mobilize compassion, resources, and recognition for the plight of African-Americans in the 1960s and 70s. However, this sympathy became suspicious as the highly visible pain of sickle cell came to represent a "special interest 'ethnic disease politics'."[125] This scepticism grew in the increasingly conservative environment of the 1980s and 90s, as fears of drug addiction put the credibility of self-reports of pain in question and warned against "too liberal" pain management. [126]

Pernick and Wailoo's studies draw our attention to a theme that has recurred in many of the studies I have reviewed here: the interaction between representations and evaluations of pain and notions of human difference. As we have seen, it has been pointed out that some people's pains have been more visible that others', while representations of the "Other's" pain can fail to draw attention to neglect and injustice unless they are made under favourable socio-political conditions.[127] There are other ways in which pains have been differentiated. Anthropologists, for example, have noted that some people—because of their powerlessness rather than their sensitivity—are more vulnerable to shouldering society's burden of suffering.[128] While useful for drawing attention to structural injustice, this statement must be seen as a specific political and historical explanation for differences in susceptibility to pain. The ways in which individual and collective responses to pain have been compared and explained can

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[124] Pernick, *A Calculus of Suffering*, 171-195.
[125] Wailoo, *Dying in the City of Blues,* 200
[126] Wailoo, *Dying in the City of Blues.*
[127] Clarke, "The Sacred Rights of the Weak,"463; Wood, *Blind Memory*, 231-233; Wailoo, *Dying in the City of Blues.*
[128] Del-Vecchio Good, et al., *Pain as Human Experience.* Kleinman, et al., *Social Suffering.*

also be related to historically specific notions of how bodies, psyches, practices, and environments interact to fashion subjective experiences that are differentiated along lines of race, gender, class, personality, culture, power, etc.

Pain-measuring technologies were designed both to control for and to measure interpersonal variations in experiences of, responses to, and expressions of pain. In other words, they were meant to eliminate irrelevant sources of variation that could confuse the measurement of a particular aspect of pain, for example, the influence of personal meaning on the pain relief provided by an analgesic drug. However, they were also used to compare pain responses, for example, between groups of neurotic and normal individuals. These actions both presupposed and produced ideas about why and how people differ in pain.

As I will show in Chapter 3, when the first pain-measuring instruments were invented in the late 19th century, differences in pain were thought of as physiological differences in nervous sensitivity that were projected—through the lens of social Darwinism—onto social and evolutionary divisions of gender, race, deviance, and age. My findings confirm observations by Mary Gibson, Lucy Bending, and Martin Pernick.[129]

This understanding of differences in pain was different from previous ones, as is shown by Joyce Chaplin's study of English views of American native bodies and practices in the context of colonization in the 17th century. The English saw "Indian" resistance to pain as the artificial product of bodily techniques, transmitted through upbringing and therapeutic means, rather than as a natural state proving their inherent strength. While this attitude might be seen to praise native skill in hardening their bodies, the English saw a preoccupation with these bodily techniques as hampering the progress of other kinds of technical tools and skills, in which they themselves surpassed the natives, and as a confirmation of

[129] Mary Gibson, "On the Insensitivity of Women: Science and the Woman Question in Liberal Italy, 1890-1910," *Journal of Women's History* 2 (1990): 11-41; Pernick, *A Calculus of Suffering*; Bending, *Representations of Bodily Pain.*

their own conviction that they possessed the strength to populate America.[130] More studies are needed to bridge the gap between the 17th and 19th centuries.

As anthropologists and psychologists broke away from Spencerian Social Darwinism in the first decades of the 20th century, they began to draw on psychoanalytical notions to explain human differences.[131] As I show in chapter 4, the development of new techniques for measuring psychosomatic correlations between physical stimulation and emotional experience, and between emotional events and bodily experience, allowed researchers to measure pain as a psychological event. The cause of differences in individuals' responses to pain was no longer seen to be linked tovariations in physiological sensitivity but instead to variations in psychological responsitivity. The former was associated with biological categories of difference—race, age, sex—and the latter with psychological ones—personality, culture, and past experience.

In chapters 5,6, and 7, we will also see that a new method for measuring pain-relieving efficacy supported a model of pain that was open to multiple psychological influences from within and without. The analgesic clinical trial measured collective rather than individual pain relief, and controlled the phenomenon it measured from irrelevant influences though social and statistical means, rather than individual psychological and physiological means. This new mode of experimental control made it possible to conceptualise pain as a "disembodied" experience, modulated by personal and social factors more than by physical stimulation or individual bodies.

In the 1980s and 90s, some anthropologists made efforts to move away from an individualized and psychological framework towards a more collective, inter-subjective, and political perspective on the roots of differences in susceptibility to pain.[132] From the 1990s, however, as more funds have been directed towards the question of differences in pain in the U. S.—partly as a result

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[130] Joyce E. Chaplin, *Subject Matter: Technology, the Body, and Science on the Anglo-American Frontier, 1500-1676* (Cambride, Mass. and London: Harvard University Press, 2001), 245-279.
[131] George W. Stocking, *Race, Culture and Evolution; Essays in the History of Anthropology* (New York: Free Press, 1968).
[132] Arthur Kleinman, Veena Das and Margaret Lock, *Social Suffering* (Berkeley, CA and London: University of California Press, 1997).

of the integration of lobbies for pain and women's health research within the NIH, along with new concerns about the introduction of "diversity" regulations in clinical testing, as well as interests in a potentially lucrative market for individually tailored analgesic treatment—attention has been focused on the genetic, hormonal and neuronal determinants of such variation.[133] Thus, by 2003, the NIH had firmly put the "bio" back into research on differences in pain, calling for proposals on its "bio-behavioural" determinants.[134]

My own work seeks to explain how changing conceptions of inter-individual differences in responses to pain were linked with new definitions of pain as "subjective," and the development of measurement techniques.

Conclusion

The concerns articulated by historians and social scientists working on pain in a range of periods and social contexts have encouraged me to look at pain-measuring instruments as one set, among many different types, of strategies for transforming the experience of pain into collectively readable and meaningful

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[133] For example, a conference on "Pain and Gender" was organised in 1998 by various institutes of the NIH, mainly by the NIH Pain Consortium, created in 1996, and the Office for Research on Women's Health (ORWH), also affiliated with the NIH and created in 1990. The majority of the papers presented at this conference suggested that neurological, hormonal and genetic mechanisms accounted for findings of differential responses to pain in men and women (or female and male animals). Only a few papers emphasized the psychological and psychosocial mechanisms of gender differences in pain thresholds, pain coping and pain behaviour. Though the research presented at this conference mainly addressed women's greater vulnerability to pain, it also expressed a new interest in gender differences in responses to analgesic therapy. Since the late 1980s, the NIH had established a policy mandating the inclusion of women and minorities as subjects in clinical research, which was made into public law with the NIH Revitalization Act of 1993. This new policy focused attention on the ways in which gender/sex and ethnicity/race might affect responses to analgesic therapy. This information was considered to be of importance not only for designing experiments, but also for designing drugs. A growing body of research on the genetic modulation of pain in men and women was considered to be promising in guiding the pharmacogenetic tayloring of gender-specific analgesic therapy. For a summary of papers presented at the conference, see: http://painconsortium.nih.gov/genderandpain/Default.htm. For information about the Pain Consortium and the ORWH, see: http://painconsortium.nih.gov/index.html; http://orwh.od.nih.gov/.

[134] In 2003, the NIH advertised a "biohavioral pain research program," inviting applications " to study individual differences in pain responses that may be due to factors such as genetic differences, endocrine activity, neural activity, immune function, psychological state, developmental stage, cognitive capacity, disability state, age, gender, social context and cultural background. See: http://grants.nih.gov/grants/guide/pa-files/PA-03-152.html

information. Pain-measuring instruments might, in this sense, be seen as technologies of communication, technologies to facilitate the exchange and interpretation of information about experiences of pain.

This literature also suggests a number of questions to ask of these technologies or of other types of strategies for communicating information about pain. Have these strategies, by making pain more intelligible, "socialized" it and created new social bonds? Or have disagreements about the use and interpretation of pain-measuring instruments instead aggravated a lack of common understanding? Have these strategies, in narrowing the range of possible expressions of pain, silenced the voice of the sufferer and misrepresented or misappropriated subjects' pain? Or has this narrowing or framing of expression given sufferers more equitable access to an intelligible language of pain, and thus a fairer distribution of resources—including acknowledgment—to deal with their pain? Have these instruments been used to differentiate sufferers, excluding some from participation in the production of knowledge about pain or the use of available resources to fight it? Who has decided what kind of pain, and whose pain, can be communicated, and in what form?

None of these questions can be answered if strategies of pain communication, including pain-measuring technologies, are considered in abstraction from the contexts in which they were used and the concerns of those who designed and adopted them.

3. The Emergence of Algometry, 1890s-1930s

The first pain-measuring instruments were invented in the last decades of the 19th century. These algometers, despite their name, were not used to measure how much pain a person was feeling. What they quantified, as well as standardised, was the intensity of a pain-producing stimulus. To translate stimulus-intensity into a measurable dimension of the experience of pain, researchers determined the point at which a person just began to feel pain: their threshold of pain sensation, or sensitivity. A quantified sensitivity became a comparable sensitivity. People adopted algometers to compare the sensitivity of different body surfaces, the sensitivity of different persons, or the sensitivity of a person in different states, for example, under the influence of a potentially sensitivity-altering drug.

This chapter situates the origins and early uses of algometers from the 1890s through the first decades of the 20th century in different epistemological and professional contexts. The central questions that orient this chapter are the following: What drove the first quests for precision, consistency and quantification in the study of pain? For whom, and in what contexts, did it make sense to number, classify and compare sensitivities to pain? Algometry can also be explained as part of a broader phenomenon, and explored by asking the more general question: for whom did it make sense to measure sensation?

When they were invented, algometers did not stand alone in their class of instruments, but joined a growing number of meters for the senses, including acoumeters, sonometers, photometers, ophtalmometers, olfactometers, and aesthesiometers. Algometers can first be situated as part of the creation of a larger psychophysical toolbox and thus, as a product of a turn to sensation in experimental physiology in the early 19th century, and of the later articulation of psychophysics as a branch of experimental psychology.

Like other psychophysical instruments, algometers were considered to be useful in elucidating basic, universal mechanisms of sensation. Along with other forms of measurement, algometry helped to define psychology as a science

grounded in experimental and quantitative practices.[135] More specifically, psychologists used evidence produced with algometric methods to contribute to a heated debate between the advocates of two theories of pain mechanisms that took place during the early decades of the 20th century.

As with other psychophysical instruments, the use of algometers was extended from the study of universal laws of sensation to the study of variations in sensitivity in general populations. While the first could be generalised from data obtained in a handful of good "universal" subjects, the second relied on large numbers of categorized subjects, which had to be organized into comparison groups. In the late 19th century, psychophysical instruments were used, along with anthropometric tools, by psychological, physical and criminal anthropologists in their exploration of human differences.[136] With algometry, sensitivity to pain joined brain size and facial angle as a measurable quantity used to classify humans on scales of evolution and deviance. Attempts to align differential sensitivity to pain with categories such as sex, criminality, intelligence, primitivism, and age made social and scientific sense within a conceptual framework that rooted mental and moral abilities in measurable biological characteristics. Pain sensibility, in particular, had, since at least the 18th century, been associated in Anglo-American society with other kinds of attributes that were used to distinguish classes of humans from each other, as well as from

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[135] For an argument about the value of quantitative techniques in the foundation of experimental psychology, see Gail A. Hornstein, "Quantifying Psychological Phenomena: Debates, Dilemmas, and Implications," in *The Rise of Experimentation in American Psychology,* ed. J. G. Morawski (New Haven and London: Yale University Press, 1988), 1-34.

[136] The use of anthropometrical tools in racial and sex differentiation has received quite a bit of attention from historians. However, the use of psychophysical measurement as a means to root perceptual abilities in measurable biological differences has received some attention in recent histories of concepts of race in psychology: see Graham Richards, *'Race', Racism and Psychology: Towards a Reflexive History* (London and New York: Routledge, 1997). Andrew. S. Winston, ed., *Defining Difference: Race and Racism in the History of Psychology* (Washington, DC: American Psychological Association, 2004). For a useful textbook overview of the treatment of race, culture and ethnicity in psychology, see Paul Voestermans and Jeroen Jansz, "Culture and Ethnicity," in *A Social History of Psychology,* ed. J. Jansz and P. van Drunen, 165-194 (Oxford: Blackwell Publishing, 2004). One detailed study of psychophysical differentiation deals with specifically with pain sensitivity measurements: Mary Gibson, "On the Insensitivity of Women: Science and the Woman Question in Liberal Italy, 1890-1910," *Journal of Women's History* 2, no.2 (1990): 11-41.

animals; these included civilisation, refinement, courage, intelligence, sympathy and moral sensibility.

Two novel uses for algometry were proposed in the early decades of the 20th century. An American clinician, Emmanuel Libman, promoted the use of a test of pain sensitivity as a means of improving diagnostic judgment. This project can be related to Libman's larger preoccupation—shared by many other contemporary American physicians—with the status of clinical observation in American medicine. Secondly, the use of algometric instruments was extended to the quantification of drug effects on the pain threshold. Only two experiments, by two different groups of researchers, were reported in the mid-1910s. No further attempts were made to compare pain-relieving drugs by algometric methods in the U. S. until twenty years later. By the mid-1930s, a growing interest in analgesic drug testing stimulated an unprecedented quest for quantitative tools for the evaluation of pain relief, and gave a new importance to algometry. In the mid-1910s, however, bringing quantitative precision to analgesic testing was not a sustainable project.

These different uses of algometry are part of distinct narratives: the history of sensory physiology and psychology, the history of the differentiation of individual and group psychological characteristics, the history of the interpretation of symptoms in clinical diagnosis, and the history of drug evaluation. Algometers, and future pain-measuring technologies, became important to different people, for different reasons, and at different times, within each of these histories.

Yet, despite differences in the evolution of the function, importance and status of pain-measuring technologies in each of these narratives, they share a roughly parallel chronology in the conceptualisation of pain and of its measurability. This is illustrated in the way pain-measuring instruments were often created in one context, and then transferred into another. Though psycho-physicists, criminal anthropologists, clinicians and pharmacologists used algometers for different reasons in the late 19th and early 20th centuries, they adopted a similar psychophysical model of pain sensitivity as rooted in a

physiological sensory apparatus, measurable by means of algometry, and as varying from one individual to another. Each of these notions would be challenged in the 1940s and 50s.

3. 1 Algometry in the Laboratory

The question that prompted a search for more precise methods of pain sensitivity in physiology was the following: was pain a specific sensory modality, served by its own neural apparatus, or was it an attribute of other sensations, for example, the result of intense stimulation applied to pressure receptors? The first position has been called the intensive, summation or pattern theory of pain. It was opposed to the specificity theory of pain, according to which sensations were differentiated because nerves were specialised to respond to a precise type of stimulation. This latter theory has been traced back to Johannes Müller's doctrine of specific nerve energies, according to which the differentiation of the senses originated in a physiological differentiation of the nerves, and thus explained that a single type of stimulation, such as electricity, could generate different sensations (light, pressure, warmth, pain, sound) when applied to different organs.[137] To solve this question empirically, late-19th century physiologists in Sweden, Germany and the U. S. took on the ambitious project of mapping out the sensory qualities elicited by stimulation on the surface of the body. This project required the elaboration of techniques to apply discrete stimuli to minuscule areas of skin. In the 1880s, Blix and Goldscheider began mapping out the distribution of sensory spots using needles, faradic current from a single electrode, narrow jets of hot and cold water, cork points, and small drops of ether.[138] To pursue this work, Max von Frey, a German physiologist, searched for even more precise and consistent techniques of

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[137] Edwin G. Boring, *Sensation and Perception in the History of Psychology* (New York: Appleton-Century Company, 1942), 467. For a discussion of how Müller's doctrine influenced conceptions of pain, see Roselyne Rey, *The History of Pain*, trans. Louise Elliott Wallace (Cambrige, MA: Harvard University Press, 1995), 192-196. On Müller's doctrine and the study of vision, and particularly the constitution of a subjective observer in the early 19th century, see Jonathan Crary, *Techniques of the Observer: on Vision and Modernity in the Nineteenth Century* (Cambridge, MA: MIT Press, 1990), 88-96.

[138] Boring, *Sensation and Perception*, 467

stimulation that would ensure that stimuli were of comparable magnitudes each time they were applied to the skin.[139]

Von Frey invented an instrument consisting of a wooden stick to which a hair—obtained from a man, woman, horse or hog—was attached with sealing wax. A hair of a specified diameter and length applied a precise amount of pressure when pressed perpendicularly on the skin until it was bent, but not broken. By varying the hair lengths and diameters, it was possible to determine the relative amount of pressure required to elicit sensations of pressure or pain on "microscopic" areas of skin. Von Frey also constructed a modified instrument, in which the hair protruded from a narrow hole in an adjustable sleeve, thus making it easier to change the length of the hair.[140] Von Frey Hairs, or the Von Frey aesthesiometer (that is, an instrument to measure tactile sensation, thus named rather than "algometer" because it was also used to measure pressure thresholds), has commonly been cited by 20th century pain researchers as the first precision pain-measuring instrument.[141]

Von Frey's instrument joined a growing number of meters for the senses that equipped physiology laboratories and the new laboratories of psychology in the second half of the 19th Century. Why were so many sense-meters invented? The discovery of separate sensory and motor nerves in the spinal cord in the early 19th century established sensation as a physiological matter. Previously the realm of philosophers, sensation had been studied as a phenomenon of the mind rather than of the body. Physiologists who became interested in the study of sensation in the early to mid-19th century began creating instruments and methods to measure sensory thresholds and discrimination. For example, E. H. Weber designed an aesthesiometer, an instrument that was similar to a two-point compass, to measure the minimum length between two points at which the subject felt two distinct contacts, rather than a single one. Other instruments were developed to study sensations included the acoumeter, sonometer, ophtalmotrope, photometer,

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[139] Rey, *The History of Pain*, 215-216
[140] Boring, *Sensation and Perception*, 488-89
[141] For example: B. Berthold Wolff, "Laboratory Methods of Pain Measurement," *Pain Measurement and Assessment.* Ed. Ronald Melzack (New York: Raven Press, 1983), 7.

stroboscope, stereoscope, spectroscope, kinesimeter, aesthesiometer, inductorium, olfactometer, as well as sets of standardised stimuli such as piano keyboards, colour mixers and fruit flavours.

Some physiologists saw in the experimental study of sensation a potential for articulating a distinct branch of investigation concerned with the science of the mind: psychophysiology. Building on Weber's work, Gustav Fechner systematized the principles of psychophysiology. The main assumption of psychophysical measurement was that sensory judgment could be correlated with the magnitude of a stimulus such as a ray of light of a particular brightness. Fechner proposed that three kinds of sensory judgments could be quantified: the point at which a stimulus became perceptible (the absolute limen, or sensory threshold), the point at which two stimuli became distinguishable (the differential limen, or "just noticeable differences"), and the point at which two stimuli appeared to be the same.[142]

The use of psychophysical instruments, and the development of principles of measurement, was thus greatly stimulated by the definition of psychophysics as a field of experimental psychology in the second half of the 19th Century. As Gail Hornstein has argued, quantification was central to the project of carving a space for the new science of psychology that was methodologically distinct from philosophy and pseudo-psychology, such as phrenology and palmistry, with which it shared its subject-matter: the phenomena of the mind. In order to make psychology quantitative, the early psychologists delimited their new field by selecting objects of study that were amenable to quantification, and rejecting those that were not. Because of its interest to physiologists, sensation was one of the research topics that seemed apt for establishing psychology as a quantitative and experimental science.[143]

Defining psychophysical measurement as the basis of psychology was challenged by some psychologists who favoured non-quantitative methods of

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[142] Hornstein, "Quantifying Psychological Phenomena," 3-4; Boring, *Sensation and Perception,* 34-45; Crary, *Techniques of the Observer*, 141-149.
[143]Hornstein, "Quantifying Psychological Phenomena," 1-3,18.

inquiry into the mind. They charged that the mind did not make quantitative distinctions, and thus that sensation—as a mental phenomenon—could not be measured. Yet, experimental psychologists continued to practice psychophysical measurement without fully addressing this objection, despite its seriousness. Hornstein has argued that this can best be understood if the debate about quantification is situated within a larger battle to establish a scientific psychology: the alternative proposed by the critics was introspection, a method of study which was not seen as precise and reliable enough to give psychologists the institutional and professional status they sought.[144]

The success of psychophysiology is important to the history of pain, because it provided both the general framework and quantitative techniques for conceptualising pain as measurable and as a sensation. It was by defining pain as a topic amenable to psychophysical investigation that algometers became thinkable and relevant. The psychophysical concept of sensation was based on the assumption of a direct relationship between the type and intensity of a stimulus, the physiological sensory apparatus and the psychological detection of the presence and quality of a sensation.[145] This concept dominated thinking about pain throughout the first decades of the 20th century.

The success of psychophysiology helps to explain not only the emergence, but also the function of algometers as instruments for building up the quantitative techniques of early psychology. Algometers seem to have become part of the standard set of psychological laboratory tools, which included a range of meters for the senses, stimulation devices, chronometric instruments and physiological measuring technologies. For instance, when American psychologist E. B. Titchener produced a guide for equipping a psychological laboratory on the Cornell model, he included "pain aesthesiometers" as well as an algometer designed by James McKeen Cattell, one of the pioneers of American psychology,

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[144] Hornstein, "Quantifying Psychological Phenomena," 5-8.

[145] For an overview of the study of sensation in physiology and psychology, see Boring, *Sensation and Perception.* For a more detailed study of physiological pain research in the 19th century, see Rey, *The History of Pain.* For a discussion of physiological research on vision, see Crary, *Techniques of the Observer.*

in his recommended purchase list.[146] The Cattell algometer, which cost $15 in 1900, was manufactured by Brown & Getty in Camden, New Jersey, which suggests not only that standardisation of the instrument was considered to be important, but also that it had a relatively wide distribution.[147] Several early algometers can also be found in the online apparatus collection of the Archives of the History of American Psychology.[148]

More specifically, algometric methods were given an important role in resolving fundamental questions about the mechanism of pain sensation. They were invented and used to produce evidence that might help resolve the enduring controversy between specificity and summation theories of pain.

With the help of his hairs, which showed differential pressure and pain thresholds at distinct points, Von Frey had concluded in the 1890s that pain was a specific sensory modality. He then correlated the distribution of sensory spots—hot, cold, pain, and pressure—with that of the different cutaneous nerve-endings that had recently been discovered through histological observations. This theory, though constantly reinterpreted, strongly influenced research and thinking on pain for much of the 20th century. It was, nonetheless, "passionately debated and criticized."[149] Its most vocal critic was Goldscheider, who, though he had initially interpreted his sensory-spot findings as evidence of pain specificity, had, after failing to locate any analgesic spots, concluded that the sensation of pain resulted from the intense stimulation of pressure receptors.

Pain theories came to occupy American psychologists, particularly in the laboratory of psychology at Cornell University. In 1919, Cornell researchers found that, surprisingly, pain did show the phenomenon of adaptation, that is, that

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[146] Edward B. Titchener, "The Equipment of a Psychological Laboratory," *American Journal of Psychology* 11, no.2 (1900): 251-265.
[147] Titchener "The Equipment of a Psychological Laboratory," 260; Alexander MacDonald, *Juvenile Crime and Reformation* (Washington DC: Government Printing Office, 1908), 196
[148] Archives of the American History of Psychology, "Apparatus Collection- Cutaneous," http://www3.uakron.edu/ahap/apparatus/category_list.phtml?code_id=2 (accessed January 22, 2006).
[149] Rey, *The History of Pain*, 217.

it subsided after continued, constant stimulation.[150] Adaptation became the object of an *experimentum crucis* in the specificity *vs.* summation debate. Both Von Frey and Goldscheider produced data that supported their theories; Goldscheider found that, before disappearing, the sensation underwent a qualitative change towards one of pressure or contact. Von Frey, however, showed that, given an adequate source of stimulation—a ray of heat focused by a magnifying glass—pain simply died away, without qualitative change. At this point, another Cornell researcher, Karl Dallenbach, undertook a series of experiments in the hopes of resolving this issue.

For Dallenbach, the Von Frey-Goldscheider controversy touched on "one of the fundamental problems in sensory physiology and psychology."[151] To settle the problem, Dallenbach would need to come up with imaginative new means of inflicting precise amounts of painful stimulation to his colleagues and graduate students. For one set of experiments, a stimulator was devised that consisted of a coil of resistance wire placed inside a glass tube and insulated with asbestos. When the coil was heated by a current of specified strength, heat radiated onto the forearm of the observer. Further mechanisms of precision included placing the observer's arm in a plaster cast to avoid movement and the timing of observations with a stopwatch. In another experiment, a similarly structured stimulator made out of brass was filled with dry ice, and was insulated with asbestos and cotton.

Such elaborate instruments were necessary because it was crucial that heat and cold be produced without pressure, and applied to the skin in a controlled and focused stream, in order to determine whether the gradual disappearance of pain was preceded by qualitative changes, and, if so, whether these changes corresponded to the type of stimulation or the nature of the receptor. Though this goal did not, in fact, require the measurement of stimulation, Dallenbach's attention to the calibration of stimulation and the measurement of time responses reflect a concern for greater standardisation in the study of pain.

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[150] H. H. Straus and R. F. Uhlman, "Adaptation of Superficial Pain," *American Journal of Psychology* 30, no.4 (1919): 422-424.
[151] Karl M. Dallenbach, "Pain: History and Present Status," *American Journal of Psychology* 52 (1939): 331-347.

Dallenbach placed the results of these studies at the conclusion of an ambitious review of the history of the principal debates concerning the nature of pain in Western thought, which he published in the *American Journal of Psychology* in 1939.[152] Presenting his data on the observation of precise stimulations of pure heat, cold and pressure, Dallenbach showed not only that pain indeed underwent adaptation, but also that the qualitative changes experienced by observers during the disappearance of pain were derived from the type of stimulation used—cold, heat or pressure—and were not of a "pressury" nature in each case. If pain did not result from the intensive stimulation of pressure receptors, then Goldscheider's theory would be disproved.

Dallenbach rested the strength of his conclusion not only on the quality of his methods for producing pain, but also on the quality of his observers:

> Did our observers, in the two studies with temperature, miss Goldscheider's pressures? Were those pressures so weak that they escaped notice? We think not. Our Os were highly trained in cutaneous observation; it is hardly possible that all of them would have failed all the time to report those qualities if they occurred, particularly as our experimental conditions fulfilled all the requirements that Goldscheider laid down and should, therefore, have been highly favourable for their observation.[153]

This comment raises an important point. Though psychophysical instruments provided carefully controlled and measurable stimuli, they did not obviate the need for subjects (or observers, as they were called previously) to make fine distinctions in observing their inner sensations. Variations in the ability of individuals to make such judgments accurately and consistently would come to preoccupy many pain-measurers. In the early psychology laboratory, however, this potential objection was avoided by controlling for the quality of observers' judgments. Psychological observers—who we now call subjects—usually consisted of, in algometric as well as other laboratory experiments, a small number of graduate students, colleagues or even the experimenter. They were

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[152] Dallenbach, "Pain : History and Present Status," 331-347.
[153] Dallenbach, "Pain : History and Present Status," 346.

familiar with the principles of experimentation, often highly trained, and could be trusted to maintain a scientific attitude of both attentiveness and detachment towards their sensations. The expertise of observers could be ascertained by the readers of published reports, in which their names, experience and individual results were often provided.[154]

To settle fundamental theoretical issues of psycho-physiology, results from a few good observers, who were considered as "universal minds" could be generalised into laws of normal human pain sensation. For some researchers, however, algometry mattered not because it was informative about universal mechanisms of pain sensation, but because it allowed a finer differentiation of inter-individual variations in the degree of sensitivity to pain. This was another early use for which algometers were developed.

3.2 Algometry and the "Great Chain of Feeling"

"In view of the oft-repeated statement that savages in general are less susceptible to pain than white men," reported William McDougall, "it seemed a matter of some interest to obtain a measure of the threshold of sensibility to pain."[155] With the help of a Cattell algometer, McDougall made a quantitative comparison of pain sensibility of Torres Straits Islanders and English men and boys. McDougall had gone to the Torres Straits as a member of the Cambridge Anthropological Expedition, which, in 1898, set out on an ambitious scientific mission to study the Islanders, before the "civilising" efforts of traders and missionaries could erode their state of "primitiveness."[156] Among the numerous facets of the lives, bodies and minds of the Murray Islanders and Sea Dayaks that were scrutinised by the Expedition—which included language, customs and physical dimensions—was the acuity of their senses, to be measured with the instruments of the

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[154] Karl Danziger, "A Question of Identity: Who Participated in Psychological Experiments?" in *The Rise of Experimental Psychology*, 35-52; *Constructing the Subject*, (New York: Cambridge University Press, 1990), 5.
[155] Alfred C. Haddon, ed. *Reports of the Cambridge Anthropological Expedition to Torres Straits* (Cambridge: University Press, 1901-1935) Vol. 2, Part 2, 194.
[156] Richards, *Race, Racism and Psychology*, 44.

psychophysical laboratory.[157] Responsible for the cutaneous senses, McDougall measured pain sensibility thresholds on the fingernails and foreheads of a group of Murray Islanders, five Dayaks and, upon returning to England, performed similar measurements on a group of men and boys in a 'Cheadle convalescent home.'[158] After comparing results from these two groups, McDougall concluded that the Murray Islanders were only half as sensitive to pain as the English.[159]

McDougall's results provided quantitative confirmation of widely held beliefs about differences in sensitivity that Martin Pernick has evocatively described as the "great chain of feeling."[160] Pernick used this expression, a play on the words "great chain of being," to describe the concept of a scale of differential pain sensibility that could be mapped onto a hierarchy of social and evolutionary development. Even before the invention of algometers, it was widely believed that people differed in their ability to feel pain, and that sensibility to pain—both to one's own pain and, in sympathetic resonance, to that of others—was a marker of social and biological difference.

A number of historical studies have attested to the pervasiveness of this belief in various aspects of 19th century European and American thought and

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[157] For a more detailed description of the Cambridge Anthropological Expedition and an analysis of its significance for the conceptualisation of race in the history of psychology and anthropology, see Richards, Race, *Racism and Psychology*, 41-64. Richards situates the expedition within the general framework of Scientific Racism, and sees it as implicitly reinforcing the acceptance of British superiority and domination of colonial subjects on the basis of their primitivism. Henrika Kuklick discusses the significance of this episode in the history of British anthropology in *The Savage Within: The Social History of British Anthropology, 1885-1945* (Cambridge and New York: Cambridge University Press, 1991), 133-149.

[158] Richards, *Race, Racism and Psychology*, 46

[159] Haddon, *Reports*, 195.This finding and other findings of the expedition confirmed the expectation, according to Herbert Spencer's theory of Social Darwinism, that the "lower" senses such as touch and vision were more highly developed in "primitive races," while "higher" senses such as pain were more highly developed in "civilized races." See, Voestermans and Jansz, "Culture and Ethnicity," 173.

[160] Martin S. Pernick, *A Calculus of Suffering Pain, Professionalism, and Anesthesia in Nineteenth-Century America* (New York: Columbia University Press, 1985), 157: "Thus all living things might be arranged in a hierarchy of sensitivity, a great chain of feeling. Brute animals, savages, purebred nonwhites, the poor and oppressed, the inebriated, and the old, constituted the lower orders. The most sensitive included women: the rich, civilized, educated and sophisticated; sober drunkards; and mulattos. Children were usually considered feminine in sensitivity, though infants were sometimes believed not to feel. Occupying the virtuous middle ground were the study yeoman farmers."

practice.[161] In a wide-ranging study of the representation of bodily pain in late Victorian medical, religious, scientific, literary and humanitarian discourses, Lucy Bending has shown that, by selectively attributing to themselves the ability to feel pain, the English elite distanced themselves from supposedly insensible criminals, savages, and other forms of degeneration and brutality, while also defining themselves as civilised. Physiological sensibility to one's own pain was seen as being correlated with moral sensibility to the pain of others, and thus was linked with the ability to inflict pain. One of the major concerns of humanitarian reformers was with the brutalising or dehumanising effects of both viewing violence and inflicting pain on others. These concerns were also articulated in evolutionary terms that made a parallel between sensibility and civilisation. Bending noted, for example, that antivivisectionist tracts qualified as savage the insensibility of those who dissected live animals.[162]

Other historians of humanitarian movements have also drawn attention to the equations made by reformers to link sensibility, civilisation and virtue. Some of these studies have, in addition, emphasized that reformers often felt they had to confront widely held beliefs about the insensibility of slaves, animals, criminals or the insane in order to elicit sympathy for the suffering of the abused.[163] Analysts of literary and visual representations of slaves' suffering, for example, have pointed out the challenges faced by abolitionists in endowing slaves' bodies with the capacity to suffer, and thus humanizing them as subjects.[164] Some abolitionists, on the other hand, accepted that slaves were insensible to pain, but

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[161] Pernick, *A Calculus of Suffering*, 148, has remarked : "The belief that people varied widely in their ability to feel pain influenced almost every aspect of nineteenth-century social and professional life..."

[162] Lucy Bending, *The Representation of Bodily Pain in Late Nineteenth-Century English Culture* (Oxford, New York: Clarendon Press. Oxford University Press, 2000), 3-4, 123-4.

[163] For example, Karen Halttunen, "Humanitarianism and the Pornography of Pain in Anglo-American Culture," *American Historical Review* 100 (1995): 303-34. Elizabeth R Clark, "'The Sacred Rights of the Weak": Pain, Sympathy and the Origins of Humanitarian Sensibility," *Journal of American History* 82, no. 2 (1995): 463-93. James Turner, *Reckoning with the Beast: Animals, Pain, and Humanity in the Victorian Mind.* (Baltimore, MD: Johns Hopkins University Press, 1980).

[164] Clark, "Sacred Rights," 474. Marcus Wood, *Blind Memory: Visual Representations of Slavery in England and America, 1780-1865* (New York: Routledge, 2000), 215-231.

argued that repeated abuse, rather than inherent racial features, had robbed them of their sensibility.[165]

Admission into the world of sentience entitled individuals not only to sympathy and humane treatment, but also to medical relief and to the recognition of their intellectual achievements. Pernick's study of professional judgment and anaesthesia has shown that beliefs about the greater sensitivity of women, the insensitivity of slaves and decreasing sensitivity with age entered into American physicians' decisions about who should receive anaesthesia.[166] Rebecca Herzig has also pointed out that individuals judged as insensible were also judged unable to freely submit to, and heroically endure, acts of noble self-sacrifice. Herzig has argued that an ethic of self-sacrifice was increasingly used to characterize the disinterested pursuit of scientific knowledge in the 19th century. However, to sacrifice oneself, an individual required not only courage, but also the ability to feel pain as well as full possession of one's self and body. These possibilities were not extended equally to all beings. Thus, the "Eskimo" and "Negro" participants of the American polar expeditions were excluded from the praise and credibility extended to the expedition leaders for their contribution of suffering for science.[167]

All of these analyses show that the 19th century concept of sensibility was not limited to a narrow physiological meaning but also encompassed moral and intellectual sensibility. Stating that a person, or animal, was more or less sensitive to pain in 19th century Europe or America was not a neutral judgment about their responsiveness to bodily harm. Judgments about sensitivity to pain were also judgments about a person's moral qualities of virtue, courage and sympathy; about their intellectual abilities and social rank; and about what kind of protection from pain was owed to them, whether this protection consisted of anaesthesia or freedom from slavery.

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[165] Pernick, *A Calculus of Suffering*, 156,158.
[166] Pernick, *A Calculus of Suffering,* 171-195.
[167] Rebecca Herzig, *Suffering for Science: Reason and Sacrifice in Modern America* (New Brunswick, NJ and London: Rutgers University Press, 2005), 24, 65, 80-81.

Alongside its social and moral implications, the concept of sensibility was, since the 18th century, firmly anchored in the physical reactivity of nerves.[168] This was reinforced by the development of sensory physiology in the 19th century. By the end of the century, differences in sensitivity were increasingly understood with reference to degrees of physical evolutionary development.[169] A more highly developed nervous system would be quicker to transmit the feeling of pain. Associations of sensibility with social and moral qualities were not excluded by this evolutionary framework, but instead strengthened by proponents of Social Darwinism.[170] In its simplest formulation, this doctrine, chiefly associated with Herbert Spencer, superimposed the differentiation and classification of social, psychological and biological characteristics onto a hierarchical evolutionary scale. This framework provided a rationale for the production and comparison of algometric data that could be mapped onto categories of social, biological and pathological identities.

In the last decades of the 19th century, a few scientists took up algometry as part of larger projects of measurement and classification of human difference and deviance. Criminal anthropologists in Italy such as Cesare Lombroso and Salvatore Ottolenghi, William MacDougall—British psychologist and member of the Cambridge Anthropological Expedition—and Arthur MacDonald—Specialist in the U. S. Department of Education—counted algometers as an important item in their arsenals of instruments of precision. A variety of other anthropometric and psycho-physiological tools, such as Broca callipers, craniographs,

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[168]G. J. Barker-Benfield, *The Culture of Sensibility: Sex and Society in Eighteenth-Century Britain* (Chicago: University of Chicago Press, 1992), 1-36, traces the anchoring of sensibility in nerves back to physiological theory of Newton, but shows that the notion was rapidly popularised in the genre of the literature of sensibility, where it was given social meanings, in particular a gendered dimension.

[169] For description of pain and sensitivity as understood within a late 19th century evolutionary framework, see Bending, *The Representations of Bodily Pain,* 66, 178-194; Herzig, *Suffering for Science*, 34-5.

[170] On criminal anthropology as a form of Social Darwinism, see Gibson, "On the Insensitivity of Women," 13. On the influence of evolutionary and degeneration theory on Lombroso, see Leonard Savitz, "Introduction to the Reprint Edition," in *Criminal Man According to the Classification of Cesare Lombroso*, ed. G. Lombroso-Ferrero, (Montclair, N. J.: Patterson Smith, 1972), vii. On the influence of Spencerian thought on the Cambridge Anthropological Expedition to the Torres Straits, see Voestermans and Jansz, "Culture and Ethnicity," 173.

pelvimeters, aesthesiometers, thermaesthesiometers, and dynamometers were used to quantify and compare pelvic and cranial dimensions, sensibility to heat and pressure, and physical strength. Lombroso, Ottolenghi, MacDougall and MacDonald participated in a much broader movement in turn of the century anthropology, psychiatry, psychology and criminology to align categories of race, sex, age, social class, mental pathology and criminality with quantifiable differences in physiological characteristics.

Algometric measurements were scientific not only because they were precise, and thus could be compared, classified and correlated with more qualitative characteristics, but also because they allowed experimenters to delve beyond manifest variations in responses to pain to uncover "true sensitivity." Indeed, the question of whether *apparent* responsiveness to pain was justified by *true* inner physiological sensitivity, considered to be the fundamental form of interpersonal variation, was preoccupying for late 19th and early 20th century physicians and scientists.[171] Distinguishing between superficial and deep manifestations of sensitivity was an important goal for those who sought to objectify pain-measurement during this period. This goal was part of different types of projects.

Cesare Lombroso, founder of the Italian school of positivist criminology or criminal anthropology, proposed to shift the emphasis in criminology from the nature of crimes to the nature of criminals.[172] Through comparative measurements of offenders' body parts, facial features and sensibility, a "knowable, measurable

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[171] Pernick, *A Calculus of Suffering,* 161: "Nineteenth-century commentators carefully distinguished between "insensitivity" –the inability to feel pain, and "endurance"- the ability to bear it. They insisted that the differences discussed so far were real differences in the way pain was perceived, not simply differences in the capacity to withstand it." See also, D. Crompton, "Courage or Insensibility (?) to Pain During Double Amputation of the Legs Without Chloroform," *Guy's Hospital Report* 44 (1887): 142-3, in which a surgeon describes the case of a man who underwent amputation without showing any signs of pain. It is unclear whether the man was insensible, and thus felt no pain, or whether he had sufficient courage and will power to remain stoic in the face of great pain. Details of his social status are given, and seem to be the only available clue for answering this question. However, the author does not express any conclusions.

[172] Gibson, "On the Insensitivity of Women," 12-13: Positivists, led by the physician Cesare Lombroso, rejected this abstract equation of crime and punishment and instead recommended the individualised treatment of each defendant based on his or her degree of "dangerousness" to society."

and predictable" criminal type emerged: the born criminal.[173] The classification and examination of actual and potential offenders promised, according to criminal anthropologists, to make criminal justice more effective through the individualization of treatment, and even the prevention of criminality.[174] For criminal anthropologists, the biological anomalies such as insensibility that characterised born criminals were signs of both physical and moral degeneration, that is, of being less evolved than normal, morally healthy individuals.

One of the physiological markers of inherent criminality was a sensibility to pain that was "much less acute" than that of normal individuals, "and sometimes non-existent."[175] Not only was Lombroso able to support his statement with anecdotal stories about criminals who had carried on as if unbothered by serious injuries, or observations on criminals' predilection for tattoos, but he also provided his readers with a precise, quantitative range of normal sensibility.[176] These measurements were significant for Lombroso because he considered physical and moral insensibility to be tightly connected.[177] To measure general sensibility and sensibility to pain, Lombroso had adapted a common electric apparatus, developed by Du Bois-Raymond, for use as an algometer.[178]

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[173] Savitz, "Introduction to the Reprint Edition,"x-xi.

[174] This is illustrated by the introduction by Gina Lombroso-Ferrero of a chapter on the examination of criminals in *Criminal Man*, which is a synthesis of Lombroso's work, 219: "Criminal anthropologists are unanimous in insisting on the importance of the results to be gained from a careful examination of the physical and psychic individuality of the offender, with a view to establishing the extent of his responsibility, the probabilities of recidivation on his part, the cure to be prescribed or the punishment to be meted out to him; but besides furnishing the magistrate with a sound basis for his decisions, the anthropological examination will prove of great assistance to probation officers, superintendents of orphanages and rescue homes and all those who are entrusted with the destinies of actual offenders or candidates for crime."

[175] Lombroso-Ferrero, *Criminal Man,* 247

[176] Lombroso-Ferrero, *Criminal Man,* 247 and Cesare Lombroso, *Nouvelles Recherches de Psychiatrie et d'Anthropologie Criminelle,*(Paris : F. Alcan, 1892), 88. It is unclear, however, in what units Lombroso is expressing pain sensitivity. In *Criminal Man*, the normal range is given as 10-25, while *Nouvelles Recherches* states that the normal figure is 35. In both cases, the same instrument was apparently used.

[177] For example, Lombroso-Ferrero, *Criminal Man,* 39: "Cruelty depends on moral and physical insensibility, those incapable of feeling pain being indifferent to the sufferings of others." For a more general description of the criminologists' views of the criminal as morally insensible, see Gibson, 'On the Insensitivity,"28-42.

[178] There is good evidence that Lombroso's algometric method preceded von Frey's, but does not seem to have become widely known to psychophysical researchers. His method is apparently described in L'Algométria Electrica in 1867, almost two decades before von Frey published his

Essentially, the apparatus delivered precisely quantified electric shocks to the subject. Lombroso did not describe its use more extensively. For Lombroso and his followers, then, algometers were diagnostic tools that could help identify the criminal and thus reform criminal science and the criminal justice system.

In the 1880s and 90s, criminal anthropologists turned their attention to the study of female offenders. The interests of criminal anthropologists extended beyond criminality itself to the evolutionary ranking of human types and the identification of atavistic markers in idiots, epileptics, or primitives. When they studied criminal women, they also made claims about the relative status of women to men, based on data obtained from the normal subjects who served as their controls. In 1890, Lombroso claimed, controversially, that women were less sensitive to pain than men.[179] This went against the commonly stated belief that women were more sensitive than men, and the frequent pointing to hypersensitive white, urban, middle-class women as a warning of the pathological dangers of too much civilisation.[180] Lombroso, however, dismissed the hypersensitivity of women as an illusion, pointing instead to data obtained with his algometer. His work was acclaimed and pursued by fellow criminal anthropologists, most notably Salvatore Ottolenghi, whose book *Sensitivity of Women* was published in 1896. On the basis of algometric evidence, Ottolenghi argued that "true sensitivity" could be distinguished from the superficial emotionality of women. Though he found that numerous women were apparently more responsive to pain than men, Ottolenghi labelled this response as "excitability," the result of having imagined pain before it could actually be felt.[181]

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article on his hair aesthesiometer. See n.2, p 87 of *Nouvelles Recherches de Psychiatrie et d'Anthropologie Criminelle*. For a description of Lombroso's algometer, see G. Lombroso-Ferrero, *Criminal Man*, 246 and 249.

[179] Cesare Lombroso, "Tatto e Tipo Degenerativo Nelle Donne Normali," *Archivio* 11 (1890) :558, cited by Gibson, "On the Insensitivity," 15, 35n.10

[180] Pernick, *A Calculus of Suffering,* 154. See literature on the relationship between the neurasthenia and hysteria and their association both with hypersensitivity and overcivilisation, for example, L. Briggs, "The Race of Hysteria: 'Overcivilisation and the 'Savage' Woman in Late 19th Century Obstetrics and Gynecology," *American Quarterly* 52 (2000): 246-73. Francis Gosling, *Before Freud: Neurasthenia and the American Medical Community* (Urbana: University of Illinois Press, 1987).

[181] Gibson, "On the Insensitivity of Women," 17-8.

According to Mary Gibson's analysis, the Italian criminologists' inquiries into the sensibility to pain of women was meant to be a contribution to debates about the "woman question" taking place at the time in Italy. The measurement of sensibility was a central feature of the criminologists' experiments because of the associations between physical, moral and intellectual sensibility. For the criminologist, the sensitivity tests "offered an objective, scientific evaluation of women's nature." Italian criminologists recommended that restrictions on women's public roles and political engagement—on the basis of their limited moral and intellectual sensibility—be maintained by insisting on the primacy of their "natural" role as mothers.[182]

Lombroso's ideas were particularly influential in the United States.[183] One of the manifestations of this interest was a series of bills presented before the U. S. Senate for the establishment of a "psycho-physical laboratory" for the study of "the criminal, pauper and defective classes" under the federal government. Instrumental in promoting these bills was Arthur MacDonald, who came before Senate judiciary committees to give detailed descriptions of the rationale and methods of such a laboratory in 1902 and 1908.[184] "The most rigid and best method" for a scientific study of man, explained MacDonald, was that of the "laboratory, with instruments of precision in connection with sociological data."[185]

Following French and Italian criminal anthropologists, MacDonald believed that it was possible to understand and prevent the causes of social pathologies through careful correlations of quantitative anthropometrical and psycho-physiological data with information about behaviour, social class, education, intelligence, etc. The algometer was especially important in

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[182]Gibson, "On the Insensitivity of Women," 11-12, 19-26.
[183] Savitz, "Introduction to the Reprint Edition," xix.
[184] The first bill on which MacDonald was granted a hearing was H. R. 14798 in 1902, see Alexander MacDonald, *Hearing on the Bill (H. R. 14798) to Establish a Laboratory for the Study of the Criminal, Pauper, and Defective Classes, with a Bibliography* (Washington: Government Printing Office, 1902). See also, Alexander MacDonald, *A Plan for the Study of Man with Reference to Bills to Establish a Laboratory for the Study of the Criminal, Pauper and Defective Classes* (Washington: Government Printing Office, 1902).
[185]MacDonald, *Hearing on the Bill*, 8.

MacDonald's collection of instruments of precision, because he believed, like Lombroso, that physical insensibility accompanied moral insensibility.[186] Indeed, he had designed his own algometer. It consisted of a scale that indicated pressure in grams, a rod and a disk, covered in flannel to avoid eliciting cold sensations, which was applied to the subject's temple until the subject "felt the pressure to be the least bit disagreeable."[187]

The MacDonald algometer was used by him and various other American researchers to compare the pain sensibility of various categories of adults and to correlate data on sensibility with other information obtained in schoolchildren. In two hearings before the Senate, in 1902 and 1908, MacDonald presented algometric data that he and other researchers had obtained. In one table, he compared the sensibility to pain in the right and left temples of women by age and by occupation. In another, he presented data he had obtained with the Cattell algometer in groups of individuals of different classes, sexes and nationalities. MacDonald also presented the results of extensive studies of schoolchildren, which included figures on pain sensibility. MacDonald proposed that, to study the roots of criminality and abnormality, one should begin with the investigation of children. As specialist in the U. S. Bureau of Education from 1892 to 1902, MacDonald had access to data obtained in large studies of schoolchildren. In various tables, he compared the sensibility to pain of girls and boys, public and private school students, and according to age, the season of birth, strength, and puberty. At least two other studies that focused on the measurement of sensibility in schoolchildren with MacDonald's algometer were published in the *American Journal of Psychology*. In one study of Michigan schoolchildren, sensibility to pain was correlated with age, sex, hair and eye colour, birth order, and

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[186]MacDonald, *Hearing on the Bill*, 41, "we shall see that from this physical insensibility comes in great part moral insensibility."

[187] Descriptions of MacDonald's temple algometer are found in MacDonald, *Hearing on the Bill*, 98-99; MacDonald, *A Plan for the Study of Man,* 26-28; and MacDonald, *Juvenile Crime and Reformation*, 190-193. In the latter MacDonald also decribes Cattell's and Chéron's algometers. MacDonald also emphasized in each of these texts the importance of using instruments of precision in the study of humans. See, MacDonald, *A Plan,* 8: "The time has come when it is important to study a child with as much exactness as we investigate the chemical elements of a stone or measure the mountains on the moon."

"brightness" or "dullness."[188] A further study of pain sensibility in schoolchildren attempted to verify these findings with respect to age and mental ability.[189]

While, according to available evidence, there were probably fewer than a dozen algometric studies conducted in the United States at the turn of the century, some of these studies included hundreds and even thousands of children. Even more significant is the fact that repeated attempts were made to introduce bills for the establishment of a federal laboratory for the study of "abnormal classes" and that these received endorsements from about three dozen senators and congressmen, 55 "American Specialists," 20 "European Specialists" including Lombroso, Ottolenghi and Havelock Ellis, as well as a motion passed by the 5th International Congress for Criminal Anthropology.[190] James B. Gilbert has shown that MacDonald's theories enjoyed brief popularity in Congress, and that he was viewed as an eminent criminologist doing reputable work until he was dismissed as a "crank" in the 1930s.[191] If the bills to establish such a laboratory had not been defeated, it is possible to imagine that algometric measurement would have been practiced much more extensively.

Algometry thus enjoyed a brief period of popularity at the turn of century, especially in the U. S., as a means of measuring group differences in pain sensibility. This way of using algometers resonated with broader trends in thinking about the significance of differences in pain sensibility; the relevance of correlations of social and biological data; the measurability of pain as a psycho-physiological event determined by nervous sensibility; and the use of common typologies of social, biological and pathological classification. Despite contradictions in algometric findings, and the relatively limited scale on which it

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[188] A. Carman, "Pain and Strength Measurements of 1,507 School Children in Saginaw, Michigan," *American Journal of Psychology* 10 (1899): 392-398.
[189] E. J. Swift, "Sensibility to Pain," *American Journal of Psychology* 11 (1900): 312-317.
[190]MacDonald, *Hearing on the Bill*, 135-7, also includes excerpts from texts published in scientific medical and legal journals expressing support for the psycho-physical laboratory.
[191] James B. Gilbert, "Anthropometrics in the U. S. Bureau of Education: The Case of Arthur MacDonald's 'Laboratory,'"*History of Education Quarterly* 17 (1977): 169-195. Gilbert also argues that a major source of opposition to MacDOnald's lab was his employer in the bureau of education Willam T. Harris. Gilbert suggests that had MacDonald not been employed in this branch of government, his laboratory might have been established more easily.

was practiced, turn of the century algometry is revealing of more widely accepted notions about pain, individual difference and the value of quantification.

Specific claims about differences in pain sensibility were not universally accepted. Nor did everyone agree about the social and political implications of observed differences. For example, as we have seen, claims were made both for the superior and inferior sensitivity of women to pain. Italian women's rights activists rejected the criminologists' findings as a basis for granting civil and political rights, and called for a "friendly" and "objective" science to replace the biased and illogical methods of the criminologists.[192] In the United States, many feminists agreed that women were more sensitive to pain than men. While some saw this as an indicator of women's moral superiority, others blamed Victorian medical culture for making women hypersensitive.[193] There was also some confusion about whether infants were insensible, like animals, or hypersensitive, like women; debates about whether slaves' insensitivity was "natural" or could be cured by freedom from brutality;[194] and different positions on whether increased pain sensibility was proportional, or inversely correlated with the acuity of "lower" senses such as touch and vision.[195]

That specific claims about the sensitivity to pain of certain groups were often contested reveals the importance given to the implications of differences in

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[192] M. Gibson, "On the Insensitivity of Women," 26-31, for the response of women's groups to Lombroso and Ottolenghi's findings.

[193] Pernick *A Calculus of Suffering*, 160-1.

[194] Pernick, *A Calculus of Suffering,* 153, points out that both some abolitionists and some proponents of slavery argued that slaves' insensibility was the result of brutal treatment, the first to criticize slavery, and the second to "improve" slavery by making it less brutal, and thus more defensible.

[195] Spencer believed that the "lower" senses such as sight and tactile discrimination were more highly developed in primitive peoples, as they were in animals, leaving less nervous energy for rational thought, while sensibility to pain was more highly developed in English subjects, see note 23. Similarly, Ottolenghi had explained divergences in his findings between general sensitivity and pain sensibility in women by arguing that the first represented only the delicacy of the skin, while the second was more directly linked to the level of cerebral evolution, see M. Gibson, "On the Insensitivity of Women." Others, like Galton, argued that all sensory perceptiveness was linked to mental ability, but what counted was not the measurement of the first threshold of perception, but the differences in intervals that could be discriminated. Thus Galton distinguished between the delicacy fo some women's nerves, which he likened to a morbid hypersensitivity, and the power to discriminate between minute differences in intensity of stimulation, which was greatest in men. See Francis Galton, *Inquiry into Human Faculty and its Development* (London: J. M. Dent & Sons, 1911).

sensitivity. In addition, those who commented on the topic generally described sensitivity to pain as a physiological characteristic, a stable, if not fixed, attribute of the responsiveness of the nervous system. Criticisms of particular experiments or statements often confirmed the belief that inter-individual variations in sensibility were not only real, but could be measured and patterned according to a specific typology.[196]

By the first decades of the 20th century, however, after the failure of MacDonald's project, interest in this type of algometric practice waned. While psychologists and anthropologists seem to have lost their interest in algometric studies of differential pain sensibility in race, sex, age and intelligence after the turn of the century, American clinicians drew on the techniques and concepts of these studies to develop methods to refine the accuracy of clinical judgment. They proposed to use this technique not to differentiate racial and class-based sensibility, but to test their individual patients' "sensitiveness" to pain. They were critical of categorical generalisations about pain sensibility, but did not reject the relevance of finer differentiations. For them, the utility of algometric principles, which they applied without instrumentation, was to improve the quality of their clinical judgment.

3.3 The Importance of Clinical Observation

A typical view on pain and differences was Richard J. Behan's, author of a popular American clinical textbook on pain, initially published in 1914 and several times re-edited. Behan dismissed such "general statements" about racial differences in sensitivity, for example, he warned that a comment made in a BMJ editorial—that "the Hebrew stands pain less easily than any other race"—

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[196] For example E. B. Titchener, "On Ethnological Tests of Sensation and Perception with Special Reference to Tests of Color Vision and Tactile Discrimination described in the Reports of the Cambridge Anthropological Expedition to the Torres Straits," *Proceedings of the American Philosophical Society*, 55 (1916), 204-36: criticised the techniques used by expedition members, but applauded the effort and offered constructive criticism on how to conduct psychophysical measurements in the field.

"smack[ed] of the feuilletonist and [was] not to be taken too seriously."[197] He also advised his readers to take "the generalisation" made by MacDonald and Ottolenghi on differences in pain sensibility in different classes of women "*cum grano salis*" (with a grain of salt)[198] Behan nevertheless accepted the idea of differential susceptibility to pain, and suggested that temperament and complexion affected sensibility, while he also accepted "a certain relationship between the degree of mentality and susceptibility to pain. The higher the development and the more vivid the imagination, the greater is the susceptibility."[199]

Behan advised that differences in susceptibility to pain be taken into account when estimating the intensity of a patients' suffering. While he praised the value and accuracy of algometers, Behan instead proposed a simple pinching test to determine patients' quickness to react to pain. In this manner, it was possible to weigh patients' complaints against a rough measure of their physiological sensitivity. Descriptions of suffering alone were deemed not to be "of much practical assistance in deciding upon the severity of a pain."[200] Variations in "descriptive ability, powers of imagination, and vocabulary"[201] made one patient more convincing than another, even while both may be experiencing the same degree of pain. Thus, for Behan, algometric measurement would theoretically be a means of putting patients on same level, despite their different abilities to access language (a quality that was presumably linked to education level, and therefore socioeconomic status). Behan emphasised the importance of pain patterns as a diagnostic guide for physicians. Along with an examination of patients' appearance, reflexes and blood pressure, knowledge of their sensitivity to pain helped to provide an accurate clinical picture of their real symptoms. For example, more sensitive patients afflicted by the same disease

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[197] Richard J. Behan, *Pain, its Origin, Conduction, Perception and Diagnostic Significance* (New York: D. Appleton, 1914), 112, the reference given was to the *BMJ* (1906), 880. *Pain, its Origin* was again published in 1916, 1920, 1921 and 1926
[198] Behan, *Pain*, 113.
[199] Behan, *Pain*, 115.
[200] Behan, *Pain*, 128.
[201] Behan, *Pain*, 128.

might present a different pattern of symptoms from less sensitive ones. Differences in patients' expressiveness, however, might mask these essential differences.[202]

Emmanuel Libman was one of the physicians who agreed with Behan that testing patients' sensitivity to pain could help decipher the relationship between the intensity of symptoms and the severity of disease, without running the chance of being deceived by patients' inability to express, or to feel, their own pain.

In the late 1920s, Emmanuel Libman, a distinguished American physician, described a test designed to determine sensitiveness to pain, which he had reportedly been using "for a number of years."[203] It was a simple technique by which the physician applied strong pressure to patients' mastoid bone with his fingers, observed their reaction and then questioned them about whether they had felt pain. It was important that the patient be unaware of the test, which was one of the reasons why Libman felt that algometers were not appropriate for clinical use. By surprising the patient, it was possible to bypass their own interpretations of suffering and thus distinguish between the "natural sensitivity"[204] of the patient, the "sensitiveness to which he is born,"[205] as measured on the basis of their spontaneous response to the painful pressure, and their reactivity to, or tolerance of pain, which was expressed in their tendency to complain. A patient who showed little or no reaction to the test was given a score of 0 or + and classified as hyposensitive, while one who cried out or withdrew was called hypersensitive and scored as +++. The Libman test was shown to be sensitive to

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[202] Behan, *Pain*, 120-129. This section was section titled "estimation of the intensity of pain."
[203] Emanuel Libman, "Practical lecture, New York Academy of Medicine, Dr Emanuel Libman, Friday afternoon, January 11, 1929," Libman Papers, Modern Manuscripts Collection (MMC) 406, Box 18, History of Medicine Division (HMD), National Library of Medicine (NLM). See also Emmanuel Libman, "Observations on Sensitiveness to Pain," *Transactions of the Association of American Physicians* 41 (1926): 305-8.
[204] Emmanuel Libman, "Observations on Individual Sensitiveness to Pain With Special Reference to Abdominal Disorders," *Journal of the American Medical Association* 102 (1934): 355-41. Libman writes: "It is of course necessary to distinguish between the sensitiveness to pain and the way in which the patient acts in response to what he feels. The increased or decreased response can be termed decreased or increased tolerance (...) The sensitiveness that is determined by means of the test is regarded as the natural sensitivity."
[205] Emanuel Libman, "Practical lecture, New York Academy of Medicine, Dr Emanuel Libman, Friday afternoon, January 11, 1929," Libman Papers, MMC 406, Box 18, HMD, NLM.

racial differences: 98% of "Pueblo Indians" had scored as hyposensitive, based on a study conducted by a medical student. Libman himself had found a high proportion of hyposensitives among pugilists (boxers).[206]

While Libman encouraged further testing of age, sex, race and even species-based differences in sensitiveness to pain by means of his test, his real interest was in improving physicians' ability to match clinical symptom patterns with cardiac and abdominal disorders.[207] He was particularly interested in the diagnostic challenges presented by hyposensitive individuals. Libman complained that textbook descriptions of pain patterns in particular disorders were based on hypersensitive patients, while hyposensitives—who constituted 30% of his own patients and over 90% in particular groups—presented an entirely different constellation of symptoms.[208] Knowing the hyposensitive status of a patient could draw a clinician's attention to what Libman called "substitution" or "covered" symptoms that would have been manifested as pain in more sensitive patients suffering from the same disorder.[209]

While it may not be possible to determine how widely the Libman test was used in routine clinical practice, his observations were both familiar to, and commended by, many physicians including eminent American physicians such as the Mayo brothers, founders of the Mayo Clinic, Walter Alvarez, and M. C. Winternitz, Dean of the Yale University School of Medicine.[210] Libman was often invited to lecture specifically on pain, or to comment on papers dealing with

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[206] Libman, "Observations on Sensitiveness," 305-8.
[207] Libman, "Observations on Individual Sensitiveness," 355-41. This article was based on a paper read before Section of Gastroenterology and Proctology of AMA session 1933.
[208] "Classifies patients by sensitiveness to pain: Dr Libman Says Some People Feel None, Though Disease. Calls for Text-book Revision," *New York Times* (January 12, 1929), in Libman Papers, MMC 406, Box 18, HMD, NLM.
[209] Libman, "Observations on Individual Sensitiveness," 355-41; Emanuel Libman, "Practical lecture, New York Academy of Medicine, Dr Emanuel Libman, Friday afternoon, January 11, 1929," Libman Papers, MMC 406, Box 18, HMD, NLM.
[210] William J. Mayo to Emanuel Libman, August 24, 1933, Libman Papers, MMC 406, Box 5, HMD, NLM; Walter Alvarez to Emanuel Libman, May 15, 1925, Libman Papers, MMC 406, Box 3, HMD, NLM; M. C. Winternitz to Emanuel Libman, n.d., Libman Papers, MMC 406, Box 6, HMD, NLM : "You recall that we talked about sensitiveness to pain when I saw you last, and I am particularly impressed by the clear way in which you have stated this difficult problem."

painful symptoms in cardiac and gastrointestinal disorders.[211] In several lectures and at least three published articles, Libman described the use of his test.[212] One of these lectures was reviewed in a *New York Times* article, in which it was reported that "a number of physicians present said it was of extreme practical importance, especially to the general practitioner."[213] His publications, in particular an article in JAMA, were later qualified as "classic" contributions on the topic of pain symptoms.[214]

The apparent appeal of Libman's method, and of his detailed observations on the relationship between sensitivity, symptoms and disease, can be linked to contemporary preoccupations among elite clinicians about the status of clinical observation. Libman's test of pain sensitivity did not challenge traditional clinical judgment. It was meant as an aid, rather than a replacement, of a physician's diagnostic skills and powers of observation. It thus conformed to older styles of celebrating physicians' uncanny ability to "read" patients' pains through the art, rather than science, of medicine.[215] Libman acknowledged that, to apply the test

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[211]Harold Brunn (Mount Zion Hospital) to Emanuel Libman, January 7, 1941, [MS C 406, Emanuel Libman Papers, Box 11], mentions that the members of staff of Mount Zion Hospital requested that Libman speak about pain at a dinner, and that Libman should also speak on the topic of pain to the students the following afternoon. William J. Kerr (University of California) to Emanuel Libman, January 16, 1941, [MS C 406, Emanuel Libman Papers, Box 11]: "I think it would be of great interest to the students and to those of us on the Faculty who are able to attend If you would discuss the subject of pain in general and with particular reference to your own contributions. I always ave been tremendously impressed by your discussions and writings on this subject."H. L. Blockus (American Medical Association) to Emanuel Libman, January 12, 1933, [MS C 406, Emanuel Libman Papers, Box 12], invites Libman to participate in a session on abdominal pain during a meeting of the section of Gastroenterology of the AMA.

[212]"Copy of notes taken by stenographer of Diagnostic Clinic given by Dr. E. Libman at the meeting of the Inter-State Post Graduate Assembly at Cleveland Ohio on October 18, 1926," [MS C 406, Emanuel Libman Papers, Box 18]; Libman, "Observations on Individual Sensitiveness," 355-41; "Observations on Sensitiveness,"305-308; "Studies in Pain," *Transactions of the Association of American Physicians* 44 (1929): 52.

[213] "Classifies patients by sensitiveness to pain: Dr Libman Says Some People Feel None, Though Disease. Calls for Text-book Revision," *New York Times* (January 12, 1929), Libman Papers, MMC 406, Box 18, HMD, NLM : "Though Dr. Libman disclaimed after the lecture any revolutionary importance for his work, a number of physicians present said it was of extreme practical importance, especially to the general practitioner."

[214] L. Pelner, "The Determination of Sensitivity to Pain. A Simple Clinical Method," *Journal of Laboratory and Clinical Medicine* 27 (1941): 248-51; E. D. Sherman, "Sensitivity to Pain. (With an Analysis of 450 Cases)," *Canadian Medical Association Journal* 48 (1943): 437-41.

[215] A good illustration of how pain would ideally be read by the skilful, observant physician in the early 20th century is given in: "Gestures' Meaning in the Pain-Striken," *New York Times* (June 30, 1907), 12, an article about Professor Thomson, a "minutely observant physician" who was able to

and interpret its results correctly, experience and judgment on the part of the physician were required.[216] Applying finger pressure on the styloid process of the patients' mastoid bone had to be done with precision and consistency, a task that was not standardised by an instrument. In addition, the test enabled physicians to tailor their diagnostic interpretations to the individual characteristics of the patient. Thus, greater accuracy in diagnosis did not necessarily have to depend on recourse to laboratory tests; it could also originate in a more individualised reading of symptoms.

Libman's interest in pain is explicitly linked, in his lecture notes, to a concern about the waning prestige of clinical medicine and the authority of laboratory research. At the conclusion of his lecture to the New York Academy of Medicine, Libman explained: "I did not choose this subject [pain] at random. I chose this subject for specific reasons. I wanted to speak on something that was purely clinical. We all realize that clinical medicine is falling behind, not only in this country, but all over. The practitioner is ashamed that he is not doing laboratory work."[217] In another set of undated "lecture notes on clinical medicine," Libman elaborated on the importance of clinical observation, and emphasized the dangers of not conducting a thorough examination *before* calling for laboratory tests. Quoting Robert McNair Wilson and Alexis Carrel, Libman presented the progress of clinical medicine as depending on physicians' efforts to attain "the widest possible knowledge of human nature" and their "ability to grasp the characteristics which make each human being an individual."[218] Libman's emphasis on the importance of clinical observation sounds similar to the

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diagnose diseases simply by reading their body language while they described their pain. Thomson explained this to his students, the article reported, "in an effort to teach medical students the value of cultivating habits of intelligent observation."

[216] Untitled, n.d., Libman Papers, MMC 406, Box 19, HMD, NLM : "I fear that Dr. Boles has had little experience with the test. Whatever method is employed, errors can be minimized only by much experience

[217] Libman, "Practical lecture,"

[218] "Lecture notes on clinical medicine," Libman Papers, MMC 406, Box 19, HMD, NLM. The Wilson quote, drawn from *Pygmalion or the Doctor of the Future* (1926), depicts the doctor of the future not as a laboratory scientist but as a humanist, having "the widest possible knowledge of human nature, and the deepest possible understanding of human motives. He will be a cultured man, ripe in intellectual attainments, but not lacking in emotional sympathy..." The Carrel quote is probably drawn from Alexis Carrel, *Man, the Unknown* (1935).

antireductionist stance described by Christopher Lawrence in the rhetoric of elite British clinicians during the Interwar period.[219] This rhetoric defended the value of knowledge produced by bedside observation, and thus of special skills and gentlemanly status of elite clinicians, against the dominance of laboratory-generated knowledge. In suggesting that patients' sensitivity should be tested, Libman did not promote instrumental precision or inter-observer standardisation in the measurement of symptoms, nor did he promise impersonal diagnostic accuracy. Instead, Libman, like many American and British clinicians in the late 19th and early 20th centuries, believed that the potential power of the laboratory, rationalisation and instruments of objectivity should be harnessed without threatening their traditional claims to authority: humanism, skill and individualisation.[220]

While agreeing with Libman's emphasis on reading symptoms in the light of sensitivity, several physicians criticized his test in the late 1930s and early 1940s for its lack of precision and calibration. They charged that the constancy of the degree of pressure applied by the physician's finger might vary from one test or examiner to the next, and that the imprecision of the stimuli gave only a rough estimate of sensitivity. Laboratory methods of algometry, which had multiplied since the 1890s, were, unfortunately, too unwieldy and time-consuming for clinical use. Two new, more accurate but still clinically convenient, methods of sensitivity measurement were proposed.

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[219] Christopher Lawrence, "Still Incommunicable: Clinical Holists and Medical Knowledge in Interwar Britain,"in *Greater Than the Parts: Holism in Biomedicine, 1920-1950,* ed. C. Lawrence and G. Weisz, 94-111 (New York and Oxford: Oxford University Press, 1998).

220 There is a fairly extensive literature on medical "resistance" or "conditional acceptance" of diagnostic, laboratory and standardising technologies with reference to a desire to protect traditional medical autonomy, authority and skills. Some examples that are relevant for this period in the U. S. and U. K. include: Christopher Lawrence and George Weisz, eds. *Greater Than the Parts: Holism in Biomedicine, 1920-1950.* (Oxford: Oxford University Press, 1998); Joel Howell, *Technology in the Hospital: Transforming Patient Care in the Early 20th Century* (Baltimore and London: The Johns Hopkins University Press, 1995). Steven Sturdy and Roger Cooter, "Science, Scientific Management, and the Transformation of Medicine in Britain c. 1870-1950," *History of Science* 36 (1998): 421-466; Harry M. Marks, "Medical Technologies: Social Contexts and Consequences," in *Companion Encyclopedia of the History of Medicine*, ed. W.F. Bynum and R. Porter (London and New York: Routledge, 1993), 1592-1618.

The first, a "clinical gauge for sensitivity to pain," which was described in 1938 by E. Hollander, consisted of an elliptical metal grater that was taped to the inner surface of a blood pressure cuff. As the cuff was inflated, the grater pressed into the skin without puncturing it. When patients either winced or made a "verbal protest," the mercury level indicated on the cuff was read. The instrument could easily be reconverted to its original function. Hollander, agreeing with Libman, advised that patients not be informed about the test in order to obtain a "spontaneous, unconditioned reaction." His concern was to obtain a reading that was untainted by patients' own judgment of their sensitivity.[221]

The second instrument was Pelner's sensometer, which could be made by adapting a universally available instrument called a Geneva Lens Measure. Unlike Hollander's gauge, Pelner argued, the sensometer could be used in hypertensive patients and would not be affected by the amount of subcutaneous fat on patients' arms. The sensometer consisted of two fixed points, which were pressed onto the non-fleshy surface of the thumb "until the pain becomes unbearable," while a central point moved in response to the pressure exerted and could be read off a dial. Pelner described the calibration and recording measurements in detail, and noted that the instrument was "standardised the same way all over the world." Pelner was thus concerned with the replication of measurement from one observer to another.[222]

While those physicians who used the Hollander gauge and sensometer between 1938 and 1944 explicitly placed themselves within the framework of Libman's "classic" work on the relationship of sensitiveness to symptoms of pain, their concerns in testing sensitiveness was different from Libman's. First, as we have seen, they seemed much more preoccupied with standardisation and quantitative precision, for which instruments seemed necessary. They were also interested in making a new kind of comparison: between the sensitivity of neurotic and anxious patients who suffered from ill-defined somatic complaints,

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[221] E. Hollander, "A Clinical Gauge for Sensitivity to Pain," *Journal of Laboratory and Clinical Medicine* 24 (1938-1939): 537-38.
[222] Pelner, "The Determination of Sensitivity," 248-51.

and the sensitivity of apparently mentally-healthy patients who clearly suffered from organic disorders or did not suffer at all.[223] While there is no evidence that the sensometer and Hollander gauge continued to be used beyond these few experiments, this new interest in the relationship between psychological characteristics such as neuroticism and differential responsiveness to pain would persist during the next few decades.

3.4 Precision in Therapeutic Evaluation

Two articles, published in 1915 and 1916, reported the use of an algometric method to measure drug effects. The first was written by a team of researchers from the Laboratory of Physiology at Harvard Medical School, who had developed a quantitative method of measuring sensitivity by means of faradic current, which was part of a bigger project on the measurement of induction shocks.[224] This article was part of a series of publications describing the calibration of their method and its use for measuring variations in sensitivity over time, as well as under the influence of fatigue and drugs.[225] In reporting this small experiment, conducted on two subjects, who were medical students, they emphasized the validity of their method for measuring physiological sensitivity rather than the significance of their findings or the implications of their method for pharmacological testing.

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[223] R. M. Wilder, Jr., "Sensitivity to Pain," *Proceedings of the Staff Meetings Mayo Clinic* 15 (1940): 551; Sherman, "Sensitivity to Pain," 437-41. Wilder and Sherman specifically compared pain sensitivity in patients with organic and those with functional complaints, including nervousness, anxiety, exhaustion, and vague and ill-defined pains. See also J. O. Haman, "Pain Threshold in Dysmennorrhea," *American Journal of Obstretics and Gynecology* 47 (1944): 686-91, the focus in this article was somewhat different. It attempted to determine whether women who experienced more menstrual pain received more physiological pain signals, or were simply more sensitive to pain. The implicit question may have been about whether dysmennorrheic women had a "psychological" susceptibility to pain.

[224] E. G. Martin, C. M. Grace and J. H. McGuire, "The Influence of Drugs on the Human Sensory Threshold," *Journal of Pharmacology* 6 (1915): 527-32. Martin, *The Measurement of Induction Shocks; a Manual for the Quantitative Use of Faradic Stimuli* (New York: J Wiley & Sons, 1912).

[225] E. G. Martin, "A Quantitative Study of Faradic Stimulation. II. the Calibration of the Inductotherm for Break Shocks," *American Journal of Physiology* 22 (1908): 116. E. G. Martin, P. R. Withington, and J. J. Putnam, "Variation in the Sensory Threshold for Faradic Stimulation in Normal Human Subjects III. The Influence of General Fatigue," *American Journal of Physiology* 34 (1914): 97.

The second team used a similar method of measurement, but expressed more concern about the need for objectivity and accuracy in therapeutic evaluation. Accurate information about the pain-relieving efficacy of the opiate drugs was lacking, the authors pointed out, because no attempt had been made to quantify pain, and because of the "notorious subjectivity" of patients' reports of analgesia.[226] They "blinded" their two subjects (kept them ignorant of what they received), who were also medical students, and tested the effects of both active and inactive substances on their pain thresholds. That inactive substances produced no change in thresholds was presented as proof of the validity of the method. The team placed a strong emphasis on the control of the conditions in which measurements were made, ensuring that subjects were not distracted by noise or drafts, and that their expectations did not influence their reports. This experiment was part of a more general investigation of the pharmacological effects of opiate drugs that was supported by a grant from the American Medical Associations's Council on Pharmacy and Chemistry.[227] These experiments fitted well with the Council's goal of ensuring that claims for therapeutic efficacy—especially those made by the pharmaceutical industry—were evaluated expertly and objectively. Despite this, there was no follow-up to these studies until the mid-1930s. Only then did interest in analgesic innovation and testing become strong enough to stimulate the development and use of more precise techniques to measure pain relief.

Conclusion

By the late 1920s, algometry had been a part of four very different kinds of projects. Basic psychophysiological research on pain continued to create a modest demand for algometers into the 1930s. The application of psycho-physiological

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[226] D. I. Macht, "Action of the Opium Alkaloids, Individually and in Combination With Each Other, on the Respiration," *Journal of Pharmacology and Experimental Therapy* 7 (1915): 339-73.
[227] See also D. I. Macht, "Action of the Opium Alkaloids, Individually and in Combination With Each Other, on the Respiration," *Journal of Pharmacology and Experimental Therapy* 7 (1915): 339-73.

instruments by psychophysical and criminal anthropologists was brief: its popularity doesn't seem to have endured past the first years of the 20th century. Afterwards, from the 1920s, algometric data on differential pain sensitivity was produced by clinicians, who were more interested in the diagnosis and aetiology of individual ills than that of social pathologies. In the late 1930s, clinicians proposed new, more precise and consistent methods to measure pain sensitivity in patients. In addition, they became interested in the sensitivity of anxious and neurotic patients, an interest that would continue to develop in the 1940s. In the mid-1910s, two research groups used algometric techniques to obtain quantitative measures of the efficacy of analgesic drugs by algometric means, but there was no follow-up to these attempts in the U. S. until the mid-1930s.

How did these researchers seek to transform the evaluation of pain sensibility by using algometers? Because they sought to compare and classify human responses to pain, they valued quantitative precision. However, they did not all aspire to the same level of precision. For early 20th century clinicians, rough comparative classifications of sensitivity were sufficient. For them, algometry would function as an aid to clinical judgment rather than a means to eliminate it. To map out the sensitivity of skin surfaces, more precise and neutral values were required. Because algometers were not used as part of large collaborative projects, it did not matter that the type of instrument, how it was used, and what units it measured in, was not standardised among pain-measurers.

These researchers also sought to move beyond what they saw as superficial differences in subjects' pain responses—their emotional fluctuations, variations in their ability to express themselves, or their willingness to show their suffering—to detect a stable, inner "true" physiological sensitivity. How they did this varied to some extent, and this seems to have depended both on the nature of the project and of the subjects. In many cases, subjects were required to make rather difficult judgments in identifying the precise moment at which a stimulus became painful. The mechanization of the stimulus, using instruments that could deliver discrete, measurable, constant sources of pain, was meant to facilitate the precision of this judgment. In other cases, especially when clinical patients were

used as subjects, algometers were used to measure automatic or spontaneous responses, such as movements or complaints. In both cases, there was a concern to make subjects' responses more automatic, to mechanize them to a certain extent either by training subjects to become impartial "instruments" or by keeping them naïve and eliciting responses that were not the product of conscious and deliberate judgments.

Late 19th and early 20th century pain-measurers conceptualized differences in sensitivity to pain as physiological differences that mapped onto moral and intellectual qualities, and were aligned with a biological classification of human differences. Within this conceptualisation of pain, however, there was room for different categorisations of difference and political positions, and for the influence of environmental modulation.

In 1940, the introduction of a new algometric technique promised greater precision and accuracy in pain-threshold measurement, and produced data that suggested a new way of conceptualising pain. The reception of this method, the Hardy-Wolff-Goodell radiant-heat or dolorimetric method, is revealing of changes in the purpose and value given to the quantification and standardisation of pain. According to its creators, the dolorimeter showed that the sensory threshold for pain—that is, physiological sensitivity—was constant in all neurologically normal individuals, independent of gender, personality, fatigue or emotional state.[228]

The two following chapters will examine how the potential of the dolorimetric method, and the implications of its findings, were embraced by different groups of researchers. I will argue that the enthusiastic reception of the dolorimeter was indicative of broader transformations in the meaning and importance of pain-measurement. In chapter 4, I will describe how the search for differences in responses to pain shifted its focus from biological to psychological sources of variation. This shift conformed with dolorimetric data suggesting that

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[228] A. Schumaker, H. Goodell, J. D. Hardy, H. G. Wolff, "Uniformity of the Pain Threshold in Man," *Science* 92, no. August 2 (1940): 110-112. See also, J. D. Hardy, H. G. Wolff, H. Goodell, "Studies on Pain. A New Method for Measuring Pain Threshold: Observations on Spation Summation of Pain," *Journal of Clinical Investigation* 19 (1940): 649-57.

variations in responses to pain originated in differing psychological and emotional responses to pain sensation rather than in differing physiological capacities to perceive pain sensation. However, this shift also followed from a broader social conviction that culture and personality were at the root of significant differences in human behaviour and subjective somatic experience, and that personality could be "algogenic". The measurement of sensory thresholds was replaced by the measurement of psychological reactivity to pain, while new tools, developed to measure personality characteristics and attitudes towards pain, were also applied in the study of differential responses to pain.

The precision offered by the dolorimeter was also welcomed by those who were seeking a better means of testing analgesics. In chapter 5, I will describe how a growing interest in analgesic innovation intensified the search for pain-measuring technologies in the late 1930s. The dolorimeter was well-suited to the requirements formulated in this search. However, the continuing interest in analgesic innovation and testing eventually created new demands and possibilities for methods of measuring pain-relief, and the dolorimeter was displaced by the techniques of the analgesic clinical trial.

4. "All Brothers under the Skin": How Responses to Pain Became a Psychological Matter

In 1940, a group of researchers from Cornell University Medical School announced before the National Academy of Sciences that everyone had the same capacity to perceive pain.[229] Results obtained with the dolorimeter, a new pain-measuring instrument, in 150 subjects, of different ages and sexes, and of different self-reported sensitivities, showed that the pain perception threshold was stable and uniform.[230] This was an astonishing finding. Not only did it contradict five decades of algometric measurement, but it also went against the plainly obvious: that people varied widely in their responses to pain. Three implications of this dramatic revelation were emphasized by its authors and their audience: First, despite their apparent differences in responses to pain, individuals were, deep inside, equal in their physiological sensitivity. Second, a pain-threshold measuring method of unprecedented precision and accuracy, one capable of detecting the true pain-perception threshold, had been discovered by the Cornell researchers. Finally, if the explanation for differences in responses to pain was not to be found at the level of sensory physiology, then it must have something to do with the psychological state of the experiencing subject. Though all three of these statements would be criticized and modified, the last had perhaps the greatest long-term impact on pain-measuring practices.

As I will show in this chapter, however, the integration of this proposition—that individuals varied in their psychological reactivity rather than their physiological sensitivity to pain—into research practice had fairly little to do with the dolorimeter itself. Instead, this integration depended on the appropriation of techniques and conceptual models from other areas of research that could broadly be defined as "psychosomatic." These were techniques and models that

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[229] "Brothers Under the Skin: We All Feel Pain in Same Way, National Academy is Told", Newsweek (May 6, 1940) in Wolff Papers, Box 1, Folder 6, Medical Center Archives of New York-Presbyterian/Weill Cornell.
[230] A. Schumaker, H. Goodell, J. Hardy, H. G. Wolff, "Uniformity of the Pain Threshold in Man," *Science* 92 (1940): 110-112.

allowed researchers to define, standardise and measure the influence of psychological processes on bodily phenomena and experience, and conversely, of psychological reaction to physiological stimulation. While the dolorimeter may not have, on its own, brought about a dramatic shift in ways of thinking about pain, the impact of this shift on pain research is reflected in the ways in which some contemporary researchers took up this new instrument and adopted the theoretical distinction between the *sensation of*, and the *reaction to*, pain formulated by its creators Hardy, Wolff and Goodell. There were other dimensions of the contemporary response to the dolorimeter and its findings, which were adopted, modified and criticized by a new generation of researchers. I will briefly describe these as a means of illustrating the significance of the dolorimeter, and of exploring the different meanings and uses of pain-measurement circa 1940: Why were people interested in pain-measurement? What exactly did they want to measure? What was the value of data produced by measuring technologies?

Popular news coverage of the discovery drew attention to the broad social and cultural implications of the discovery, heralding a break with old beliefs about sensory inequality and proclaiming a new scientific position of sensory egalitarianism. "Science" had now shown that Americans were, in matters of pain sensitivity, "brothers under the skin," according to the title of a *Newsweek* article.[231] This discovery went against everyday experiences and popular notions of sensitivity, which would not be "so easily shaken," according to the Victoria *Colonist*.[232] The *American Weekly* gave an example: "You no doubt have heard an individual brag and say he never feels pain, while someone else cringes at the very thought of going to the dentist and says, 'am so sensitive',"[233] which was echoed by *Newsweek* "as far as physical sensitivity to pain is concerned, a

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[231] "Brothers Under the Skin," in Wolff Papers, Box 1, Folder 6, Medical Center Archives of New York-Presbyterian/Weill Cornell.
[232] "Threshold of pain," *The Victoria Colonist* (September 6, 1941) in Wolff Papers, Box 1, Folder 6, Medical Center Archives of New York-Presbyterian/Weill Cornell.
[233] "When You Begin to Say 'Ouch!'" *American Weekly* (July 7, 1940), in Wolff Papers, Box 1, Folder 6, Medical Center Archives of New York-Presbyterian/Weill Cornell.

phlegmatic labourer and a high-strung woman are in the same boat."[234] The *Sunday Mirror Magazine Section* offered an evocative illustration of the implications: "if two persons are subjected to the same stimulus and one yells 'Ouch!' louder than the other, it does not mean that the louder one has greater sensitivity to pain but merely that he is in better voice."[235] Other articles, including those published by the *New York Times* and *Life* magazine, echoed this tone.[236] For the general public, then, measuring pain was significant for what it could say about how individuals interacted with each other and about the ways they were, or were not, fundamentally different.

The inventors of the dolorimeter, James Hardy, Harold G. Wolff and Helen Goodell, emphasized the methodological implications of the constant threshold: their method had succeeded where others had failed. Previous researchers such as Libman, explained the Cornell team, had found wide variations in sensitivity because their testing methods had failed to discriminate between subjects' *perception* of the painful stimulus, and their *reaction* to the experience of pain. In the dolorimetric method, a focused beam of heat was used as a stimulus, one which allowed subjects to differentiate sharply between the sensation of warmth and the beginning of the sensation of pain. In other words, it made it easy for subjects to become detached and accurate judges of an isolated "pure" pain sensation. Previous algometers, on the other hand, had measured a mixture of sensory and emotional responses to pain. While the sensory threshold for pain was the same for all neurologically normal individuals, explained Hardy, Wolff and Goodell, the psychological reaction to pain varied greatly from one person to another.[237]

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[234] "Brothers Under the Skin."
[235] "Why you Holler 'Ouch'," *Sunday Mirror Magazine Section* (September 7, 1941) in Wolff Papers, Box 1, Folder 6, Medical Center Archives of New York-Presbyterian/Weill Cornell.
[236] Harry M. Davis, "Science in the News" *New York Times* (August 31, 1941); "Pain: Researchers at New York Hospital Discover that everyone is equally Sensitive to it," *Life* (March 3, 1941), 46, 49-50, in Wolff Papers, Box 1, Folder 6, Medical Center Archives of New York-Presbyterian/Weill Cornell.
[237] Hardy, Wolff and Goodell often explained this distinction in their numerous publications. It is discussed at length in their book J. D. Hardy, H. G Wolff, and H. Goodell, *Pain Sensations and Reactions* (Baltimore: Williams & Wilkins, 1952). See also: Schumaker et al., "Uniformity of the Pain Threshold," 110-112. J. D. Hardy, "The Nature of Pain," *Journal of Chronic Diseases* 4

The separation of pain into these two components—sensation and reaction—was not new. It had been articulated, for example, by American psychologist C. A. Strong as a means of reconciling the "quale" theory of pain, in which pain was defined as an emotion of displeasure, with the definition of pain as a sensory event that could be studied by psychophysical methods.[238] Clinicians such as Behan and Libman had also discussed the difference between individuals' sensitivity to painful stimulation and their capacity to endure or tolerate the experience of pain. However, they, and many of their contemporaries, had defined sensitivity as the "true" seat of pain feeling. Libman and Behan had valued algometric tests because they thought it important to dig beyond differences in the proclivity to show or express feelings of pain, differences which were only superficial and of little medical interest per se, to uncover this "true sensitivity."[239] What was new in 1940, then, was to claim that manifest variations in responses to pain were *not* due to differences in sensitivity, and to suppose that they were wholly psychological rather than physiological in nature. It was also the first time that results indicating universal uniformity between subjects, rather than findings of regular patterns of difference between groups, had been held up as evidence for the validity of an algometric method.

Hardy, Wolff and Goodell suggested that the stable sensory threshold produced by the dolorimeter could be useful in several contexts. Its capacity to detect and quantify the effect of analgesic drugs on stable threshold made it useful as a means of comparing the potency of different drugs.[240] As we will see in the following chapter, this possibility was welcomed by those who, in the context a

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(1956): 22-51. H. G. Wolff, "Cornell Conference on Therapy: Psychologic Aspects of Treatment of Pain," *New York State Medical Journal* 45 (1945): 1003-9.

[238]C. A. Strong, "The Psychology of Pain," *The Psychological Review* 2 (1895), 329-347. This was the published version of a paper read before the American Psychological Association at its Princeton meeting, and was given in response to the publication in 1894 of *Pain, Pleasure and Aesthetics* written by proponent of the quale theory of pain Henry R. Marshall.

[239] See previous chapter. See also: E. Libman, "Observations on Individual Sensitiveness to Pain With Special Reference to Abdominal Disorders," *Journal of the American Medical Association* 102 (1934): 355-41; Richard Joseph Behan, *Pain Its Origin, Conduction, Perception and Diagnostic Significance* (New York, London: D. Appleton, 1914).

[240] H. G. Wolff, J.D. Hardy and H. Goodell, "Studies on Pain. Measurement of the Effect of Morphine, Codeine and Other Opiates on the Pain Threshold and an Analysis of Their Relation to the Pain Experience," *Journal of Clinical Investigation* 19 (1940): 659-80.

growing interest in analgesic innovation in the 1930s and 40s, were searching for a more precise and consistent test of analgesic efficacy.

The dolorimetric capacity to isolate the sensory component of pain was also exploited by its creators in launching "a wholehearted attack" in the area of the quantitative psychophysics of pain "in a manner analogous to the psychophysics of vision or hearing."[241] The Cornell team used the dolorimeter to apply psychophysical measurement techniques, such as the measurement of just noticeable differences (jnd), which had been used in other areas of sensation, but not to pain. On the basis of jnd measurements, Hardy, Wolff and Goodell elaborated a scale of pain intensity in dol units, specifying the range, magnitude and number of degrees of discriminable intensities of stimulation. The focus of this research was on universal laws of pain psychophysics. While Hardy, Wolff and Goodell did show that the threshold for pain perception was alterable by suggestion and distraction, these were transitory modifications of a threshold which, under conditions of attentive and detached observation, was said to be usually constant. That the threshold could be altered in these ways did not imply stable patterns of individual difference, unless one considered that individuals were differentially susceptible to the effects of suggestion.[242]

Although this line of investigation was commended as the first real attempt to develop a thorough and quantitative psychophysical investigation of pain,[243] few researchers followed up on it. Nor were the few attempts to make the dolorimeter relevant in clinical pain evaluation successful in converting clinicians to its use.[244] Apart from those who adopted the dolorimeter for analgesic

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[241] W. Edwards, "Recent Research on Pain Perception," *Psychological Bulletin* 47 (1950): 449-74.
[242] J. D. Hardy, H. Wolff, and H. Goodell, "Studies on Pain: Discrimination of Differences in Intensity of Painful Stimuli As a Basis of a Scale of Pain Intensity," *Journal of Clinical Investigation* 26 (1947): 1152-8; J. D. Hardy, H. Wolff, and H. Goodell, "Studies on Pain: An Investigation of Some Quantitative Aspects of the DOL Scale of Pain Intensity," *Journal of Clinical Investigation* 27 (1948): 380-6. H. G. Wolff and H. Goodell, "The Relation of Attitude and Suggestion to the Perception of and Reaction to Pain," *Proceedings of the Association for Research on Nervous and Mental Disease* 23 (1943): 434-48.
[243] Edwards, "Recent Research on Pain Perception," 449-74; Edwin G. Boring, "Introduction," in Hardy et al., *Pain Sensations and Reactions*.
[244] James Hardy and Carl T. Javert, "Studies on Pain: Measurements of Pain Intensity in Childbirth," *Journal of Clinical Investigation* 28 (1949): 153-62; Frederik P. Haugen and William

evaluation, researchers seemed to be more interested in the reaction to pain, which, according to Hardy, Wolff, and Goodell, was dominated by the psychological characteristics of the individual (their previous experience, the meaning they attributed to the pain, their personality), than in its sensory perception.[245]

Indeed, many researchers in the 1940s, 50s and 60s were more preoccupied by the manifest variability of responses to the infliction of pain, than in the underlying uniformity in sensory sensitivity. In searching for the patterns and causes of this variation, researchers after 1940 turned towards the study of reactivity to pain. Some even challenged the uniformity of the pain perception threshold proposed by Hardy, Wolff and Goodell in the 1940s,[246] and it became widely accepted by the 1950s that even the sensory aspects of pain could not be isolated from a person's psychological disposition.[247] In other words, while some researchers claimed that even the sensory threshold was, to some extent, modulated by psychological influences, and others claimed that it was stable, most saw the *reaction* threshold as wholly determined by individual's previous experience, personality and interpretations of the painful event, and thus as key in investigating these sources of modulation.[248]

K. Livingston, "Experiences With the Hardy-Wolff-Goodell Dolorimeter," *Anesthesiology* 14 (1953): 109-16.

[245] Hardy, et al. *Pain Sensations and Reactions.* See also: Schumaker et al., "Uniformity of the Pain Threshold," 110-112; Hardy, "The Nature of Pain," 22-51; Wolff, "Cornell Conference on Therapy," 1003-9.

[246] L. H. Lanier, "Variability in the Pain Threshold," *Science* 97 (1943): 49-50. J. R. Schamp, R.M. Schamp, "Variability of the Pain Threshold in Man," *Journal of Dental Research* 25 (1946): 101-4. There were also some teams who obtained high levels of variability in the data they obtained from the Hardy-Wolff-Goodell apparatus but continued to consider it valuable nonetheless. See chapter 3 for references.

[247] Edwards, "Recent Research on Pain Perception," 449-74.

[248] Some who rejected the uniformity of the pain threshold believed that there did exist a purely sensory threshold but that it was not necessarily invariable, for example: W. P. Chapman, C.M. Jones, "Variations in Cutaneous and Visceral Pain Sensitivity in Normal Subjects," *Journal of Clinical Investigation* 23 (1944): 81-91. J. W. Clark, "Factors Affecting Human Responses to Pain Stimulation," (PhD Thesis, McGill University, 1955). Others, however, rejected the possibility of a measurable sensory threshold because, they argued, by the time the sensation of pain enters consciousness, a subjects' brain had already begun to "react" to the pain, and thus sensation and reaction cannot be separated. For example: H. K. Beecher, "Relationship of Significance of Wound to Pain Experienced," *Journal of the American Medical Association* 161 (1956): 1609.

My aim in this chapter is to situate this "psychological turn" in pain research with respect to a much broader surge of interest in the relationship between emotional responses, psychological makeup (personality) and somatic experience, which preceded and accompanied the response to the dolorimeter. I will trace some of the connections between research on differential pain responses from the 40s to the 60s and the preoccupations, as well as the methods, of a current of psychosomatic interest that touched some quarters of American medicine, psychiatry, psychology and anthropology. I will thus argue that Hardy, Wolff and Goodell's claim that differences in responses to pain were psychological in nature was convincing because it fit within the concerns and explanatory framework of this broader psychosomatic movement. Perhaps more importantly, this claim was convincing because it was possible to investigate it using experimental techniques and categories developed by psychologists, psychiatrists and physiologists that connected emotional, mental and psychological processes.

4.1 The American Psychosomatic Movement and the Painful Personality

In 1940, when the first articles on the dolorimeter were published, words such as "personality," "stress," "neurotic" and "psychosomatic" seemed to be seeping into the vocabulary of a growing number of American medical scientists and clinicians. These words would soon enter into the writings of pain researchers. The increasingly prevalent use of such terms was a consequence of the growth of psychosomatic medicine in the U.S. in the 1930s and 40s. While psychosomatic ideas had existed much earlier in physiology and psychiatry, psychosomatic research, as an organized and funded field, began to emerge in the 1930s.

Robert Powell has traced the roots of this modern American psychosomatic movement to three different strands: the physiological investigation of emotional responses, pioneered by Walter B. Cannon and influenced by the work of Pavlov; the psychobiological, holistic approaches to medicine developed by American psychiatrist Adolf Meyer, and pursued by Helen Flanders Dunbar; and the psychoanalytic approaches to the psychogenesis

of somatic symptoms proposed by European-influenced American psychiatrists such as Franz Alexander.[249]

In the 1930s, the efforts of advocates of psychosomatic medicine such as Dunbar and Alexander, [250] combined with new support from American philanthropies, began delineating this field as an active, coherent and laboratory-based area of research. Psychosomatic research was presented by its supporters as a strategy for filling the gaps left behind by rapid but selective medical progress. Introductions to books and conferences on psychosomatic medicine, for example, sometimes began with descriptions of the significant advances in medical diagnosis and treatment resulting from bacteriology and pathology, a narrative in which Virchow and Flexner often figured as champions. Soon, however, the authors were bemoaning the paucity of etiological and therapeutic knowledge about many complex chronic disorders, the lagging status of psychiatry, and the deplorable neglect of the patient as a whole in modern medicine.[251] This view was shared by representatives of philanthropies, such as Alan Gregg of the Rockefeller Foundation, who also emphasized the importance of applying experimental and preferably quantitative methods to the study of psychical processes and their physiological correlates in order to make psychiatry stronger and more relevant to mainstream medicine.[252] The Josiah Macy Jr. Foundation began funding an

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[249] Robert Powell, "Healing and Wholeness: Helen Flanders Dunbar (1902-59) and the Extra-Medical Origins of the American Psychosomatic Movement, 1906-36," (PhD Thesis, Department of History, Duke University, 1974). For an analysis of the emergence of the physiology of the emotions, see Othniel E. Dror, "The Affect of Experiment: The Turn to Emotions in Anglo-American Physiology, 1900-1940," *Isis* 90 (1999): 205-37. For a description of how Cannon's research influenced the later psychosomatic ideas of Franz Alexander and Hans Selye, see Alan Young, "Walter Cannon and the Psychophysiology of Fear," *Greater than the Parts*, ed. C. Lawrence and G. Weisz (Oxford and NewYork: Oxford University Press, 1998), 234-256.

[250] Other key actors included Stanley Cobb, Harold G. Wolff, Stewart Wolf, John C. Whitehorn, among others…

[251] F. Alexander, "Introduction," and "The Role of Modern Psychiatry in the Development of Medicine," *Psychosomatic Medicine: Its Principles and Applications* (New York: Norton, 1950), 17-23 and 24-34.
E. Weiss and O. English, "Chapter 1: Psychosomatic Medicine," in *Psychosomatic Medicine: The Clinical Application of Psychopathology to General Medical Problems* (Philadelphia and London: W. B. Saunders, 1943), 3-20; Franklin G. Ebaugh, "Introduction to Symposium on Military Neuropsychiatry," *Proceedings of the Association for Research on Nervous and Mental Disease* 25 (1946): xiii-xv.

[252] Jack D. Pressman, "Human Understanding: Psychosomatic Medicine and the Mission of the Rockefeller Foundation," in *Greater Than the Parts: Holism in Biomedicine, 1920-1950,* ed. C.

extensive review of research on "emotions and bodily changes" in the early 1930s. The book that came out of this review, first published by Dunbar in 1935 and re-edited in 1938 and 1946, also emphasized the need for physiological and quantitative research methods in psychosomatic medicine.[253] When Dunbar's newly formed American Society for Research in Psychosomatic Problems (which would become the American Psychosomatic Society) founded the journal *Psychosomatic Medicine* in 1939, the Josiah Macy Jr. Foundation contributed to its publication.[254] The Commonwealth Fund also provided grants for psychosomatic research in the 1930s and 40s.[255] The Rockefeller Foundation initiated a significant funding program for psychosomatic research in the mid-1930s, under the direction of Alan Gregg, which notably provided support to the Chicago Institute of Psychoanalysis, directed by Franz Alexander, and the psychiatric service of the Massachusetts General Hospital run by Stanley Cobb.[256] Alexander's clinic received additional funding from the Julius Rosenwald Fund, [257] while Cobb did additional contract work for the Office for Scientific Research and Development (OSRD) during World War II.[258]

Lawrence and G. Weisz, 189-208 (Oxford and New York: Oxford University Press, 1998).J. Groen, "Foreword to Symposium on Life Stress and Bodily Disease" *Proceedings of the Association for Research on Nervous and Mental Disease* 29 (1950): xv-xviii.

[253] Helen Flanders Dunbar, *Emotions and Bodily Changes: A Survey of Literature on Psychosomatic Interrelationships, 1910-1933* (New York: Columbia University Press, 1935); *Emotions and Bodily Changes: A Survey of Literature on Psychosomatic Interrelationships, 1910-1933*, 2nd Edition (New York: Columbia University Press, 1938); *Emotions and Bodily Changes: A Survey of Literature on Psychosomatic Interrelationships, 1910-1945*, 3rd Edition (New York: Columbia University Press, 1946); *Emotions and Bodily Changes: A Survey of Literature on Psychosomatic Interrelationships, 1910-1953* (New York: Columbia University Press, 1954); *Emotions and Bodily Changes: A Survey of Literature on Psychosomatic Interrelationships, 1910-1953*, Reprinted edition (New York: Columbia University Press, 1972).

[254] Dunbar, "Introduction to the Second Edition," in *Emotions and Bodily Changes*, 2nd Edition (1938), xviii, n.2 and 4; R. Powell, "Healing and Wholeness."

[255] Various articles in Volume 29 of *Proceedings of the Association for Research on Nervous and Mental Disease* (1950) on "Life Stress and Bodily Disease" report funding from the Commonwealth Fund. The main sponsors also included the Rockefeller Foundation, the Office of Naval Research and the National Institute for Mental Health.

[256] Pressman, "Human Understanding," 189-208.

[257] Powell, "Healing and Wholeness," 30.

[258] P. D. White, S. Cobb, W. P. Chapman, et al., "Observations on neurocirculatory asthenia," *Transactions of the Association of American Physicians* 58 (1944): 129-137. The work described in this paper and others was done under a contract, recommended by the Committee on Medical Research, between the OSRD and the MGH. Responsible investigators: Stanley Cobb and Paul D. White.

This instance of OSRD funding for psychosomatic research also illustrates the importance of military and wartime concerns about neurotic disorders in the development of American psychiatry and psychosomatic research. The prevalence of psychoneuroses among military recruits and fighters during World War II focused the attention of military authorities on the need for a well-organized psychiatric service, and convinced them of the value of research into the aetiology and diagnosis of psychosomatic disorders.[259] More broadly, as Colonel Ebaugh put it in the introduction of the Proceedings of a symposium on *Military Neuropsychiatry*: "in the war experience every responsible medical man has seen the need of a medical profession better prepared to deal with emotionally determined forms of incapacitation."[260] The need for diagnostic and screening technologies for military purposes may also have stimulated the development of psychometric tools to measure dimensions of personality, such as the Minnesota Multiphasic Personality Inventory (MMPI), first described in print in 1940, and the Maudsley Personality Inventory, for which research began in 1940.[261] Such tools were soon used to determine the presence of personality disorders that were thought to make individuals susceptible to neurotic and psychosomatic illnesses. One doctor, for example, advocated the wide use of the MMPI in general practice as a means of differentiating between the organic and psychogenic aetiology of unexplained somatic symptoms.[262]

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[259] Dunbar, "Introduction to the Third Edition," in *Emotions and Bodily Changes,* 3rd Edition (1946); . Ebaugh, "Introduction to Symposium on Military Neuropsychiatry," xiii-xv. The massive epidemic of "neurotic disorders" during World War I had first stimulated the development of psychiatric services.

[260] Ebaugh, "Introduction to Symposium on Military Neuropsychiatry," xiii.

[261] S. R. Hathaway and J. C. McKinley, "A Multiphasic Personality Schedule (Minnesota): Construction of the Schedule," *Journal of Psychology* 10 (1940), 249-254; A. Petrie, *Personality and the Frontal Lobes: An Investigation of the Psychological Effects of Different Types of Leucotomy* (Philadelphia: Blakiston, 1952).

[262] A. A. White, "Evaluation of Psychogenic Symptoms in General Medicine : Use of the Minnesota Multiphasic Personality Inventory," *Journal of the American Medical Association* (1951), 1521-26. Others also investigated and advocated the use of the MMPI in general medicine, for example: D. Cohen, "Psychological Concomitants of Chronic Illness: a Study of Emotional Correlates of Pulmonary Tuberculosis, Peptic Ulcer, Arthritides, and Cardiac Disease," (PhD Thesis, University of Pittsburgh, 1949); J. Fisher, "Some MMPI Dimensions of Physical and Psychological Illness," *Journal of Clinical Psychology* 20 (1964): 369-75; L. J. Hanvik, "MMPI Profiles in Patients with Low-Back Pain," *Journal of Consulting Psychology* 15 (1951): 350-351; D. W. Hastings, et al., "Early Objective Personality Evaluation in Medical Diagnosis," *University*

Increased funding for psychosomatic research programs was followed by the publication of landmark texts on psychosomatic medicine such as Dunbar's *Emotions and Bodily Changes* (1935)[263], Weiss and English's *Psychosomatic Medicine* (1943)[264], and Alexander's *Psychosomatic Medicine* (1950).[265] Conferences on psychosomatic themes were also increasingly organized from the late 1930s[266], while a specialized journal and research society were also founded at that time. These initiatives demarcated psychosomatics as a specific area of medical research and practice. Though psychosomatic research and theory continued to encompass diverse approaches characterized by its psychoanalytic, physiological and psychological roots, the concretization of the field through funding, publications and institutional organisation provided a common heading for different types of research activities, as well as opportunities for collaboration and synthesis between researchers. The emphasis on quantitative and experimental methods also encouraged methodological cross-fertilization, particularly in adopting research techniques from physiologists of the emotions and psychologists' psychometric instruments. An illustration of the growing collaboration and emphasis on exact methods of investigation can be found in the published proceedings of a symposium on "Life Stress and Bodily Disease" sponsored by the Association for Research in Nervous and Mental Disease in 1949. Its participants included physiologists such as Hans Selye, psychoanalysts such as Franz Alexander and neurologists such as Harold G. Wolff, while its

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of Minnesota Medical Bulletin 28 (1957): 1-12; H. B. Hovey, "Somatization and Other Neurotic Reactions and MMPI Profiles," *Journal of Clinical Psychology* 5 (1949): 153-57.

[263] Dunbar, *Emotions and Bodily Changes* (1935, 1938, 1946, 1954, 1972).

[264] Weiss and English, *Psychosomatic Medicine* (1943, 1949), followed by Weiss, E., and O. S. English. *Psychosomatic Medicine: a Clinical Study of Psychophysiologic Reaction* (Philadelphia: Saunders, 1957).

[265] Alexander, *Psychosomatic Medicine,* (1950, 1952).

[266] See, for example: Dunbar, "Introduction to the Second Edition," xviii-xix n.5: lists a few of the meetings on psychosomatic themes held in the late 1930s by the American Psychiatric Association (a yearly symposium on psychosomatic problems inaugurated in 1933-34; the Tenth Medical Congress for Psychotherapy (a major section devoted to psychosomatic problems in 1938), and the Association for Research on Nervous and Mental Disease (the annual program in 1938 was devoted to the mind-body problem). The latter association also held meetings on the topic of Pain (1942, published in 1943), Military Neuropsychiatry (1944 published in 1946, the Frontal Lobes (published in 1948) and Life Stress and Bodily Disease (1949 published in 1950) that addressed psychosomatic issues.

papers presented results obtained from methods such as Rorschach testing of individuals with thalamic brain lesions, and analysis adrenal gland secretions in laboratory animals subjected to stressful situations.[267]

Some of the researchers who became interested in the psychological modulation of individual responses to pain participated in such meetings, and thus can easily be associated with this movement for psychosomatic research.[268] Many more, however, can be linked to psychosomatic research through their use of terminology and concepts of causation. The design and interpretation of experiments on differential pain reactions were based on the assumption that personality factors such as neuroticism, as well as individual and collective conditioning, were the main variables affecting responses to pain. Implicitly or explicitly, research reports conceptualized stimulation, response and pathogenesis in terms that made sense within the framework of psychosomatic thinking. For example, painful stimulation was often conceptualized as a form of stress, to which individuals responded emotionally and physiologically according to the "threat content" they associated with the stimulus.[269] If they had a neurotic or anxious personality, they were likely to be more reactive to painful stimulation.[270]

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[267] Various chapters in *Proceedings of the Association for Research on Nervous and Mental Disease* 29 (1950).

[268] M. Furer and J. Hardy, "The Reaction to Pain as Determined by the Galvanic Response," *Proceedings of the Association for Research on Nervous and Mental Disease* 29 (1950), 72-89. W. P. Chapman, A. S. Rose, and H. C. Solomon, "Measurements of Heat Stimulus Producing Motor Withdrawal Reaction in Patients Following Frontal Lobotomy," *Proceedings of the Association for Research on Nervous and Mental Disease* 27 (1948): 754-768; W. P. Chapman, "Measurements of Pain Sensitivity in Normal Control Subjects and in Psychoneurotic Patients," *Psychosomatic Medicine* 6 (1944): 252-57. R. B. Malmo, C. Shagass, F. H. Davis, "Specificity of Bodily Reactions Under Stress. A Physiological Study of Somatic Symptom Mechanisms in Psychiatric Patients," *Proceedings of the Association for Research on Nervous and Mental Disease* 29 (1950)237-268.

[269] R. B. Malmo, C. Shagass, J. F. Davis, "Pain As a Standardized Stimulus for Eliciting Differential Physiological Responses in Anxiety," *American Psychologist* 2 (1947): 344; R. B. Malmo, et al., "Standardized Pain Stimulation As Controlled Stress in Physiological Studies of Psychoneurosis,"*Science* 108 (1948): 509-11; J. D. Hardy and M. Furer, "Reaction to Pain As Determined by Galvanic Skin Response," *Federation Proceedings* 9 (1950): 56.

[270] Malmo, Shagass and Davis, "Pain As a Standardized Stimulus," 344. Malmo et al., "Standardized Pain Stimulation," 509-11; W. P. Chapman, J. E. Finesinger, C. M. Jones, and S. Cobb, "Measurements of Pain Sensitivity in Patients With Psychoneurosis," *Archives of Neurology and Psychiatry* 57 (1947): 321-31.

Conversely, the relief of anxiety raised the reaction threshold.[271] The way in which individuals responded to, or (mis-)adapted to stress could also put them at risk of developing psychosomatic disorders, which often included symptoms of pain such as headache, chest and low back pain.[272] The level of threat content that was attributed to different kinds of stressors, including painful stimulation, was informed by personal and cultural conditioning, while the range of acceptable responses to stress was constrained by both cultural and self-imposed norms and values.[273] This conception of psychological reactivity to pain was not only to be found in experiments of differential pain reaction, but was also reflected in new emphases on central (cerebral), as opposed to peripheral, mechanisms in the neurophysiology of pain, [274] and, among a few psychiatrists, in reflections on the psychodynamic processes that could cause psychogenic experiences of pain.[275]

Investigations of the pain reaction can also be linked more concretely with contemporary developments in psychosomatic research through their methodologies. To correlate responses to pain with variables such as personality and past experience, researchers needed not only the means to standardise specific dimensions of pain responses, but also the means to standardise and manipulate psychological states and processes. Several different strategies were adopted by those who investigated differential pain reactions from the 1940s to the 1960s.

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[271] H. E. Hill, R. E. Belleville, and A. Wikler, "Anxiety Reduction as a Measure of the Analgesic Effectiveness of Drugs," *Science* 120 (1954): 153.

[272] For example: Thomas H. Holmes and Harold G. Wolff, "Life Situations, Emotions and Backache," *Proceedings of the Association for Research on Nervous and Mental Disease* 29 (1950): 750-772.

[273] H. G. Wolff, "Life Stress and Bodily Disease—A Formulation," *Proceedings of the Association for Research on Nervous and Mental Disease* 29 (1950):1059-1094; Hardy, et al., *Pain Sensations and Reactions*; Mark Zborowski, "Cultural Components in Response to Pain," *Journal of Social Issues* 8 (1952): 16-30; Ronald Melzack, "The Effects of Early Experience on the Emotional Responses to Pain," (Phd Thesis: McGill University, 1954)

[274] Edwards, "Recent Research on Pain Perception," 449-74; T. Barber, "Toward a Theory of Pain: Relief of Chronic Pain by Prefrontal Leucotomy, Opiates, Placebos, and Hypnosis," *Psychological Bulletin* 56 (1959): 430-460.

[275] L. Rangell, "Psychiatric Aspects of Pain," *Psychosomatic Medicine* 35, no. 1 (1953). George L. Engel, "Psychogenic Pain and the Pain-Prone Patient," *American Journal of Medicine* 26 (1959): 899-918. Engel, "Psychogenic Pain," *Journal of Occupational Medicine* 31 (1961): 249-56. See also the literature on low back pain, which increasingly emphasizes the emotional aspects of pain beginning in the 1950s: Steindler, A., *Lectures on the Interpretation of Pain in Orthopedic Practice*, (Springfield, Ill.: Charles C. Thomas, 1959); Holmes and Wolff, "Life Situations," 750-772.

First, new means of measuring pain were designed to focus on more psychologically labile dimensions of how individuals responded to pain. However, in most cases, the measurement of these responses alone told researchers little about what they meant. To make them meaningful, data on pain responses had to be correlated with information about the psychological makeup of subjects. Drawing on a wide range of psychological categories and technologies that had recently been defined in psychiatry, psychology, physiology and even anthropology, investigators of pain reactions began to measure, model and even alter the personalities of their experimental subjects. In the remainder of this chapter, I will describe the techniques used to make measurements of responses to pain psychologically meaningful.

4.2 New Measures of Pain

Most researchers adopted the dolorimeter, or other algometric methods, to apply standardised painful stimuli. However, both *who* and *what* they measured with these instruments differed from previous practices of sensibility or sensitivity measurement. Instead of, or in addition to, measuring the introspective detection of pain perception, they measured the point at which subjects showed a motor reaction (wincing or withdrawal),[276] the point at which a subject could no longer endure the painful stimulation (asked for it to be stopped),[277] or the autonomic response of subjects to painful stimulation (such as sweating, finger tremor, increased pulse, etc.).[278] These measures of reaction, tolerance and autonomic indicators were understood to represent emotional responses to pain. In the latter

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[276] This was the strategy employed by W. P. Chapman in his numerous experiments on pain reactions. Chapman also measured pain perception thresholds, by the same method as Hardy, Wolff and Goodell, and compared the variance in both thresholds, as well as the differences between them.

[277] This method was used by Asenath Petrie in the late 1950s: A. Petrie, W. Collins, and P. Solomon, "Pain Sensitivity, Sensory Deprivation, and Susceptibility to Satiation," *Science* 128 (1958): 1431-33.

[278] For example, Malmo, et al. "Pain As a Standardized," 344; Malmo, et al., "Standardized Pain," 509-11; Furer and Hardy, "The Reaction to Pain," 72-89; Richard A. Sternbach and Bernard Tursky, "Ethnic Differences Among Housewives in Psychophysical and Skin Potential Responses to Electric Shock," *Psychophysiology* 1 (1965): 241-46.

case, the development of measurement techniques, and the association of autonomic indicators with emotional responses, can be traced to research on the physiology of emotions.[279]

A few other innovations were made in developing methods to measure pain. One psychologist dedicated his doctoral research to the development and validation of a Pain Apperception Test. This was a self-rating instrument, in which subjects rated the emotional content of a standard set of images.[280] In itself, this method seems to have had little impact, but it represented a new approach to the standardisation and quantification of subjects' own reports that was not only becoming increasingly common in contemporary psychometric practices, but which would also, two decades later, become prevalent in the design of pain assessment tools. Finally, in one study, qualitative ethnographic methods were used to investigate culture as a determinant of differential pain responses.[281] This approach was not picked up by other anthropologists, but the results of the study became widely cited among pain researchers, and made ethnicity into a meaningful category for future quantitative studies on pain. [282]

4.2.1 Choosing Subjects, Modelling Personality

While measures of reactions to pain such as withdrawal were labelled as emotional responses, data obtained on these reactions in samples of "normal" subjects gave ambiguous results. It was clear that they varied, but what did this mean? Selecting specific comparison groups of subjects was one way in which

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[279] See O. Dror, "The Scientific Image of Emotion: Experience and Technologies of Inscription," *Configurations* 7 (1999), 335-401, for an analysis of the way in which physiological recording techniques were constructed as means of representing emotional processes, in particular, his comments on "emotion-gauging technologies," 361-368. See also the section on "the problem of measurement" in H. F. Dunbar, *Emotions and Bodily Changes*, 81-107(discussion) and 458-474 (bibliography) to see how technologies such as the psychogalvanometer were debated in relation to the question of the measurability of psychosomatic mechanisms.

[280] Donald V. Petrovich, "The Pain Apperception Test: Psychological Correlates of Pain Perception," *Journal of Clinical Psychology* 14 (1958): 367-74.

[281] Zborowski, "Cultural Components," 16-30.

[282] Sternbach and Tursky, "Ethnic Differences," 241-46; B. Tursky and R. A. Sternbach, "Further Physiological Correlates of Ethnic Differences in Response to Shock," *Psychophysiology* 1 (1967): 151-62.

investigators could produce more meaningful data. After obtaining ambiguous measures of pain perception and reaction thresholds in a series of "normal" subjects, William P. Chapman, research fellow at Harvard Medical School, turned to the study of "psychoneurotics."[283] For Chapman, neurotic subjects would potentially clarify the meaning of variations in perception and reaction thresholds.[284] The pain reactivity of psychoneurotic patients was found to be higher than that of normal patients, which was consistent with contemporary beliefs, but their ability to perceive pain was no different. Thus, it was the choice of subjects that confirmed the pain-reaction to be a psychologically modulated response. Another team, from the Allan Memorial Psychiatric Institute took up the dolorimeter to measure more precisely the difference between psychoneurotic and normal responses to stress. For these researchers, standardised pain stimulation was seen as a means to produce a standard stress situation, and thus to obtain more exact measurements of the characteristic features of psychoneurotics. These measures consisted of physiological processes associated with stress reactions: lymphocyte counts, finger tremor, galvanic skin resistance and the EEG.[285]

Once the correlation between neuroticism and increased reactivity—but not sensitivity—to painful stimulation was established, several groups of researchers explored the use of the pain-reaction test as a means to identify possible psychological disturbances in patients afflicted by chronic painful conditions.[286] One such attempt linked pain reaction measurements to military

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[283] Chapman and Jones, "Variations in Cutaneous," 81-91.

[284] Chapman and Jones, "Variations in Cutaneous," 81-91, the experiment reported in this article shows that both perception and reaction threshold vary in groups of normal subjects. While it is hypothesized that the reaction threshold indicates a psychological processing of the pain sensation, while the perception threshold indicates a purely sensory phenomenon. In the concluding comments, Chapman and Jones suggest that that the psychological reaction would dominate in clinically neurotic individuals, implying that an experiment using neurotic subjects might clarify the psychological meaning of the reaction threshold. It was followed by such a study: Chapman, "Measurements of Pain," 252-57. See also: Chapman et al., "Measurements of Pain Sensitivity," 321-31.

[285] Malmo, Shagass and Davis, "Pain As a Standardized Stimulus," 344. Malmo et al., "Standardized Pain Stimulation," 509-11.

[286] Margaret A. Kennard, "The Responses to Painful Stimuli of Patients With Severe Chronic Painful Conditions," *Journal of Clinical Investigation* 31 (1951): 245-52.

sponsored research investigating the possible psychological aetiology of chronic disorders that were prevalent among soldiers and recruits. One such disorder was neurocirculatory asthenia (N. C. A.), also called effort syndrome and sometimes associated with anxiety neurosis, which not only affected many military men, but was also often found in recruits during draft board medical examinations. A study of N. C. A. was contracted by the OSRD to members of both medical and psychiatric divisions of the Massachusetts General Hospital and Harvard Medical School, under the supervision of Paul D. White and Stanley Cobb.[287] The choice of investigators for this study reflects a concern to study the psychosomatic aspects of N. C. A., and to differentiate between physiological and affective mechanisms. The aetiology of N. C. A. was contested, and some doctors firmly maintained that disorders of the cardiovascular system were responsible for the condition.[288] The Harvard team, however, after measuring the lactic acid content of muscles after training, the capillaries of the nail folds, the scores of patients on the Psychosomatic Experience Test and the MMPI, as well as their reactivity to painful stimulation (an experiment conducted by Chapman), concluded that, in its chronic form, N. C. A. was "not simply [a disorder] of disturbed circulatory reaction and symptoms alone, but is associated with measurable psychological symptoms and abnormal behaviour and difficulty in adjusting to life situations."[289] Thus, in this study, Chapman's method of comparing reactivity and sensitivity to pain was used as a means of distinguishing between the physiological and psychological basis of a pathological state.

Lobotomized individuals were another category from which pain-measurers recruited their subjects. In the early 1940s, the American pioneers of psychosurgery, James W. Watts and Walter Freeman, had begun operating on

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[287] White, et al. "Observations on Neurocirculatory Asthenia," 129-137; W. P. Chapman, M. E. Cohen, S. Cobb, "Measurements Related to Pain in Neurocirculatory Asthenia, Anxiety Neurosis, or Effort Syndrome: Levels of Heat Stimulus Perceived As Painful an Producing Wince and Withdrawal Reactions," *Journal of Clinical Investigation* 25 (1946): 890-896.

[288] I. Starr, "Studies on the Circulation of Draftees Rejected for Neurocirculatory Asthenia," *Transactions of the Association of American Physicians,* 58 (1944): 138-140, claimed that neuroticism was a result rather than a cause of the symptoms experienced by individuals in neurocirculatory asthenia.

[289] P. D. White, S. Cobb, W. P. Chapman, et al. "Observations on Neurocirculatory Asthenia," *Transactions of the Association of American Physicians,* 58 (1944): 129-137.

patients who suffered from intractable pain after noticing that some of their patients, operated on for psychiatric reasons, no longer complained of pain after the operation. [290] When asked directly about their pain, however, patients sometimes admitted that pain was still present, and even that it was undiminished in intensity, but they no longer seemed to be bothered by it. As lobotomies were increasingly performed specifically for the relief of intractable pain, more opportunities arose to observe this phenomenon. The peculiar attitude of the lobotomized was made into an illustrative example of dissociation between the distinct sensory and emotional components of pain.[291] Measuring the "before and after" pain thresholds of individuals who underwent lobotomy was a means of providing quantitative confirmation of this dissociation by showing that the pain reaction was significantly altered, while sensory perception remained unchanged.[292] These measurements also confirmed the personality-dependent nature of the pain reaction threshold. Lobotomized patients were popular subjects not only for pain threshold measurements, but also for a range of psychometric personality assessments that were being developed from the late 1930s and increasingly in the 1940s such as the MMPI and items of what would become known as the Maudsley Personality Inventory.[293] Through these assessments, specific techniques of lobotomy were defined in terms of their personality-altering effects.

These personality assessments were also applied to psychoneurotic patients, and in individuals with various suspected psychosomatic disorders such

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[290] J. W. Watts and W. Freeman, "Frontal Lobotomy in the Treatment of Unbearable Pain," *Proceedings of the Association for Research on Nervous and Mental Disease* 27 (1948): 715-722.
[291] Watts and Freeman, "Frontal Lobotomy," 715-722; Hardy, et al., *Pain Sensations and Reactions*. Barber, "Toward a Theory of Pain,"430-460.
[292] Chapman, et al., "Measurements of Heat," 754-68.W. P. Chapman, H. C. Solomon, A. S. Rose, "Measurements of Motor Withdrawal Reaction in Patients Following Frontal Lobotomy," in *Studies in Lobotomy*, ed. M. Greenblatt, R. Arnot, and H. C. Solomon, 386-392 (New York: Grune & Stratton, 1950); H. E. King, J. Clausen, J. E. Scarff, "Cutaneous Thresholds for Pain Before and After Unilateral Prefrontal Lobotomy," *Journal of Nervous and Mental Disorders* 112 (1950), 93-96; Hardy, et al., *Pain Sensations and Reactions*.
[293] Petrie, *Personality and the Frontal Lobes*, 5-6; A. L. Anderson, "Personality changes following prefrontal lobotomy," *Journal of Consulting Psychology* 13 (1943): 105-107; M. Vidor, "Personality Changes Following Prefrontal Leucotomy As Reflected by the MMPI and the Results of Psychometric Testing,"*Journal of Mental Science* 97 (1951): 159-73.

as N. C. A. and hypochondria.[294] Thus, the psychoneurotic and lobotomized subjects whose pain reactions were measured had not only been literally created by psychiatric techniques of diagnosis and therapy. They had also been constructed as subjects in states of disordered or altered personality by psychometric personality tests, and more generally by psychiatric theory and terminology. The definition of these subjects' personality dimensions made pain reaction measurements meaningful as personality-dependent variables.

2.2.2 Pain and the Measurement of Personality

Personality tests were also used to make more direct correlations between personality characteristics and responses to pain. An approach that elicited a lot of interest in the 50s and 60s was that of Asenath Petrie, who proposed that individuals' pain tolerance, their personal way of experiencing pain in the world, was a product of the perceptual style that was associated with their personality. Petrie's understanding of personality, and of the way in which personality could be broken down into measurable components, was based on the work of psychologist Hans J. Eysenck. In response to wartime demands for techniques to diagnose neurotic disorders, Eysenck, then affiliated with the Maudsley Hospital, a psychiatric hospital in London, had begun searching for psychometric measures of personality.[295] He defined personality as being composed of two principal axes: neuroticism and introversion/extroversion. He then correlated these personality features with the results of tasks, tests or interviews.[296] For example, an extroverted person would complete a particular task with more concern for

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[294] J. C. McKinley, S. R. Hathaway, "A Multiphasic Personality Schedule (Minnesota): A Differential Study of Hypochondriasis," *Journal of Psychology* 10 (1940), 255-268. J. C. McKinley, S. R. Hathaway, "The identification and Measurement of the Psychoneuroses in Medical Practice: Minnesota Multiphasic Personality Inventory," *Journal of the American Medical Association* 122 (1943) 161-167; Asenath Petrie, "Repression and Suggestibility as Related to Temperament," *Journal of Personality* 16 (1948):449 cited in Petrie, *Personality and the Frontal Lobes*, 6, in which Petrie reports on the use of Eysenck's personality tests in a wartime neurosis centre.

[295] Petrie, *Personality and the Frontal Lobes*, 5-6.

[296] H. B. Gibson, *Hans Eysenck: The Man and his Work* (London: Peter Owen, 1981), 118-144, on Eysenck's personality theory. See also: Hans Eysenck, *The Structure of Human Personality* (London: Methuen & Co Ltd, 1953).

speed, while an introverted person would be more concerned with accuracy. Petrie had adopted these tests in studies of patients in a British wartime neurosis centre, and in a study of personality changes in patients who had undergone lobotomy.[297] Her observations on the parallel changes both in personality and in the attitude towards pain of lobotomized patients interested her in pursuing an investigation of this correlation in normal subjects.[298]

Meanwhile, Eysenck had extended and systematized his battery of psychometric tests as the Maudsley Personality Inventory. Petrie adopted a test of susceptibility to satiation, which Eysenck showed to be sensitive to personality differences on the introversion/extraversion axis, as a promising correlate of pain tolerance. A blindfolded subject was instructed to rub a standard, and then a larger, block of wood for a certain amount of time, and then to estimate its size on another piece of wood.[299] Petrie labelled subjects who systematically overestimated the size of the wood block "augmenters" and those who underestimated it "reducers." After sorting subjects out on the basis of their susceptibility to satiation, Petrie tested their tolerance to pain using the Hardy-Wolff-Goodell dolorimeter. Tolerance was defined as the difference between the threshold of pain perception and the maximum time for which a subject could endure painful stimulation. She also tested subjects' tolerance to sensory deprivation by measuring the amount of time they were willing to remain in a specially designed tank. In the late 1950s, Petrie and her colleagues began announcing positive results: "augmenters" were less tolerant to painful stimulation, and more tolerant of sensory deprivation, "reducers" showed the opposite tendency.[300] By the late 60s, she had extended this model of perceptual style measurement to the study of delinquency, alcoholism, hypochondria,

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[297] Petrie, "Repression and Suggestibility;" Petrie, *Personality and the Frontal Lobes.*
[298] Petrie, *Personality and the Frontal Lobes* (1952) Asenath Petrie, *Individuality in Pain and Suffering* (Chicago: University of Chicago Press, 1967), 16-21.
[299] Petrie, Collins, and Solomon, "Pain Sensitivity,"1431-33; Petrie, Collins, and Solomon, "The Tolerance for Pain and for Sensory Deprivation," *American Journal of Psychology* 73 (1960); Petrie, *Individuality in Pain.*
[300] Petrie, Collins, and Solomon, "Pain Sensitivity," 1431-33.

smoking, etc. in addition to individual differences in response to painful stimulation such as in childbirth or in relief obtained from drugs.[301]

Another psychologist, Donald Petrovich, became interested in the 1950s in the question of whether responses to pain were "a unique experience in the psychology of the individual," or associated with personality attributes such as neuroticism and anxiety.[302] Petrovich did not measure pain thresholds. He had instead, as a doctoral student, developed a Pain Apperception Test, which consisted of a standard set of pictures depicting various situations in which pain was anticipated, felt, inflicted by oneself, or inflicted by another, and which subjects were asked to rate according to "painfulness" on numeric scales. The results of this test were correlated with data obtained with other psychometric questionnaires measuring self-reports of physical and psychological complaints associated with neuroticism (measured with the Medical Questionnaire) and of manifest anxiety (measured with the Taylor Manifest Anxiety Scale). The reliability of the correlations was conclusive, confirming the value of the Pain Apperception Test for studying the psychological correlates of pain, as well as the role of personality factors in shaping the response to pain.[303]

Personality assessment tools, in particular the MMPI, began being used more frequently in the 1960s in studies of both psychiatric and medical patients who suffered from unexplained symptoms of pain.[304] The psychiatric examination of patients, including an assessment with the MMPI, was also instituted in the 1960s at the interdisciplinary pain clinic at the University of Washington founded by J. J. Bonica in 1960.[305] Some clinicians recommended its widespread use in medical practice for differentiating between organic and psychogenic disorders,

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[301] Petrie, *Individuality in Pain.*
[302] Petrovich, "The Pain Apperception Test," 367-74.
[303] Petrovich, "The Pain Apperception Test," 367-74.
[304] For example: L. J. Hanvik, "MMPI profiles," 350-3. E. L. Phillips, "Some Psychological Characteristics Associated with Orthopaedic Complaints," *Current Practice in Orthopedic Surgery* 23 (1964):165-76; M. J. Martin, "Tension Headache, a Psychiatric study," *Headache* 6, no.2 (1966): 47-54; L. F. Pilling, T. L. Brannick, W. M. Swenson, "Psychologic characteristics of psychiatric patients having pain as a presenting Symptom," *Canadian Medical Association Journal* 97(1967):387-94.
[305] Bonica Papers, MS C 118, Box 135, Folders 20-25, History & Special Collections Division (HSCD), Louise M. Darling Biomedical Library (Darling Library), UCLA.

many of which were associated with painful symptoms of unknown aetiology. The standardisation of psychological characteristics allowed researchers and clinicians to base their diagnostic judgment on positive findings of personality disturbance rather than using a process of elimination on the basis of negative findings of organic lesions. Both the MMPI and similar questionnaires designed specifically for complaints of pain became more widely used with the expansion of pain clinics in the 1970s.

4.2.3 Early Experience or the Formation of Individual and Collective Psychologies

Researchers in the 1950s and 60s also became interested in studying the relationship between early experience and differential responses to pain. The factor of early experience had long been thought to influence pain responses, but had not been tested empirically. A cluster of studies published in the 1950s and early 60s proposed to fill this gap. While these studies did not explicitly link early experience to specific personality types, they drew on a conception of factors such as upbringing, conditioning, and family structure as psychological forces that shaped patterns of individual and collective behavioural, emotional and cognitive responses. This conception was shared by many contemporary psychologists and anthropologists. In order to correlate pain responses with early experience, investigators faced the challenge of standardising the latter factor.

The solution for one psychologist, Ronald Melzack, was to study reactions to pain in laboratory animals whose upbringing could be directly manipulated. Melzack raised some groups of rats and dogs isolated in "restriction cages," in which they were exposed to minimal sensory stimulation. The rats in the control group were raised in normal cages, where they could interact with each other, and were exposed to electric shocks at early ages. The isolated and control rats were then subjected to a learning test in which electric shocks were used as punishment, and compared on the basis of the number of trials required to learn the task. The control dogs had been raised in a home or in the laboratory, and

exposed to a normal range of stimulation. Their avoidance response to electric shocks, as well as lit matches, pinching, hitting on the head, and piercing of the skin were compared with those of the isolated dogs in a number of tasks. Based on both quantitative data (the number of trials required to learn an avoidance response) and qualitative observations, Melzack concluded that the "behaviour of dogs reared without opportunity for contact with noxious stimuli is grossly abnormal." Using psychological methods of research in laboratory animals, Melzack was able to demonstrate the effect of early experience on the emotional response to pain.[306]

Ethnic categorization, for anthropologist Mark Zborowski, was another meaningful way of creating subjects whose pain responses could be compared on the basis of their early experience. In the early 1950s, Zborowski conducted a study on the "Cultural Components in Attitudes toward Pain," funded by the U. S. Public Health Service.[307] Zborowski proposed to investigate the suggestion, made by Hardy, Wolff and Goodell, that the "conditioning influence" of "culture" shaped collective patterns of reaction to pain. In their understanding of culture, and of its potential effect on psychosomatic processes, both Zborowski and the Cornell researchers, particularly H. G. Wolff, were influenced by "culture and personality" theory that had become popular among American anthropologists from the 1930s.[308] Zborowski shared Margaret Mead's view of upbringing as a

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[306] Melzack, "The Effects of Early Experience."
[307] Zborowski, "Cultural Components in Response," 16-30.
[308] G. W. Stocking, "Essays on Culture and Personality," G. W. Stocking, Jr. (Ed.) *Malinowski, Rivers, Benedict and Others: Essays on Culture and Personality* (Madison: The University of Wisconsin Press, 1986), describes how cultural psychological determinism was adopted as an alternative to Spencerian evolutionary racism as a means of explaining differences in mental characteristics between human groups as a result of the work of Frank Boas in the first decades of the 20th century, and then developed by a new generation of anthropologists who were also influenced by psychoanalytic theories: Ruth Benedict, Margaret Mead, W. H. Rivers, Bronislaw Malinoski, and anthropologically-inclined psychiatrists such as Abram Kardiner. The shift from bio-racial to psycho-cultural based mechanisms to explain human differences in both psychology and anthropology is described more succintly in: P. Voestermans, J. Jansz, "Culture and Ethnicity," in J. Jansz, P. van Drunen (eds.), *A Social History of Psychology* (Malden, MA: Blackwell Publishing, 2004): 182-3.
The interconnections between personality and culture theory in anthropology and psychosomatic research have yet to be explored.

mechanism for the transmission of cultural values and norms.[309] Zborowski also counted among his influences the work of Lawrence Frank, another important player in the rise of 'culture and personality' research, [310] who had demonstrated the profound influence of culture on biological functions and physiological sensations, such as hunger and sexual desire, which were normally labelled "instinctive."[311] In the design of his study, as in his choice of advisors—Frank and Wolff—Zborowski drew on definitions of both pain and culture that were informed by psychosomatic thinking.

Zborowski conducted qualitative ethnographic research in a Veterans Administration Hospital in order to understand the origins of differences in manifest behaviours towards pain in four "ethnic" groups: Jews, Italians, Irish and Old Americans. These groups were selected because they were described by the hospital staff as adopting strikingly different attitudes towards pain. On the basis of interviews conducted with patients on their present and past experiences of pain, Zborowski suggested that explicit child-rearing practices, combined with children's observation of accepted behaviours in their immediate social environment—which could also be drawn from the dominant culture of the host country—profoundly shaped attitudes and behaviours towards pain. Zborowski also distinguished between present and future-oriented concerns about pain to explore ethnic differences. Thus, the seemingly similar vocal and emotional responses to pain of Jews and Italians were in fact rooted in different attitudes; Jews were more anxious about the possible ramifications of painful symptoms (such as disease and death), while Italians were more preoccupied with obtaining immediate symptomatic relief.[312]

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[309] The connection between Zborowski and Mead can be found not only in his text, but also in his biography. Zborowski worked as a research assistant for M. Mead, and she wrote the introduction to his book *People in Pain* published in 1969.

[310] R. Darnell, "Personality and Culture: The Fate of the Sapirian Alternative," and V. Yans-McLaughlin, "Science, Democracy and Ethics: Mobilizing Culture and Personality for World War II," in G. W. Stocking, Jr. (Ed.) *Malinowski, Rivers, Benedict and Others: Essays on Culture and Personality* (Madison: The University of Wisconsin Press, 1986), 163 and 196.

[311] Mark Zborowski, *People in Pain*, (San Francisco: Jossey-Bass, Inc. 1969).

[312] Zborowski, "Cultural Components in Response to Pain,"16-30; *People in Pain.*

While there was little interest among anthropologists in following up on Zborowski's work, psychologists and psychiatrists often quoted his studies as evidence for ethnic and attitudinal differences in responses to pain. One team of psychiatrists from Harvard explicitly drew on Zborowski's findings in designing a study of the influence of attitudinal differences on physiological responses to pain in the mid-1960s. Bernard Tursky and Richard A. Sternbach recruited "housewives" (so, they explained, that the process of transmitting attitudes toward pain could be explored in interviews) from the same ethnic groups as studied by Zborowski, and studied their reactions to electrical shocks. Combining psychophysical methods for producing painful stimulation with methods to record autonomic responses (heart rate, sweating, skin temperature), Tursky and Sternbach found quantitative differences that were compatible with Zborowski's qualitative differentiation between ethnic groups.[313]

Birth order, or family structure, was also identified as a potential influence on pain responses in the early 60s. After one psychologist incidentally found a relationship between reactions to electric shock and birth order that suggested that first-born and only children were less willing or able to withstand pain,[314] a cluster of experiments were designed to test this hypothesis. One study, using a structured interview, found a significant correlation between family size and persistent complaints of pain among neurological patients.[315] However, two other studies did not find significant correlations. One of these used an ultrasonic therapy unit as a heat-producing stimulus in order to measure pain tolerance in

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[313] Sternbach and Tursky, "Ethnic Differences," 241-46; "Further Physiological Correlates," 151-62. In the last article, the authors declared that they were not interested in ethnic, or sub-cultural differences, per se. Rather, they sought to demonstrate the influence of "sets" (implicit attitudes) on psychophysiological activity and autonomic functioning. They presented their results as a warning of the "extraneous" variables that could "distort, or subtly influence or greatly increase the variance of the results in our experimental procedures, often without the experimenter's awareness." These concerns echoed more general concerns expressed during the 1950s and 60s about the control of interpersonal variation in experiments dealing with aspects of psychological and subjective experience.

[314] S. Schacter, *The Psychology of Affiliation* (Stanford: Stanford University Press, 1959).

[315] T. A. Gonda, "The Relation Between Complaints of Persistent Pain and Family Size," *Journal of Neurology, Neurosurgery, and Psychiatry* 25 (1962): 277-81.

female nurses. [316] Another study combined pain threshold measurements using electric current with responses on two questionnaires: items of the MMPI related to pain and a Pain Complaint Scale (PCS).[317] Though the debate on the relationship between family and pain was inconclusive, it shows that this correlation was identified as a potentially meaningful one, while also illustrating the growing use of psychometric measurement techniques to study responses to pain.

Conclusion

The measurement of psychological reactions to pain from the 1940s to the 1960s was not practiced on a large scale, nor was it part of a well-defined investigation of pain mechanisms. Rather, it was characterized by small clusters of experiments on particular topics, which are remarkable in the similarity of their objective: to study correlations between some aspect of subjects' responses to pain and some aspect of their psychological makeup (often called personality). This way of using pain-measuring technologies was quite unlike the function they were given before 1940 and would continue to expand in the 1970s. As often as not, the chief motivation for measuring pain in the 1940s, 50s and 60s was not an interest in the nature of pain, but rather, an interest in the nature of the psychological modulation of physical experiences. In the 1970s, however, pain would become the central focus of such investigations.

If the investigators who ran these studies were not participating in any one project: who were they and what were their objectives? They were graduate students in psychology who began, in the 1950s, wanting to study the emotional modulation of pain responses (Melzack, Petrovich and others), [318] psychiatrists

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[316] S. Gelfand, "The Relationship of Birth Order to Pain Tolerance," *Journal of Clinical Psychology* 19 (1963): 406.
[317] L. G. Collins and L. A. Stone, "Family Structure and Pain Reactivity," *Journal of Clinical Psychology* 22 (1966): 33.
[318] Several other dissertations and theses illustrate this. For example: R. D. Nemoff, "A Study of Pain Sensitivity and its Relationship to Certain Manifestations of Anxiety," (PhD Dissertation: New York University, 1954); James W. Clark, "Factors Affecting Human Response to Pain Stimulation," (MA Thesis: McGill University, 1955).

who wanted to obtain physiological measurements from psychiatric patients (Malmo & Shagass), anthropologists interested in the cultural determination of bodily experience (Zborowski), psychologists seeking to explain the roots of "individuality in suffering" as well as those of alcoholism and juvenile delinquency (Petrie), or psychiatrists interested in identifying attitudinal variables that might affect experimental responses to pain (Tursky and Sternbach).

The important shift in thinking about pain took place starting in 1940, this was stimulated by the discovery of the dolorimeter and of the uniform pain threshold, and influenced by the later work of Hardy, Wolff and Goodell. However, it was principally enabled by important shifts not only in thinking about the correlation between physical experience and psychological processes, but also in the creation of the methods to measure it. These came as a result, in part, of private funding strategies that favoured psychosomatic research in the 1930s, and of new military as well as medical concerns about psychoneurotic disorders growing out from wartime experiences. Unfortunately, there have been only a few serious historical investigations of these phenomena, none of which have focused specifically on the development of research and measurement techniques such as the personality tests that would become so widely used in the post-war decades. My reading of both the primary literature on pain research and the secondary literature on psychosomatic research, psychiatry, psychology and anthropology during this period, suggests that there are important links to be made between, on the one hand, World War II, new funding patterns and a need for new ways of managing inter-individual differences and emotional stress, and, on the other hand, the development of new psychological measuring techniques and new ways of differentiating human experiences, abilities and susceptibilities.

Pain-measuring practices during this period indicate that a process of "psychologisation" of pain took place after 1940. Pain had previously been seen as having both psychological and physiological dimensions. After 1940, however, psychological influences were seen to *define* the experience of pain, to be its chief causal, precipitating and modulating factor, and to act independently of the magnitude, or even absence of physiological stimulation.

What did this psychologisation mean for the objectification of pain? Generally, it implied that it was impossible, in fact that it made no sense, to eliminate or control for the subject's personal thoughts, feelings and interpretations in the measurement of pain. After 1950, it was widely accepted that those emotions and meanings constituted the very experience of pain that was being measured. This was true even for analgesic testing, where the ideal of an emotionally-neutral and impersonal pain had persisted in some quarters throughout the 1940s. But in later decades, human and even animal models of pain came to integrate variables such as fear and anxiety in order to reproduce an "authentic" pain, which would reveal the "true" effects of analgesic therapies.[319]

Technologies for measuring this newly "psychologised" pain produced objectivity in at least two different ways. Some researchers began measuring autonomic indicators (pulse, skin resistance, blood pressure) and automatic behaviours (withdrawing, wincing, finger tremor) that did not require the conscious and cooperative participation of subjects. These measures were arguably more "objective" that earlier algometric measurements that required subjects to make introspective judgments and to express them verbally in the sense that they removed a layer of human interpretation. On the other hand, subjects were not trained or instructed to remain neutral and emotionally detached. Quite the opposite, since investigators were interested in the effects of emotional fluctuations. These methods were thus associated with a very different form of "mechanical objectivity" from that which I described in the previous chapter, in which, ideally, the subject's mind would be "mechanised."

There was a second way of measuring "psychologised" pain from the 1940s onwards through the use of self-rating instruments such as scales and questionnaires. The subjects' full, cooperative and highly personal participation was required to make these tools function. How did researchers make sense of subjects' idiosyncratic interpretations and responses to the questions they were asked? By standardising the questions and obtaining measurements from large

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[319] H. E. Hill, R. E. Belleville, and A. Wikler, "Anxiety Reduction as a Measure of the Analgesic Effectiveness of Drugs," *Science* 120 (1954): 153.

numbers of patients, they were able to identify large-scale trends and correlations, thus determining the validity and meaning of their measurements. The objectivity of the results produced by these tools thus depended on large numbers and procedural standardisation rather than on eliminating patients' personal interpretation of their pain.

These different techniques for measuring the modulation of pain were also associated with different ways of thinking about why and how people varied in their responses to pain. In the late 19th and early 20th centuries, pain-measurers had been measuring the delicacy of subjects' nerves, their quickness to feel sensory stimulation that made them more susceptible to suffering, regardless of whether they suffered in silence or not. After about 1940, they began measuring the influence of personality—whether it was writ small in individual upbringing and past experience or writ large in 'culture'—on subjects' emotional reactivity to pain. As we will see, particularly in chapters 6 and 7, analgesic evaluators became preoccupied with subjects' differential susceptibility to suggestion, but they also associated suggestibility with the emotional status, personality and culture of subjects. While nerve-based sensitivity had been associated with an evolutionary, biologically racialised and gendered classification of people, personality-based reactivity differentiated individuals according to their psychological and cultural experiences. The latter classification was still normative, in that it identified some levels of reactivity as pathological, but it was less hierarchical and rigid than the previous nerve-based model. Perhaps the most important questions that remain to be asked, however, are: Who was favoured and included by specific ways of measuring and classifying pain? And who was disadvantaged and excluded by them?

5. The Rise and Fall of the Hardy-Wolff-Goodell Dolorimeter: Modelling Pain for Analgesic Testing

In 1940, the future was bright for the Hardy-Wolff-Goodell dolorimeter. Hailed as "elegant"[320] and "ingenious,"[321] this new measuring device announced a revolution in the quantification of pain and analgesia. The method had immediate appeal: it seemed easy to use, efficient and inexpensive; it offered results that were precise and astonishingly stable. As measured by the dolorimeter, the human pain threshold was uniform, no matter who the subject, no matter what their mood. Never had human pain manifested itself in such regular patterns.

This astonishing news was particularly welcome in the field of analgesic testing. For some years, researchers had been looking for a way to quantify the relief of pain, a symptom that remained stubbornly erratic, vague and unpredictable in humans. Over the next decade, researchers in laboratories across the United States took up the promise of the dolorimeter. By 1950, all signs pointed to its success: The apparatus had travelled to Britain and Canada. Over 20 research teams had published data produced with a dolorimeter.[322] Students at Johns Hopkins and Cornell medical schools experimented with the method in their physiology and neurology classes.[323] It could be purchased as a two-piece

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[320] G. Woolfe and A. D. Macdonald, "The Evaluation of the Analgesic Action of Pethidine Hydrochloride (Demerol)," *Journal of Pharmacology* 80 (1944): 300-307.

[321] Lyndon E. Lee, "Medication in the Control of Pain in Terminal Cancer, With Reference to the Study of Newer Synthetic Analgesics," *Journal of the American Medical Association* 116 (1941): 216-20.

[322] This approximate number was calculated from my literature review and the bibliography provided by A. H. Kutscher and H. W. Kutscher, "Evaluation of the Hardy-Wolff-Goodell Pain Threshold Apparatus and Technique: Review of the Literature," *International Record of Medicine* (April 1957), 228-230.

[323] Harold G. Wolff and Henry S. Dunning, "The Teaching of Clinical Neurology," *Journal of the Association of American Medical Colleges* 22 (1947), 263-273, describe the use of the dolorimeter at Cornell Medical School. R. A. Kuhn and R. B. Bromiley, "Human Pain Thresholds Determined by the Radiant Heat Technique and the Effect Upon Them of Acetylsalicylic Acid, Morphine Sulfate, and Sodium Phenobarbitol," *Journal of* Pharmacology 101 (1951): 47-55, this study was "undertaken when medical students using the HWG pain threshold apparatus during their laboratory course in physiology were unable to reproduce phenomena that have been described and widely quoted."

apparatus from the Experimental Engineering Corporation, in Bergenfield, NJ.[324] The commercialisation of the instrument seemed, in particular, to indicate a stabilization of the technique and expectations that it would remain popular.[325]

By 1953, however, signs of the dolorimeter's decline were already apparent. Donald Williamson, president of the Co-Design Corporation, complained about their recently redesigned and marketed dolorimeter: "Frankly, the volume on this instrument has been disappointing." [326] Concerned by the low sales of the instrument, Williamson wrote to a selected group of researchers interested in pain-measurement to find out what they thought: Why did no one buy the dolorimeter when such a strong demand had been predicted for methods of pain-measurement? Was there a flaw in its design, or in the company's marketing campaign? The disenchantment with the dolorimeter apparently ran deeper. In the pages of *Science*, that same year, the dolorimetric method's validity was under attack.[327] In laboratories too it seemed to be falling into disuse: after 1950, the number of articles reporting results obtained with the dolorimeter for analgesic evaluation had levelled off.[328]

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[324] L. C. Miller, "A Critique of Analgesic Testing Methods," *Annals of the New York Academy of Science* 51 (1948): 34-50, provides this information. The Co-Design Corporation would also market the dolorimeter a few years later: Sales pamphlet for the Hardy-Wolff-Goodell Dolorimeter, Co-Design Corporation, Winchester, MA, n.d., Wolff Papers, Box 3, Folder 12, Medical Center Archives of NewYork-Presbyterian/Weill Cornell. This pamphlet can also be found in: Travell Papers, Box 19, Folder 30, Special Collections and University Archives, The George Washington University (GWU).

[325] Miller, "A Critique of Analgesic Testing," 34-50, points this out in his review of analgesic testing methods.

[326] Donald E. Williamson to Janet Travell, June 16, 1953, Travell Papers, Box 19, Folder 30, Special Collections and University Archives, GWU.

[327] Henry K. Beecher, "Experimental Pharmacology and the Measurement of Subjective Response," *Science* 116 (1952): 157-62; James D. Hardy, Harold G. Wolff and Helen Goodell, "Pain - Controlled and Uncontrolled," *Science* 117 (1953): 164-67; Beecher, "Pain- Controlled and Uncontrolled. Rejoinder to Dr. Hardy, Dr. Wolff and Miss Goodell," *Science* 117 (1953): 164-67.

[328] This trend is suggested in the number of publications reporting results using the dolorimeter in my own literature review and confirmed by the references enumerated in the bibliography provided by Kutscher and Kutscher, "Evaluation of the Hardy-Wolff-Goodell," 228-230. It is of course possible that the dolorimeter was still in use after 1950 but that results were rarely published. For example, in 1953, one group reported that they still used the dolorimeter, but only as a test preliminary to clinical studies. See S. C. Cullen and E. G. Gross, "Analgesic Testing Methods," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 626. Nevertheless, it seems unlikely that the method was widely used.

After little over a decade of acclaim, the validity of the dolorimeter was disputed and its utility largely rejected in the field of analgesic evaluation. What, apparently so unexpectedly, had changed? The most obvious sign of change was the emergence of a new technology for testing analgesic efficacy: the analgesic clinical trial. By the early 1950s, sponsorship for analgesic testing was mainly directed towards the use of this new technology. Not surprisingly, the strongest advocates of the analgesic clinical trial—of whom the most vocal was Henry K. Beecher—were the harshest critics of the dolorimeter.

Advocates of the clinical trial objected to the dolorimeter on both technical and theoretical grounds. Its results were not replicable, Beecher claimed, while those of the clinical trial were more robust. The dolorimetric method did not fulfil essential conditions to safeguard against sources of bias, conditions that were at the core of the clinical trial design. The dolorimeter evaluated analgesics on an "artificial" pain that was induced in the laboratory, while the clinical trial measured analgesic effects on the more realistic "natural" pain that occurred in the clinic.[329] But why did this criticism become persuasive in the early 1950s? Why had the dolorimeter been so appealing as an analgesic test only a few years earlier?

In this chapter, I will argue that the displacement of the dolorimeter can only be understood by paying close attention to transformations in the social and material conditions under which analgesics were evaluated from the late 1930s to the early 1950s. Who used analgesic testing technologies? What resources were made available to them? What kinds of investments, collaborations and compromises did it take to make these technologies work? What was expected of these technologies? These, I will argue, are key questions for explaining how the validity and value of analgesic testing technologies were defined and appreciated.

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[329] Beecher attacked the dolorimeter in many of his writings. The earliest seems to have been in Beecher, "Experimental Pharmacology," 157-62, while the most detailed formulation of his arguments can be found in Beecher, *Measurement of Subjective Responses: Quantitative Effects of Drugs* (New York: Oxford University Press, 1959). Beecher and his colleagues also wrote often about the value of clinical, pathological or naturally occurring pain. See particularly: Arthur S. Keats, Henry K. Beecher, and Frederic C. Mosteller, "Measurement of Pathological Pain in Distinction to Experimental Pain," *Journal of Experimental Physiology* 3, no. 1 (1950): 35.

My intention is not to write off the importance of either technical or theoretical considerations in comparing the value attributed to these two methods. I do, however, suggest that we need to look beyond the technical and theoretical arguments that were used to compare these methods explicitly, and pay attention to the ways in which the technical capacities of these instruments interacted with the social and material conditions under which they were operated.

In the late 1930s and 1940s, the demand for information about analgesic efficacy and the resources that were made available for analgesic testing created conditions under which the dolorimeter made sense. Running successful clinical trials required human and material resources that had not been made available for analgesic evaluation before the later 1940s. At that time, however, new investments—monetary, but also professional and institutional—made it possible to create, promote and support the analgesic clinical trial as a valid and valuable technology. The dolorimeter's displacement appears to be tightly bound to the emergence of the clinical trial as an effective strategy for analgesic assessment.

My approach has allowed me to link debates about the validity and utility of analgesic testing methods to the broader dynamics of American analgesic innovation and therapeutic evaluation during this period. In particular, I will examine the role of key actors who contributed funding, time and expertise to analgesic evaluation: various professional groups of experimenters (pharmacologists, physiologists, anaesthesiologists, clinical pharmacologists); different types of observers (nurses, technicians, physicians, nurse-observers), of subjects (laboratory animals, hospital patients, medical students, self-experimenters, prisoners), as well as sponsors (pharmaceutical firms, public institutions, philanthropies). The differences between methods of analgesic evaluation were not just theoretical. These different tools required different kinds of resources to work, and were implemented under conditions that were dictated by fluctuating commercial, medical and regulatory interests in analgesics, as well as by the larger professional and institutional organization of medical research and of therapeutic evaluation.

I will begin this chapter by comparing the two methods, describing how the dolorimeter and the analgesic clinical trial offered very different strategies for managing variability and imprecision in order to create a measurable pain. Each of these models thus proposed a different form of control, and suggested different ideas of what needed to be controlled, in order to measure analgesic efficacy. Paying attention to pain-measurers' own arguments about the advantages and drawbacks of each method, I will point out how each model of measurement relied on a specific collection of expertise, instruments, authority, material, subjects, practices and other financial, spatial and organisational resources.

Which resources were available to experimenters was contingent on the broader professional, political and institutional framework of analgesic evaluation. To explain the initial appeal of the dolorimeter, and its later displacement, I will then fit the trajectory of the dolorimeter into the history of American interests in analgesic research. Who showed an interest in the dolorimeter? What kinds of problems was it called in to solve? An expansion in American analgesic innovation, which began in the 1930s, created a gap in analgesic testing methods that the dolorimeter proposed to occupy.

In a third section, I will clarify the nature of this gap, which was not only a conceptual, but also a material, professional and financial one. I will then visit the dolorimeter at work. Focusing on the controversial issue of subject-training, I will examine debates about the proper use of the dolorimeter. Several researchers in the 1940s were willing to make certain concessions and adaptations to make the dolorimeter work. Others, most notably Beecher, rejected these conditions as inappropriate.

Beecher instead proposed a different way of making subjects, and their statements about pain, that were suitable for measuring analgesic effect. In the fourth part of this chapter, I will describe the shifts in the organisation and funding of analgesic evaluation that made Beecher's clinical trial feasible in the late 1940s and his arguments more convincing by the early 1950s.

In conclusion, I will discuss how each model of analgesic evaluation enabled a different conception of pain: what it was, how subjects experienced and

interacted with it, how it travelled through minds, bodies and environments and what influenced its trajectory. I will argue that beyond the surface of a debate about pain's authentic nature and intelligibility was a contest between different practices for the experimental management of its variability.

5.1 Two Sources of Precision, Two Models of Control: High Volume *versus* High Resolution.

What exactly was the dolorimeter? In 1940, two articles by neurologist Harold G. Wolff, physiologist James Hardy and a research assistant, Helen Goodell, of Cornell Medical School appeared back to back in the *Journal of Clinical Investigation.*[330] The first offered a general description of an experimental set-up for measuring pain thresholds in human subjects. Essentially, it consisted of a strong beam of light from 1000 watt bulb that was focused through a fixed aperture onto a small area of a person's forehead that had been blackened with China ink. This light, of variable intensity, was delivered in exposures timed to exactly 3 seconds by an automatic shutter. The subjects' task was simple, a task it seemed almost anyone could perform: To identify the exposure that produced in them a sensation described as: "heat finally 'swelling' to a distinct, sharp stab of pain at the end."[331] What was remarkable about this sensation is that it was "easily recognizable, even by untrained subjects"[332] and that it was consistently identified at the same level of intensity by all subjects, at all times. This sensation was the subject's pain perception threshold. Through a correlation of the moment

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[330] Marcia L. Meldrum, "'Departures From the Design': The Randomized Clinical Trial in Historical Context, 1946-1970," (PhD dissertation, State University of New York, Department of History, 1994), 282-283, n.236, writes : "James Hardy received his PhD at Johns Hopkins in 1930 in physics, but began working shortly after on physiological problems at the Russell Sage Institute; he joined the Cornell faculty in 1941. His research interests turned to aviation medicine in the 1950s. Harold G. Wolff was professor of neurology at Cornell. Helen Goodell was a research associate in the department of medicine; as happens too frequently with women scientists, I have been unable to learn much more about her." It might be added, however, that Goodell worked closely with Wolff on a variety of projects and that they published fairly extensively together. For example : Harold G. Wolff, Stewart Wolf, and Helen Goodell, *Stress and Disease* (Springfield, Ill: Thomas, 1968).

[331] Hardy, Wolff, and Goodell, "Studies on Pain. A New Method for Measuring Pain Threshold: Observations on Spatial Summation of Pain," *Journal of Clinical Investigation* 19 (1940):649-657.

[332] Hardy, Wolff and Goodell, "Studies on Pain. A New Method," 650.

of sensation to the intensity of the exposure at which it had appeared, the threshold was a quantifiable entity. The job of the experimenter was to keep the machine calibrated, to vary the intensity of each exposure by means of a rheostat and, when the threshold level had been confidently identified by the subject, to read off the measurement indicated by the dial of a radiometer.

The second of these articles proposed the dolorimeter as a solution to the problems that had been plaguing attempts to evaluate analgesics in humans: vagueness, subjectivity, variability.[333] As I have described in chapter 3, some attempts were made in the 1910s to replace clinicians' observations of patients' responses to painkillers with algometric measurements of changes in the pain threshold. [334] At that time, however, there was little real interest in making analgesic drug evaluation more objective. By 1940, analgesic testing had taken on new significance as a result of a large investment in analgesic innovation. New attempts were made to adapt algometric methods for pharmacological purposes. A most difficult problem remained, however. The data produced by these methods was highly variable, both between and within individual subjects. Analgesic drugs were just one source of influence on pain that had to be isolated from other factors that were known to make its intensity vary: personality, sensitivity, attention, mood, emotion, and so on.[335] The problem was to distinguish therapeutic effect from these other causes of variation. The baseline pain threshold produced by the Hardy-Wolff-Goodell dolorimeter was reported to be remarkably constant, impervious to changes in mood or personality, but sensitive to drug effects. It was the perfect medium for human drug assays: standardised, purified, stable and amenable to quantitative measurement.

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[333] Wolff, Hardy and Goodell, "Studies on Pain. Measurement of the Effect of Morphine, Codeine and Other Opiates on the Pain Threshold and an Analysis of Their Relation to the Pain Experience," *Journal of Clinical Investigation* 19 (1940): 659-80.

[334] For example, D. I. Macht, N. B. Herman and C. S. Levy, "A Quantitative Study of the Analgesia Produced by Opium Alkaloids, Individually and in Combination With Each Other, in Normal Man," *Journal of Pharmacology* 8 (1916): 1-37.

[335] In the few discussions of attempts to evaluate analgesics before 1940, variation between measurements was usually cited as the main obstacle to the objective evaluation of this class of drugs.

While Hardy, Wolff and Goodell were fiddling with radiometers, rheostats and potentiometers, the Harvard anaesthesiologist Henry K. Beecher apparently contented himself with pencils and a notebook for the measurement of analgesic potency. In 1953, Beecher wrote of his analgesic clinical trial design, while not too subtly attacking the dolorimeter:

> We are concerned incidentally, of course, with simplicity. A method that can function with no apparatus other than a notebook and a pencil is manifestly more desirable and more broadly useful, other things being equal, than one that requires complex and delicate apparatus which needs calibration by a well-trained physicist.[336]

In addition, Beecher chose as his experimental medium the more "natural" pathological pain of real hospital patients, while he portrayed Hardy, Wolff and Goodell's stimulated pain as highly contrived. Hardy, Wolff and Goodell measured each individual's pain threshold as the intensity of radiation specified in hundreds of millicalories per square centimetre per second, and the effect of analgesics was calculated as percentage changes between normal and drug-induced readings. Beecher's experimental subjects, on the other hand, were asked to estimate their relief as "none, slight, moderate or complete."[337] Later this question was refined, and patients could choose between only two options: Was their pain significantly relieved, more than 50%? Or was it not?

In the basic model of the analgesic clinical trial, as designed by Beecher and various colleagues, subjects were recruited from the surgical wards of a major teaching hospital. They were given, for their post-operative pain, a randomly chosen analgesic drug, which could be a standard drug (usually morphine), a test drug, or a placebo (usually saline). After his initial trials in the late 1940s, Beecher introduced a new experimental device. Instead of alternating doses in different patients, the different drugs –standard, test and placebo- would be administered in a sequence in the same patient. Therefore, test substances could be compared on the basis of how they affected each patient, using, in Marcia

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[336] Beecher, "Experimental Pharmacology," 160.
[337] Jane E. Denton and Henry K. Beecher, "New Analgesics. I. Methods in the Clinical Evaluation of New Analgesics," *Journal of the American Medical Association* 141 (1949): 1051-57.

Meldrum's words, each patient as their "own control."[338] At regular intervals after each dose was administered, a technician or observer came by to interrogate subjects, using standard questions, about how much pain they had, or how well their pain had been relieved. Neither subjects nor observers were informed of the nature of the drug that was administered. Data collected by observers was then compiled and analysed.

We should not be fooled by the appearance of simplicity that Beecher gave his method. Beecher's method, no less than Hardy, Wolff and Goodell's, intervened to manage sources of variability and to create a measurable pain for the evaluation of analgesic efficacy. The complexity, control and precision of the analgesic clinical trial lay elsewhere: in the creation of the optimal conditions for the collection of data, and in the coordination of the work of numerous collaborators; observers, consultants and subjects.

How did each method manage the variability of experiences and expressions of pain? [339] The dolorimeter eliminated it. Dolorimetric subjects were "all brothers under the skin," as *Newsweek* had announced. "Tom, Dick and Harry, all ages, all races, if they are alert and attentive, exhibit the same pain threshold, i.e., the intensity of stimulation at which pain is first felt under fixed circumstances is uniform," explained Harold Wolff at a session on "the Psychologic Aspects of the Treatment of Pain" of the Cornell Conferences on Therapy.[340] As we saw in chapters 3 and 4, differences in pain thresholds had previously been explained in terms of differences in nervous sensitivity. According to Hardy, Wolff and Goodell, the dolorimetric method was successful in producing a uniform threshold because it split the perceptive and reactive elements of pain. In the isolated perception of pain, those factors that made

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[338] Marcia Meldrum, "Each Patient His Own Control: James Hardy and Henry Beecher on the Problem of Pain Measurement," *American Pain Society Bulletin* 9, n. 1 (1999): http://www.ampainsoc.org/pub/bulletin/jan99/history.htm (accessed 9/29/2003).

[339] An insightful comparison of the dolorimeter and Beecher's analgesic clinical trial design can be found in Meldrum, "Each Patient His Own Control." The comparison I offer in this chapter elaborates on Meldrum's work, in particular through an examination of the social and material conditions of the practice of analgesic testing.

[340] Harold G. Wolff, "Cornell Conference on Therapy: Psychologic Aspects of Treatment of Pain," *New York State Medical Journal* 45 (1945): 1003-9.

responses to pain vary—that is, the confusing and inconvenient aspects of individuals' interpretations of their experience, and of their personalities, moods, memories and expectations—were eliminated. The dolorimetric experience allowed subjects to maintain a neutral attitude towards the beam that was heating their foreheads; it produced a pain that was precise, unthreatening, and temporary. This pain was "free of suffering," a sensory experience in which "the implications of pain is not a major factor."[341]

Hardy, Wolff and Goodell recognised that this pain was only a fragment of the "whole pain experience" and that its responsiveness to analgesic drugs represented only a part of the whole experience of analgesia. This is precisely what made it valuable. This pain could be isolated, manipulated, and quantified. In response to Beecher's criticism, Hardy, Wolff and Goodell pointed out: "it is obvious that pain sensation itself is only a part of this constellation [the components of pain] and bears somewhat the same relation to it that vision does to graphic art. It would be only the extreme aesthete who would insist upon limiting the study of vision to the art gallery."[342]

The dolorimeter thus standardised the painful stimulation, but also, by extension, the experience of pain, and even the experiencing subject. By ensuring the accurate quantification and calibration of the stimulus, and by maximising the distinctiveness of the sensation to be identified, the dolorimeter provided each subject with optimal conditions for making an exact, reproducible judgment. In theory, the quality of the sensation itself ensured this standardisation. In practice, users of the dolorimeter found that subjects had to acquire the capacity to discriminate the endpoint consistently and with emotional detachment through a period of "familiarisation" or "training." Hence, the dolorimeter itself standardised the stimulus and the conditions of stimulation, but it was through its "proper use" that subjects' ability to perceive it was standardised. In any case, the control exercised by the dolorimetric method targeted the individual experience of pain.

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[341]Hardy, Wolff and Goodell, "Pain—Controlled and Uncontrolled," 165.
[342] Hardy, Wolff and Goodell, "Pain—Controlled and Uncontrolled," 165.

By contrast, Beecher's method of the clinical trial exercised little control at the level of individual subjects or their pains. The source of pain was a surgical wound. Similar wounds, Beecher insisted, did not necessarily give similar pains.[343] Protocols for the trials did not specify any means of standardising subjects themselves, beyond selecting postoperative patients for intelligence and cooperativeness. We do know that, in practice, further efforts were made to make patient samples more uniform. For example, Beecher reportedly favoured male subjects because "the menstrual cycle requires troublesome controls."[344] Two other techniques of standardisation were described explicitly in research reports. For some years Beecher maintained that "placebo-reactors" should be screened out because they "diluted" the data.[345] In addition, Beecher's novel technique of using each subject "as their own control" allowed some variability to be eliminated. If patients were matched against themselves, their idiosyncratic judgments about pain intensity could be cancelled out. However, both these techniques—eliminating placebo responders and using subjects as their own controls—were measures of economy rather than devices essential to the functioning of the method. They were useful, but, it was recognised, if sufficiently large series of subjects were employed, they were unnecessary.

Abundance, rather than a capacity to make certain types of judgments, was the main virtue of Beecher's experimental subjects. "The concept of group effect will perhaps always be necessary in dealing with general problems of pain and other subjective ailments," wrote Beecher in 1953.[346] Beecher's method measured this group effect, and not the pain relief of individuals. While individual

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[343] Keats, Beecher, and Mosteller, "Measurement of Pathological," 35.
Beecher had formulated this idea earlier, but it was only in the later article that he discussed its implications for analgesic testing. See: "Pain in Men Wounded in Battle," *Bulletin of United States Army Medical Department* 5 (1946): 445-54.

[344] Beecher, "Experimental Pharmacology and Measurement of the Subjective Response," 157-162.

[345] Louis Lasagna, Frederic Mosteller, John M. von Felsinger, and Henry K. Beecher, "A Study of the Placebo Response," *American Journal of Medicine* 16 (1954): 770-779, determining a technique to screen out placebo responders from analgesic clinical trials was cited as one of the rationales for this study. However, later results showed that it was very difficult, if not impossible, to identify those who would respond to placebos consistently.

[346] Henry K. Beecher, "A Method for Quantifying the Intensity of Pain," *Science* 118 (1953): 322-24.

testimonies of pain and relief could be expressed in number form, they were qualitative judgments rather than quantitative ones. The calculation of the efficacy of a test drug was made on the basis of how many patients it relieved. Large numbers also flattened out variability: "When the series is large... one can control suggestion, inherent or implied, the presence of the investigator, practice effect, learning, motivation, interest, the subject's anticipation of an unknown medication, his drug history...," adding that "sufficient numbers" would also "cancel out normal mood swings, above and below par."[347]

The validity of pain-measurements for analgesic evaluation thus depended on their quantity, but it also depended on the conditions under which measurements were made. Observers had to be consistent and neutral. They used pre-determined questions to interrogate subjects. The expectations of both subjects and observers concerning fluctuations in pain had to be avoided and eliminated through "blinding," that is, ignorance of the nature of the dose administered to the patient. Finally, the validity of these numbers was ensured through statistical verifications and interventions. Implementing these conditions and controls required coordinated collective action.

Hardy, Wolff and Goodell had not paid much attention to the role of the observer who measured out pain thresholds—the real observer was understood to be the subject herself—nor did they emphasize details of their relatively uncomplicated statistical analyses. Instead, they calibrated both the pain and its perceiver through the instrumental production of pain and, by extension, of sensory judgment. Beecher used simple interrogation, "fairly primitive questions" as one of his colleagues would later say, to obtain data about relief, and did not attempt to standardise the source of pain.[348] He relied, however, on a whole team of observers, subjects and consultants to collect and manipulate information under appropriate conditions, and on a sufficiently large scale.

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[347] Beecher, "Experimental Pharmacology," 160.
[348] Marcia Meldrum, "Oral History Interview with Louis Lasagna," 8 September 1995, MS C 127.19, John C. Liebeskind History of Pain Collection, HSCD, Darling Library, UCLA, 6.

Each method offered a different model for pain-measurement and analgesic evaluation that depended on a different scale and target of control. Indeed, control was a central issue in the debate between the Harvard and Cornell teams, a debate in which each team accused the other of inadequately controlling the experimental evaluation of analgesic effect. Part of this debate was published as two open letters in the "comments and communications" section of the journal *Science,* which were aptly titled "Pain—Controlled and Uncontrolled." Hardy, Wolff and Goodell claimed that pain had previously eluded precise measurement because clinicians and researchers had failed to recognise the difference between the sensation of pain and a reaction to it. Only by isolating the sensory aspects of pain could its measurement be objective. This could only be done under specific conditions, as Hardy, Wolff and Goodell insisted: "pain *sensation* is far more difficult to investigate when an individual is extremely frightened, inattentive, obtunded, prostrated, "sick" or exhausted."[349]

Beecher, on the other hand, saw the experienced subject as the one who was, experimentally speaking, out of control: "I doubt if they can achieve [the double-blind experiment] with their highly trained, drug-wise subjects... Subjects who know how to recognize the subjective sensations of analgesics and who have an interest in the outcome cannot be considered as unbiased."[350] Beecher admitted the utility of distinguishing between pain sensations from reactions, but he denied that the dolorimeter had succeeded in doing so, and furthermore, doubted that this experimental separation was possible. His later writings affirmed more confidently that pain experience was indivisible, and that the concepts of sensation and reaction were only theoretical. The inconsistencies in data obtained with the dolorimeter by different investigators showed that some "reaction" leaked into the experience. No human experience of pain was immune from the experiencing subject. By failing to acknowledge this, dolorimetric experimenters were, according to Beecher, turning a blind eye to the necessary controls and precautions.

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[349] Hardy, Wolff and Goodell, "Pain—Controlled and Uncontrolled," 165..
[350] Beecher, "Pain- Controlled and Uncontrolled," 167.

Thus, each team pointed to how the other allowed their measurements to be contaminated by "bad" subjectivity: bias, suggestion, emotion, expectations. The Cornell and Harvard teams also accused each other of sacrificing elements of a useful kind of subjectivity. For Beecher, a pain worth measuring was an authentic, whole experience of pain. For the Cornell team, the subject's sensory judgment could be honed and guided to be made more precise, neutral and accurate with the dolorimetric method, but this was impossible if subjects were not in a condition to exercise their judgment properly.

Why did Beecher's model of control become so widely persuasive in the early 1950s? There are several possible explanations. It might be suggested that Beecher's model was self-evidently superior in that it produced more stable results without resorting to the "tricks" employed by users of the dolorimeter, such as using trained, un-blinded and drug-wise subjects. Just a few years earlier, however, the dolorimeter had been recognised to work quite well under appropriate conditions, and to present many advantages over clinical trials. In addition, Beecher's clinical trial was expensive, slow and also required work and specific conditions to produce good results.

It might also be suggested that Hardy, Wolff and Goodell's "pure" pain perception was anachronistic, while Beecher's indivisible, emotional and idiosyncratic pain was more persuasive and authentic given the process of "psychologisation" of pain I described in chapter 4. To a certain extent, this is a useful explanation. However, I would argue that, because analgesic testing methods were in demand, a new way of controlling pain had to emerge before the old one no longer made sense. The distinction between "clinical" and "experimental" pain had preceded the Beecher *versus* Hardy, Wolff and Goodell debate.[351] However, only experimental pain had been seen as appropriate for providing objective and precise information about analgesic efficacy. Beecher was able to argue for the superiority of clinical pain as a medium for analgesic

[351] For example: Robert C. Batterman, "The Clinical Aspects of Evaluating Analgesic Agents," *Yale Journal of Biology and Medicine* 18 (1946): 595-607.

testing when he was also able to argue that it could be brought under experimental control.

I suggest that a crucial distinction between these two methods needs to be made on the basis of what kinds of resources and conditions were required to make each of them *work*, and what type of information each of these methods offered. It will then be possible to explain how transformations in the demand, and resources made available, for analgesic testing in the late 1940s underlay the shift from the dolorimeter to the analgesic clinical trial, and thus to re-evaluate the terms of the debate between Beecher and Hardy, Wolff and Goodell.

What did it take to make each method work? The method of Hardy, Wolff and Goodell depended on a precise and mechanical source of stimulation, which produced a stimulus that was quantifiable and easily discriminated. This source was an instrument that was relatively cheap, but which needed to be constructed and used identically by each team that hoped to obtain comparable results. The best way of using this instrument seemed to be through the recruitment of a small number of reliable subjects who could be trained, and thus were available for repeated trials over a period of time. The instrument could be used in a fairly small, quiet space, ideally some sort of laboratory. It could be used by anyone who had the technical expertise to set it up and calibrate it, access to the right type of subjects, and who could afford to buy the instrument.

For all Beecher's talk of simplicity, the analgesic clinical trial was more expensive, time-consuming and difficult to coordinate. It required full-time observers, assistants and statistical experts who had to be paid salaries and consultant fees. It depended on a steady, plentiful supply of patient "material." The investigator needed access to a clinical facility and had to have sufficient clinical authority to recruit patient-subjects, oversee their care, and coordinate the clinical activities that entered into the operation of the clinical trial.

What did these different requirements mean in terms of choosing an analgesic testing method? I will answer this question by reviewing the history of analgesic innovation and evaluation over the course of almost two decades using the following question: Who wanted analgesic efficacy to be measured, and for

what? By whom, and with what, were analgesic-testing technologies operated? On the basis of these questions, I have divided the early history of American analgesic evaluation into three waves. From the mid-1930s to about 1940, a search for new, more precise means of measuring analgesic efficacy emerged from a systematic research program to develop a non-addictive analgesic. The initial appeal of the dolorimeter, when it was introduced in 1940, was defined in terms of the requirements formulated within this search. A second wave came with the introduction into the U. S. of German synthetic analgesics in the early 1940s. Pharmaceutical firms were very interested in these new compounds. The dolorimeter was useful for laboratory scientists who tested these analgesics using modest grants from industry. By the late 1940s, the evaluation of the new synthetics began to attract larger investments, first through the army and then through a consolidation of industry funding. This represented a third wave of analgesic evaluation, which was characterised by more concentrated sponsorship of analgesic testing, as well as by the new involvement of clinicians in operating analgesic-testing technologies.

5.2 The Appeal of the Dolorimeter and the Quest for a Non-narcotic Analgesic

The precision and consistency of dolorimetric data took on value within the context of an American expansion in analgesic research that had begun in the early 1930s. The search for better methods of analgesic evaluation emerged out of another quest: to find an analgesic capable of relieving pain without causing addiction. Such a program had been launched by the Committee on Drug Addiction, under the auspices of the National Research Council, in 1929.[352] By

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[352] The history of this program has been analysed by Caroline Acker. See Caroline J. Acker, "Addiction and the Laboratory: The Work of the National Research Council's Committee on Drug Addiction, 1928-1939," *Isis* 86 (1995): 167-93. And the chapter titled "The Technological Fix: The Search for a Nonaddicting Analgesic," in Caroline J. Acker, *Creating the American Junkie: Addiction Research in the Classic Era of Narcotic Control* (Baltimore: Johns Hopkins University Press, 2002), 62-97. Useful details about areas of research, funding and participants in the program can also be found in Nathan B. Eddy, *The National Research Council Involvement in the Opiate Problem, 1928-1971* (Washington, D. C. National Academies of Science, 1973).

the early 1930s, the program had begun generating new substances that needed to be evaluated and a demand for methods to assess pain relief in humans and animals. The program also created a framework—a set of objectives, a social network, specific financial and human resources—that determined what kind of testing was required, and what kind of testing would be feasible. When the dolorimeter was introduced in 1939-1940, it was exactly what members of the Committee on Drug Addiction had been looking for.

The origins of this quest for a non-addictive painkiller can be traced to a growing conviction, in the 1920s, that pharmacological innovation was a promising solution to the American "drug problem." As Caroline Acker has shown, a network of influential groups subscribed to this idea, and rallied around the goal of constructing an American opiate research program. The quest for a non-addictive analgesic became a "boundary object" for these various groups, a common objective towards which they each invested, with the expectation of different returns.[353] Elite American pharmacologists, for example, aspired to the expansion of drug research along the lines of the German model. They were instrumental in persuading the Committee on Drug Addiction (CDA) to create a program of analgesic innovation. For representatives of the American Medical Association (AMA), and many of its members, a safe analgesic would relieve the medical profession of its responsibility for the high rate of iatrogenic addiction, which had been reported to be alarmingly high.[354] If successful, such a project would also offer greater control over drug supply and distribution, and fit in with the increasing support for the criminalisation of drug addiction in public health and drug enforcement circles. Through their continuing links with Committee, each interest group contributed resources to the accomplishment of the project: academic links, publications, confiscated drugs, research facilities, manufacturing

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[353] Acker, *Creating the American Junkie,* 65. Acker borrows the term "boundary object" from sociologists Susan Leigh Star and James Griesemer.

[354] Acker argues that efforts by physicians to control opiate use were part of a broader effort to reform American medicine through self-regulation. See Acker, "From All Purpose Anodyne to Marker of Deviance: Physicians' Attitudes Towads Opiates in the U. S. From 1890 to 1940," in *Drugs and Narcotics in History,* eds. Roy Porter and MikulasTeich, 114-32 (Cambridge: Cambridge University Press, 1995).

of substances, etc. Funding was provided by the Rockefeller Foundation, while the National Research Council provided the Committee with an institutional home and the coordination of its research activities.

The objectives and resources contributed by the constituents who had a stake in opiate research shaped the early development of analgesic testing methods. The general objective of the program was to design a drug that targeted pain selectively, and thus was driven by the expectation that the desirable therapeutic properties of opiates could be chemically dissociated from their unwanted side effects, mainly addictiveness. To achieve this chemical dissociation, it would also be necessary to differentiate, and measure, the various pharmacological effects of opiate substances in living organisms, particularly pain and addictiveness. In other words, the success of the program depended on finding a means to isolate pain as an experimental variable.

The evaluation of analgesic potency also had to fit within the organisation of the Committee's activities. The Committee's analgesic program was conceived to coordinate and harmonize chemical and pharmacological work so that both areas of activity would inform each other and guide the process of innovation. Two sites were initially selected. In the chemistry laboratory of the University of Virginia, under the direction of Lyndon F. Small, morphine-like structures would be manipulated and modified into new substances. The generation of these substances required new ways of defining and quantifying aspects of animal and human experience such as pain, tolerance, dependence, euphoria, respiratory depression, nausea, and cough. Initial screening and testing was carried out in laboratory animals at the University of Michigan by Nathan B. Eddy, under the supervision of Charles W. Edmunds. In the early years, there were frequent exchange visits of staff between the two laboratories, and the collaboration between them gave rise to new knowledge about the structure-action relationships of opiate substances.[355]

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[355] Eddy, *The National Research Council*, 20.

Eddy set to work on developing a method of testing analgesic potency in cats.[356] Initially, Eddy's animal tests provided sufficient information to estimate the analgesic potential of the substances synthesized by Small. Indeed, because of the high volume of tests substances that needed to be screened—in first two and a half years, 66 substances had been prepared and sent to Michigan—the convenience and rapidity of animal tests was probably considered to be crucial.[357]

On the basis of Eddy's findings, two substances were eventually recommended for further testing in humans. At first, the priority of human testing was to obtain more conclusive evidence on addictive liability. The first facility to be made available for this purpose was the federal Prison Annex at Fort Leavenworth, Kansas, where narcotic addicts were incarcerated and treated. A quantitative scale was devised to determine the presence of abstinence—as an indicator of addiction—in the prisoner subjects recruited at Fort Leavenworth, and then at the Public Health Service's Narcotics Hospital in Lexington, Kentucky when it opened in 1935.[358] For the abstinence test, subjects were given the test substance for a certain period of time, which was then withdrawn. An observer then determined the intensity of their symptoms of abstinence, or withdrawal, such as nausea, sweating and running eyes.

Members of the committee disagreed about the validity of the abstinence test in these subjects, who had previous experience of narcotic addiction, and it was resolved that the crucial test would be an evaluation in subjects who had never been addicted.[359] In the mid-30s, arrangements were made by the Committee, with the help of the Surgeon General, to obtain access to clinical

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[356] A description of this method can be found in: Nathan B. Eddy, "Studies on Morphine, Codeine and Their Derivatives," *Journal of Pharmacology* 45 (1932): 339-59.
[357] Eddy, *The National Research Council*, 19.
[358] Walter Treadway (Office of the Surgeon General) to Charles White, April 30, 1935, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, Committee on Drug Addiction (CDA), National Academy of Sciences Archives (NASA).
[359] Walter Treadway to Charles White, July 23, 1934, and Nathan B. Eddy to Charles White, June 29, 1934, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA.

facilities of the Massachusetts Department of Health.[360] Chronic cancer and tuberculosis patients were selected as populations to whom promising, but potentially addictive substances could be administered over prolonged periods for the relief of pain and cough. Clifton Himmelsbach, a young PHS officer who had worked on the development of abstinence tests in animals and the Leavenworth prisoners, was given the responsibility to oversee these studies. While the emphasis in this trial was on the determination of addictive liability, impressions of therapeutic efficacy were also recorded.[361]

Another disagreement between committee members put tests of analgesic efficacy higher on the agenda. When Himmelsbach showed that one of the promising drugs, called desomorphine, produced withdrawal symptoms in cancer patients, Eddy expressed concern about the excessive dosages they had been administered. He suggested that the calculation of effective dosages on the basis of animal tests was not reliable, and that the drug might still be salvageable. On account of its high potency, Eddy pointed out, in April of 1935, desomorphine might still be found to be "a very valuable drug."[362] To determine its true value, its propensity to bring about addiction would have to be carefully weighed against its power to relieve pain. The search for a *less* addictive rather than a *non-*addictive painkiller demanded a finer calculus of analgesic efficacy, leading to a stronger emphasis on pain-measurement in humans.

This emphasis may also have been stimulated by the opening of the Lexington narcotics hospital, which was made available to the Committee for human experimentation in 1935. The implementation of clinical trials of metopon,

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[360] H. S. Cumming (Surgeon General) to Henry D. Chadwick (Commissioner of Public Health), June 23, 1934, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA.

[361] Clifton Himmelsbach to Office of the Surgeon General, January 12, 1935, cc- White, Chadwick, Pope, Parker, Daland, Blake, Barrows, Gregg, Lambert, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA.

[362] N. B. Eddy to C. White, April 24, 1935, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA: "It seems to me there is still a possibility that this may be a very valuable drug in some fields on account of the intensity of its action. We need to know primarily the minimal effective clinical dose of the substance, information which Doctor Seevers has volunteered to obtain for us. With that as a basis trial of the substance by several experienced men in different fields under the most rigid and close control of our organization could determine its therapeutic advantages or otherwise."

another promising but somewhat addictive drug, in a network of five sites in 1937-39 provided further impetus for developing a method to collect data about pain relief. In any case, from 1935, committee members embarked on a more concerted search for human measures of pain and its relief.[363]

What was the Committee looking for in human tests of analgesic efficacy? The nature of Committee members' inquiries and attempts in this area gives us an idea of their criteria. Their efforts were directed towards two types of potential measures of pain: thresholds of pain perception and physiological correlates of the pain experience. The latter physiological avenue represented a desire for more objective measurement, where 'objective' meant bypassing the conscious deliberate statement of the subject by drawing information directly from their bodies' autonomic (and automatic) responses. Nathan B. Eddy, the Michigan pharmacologist who also began to take over the coordination of the CDA's activities, contacted Drs Cobb[364] and Whitehorn[365] in 1935 to ask them what they though of using a device referred to as a cardiotachometer for purposes of pain-measurement or detection.[366] Inquiries were also made of Walter Cannon, a Harvard physiologist well-known for his studies of the effects of emotions on involuntary body processes,[367] "in regard to some possible objective measurements of pain, and if Dr. Cannon can give us no suggestions along that line to see whether he feels observations on blood pressure, pulse rate, and possibly skin temperatures, could be used as measurements of pain."[368]

These inquiries represent an attempt to draw on what Othniel Dror has called 'emotion-gauging technologies,' methods used to locate and represent

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[363] This is particularly noticeable in the correspondence contained in: Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.
[364] Though his full name is not specified in Eddy's letter, he is presumably referring to Stanley Cobb, a Professor of Neuropathology at Harvard Medical School and previous member of the Committee on Drug Addiction.
[365] Presumably John C. Whitehorn, who was also involved in psychosomatic research.
[366] Eddy to Himmelsbach, July 15, 1935, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.
[367] See for example Walter B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage : an Account of Recent Researches into the Function of Emotional Excitement* (New York and London: D. Appleton and Company, 1915).
368 Pope to White, March 23, 1937, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA.

emotional traces in physiological and autonomic bodily processes. Cannon, Cobb and Whitehorn were all active participants in the "emotional turn" of research in American physiology, psychiatry and neurology in the 1930s. According to Dror's analysis, the power of these technologies was seen to lie in their ability to investigate "emotion" without relying on language, will, and personal interaction. Reading directly from the interior body, they could expose a preconscious, pre-linguistic truth about emotional fluctuations both in adult subjects, who could not be relied on to express their emotions objectively, and mute ones, like animals and young infants, who could not express them at all.[369] The appeal to these "emotionologists" for help with the problem of pain-measurement may indicate that these values were considered to be relevant by members of the committee. For reasons that were not specified in this correspondence, Whitehorn and Cannon were, unfortunately, not enthusiastic about the extension of these "emotion-gauging" technologies for the purpose of analgesic evaluation.

The other hopeful route to precision in analgesic measurement was through threshold-based techniques. When Eddy suggested that it might be possible to salvage desomorphine on the basis of more precise data about efficacy, it occurred to him immediately that Maurice Seevers might be able to help.[370] Seevers was a pharmacologist whose field of expertise was drug addiction, and whose main contribution to opiate research would later be the development of a test of addiction potentiality in the monkey. Around 1935, Seevers was working on modifying a technique developed in the late 19th century by von Frey, which I described in chapter 3, and to adapt it to the measurement of changes in sensitivity produced by analgesics.[371] This basic principle of measurement was first applied to the evaluation of analgesics around 1915.[372]

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[369] Othniel E. Dror, "The Scientific Image of Emotion: Experience and Technologies of Inscription," *Configurations* 7 (1999): 366-7.
[370] Eddy to White, April 24, 1935, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA.
[371] Maurice H. Seevers and Carl C. Pfeiffer, "A Study of the Analgesia, Subjective Depression, and Euphoria Produced by Morphine, Heroin, Dilaudid and Codeine in the Normal Human Subject," *Journal of Pharmacology* 56 (1936): 166-87.
[372] D. I. Macht, N. B. Herman, C. S. Levy, "A Quantitative Study of the Analgesia Produced by Opium Alkaloids, Individually and in Combination With Each Other, in Normal Man," *Journal of*

But no further reports seem to have appeared until 1936, when Seevers and his colleague Carl Pfeiffer published the results of their studies. They contrasted their algometric measurements of analgesia with the testimony of the "average clinical patient" for whom it was difficult to "critically analyze and report his sensations."[373]

CDA researchers, however, were unable to eliminate variations by using threshold-measuring instruments to evaluate analgesics. It had been hoped that Seevers' method would be useful for adding precision and objectivity to the clinical observations to be carried out at Pondville Cancer Hospital, as long as use of the device was not too time-consuming.[374] The principal problem, it turned out, was the high variability in his readings.[375] Attempts to find more reliable measures of analgesia based on threshold measurement at Lexington were similarly unsuccessful. Using the inductorium, a device that delivered electric shocks, Lexington researchers obtained such high fluctuations over time, even within the same subject, that the Director of Laboratories was forced to conclude that: "judgemental and interpretive factors were of more significance in producing pain than any particular degree of stimulation." Another approach was devised, which at first seemed to give reproducible results, but "further work with the method... revealed the fact that here too, other factors were at work which were uncontrollable and, to some extent, little understood." Intensive work along these lines was abandoned. [376]

Pharmacology 8 (1916): 1-37, this research team seems to have been mainly interested in opiate pharmacology. E. G. Martin, C. M. Grace, and J. H. McGuire, "The Influence of Drugs on the Human Sensory Threshold," *Journal of Pharmacology* 6 (1915): 527-32, the aim of this study, conducted in the Laboratory of Physiology of Harvard Medical School was to develop a quantitative method to determine sensitivity. It was part of a series of studies examining the influence of various factors, in this case drugs, on the sensory threshold.

[373] Seevers and Pfeiffer, "A Study of the Analgesia," 166-87.

[374] "Memorandum Number 2, June 18, 1936," Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital: outlines the protocol for the Pondville clinical study, CDA, NASA.

[375] L. E. Lee, Jr., "Studies of Morphine, Codeine and Their Derivatives," *Journal of Pharmacology* 75 (1942): 161-73.

[376] E. Williams, "A Quantitative Measure of Analgesia (Summary of Work Done at Lexington in Past Year, November 12, 1938" in L. Kolb to White, November 12, 1938, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.

The Committee was therefore left without an objective means of measuring analgesia. Clinical studies of metopon were nevertheless initiated at the University of Michigan Hospital, Pondville Cancer Hospital, Walter Reed Army Hospital, Marine Hospital and eventually the Massachusetts General Hospital. Determinations of pain relief were made on the basis of impressions obtained by nurses, patients and attending physicians but, as noted in one report, "not, unfortunately, upon any quantitative measure of analgesic effect."[377] Unable to distinguish between degrees of relief, researchers produced relatively imprecise data. To improve consistency, attempts were made to control rigidly the conditions of observation: patients and observers were to be blinded, nurses were to fill in standardised protocol forms to be sent to the Committee for tabulation, and comparison groups were to be created by pairing patients who experienced pains of similar origin and intensity.[378] It was difficult to enforce these controls. Committee researchers complained about the shortage of patient "material," which made it difficult to form control groups; about the failure of nurses to completely fill out protocol forms; about the difficulty of obtaining the cooperation of attending staff; about the unreliability of certain patients.[379] They had little clinical authority in the sites where studies were carried out, and little means of coordinating the production of data in different sites. These difficulties help us to understand the appeal of the precision and standardisation offered by the dolorimeter when it was introduced to the chairman of the Committee in 1939,

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[377] "Report on Work on Analgesia at the University of Michigan, October 6, 1937," Projects: Development of Nonaddictive Analgesics: Clinical Studies: University of Michigan Hospital, CDA, NASA.

[378] "Memorandum Number 2, June 18, 1936," Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA; Eddy to Metcalfe, November 18, 1938, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Walter Reed General Hospital, CDA, NASA; Lyndon Lee to Eddy, November 21, 1938, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA.

[379] Lee to White, December 8, 1938, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Pondville Hospital, CDA, NASA: "Admissions to Pondville Hospital are at a low ebb at present, consequently suitable study cases appear very infrequently."
Denney to Eddy, November 13, 1939, Projects: Development of Nonaddictive Analgesics: Clinical Studies: Marine Hospital, 1938-1939, CDA, NASA: Descriptions of individual patients and the problems in using them for analgesic studies can be found in the Committee's correspondence. For example, in this letter, Denney describes one patient as being: "a highly emotional, unstable person."

after these few years of intensive and futile search for such a method. It is also evident from these comments that clinical trials of analgesic drugs were not precision-measuring technologies at this time.

William Charles White, chairman of the CDA, was approached by Wolff at a meeting of the Division of Medical Sciences in 1939. Wolff demonstrated the apparatus to White and offered to make it available to the Committee.[380] White had been impressed by the quality of Wolff's results, noted that the method was inexpensive and, at his recommendation, Nathan Eddy, H. L. Andrews, a Lexington physiologist and Lee, who oversaw the Pondville Hospital studies, were sent to visit the Cornell laboratory.[381]

The Committee had high hopes for the dolorimeter. It was estimated that the equipment could be duplicated for about $300.[382] The consistency of its results was met with much approval.[383] And, while the development of a portable apparatus to use in clinical studies was discussed, the main hope was to use this method with "normal" subjects by Andrews at Lexington.[384] Since recruiting non-addict subjects was expected to be difficult, it was hoped that the post-addict prisoners at Lexington—the subjects to which CDA researchers had the quickest

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[380] White to Treadway, May 8, 1939, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.
[381] White to Harold G. Wolff, cc Lee, Andrews and Kolb, June 30, 1939, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.
[382] Eddy to Howard L. Andrews, June 30, 1939, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA: reports that the apparatus could be replicated for 300$. However, in Andrews' "Report to the Surgeon General on his visit to Wolff's laboratory by H. L. Andrews, July 28,1939," Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA: he states that since the Lexington laboratory already had a potentiometer, the rest of the apparatus would cost only 50$.
[383] Lee to White, July 24, 1939, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA: "the uniformity of Dr. Wolff's results with a small group of subjects is remarkable"; "Report to the Surgeon General on his visit to Wolff's laboratory by H. L. Andrews, July 28,1939," Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA: "The fact that the threshold has practically the same value for all 'normal' individuals is another feature in favor of the method."
[384] Lee to White, July 24, 1939, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.

and easiest access—could function as "normals."[385] These subjects could also be more easily manipulated and studied in a controlled environment. Human data that could be obtained quickly, without waiting the years it took to organise and run even modest clinical studies, was clearly desirable. After his visit to the Cornell lab, Andrews reported: "I feel that this technique fills a long-felt need in our substitute drug program,"[386] and that he was eager to begin work immediately "because of the fact that the complete evaluation of the addiction liability of a drug requires a knowledge of its analgetic power."[387]

Though Andrews did take up the dolorimeter in his laboratory, the Committee disbanded soon afterwards. The termination of Rockefeller funding, and then the war, interrupted the Committee's testing activities in 1939.[388] Eddy and Small were transferred to the National Institutes of Health, where they would be active in wartime research on malaria drugs.[389] However, H. L. Andrews would continue to study the potential of the dolorimeter in his laboratory at the Lexington narcotics hospital.

5.3 Synthetic Analgesics and the Bright Future of the Dolorimeter

While the Committee's relationship with the dolorimeter was short-lived, the demand for analgesic testing did not dry up. The arrival of two new substances from Germany sparked the interest of American pharmaceutical researchers and manufacturers: Demerol in the early 40s and Methadone at the end of the war. A small testing industry grew up around these two drugs and their hopeful derivatives, while general research in opiate pharmacology was also given a boost. Their arrival was marked by clusters of scientific articles in major

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[385] "Report to the Surgeon General on his visit to Wolff's laboratory by H. L. Andrews, July 28,1939," Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.
[386] Andrews to White, July 28, 1939, Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.
[387] "Report to the Surgeon General on his visit to Wolff's laboratory by H. L. Andrews, July 28,1939," Projects: Development of Nonaddictive Analgesics: Clinical studies: PHS Hospital: Lexington General, CDA, NASA.
[388] Eddy, *The National Research Council*, 42-51.
[389] Leo Slater, personal communication, 2005.

journals reporting on their pharmacological assessment.[390] Pharmaceutical firms, particularly Hoffmann-LaRoche, were also interested in determining the value of anticholinesterase drugs as potentiating agents that would heighten the effectiveness of morphine, and thus allow smaller doses to be used.[391] The Hardy-Wolff-Goodell dolorimeter proved to be very popular during this expansion of analgesic testing.[392]

Demerol (also called meperidine in the U. S.) was first synthesized in Germany in 1939 by Eisleb and Schaumann.[393] Testing in the United States seems to have begun in 1941, and the drug was approved by the Food and Drug Administration in 1942.[394] The compound represented a departure from previous attempts to create new analgesics, which had focused on tinkering with the morphine molecule. As a piperidine compound, Demerol represented a new avenue for analgesic pharmacological innovation. Demerol began to be tested clinically at Bellevue, a teaching hospital of New York University, in 1941, by R. C. Batterman.[395] But, perhaps dissatisfied with the lack of objectivity of these clinical data, Batterman and Himmelsbach published results obtained with the dolorimeter in 1943.[396] While they concluded that side effects made Demerol a poor substitute for morphine, they pointed out the advantages of fully synthetic

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[390] A systematic review of publications on analgesics and opiates in the *Federation Proceedings*, particularly in the abstracts of the Society for Pharmacology and Experimental Therapeutics, during the 1940s, indicates first that analgesics became a more popular topic during this decade, and that more numerous abstracts appeared in 1942, when Demerol was being tested, and then again beginning in 1946 with the arrival of methadone.

[391] Donald Slaughter and D. W. Munsell, "Some New Aspects of Morphine Action. Effects on Pain," *Journal of Pharmacology* 68 (1940): 104-12. This study was funded by Hoffmann-Laroche, which also provided the test drug. Hoffmann-Laroche would also fund these researchers to examine the value of the dolorimeter for evaluating its efficacy, see: Slaughter and F. T. Wright, "A Modification of the Hardy-Wolff-Goodell Pain-Threshold Apparatus," *Current Research in Anesthesia and Analgesia* 23 (1944): 115-19.

[392] This can also be ascertained from a review of the abstracts of the Society for Pharmacology and Experimental Therapeutics published in *Federation Proceedings* for 1940-1950.

[393] 1-methyl-4-phenyl-piperidine-4-carboxylic acid ethyl ester, also known in various countries as dolantin (the name it was first given by the German researchers), isonipecaine, dolantol, dolasol, pethidine (often used by British researchers), mefedine and lidol.

[394] Robert C. Batterman and Clifton K. Himmelsbach, "Demerol - A New Synthetic Analgesic," *Journal of the American Medical Association* 122 (1943): 222.

[395] Robert C. Batterman, "Clinical Effectiveness and Safety of a New Synthetic Analgesic Drug, Demerol," *Archives of Internal Medicine* 71 (1943): 345-56.

[396] Batterman and Himmelsbach," Demerol," 222-26.

narcotics. Demerol was soon presented as a potential solution to increased wartime needs for pain-relief and possible threats to American opium supplies.[397]

News of more compounds synthesized by German chemists was disclosed by the U. S. Department of Commerce in a report based on information gathered by a team headed by Dr. Kleiderer of the Technical Industrial Intelligence Committee, under the Foreign Economic Administration. [398] Of these compounds, the one with the serial number 10820 appeared particularly promising: it was called amidone by the Germans, but in 1947 the Council on Pharmacy and Chemistry of the American Medical Association announced that its generic name would be methadone.[399]

Methadone was approved by the FDA soon after the initial American report under the trade name of Dolophine, which was already manufactured by the pharmaceutical firm Eli Lilly and Company. Indeed, Eli Lilly had been quick to act. By March of 1946, two of its researchers, C. C. Scott and K. K. Chen, were already announcing the results of analgesic tests of compound 10820 to the American Society for Pharmacology and Experimental Therapeutics. They had conducted tests in albino rats, 7 trained dogs and 3 human subjects. For the last, they had used the Wolff-Hardy-Goodell apparatus. The Lilly Research Laboratories continued to keep busy testing the analgesic activity of other compounds in the German series, and determining the relative potency of the two different isomer forms of methadone, for which they also used the dolorimeter. [400]

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[397] Batterman and Himmelsbach," Demerol," 222.

[398] C. C. Scott and K. K. Chen, "The Action of 1, 1-diphenyl-1-(dimethylaminoisopropyl) butanone-2, a Potent Analgesic Agent," *Journal of Pharmacology and Experimental Therapy* 87 (1946), 63-71.

[399] Methadone is now popularly known as a heroin substitute that is used for the therapeutic maintenance or controlled withdrawal of heroin addition. However, it first attracted interest as a fully-synthetic potent analgesic. Other names given to the compound 1, 1-Diphenyl-1-(Dimethylaminoisopropyl)-butanone-2 included: amidon, methadon, "10,820", miadone, "AN-148", butalgin and dolophine.

[400] C. C. Scott, E. B. Robbins and K. K. Chen, "Pharmacologic Comparison of the Optical Isomers of Methadon," *Journal of Pharmacology and Experimental Therapy* 93 (1948) 282-6: compared the two forms of methadone and obtained different results with each organism: Rats with Haffner tail-pinching technique found the l-isomer to be 7.5 times more potent in its analgesic action than the d-form. In dogs the l-isomer was 25 times more potent an in humans, 50 times. Scott and Chen, "The Action of 1, 1-Diphenyl..." 63-71, note that details of pharmacological work not available from the report of the Department of Commerce.

Hoffman-LaRoche had also taken an active interest in analgesic testing following the arrival of the new synthetics. Scientists employed by its research department organized a collaborative study of analgesic testing methods in which academic and industry researchers compared five compounds, including Demerol and methadone. The firm did not only test the German substances, but also began modifying molecular structures to obtain novel and hopefully profitable substances. In 1947, its pharmacology department screened a series of piperidine derivatives (like Demerol) in rats, using a modified Hardy-Wolff-Goodell set-up.[401] Three of these showed an interesting level of potency, and were handed over, along with a research grant, to a research team at the University of Iowa. The team, headed by E. G. Gross assessed these compounds in medical students using the original Hardy-Wolff-Goodell dolorimeter.[402]

This was a time of expanding analgesic research, but the 1940s was also a time when academic researchers—particularly pharmacologists—could increasingly hope to obtain research funding from the pharmaceutical industry.[403] Most of the human testing of analgesics was carried out in academic settings, though one group used it in an industrial laboratory. In commercial settings, the use of modified Hardy-Wolff-Goodell designs adapted for animal subjects was popular.[404] But pharmaceutical firms obviously thought that human data on analgesic efficacy was important enough to justify providing grants to academic pharmacologists for drug evaluation. Eli Lilly,[405] Hoffman-LaRoche[406] and Whitehall Pharmacal[407] all provided grants for specific studies using the dolorimeter, while Smith, Kline & French funded a fellow who worked

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[401] R. H. K. Foster and A. J. Carman, "Studies in Analgesia: Piperidine Derivatives With Morphine-Like Activity," *Journal of Pharmacology* 91 (1947): 195-209.
[402] E. G. Gross, H. L. Holland and F. W. Schueler, "Human Studies on Analgesic Piperidine Derivatives," *Journal of Applied Physiology* 1 (1948): 298-303.
[403] John P. Swann, *Academic Scientists and the Pharmaceutical Industry: Cooperative Research in 20th-Century America* (Baltimore: Johns Hopkins University Press, 1988), 25.
[404] Pfeiffer, et al., "Experimental Methods for Studying Analgesia," *Annals of the New York Academy of Science* 51, no.1 (1948): 21-33.
[405] Scott, Robbins and Chen, "Pharmacologic Comparison," 282-6.
[406] Slaughter and Wright, "A Modification of the Hardy-Wolff-Goodell,"115-19; E. G. Gross, H. L. Holland, and F. W. Schueler, "Human Studies," 298-303.
[407] F. B. Flinn and A. S. Chaikelis, "An Improved Instrument for the Determination of Changes in the Pain Threshold Caused by Drugs," *American Journal of Psychiatry* 103 (1946-1947): 349-50.

intensively with other academic researchers on evaluating the dolorimetric method.[408]

During the years from 1948 to 1950, the Hardy-Wolff-Goodell dolorimeter appears to have been at the peak of its career. The recent spurt of activity in synthesis and testing of analgesics was in full swing. In the introduction to a volume on the "newer synthetic analgesics" published by the *Annals of the New York Academy of Science*, E. L. Tainter, of the Sterling-Winthrop Research Institute commented on the intimate relationship between the modern history of analgesic drugs and the development of methods in quantitative pharmacology. While the first three periods of analgesic history had been characterized by the discovery of substances and of new chemical methods of isolation and synthesis, the fourth, or "modern" period was "characterised by the development of quantitative pharmacological methods of testing" which guided the design of new molecules based on knowledge of the relationship between structure and effect.[409] Reviews of analgesic testing methods by industry researchers sang the praises of the dolorimeter: "Of the methods proposed and used in the last decade, it is safe to say that the Hardy-Wolff-Goodell procedure has gained the widest acceptance,"[410] commented Miller of the Sterling-Winthrop Research Institute, a statement echoed by Chen of the Eli Lilly Research Laboratories who declared it to be "the best."[411] Both noted that the apparatus had recently been made available commercially: someone clearly had had the impression that, given the surge of interest in analgesic innovation, there would be a growing market for this type of equipment.

Even at the height of its popularity, few denied that using the Hardy-Wolff-Goodell method required adjustments and compromises. Nor did anyone dispute the need for clinical confirmations of therapeutic efficacy. In addition,

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[408] R. R. Sonnenschein and A. C. Ivy, "Failure of Oral Antipyretic Drugs to Alter Normal Human Pain Thresholds," *Journal of Pharmacology* 97 (1949): 308-13. R. Sonnenschein was a fellow of Smith, Kline and French.
[409] E. L. Tainter, "Pain," *Annals of the New York Academy of Science* 51, no.1 (1948): 3-11. The publication of this volume on the new synthetic analgesics was funded by pharmaceutical firms.
[410] Miller, "A Critique of Analgesic Testing," 34-50.
[411] K. K. Chen, "Physiological and Pharmacological Background Including Methods of Evaluation of Analgesic Agents," *Journal of the American Pharmaceutical Association* 38 (1949): 51-55.

obtaining consistent results turned out to be more difficult than initially anticipated. As we will see, sources of variation in the data were not always easy to identify, manage and interpret. Of particular concern to the pharmaceutical industry was the apparent lack of sensitivity of the dolorimetric method to weak analgesics, particularly antipyretics such as aspirin.[412] However, the dolorimeter served an important purpose: rapidly estimating the analgesic potency—accurate in humans—of new, exciting compounds. Neither animal tests nor clinical studies seemed to be able to yield this kind of data at that particular time. The dolorimeter situated itself within this gap.

5.3.1. Filling a Gap: between Animal "Pain" and the "Total" Pain Experience

From the outset, Hardy, Wolff and Goodell carved a space for the dolorimeter, a gap in objectivity between the "animal experimentation" and "clinical impression" that had previously informed researchers about analgesic action.[413] As I have already suggested, this gap had started being defined through the CDA's activities in the late 1930s. Animal tests, such as Eddy's technique of squeezing cats' tails, were useful for initial, high-volume screening, but were sometimes found to give inaccurate values for human dosages and potency. However, animal tests presented various advantages, both practical and epistemological. Clinical studies, on the other hand, were judged to be extremely difficult to control. "Clinical pain" was undoubtedly seen as more authentic, but its measurement lacked precision, standardisation and objectivity. The level of control obtained in the clinical studies organised by the CDA, or the handful of other clinical studies run during the 1940s, did not greatly contribute to bridging

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[412] Miller, "A Critique of Analgesic Testing," 34-50. The lucrative market for weaker over-the-counter analgesics may have made this a big disadvantage from the perspective of the pharmaceutical firms.

[413] Wolff, Hardy and Goodell, "Studies on Pain. Measurement of the Effect of Morphine,"659-680: Introduce this report by stating that all previous studies had either relied on animal tests or "clinical impression" but that: "since the prime purpose of an analgesic drug concerns its action in man, it is desirable to measure accurately its effect on man's pain threshold. For such measurement a suitable method has now been developed."

the gap between laboratory "animal pain" and clinical impression. This gap was an epistemological one, but was also, significantly, a financial, logistical and professional one. It only began to narrow in the late 1940s.

For data on analgesic efficacy to be useful, it had to be both meaningful and comprehensible. But to what extent could the phenomenon of pain be made more intelligible through experimental manipulation before it lost its original meaning? This question was present, at least implicitly, in most discussions about the merits of testing methods. The different possible qualities that data could take on—precision, authenticity, clarity, uniformity, exactitude, consistency—had to be weighed against each other. These qualities came at a cost. The standardisation of experimental practice often entailed a loss of accuracy and precision. There were other, more banal costs for these qualities: the price of equipment, the difficulty of obtaining subjects, the time needed to produce data, the volume of data that had to be produced, the authority required to control practices of data collection, the staff needed to collect data, the facilities required for experimentation, and so on.

These issues were certainly not peculiar to analgesia as an experimental object.[414] Working with animals rather than humans presented similar advantages for analgesic testing as it did for other kinds of laboratory work; animals offered more possibilities for standardisation, they were easy to recruit or to purchase, and their whole lives could be dedicated to therapeutic testing. Animal models for measurement were very useful for industrial laboratories, where modified animal versions of the Hardy-Wolff-Goodell proved to be very popular.[415] There, animals could be kept readily available, and the methods could be operated by pharmacologists and physiologists on company payrolls. Results could be produced fast enough to affect decisions about manufacturing.

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[414] For example, see : Thomas Schlich, *Surgery, Science and Industry* (Houndmills and New York : Palgrave Macmillan,2002), 86-109, for an analysis of the debates about the relevance of laboratory-produced knowledge for clinical practice on humans in fracture surgery.
[415] H. E. Hill, R. E. Belleville, and A. Wikler, "Anxiety Reduction as a Measure of the Analgesic Effectiveness of Drugs," *Science* 120 (1954): 153.

The trade-offs, however, were in some ways specific to the way in which experimenters perceived pain as an experimental variable, as well as to the context in which analgesic research was funded and organized. As one reviewer put it: "Because pain is a subjective phenomenon, more difficulty arises on this point with animals than with human subjects, a situation which is the reverse of that usually encountered in pharmacological studies."[416] One of the most significant merits of animal measures of analgesia was also their principal fault: the subjects did not speak. Animal responses to painful stimulation could only be read directly in their bodies, which made animal tests more objective but also less informative, as well as potentially less accurate.[417]

Animal researchers accepted, implicitly or explicitly, that what they studied bore an uncertain, possibly distant relationship to human experiences of pain. Animal "pain" was sometimes deliberately framed in quotation marks, [418] while other researchers refused to use the term altogether, preferring, for example, to write about an "avoiding reflex" instead of a "pain threshold." [419] In the 1930s, there was still hope of objectifying human pain on the same "pre-linguistic, preconscious" level as in animal tests. During the 1940s, however, as such hopes

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[416] Miller, "A Critique of Analgesic Testing," 34-50.

[417] There are other instances in which scientists were willing to make trade-offs between accuracy or information and objectivity, see for example: Lorraine Daston and Peter Galison, "The Image of Objectivity," *Representations* 40 (1992): 81-128.
Molitor and H. Robinson, "The Quantitative Determination of a Weak Local Anesthetic Action," *Current Research in Anesthesia and Analgesia* 17 (1938): 188-94, explain how animal reactions were less informative than human ones: "In experiments on man it is possible to depend to a certain degree on his statements provided he is intelligent and cooperative, but in animals we must rely entirely on reactions which accompany the sensation of pain, such as cry, flight reflex, changes in respiration, blood pressure, pupillar diameter or nerve and brain action currents." Frank R. Goetzl, D. Y. Burrill and A. C. Ivy, "A Critical Analysis of Algesimetric Methods With Suggestions for a Useful Procedure," *Quarterly Bulletin of the Northwestern University Medical School* 17 (1943): 280-291, explain how these reactions might be seen as more objective: "...an avoiding reflex, an objective phenomenon... should be more reliable for the determination of pain threshold than the subjective report of a human subject. This should obtain particularly when drugs are used which effect the subjective feeling tone or the mental attitude..."

[418] For example: G. C. Knowlton and E. G. Gross, "A Method for Studying the Analgetic Effect of Drugs in Animals," *Journal of Pharmacology* 78 (1943): 99, conclude that their method is suitable for determining "pain"thresholds in animals (quotation marks in original, though not used consistently throughout article).

[419] F. R. Goetzl, D. Y. Burrill, and A. C. Ivy, "Observations on the Analgesic Effect of Morphine During Continued Daily Administration of Small and Uniform Doses to Dogs," *Journal of Pharmacology* 82 (1944): 110-119.

were abandoned; the gap between human and animal measures of pain seemed only to grow wider.

If animal tests did not measure pain, what did they measure? The basic principle was the same in nearly all the experimental setups: an animal, when subjected to a strong, presumably unpleasant stimulus, would, at some point, exhibit some responsive behaviour that could be measured.[420] By choosing a suitable animal, an appropriate stimulus and a recognisable and reliable response, a researcher could produce a measurable endpoint that would be sensitive to drug effects. An astonishing variety of contraptions were designed to do this from the 1930s onwards in the U. S., which shows just how valuable such tests were considered, but also how difficult it was for researchers to agree on the precise merits of each one. Whether they subjected cats, dogs, rats or mice to the violence of tail-pinching, tooth shocking or being made to stand on hot-plates, researchers competed in producing endpoints—if possible cheaply and easily—that were definite, observable, quantifiable, consistent, and discriminative.[421] Examples of such endpoints were: lifting of hind legs, skin twitch, tail twitch, widening of

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[420] This type of set-up is fairly typical for behaviourist experiments in psychology, though in this case what is being measured is the impact of drug effect rather than the impact of learning or conditioning on responses.

[421] Woolfe and Macdonald, "The Evaluation of the Analgesic," 300-307: state that, in their method, the
lifting of hind legs was chosen as "criterion of acute discomfort" because the first response to the heat was to lift the front paws to cool them, a response that could easily be confused with normal grooming behaviours. The lifting of hind legs was therefore more definite.
D. Slaugther and D. W. Munsell, "Some New Aspects of Morphine Action. Effects on Pain," *Journal of Pharmacology* 68 (1940): 104-12: state that the recording of responses was done by two different observers to "obviate any personal error," the lack of changes in results made this "work more acceptable."
R. E. Lee, H. L. Williams and C. C. Pfeiffer, "A Warm Wire Algesimeter," *Federation Proceedings* 8 (1949): 314: this device was said to obtain "more quantitative results" in mice than the "usual tail-pinching method.
Fred E. D'Amour and Donn L. Smith, "A Method for Determining Loss of Pain Sensation," *Journal of Clinical Investigation* 72 (1941): 74-80: previous methods for testing analgesic in animals were said to "suffer from two main defects: doubt exists as to the stimulus, i.e., whether it is pain or merely touch, and there are great individual variations in test animals." This method was valuable because the individual variation was found to be small. See also: Knowlton and Gross, "A Method for Studying," 93-99: the opening or widening of eyelids was chosen as an endpoint even though "the only sure sign of pain in an animal is a typical cry or howl" because the sequence of responses was so uniform, and it could be elicited consistently with little variation in stimulation

eyelids, and squeaks. Even devoid of its cortex, it was observed, an animal would display such reactions to strong stimulation.[422]

As pre-cortical reactions, animal expressions of "pain" were protected from the interference of the conscious will. But, stripped of its meaning and experiential qualities, how could this reflex animal behaviour be translated back into information about the relief of clinical suffering? On the other hand, clinical studies were faced with the problem of differentiating the effect of analgesics from a tangle of other possible influences on pain: expectations of relief, fear, mental processes, mood, psychological disposition, variations in personality and sensitivity.[423] Animal subjects were presumably less prone than human ones to infuse meaning into their experiences and to ponder the implications of their sensations, while they offered numerous practical advantages. Thus, as is made evident by the large number of animal analgesia tests that were created and put into use during the 1930s and 1940s, their advantages made certain compromises in "authenticity" and accuracy worthwhile.

At the other end of the spectrum was the human patient, living a "total" experience of pain. Information about this pain could be collected in hospitals, usually in post-operative, obstetric and cancer wards. Unquestionably more authentic, clinical pain was subject to complex influences and fluctuations; it was confusing, unwieldy, difficult to control and to standardise. To be useful, information about the relief of clinical pain had to be obtained in large volumes. Rare reports of analgesic clinical studies in the 1940s were published only when they had evaluated analgesics in hundreds of patients (as many as 1200 and no fewer than 47), sometimes over more than a year. The "psychological make-up" of the patient was considered to be "of prime importance" in clinical evaluations

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[422] H. Molitor and H. Robinson, "The Quantitative Determination," 188-94.

[423] For example: Batterman, "The Clinical Aspects," 596. Though Batterman called for the evaluation of analgesics in clinical patients, he cautioned: "consider the difficulties, however, of evaluating the complaints of a patient who is supposedly in distress on the basis of organic disease. In addition to the perception of the painful stimulus, every patient varies to an astounding degree in the development of associated ideas or introspective analyses of the responsible factor for the discomfort."

of pain relief.[424] This was a difficult factor to control, and was probably done in large part through the informal selection of subjects. In addition, control groups were usually matched up with test groups on the basis of the severity of their pain. It was essential, then, that clinical analgesic evaluators have access not only to "clinical material" but a lot of it. Physiologists and pharmacologists, the ones usually entrusted with the testing of analgesics, did not commonly have this kind of access. When they did, they may have lacked authority over the recruitment and care of experimental subjects that made it possible to select proper groups, and implement controls. Despite several appeals to the importance of using placebos in any study of therapeutic effect where pain was involved,[425] it may have been difficult to convince regular hospital staff to administer placebos for pain relief. For whatever reason, placebos were rarely used.

The results offered in the few available reports of clinical studies of analgesics were approximate compilations of qualitative judgments, for example, the number of patients reporting "satisfactory relief,"[426] or, "complete, moderate, slight or none."[427] In addition, these judgments were often based on patients' own statements. In the early 1940s, no claims were made about the desirability of relying on the subjects' impression of pain relief. This was seen as an unfortunate necessity, to be supplemented when possible by adding physicians' and nurses' objective observations of patients' behaviours—restlessness, moaning, appearance of comfort—as well as their evaluations of the trustworthiness of patients' complaints.[428] These observations and evaluation were rarely standardised, thus making it difficult to compare them.

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[424] Batterman, "Clinical Effectiveness," 345-56.
[425] Harry Gold, N. T. Kwit and H. Otto, "The Xanthines (Theobromine and Aminophylline) in the Treatment of Cardiac Pain," *Journal of the American Medical Association* 108 (1937): 2173-2179.; E. M. Jellinek, "Clinical Tests on Comparative Effectiveness of Analgesic Drugs," *Biometrics Bulletin* 2 (1946): 87-91.; Batterman, "The Clinical Aspects of Evaluating," 595-607.
[426] C. C. Scott, K. G. Kohlstaedt, and K. K. Chen, "Comparison of the Pharmacologic Properties of Some New Analgesic Substances," *Anesthesia and Analgesia* (1947): 12-17.
[427] Batterman, "Clinical Effectiveness," 345-356; Elizabeth B. Troxil, "Clinical Evaluation of the Analgesic Methadon," *Journal of the American Medical Association* 136 (1948): 920-923.
[428] For example: Batterman, "Clinical Effectiveness,"345-356; Lyndon E. Lee Jr., "Studies of Morphine, Codeine and Their Derivatives," *Journal of Pharmacology* 75 (1942): 161-73.

Given these difficulties, it is not surprising that few clinical studies of analgesics were conducted during the 1940s. These would later be dismissed as poorly controlled. Such studies were not only long and complex, but also expensive. Pharmaceutical companies, who funded most of the analgesic testing during this decade, may not have been willing to invest that kind of money for data. In this context, rare calls for more and better controlled clinical evaluation were theoretical.[429] Controlling clinical pain, its relief and evaluation required resources that had not yet been made widely available for analgesic testing. In the 1940s, the clinical trial was not seen as a specific technology, much less a precision-measurement instrument, for gathering data about pain relief.

Between the high cost in authenticity and certainty of animal "pain" and clinical studies' high cost in time, subjects, money, staffing and clinical authority, the dolorimeter was apparently a good option. These were human data that pharmaceutical companies could afford: the dolorimeter cost $850.00 in 1953 from the Co-Design Corporation, it could be operated by pharmacologists or physiologists with presumably fairly modest grants, given the amount of time (a matter of days) and subjects (as few as 3) required to complete a trial.[430] The apparatus was even used in pharmaceutical company laboratories. The dolorimeter did not require patients as subjects. A handful of colleagues, medical students, or prisoners, in the case of the studies at Lexington, were presumably easy to recruit and to see over several days in order to multiply observations. Thus, the use of the dolorimeter required the kinds of resources and the type of professional expertise which pharmaceutical companies and their academic contacts were able and willing to make available for analgesic evaluation in the 1940s. In addition, it combined the objectivity and practicality of animals tests with the epistemological advantages of data obtained from human subjects.

5.3.2 At Work with the Dolorimeter

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[429] For example, Batterman, "The Clinical Aspects of Evaluating," 595-607.
[430] The price of the dolorimeter is in: Sales pamphlet for the Hardy-Wolff-Goodell Dolorimeter, Co-Design Corporation, Winchester, MA, n.d., Wolff Papers, Box 3, Folder 12, Medical Center Archives of NewYork-Presbyterian/Weill Cornell, New York, NY.

A dolorimeter, a modest grant, some laboratory space, a handful of subjects and a physiologist or pharmacologist… it took more than this to make the dolorimeter work. In this section, I will examine the practice of dolorimetric experimentation. First, I will describe one research group's experience with the dolorimeter.

Janet Travell was one of the investigators who, in the 1940s, identified the dolorimeter as a potentially useful therapeutic test. Having developed a therapeutic procedure of spraying ethyl chloride, a vapo-coolant substance, to relieve cardiac pain, she was exploring different ways of determining its analgesic efficacy. Recently promoted to Assistant Professor in clinical pharmacology at Cornell University, and soon to become Associate Professor, Travell was well indoctrinated in the principles of the new clinical pharmacology advocated by her colleagues McKeen Cattell and Harry Gold, pioneers in the promotion of placebo controlling and double-blinding. Travell was thus familiar with the dangers of hasty therapeutic judgments and sought to establish the value of her new procedure on a firm basis.[431] In 1949-50, trials with the dolorimeter took place in her laboratory. Among her notes, I found the following record of her colleague's experiences as a dolorimetric subject:

Kropowska- forehead
12/22/49
170- warmer than
180- prick? Warmer than bef
190-about the same as bef
200- beginning to burn sensation- burns afterwards
210-sharp sudden "burning" prick?
220- more like a prick, momentary
230- burning pain
195- no distinct prick
200-202 a prick maybe- not sure
205- a prick
235-240 burning-intense heat
250 a little like needles
260- burning pain

[431] Janet Travell, *Office Hours: Day and Night* (New York and Cleveland: The World Publishing Company, 1968): 245.

250 "pricks" [432]

The procedure, according to other laboratory notes, was to begin with an exposure below 200 so "as not to frighten" the subject, and then to vary intensities until the point "at which just feel "pin-prick"—not pain, not heat"[433] had been bracketed by exposures over and under the threshold. Question marks frequently dotted this brief set of notes. After subjects had hesitated about whether or not they had encountered the "prick," experimenters had to decide how to interpret results and figure out how to fix procedural problems:

> Pain thresholds lower than before! Valid or not?
> II cause lowered temp
> Pain threshold determinations cause temp incr
> On both these exp skin irritation, erythema…
> Maybe explains pain thresholds being lower than previously
> Determinations should not be made as soon as one minute apart.
> 1) start below
> 2) work up but don't go backwards!
> 3) Let 2 min interval between tests[434]

These notes suggest that making judgments about the validity of dolorimetric data wasn't always clear-cut. While it was, unfortunately, impossible to locate more of these kinds of laboratory notes, even the published reports of dolorimetric trials provide some clues about the kinds of efforts and adjustments that were necessary for making the dolorimeter work. Decisions had to be made about what kinds of modifications to make; about whether these practices were legitimate uses of the apparatus; and about whether results had reached an acceptable level of consistency, certainty and precision.

Historical and sociological studies of experiment have shown that replicating results has not always come easily in scientific practice. Based on his

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[432] "Pain threshold experiments 1949-50," Travell Papers, Box 25, Folder 21, Special Collections and University Archives, GWU: The numbers in the left column correspond to the intensity of the radiation in millicalories per second to which Irene Kropowska's forehead was exposed, and the comments in the right column are presumably the subject's own.
[433] "Pain threshold experiments 1949-50," Travell Papers, Box 25, Folder 21, Special Collections and University Archives, GWU.
[434] "Pain threshold experiments 1949-50," Travell Papers, Box 25, Folder 21, Special Collections and University Archives, GWU.

study of laser-building, Harry Collins has proposed that achieving replication in practice requires "experimental ability," a form of tacit knowledge, or skill, which is not easily acquired. It travels capriciously, and depends on practice and often on personal contacts between investigators. Thus, according to Collins, replicability is not an "inherent" quality of the experimental result, nor a direct indication of the validity of the experimental method, but is a property that is also defined by the social context in which experiments are performed and debated.[435]

Because of this, it is often not a straightforward matter to determine whether a specific instance of experimentation constitutes "replication." Collins has described a vicious epistemological cycle in which experimental competence and the "right tools" are defined by the achievement of appropriate results, and results can only be judged on the basis of whether an experiment has been performed competently and with the right tools. Simon Schaeffer has given an example of this "experimenter's regress" in Newton's refusal to accept evidence that contradicted his experiments with the glass prism. Those who had failed to obtain the same results were accused of having used bad prisms. Thus, the decisive criterion of a good experiment, as defined by Newton, was that it produced the right results. In this type of situation, Collins pointed out, "scientific" criteria are insufficient for reaching closure. The cycle can only be broken by negotiating the meaning of "good experiment" and "right results" in the social arena. In Newton's case, he acquired sufficient clout in contemporary scientific society to achieve authority over the identification of proper instruments, and thus of decisive experiments.[436]

Dolorimetric experimenters seemed to accept that replication would not necessarily be immediate. When they failed to obtain the same results as Hardy, Wolff and Goodell, many were willing to figure out how use the dolorimeter properly before it would give the "right results," and/or to tolerate some amount

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[435] Harry M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Chicago: University of Chicago Press, 1992), 128-157.
[436] Simon Schaffer, "Glass Works: Newton's Prisms and the Uses of Experiment," in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge and New York: Cambridge University Press, 1989), 67-104.

of discrepancy in their data. Others, like Janet Travell, gave up on the dolorimeter, and determined it was not useful for their particular purpose. Beecher was the first, nearly a decade after the introduction of the dolorimeter, to make a big deal out of these discrepancies and to formulate an attack on the validity of the dolorimeter on this basis. To his criticism, Hardy, Wolff and Goodell responded: "These disagreements show the importance of care and experience in planning studies on pain and in the operation of pain threshold measuring equipment."[437] Thus, issues of validity and replicability were debated alongside questions of proper use and of "good experiment."

Beyond the immediate question of whether the dolorimeter produced valid results, experimenters also asked themselves: Under what conditions can we make the dolorimeter work? Are these legitimate conditions under which to measure pain and analgesia? I will explore debates about the replicability of dolorimetric results by focusing on a specific issue: the training of subjects. The question was seen as an important and sensitive one; it was discussed throughout the 1940s and taken up by Beecher as a key point in his criticism.

We will recall that, in their original article, Hardy, Wolff and Goodell claimed that the pain threshold was "easily recognized, even by untrained subjects..."[438] But it was not long before other investigators reported that only subjects who were familiar with the apparatus gave consistent results. Some seemed to accept this as an unexceptional measure required to standardise the operation of the apparatus, a technical requirement akin to calibrating the intensity of radiation or using a skilled technician.

Indeed, the training of subjects was not always viewed with suspicion in psychophysical experimentation. On the contrary, in late 19th and early 20th century European and American psychology, sophisticated subjects were not only tolerated but valued as highly capable and standardised 'tools' of introspection. The function of training in late 19th century Leipzig, according to Ruth Benschop and Douwe Draaisma, was to "calibrate" the minds of experimental subjects to

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[437] Hardy, Wolff and Goodell, "Pain—Controlled and Uncontrolled," 165.
[438] Hardy, Wolff and Goodell, "Studies on Pain: A New Method," 650.

transform them into "a point of access to what has been called the "generalized mind," that is, being representative of the "universal features of adult mental life." In this context, training subjects was part of a quest for precision that was associated with a broader program to transform psychology into an experimental and 'natural' science.[439] In American psychology at the turn of the 20th century, according to Deborah Coon, training was meant both to turn the subject into a kind of machine, and to give the subject the skills of a technician for operating his own self-machine. According to Coon's analysis, the objective was to mechanise psychological practice in order to make it conform to the ideals of an industrializing society.[440]

Training was a technique for reducing the variability of outcomes, but 'expert' subjects also acquired credentials that guaranteed the validity and reliability of their observations.[441] According to Kurt Danziger, such subjects were part of the social arrangements of early psychology in which experimenters and subjects (called observers) usually knew each other personally and were on a similar social footing, either as colleagues or professors and graduate students. In fact, these roles were often interchangeable. Subjects were often identified by name or initials in published research reports. Over the course of the first half of the 20th century, the trend in American psychology was to replace these highly qualified, identified "observers"—who fulfilled the role of a trained technician—with anonymous "subjects", whose "function is that of an object of experimental manipulation and scrutiny. In other words, there is a shift away from the expert observer, toward the manipulated object of observation."[442] This new model, which usually implied the use of large numbers of undergraduate students as

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[439] Ruth Benschop and Douwe Draaisma, "In Pursuit of Precision: the Calibration of Minds and Machines in Late Nineteenth Century Psychology," *Annals of* Science 57 (2000), 18-19

[440] Deborah J. Coon, "Standardizing the Subject: Experimental Psychologists, Introspection, and the Quest for a Technoscientific Ideal," *Technology and Culture* 34 (1993): 775.

[441] Benschop and Draaisma, "In Pursuit of Precision," 18-19.

[442] Kurt Danziger, "A Question of Identity: Who Participated in Psychological Experiments?" in *The Rise of Experimental Psychology,* ed. J. G. Morawski (New Haven and London: Yale University Press, 1988), 43. See also: Danzinger, *Constructing the Subject* (New York: Cambridge University Press, 1990).

experimental subjects, was more in line with the asymmetrical power relations of medical experimentation.

Hardy, Wolff and Goodell could have appealed to the qualities of the expert observer to establish the validity of their initial dolorimetric data. This data had been obtained using themselves as subjects. After much practice, they were surely highly skilled in making observations and could have presented themselves as competent and reliable subjects. Perhaps worried that their self-experimentation might be held against them, they appealed instead to the value of anonymous, interchangeable subjects by stating that the threshold could be easily and consistently identified by the untrained. This kind of subject could be found anywhere, recruited indiscriminately, employed briefly, and in large volumes. The usability of these subjects entailed practical benefits—easy and rapid implementation of the method—but it also underscored the epistemological qualities of the method: an 'endpoint' that could be achieved *consistently* without particular training was immune to individual variations in skill, personality and expectation. A method that could be used successfully with little skill was presented as more trustworthy.

As Hardy, Wolff and Goodell may have anticipated, their self-experimentation was indeed accused as the secret of their success. Their first critics were researchers from Northwestern University who had designed a method of measuring pain thresholds that delivered electric shocks to the teeth of subjects. The tooth pulp, they argued, was an ideal target for stimulation because it was innervated solely by pain receptors, making the threshold impossible to confuse with irrelevant sensations of warmth or pressure. Such confusion could lead to variation, which had to be eliminated by training, as they explained in a thinly veiled attack on the dolorimeter:

> If an unsuitable stimuli, or an unsuitable receptive field, or both, are selected, the subjects are unable to discriminate sharply between the sensation of pain and the sensation which follows the simultaneous, quantitatively and qualitatively, inconstant stimulation of other afferent systems. To obtain more accurate results it was necessary, therefore, to use

> only 'trained subjects', and in many instances investigators could conduct their algesimetric studies only on themselves.[443]

In contrast, their own method eliminated the necessity of using specially 'trained subjects'"[444] During the 1940s, it was this research team—not the proponents of clinical trials—that challenged the superiority of the dolorimeter as an analgesic testing method. That the competition was another, essentially similar method using 'normal' subjects and 'experimental pain,' is proof of the perceived desirability of such methods during this decade. It also meant that the dolorimeter was criticised on its own terms: the validity and precision of a pain threshold measuring method was enhanced by using a stimulus that was easily, distinctly perceived by the subject. It was on these grounds that the Northwestern team made subject-training seem like a serious flaw.

Meanwhile, however, other investigators were training their dolorimetric subjects without making a fuss about it, seemingly accepting this as a reasonable condition for making the apparatus function optimally. Already by 1943, H. L. Andrews, a physiologist at the Lexington Public Health Service Hospital, reported that his subjects, who were "post-addicts" (narcotic drug users who had been clean for 6 months or more), were given a preliminary period of training to familiarise themselves with the use of the apparatus. A few subjects were also excluded from the trial because of their "inability to distinguish the various stimulus intensities."[445] E. G. Gross at the University of Iowa used medical students, who were "found to be particularly reliable in these studies because of their great interest in the drug effects." He trained these subjects for about 10 days, "until they could consistently recognise the normal end point... Provided the end point is well defined in the mind of each volunteer the results obtained are

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[443] Goetzl, Burrill and Ivy, "A Critical Analysis of Algesimetric," 280-291.
[444] Goetzl, Burrill and Ivy, "A Critical Analysis of Algesimetric," 280-291.
[445] H. L. Andrews, "The Effects of Opiates on the Pain Threshold of Post-Addicts," *Journal of Clinical Investigation* 22 (1943): 511-16. See also: Andrews, "Skin Resistance Changes and Measurements of Pain Threshold," *Journal of Clinical Investigation* 22 (1943): 517-20: the subjects were probably kept for this following experiment in which the subjects were reported to have "had considerable experience with the experimental procedure before being used in the present study."

remarkably consistent." [446] Scott and his colleagues reported, adding no further comment, that "with both dogs and men considerable preliminary training was needed in order to obtain consistent and reliable data."[447]

For Andrews and Gross, training subjects did not present great difficulties. Their subjects were available for multiple sessions; they were generally healthy and lucid enough to follow instructions closely. Training did pose problems, however, when use of the apparatus was transferred to clinical settings. Having attempted to use the technique to obtain more objective data on pain relief in cancer patients, a team of pharmacologists at Memorial Hospital in New York reported that: "It was exceedingly difficult to train these patients, since most of them were desperately ill... In the few that we were able to train, the results were so frequently inconsistent that it was impractical to continue the procedure."[448] Training was thus seen by some as a reasonable—if not always practical—means of reducing the range of variation of subjects' 'normal' responses before submitting them to experimental trials.

Other investigators instead saw variability in normal thresholds, rather than training, as the reasonable concession they had to make in using the dolorimeter. Having found a discrepancy between the consistency of their data and the Cornell team's, Slaughter and Wright concluded: "This difference, we feel, is what one might reasonably expect. It is certain that, clinically, humans differ considerably in their reaction to pain—it is therefore quite likely that 'normal' pain-threshold responses differ in different individuals." [449] Indeed, not all had accepted Hardy, Wolff and Goodell's assertion that the pain threshold was

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[446] E. G. Gross, H. L. Holland and F. W. Schueler, "Human Studies on Analgesic Piperidine Derivatives," *Journal of Applied Physiology* 1 (1948): 298-303.
[447] Scott, Kohlstaedt, and Chen, "Comparison of the Pharmacologic," 12-17.
[448] R. W. Houde, L. H. Rasmussen and J. S. LaDue, "Preliminary Experience in the Use of Some of the Newer Analgesics in the Relief of Pain Due to Cancer," *Annals of the New York Academy of Science* 51 (1948): 161-74. See also: J. Clausen, H. E. King, "Determination of Pain Threshold in Untrained Subjects," *Journal of Psychology* 30 (1950): 299-306.
[449] Slaughter and Wright, "A Modification of the Hardy-Wolff-Goodell,"115-19

in fact constant. They attributed variability in results to the reality of the threshold itself rather than to the failure of the method of measurement. [450]

Hardy, Wolff and Goodell themselves had eventually conceded that trained subjects were more reliable in establishing the "occurrence" of pain, and that untrained subjects "even of high intelligence" were unsuited for analgesic studies with the apparatus.[451] Thus, they salvaged an older model of the ideal subject as an expert and neutral observer. They had long maintained that a good subject was one who was able to focus and to maintain an objective attitude towards their sensations. Over-reaction, anxiety, suggestibility and prejudice towards the experimental situation made a subject's judgment unreliable: these were the causes of erratic fluctuations in pain thresholds and of errors in the measurement of analgesic effect. [452] In later publications, they described the process of instruction, which consisted in telling the subject what the endpoint should feel like, and of encouraging emotional detachment from the experimental situation by telling them not to consider this a test of endurance, nor to try to please or displease the operator. In addition, subjects were made to practice until their "estimates of pain threshold are consistent under various circumstances," at which point they were considered to be "trained in the analysis of the sensation."[453] This training was compared to acquiring the proficiency of a technician in reading their own interior sensations: "The instruction given is in the same category as that given to a student to enable him to read an endpoint in a titration, or on a colorimeter, or to read a Vernier Scale."[454] Such a subject produced higher quality data. Training "provide[d] a more uniform index for observing the changes brought about by experimental procedures" and thus

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[450] J. R. Schamp and R. M. Schamp, "Variability of the Pain Threshold in Man," *Journal of Dental Research* 25 (1946): 101-4; L. H. Lanier, "Variability in the Pain Threshold," *Science* 97 (1943): 49-50.
[451] J. D. Hardy and M. Cattell, "Measurement of Pain Threshold-Raising Action of Aspirin, Codein, and Meperidine (Demerol)," *Federation Proceedings* 9 (1950): 282; Hardy, *Pain Sensations and Reactions*.
[452] Wolff and Hardy, "On the Nature of Pain," *Physiological Review* 27 (1947): 167-99.
[453] Hardy, "The Nature of Pain," *Journal of Chronic Diseases* 4 (1956): 22-51.
[454] James D. Hardy, *Pain Sensations and Reactions* (Baltimore: Williams & Wilkins, 1952).

lowered the threshold of significance of data: 5-10 % changes in pain intensity for trained subjects, in comparison with 20% or more in untrained subjects.[455]

By the end of the decade, the training of subjects seems to have been fairly widely accepted as a feature of the standardisation of the use of the Hardy-Wolff-Goodell method. Though striving for the highest level of automaticity and impersonality in their method, the designers of an improved version of the radiant heat apparatus admitted that they did "not feel that one can take a person off the street and make accurate observations for the comparison of one drug with another." Because of the subjectivity of the pain sensation, a period of 'familiarization' with the apparatus was necessary.[456] A review of analgesic testing methods suggested that some researchers' lack of success in using the apparatus might have been caused by a failure to train subjects. In support of this conclusion, Miller reported Gross' comments that "with training, each subject comes to recognise the intensity of heat stimulus marking his threshold and that the threshold is very nearly the same for all subjects." Even rats, Miller remarked, had to be trained to use the procedure.[457] Training provided higher accuracy, making it possible to economise on the number of subjects and the volume of data produced, while also making subjects more neutral, more precise and less susceptible to the influence of irrelevant information.

Beecher, however, attacked subject-training as dangerous: "The failure to eliminate [trained subjects'] bias can have devastating results," wrote Beecher, and, though learning had to be watched for in any subject, "the hazard is far greater with the experienced group."[458] A trained subject could not be properly blinded to the identity of the drug under study, and was therefore more susceptible to placebo effects. In the course of training, subjects came to have a vested interest in the outcome of the study, whether scientific, monetary or personal. Training, for Beecher, was the source of many biases. Only "green" and ignorant subjects were trustworthy. But how could the judgment of such naive subjects be

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[455] Hardy, *Pain Sensation*, 82-83.
[456] Flinn and Chaikelis, "An Improved Instrument," 349-50.
[457] Miller, "A Critique of Analgesic Testing," 34-50
[458] Beecher, "Pain –Controlled and Uncontrolled," 167.

made to yield useful and accurate information about analgesic efficacy? Beecher's naïve subject was not, in himself, a source of precise information about pain and analgesia.[459] Nor was he immune to suggestion and expectation. In addition, his experience could not be considered to be representative of the efficacy of a painkiller. No single individual had the authority, whether derived from training or the use of a particular instrument, to serve as spokesperson for the general pain experience, or for average analgesic efficacy. A new type of subject had to be created, a composite subject whose "collective pain" could be measured through the coordinated and controlled collection of data in a clinical setting.[460] This happened in the late 1940s.

5.4 Displacing the Dolorimeter: The Emergence of the Analgesic Clinical Trial

Surprisingly, an increase in funding and resources for analgesic evaluation seems to have initiated the demise of the dolorimeter. While the synthetic analgesics initially attracted the interest of individual pharmaceutical firms, they eventually stimulated larger-scale investments of time and money into analgesic testing. The emergence of new interests in analgesic testing, and the re-organisation of old ones, resulted in larger grants, given over longer periods to clinical researchers, thus creating conditions in which "clinical pain" could be made to yield reliable and consistent information about analgesic efficacy. Support for clinical analgesic testing was also provided by therapeutic reformers during a crucial period in the promotion of new methods of therapeutic evaluation. Though I will describe the emergence of the analgesic clinical trial in greater detail in chapter 6, I will briefly described the new forms of support for clinical analgesic testing that appeared in the late 1940s and which help to explain the displacement of the dolorimeter.

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[459] The masculine is used deliberately here since Beecher generally excluded women from his analgesic trials, see: Beecher, "Experimental Pharmacology,"157.
[460] Keats, Beecher and Mosteller, "Measurement of Pathological Pain," 35.

The potential usefulness of methadone as a substitute for opiate-based drugs had not gone unnoticed by the U. S. Army. World War II had raised the spectre of a possible "national emergency" due to the cutting off opium supplies, a fear that remained present in the first years of the cold war.[461] During the years after the war, the Army and Navy Munitions Board sought definitive advice on the necessity of stockpiling opium, and the possibility of replacing standard analgesics with the new synthetics such as methadone. While the Bureau of Narcotics had given them advice on this point, the Surgeon General of the Army, R. W. Bliss stated that "professional military personnel [were] reluctant to accept the statements expressed by the Bureau of Narcotics at this time due to the fact that extensive clinical and practical experience has not been obtained..."[462] It is not immediately obvious why the military insisted on obtaining clinical evidence. A likely reason was that they were more concerned with the general therapeutic value of these drugs and their acceptance by physicians and patients than with precise calculations of ratios of analgesic potency to addictive liability that had been useful in process of designing a safer analgesic. [463]

In order to obtain this "badly needed information about analgesics,"[464] the Medical Research and Development Board (MRDB) of the U. S. Army provided Henry K. Beecher with funding to run his first large-scale clinical trial of the isomers of methadone at the Massachusetts General Hospital (MGH). This funding came as part of a larger grant for the Study of Sedatives that also included other matters of military interest such as the use of barbiturates for sleep and narcoanalysis. Further military support was given to Beecher when he expressed

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[461] Minutes of the meetings of the Committee on Drug Addiction and Narcotics, successor of the CDA, are filled with requests from the Army for advice on its opium stockpiling policies. Military leaders wanted to know whether synthetic analgesics were adequate in replacing the opium-based standard remedies.

[462] "R. W. Bliss, Surgeon General to Dr Weed (NRC), August 5, 1947," in *Bulletin of the Committee on Drug Addiction and Narcotics* (1947):12.

[463] This letter (R. W. Bliss, Surgeon General to Dr Weed (NRC), August 5, 1947) specifically asked for the opinion of the Bureau of Narcotics on the general acceptance of the synthetic analgesics by the medical profession, in addition to whether this class of drugs could satisfy military and civilian needs in case of emergency, as well as whether the Office should consider reducing its stockpile of natural opiates.

[464] William S. Stone to Henry K. Beecher, January 5, 1950, Beecher Papers, MS C 64, Box 22, Folder 15, Countway Library of Medicine.

an interest in conducting a field trial of these substances on the battlefields of the Korean War in 1950.[465]

Beecher's methodological work, developed in collaboration with Jane Denton, subsequently received additional sources of support. Their results were given wide exposure and a stamp of approval by the Therapeutic Trials Committee of the AMA's Council on Pharmacy and Chemistry. Two articles were published in the *Journal of the American Medical Association* (JAMA) at the invitation of the Committee's secretary, and were prefaced with a statement approved by the Council that underlined the importance of their achievement. Denton and Beecher's work had represented "a distinct advance in the methods available for quantitative evaluation of the therapeutic efficacy of [analgesic and narcotic drugs]." These advances were all the more significant since they dealt with a class of drugs that presented "difficulties not encountered ordinarily in the appraisal of other therapeutic agents."[466] The Council's support associated Beecher's work to a movement of therapeutic reform to instil specific values, and techniques—particularly those of the randomised clinical trial—in American drug testing. This movement has been described as a current of elite activism for the promotion of a "rational therapeutics" that would be dictated by the norms of scientific evidence and medical professionalism, and protected against the excessive commercial aspirations of the pharmaceutical industry.[467] Unlike the Army, the AMA's was interested in Beecher's methodological innovations rather than in the precise potency of these new analgesics. When Beecher submitted a report of the results of his field trial of methadone in Korea, JAMA rejected the article on the basis of its "limited appeal to readers" when morphine was readily available.[468]

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[465] Beecher to Stone, August 15, 1950, Beecher Papers, MS C 64, Box 22, Folder 15, Countway Library of Medicine; Beecher to Stone, January 27, 1951, Beecher Papers, MS C 64, Box 22, Folder 16, Countway Library of Medicine.
[466] Walton Van Winkle Jr., "Report to the Council," *Journal of the American Medical Association* 141 (1949): 1051.
[467] Harry M. Marks, *The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900-1990* (Cambridge and New York: Cambridge University Press, 1997).
[468] *Journal of the American Medical Association* to Beecher, April 2, 1951, Beecher Papers, MS C 64, Box 26, Folder 54, Countway Library of Medicine.

Though Beecher designed and first ran analgesic clinical trials thanks to military funding, this technology was further refined and diffused under sponsorship from the Committee on Drug Addiction and Narcotics (CDAN), the successor of the CDA. As I will argue in the next chapter, it was under the conditions provided by the Committee and its allies that the clinical trial became an effective and valuable analgesic testing technology. The Committee had been brought back to life as a response to the military, commercial and regulatory interests in the new synthetic analgesics. CDAN was no longer involved in creating new substances. It now aimed to "provide impartial advice to government," such as advice to the military about analgesic drug stockpiling policies, as well as to the Public Health Service (PHS) and Federal Drug Administration (FDA) on the addictive liability of new drugs. To fulfil this role, Committee members emphasised the need to sponsor the creation and use of analgesic testing methods, both for therapeutic efficacy and addictive liability, which would be used to evaluate the analgesics generated by the pharmaceutical industry.

Beecher's method appeared particularly promising to the members of the new committee, but they knew they would have to create new alliances to fund its further development and sought the support of pharmaceutical firms. Indeed, at a meeting with key committee members, pharmaceutical industry representatives expressed particular interest in the development of a more reliable test of pain-relieving efficacy in humans. However, as one representative stated, no single company was willing to underwrite the substantial, long-term funding it would take to do this.[469] By pooling together the contributions from major U. S.-based drug companies, CDAN was able to continue funding Beecher's and others' work on analgesic clinical trials until the mid-1960s.

As a clinician, and particularly as an anaesthesiologist, Beecher had also provided new resources for analgesic testing. As Chief of the Anesthesia Service

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[469] "Minutes of Conference with Representatives of Drug Manufacturers, July 1, 1949," Box 1: Minutes, July 1949 Conference with Representatives of Drug Manufacturers, Committee on Drug Addiction and Narcotics (CDAN), NASA.

at Massachusetts General Hospital, Beecher was responsible for its operative patients. This gave him ready access to a plentiful supply of subjects. Beecher also had the necessary authority over patient care and his staff, which included both a clinical and research staff, to ensure that the correct experimental drugs were administered according to a specific protocol. An essential means of controlling for suggestion and expectancy was to include placebos in the series, and to ensure that each drug was unknown to nurses, observers and patients. For this to be done properly, it was essential to obtain the cooperation of the staff, including the hospital pharmacy which prepared and labelled the doses with secret codes.

Clinical trials also required the collaboration of consultants and observers. While previous clinical studies had relied on busy clinicians, whether nurses or physicians, to carry out this task, Beecher hired full-time observers. This was an important means of eliminating additional variation. Special observers were more apt to follow instructions, to accept being "blinded," to question patients consistently and neutrally, to follow strict schedules and to assure continuity between interviews. But full-time observers, whether they were technicians or specially-hired nurses, were not cheap. Over the years, Beecher and subsequent clinical experimenters would spend a large proportion of their research grants on the salary of their observers.

Once large amounts of data had been collected under comparative and controlled conditions, it would not be intelligible until it had been statistically analysed. Variability in patients' experiences of pain and relief needed to be managed by a professional statistician. For this, Beecher sought the guidance of Frederick Mosteller, a Harvard colleague, thus helping to launch his career as one of the prominent pioneers of American biostatistics. Beecher also paid Mosteller consultant fees from his grants. Statistical expertise was required not only for manipulating results, but also for designing the method so as to produce data that would be optimal for statistical analysis. In the end, Beecher was able to attain a high level of quantitative precision: reporting relief in percentages and plotting

dose-effect curves that gave clear distinctions between the efficacy of test and standard analgesics.

Beecher's clinical authority as well as his use of paid full-time staff and consultants allowed him to standardise clinical testing conditions to a greater extent than the CDA clinical studies of the late 1930s. In addition, Beecher began his studies in a single site, and the diffusion of his technology to multiple sites was slower and more tightly controlled than either the CDA clinical studies or the use of the dolorimeter. Thus, as these new resources became available for clinical analgesic evaluation in the late 1940s, new techniques for controlling pain and its measurement became possible. Beecher was able to show that the clinic and its supplies of suffering could be brought under experimental control. The gap between animal experimentation and clinical studies, into which the Hardy-Wolff-Goodell dolorimeter had fit so well, began to narrow.

Conclusion

The faults Beecher imputed to the dolorimeter must be situated in the context of this story about how various groups demanded, supported and operated methods to measure pain and evaluate analgesics from the mid-1930s to the early 1950s. Not only did the dolorimeter correspond to requirements and values of earlier sponsors, but it is clear that the resources that were necessary to develop and implement a method such as Beecher's were not made available before the late 1940s. Beecher benefited from new sources of support for analgesic testing—the Army and the AMA's Council on Pharmacy and Chemistry—who expected different things from pain-measuring methods than had previous sponsors. While the CDA and pharmaceutical companies in the 1930s and1940s needed rapid and precise information to guide pharmacological innovation and addictive liability testing, the Army wanted to know whether methadone would be accepted as a morphine substitute by those who would use it to relieve pain, and the AMA wished to ensure that analgesic drug evaluation would be controlled by rational principles and by clinical researchers.

When the CDAN was reconvened in the late 1940s, its focus was no longer on pharmacological innovation but rather on methodological innovation in drug testing. The Committee also had to develop a new funding mechanism, and sought the support of those who were then willing to invest in better methods of analgesic testing: pharmaceutical firms. Through the Committee, contributions from industry were combined into larger chunks and channelled towards promising, if expensive and time-consuming projects such as Beecher's. Beecher himself also made new resources available for analgesic evaluation: an anaesthesia research laboratory, access to an abundant source of post-operative patients, and a team of cooperative staff and consultants.

In the early to late 1940s, and then in the late 1940s to 1950s, two different configurations of money, research settings, experimenters, subjects, expertise, and demands for specific types of information, favoured two different types of analgesic-testing technologies. The Hardy-Wolff-Goodell dolorimeter was well suited to the production of relatively cheap but low-volume data using few subjects. The method offered results that were highly precise at the level of the individual through the exercise of a focused type of psycho-sensory control on the evaluation of pain through instrumental means. By contrast, Beecher's analgesic clinical trial produced high volumes of relatively expensive data. It exercised collective control through procedural and statistical means that depended on money and rules, as well as the authority to enforce them, targeted towards the coordination of labour.

By controlling the evaluation of pain relief in different ways, each method was designed and operated with a different notion of what pain was, and especially of how the experience of pain was susceptible to experimental variables and experimental interventions.[470] Indeed, an important issue in the

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[470] Davis Baird, *Thing Knowledge: A Philosophy of Scientific Instruments* (Berkeley: University of California Press, 2004), 67-88; W. D. Hackmann "Scientific Instruments: Models of Brass and Aids to Discovery," in *The Uses of Experiment*, 31-65, makes a distinction between passive and active instruments, the former being instruments that measure naturally occurring phenomena, while active ones produced novel ones; J. A. Bennett, "A Viol of Water or a Wedge of Glass," in *The Uses of Experiment*, 105-114, criticizes this distinction by pointing out, using the barometer as an example, that many "passive" instruments interact with theory. He concludes that such a

debate between the Cornell and Harvard teams concerned the "true" nature of pain. As we have seen, Hardy, Wolff and Goodell maintained that the experience of pain could be divided, in theory but *also* in practice, into sensation and reaction. While the reaction of pain was modulated by the idiosyncratic characteristics of the experiencing subject—personality, past, emotional status, interpretations—the sensation of pain was stable. Pain sensation was conceptualised as a straightforward trajectory of impulses travelling from the stimulus through the sensory apparatus to the perceiving subject. In this model, the intensity of stimulation bore a stable relationship to perceived intensity. That is, as long as "reactive" elements were eliminated though the use of an appropriate stimulus as well experienced, self-disciplined subjects, and given a normal sensory apparatus, equally sensitive to any other set of normal nerves.[471]

Beecher, however, argued that pain was indivisible at the level of experience. The "reaction" to painful stimulation began as soon as the organism was processing sensory information, and thus preceded conscious awareness of an experience of pain. Thus, the essential sameness of pain sensation in different subjects was theoretical, while the inevitable reaction was, for Beecher, "never alike for any two individuals and, indeed, with the passing of time and accumulation of life experience, is never exactly the same for the same individual from one time to another."[472] This idiosyncratic reaction was thus an unavoidable and even crucial component of any experience worth calling "pain." The dolorimetric pain threshold, as Beecher saw it, was in reality a mixture of sensation and reaction that paraded itself as pure sensation, and thus was both distorted and misleading. To find true pain, one had to look for it in its natural habitat: the patient who was suffering from an injury or disease. Dolorimetric pain was neither a representative model of this pain nor a discrete component of it, and thus there was no justification for its use in analgesic testing. Especially since, as

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distinction isn't useful, and that the taxonomies in use during the historical period being studied should be preserved by the historian.

[471] The implications of the Cornell team's method for their model of pain can be best discerned from: Hardy, *Pain Sensations and Reactions*.

[472] Beecher, "Limiting Factors in Experimental Pain," *Journal of Chronic Diseases* 4 (1956): 11-21.

Hardy, Wolff and Goodell themselves had argued, analgesics seemed to act largely on the reaction rather than the sensation of pain.[473]

Hardy, Wolff and Goodell's method intervened in the subject's experience of pain through the manipulation of the stimulus, but Beecher argued that there was no direct or predictable correlation between intervention and experience. There were simply too many factors that could influence the experience of pain, least of which was the intensity of stimulation. Indeed, Beecher firmly rejected the principle of proportionality between the severity of injury or the intensity of stimulation and the intensity of the pain experience.[474]

It has been suggested that Beecher adopted this position as a response to his observations during World War II.[475] In the course of his military service, Beecher had noticed that many badly wounded soldiers refused morphine and denied experiencing pain. He concluded that strong emotion, rather than any modification of sensitivity, accounted for the disproportionality between the intensity of pain and the severity of the wound.[476] There are other reasons why ideas about pain might have been changing in analgesic evaluation. The shift from the psycho-physiological notion of pain advocated by Hardy, Wolff and Goodell to Beecher's more emotional, holistic and idiosyncratic 'clinical pain' seems to have announced a broader theoretical shift in the conceptualization of pain from mechanistic schemas to more cognitive, integrative and individualised ones. The roots of these new models of pain have been linked back, for example, to the clinical work of William K. Livingston and John J. Bonica with chronic pain and nerve-injured patients during and after World War II, Bonica's model of the interdisciplinary pain clinic that he described in the 1950s and implemented in the

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[473] Wolff, Hardy and Goodell, "Studies on Pain: Measurement of the Effect of Morphine," 659-680.

[474] Keats, Beecher, and Mosteller, "Measurement of Pathological Pain," 35. See also: note 25 of this chapter.

[475] Meldrum, "Each Patient His Own Control,"; "'Departures from the Design'," 287-288.

[476] Though the specific explanation given by Beecher was later replaced by reference to the action of endogenous opiates (such as endorphins), this study would often be cited by researchers who sought to reform conceptions of pain by pointing out that sensory-physiological events were only a small, sometimes insignificant part of the total subjective pain experience.

1960s, and the publication by Ronald Melzack and Patrick Wall of the gate-control theory of pain in 1965.[477]

The way in which pain was defined for and by the analgesic clinical trial participated in these broader shifts, drawing on, but also actively contributing to new models of pain.[478] Beecher did not simply argue that his was a more realistic definition of pain than Hardy, Wolff and Goodell's; he also made it manageable for analgesic experimentation and thus made the more mechanistic definition no longer useful in this context.[479] The idea that pain was subjective, emotional and idiosyncratic was news to no one, especially not to the Cornell team researchers, who were active in psychosomatic research on stress, emotion, pain and disease in the 1940s and 1950s. This idea predated the analgesic clinical trial, and even the wartime observations of Beecher, Bonica and Livingston. But the idea that such an experience could be measured reliably and objectively without being reduced to something simpler, without being transformed into a more predictable and less personal phenomenon, was indeed new. It was a product of the successful use of

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[477] A good description of changing conceptions of pain in theory and therapy is given by Isabelle Baszanger, *Inventing Pain Medicine From the Laboratory to the Clinic* (New Brunswick, N.J: Rutgers University Press, 1998). See also original works by these key clinicians and scientists: William K. Livingston, "What Is Pain?" *Scientific American* 196 (1953): 59-66; Livingston, *Pain Mechanisms: A Physiologic Interpretation of Causalgia and its Related States* (New York: MacMillan, 1944); John J. Bonica, *The Management of Pain With Special Emphasis on the Use of Analgesic Block in Diagnosis, Prognosis, and Therapy* (Philadelphia: Lea & Febiger, 1953): Bonica defined a new clinical entity, chronic pain, which was characterised not only by its duration but by its psychological components, which developed over time even if the pain had a physical origin. This type of pain affected the whole person and thus required a multidisciplinary clinical approach in a team including psychologists and psychiatrists; Ronald Melzack and Patrick Wall, "Pain Mechanisms: A New Theory" *Science* 150, no. 699 (1965): 971-979, this theory of pain drew on a more dynamic and integrative model of the nervous system, in which pain signals could be modulated at various levels before they reached consciousness. Thus, factors such as past experience, meaning, interpretation, emotion, etc. could act on the transmission of impulses in the nervous system and thus determine what information reached the brain to be perceived as '

[478] I made a similar argument in the preceding chapter, arguing that new techniques for measuring psychosomatic relations made it possible to measure new kinds of responses to pain, and thus to redefine pain. The analgesic clinical trial also redefined what counted as measurable pain in a similar way, giving emotions, psychosomatic processes, as well as individual pasts and psychological make-up greater importance in the pain response. However, while the techniques I described in the previous chapter attempted to isolate and correlate these variables, in the analgesic clinical trial they were controlled without being specified in order to make the measurement of analgesic effect more valid.

[479] Keats, Beecher,and Mosteller, "Measurement of Pathological Pain,"35.

specific technologies of data collection and analysis, of practices that depended on certain kinds of authority, as well as material and human resources.

Beecher's rich conception of pain not only justified the analgesic clinical trial but was also justified by it. To Beecher, the factors that made every experience of pain different—emotion, interpretation, memory, attention—were too numerous, too constitutive of the very experience of pain, and of its relief, to be eliminated at the level of the individual. Yet, this variability proved itself to be regular in the bird's eye view. When data was carefully collected, under comparative conditions, from large numbers of ignorant patients, by meticulous and constant observers, and then expertly analysed, it revealed regular patterns.[480] The distribution of clinical data on analgesic efficacy was normal. It clearly distinguished between placebos and active drugs. And, as it turned out, it was reproducible.

The persisting search for cheaper laboratory-based analgesic testing methods would seem to confirm a link between operational definitions of pain and the costs of testing practices. Controlling the evaluation of clinical pain relief was expensive and time-consuming, and researchers, including some who had strongly advocated analgesic clinical trials, did not abandon hope of developing effective laboratory-based pain-measuring techniques. Even Beecher, who had declared experimental pain to be incommensurable with clinical pain in the early 1950s, sought, in the 1960s, to recover a form of experimental pain that could be more easily controlled and manipulated for analgesic testing. The method he developed was a tourniquet pain test, in which the "normal" subject was required to perform a certain amount of work with muscles to which the blood supply was cut off. This induced an experience of pain which, because it was sustained and included an element of anxiety, more successfully mimicked clinical pain. But it was not clinical pain: It could be produced and manipulated predictably under laboratory conditions, at an individual level, in normal subjects.

Such as test was important, Beecher explained in 1966, because: "practically, there is great need for a method of appraising in man, conveniently

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[480] Louis Lasagna, "The Lawfulness of Clinical Pain," *Federation Proceedings* 20 (1961): 309.

and accurately, the effectiveness of new pain-relieving agents. There is great need for a method which will not require the tedious use of pathological pain. Thus, the present findings appear to have wide usefulness to the pharmaceutical industry."[481] Conscious of the realities of the social and material conditions of analgesic testing, and of the demand for quicker, cheaper methods, Beecher had been willing to attempt to redefine what counted as an acceptable and measurable pain.

[481]Beecher, "Pain: One Mystery Solved," *Science* 151 (1966): 840-841.

6. Working with Clinical Trials: the Committee on Drug Addiction and Narcotics and the Measurement of Analgesic Effect, 1947-1965.

By the late 1940s, the possibilities offered by fully synthetic analgesics, such as Demerol and methadone, had awakened new hopes, and new worries. Pharmaceutical manufacturers were attracted by the possibility of lucrative new products. The dream of a more effective, less addictive analgesic continued to animate medical, public health and drug enforcement circles. Hoping to eliminate the need to stockpile opium in the eventuality of national emergencies, the Surgeon Generals of the Army and Navy began pressing the Bureau of Narcotics and the National Research Council for reliable information on the efficacy of synthetics.[482] Some groups responded to this enthusiasm with caution: Elite physicians, public health officials and regulatory agencies were concerned that the market might be flooded with badly tested drugs of dubious efficacy and unknown addictive liability.[483]

Under pressure to respond to these concerns, the National Research Council (NRC) decided to re-establish a committee to deal with narcotics research. The former Committee on Drug Addiction (1929-1940), which had been maintained as a relatively inactive advisory committee during the war, was revived in 1947 as the Committee on Drug Addiction and Narcotics (CDAN). Its initial membership was composed of a core group of elite research

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[482] "R. W. Bliss (Office of the Surgeon General) to Dr. Weed (NRC), August 5, 1947," *Bulletin of the Committee on Drug Addiction and Narcotics* (1947), 12; "Dr. Swanson (Office of the Surgeon General of the Navy) to Dr. Weed (NRC), August 11, 1947," *Bulletin of the Committee on Drug Addiction and Narcotics* (1947), 15.

[483] "Minutes of Conference with Representatives of Drug Manufacturers, July 1, 1949," Minutes, CDAN, NASA: In his introduction to the meeting, Isaac Starr described the function of the Committee as giving impartial advice to government. Recently, however, the activities of the committee had expanded because, as Starr explained: "For example, the government is confronted with the problem of addictive drugs. The market is likely to be flooded with a large number of addiction-producing derivatives. The Bureau of Narcotics consulted the Committee and as a results a program for the testing of the narcotic liability of drugs was initiated. This has proved to be a service, the Chairman pointed out, both to industry and government. It is in connection with this extension of interest that this meeting was called."
See also, "Minutes of the 1st Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1947), 6.

pharmacologists.[484] Among these were the Committee's new Chairman, Isaac Starr, Professor of Therapeutic Research at the University of Pennsylvania Medical School, and its secretary Nathan Eddy, now Principal Pharmacologist at the NIH. The Committee was completed by the Chief of the Laboratory of Chemistry of the NIH, the Assistant Chief of the PHS Division of Mental Hygiene, the Director of the Public Health Research Institute of the City of New York and the U. S. Commissioner of Narcotics.

In 1947, the members of CDAN met for the first time, in the company of representatives of the U. S. Army and Navy, the NRC, the FDA, the American Drug Manufacturers' Association and the AMA's Therapeutic Trials Committee, to discuss the future of narcotics research and the function of the new Committee. One of the principal topics of discussion at this meeting, and subsequent ones, was the evaluation of drug safety and efficacy. How could the Committee keep tabs on the industry's activities and provide the necessary guidance to ensure objective and accurate testing of new narcotics? Manufacturers, it was agreed, should be requested to provide information about new narcotics to the Committee. It also seemed important to advise manufacturers on where, and by whom, narcotics testing could be done competently.

At the following meeting, however, discussion shifted from the selection of competent investigators to the lack of reliable methods for testing narcotic drugs. It became clear that, in order to fulfil its function as an advisory committee, CDAN would have to lead a quest for new drug testing methodologies. While addiction-liability testing was still an important concern, the evaluation of pain-relieving efficacy stood out as a particularly thorny problem. CDAN members agreed that the Hardy-Wolff-Goodell dolorimeter was inapplicable to large groups of subjects. A more promising option for analgesic testing was Henry K. Beecher's work on the analgesic clinical trial, begun under army funding at the

[484] These included: Isaac Starr, Raymond N. Bieter, Nathan B. Eddy, and Maurice H. Seevers.

Massachusetts General Hospital (MGH).[485] Beecher's funding, however, was about to run out.

After considering various potential sources of funding this quest, CDAN turned towards the pharmaceutical industry.[486] Eddy invited industry collaboration in an article published in *Drug and Allied Industries,* in which he emphasised that pharmaceutical firms should be interested in sponsoring narcotics research for their own benefit.[487] The mutual interest of industry and CDAN in analgesic testing methodologies was confirmed: In a letter to Eddy, Edward Henderson, of the Schering Corporation, expressed his interest and suggested that a common fund be established with the collaboration of other pharmaceutical firms.[488] Months later, a meeting was held between key members of CDAN and twenty representatives of the pharmaceutical industry.[489] Industry representatives saw analgesic measurement as a crucial issue but also as an expensive proposition. According to the meeting's minutes, Dr. Spoor of Bristol-Myers Laboratories stated that: "there is no adequate test…for analgesic action of a drug

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[485] See previous chapter for more information about Beecher, and how he first developed his clinical trial methodology under army funding.

[486] Among other possible sponsors considered by committee members were the National Institutes of Health (NIH), the Bureau of Narcotics, the Research Grants Division of the NRC, the American Society of Anesthetists, and a new "Analgesic Foundation."[486] They managed to get some temporary NIH funding, but apparently could not get it renewed. See: "Minutes of the 2nd Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics*, 25.

[487] Nathan B. Eddy, "Cooperation in Narcotics," *Drug and Allied Industries* (1949), in which he explained the nature of CDAN and asked pharmaceutical firms to seek advice from, as well as provide data to, CDAN, and also proposed the constitution of a research fund for projects on analgesia.

[488] "Minutes of Conference with Representatives of Drug Manufacturers, July 1, 1949, Appendix A: Edward Henderson to Eddy, April 6 1949," Minutes, CDAN, NASA: Henderson, of the Schering Corporation, shared with Eddy the difficulties faced by the pharmaceutical industry in doing research on analgesics, of which "one of the most remarkable difficulties in this field is the absence of a reliable measure of pain response in man." Henderson also added that: "the budget proposed for the study of analgesics through the NRC does not appear large. It is possible that the only way in which basic studies (such as determining the laboratory measure most closely corresponding to clinical analgesia) could be supported is by contributions to such a common fund."

[489] "Minutes of Conference with Representatives of Drug Manufacturers, July 1, 1949," Minutes, CDAN, NASA: in attendance were representatives from the American Drug Manufacturer's Association, Winthrop-Stearns, E.R. Squibb and Sons, Sharpe and Dohme, Schering Corporation, Shenley Laboratories, Parke Davis and Co,. New York Quinine & Chemical Works, Merck Research Institute, Maltbie Chemical Co., Lilly Research Laboratories, Ciba Pharmaceutical Co,, Burroughs-Wellcome & Co., J. T. Baker Chemical Company, and Bristol Laboratories. From CDAN, were present its chair, Isaac Starr, its secretary, Nathan B. Eddy and one of its members, Lyndon Small, chemist

in man... but his company alone cannot afford the basic research necessary to develop such a test which he estimated would require the expenditure of perhaps $50,000 per year over a ten year period."[490] This statement was echoed by other meeting participants who, while they recognised the importance of improving addiction-liability testing, gave analgesia assessment a higher priority. And they clearly identified analgesic research in humans to be more meaningful, even if potentially more expensive, than tests in animals: a vote came out 7-3 in favour of human studies. Wasn't it possible to get the Public Health Service to fund these studies, enquired the representatives of Shenly Laboratories and of Sharpe and Dohme? The PHS had turned down the projects recommended by the Committee, said Eddy, and the Committee didn't have sufficient funds to support potentially promising research programs. If the pharmaceutical industry wanted to benefit from these studies, they would have to take matters into their own hands.

So, at a time when public institutions refused to fund these projects, the Committee was able to tap into industry interests in order to fund analgesic drug evaluation. And, while individual companies were unwilling to fork out sums sufficient to make these projects viable, CDAN channelled their contributions into a combined pool to be administered as research grants. Pharmaceutical companies would benefit by obtaining new methodological tools, but also by associating themselves with the neutrality and prestige of the National Research Council. From 1950 to 1960, pharmaceutical firms provided the entirety of CDAN's research budget, and continued to contribute a significant amount—ranging from $73,125 to $198,225—until 1970.[491] From 13 contributing firms in 1950, the number grew to 51 in 1970. Contributions from the Office for Civil Defense (OCD) and the Veterans Administration (VA), motivated by the specific interests of these organisations in analgesic drug development, would later be added to CDAN's grant fund. At least eight principal investigators and their teams were provided with grants from these funds to work on analgesic testing methodology.

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[490] "Minutes of Conference with Representatives of Drug Manufacturers, July 1, 1949," Minutes, CDAN, NASA.

[491] Nathan B. Eddy, *The National Research Council Involvement in the Opiate Problem, 1928-1971* (Washington, D. C. : National Academies of Science, 1973), 164, Appendix 3, Table 3.

Beecher's experimental model—a variant of the randomised clinical trial (RCT) design—was adopted, adapted and standardised for the evaluation of new analgesic drugs. The alliance forged between the Committee, its sponsors and its grantees would shape the future of American analgesic evaluation.

In this chapter, I will examine how analgesic clinical trials were designed and implemented under CDAN sponsorship and discuss the implications of this case for the history of pain-measuring technologies. I suggest that the networks created by CDAN's alliances and activities, through which funding and information were mobilised and distributed, were crucial for making the clinical trial into an effective technology of analgesic evaluation. I will show this by identifying ways in which various types of resources were obtained, exchanged, combined and manipulated by committee members and their allies (sponsors, grantees, and their research teams) in order to make the analgesic clinical trial produce better—more valuable, accurate and trustworthy—results. The focus of my analysis is on the interaction between the social and material organization of experimental practice, and the epistemological validation of the analgesic clinical trial.

This analysis operates on three levels: First, I look at the relationships of material exchange, communication, trust and authority among the different actors—sponsors, members, and grantees, their research subjects and collaborators—who participated in CDAN's quest for better analgesic-testing technology. Each of these actors contributed something towards this goal: time, money, skills, labour, ideas, ambitions… What motivated them to make such investments? How were these resources valued, combined and translated into practice? My second focus is on the experimental practices by which analgesic testing technologies were implemented and improved. In particular, I have tried to identify ways in which material conditions and social interactions shaped the experimental practice of analgesic clinical trials. Finally, I point out the ways in which experimental practice contributed to the validity of clinical trial results.

Why is this case study interesting? First, the CDAN-sponsored analgesic clinical trial was an early—and rare—success story in the history of pain-

measurement and analgesic evaluation. As we saw in the previous chapter, the success of the Hardy-Wolff-Goodell dolorimeter was short-lived. And, as the next chapter will show, it proved extremely difficult in other contexts for people to agree on how pain-relief should be measured and what counted as a valid experiment of pain-relieving therapeutic efficacy. How, then, were CDAN grantees able to make the analgesic clinical trial work? I suggest that the key in answering this question is to pay attention to the collective nature of the effort that went into making this technology work, and to examine how the interaction between various actors shaped its practices and results. Analysing the social relations of the analgesic clinical trial is thus relevant to one of my central concerns in this thesis: Under what conditions can—and has—pain become measurable? In addition, CDAN-sponsorship of analgesic evaluation represented an unprecedented, even if relatively modest, financial investment in research on pain and its relief.[492] To improve the efficacy of their methods, CDAN grantees investigated how experiences and expressions of pain were influenced by different variables, such as anxiety, conditioning and suggestion. They thus produced new kinds of knowledge that reinforced, and added precision to, a model of pain as an emotional experience that was person—and context—specific. Somewhat paradoxically, then, efforts to make the analgesic clinical trial more objective seemed to depend on defining experimental subjects as thinking and feeling beings who participated in the production of experimental data.

CDAN sponsorship of analgesic clinical trials is also an important episode in the history of clinical trials in the U. S. Indeed, it was one of the earliest, though certainly not the largest—when compared to the large, publicly funded trials of antibiotics and cancer chemotherapy—[493]investments into the

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[492]It has been said that the first substantial grants for pain research were not allocated until the 1970s. See: Isabelle Baszanger, *Inventing Pain Medicine: From the Laboratory to the Clinic.* New Brunswick, N.J: Rutgers University Press, 1998, 63. Pain researchers have continued to complain that this area is underfunded. While CDAN grants were not large or specifically earmarked for research on pain, they nevertheless show that pain research was indirectly funded before the 1970s.

[493]On the NRC's study of penicillin and the Veteran's Administration's study of streptomycin, see: Harry M. Marks, *The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900-1990* (Cambridge and New York: Cambridge University Press, 1997), 98-128. On

implementation of randomised controlled clinical trials (RCTs). This case study can thus help us understand how and why the RCT became a widely used technology in the U. S. during the post-war decades. Historians have already explored some of the reasons for this. In particular, Harry Marks' study of clinical experimentation in the U. S. has emphasised the role of "therapeutic reformers" in pushing for more rigorous methods of therapeutic evaluation. What united these reformers was a shared belief in the value of experimentation, and a mistrust of commercial motivations, as a guide for medical practice. In the second half of the 20th century, the ideal source of scientific authority promoted by these reformers shifted from the judgment of experts to the impersonal methods embodied by the RCT.

Marks' detailed study of one of the first major RCTs in the U. S.—the VA's evaluation of streptomycin—is useful for understanding how it became possible to implement this technology during the post-war years. This case, Marks points out, emphasises the opportunity created by the wartime reorganization of scientific research for elite researchers to put their vision of methodological reform in action. This reorganisation centralised the coordination and funding of research activities, thus making it possible to control them on a large scale. Once completed, this large cooperative trial was used as a model by reformers for future drug evaluation. While this trial may indeed have been influential in the early development of RCTs, as Marcia Meldrum has shown, early trials varied widely in their sponsorship, designs, and rationales, as well as in the types of resistance they encountered, depending on the nature of the therapy being tested and on the social setting in which the therapy was developed and tested. This suggests the importance of obtaining knowledge about a variety of conditions under which the RCT emerged, to which I propose to contribute. In addition, as Marks and Meldrum have pointed out, RCTs were expensive, time-consuming, required the

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early chemotherapy trials sponsored by the Cancer Chemotherapy National Service Center of the National Cancer Institute, see Ilana Lowy, *Between Bench and Bedside* (Cambridge, MA and London: Harvard University Press, 1996), 43-68.

cooperation of its numerous participants and authority over their activities.[494] It seems important, then, to pay attention to the different social and material arrangements underlying the practice of these early clinical trials.

Finally, in this case, RCT methodology was developed and promoted for a very particular purpose: to measure subjective drug effects. As we will see, this goal gave RCT methodology a distinctive rationale and value, while its implementation required different kinds of efforts and adjustments than it did when it was used to evaluate therapies that had outcomes that could be measured "objectively" with machines (blood pressure, for example) or were much more definitive (such as death). Beecher and his colleagues were able to show that it was possible to measure subjective drug effects in an objective *manner* without transforming what they were measuring—in this case pain—into something less subjective. They also argued that it was possible to render drug evaluation objective in the absence of mechanical instrumental technologies for controlling variables or collecting data (though data analysis was mechanised using computer programs in the 1960s). The objectivity of their technology depended almost entirely on procedural rules to mediate the interaction between experimenters and experimental phenomena. This makes the analgesic clinical trial an especially interesting case for the study of the collective nature of knowledge-production, and particularly of how the social relations of experimental practice were managed to produce objective experimental knowledge, or, as Steven Shapin has put it, how "social theory [is] made manifest as epistemology."[495] The socio-epistemic model of the analgesic clinical trial would become a model for the evaluation of the effects of psychotropic drugs, a class of drugs that would mushroom in the 1950s and 1960s, while it also set a precedent for future clinical pain assessment.

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[494] Marks, *The Progress of Experiment*; Marcia L. Meldrum "'Departures From the Design': The Randomized Clinical Trial in Historical Context, 1946-1970," (PhD Dissertation, Department of History, State University of New York at Stony Brook, 1994). See also the previous chapter.
[495] Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago, 1994), 359.

This chapter centres on four sets of relationships. First, I will describe how the Committee created alliances, on which the success of its research program depended, with investigators whose professional status and ambitions made them willing and able to work on analgesic testing methodology. Who were these investigators? What ambitions, expertise and work did they bring to analgesic evaluation? Focusing on the careers of some of the principal CDAN grantees, I will show that the Committee was able to support, and tap into, contemporary professional resources and aspirations in making the analgesic clinical trial a successful technology.

Principal investigators did not work alone. To operate and improve the analgesic clinical trial, investigators were assisted by a team of assistants, consultants and observers, while they also relied on the cooperation of hospital staff and patients. As Steven Shapin has shown, in order to fully understand the collective nature of experimental knowledge-production, historians (and sociologists) must uncover the "epistemic role support personnel" which is often written out of, or at least undervalued in, public records of experimental practice. For example, Shapin has shown that Robert Boyle's 17th century experimental observations were presented, publicly, as the testimony of a "free and independent gentleman" of his direct unmediated experience of "nature." It was on the basis of these qualities that Boyle claimed the trustworthiness of his knowledge. In practice, however, Shapin argues that it was often Boyle's paid assistants who mediated this access to experience, and it was Boyle's trust in them that ensured the reliability of experimental results. The "trust relationship" between experimenter and assistant that was necessary for this collective production of knowledge was then erased from public presentations of experiments.[496] Likewise, the design of the analgesic clinical trial was presented as the bearer of objective data, but the reliability of this data, in practice, also depended on the labour, skills, judgment and knowledge of the people who produced it. I have tried, in this section, to make the "epistemic role" and work of each group of clinical trial participant as visible as possible. I will examine the tasks assigned by

[496] Shapin, *A Social History*, 355-407.

investigators to their collaborators, the strategies they developed to ensure that collaborators performed their tasks correctly, and the ways in which each group's work was seen to contribute to the results of analgesic evaluation.

As we've already seen, committee members created an alliance with various sponsors, particularly pharmaceutical firms, in order to obtain funding for the development of analgesic testing methodology. In my third section, I will examine how CDAN members mediated the relationship between sponsors and grantees through the attribution of research funding. These groups had different ideas of what constituted worthwhile research for improving analgesic evaluation. Their negotiations shaped a research program that was open to methodological explorations and innovations that did not necessarily have immediate practical applications. As a result, the analgesic clinical trial was not just worked *with*, but also extensively worked *on*.

In the last section of this chapter, I will describe how CDAN mediated the relationship between different research teams who operated analgesic clinical trials. Initially provided only to Beecher's team, CDAN grants for analgesia research were extended to two other research teams in 1955, and two more in 1958. By 1962, a multi-site cooperative study of analgesics was established in hospitals of the Veteran's Administration. CDAN facilitated the diffusion of information and expertise between research teams, and encouraged the development of tools to help make practices of analgesic evaluation more uniform and their results comparable. The successful diffusion of methodological models to new research sites helped to validate this technology, while also making it more productive in providing larger volumes of data to sponsors and regulators.

6.1 A New Kind of Pharmacology: CDAN and its Grantees

Who were CDAN's grantees? Several, such as Henry K. Beecher, Louis Lasagna and Raymond Houde, were pioneers in their specialties—anaesthesia and clinical pharmacology—as well as in clinical therapeutic experimentation. Within their biographies, research sponsored by CDAN converged with campaigns to promote

new approaches to therapeutic evaluation, aspirations to expand the breadth of professional opportunities, and agendas to establish new programs of research and training. Through its contracts with its grantees, the Committee subsidised, and also tapped into, these professional ambitions. CDAN grantees obtained funding to expand their research activities and produce persuasive results, which they used to campaign for new kinds of research and training. They also helped to recruit and train competent investigators who would contribute their expertise to CDAN's program. They had access to research facilities and abundant sources of experimental subjects, and were able to combine CDAN grants with other sources of public and private funding.

The man initially chosen for the job of making analgesia measurable was Henry K. Beecher. The men who assigned him this job saw it as a difficult, important, but also as a fairly tedious one. Would Beecher have what it took, they wondered? Why would Beecher, or any assistant who had any "spark," be interested in taking up such a "dull job," asked Isaac Starr?[497] Philip Owen, executive officer of the NRC, agreed that the "touch of genius (...) required to solve the problem of assessing the effect of analgesics in man [was] not likely be found in the routine operations of a Department of Anesthesia."[498] Indeed, in 1949, anaesthesia was not likely to be perceived as a thriving field of cutting-edge medical research. Yet, perhaps what made Beecher the right man for the job were his ambitions for the future of this field. While Beecher agreed that analgesic testing was "painstaking and tedious work," he was willing—and encouraged others—to invest in what he insisted was "a costly field, but one that promises to yield on cultivation an astonishingly rich harvest."[499]

By examining Beecher's career and his campaign to promote both anaesthesia research and clinical therapeutic experimentation, it is possible to understand how his involvement with CDAN fit with his professional aspirations.

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[497] Starr to Eddy, July 13, 1949, Box 2: General, 1947-June 1959, CDAN, NASA.
[498] Philip Owen (NRC) to Starr, July 20, 1949, Box 2: General, 1947-June 1959, CDAN, NASA. For Eddy's agreement, see: Eddy to Starr, July 22, 1949, Box 2: General, 1947-June 1959, CDAN, NASA.
[499] Henry K. Beecher, "Experimental Pharmacology and Measurement of the Subjective Response," *Science* 116 (1952):162.

A good question, asked by his former colleagues and collaborators in a memorial article, is: Why, "in the days when anesthesia afforded little professional prestige," had Beecher accepted the position of Chief of Anaesthesia Service at the Massachusetts General Hospital? Beecher had been headed for a career in surgery when his mentor, the Chief of Surgery, Edward Churchill, suggested he take up the post in 1936. The memorializing authors offered: "perhaps his far-sighted unconventional mind recognised the potential for a bright future when others did not."[500] In another biographical piece, a former colleague suggested that: "Beecher was receptive to the idea, seeing anesthesiology as an untapped laboratory for physiologic and pharmacologic research."[501]

Indeed, upon taking up his post, Beecher began searching for ways to develop an anaesthesia research program. One of the first things he did was to create the Anaesthesia Laboratory of the Harvard Medical School at the Massachusetts General Hospital. This "laboratory" enabled Beecher and his collaborators to mark their published papers with the stamp of anaesthesia research instead of affiliating themselves with the Department of Surgery, of which they were dependent.[502] Having studied physiology for a year with Nobel Prize winner August Krogh in Copenhagen in 1935, Beecher was equipped scientifically to launch a program of physiological research on anaesthetic procedures. He also proved to be talented at creating research opportunities and finding funds. It was rumoured that he charged outrageous fees from his private patients, keeping only a limited allowable amount for his own salary and putting the rest into the department.[503] In 1941, he also befriended Edward Mallinckrodt,

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[500] O. Cope, et al. "Henry Knowles Beecher: Pioneer in Anaesthesiology and Medical Ethics. Faculty of Medicine—Memorial Minutes," reprinted from *The Harvard Gazette* (January 13, 1978) in, *'This is No Humbug!' Reminiscences of the Department of Anesthesia at the Massachusetts General Hospital: A History*, ed. R. Kitz, et al. (Ashland, OH: Atlas Books, 2003), 104.

[501] G. E. Battit, "Henry K. Beecher and the Early Years of the Anesthesia Service," in *This is No Humbug!*, 108

[502] B. McPeek, "Pain" in *This is No Humbug!*, 225.

[503] Battit, "Henry K. Beecher," 111. See also: "Report of the NRC Committee, Dr Isaac Starr, chairman, to visit Dr. Henry K. Beecher's laboratory in Boston, Staturday, February 24th" in "Starr to Winternitz, March 8, 1951," Box1: Grantees: Beecher, H. K., 1950-1966, CDAN, NASA: "Dr. Beecher runs a large "show" and his budget approximates $250,000 a year, about half of which comes from patients' fees for anesthesia."

owner of the company that manufactured ether, from whom he obtained private research grants.[504]

The war also turned into a research opportunity for Beecher. He became a medical consultant for the United States Army and was appointed to two sub-committees on shock and anaesthesia. In 1943, he was called into active service in North Africa, where he served as a consultant for twenty-five months. This was an extremely productive time for him. Beecher recorded his observations the effect of anaesthetic and analgesic practices on wounded soldiers and, with the help of his associates back in Boston, published 30 papers during this period. In particular, Beecher developed a greater interest for pharmacology and pain.[505]

Beecher's research not only benefited the fledgling field of anaesthesia, but was also of interest to the army. After the war, Beecher's laboratory was given a grant from the army's Medical Research and Development Board (MRDB) to study sedatives. The grant supported research on topics of military importance: the effect of sedatives on the restoring value of sleep, narcoanalysis (the potential use of "truth serums") and comparisons of the effectiveness of synthetic and standard analgesics. It was for this latter project that Beecher began developing his method of placebo-controlled, double-blind studies of analgesia. Eager to continue developing this work, he appealed to CDAN in 1948. CDAN members assisted Beecher in obtaining NIH funding for 1949, and, in 1950, began providing him with grants "from funds contributed by a group of interested pharmaceutical manufacturers," as it said on the standard disclaimer included in publications.

Beecher kept his eyes open for opportunities to refine and publicise his methodological innovations. In August of 1950, Beecher wrote to Colonel Stone, chairman of the MRDB, asking for a chance to conduct field trials of analgesics in Korea: "I should like to emphasize, if I may, what an extraordinarily good opportunity the fighting in Korea offer to make a definitive field appraisal of the new narcotic agents..." If these trials confirmed that either of the methadones was

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[504] Battit, "Henry K. Beecher," 111.
[505] McPeek, "Pain," 226.

"suitable for military use...the Surgeon General will be free from any necessity for stock piling morphine."[506] By the following January, Beecher reported back to Stone: "It seems to me that on the whole the expedition to Korea was a very profitable one. The Army has had some good publicity out of it, and I have too."[507]

The combined funding from CDAN and the Army allowed Beecher and his colleagues to amass material, which they transformed into over 50 scientific articles between 1948 and 1960, numerous public addresses and a book, published in 1959, under the title *The Measurement of Subjective Responses: Quantitative Effects of Drugs*. With these publications, Beecher promoted his model of analgesia evaluation as a prototype for a new kind of pharmacological measurement. As he wrote, in 1951, in an application to renew his army contract: "Clearly what is emerging here is a new approach, or at least a heretofore un-crystallised approach, to pharmacology, what might be called the pharmacology of the subjective response."[508] What followed was a decade-long campaign to persuade a larger audience of the power of double-blinded, placebo-controlled clinical trials for the measurement of drug effects on subjective experience. Beecher emphasised the special relevance of these methodological devices for the study of psychoactive drug effects, making no claims, at least initially, for their general necessity in therapeutic evaluation. When it came to measuring effects such as pain-relief, drowsiness, itching, or even "warm glow," "sensation of drunkenness", or "fullness in the head," these controls could, in a sense, act as substitutes for the mechanical quantification of physiological phenomena. When the proper precautions were taken to eliminate bias, both conscious and unconscious, it was possible to transform subjective reports into hard data. For Beecher, experimental controls such as placebos, blinding, rigorous statistical analysis and the hiring of technicians did not constrain what would be counted as

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[506] Beecher to Col. Stone, August 15, 1950, Beecher Papers, MS C 64, Box 22, Folder 16, Countway Library.
[507] Beecher to Stone, January 27, 1951, Beecher Papers, MS C 64, Box 22, Folder 16, Countway Library.
[508] "Application for Renewal of the Contract on Sedatives (Dept of the Army), May 25, 1951," Box 1: Grantees, Beecher, H. K., 1950-1966, CDAN, NASA.

valid evidence, but were instead keys for opening a whole new area of legitimate clinical research.[509]

The use of these controls was not common in the early 1950s, and Beecher evidently felt the need to demonstrate that this new kind of pharmacology, though expensive and time-consuming, was a worthwhile investment. Using data he collected through CDAN-sponsored experiments on analgesic evaluation, Beecher promoted his methodology extensively. For example, during the 1950s, he continued to build on his distinction between experimental and clinical pain, as I have described in the previous chapter, and to argue that only clinical pain was relevant to the evaluation of analgesics. In one article, he compared the data he had accumulated during the war on the pain experience of wounded soldiers with new data, from a recent study, on the experience of civilians who had undergone surgery. The data showed that the two groups' experiences of pain differed significantly: the civilians experienced far more pain from lesser wounds than had the soldiers. To Beecher, these results suggested that anxiety, fear and the meaning of the wound had more impact on the intensity of the pain experience than the severity of the injury.[510] Beecher and his colleagues also conducted extensive research on placebos. Beecher was initially searching for a means of filtering out data from "placebo-responders" in order to sharpen his testing results. Though this search was unsuccessful, Beecher was able to transform data on the placebo effect into a powerful argument for the need to employ placebos and the "unknowns technique" in analgesic evaluation.[511]

The articles on placebos published by Beecher's research team have, more recently, been identified as watersheds in the history of clinical research. In 1997,

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[509] Key publications that outline Beecher's "program" include: Beecher, "Experimental Pharmacology and the Measurement of Subjective Response," *Science* 116 (1952): 157-62; "Limiting Factors in Experimental Pain," *Journal of Chronic Diseases* 4 (1956): 11-21; "The Measurement of Pain. Prototype for the Quantitative Study of Subjective Responses," *Pharmacological Review* 9 (1957): 59-209; *Measurement of Subjective Responses : Quantitative Effects of Drugs* (New York: Oxford University Press, 1959).

[510] Beecher, "Relationship of Significance of Wound to Pain Experienced," *Journal of the American Medical Association* 161 (1956): 1609.

[511] Henry K. Beecher, "The Powerful Placebo" *Journal of the American Medical Association* 159 (1955): 1602-1606; Ted J. Kraptchuk, "Powerful Placebo: The Dark Side of the Randomised Controlled Trial," *The Lancet* 351 (1998): 1722-1725.

a manifesto for making a "canon" part of medical education listed "A Study of the Placebo Response," published in 1954 in the *American Journal of Medicine*, as one of twenty-seven "historically decisive works" that future doctors should read. The article, co-authored by Beecher and his collaborators, was cited as a "fine example" of a "genre" that marked the flourishing of clinical science in the "Age of Experiment."[512] Ted Kraptchuk has credited another, similar article, "The Powerful Placebo," published by Beecher in *JAMA*, with the introduction of the modern conception of the placebo, a new conception that was associated with the rise of controlled clinical trials. Kraptchuk argues that the transformation of the placebo from "insipid decoy to a mischievous genie that could trick the most discerning clinician" was the product of efforts, on the part of elite physicians, to promote placebo-controlling and double-blinding as new sources of authority in therapeutic evaluation.[513] The goal was to shift the responsibility for judging therapeutic efficacy away from "recognised leaders of the medical profession" and place it on the methods themselves, from the personal authority of experts to the impersonal authority of procedures. To persuade reticent physicians to adopt clinical trial methods, these reformers depicted the placebo-effect as powerful, deceptive, and in need of careful control.[514] Such was, according to Kraptchuk, the "entire point of Beecher's exercise" in the JAMA article, from which he cited: "'Clinical impression' is hardly a dependable source of information without the essential safeguards of the double unknown technique, the use of placebos also as unknowns, randomisation of administration..."[515]

Thus, Beecher used CDAN-sponsored research to promote a new kind of clinical pharmacology. Beecher's ambitions also benefited CDAN and its sponsors. At a general level, Beecher's efforts probably helped obtain more widespread acceptance of the value and validity of the clinical trial, and particularly for the evaluation of analgesics. More specifically, Beecher also

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[512] R. Hilton, "A Manifesto for Reading Medicine," *The Lancet* 349 (1997): 872-874.
[513] Kraptchuk, "Powerful Placebo," 1723.
[514] Kraptchuk, "Powerful Placebo," 1724, suggests: "An enhanced 'placebo-effect' came to serve a valuable scientific and rhetorical function of persuading colleagues of the necessity of the RCT."
[515] Beecher, "The Powerful Placebo," cited in Kraptchuk, "Powerful Placebo," 1724.

helped to create the knowledge and expertise that were necessary to run analgesic clinical trials successfully. Because clinical trials were uncommon in the early1950s, there were few researchers who had the relevant knowledge and expertise to run them. Beecher's research helped to produce such knowledge, as well as to recruit—both directly and indirectly—new investigators among a new generation of anaesthesiologists and clinical pharmacologists. A small group of experts in analgesic clinical trials was created, who were available to CDAN when it expanded its granting program. Later, these researchers were sought after to do research or provide advice by pharmaceutical companies, the FDA and even Senate Committees.[516]

Beecher's appeal to young researchers from the growing medical specialty of anaesthesiology was timely. During the 1940s and early 50s, the authors of several articles and editorials in *Anesthesiology* urged their readers to help elevate the subordinate status of anaesthesia, the "handmaiden of surgery," in one of two ways. Either they could put an emphasis on the clinical skill required for good anaesthetic care, and thus help to create distance, in the surgeon's, patient's and 'public's' minds, between medically trained anaesthesiologists and nurse or technician anaesthetists.[517] Or, they could engage in research as a means to show anaesthesiologists' ability to contribute to the advancement of medical science.[518]

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[516] National Research Council, *Drug Efficacy Study: Final Report to the Commissioner of Food and Drugs, Food and Drug Administration* (Washington: National Academy of Sciences, 1969); *Hearings before the Subcommittee on Monopoly of the Select Committee on Small Business*, United States Senate, Second Session on the Present Status of Competition in the Pharmaceutical Industry, Part 19, (U. S. Government Printing Office, Washington: 1970), in which two pharmacologists with CDAN connections, W. T. Beaver and R. Houde, as well as an anaesthesiologist, J. Adriani, presented evidence.

[517] H. W. Haggard, "The Anesthetist in American Medicine" *Anesthesiology* 1 (1940): 1-12: this editorial, which introduces the first volume of the journal, called all anaesthesiologists to help "shape public opinion" to "raise the prestige of anesthesia" by emphasizing the skill required to practice it. Editorial, "Postoperative pain" *Anesthesiology* 9 (1948): 311-312, emphasized the need for the anaesthesiologist to extend care into the postoperative period. H. Boyd Stewart "Editorial: Anesthesiology in the Practice of Medicine" *Anesthesiology* 10 (1949): 223-228, recounts the history of anesthesia: As more doctors became interested in the field, anesthesia became more respectable. However, to give the field a better image, the anesthetist was encouraged to take an active part in public life, in the hospital, county medical societies, civic clubs, chambers of commerce,etc. : "We must exercise our ingenuity to keep out of the shadow which clouded the specialty when we took over from the technicians." In the same volume, Editorial: "The Anesthesiologist and the Public" *Anesthesiology* 10 (1949): 634-635, carried a similar message: "We have opportunities daily to convince patients of the significance of good anesthesiological

Analgesic testing, as Beecher envisioned it, provided one kind of opportunity for anaesthesiologists to engage in clinical research. Beecher's general research program had shown that experimental research in physiology and pharmacology was an appropriate activity for anaesthesiologists.[519] More directly, Beecher actively recruited young interns to work in his laboratory, and delivered public addresses, thus inviting fellow anaesthesiologists to take up research activities among which was analgesic evaluation. At the Annual Meeting of the American Society of Anesthesiologists in 1950, which was published in *Anesthesiology,* Beecher detailed his methodology, emphasizing to his audience that "in awareness of pain we have a factor that can be measured," and second that "there is an abundance of material ready at hand for study."[520] Beecher's method relied on the use of post-operative patients as experimental subjects, making anaesthesiologists—who had access to these subjects and the responsibility to care for their pain—the ideal investigators to carry out analgesic evaluation.[521]

Over the following decade, anaesthesiologists became increasingly involved in pharmacological research, and particularly in clinical trials of analgesics. The majority of CDAN's grantees and their collaborators were anaesthesiologists. When CDAN began awarding more grants, several new investigators were anaesthesiologists: Arthur Keats worked on the clinical

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service," by visiting patients pre- and post- operatively. It was also good to use the press and personal contact to ensure that anaesthesiologists would one day get their due as medical specialists.

[518] R. Charles Adams "Clinical Research in Anesthesiology," *Anesthesiology* 11 (1950): 178-184, argued that research was not only of scientific interest, but was also good for the status of anaesthsiology. Research was "bound to stimulate the interest of surgeons and other specialists in the importance of a well-organised anesthesia department." "Editorial: Clinical investigation," *Anesthesiology* 12 (1951): 114-118, stated: "Clinical investigation can be a respectable and tremendously potent contributor to the science of medicine..."

[519] N. M. Greene, *Anesthesiology and the University* (Philadelphia and Toronto: J. B. Lippincott Company, 1975): 45-48, identified Beecher as the pioneer of an "approach to professionalism in anesthesiology," which emphasized the scientific rather than clinical potential of the profession. E. M. Papper, R. Dripps, and S. C. Cullen later joined Beecher in that approach. The pioneers of the clinical emphasis in the promotion of professionalism in anesthesia were R. Waters, J. S. Lundy and E. A. Rovenstine.

[520] H. K. Beecher, "Pain and some of the factors that modify it" *Anesthesiology* 12 (1951): 633-641.

[521] Editorial "Postoperative pain," 311-12

evaluation of analgesics for CDAN from 1958 to 1970, Thomas DeKornfeld from 1965 to 1972. Both previously worked with CDAN funding in collaboration with other investigators: Keats with Beecher and DeKornfeld with Louis Lasagna, who himself had worked with Beecher. Raymond Houde, a clinical pharmacologist at Sloan-Kettering Memorial Cancer Centre, worked with anaesthesiologist Weldon Bellville on CDAN-funded studies. When Bellville moved on to Stanford Medical Center and the VA hospital in Palo, he established his own program of research. He then became one of the seven Chiefs of Anesthesia in various VA hospitals to coordinate trials for the VA Cooperative Analgesic Study.

In the 1950s and 1960s, anaesthesiologists became analgesic evaluators outside the committee granting program as well. John Adriani, for example, conducted trials of analgesics on his postoperative patients for various pharmaceutical companies in the 1960s, and was appointed as member of the FDA's Panel for the Review of Topical Analgesics, a section of its ambitious evaluation of Over-the-Counter drugs in the early 1970s.[522] As anaesthesiologists became increasingly involved in the treatment of chronic pain, they obtained access to another population of potential subjects for analgesic testing. John Bonica, who ran a pain clinic at the University of Washington, was also in demand by pharmaceutical companies to run trials of analgesic drugs.[523]

There was also, among CDAN grantees, a clinical pharmacologist who was influenced by Beecher's work and took up clinical analgesic evaluation: Louis Lasagna. In addition, Raymond Houde, originally an internist, was trained to work as a clinical pharmacologist, while William T. Beaver, who worked briefly with Houde, became an expert in the clinical pharmacology of analgesics.[524] Beecher's model had appealed to these researchers who, like him, believed that "the properly controlled, quantitative approach holds the only real

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[522] "FDA Advisory Work: OTC Panel 1971-74" Adriani Papers, MMC 453, Box 35, HMD, NLM; "Squibb Institute for Medical Research, Evaluation of Bandol," Adriani Papers, MMC 453, Box 23, HMD, NLM; "Endo Laboratories Numorphan/Morphine/Meperidine 1959-65," Adriani Papers, MMC 453, Box 22, HMD, NLM.

[523] "Drug Companies Correspondence, 1955-1958," Bonica Papers, MS C 118, Box 67, Folder 13, Darling Library, UCLA; "Drug Companies Correspondence, 1959-1963," Bonica Papers, MS C 118, Box 67, Folder 14, Darling Library, UCLA.

[524] Meldrum, "Departures from the Design,"342.

hope for dealing with the oncoming flood of new drugs" of the post-war decades.[525]

In addition, Beecher had drawn attention to the fact that a great deal of the products of this "pharmaceutical revolution" targeted subjective symptom relief rather than the cure of objective disease processes, thus emphasizing the importance of the measurement of subjective drug effects.[526] The lack of attention given to this kind of measurement had, Beecher pointed out, hampered the progress of psychiatry.[527] There was, he declared, an urgent need to develop a "systematic neuropharmacology or psychopharmacology."[528]

The professional trajectory of Louis Lasagna connects Beecher's laboratory, CDAN, and analgesic testing to the emergence of clinical pharmacology and psychopharmacology as distinct branches of medical science, as well as to regulatory reform during the era of the "pharmaceutical revolution." According to his oral history interview, Lasagna had already developed an interest in drug testing methodology while he was a medical student.[529] Because of his interests, he was advised to do his post-doctorate at Johns Hopkins, where a new program linking medicine and pharmacology was being started by Gordon Zubrod. He did this from 1950 to 1952. He then heard about Beecher's work from his friend Arthur Keats, who was a resident in anaesthesia at MGH: "he told me about this exciting stuff they were doing, which was trying to quantify subjective

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[525] Beecher, *Measurement of Subjective Responses*, viii.

[526] Beecher, "Limiting Factors in Experimental Pain," *Journal of Chronic Diseases* 4 (1956): 11, citing MacDonald, Beecher notes that, though chemotherapy has captured much of the limelight in therapy in recent years, most medications remain concerned with the treatment of symptoms, among which the most important is pain.

[527] Beecher, "Experimental Pharmacology,"157, notes that the "general growth of medicine in recent years...has served indirectly to emphasize areas where development has lagged. Notable, for example, is the slowness of enduring growth in experimental psychiatry...it is possible that growth in this field has been retarded because pharmacology as it deals with the subjective response has not been given the attention it deserves."

[528] Beecher, *Measurement of Subjective Responses: Quantitative Effects of Drugs* (New York and Oxford: Oxford University Press, 1959), vii-viii.

[529] Marcia Meldrum, "Oral History Interview with Louis Lasagna," 8 September 1995, MS C 127.19, John C. Liebeskind History of Pain Collection, HSCD, Darling Library, UCLA, 8, Lasagna says that he read the Cornell Conferences on Therapy, edited by Harry Gold, in which the importance of blinding and placebo-controlling were emphasized long before they became commonly practiced.

measurements, and the more I heard about it, the more excited I became."[530] Always eager to expand his research program, Beecher managed to get Lasagna assigned to his Army-funded project. [531] While he worked with Beecher, Lasagna also participated in CDAN funded studies, and "began to learn how to conduct analgesic trials, studied the interaction between psychological variables and response to placebos and CNS drugs, performed the first modern clinical trial of hypnotics, and became convinced that clinical pharmacology would be a satisfying and exciting career."[532]

Thus, in Lasagna's autobiographical version of events, it was Beecher's laboratory that inspired him to become the researcher who would eventually be credited as having "invented the discipline" of clinical pharmacology.[533] After leaving MGH, Lasagna realised Zubrod's unfinished project by setting up the first Division of Clinical Pharmacology in the U. S. at Johns Hopkins.[534] There he began teaching people "to study drugs rationally and to train people to study drugs well."[535] Lasagna also published extensively on drug regulation and, from 1960 to 1962, was one of the key witnesses to the Kefauver committee. Along with Walter Modell, Lasagna pushed for, in his words, "randomized controlled trials as being a necessity if you're going to approve a drug."[536] They were successful in making the Kefauver-Harris amendments to the Food and Drug Act of 1962 into what has been seen as one of the major victories in the historical

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[530] "Oral History Interview with Louis Lasagna," 2.
[531] "Oral History Interview with Louis Lasagna," 2, Lasagna was hired on Beecher's army grant as a form of public service he owed for having gone to medical school on a Navy program.
[532] Louis Lasagna, "Clinical Pharmacology in the United States: A Personal Reminiscence,"
[533] D. J. Greenblatt, "The Maturation fo Clinical Pharmacology: Recognizing the Contributions of Dr. Louis Lasagna," *Journal of Clinical Pharmacology* 38 (1998), 572. See also S. Erill, (ed.), *Clinical Pharmacology Through the Pen of Louis Lasagna* Vol.1 (Barcelona and Philadelphia: Prous Science, 1997). This series, which aimed to explore "basic or seminal articles" in the history of pharmacotherapy, began with a volume on clinical pharmacology because of its "outstanding role in the shaping of modern therapeutics." To represent such an important history, the articles of a single author were chosen by the editor. Though the editor does not justify this choice, and indeed states that "any attempt at justifying the choice may be preposterous," that he made it seems to indicate that Lasagna was viewed highly as a pioneer of the discipline.
[534] "Oral History Interview with Louis Lasagna," 16.
[535] "Oral History Interview with Louis Lasagna," 16.
[536] "Oral History Interview with Louis Lasagna," 8.

process of making clinical trials methods into the "gold standard" of American therapeutic evaluation.

While he established his program and lobbied for regulatory reform, Lasagna continued to conduct analgesic studies funded by CDAN grants, from 1958 to 1964, and was appointed as a member of CDAN from 1969 to 1971. Lasagna also chaired the Committee on Analgesics for the National Research Council-National Academies of Science Drug Efficacy Study, commissioned by the FDA to evaluate drugs approved prior to 1962.[537]

Lasagna has also been counted as a key figure in the emergence of psychopharmacology in the 1950s and 60s.[538] Not only did he conduct studies of psychoactive drugs such as hypnotics and antipsychotics, but also pushed for the establishment, and then presided, the American College of Neuropsychopharmacology. In addition, Lasagna and Beecher's analgesic trials came to be seen as models for the study of psychotropic drugs. According to David Healy, when federal funds were made available for the evaluation of psychiatric drugs in the 1950s, researchers were still divided on the question of whether adequate tools were available for measuring the effects of these drugs. At the inaugural conference of the Psychopharmacology Service Center in 1956, a key topic of discussion was methodology. Healy described the discussion:

> There was a general belief that it was not possible to measure subjective change of the kind that seemed to be involved in psychiatric illness. The studies of Beecher, Gold, and Lasagna on analgesia, however, were probably critical in helping physicians and researchers realise that what had previously seemed impossible might after all be feasible. Pain, after all, was just as subjective as depression. Stemming from this, one of the key issues at the conference was the use of rating scales for the mapping of clinical change.[539]

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[537] National Research Council, *Drug Efficacy Study.*
[538] Lasagna was interviewed for David Healy, *The Psychopharmacologists: Interviews* (London and New York: Altman, 1996), and was invited to contribute to: David Healy, *The Creation of Psychopharmacology* (Cambridge, MA: Harvard University Press, 2002).
[539] David Healy, *The Antidepressant Era* (Cambridge, MA: Harvard University Press, 1997), 95-96.

For both Beecher and Lasagna, working for CDAN on the improvement of analgesic testing was clearly only a small part of their broader ambitions. These included the creation of new professional opportunities in clinical pharmacology, psychopharmacology and anaesthesiology and the reform of therapeutic evaluation in the U. S.. Beecher's success in promoting the clinical trial and in recruiting clinical analgesic evaluators was helped by CDAN sponsorship, but also facilitated by broader trends in these emerging specialties and in the reform of therapeutic evaluation. The participation of Keats and Lasagna in CDAN's research program, first as Beecher's assistants, and then as principal investigators, are proof of Beecher's direct success in broadening the pool of expertise in analgesic testing in a way that benefited the Committee directly.

Raymond Houde's trajectory was different from Beecher, Lasagna and Keats', but seems to have also been guided by Beecher's work and the hand of Committee members. Originally trained as an internist, Houde was, according to his own recollections, recruited to launch a program of analgesic evaluation at Memorial Sloan-Kettering Cancer Hospital. Having dabbled in studies on analgesics in animals, he was invited to attend a meeting attended by "all important people from the pharmaceutical industry but also from government, like I, Dr. Eddy I think was there, Nathan B. Eddy" who were interested in the development of synthetic analgesics.[540] Houde was given a sabbatical year—probably in 1949—to study pharmacology, analgesics and study designs with Maurice Seevers at the University of Michigan and Abraham Wikler at Lexington Hospital, both members of CDAN.[541] Through them, he was introduced to Beecher's work and to the Committee, and soon met Beecher at a Committee meeting.[542] On the basis of the scattered information provided in his oral history, it seems likely that Houde—a young clinician, who held a position in a large clinical facility filled with patients who required analgesic therapy, and who had some knowledge of pharmacology and of the statistical requirements of study

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[540] Marcia Meldrum, "Oral History Interview with Raymond W. Houde, 11 September 1995," MS C 127.16, History of Pain Collection, Darling Library, UCLA, 37.
[541] "Oral History Interview with Raymond W. Houde," 38 and 50.
[542] "Oral History Interview with Raymond W. Houde," 41 and 50

designs—was handpicked by Committee members, and groomed to become a clinical pharmacologist specialised in analgesic studies.

The points of personal contact and lines of influence that linked CDAN members and grantees created analgesic-testing careers for anaesthesiologists and clinical pharmacologists, and new expertise for the Committee's program. As we will see in the last section of this chapter, these connections also probably facilitated the replication of clinical trials conducted in different sites.

6.2 The Women and Men who Counted: Investigators and their Collaborators

Refining and implementing clinical experimental designs required the collaboration and compliance of a wide variety of actors, from the hospital pharmacist who coded drug doses to the patients themselves who rated their feelings of pain using pre-selected terms. As Starr reportedly pointed out, in a discussion of Beecher's first CDAN grant application: "No equipment was required: the whole financial outlay was concerned with the gathering of data."[543] Consultant fees and salaries often made up more than three quarters of the budgets presented in applications for support from CDAN for analgesic clinical trials. The precision and reliability of this technology depended on the quality of the labour furnished for these salaries, as well as the coordination of additional unpaid collaboration provided by ward staff and patients.

In this section, I will examine how this labour was distributed, valued and coordinated in the practice of analgesic clinical trials. I will focus mainly on the ways in which investigators perceived the role of their various collaborators, and the means they devised to ensure they performed their role "properly." When possible, I have included descriptions of how these collaborators perceived their own work.

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[543] "Minutes of the 6th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1950), 114.

I suggest that these collaborators' conscientious work, including the active contribution of experimental subjects, was crucially important in ensuring the reliability of this technology. To a certain extent, the work performed by these collaborators was invisible in published descriptions of experimental methodology and results.[544] The design of the clinical trial itself, and not the people who put it in practice, was supposed to ensure the objectivity of its results. Various assistants were mentioned in print only when their identity was an element of the method itself, for example, when Beecher described the replacement of regular ward nurses with full-time observers for the collection of data. In CDAN meetings and reports, however, investigators exchanged more details about the role of particular assistants, particularly when discussing how to avoid potential obstacles to the implementation or replication of experimental methods. The importance they accorded to particular types of work and expertise for ensuring the reliability of trial results is also made evident by the attention they gave to the selection and training of both assistants and subjects for participation in trials. Budget records provide additional information about job descriptions, salaries and consultant fees. The oral testimony of a prominent analgesic nurse observed, Ada Rogers, provides a fascinating glimpse of the kinds of skills and experience she contributed to the running of clinical trials.

While these sources make the labour and skills of analgesic trial workers only partly visible, they do suggest, as opposed to what some published reports seemed to imply, that investigators did *not* see clinical trial participants as interchangeable parts in the operation of this technology. Objectivity did not emerge directly out of experimental protocols, but through the abilities and attitudes of the people who made it work.[545]

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[544] The "triple invisibility" of 17th century laboratory technicians has been described by Shapin, who pointed out that they were invisible first to historians and sociologists, second in the formal record of experimental practice, and third, as relevant actors in the production of scientific knowledge. See: *A Social History*, 360. Clinical trial personnel and consultants did appear in published descriptions of experiments, and their work was of some concern because of preoccupations about the standardisation of methodological procedures, but I suggest that their contribution was more extensive than acknowledged in published records.

[545] Whereas Shapin suggests that 17th century experimenters such as Boyle based the credibility of their experiments on their own authoritative testimony, in this case methodology, rather than

6.2.1 Hospital Staff

Previous clinical studies of analgesics, such as those carried out by the Committee on Drug Addiction in the late 1930s, had relied on ward staff—nurses and physicians—to administer drugs and record their impressions of efficacy.[546] Studies were coordinated either by a senior member of staff, or by an outsider—a member of the Committee or a pharmacologist—who did not hold a hospital appointment. Army and CDAN grants made it possible for Beecher and future CDAN grantees to hire special research staff—usually an associate and one or more observers, sometimes a secretary—to assist the principal investigator. Hospital staff members were therefore given less active roles in CDAN-funded studies. Nevertheless, their cooperation was considered to be essential to running clinical trials and experimenting with it, especially with respect to the administration of placebos or unknown substances. For example, when Committee members discussed Beecher's proposal to study the effect of positive and negative suggestion on drug effects, one member commented: "If the attendant offered medication to the patient but said, "I don't think this will do you much good," difficulty might be encountered not only wit the patient but with the staff." In response, Eddy "said that Dr. Beecher had *full control* over the wards where the work was carried out so that no difficulty with the staff was to be expected."[547]

Other types of resistance might be encountered, particularly in hospitals that were not affiliated with a university or a federal institution. In a report on the establishment of a new clinical facility for analgesic testing, Lyndon Lee drew attention to strategies for promoting staff acceptance of experimental practices. To administrators, who might respond with "righteous indignation against

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personal authority, was presented as trustworthy. See: *A Social History*, 383. However, in both cases, trust in "support personnel," in this case including experimental subjects, was necessary for producing knowledge that would be considered to be reliable.

[546] For descriptions of these studies, see chapter 4 of this thesis.

[547] "Minutes of 6th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1950), 115, emphasis mine.

'experimenting' with patients entrusted with our care," it was best to emphasise the reputability of the sponsors, the 'educational and patient care benefits," as well as the safety of the drugs being tested. "Semantics" were also important: it was best not to label the activity as "research" but to speak of a "clinical study." Professional and ancillary staff were more difficult to persuade, and should be approached with tact. Private patients—a delicate subject for staff—should be categorically excluded from studies, and it was best not to overly emphasise (or mention) the use of unknowns. Indeed, the use of unknown substances was best facilitated by "the introduction of the study technique of a special observer" since it was no longer necessary "to enlist nursing office and staff acceptance of the need for drugs to be administered and study observations to be recorded by regular ward personnel."[548] Education might also be persuasive: Investigators were encouraged to accept "impromptu opportunities as well as formal lecture and seminar engagements" in order to "create a certain dissatisfaction with standard analgesics."

Lee's comments were relevant, remarked Starr, because of "how very recent this kind of clinical investigation is," even in elite research institutions.[549] To carry out successful clinical trials in the 1950s, it was necessary to be innovative not only in designing studies, but also in finding ways—by command, omission or education—to overcome resistance to certain practices. Introducing special research assistants, who were paid and educated to accept the need for blinding and regular recording of data, was part of the solution. However, hospital staff still had to be persuaded to accept the presence of researchers in their workspace and to collaborate, or at very least not to interfere, with practices that might seem intrusive of their space and harmful to patients.

6.2.2 Observers

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[548] L. Lee Jr., "Progress Report on Establishment of a New Clinical Facility for Testing Analgesics," *Bulletin of the Committee on Drug Addiction and Narcotics* (1956), 1404-1408.
[549] Isaac Starr, comments on: L. Lee Jr., "Progress Report on Establishment of a New Clinical Facility for Testing Analgesics," *Bulletin of the Committee on Drug Addiction and Narcotics* (1956): 1408.

Full-time observers were introduced to analgesic testing by Beecher as a means of eliminating prejudice, suggestion and personal quirks from the practice of collecting data. Beecher spoke of these technicians almost as if they were a component of the method itself: they were listed alongside placebos and randomization as one of the essential controls in tests of subjective drug effects.[550] He valued the technician for her lack of investment in research or patient care: not knowing, and not caring, about the patient's treatment kept her neutral. Naïve observers with a high turnover rate were the best: Beecher used "college girls."[551] Later CDAN grantees, however, preferred observers to have nursing training and increasingly counted them as full members of analgesic research teams. They valued the observer for the expertise she brought to data collection, and began to pay more attention to training and understanding of experimental rationale as a means to instil in new observers the necessary skills and motivation to perform their task—questioning patients in a consistent and objective manner—adequately. From the late 1950s, observers began to be referred to by name in publications and CDAN reports, either in the discussion, acknowledgments or authors.[552]

Whether they had skilled research nurses or interchangeable technicians in mind, CDAN members and grantees gave observers a central role in analgesic evaluation. They criticised the use of ward nurses to collect impressions of

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[550] For example, Beecher, "Experimental Pharmacology,"160, describes the importance of using a constant investigating team under a subsection titled "design of the experiment."

[551] Henry K. Beecher, "Studies on Narcotics: Annual Report," *Bulletin of the Committee on Drug Addiction and Narcotics* (1955): 1073.

[552] Stanley L. Wallenstein, Ada Rogers and Raymond W. Houde, "Relative Analgesic Potency of Phenazocine and Morphine," *Pharmacologist,* 1 (1959): 78, this is the first article I have found in which A. Rogers, the nurse-observer, is listed as one of the authors in reports of the Sloan-Kettering Memorial Cancer Hospital team. See also: Richard B. Paddock, "Report of the VA Cooperative Analgesic study" *Bulletin of the Committee on Drug Addiction and Narcotics* (1964), 3900, in which he thanks the VA nurse observers in the acknowledgments. Their names were Mrs Margaret Armour, Mrs Mary Lou Garvey, Mrs Sharon Sellers, Mrs Shirley Schmelzer, Miss Mary Boyle, Mrs Betty Davis, Mrs Virginia Snyder. Thomas G. Kantor, "Application for Grants," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966): 4457-4466, in a list of previous studies performed, Kantor gives basic information about the study (date, substances tested, number of subjects) including the name of the nurse who performed the study. In the discussion of these studies, the observers are also named.

analgesic efficacy in trials.[553] An impartial observer made all the difference between "clinical impression" and "clinical experimentation." As Beecher wrote in 1948: "the responsibility of testing falls on full-time observers such that objective, quantitative data, rather than clinical impressions, are obtained."[554] This statement echoes similar comments made by CDAN members and grantees in their reports and meeting discussions.

Despite the modesty of these salaries, observers added significantly to the cost of running clinical studies, and were usually paid from CDAN grants. CDAN members and grantees thus showed the valued they placed on these workers by their willingness to invest limited research funding towards their salaries. The creation of the full-time observer was a direct result of the increase in size of grants for analgesic testing. Beecher had introduced this practice when he was a beneficiary of his Army grant, and was able to continue it with CDAN funding. On the basis of scattered evidence of grantees' finances, we can see that a significant proportion of research budgets was allocated to observer salaries. In 1952, Beecher asked for $5,980 to pay technicians out of a total $16,679 budget (part of this budget was for a separate study of side-effects, so the salary's proportion of the cost of clinical trial work was even higher).[555] When Lyndon Lee applied to CDAN for funding to carry out analgesic research in 1955, he requested the amount to cover the salary of an observer: $3,564 for a lay observer or $4,070 for a nurse at base pay. Raymond Houde's budget for 1963-64 included four salaries for his two research associates, a secretary, and his "clinical research technician."[556] When a VA study was envisaged in the early 1960s, Richard

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[553] "Minutes of the 12th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 643-644, when a team of researchers not funded by CDAN presented its results at one of a 1953 committee meeting, Beecher and Lasagna criticized the study design by pointing out the disadvantages of using ward nurses as observers.

[554] "Minutes of the 4th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1948): 61, where Beecher presented a preliminary report of his investigation of sedatives at MGH under army contract.

[555] Beecher, "Annual Report and Application for Renewal of Support to the Committee on Narcotics and Drug Addiction of the National Research Council, 21 January 1952," *Bulletin of the Committee on Drug Addiction and Narcotics* (1952): 225.

[556] Houde, "Application for Grant," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963), 3707.

Paddock, chief of anaesthesiology in the San Francisco VA hospital, wrote to Eddy saying that his team was "anxious to participate in the study" but, he implied, this would be impossible without sufficient resources to hire an observer and a clerical assistant.[557] Thus, observers were not an optional expense that might improve the analgesic clinical trial but one of the conditions that made it possible.[558]

Though multiple observers made trials more expensive, collecting data over a period of several shifts could speed up studies significantly. This was seen as particularly important when studies used postoperative patients, whose pain diminished rapidly over a period of days, and who received fewer doses of medication. Lee had proposed to begin with a single observer, but once the value of the study had been established, he hoped to increase his budget to $12,000 in order to carry out full-time observation.[559] In 1956, he began to apply pressure to obtain a second observer. Recognising the need for this, Nathan Eddy, secretary of CDAN, arranged to obtain funding from Merck to pay a second observer.[560] CDAN's principal financial contribution to the VA study in the mid-1960s was the salary of second observers.[561]

At Sloan-Kettering, however, where analgesic studies were conducted on chronic cancer patients, a single observer did all the work. Houde and his team often insisted that this was the only way to ensure constancy in "the relationship between the observer and the patients." This relationship was a "variable" that

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[557] Richard Paddock to Eddy, August 7, 1961, Box 1: Grantees: Lee, Lyndon E. Jr, CDAN, NASA.
[558] CDAN members were, nevertheless, ready to find ways to cut the costs of hiring observers. They probably hired women, and lay observers, because they could pay them less. For example, Eddy suggested to Paddock that he hire lay observers, such as medical students' wives, to save some money, though nursing training was valuable: "while the observers will be rated as technicians, it would be desirable if these individuals have nursing training to such an extent that the hospital can authorize them to administer the medications,"in: Eddy to Lee, December 20, 1961, Box 1: Grantees Lee, Lyndon E. Jr., CDAN, NASA.
[559] Lyndon Lee Jr., "Protocol: Clinical Investigation of Analgesic Drugs, Wayne County General Hospital and Infirmary, Eloise, Michigan, n.d.," Box 1: Grantees: Lee, Lyndon E. Jr., 1954-1969, CDAN, NASA.
[560] Eddy to Starr, May 18, 1956, Box 1: Grantees: Lee, Lyndon E. Jr., 1954-1969, CDAN, NASA.
[561] Eddy, *The National Research Council*, 107.

could not be controlled otherwise.[562] Throughout their involvement with the CDAN research program, the Sloan-Kettering team included the same observer: Ada Rogers. Over the 42 years she worked on analgesic studies at Sloan-Kettering Memorial hospital, the whole time on a "lousy" salary paid out from grants, Rogers developed considerable expertise in running analgesic clinical trials.[563]

Apart from being responsible for the collection of data, Rogers was consulted in designing analgesic study methods. Eventually, she also became involved in the training of new observers, the selection of subjects, and was included as a co-author in published reports from Sloan-Kettering starting in 1959. As Rogers herself put it: "the first couple of papers" published by Houde and Wallenstein, "my name was never put on it and I complained about it because I did the work."[564]

Her oral history, collected for the John C. Liebeskind History of Pain Collection, provides a sense of how she viewed her own expertise, what made her a good analgesic trial observer, and the skills she identified as necessary in other observers. Though Rogers' lengthy experience was probably not representative of other observers', particularly not of the lay technicians employed by Beecher, about whom little information was recorded, her views were influential: not only did Rogers herself conduct many CDAN-funded and other trials over the years, but she was also involved in selecting and training new observers at Sloan-Kettering and elsewhere. In particular, she trained some of the observers who would work on the VA cooperative study. In addition, the model she represented—of the trained research-nurse rather than the naïve technician—would endure: By the mid-60s, "especially trained observers" seem to have

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[562] Houde and Wallenstein, "A Method for Evaluating Analgesics in Patients with Chronic Pain," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 661.
[563] Marcia Meldrum, Oral History Interview with Ada Rogers, 12 September 1995, MS C 127.31, History of Pain Collection, Darling Library, UCLA, 7-8.
[564] "Oral History Interview with Ada Rogers," 26.

become the norm in the design of studies reported in CDAN.[565] In 1991, an edited volume on analgesic clinical trials included a section describing the training and skills of nurse-observers, to which Rogers contributed. The principal article of the section concluded: "The nurse-observer, or analgesic study nurse, is the central figure in an analgesic study. She provides day-to-day standardisation for the system of measurement. Appropriate selection, training, and monitoring of the nurse-observer increases the probability of being able to repeatedly conduct sensitive analgesic assays." [566]

Rogers developed a new type of expertise as both a researcher and a nurse. On one hand, after years of experience, she tells that: "...I didn't consider myself a nurse per se any more. I considered myself a clinical pharmacologist, really, basically."[567] She wore a lab coat, was respected by patients and doctors, and envied by other nurses. She was the first nurse at Sloan-Kettering Hospital to become a researcher in 1951, and, when she was promoted to research associate in 1980, was the first to occupy a professional position. [568] On the other hand, she had the bedside contact that distinguished her from physician investigators. Her knowledge was different from theirs because they were "not at the bedside doing studies and seeing these people, and knowing how they respond to things."[569]

Rogers listed bedside experience among her criteria for recruiting new observers.[570] Good bedside manners, however, did not involve becoming personally involved with patients, or assuming that one knew them well enough to judge their level of pain. When it came to collecting data, Rogers didn't praise observers' skills in making judgments but their ability to suspend them. One of the main lessons she taught new observers was how to question patients objectively, refraining from asking leading questions, making assumptions about

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[565] T. G. Kantor, "Application for Grants," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966), 4458. J. Parkhouse, "Observers, Patients and Drugs in the Study of Mild Analgesics," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966), 4769-4778.
[566] J. A. Forbes, "The Nurse-Observer: Observation Methods and Training," in *The Design of Analgesic Clinical Trials*, Advances in Pain Research and Therapy Vol. 18, ed. M. Max, R. Portenoy and E. Laska (New York, Raven Press: 1991), 619.
[567] "Oral History Interview with Ada Rogers," 16.
[568] "Oral History Interview with Ada Rogers," 7.
[569] "Oral History Interview with Ada Rogers," 21.
[570] "Oral History Interview with Ada Rogers," 67.

a patients' pain, or trying to guess which drug they got: "This is what you try to teach people, you know, how to question people, and especially if you're doing a study and you're saying, 'You're not having much pain now.' That would be leading the patient ... So you really need to train people to think before they speak, before you let it out."[571] It was important not to interfere with information given by the patient, to obtain "their" answer which was "the" answer: "when you're doing double-blind study, it's really a double blind. At least, I was very—I didn't care what they got. And I think that made me a good researcher. I had—maybe it was just that I understood what we were doing—... And it doesn't matter what they got. They gave me their answer, they gave me *the* answer.[572]

A good observer should also, according to Rogers, be able to recognise how emotion might hamper her ability to collect data impartially. For example, Rogers recounted how, when she learned that John Kennedy had been assassinated, she was glad that she had already completed the collection of data. If she hadn't, she says, she would have had to "go back and redo it." "It was just such an emotional event," she explains, "that I'm sure that would have entered into the questioning [of] the patients." A good observer was one who, because she understood "the whole principle" of the studies would have been able to recognise "that that would have been a time to redo all those drugs." She added: "I often wonder when people do studies if they really are aware of all the things that enter into being objective."[573]

To be good, according to Rogers, an observer had to "understand what you're doing and the whole principle"[574] of clinical therapeutic evaluation. This understanding was the product of experience, training and, to some extent, personal ability: "observers have to be trained by someone who's done studies. ... And you can't assume because she's a nurse that she's going to know how to do these things, and they don't, unless you train them. And some nurses are very

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[571] "Oral History Interview with Ada Rogers," 62-3.
[572] "Oral History Interview with Ada Rogers," 61.
[573] "Oral History Interview with Ada Rogers," 63.
[574] "Oral History Interview with Ada Rogers," 61.

good and some are not. It's like everything else, you know. Some people seem to grasp the meaning of the study...and others, forget it."[575]

Training observers became an important aspect of the implementation of cooperative analgesia trials in VA hospitals in the 1960s. When the protocol for the study was being discussed in 1962, Weldon Bellville reportedly "emphasized that training of observers has been, in his opinion, the most important phase of a pain study. A well trained observer who understands the purposes of the study is enthusiastic. Enthusiasm and interest in the study were felt to provide motivation for the observer to work intelligently within the framework of the experimental protocol rather than performing the job in a routine manner."[576] The observer for an initial pilot study was sent to Ada Rogers at Memorial Sloan Kettering for a week, before beginning at the Palo Alto VA Hospital. When the study was extended, new observers were sent to Palo Alto or Memorial. "Observer training has thus been standardised," it was noted in a report of the cooperative program. It is likely that the VA trainees were instilled with the knowledge and skills Rogers described, in her oral history, as being important to the success of analgesic clinical trials. The importance of the job they performed was also recognised. In a later report of the VA Cooperative Analgesic Study, Richard Paddock included in his acknowledgments "the following nurse observers without whose careful and thoughtful work the data from this study could not have been collected."[577]

Even in such recognition, however, observers were usually portrayed as workers rather than thinkers, technicians rather than scientists. The observers who worked on CDAN-funded projects were, without exception, women and relatively poorly paid. The gendered hierarchy of clinical trial work relegated them to a secondary "epistemic role": Their work was good because it was "careful and thoughtful" rather than expert and knowledgeable, while the objectivity of the

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[575] "Oral History Interview with Ada Rogers," 79.
[576] Bellville in "Minutes of the Committee Meeting, VA Study Group in Anesthesia and Analgesia, October 21, 1962," Box 2: VA Cooperative Project, CDAN, NASA.
[577] R. B. Paddock, "Report of the VA Cooperative Analgesic study" *Bulletin of the Committee on Drug Addiction and Narcotics* (1964): 3900, in which he thanks the VA nurse observers in the acknowledgments. Their names were Mrs Margaret Armour, Mrs Mary Lou Garvey, Mrs Sharon Sellers, Mrs Shirley Schmelzer, Miss Mary Boyle, Mrs Betty Davis, and Mrs Virginia Snyder.

data was protected by their restraint rather than by active decisions based on an understanding of the multiple factors that could affect data collection and the rationale of study design. However, Rogers' recollections, and the emphasis put on observer-training in the VA studies, provide a glimpse of just how important observers' knowledge, experience and sensitivity was in actively producing the consistency and reliability of analgesic data.

6.2.3 Statisticians and Psychologists

From the outset, Beecher was determined to set his method of analgesic evaluation on a sound statistical footing. In 1947, as he was developing the design of his clinical trial, Beecher formed a longstanding alliance with Frederick Mosteller. Mosteller had only recently completed his PhD and had been hired as a lecturer and research associate in Harvard's Department of Social Relations. He went on to become Professor of Mathematics in 1951 and led the effort to found a Department of Statistics at Harvard, of which he was the chairman from its inception in 1957 to 1969.[578]

Consulting the young Mosteller had not been an obvious thing for Beecher to do. As Harry Marks has pointed out, collaborations between statisticians and medical researchers had been almost inexistent before World War II and were still rare by 1950. In some ways, statisticians threatened medical authority by "creating doubt about the basis for physicians' belief" in the cause-effect relationship between drugs and changes in their patients, and by substituting the statistical management of subjects and data for physicians' ability to identify fluctuations and variations in disease processes. Despite this, Marks argues, statisticians became allies for reformers who sought to shift the responsibility for therapeutic judgment from personal sources of authority—experts and institutions—to impersonal devices—methodological designs and calculations.[579]

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[578] Stephen E. Fienberg, "Statisticians in History: Frederick Mosteller, 1916-" American Statistical Association, https://www.amstat.org/about/statisticians/index.cfm?fuseaction=biosinfo&BioID=10 (accessed April 29, 2006).
[579] Marks, *The Progress of Experiment*, 129-163.

Beecher presented statistical expertise as a crucial condition for the objective clinical assessment of subjective drug effects. As he wrote in 1952: "There is a great field for study here, but it is a field where there are many obstacles... chance or coincidence to be forced into the open by intricate and laborious statistical methods."[580]

Beecher consulted his statistician not only to analyse data more objectively, but also to introduce modifications in study designs, investigate the influence of various factors on data distribution, and determine possible ways of improving or simplifying the collection of data. Though Beecher's original methodological model seemed to give reliable data, it was repeatedly described as slow and expensive.[581] In order to make his method more sensitive to analgesic effects and more economical in time and in "subject material," Beecher embarked on a series of methodological investigations (which were funded by CDAN grants).

One of Beecher's great concerns was the question of how responses to placebos might affect analgesic study results. To investigate this question, Beecher explained to the Committee, new experimental tools were necessary. For this, Beecher argued in his grant proposal "the aid of a top level statistician is essential. Professor Mosteller has become interested in this problem and if an application is approved will be available for this aspect of the work."[582] When the Committee visited Beecher's laboratory in 1951, they noted, in their report, that "The alliance with Dr. Mosteller, the Professor of Statistics at Harvard, in the design of the experiments and the work-up of the data impressed the committee as

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[580] Beecher, "Experimental pharmacology," 162.

[581] Beecher himself acknowledged that his method demanded conditions that were "complex and exasperatingly time-consuming," as well as "costly," see: "Experimental Pharmacology," 161. See also: "Report of the NRC committee, Dr Isaac Starr, chairman, to visit Dr. Henry K. Beecher's laboratory in Boston, Staturday, February 24th" Box 1 : Grantees: Beecher, H. K., 1950-1966, CDAN, NASA: in which Beecher's analgesic clinical trial was described as "extraordinarily time-consuming, not very accurate, and extremely expensive."

[582] Beecher, "Annual Report and Application for Renewal of Support to the Committee on Narcotics and Drug Addiction of the National Research Council, January 21, 1952," *Bulletin of the Committee on Drug Addiction and Narcotics* (1952): 222-3.

very valuable."[583] In his landmark study on the placebo, Beecher also enlisted the help of psychologist John von Felsinger.

Louis Lasagna also emphasised the importance of statistical consultation for clinical research. Lasagna explained that expert statisticians were the "most attuned to the need for safeguards against bias" and could also "frequently increase the efficiency of a trial." Lasagna warned that "failure to utilize the aid of such specialists can be disastrous." It was not enough to call in the statistician at the end, to "chant a few mathematical formulas or Greek symbols over the corpse of an ill-planned experiment" in the hope of resuscitating it. Statisticians were most useful if they were involved from the planning stages.[584]

Raymond Houde developed a close and long-standing collaboration with psychologist Stanley Wallenstein, who was his research associate for over twenty years. According to Houde's research application budgets, Wallenstein was "responsible for the design and analysis of studies, statistical evaluation and psychological investigations."[585] It is not clear what, exactly, these "psychological investigations" consisted of. However, the evolution of Houde's research practices over the years show that numerous innovations in the statistical analysis of data were one of the principle means by which study designs were improved and simplified. Statistical manipulations often enabled more information to be squeezed out from fewer data. For example, Houde and Wallenstein sometimes used a "factorially designed experiment" in order to obtain "much more information with the same amount of data."[586]

Statistical means were also used to make results from different trials more comparable. Houde and his team used a statistical technique, the "ridit transformation," to convert scores obtained from individual studies into "a

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[583] "Report of the NRC committee, Dr Isaac Starr, chairman, to visit Dr. Henry K. Beecher's laboratory in Boston, Staturday, February 24th," Box 1 : Grantees: Beecher, H. K., 1950-1966, CDAN, NASA.
[584] Louis Lasagna, "Controlled Clincal Trial : Theory and Practice," *Journal of Chronic Diseases* 1 (1955): 355-356.
[585] Houde, "Application for Grant," *Bulletin of the Committee on Drug Addiction and Narcotics* (1962), 3707.
[586] Houde and Wallenstein, "A Method for Evaluating Analgesics in Patients with Chronic Pain," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 665.

probability unit relative to an identified distribution."[587] In other words, this technique could make data from various trials comparable, despite variations in results obtained from different patient samples, in order to amass a greater amount of relevant data. It proved to be particularly useful for the VA Cooperative Analgesia Study, since, it was explained, it "tend[ed] to normalize the distribution and compensate for so called 'slippage' in the data."[588] The VA Study also relied on the statistical assistance of William Brown.[589] Working with statisticians, and sometimes psychologists, was an important means by which investigators sought to manage variability in data about analgesic effects.

6.2.4 Subjects

The patients who served as study subjects were also important participants in the project of making analgesia measurable. Though they provided their services ephemerally, and, before 1962, usually unknowingly (that is, they were not informed about the study or asked for consent), the analgesic clinical trial would have been impossible without subjects' active, and conscientious, participation. Their own judgments about their feelings of pain made up the raw data that would be transformed into information about analgesic efficacy. The varying ability of subjects to make such judgments was a central and difficult preoccupation for CDAN researchers. They knew that better subjects made better data. But what made a subject "good"? How could they select such subjects? How far could they go in excluding certain types of subjects before their samples became biased?

Research reports rarely detailed the criteria for selecting subjects, beyond specifying that these should be "willing, cooperative, undistracted (sic)," [590]

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[587] Houde and Wallenstein, "Studies of Narcotics at memorial Cancer Center. I. Clinical Analgesic Studies," *Bulletin of the Committee on Drug Addiction and Narcotics* (1958), 1794-1811

[588] Paddock, et al. "A Cooperative Program for the Evaluation of Analgesics in Five Veterans Administration Hospitals," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963), 3532.

[589] Brown was affiliated with the Department of Statistics of Stanford University.

[590] Beecher, "Experimental Pharmacology," 160.

"capable of communicating their subjective experience,"[591] or able "to communicate well with the observer."[592] In addition, they usually mentioned that patients should have pain severe enough to require a narcotic, and that they should not have any adverse reactions to narcotics. Such minimal qualifications seemed to imply that subject groups were representative of "ordinary people" whose idiosyncrasies and unreliable judgment could be cancelled out by proper study designs. In practice, however, investigators gave a lot of attention to the quality of their subject samples. When Eddy remarked in a CDAN meeting that "results from analgesic studies slow to be made available," Beecher "replied that the type of work he was doing requires very careful selection of patients and that it was difficult to find enough suitable cases even among the large number of patients in the Massachusetts General Hospital."[593] Investigators' reluctance to provide details about how patients were selected suggests either that the procedure was difficult to explain or to codify, or, perhaps, that it was deemed somehow indelicate to draw attention to the handpicking of subjects. Such information might threaten the perceived representativity of samples and the image of "naturalism" with which clinical experiments were associated.

It is difficult, in most cases, to even determine whose task it was to carry out the task of selection. In an oral history, Louis Lasagna remarked that he selected patients for Beecher's studies in the early 1950s: he went to see them pre-operatively, and then wrote orders for them. He selected among non-private hospital patients, and did not explain the study to them, nor did he ask them whether they wanted to participate.[594] These subjects were also men. According to Lasagna, women were excluded because of Beecher's "stereotype that women,

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[591] Houde and Wallenstein, "Studies on Analgetics at Memorial Hospital. The Evaluation of Analgetics in Incurable Cancer Patients," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 417.
[592] Paddock, "Report of the Veterans Administration Cooperative Analgesic Study: Analgesia, Relative Potency and Side Effects with Pentazocine (Win 20,228)," *Bulletin of the Committee on Drug Addiction and Narcotics* (1964): 3883.
[593] "Minutes of the 8th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1951), 192-3.
[594] "Oral History Interview with Louis Lasagna," 4-5

because of the menstrual cycles, have more ups and downs than men do."[595] In Lasagna and DeKornfeld's studies in the late 1950s, "patients were selected by a single physician," but they do not give further details in their report. A budget record indicates that, when William Beaver joined Houde's team, his job was to "assist in the screening and selecting of patients."[596]

At some point, however, it became Rogers' job to select patients in the Memorial trials.[597] Her oral history provides more details on the criteria she used. It wasn't desirable, for example, for a patient to die on the study, in case the death was imputed to the effect of an experimental drug, and Rogers became skilled at guessing when her terminal cancer subjects were nearing the end of their lives.[598] In screening for subjects, she had to determine whether they would be able to answer questions quickly and decisively. She even claimed to be able to tell a lot from their occupation. Doctors would usually refuse to participate in studies, while "engineers or statisticians...were going to be very fussy and be very exact." Rogers "found that the patients with the least amount of education probably were better patients because they, it was a gut feeling on their part."[599] There were also "some patients who can't. They cannot tell you how much pain they have. To them, it's all one pain and it's all the same... So that type of patient doesn't make a good study patient at all. But you do run into problems like that, you know. And the thing is that, if I thought the patient was not a good subject, I would drop them.[600]

Investigators also attempted to increase the reliability and precision of their data by devising means of interrogating subjects that were adapted to their subjects' ability to discriminate between levels of pain intensity or quantities of

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[595] "Oral History Interview with Louis Lasagna," 35. Beecher also mentioned this criteria in "Experimental Pharmacology," 160.
[596] Houde, "Budget for 1963-4," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963): 3707.
[597] T. G. Kantor, "Application for Grants," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966), 4457-4466: in the Kantor's studies at Bellevue Hospital it was also the role of "especially trained nurses" to "select, medicate, and monitor patients." It seems to have become relatively common, by the 1960s, to give the job of subject selection to observers.
[598] "Oral History Interview with Ada Rogers," 15: "I could tell within 24 hours, you know."
[599] "Oral History Interview with Ada Rogers," 14.
[600] "Oral History Interview with Ada Rogers," 14.

relief. They did this by experimenting with different rating scales and assessing subjects' responses to them. Beecher's team, for example, started out with the series of pain relief categories "none" "slight" "moderate" and "complete" but, after finding that they were unable to make them "meaningful in terms of analgesic potency" they searched for other options.[601] Another set of terms: "one dollar pain", "seventy-five cent pain", etc. gave distinctions that were "not sharp enough."[602] Finally, Beecher's team settled on two categories only: relief and no relief, the first being defined as "the disappearance of 'most' or 'more than half' of the pain," while anything less was counted as "no relief."[603]

Houde's team also tried different strategies. They preferred to measure pain intensity, rather than relief, because they felt that judging relief depended on unreliable memories. They tried the responses "bearable" and "unbearable" but found that "bearable to the stoic is quite different from bearable to the hypersensitive individual."[604] Pain charts didn't work because patients failed to fill them out regularly, and attempted to use them to get attention, and «what little data was presented was too highly loaded with emotional and other factors to be valuable for our purposes." In the end, "no pain", slight, moderate and severe pain, and "agony" seemed, for Houde's team, "to represent the smallest degree of pain both meaningful to, and easily measurable by, all our patients."[605] Though they recognised the categories to be arbitrary, they worked empirically.[606]

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[601] Beecher, "A Method for Measuring Pain in Man," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 649.
[602] Beecher, "A Method for Measuring Pain in Man," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 658.
[603] Beecher, "A Method for Measuring Pain in Man," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 649.
[604] "Minutes of the 9th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1952): 205
[605] Houde and Wallenstein, "Studies on Analgetics at Memorial Hospital: The Evaluation of Analgetics in Incurable Cancer Patients" *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 417.
[606] Houde and his colleagues recognised these categories to be arbitrary, and to mean "different things to different patients,"see: Houde and Wallenstein, "A Method for Evaluating Analgesics in Patients with Chronic Pain," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 661. However, they were widely considered to "work empirically in distinguishing between active and inactive drugs." See: Lasagna and De Kornfeld, "Annual Report on Analgesic Testing," *Bulletin of the Committee on Drug Addiction and Narcotics* (1959):1981.

Other investigators, such as Lasagna and his associate Thomas DeKornfeld, nevertheless continued spending time attempting to identify other factors that might influence how patients rated relief.[607] Thus, investigators did not simply trust subjects to provide reliable assessments of their pain, but instead put a lot of emphasis on searching for means of interrogation that would make patients' judgments easier to make and thus more likely to be accurate.

Subjects' abilities to discriminate between test drugs also came under investigators' scrutiny. Beecher's studies on the placebo effect originated with the concern that a large number of "placebo-reactors" in their patient samples might "submerge" the data from "non-reactors," the latter being responsible for showing a significant drug effect.[608] Therefore, with various collaborators, Beecher attempted to determine whether some subjects responded to placebos consistently, and whether they could be identified by psychological tests—a standard interview, an IQ test, a thematic apperception test and a Rorschach test—and by the observations of ward nurses. Beecher also designed experiments to investigate the effects of conditioning and suggestion on relief, for example by giving subjects three doses of morphine and then a dose of placebo, or vice versa, to determine whether immediate previous experience influenced the placebo response.[609] In the end, there appeared to be no "tags" that investigators could "put on these people" in order to make more uniform and discerning subject

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[607] Louis Lasagna and his assistant Thomas DeKornfeld conducted a study in which they asked subjects to place different levels of pain intensity on a "pain thermometer" ranging from 0 to 100 degrees, and to judge which drop in pain intensity they would be "most grateful for." They found that subjects varied widely in how they spaced out pain intensities, and what kind of relief they would desire most. See: Lasagna and De Kornfeld, "Annual Report on Analgesic Testing," *Bulletin of the Committee on Drug Addiction and Narcotics* (1959), 1978-1986. In their previous study, the same researchers decided to replace direct questioning with a peg board in order to determine whether their results would match more closely with the results obtained by this method by another team, see : "Minutes of the 19th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1958).

[608] Beecher, "Annual Report and Application for Renewal of Support to the CDAN of the NRC, January 21, 1952" *Bulletin of the Committee on Drug Addiction and Narcotics* (1952): 222, a high proportion of placebo responders might also lead results to underestimate effective drug dosage.

[609] Beecher, "Progress Report, June 1-31 December 1950," *Bulletin of the Committee on Drug Addiction and Narcotics* (1951): 140. See also: Beecher, "Suggestion Study," *Bulletin of the Committee on Drug Addiction and Narcotics* (1952): 245, in which patients were given saline and told they were "going to receive a wonderful new drug" or morphine, described as "a new drug that probably would not relieve his pain to study the effects of suggestion on relief.

samples, but the types of experiments that were carried out clearly show that investigators hoped that greater knowledge about their subjects' psychologies would help them to produce more reliable experimental samples.[610]

Investigators' concerns about subjects' varying ability to perform their experimental role were again manifested as a response to the introduction of informed consent legislation by the FDA in 1962. This provision had provoked "considerable discussion" among committee members, who worried that the "requirements of a double-blind study might preclude the obtaining of consent..."[611] Even Beecher, now famous for blowing the whistle on a series of "unethical" experiments in 1966, wrote, in 1965, to Commissioner Larrick of the FDA to warn that, if the consent provisions were to be followed strictly, it would be "difficult, if not impossible" to continue doing this type of study. Beecher proposed that, in some cases, consent was not only unnecessary but even harmful: "It has seemed to me that no violation of ethics occurs when no discernible risk is involved and when discussion with the subject would jeopardize, if not destroy, the possibility of getting valid data. This applies as much of the work on the effects of drugs on the subjective responses."[612]

To solve the problem of maintaining the double-blind while still obtaining consent, Houde's team developed their own consent form as "a more reasonable substitute" to the one that had been imposed by the New York City Department of

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[610] Beecher, "A Study of the Placebo Response," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 377. Houde's team did not feel justified in eliminating "placebo responders" from their samples because of the risk of introducing a selection bias. However, they were also concerned with the fact that the sensitivity of their method was limited by the "discriminative ability" of their groups. Their opinion might have been different, they suggested, if there existed a means of predicting subjects' responses before they entered the study.However, Houde later pointed out that some "placebo-reactors" were also "discriminators," that is, they could "tell the difference" between active and inactive drugs, and therefore it was not wise to exclude the valuable data they contributed to positive results.See: "Minutes of the 11th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 366; Houde and Wallenstein, "Studies on Analgetics at Memorial Hospital: The Evaluation of Analgetics in Incurable Cancer Patients," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 421; Houde and Wallenstein, "A Method for Evaluating Analgesics in Patients with Chronic Pain," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953): 670.

[611] "Minutes of the 25th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963): 3115

[612] Beecher to Eddy, November 4, 1965, Box 1: Grantees: Beecher, H. K., 1950-1966, CDAN, NASA.

Hospitals. The new form provided no drug names, nor did it mention the use of placebos.[613] The self-selection of patients who accepted to participate in studies was still considered as a possible source of bias, and the question became the hypothesis of an experiment by Thomas DeKornfeld in 1966, who attempted to determine whether consenters were more or less susceptible to analgesia than non-consenters.[614] At the same time, Lasagna and his collaborator John Pearson were also conducting studies on consent issues. They found a very high refusal rate in postpartum patients, raising questions about wasted time and "the scientific value of data from such a special minority."[615]

CDAN investigators evidently remained uneasy about leaving the task of judging analgesic effects to imperfect and idiosyncratic subjects. They made various types of attempts to increase the accuracy and uniformity of data by improving subjects' responses through more rigorous selection or better methods of interrogation. To do this, they tested their subjects: their personalities, their susceptibility to suggestion and conditioning, their ability to manipulate different types of measurement instruments and their reaction to being asked for consent. Even though many of these studies did not lead to concrete methodological changes, they testify to the hope held by researchers that these would be fruitful avenues of investigation: that by obtaining more information about the patterns of subjects' responses to the experimental situation, they could tighten their experimental control. These investigations treated subjects as thinking and feeling individuals, who were likely to be influenced by their emotions, their prior experience and their environments. In other words, in order to increase the objectivity of their methods, investigators found it necessary to enrich their understanding of the subjectivity of patients' judgments of pain.

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[613] R. W. Houde, S. L. Wallenstein and W. T. Beaver, "Analgesic Studies at Memorial Sloan-Kettering Cancer Center," *Bulletin of the Committee on Drug Addiction and Narcotics* (1965): 4157-4158.

[614] T. J. DeKornfeld, "Annual Report on Analgesic Studies," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966): 4717-4725, in this study, one group of patients was asked for consent, another was not. Both were given morphine sulfate, the standard drug. Both groups seemed to respond similarly, but it was possible that the consenters received more relief than the non-consent group. DeKornfeld recommended"much additional work" on this problem.

[615] J. W. Pearson and L. Lasagna, "Recent Experience in Clinical Analgesic Evaluation," *Bulletin of the Committee on Drug Addiction and Narcotics* (1967), 5155-5158.

6.3 CDAN in the Middle: the Politics of Sponsoring

It is somewhat surprising that the pharmaceutical industry paid for the development of analgesic clinical trial methodology. As Harry Marks has shown, the RCT was promoted by many reformers who believed this technology could protect the autonomy and neutrality of therapeutic judgment against the commercial motivations of drug manufacturers. In addition, pharmaceutical firms were not required before 1962 to prove the efficacy of new drugs in order to obtain FDA approval. However, as we've seen, pharmaceutical firms seemed primarily interested in testing the efficacy of analgesics for purposes of innovation rather than regulation, though information about efficacy also entered into calculations of toxicity, and thus served a regulatory purpose. Industry interests in analgesic innovation thus motivated pharmaceutical firms to collaborate with CDAN and contribute to a common pool, and they also demanded that such tests should be done in humans. But how did this money come to be invested in clinical trial methodology, and to pay for some research that brought no clear immediate benefits to industry?

In this section, I will discuss how CDAN funds were distributed, and whose interests shaped the content of CDAN-funded research. To a large extent, the latter was determined by researchers themselves, who were free to propose projects on the basis of work they wanted to do, or thought should be done. However, pharmaceutical representatives sometimes expressed impatience when researchers embarked on seemingly tangential lines of inquiry or failed to give results rapidly enough. CDAN members thought it important to satisfy their sponsors and secure continuing support, but mainly seemed to be concerned about their grantees' scientific autonomy. They were adamant that the research program should not, as Starr put it, "degenerate into a simple matter of clinical testing," and insisted on approving "fundamental research" bearing on methodological

issues and a broader understanding of analgesia.[616] The freedom afforded to CDAN grantees, which may not have been as great under direct industry funding, made it possible to engage in methodological explorations and perhaps, ultimately, to make the analgesic clinical trial work better.

The reactions of industry sponsors to CDAN-funded research, particularly Beecher's, provoked discussions among committee members about CDAN's duties towards "science" —that is, towards the concept of "fundamental research," the NRC's reputation and their grantees' research interests—in running a research program mostly funded by, and ultimately aimed to benefit, the pharmaceutical industry. In the early 1950s, Beecher began to turn away from the immediate practical details of how to run analgesic clinical trials, and to ask questions about how suffering subjects responded to drugs in experimental situations. He began to investigate phenomena such as conditioning, suggestion and placebo effects, looking for possible sources of error in analgesic testing. These studies, however, became less directly concerned with testing methodology, and increasingly oriented towards broader elucidation of mechanisms of pain relief, and even of the nature of pain itself.

Was such work relevant to the Committee's granting program? For over a decade, Committee members answered affirmatively, but not without discussing the nature of the granting program and particularly the weight to be given to perceived opposition from their industrial sponsors. Members often described the purpose of the granting program as the support of "fundamental research." What did this mean? It seems to have been the opposite of "our program [degenerating] into a simple matter of clinical testing," a program that would "get plenty of support from various drug houses."[617] Thus, "fundamental" qualified research that did not have immediate, practical and, particularly, commercial benefits. Committee members often expressed the need to see beyond such benefits—

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[616] "Minutes of the 11th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 388.
[617] "Minutes of the 11th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 388.

which they assumed pharmaceutical manufacturers were interested in—and thus to exclude their sponsors from "scientific" decision-making.

At the same time, the Committee portrayed itself as a protector of industry's long-term interests. Starr concluded his comment: "We are interested in fundamental research in a way that the drug houses ought to be interested in the long run, what they make money on is dependent on it." "If the program degenerates into simply drug testing," he added, "the good people aren't going to want to do it and the Research Council won't want to give time and effort to it."[618]

Committee members were also concerned about keeping their sponsors happy, and occasionally interposed themselves between sponsors and grantees' work in an effort to maintain good relations. For example, they attempted to reassure their sponsors of the quality of Committee-funded research. Along with the minutes of a 1952 meeting, a letter was sent to the pharmaceutical companies that funded CDAN. The letter not only emphasised the relevance of Beecher's work, but also underlined the importance of the Committee's opinion: "While [his method] is tedious and expensive, Dr Beecher has been doing an outstanding job on the controlled study of analgesics in man and has had the courage to tackle the study of some of the disturbing and modifying factors in clinical testing of new drugs. The Committee considers it most important to continue the study of these factors."[619]

Committee members were also interested in what their sponsors had to say about their program. In 1953, a letter seeking written comments was sent to CDAN's sponsors. This action was taken in response to a verbal comment reported by Eddy to the effect that "the direction of some of the work was taking was too non-objective." To Eddy, this implied that some pharmaceutical representatives thought that "Dr. Beecher's work appeared to be going off on a tangent instead of producing concrete results in clinical evaluation of

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[618] "Minutes of the 11th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 388.
[619] Untitled (Letter to Pharmaceutical Manufacturers), March 3, 1952, Box 2: General, 1947-June 1959, CDAN, NASA"

analgesics."[620] The comments, however, ended up being more positive than Eddy had expected.[621] Though Lederle had warned that, as "diversifications turn up the main goal should not be lost sight of because of possible primary interest in the interesting side problems,"[622] Mallinckrodt was full of praise for the Committee, which was "rendering a very definite service to the drug industry" by encouraging the study of "basic problems of mutual interest... which no one organization could afford to support alone." This was "especially true of those problems having little, or no, immediate practical application." Beecher's work, in particular, was described as "a very important contribution."[623] Two companies, Abbott and Upjohn, tentatively suggested a re-exploration of laboratory tests of analgesia, which would be more economic than clinical trials.[624]

Committee members disagreed about how much consideration should be given to industry opinions. In 1955, the Committee was discussing Beecher's departure from work on that bore directly on drug testing methods towards "more theoretical aspects" of analgesia. Cameron explained that Beecher "had been asking, 'When a drug is effective, why is it effective?'" To Cameron, this was "a most important question... The committee should support work in the testing of analgesic drugs and on the manner of their action." Seevers, however, was not sure that such work should be supported with Committee funds which: "come from industry and industry has not been satisfied with Dr. Beecher's work. If long-term support is to be considered, the work must be of some recognized benefit to industry." Despite these reservations, continued funding for Beecher's work was unanimously approved.[625]

The following year, CDAN again asked industry representatives for their opinion on whether they approved continuation of support for five sponsored

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[620] "Minutes of the 11th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1953), 387.
[621] Eddy to Starr, May 11, 1953, Box 2: General, 1947-June 1959, CDAN, NASA.
[622] Eddy to Starr, May 11, 1953, Box 2: General, 1947-June 1959, CDAN, NASA.
[623] Eddy to Starr, May 11, 1953, Box 2: General, 1947-June 1959, CDAN, NASA.
[624] A couple of studies on experimental laboratory analgesic testing methods were funded by the Committee, but the Committee did not appear to be particularly interested in these projects and withdrew its support.
[625] "Minutes of the 15th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1955): 1023.

projects, and to assign a priority to each. This evoked strong opposition from Louis Lasagna, who described the "sounding out" of drug companies as a "pernicious practice." Financial support, according to Lasagna, should be attributed "on scientific merit and not to fit with the desires of drug houses."[626] This was also the position of the NRC and, generally, of Committee members.[627] Eddy, who claimed responsibility for the "questionnaire," defended himself. Lasagna has misinterpreted his intentions. It was useful to know the feelings of representatives, who were in a position to make positive recommendations to their companies, "in framing our annual appeal for continued support." In particular, Eddy had wanted to verify whether "the feeling of criticism toward Beecher" which had been hinted at by comments from the industry, if it was real, was still existent. [628]

Such discussions, however, became less frequent. As the Committee's research program began producing more concrete results on relative drug potencies from 1958, industry support seems to have become more secure. At the same time, however, industry influence also became more present in the granting program. Individual drug companies began occasionally exerting their influence over research by earmarking funds for specific studies, usually when their own drugs were being tested. For example, in 1958, Leo J. Cass conducted a study of various substances classified as mild analgesics, some of which were already sold commercially. The Committee generally refused to fund the evaluation of mild analgesics because its focus was on narcotics. For drug companies, however, over-the-counter pain relievers represented a lucrative market. For Cass' study, Eddy recalled, "the producers of the respective active agents not only supplied their products generously, but also allocated to the Committee special funds to support the project."[629] The budget for that year shows that Endo Laboratories,

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[626] Lasagna to Cannan, February 2, 1956, Box 2: Administration—General, Drug Addiction, CDAN, NASA"
[627] Cannan to Lasagna, February 6, 1956, Box 2: Administration—General, Drug Addiction, CDAN, NASA.
[628] Eddy to Cannan, February 10, 1956, Box 2: Administration—General, Drug Addiction, CDAN, NASA.
[629] Eddy, *The National Research Council*, 76.

Burroughs Wellcome, Wyeth Laboratories and Eli Lilly and Company provided funds that were specifically marked out for Cass' studies.[630] The same year, Merck and Hoffmann-LaRoche destined their grants for Arthur Keats, who was evaluating the analgesic effectiveness of opiate antagonists, that is, drugs that countered the effects of opiates (these drugs are used to treat narcotic overdoses).[631] There was hope that this class of drugs might hold the key to developing a non-addictive analgesic, which, again, was very interesting for drug manufacturers. Such direct earmarking of funds was not common within the Committee's program. However, as clinical testing methods became better established and more teams were equipped to carry out these studies, pharmaceutical companies—as well as the NIH—increasingly provided grants directly to researchers.[632]

From the early 1960s, committee members also began soliciting support from public sources. They were able, by persuading potential sponsors that their interests in analgesic testing were mutual, to obtain additional funds for the VA Cooperative Analgesic Study from the VA Committee on Medical Affairs and the Office of Civil Defense (OCD). The VA put in from $5000 to $10 000 yearly for the VA Study from 1961 to 1970.[633] When the Office of Civil Defense (OCD) became concerned with the choice of analgesic to stock in medical kits in public fallout shelters, Eddy engaged in negotiations with the OCD concerning the possibility of obtaining financial support. The OCD put in nearly $45,000 for

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[630] "Minutes of the 19th Meeting: Budget,"*Bulletin of the Committee on Drug Addiction and Narcotics* (1958): 2090

[631] "Minutes of the 19th Meeting: Budget,"*Bulletin of the Committee on Drug Addiction and Narcotics* (1958): 2090

[632] W. H. Forrest, "Report of the Veterans Administration Coopertive Ananlgesic Study," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966): 4673-4691. Supported by VA, NIH as well as Eli Lilly & Co. and E. R. Squibb and Sons. T. G. Kantor, "Application for Grants," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966), 4466, reported receiving grants from Bristol-Myers Co., Squibb Institute, and Miles-Ames Co. in amounts ranging from 7,500$ to 25,000$ for the construction of new offices and the salary of the investigator. He had also made an application to NIH for funds for another nurse observer, a programmer, a statistician and other researchers' salaires.

[633] Eddy, *The National Research Council,* 164, table 3 of Appendix 3.

three years. "It was," said Eddy, "this added resource that allowed the Committee to assist the VA Cooperative Analgesic Study."[634]

CDAN's research program was shaped by a complex mix of interests, and it is difficult to evaluate how each influenced its final content. It seems, however, that CDAN members were fairly successful in obtaining funds—by emphasising how their interests converged with potential sponsors'—that could be deployed on their own terms. Concerned about the perceived narrowness of sponsors' interests, CDAN members emphasised the dangers of compromising the scientific integrity of the program, and, to a great extent, allowed grantees to pursue their own research interests. The result was the creation of a space in which testing methodology could be not only worked with, but also worked on, and included methodological explorations of which the relevance was questioned by sponsors. This work was ultimately aimed at improving testing practices. While this was not always the case, this work did contribute to the creation of new kinds of knowledge about the nature of pain, analgesia, and clinical experimental conditions

6.4 Points of Contact: Diffusing the Analgesic Clinical Trial

Analgesic testing technologies would be of little use to the Committee, or their sponsors, it they could not produce comparable results in multiple testing sites. As funding for analgesic testing continued to increase, through both CDAN's budget and other public and private grants, Beecher's methodological model was adapted and adopted by a growing number of researchers. In the context of CDAN's research program, the diffusion of the analgesic clinical trial can be divided into two phases. Before the organisation of the VA Cooperative Study, the clinical trial was implemented in new sites, but there was little explicit discussion about how to standardise its use. Nevertheless, it is possible to trace links of contact and communication that probably facilitated the replication of experimental procedures. As plans started being made for the VA study, however, CDAN

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[634] Eddy, *The National Research Council*, 108.

members and grantees turned their attention towards more explicit means of standardising the procedures of analgesic testing.

One of Beecher's earliest stated objectives with regards to analgesic testing was to provide a means of standardising clinical studies. From his first published papers on the topic, he drew attention to his innovative methodological devices, and provided reasons why other researchers should adopt them. His persuasiveness, assisted by broader trends that made clinical trial methodology appealing to anaesthesiologists and clinical pharmacologists, probably facilitated the initial diffusion of his method among the researchers who would become CDAN grantees.

Information about how to run analgesic clinical trials travelled through both direct and indirect contacts. Several researchers had the chance to learn the ropes of analgesic testing with an experienced investigator before setting up their own experiments in a new site. Arthur Keats and Louis Lasagna worked as Beecher's assistants, Thomas DeKornfeld worked with Lasagna, while Weldon Bellville worked with Houde. They would have become familiar with certain procedures—such as selecting patients and instructing observers—which were probably important to the smooth operation of clinical trials, but were not described in detail in early publications. In addition, new teams often, if not always, ran pilot studies to work out the kinks in their procedures. Usually they tested doses of morphine against each other—5mg against 10mg, or 10mg against 10mg—to make sure their method was capable to giving the expected results.

Other researchers did not receive this training in running trials, but they patterned their methods on Beecher's model. These researchers—Raymond Houde, Leo Cass and Lyndon Lee—were also in contact with CDAN before they began developing their clinical methods. Early on, CDAN meetings became conferences on narcotics research at which grantees, and sometimes outside guests, presented their methods and results. Houde and Lee had been invited to some of these meetings. Cass and Lee consulted with Committee members while they were working out the protocols for their studies. Though their designs were not exact reproductions of Beecher's, they adopted its basic elements and

modified others. Houde's team, for example, asked subjects about the intensity of their pain instead of their degree of relief. However, initially, they also asked the same question as Beecher because they thought it important to produce comparable data.[635] Houde's modified design also become an influential model for future analgesic studies, despite the fact that Houde did not publish on these studies outside the Committee until the latter half of the decade [636]

It is difficult to evaluate the extent or importance of the contacts and influences mediated by CDAN in ensuring the successful reproduction of methods of analgesic evaluation. However, it is clear that many clinical studies conducted in the 1950s without CDAN sponsorship did *not* adopt similar techniques for evaluating pain, placebo-controlling or employing full-time observers.[637] It would seem likely that the combination of CDAN funding—which made reproducing designs, particularly in the matter of hiring observers, financially feasible—and of the contacts provided by its activities and conferences, favoured a certain amount of conformity in the adoption of analgesic testing methods.

In the VA Cooperative Analgesic Study, however, CDAN's role in standardising practices and expertise for the diffusion of clinical trial methodology is more obvious. In preparing for the study, Committee members and VA investigators explicitly discussed questions of multi-site standardisation.

The idea of running a cooperative study of analgesics in VA hospitals was reportedly inspired by the success of previous cooperative studies run by the VA, and by the capacity of this hospital system to provide the necessary resources, particularly patients, for analgesic evaluation.[638] In the late 1950s, Lyndon Lee, who had recently been hired by a VA hospital, developed an experimental protocol with Eddy's help.[639] The VA Committee on Veterans Medical Problems approved the protocol in principle, and an Ad Hoc Group to Consider Proposed

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[635] "Oral History Interview with Ada Rogers," 5.
[636] "Oral History Interview with Louis Lasagna," 42.
[637] Kraptchuk, "The Powerful Placebo."
[638] Paddock, et al. "A Cooperative Program fo the Evaluation of Analgesics in Five VA Hospitals," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963): 3530.
[639] Gilbert W. Beebe (Committee on Veterans Medical Problems) to Starr, November 19, 1957," Box 1: Grantees: Lee, Lyndon E. Jr., 1954-1969, CDAN, NASA.

Clinical Investigation of Analgesic Drugs in VA Hospitals was formed, and met in 1958.[640] It was the consensus within this group, which was composed of CDAN members, representatives of the NRC and of the Committee on Veterans Medical Problems, that "leadership" should be provided CDAN in matters of experimental design, as well as the selection of test drugs and data analysis. CDAN's methodological leadership was essential because, as the group agreed though "the specific protocols seem well-conceived ...any research in the area is beset with pitfalls for the unwary."[641]

Those who planned the VA trials seemed to agree that written direction, such as these protocols, did not provide all the information necessary required to run clinical trials successfully. Members of the Ad Hoc Committee were concerned that it might be difficult to find "interested, suitably trained professional and technical personnel."[642] To help standardise experimental procedures in multiple sites, under the supervision of "green" investigators, the Ad Hoc Committee recommended several actions. In 1958, "the group felt that potential new investigators should be given an opportunity to visit centres where successful work of this kind is already being done. Dr. Eddy offered to assist in making such arrangements." Houde and Keats' work were chosen as models for the VA protocol. It was also recommended that a pilot study needed to be run before any large-scale evaluation began.[643]

In addition to providing advice on the design of experiments, CDAN acted as a point of contact between new investigators, observers and "old hands" in analgesic testing. As a result of an initial pilot study, which was put off until 1961

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[640] Beebe to Starr, November 19, 1957, Box 1: Grantees: Lee, Lyndon E. Jr., 1954-1969, CDAN, NASA.
[641] "Report of Ad Hoc Group to Consider Proposed Clinical Investigation of Analgesic Drugs in VA Hospitals," *Bulletin of the Committee on Drug Addiction and Narcotics* (1958): 1897.
[642] "Report of Ad Hoc Group to Consider Proposed Clinical Investigation of Analgesic Drugs in VA Hospitals," *Bulletin of the Committee on Drug Addiction and Narcotics* (1958): 1897. Also After visiting Eddy, Houde and Keats in 1961, Richard Paddock, who was to be put in charge of the VA Analgesia Cooperative Study, reported that he had found that "what these investigators were going was extremely interesting and it was apparent that careful preparation and training would be necessary before I could embark on an analgesic testing program."
[643] "Report of Ad Hoc Group to Consider Proposed Clinical Investigation of Analgesic Drugs in VA Hospitals," *Bulletin of the Committee on Drug Addiction and Narcotics* (1958): 1896-1898.

because of lack of funding, additional measures of standardisation were recommended. Bellville suggested that the training of observers was crucial for preparing successful analgesic testing. A "well-trained observer" would "work intelligently within the framework of the experimental protocol." Forrest spoke in favour of computerizing data processing and analysis. The Committee agreed that observers would be trained in New York or Palo Alto, that each VA hospital would carry out a "standardisation study"—comparing 5 and 10mg of morphine against 10mg of morphine—and that plans would be made for the centralised collection and processing of data.[644]

Those who had already built up expertise in running clinical trials with the help of CDAN funding provided guidance to VA researchers in visits, meetings and the design of the protocol. "The discussions," with experienced investigators was described by Paddock as, "friendly, interesting, and at times, outright frank."[645] Investigators were also available to work out problems as they arose. In 1963, Bellville wrote to Lee complaining that "The Yes-No scoring system doesn't seem to work for us as well as it does for Dr. Keats. It might be well to get him to stop by for a visit soon to make sure we are scoring 50% responses exactly as he does."[646]

CDAN also encouraged the development of tools to standardise the collection of data and centralise its analysis. Encouraged by Eddy and Lee, CDAN and VA investigators, along with John C. Seed, got together to collaborate in the development of a standardised data collection form that would be tested, and then implemented in VA studies.[647] Such a card would have two purposes: to render data collection practices more uniform between multiple sites and observers, and to fit data into the form required for computer processing. CDAN put its weight, and the authority of the NRC, behind the final version of the form

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[644] "Minutes of the Committee Meeting, VA Study Group in Anesthesia and Analgesia, October 21, 1962," Box 2: VA Cooperative Project, CDAN, NASA.
[645] R. B. Paddock, et al. "A Cooperative Program fo the Evaluation of Analgesics in Five VA Hospitals," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963): 3531.
[646] Bellville to Lee, August 7, 1963, Box 2:VA Cooperative Project, CDAN, NASA.
[647] W. H. Forrest, et al., "A Uniform Method for Collecting and Processing Analgesic Data," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963): 3547-62. Seed had pioneered the use of such data forms at Calvary Hospital in New York.

by approving it officially. Jonathan Cole, a member of CDAN and editor of Psychopharmacology Bulletin agreed to publish the form, along with an explanation of its history, objectives, and the desirability of its widespread use in analgesic testing across the country.[648] The form was also reproduced in the minutes of the CDAN meetings.[649] Enquiries were also made about the possibility of publishing the form in *Science*.[650] The objective of the Committee was to "urge" that the form, called the National Analgesic Study Form, be used "as widely as possible for greater uniformity and comparability of data in this field." Bellville, one of the authors of the form, was told that "[the Committee's] approval and urging can be quoted."[651] Though developed for the purpose of standardising the VA Study, it was hoped that the form might help standardise analgesic testing on nothing less than a national scale.

CDAN also took the initiative to encourage the development of a computer program to analyse analgesic data. A grant of $4,500 was made to Eugene Laska in 1963.[652] Such a program was said to have several advantages: it saved labour in processing and analysing data; it made it possible to rapidly try out different ways of analysing data, and helped to understand and manage any discrepancies in data that might arise between testing sites.[653] Using computer-analyses, the VA team were able to modify study design in ways that allowed them to collect more data in less time. For example, new methods of analysis made it possible to include "incompleters"—subjects who had not taken an entire round of medication—into the calculation of results, thus saving time in completing studies.[654]

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[648] Forrest, Jr., et al., "A Uniform Method,"1-10.
[649] Eddy to Bellville, March 21 1963, Box 1: Grantees: Bellville, J. W., 1961-1963, CDAN, NASA.
[650] "Minutes of the 25th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1963): 3116
[651] Eddy to Bellville, March 21 1963," Box 1: Grantees: Bellville, J. W.: 1961-1963, CDAN, NASA.
[652] "Minutes of the 26th Meeting," *Bulletin of the Committee on Drug Addiction and Narcotics* (1964).
[653] Forrest, Jr., et al. "A Uniform Method,"1-10.
[654] W. H. Forrest, "Report of the Veterans Administration Cooperative Analgesic Study," *Bulletin of the Committee on Drug Addiction and Narcotics* (1966) 4673-4691.

By 1964, Paddock could declare that the VA sites had evolved into "pharmacological clinical laboratories": "It has taken our group about a year and a half to organise and standardise our method of study. I believe we now have, after some eighteen months of intensive work involving many interested and competent people, developed a method of drug study which is reliable. Further, we have access to a computation center and the keen minds associated with such a place. We submit that the members and consultants of the VA Study Group... are in an enviable position to provide sound objective and well documented clinical pharmacological research to the benefit of medicine and the pharmaceutical industry."[655]

The work that went into successfully standardising, diffusing and replicating clinical trial methodology is difficult to calculate because it shows itself negatively in the *absence* of debates, disputes and discrepancies. Other attempts to resolve questions about analgesic efficacy with clinical studies in different contexts do not seem to have been as successful as CDAN-funded studies. As Marcia Meldrum has shown, clinical trials of the analgesic Darvon varied in their methodology and results, and, because of these discrepancies, and disagreements about how to interpret them, the trials carried little weight in swaying therapeutic or regulatory decisions about its efficacy.[656] As we will see in the following chapter, clinical trials of acupuncture's pain-relieving efficacy failed to be conclusive because of disagreements about how they should be run, and a lack of mechanisms and authority for coordinating the implementation of enough trials to produce sufficient volumes of data that would be seen as valid. In contrast, these cases underscore the considerable achievement in a relatively smooth process of methodological diffusion in the VA study and other CDAN-funded studies, and emphasise the importance of the contacts and coordination, as well as the money, provided by CDAN that seem to have made this possible.

Those who made methodological decisions within CDAN's program were in contact with each other, and they shared tools and information. They had

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[655] Paddock to Bird, March 2, 1964, Box 2: Winthrop Laboratories (Rice), CDAN, NASA.
[656] Meldrum, "Departures from the Design," 310-372.

access to the means to replicate certain key conditions, in particular the hiring of special staff. In addition, their interests were similar, and they were unlikely to disagree on major theoretical or methodological points. This made it possible to produce fairly consistent data about pain relieving efficacy, which was considered to be reliable and objective, despite the variability of individual experiences and expressions of pain. As the next chapter will show, it would be much more difficult to achieve these conditions when analgesic testing technologies were designed, run and evaluated by a larger number of people with heterogeneous interests.

Conclusion

Making the analgesic clinical trial into a trusted technology of pain-measurement took time, effort and resources. The formation of alliances between various sponsors interested in analgesic evaluation and innovation provided the funds necessary for the introduction of key aspects of experimental design, such as the hiring of full-time observers. The exclusion of these sponsors from decisions about the distribution of grants made it possible for researchers to explore a variety of methodological issues, such as how to best manage the active participation of their experimental subjects. Investigators engaged in this methodological work for their own reasons, as we have seen from how it fit into their career trajectories, which made them committed to making the analgesic clinical trial trustworthy. They were not able to do this alone, however. They required the assistance of various collaborators, who provided labour and expertise, particularly observers who played a crucial role in ensuring the "day to day standardisation" of the method and the production of consistent and reliable data. The cooperation of subjects was also necessary, but investigators seemed somewhat uneasy with this, and sought to obtain information about their subjects' abilities and susceptibilities. Finally, the smooth replication of clinical trial methodology required the formation of points of contact for the diffusion of

information, both tacit and explicit, and was assisted by the creation of technologies of standardisation.

7. Acupuncture, Pain and the Proof of Efficacy

When acupuncture hit the American news in the early 1970s, new actors joined in public discussions about the value of pain-measuring technologies. Journalists, chronic pain sufferers, legislators, congressmen, acupuncturists, clinicians, and various types of medical researchers expressed their views on the question: By what *means* could persuasive evidence for the pain-relieving efficacy of acupuncture be produced? Their various, and often divergent answers reflected a debate over the definition of appropriate conditions and practices for making valid judgments about experiences of pain and therapeutic efficacy. More importantly, they disputed to *whom* such judgments could be entrusted. In this chapter, I will examine representations of pain-measuring technologies, particularly the clinical trial, in the American debate on the efficacy of acupuncture anaesthesia and analgesia from late 1971 into 1974.

The discovery of acupuncture anaesthesia by Americans signalled a new era in Sino-American relations. After decades of tight restrictions on travel and communication, the "bamboo curtain" became permeable to the gaze of American eyes, lifting to reveal glimpses of a new China fashioned by the Cultural Revolution. One of its most astonishing features was acupuncture anaesthesia, a technique born from the hybridisation of ancient tradition and modern medicine that rendered surgery painless using just a handful of vibrating needles, without knocking the patient out. The American fascination with acupuncture soon generated a traffic of images and people between the two countries. Dramatic eyewitness narratives of surgical anaesthesia obtained by acupuncture became a feature of journalists' and travelling scientists' reports of their journeys to the People's Republic of China (PRC). These graphic descriptions of patients lying calmly and awake, even "cheerfully conscious"[657] as they were cut open and relieved of tumours and organs, were given wide circulation in daily newspapers and the medical press.

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[657] "U. S. Doctor Hails China's Medicine: Rosen Praises Acupuncture and Mass-Care Plan." *New York Times* (31 October, 1971), 20.

These reports were met by some members of the American medical research community with scepticism: Could the apparent pain-relieving efficacy of acupuncture be an illusion? This question continued to animate debates as the evaluation of acupuncture's efficacy moved to American soil. While acupuncture's use as a surgical anaesthetic had first made headlines, it was as a treatment for chronically painful conditions that acupuncture seemed to offer the most promise to Americans.[658] Widely circulated descriptions of acupuncture in the news and medical media brought to many Americans the hope of relief from intractable pain, and they demanded information about, and access to, the therapy.[659] As the demand for acupuncture rose, and acupuncturists' offices fanned out in certain states and cities, medical authorities pressed for the establishment of mechanisms to ensure a rigorous evaluation of acupuncture's efficacy.

It turned out, however, that it was not so easy to agree on how to properly evaluate acupuncture's efficacy. No other debate about the value of a pain-relieving therapy had drawn such a wide range of participants. The acupuncture question spread from the news media into legislative assemblies, medical societies, and research institutions, making the evaluation of pain relief into a public issue on an unprecedented scale. New groups of actors vied for authority over the evaluation of acupuncture and proposed competing methods for judging its pain-relieving efficacy. Letters were sent to the editors of the *New York Times*, the *Journal of the American Medical Association*, *Science* and other medical journals. Articles, editorials, responses and debates were published. Biomedical institutions set up committees. Thematic conferences were organised, and annual

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[658]Both can be termed acupuncture analgesia, since "acupuncture anaesthesia" did not, strictly speaking, obliterate sensations of pain and pressure, or consciousness. My focus in this chapter is on the evaluation of acupuncture for the relief of chronic pain, but I also consider arguments in the debate about the evaluation of acupuncture's effects on surgical pain.

[659]J. A. Kotarba, "American Acupuncturists: the New Entrepreneurs of Hope," *Urban Life* 4 (1975): 149-177. Indeed, Kotarba argues that the demand for acupuncture revealed the magnitude of the American chronic pain epidemic. My own analysis of the debate suggests that researchers played an important part in focusing attention on chronic pain in their public discussions of acupuncture.

conferences set up acupuncture panels.[660] There were lecture tours and televised debates.[661]

While medical researchers called for a large-scale, long-term and rigorously controlled program of evaluation, many chronic sufferers presented their own bodies—in oral and written testimonies—as evidence that acupuncture worked. These sufferers, backed by acupuncturists and their allies, family members and often by the popular press, challenged researchers' authority over evaluation, affirmed the legitimacy of individual experience, and protested the withholding of a safe and effective source of relief for lengthy and inconclusive experimentation. Medical researchers, however, often depicted these sufferers as poor judges of efficacy because of their susceptibility to suggestion, their desperation for relief, and their enthusiasm for acupuncture. Meanwhile, these researchers argued among themselves over the methodological details of acupuncture experiments and complained about the lack of resources for research.

Why was it so difficult to resolve the question of acupuncture's efficacy? Various aspects of the nature of the therapy and its effects—the subjectivity of pain, the incompatibility of acupuncture with biomedicine, the inapplicability to acupuncture of models of evaluation developed for drug therapy—impeded a quick and decisive judgment of acupuncture's efficacy. I suggest, however, that the controversial nature of the debate on acupuncture can best be understood as a conflict of interests among the various actors who were involved in it. Various tensions, as well as insufficient resources—including money, authority, agreement, and legitimacy—made it difficult for any one group to impose its views on how acupuncture should be evaluated, and thus impeded a resolution of the debate.

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[660] "Acupuncture May Find Use in Rehabilitation Medicine," *Journal of the American Medical Association* 227, no. 8 (1974): 879-82, reports that the American Congress of Rehabilitation Medicine included panel discussions on acupuncture; "AMA Grams," *Journal of the American Medical Association* 224, no. 5 (1973): 565, announces that a general session of Annual Convention on June 27 would be dedicated to potential role of acupuncture in practice of medicine in western world. These are only two examples out of many.

[661] For example: "U. S. Acupuncture: Status Report, 1973," Bonica Papers, MS C 118, Box 21, Folder 35, Darling Library, UCLA.

The ways in which elite researchers portrayed, discussed and attempted to implement technologies of evaluation reveal many of these tensions and the obstacles they faced in following their own agenda for acupuncture testing. The question of acupuncture's efficacy had different implications for the various actors involved in the debate, and control over its evaluation was seen as a means of controlling its practice, as well as a means to claim expertise in the treatment of chronic pain. In this climate, the development and implementation of pain-measuring technologies came up against new challenges.

This chapter examines how the *idea* of technologies of pain-measurement was mobilised and contested in the debate on how to evaluate acupuncture. In this debate, the technology of the well-controlled clinical trial functioned as a rhetorical device: as a means for medical researchers to express their ideas about pain; how pain relief could be measured, and who could, or could not, be trusted to judge pain-relieving efficacy and to control its evaluation. In addition, some researchers—most notably John J. Bonica—made claims about who should have authority over the treatment of chronic pain. I will examine who called for the implementation of these technologies and why, and then describe how they were challenged, and by whom. I will also provide an explanation for why, in the end, these technologies were not successfully implemented. Remaining aware that measuring technologies are not just ideas, but concrete practices requiring funding, central authority, and mechanisms of coordination and agreement for their implementation, I point to reasons why these resources were lacking in this case. First, however, I will provide a chronology of the American acupuncture debate and explain why the evaluation of acupuncture became such an important issue.

7.1 Evaluating Acupuncture: What For?

The earliest American witnesses of acupuncture anaesthesia entered China through doors that began opening in the early 1970s. Strict restrictions on movement between the two countries had been imposed since the inception of the

PRC in 1949. Then in the second half of the 1960s, the Cultural Revolution (1966-1969) brought the trickle of travellers to a halt. In April of 1971, the Chinese government unexpectedly invited the American ping-pong team to compete on its soil. Seizing this opportunity, two American biologists, Ethan Singer and Arthur W. Galston, took advantage of a scientific visit to Vietnam to seek entry into China. Invited for a two-week tour, they were granted an interview with Premier Chou En-Lai and shown three universities, two factories, a commune, an opera, a ballet... and four operations performed under acupuncture anaesthesia.[662] *New York Times* journalists, allowed entry to the PRC to cover the first steps of a Sino-American détente, or "ping-pong diplomacy," were also among the first to peer upon spectacular scenes of patients eating fruit while tumours and organs were removed from their bodies.[663] American doctors were soon able to witness the phenomenon for themselves. Paul Dudley White, Victor Sidel, George Rosen and E. Grey Dimond, selected and invited by the All-China Medical Association for a tour of the Chinese healthcare system in the fall of 1971, were reportedly the first American doctors to have penetrated the Chinese border in twenty-five years.[664] These doctors were followed by others who came as members of the presidential party during Nixon's historic diplomatic visit to Peking in 1972.[665]

In 1971 and 1972, descriptions of operations performed under acupuncture anaesthesia were given wide circulation in the American news and medical press. Acupuncture anaesthesia was initially portrayed as a curious—if impressive—

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[662]Ethan Singer and Arthus W. Galston, "Education and Science in China," *Science* 175 (1972): 15-23; *U.S. Relations with the People's Republic of China*, CIS-NO: 72-S381-8, Committee on Foreign Relations. Senate, Hearing, June 24-25-28-29, July 20, 1971, 110-111, Galston testimony.
[663]Audrey Topping, "Chinese Use Acupuncture Anesthetic in Heart Surgery," *New York Times,* 24 May 1971, 10; James Reston, "Now, About My Operation in Peking," *New York Times,* 26 July 1971, 1, Reston had come to China to cover Kissinger's visit in Peking in the summer of 1971, and to write about various aspects of Chinese politics and society, when he was operated for acute appendicitis and received acupuncture for the relief of post-operative pain. He was later invited to witness acupuncture anaesthesia, which he reported in: Reston, "A View From Shanghai," *New York Times,* 22 August 1971, sec. E13.
[664] "Inside look at Chinese medicine" *American Medical News* (Oct 11, 1971)in Bonica Papers, MS C 118, Box 41, Folder 17, Darling Library, UCLA.
[665]William Tkach, author of "I Have Seen Acupuncture Work," *Today's Health* (1972): 50-6, and William Lukash, presenter featured in *Acupuncture observations in the People's Republic of China* [Videorecording] (Ritter Dental Co., 1973) were members of Nixon's party.

feature of a strange and fascinating society. Early reports were apparently met by the American medical community with disbelief and disinterest.[666]

By 1972, however, acupuncture had already begun drawing attention as a therapy meriting serious evaluation, especially for the relief of chronically painful conditions. For the next three years, the American media was filled with calls—from individual researchers and various biomedical institutions—for a rigorous and attentive evaluation of acupuncture's efficacy, suggestions on how to do so, and assurances that it would be done.

The institutional response to acupuncture was swift. In 1972, the National Institutes of Health (NIH) appointed an advisory Ad Hoc Committee on Acupuncture to look into the question. The committee advised that serious scientific research should be undertaken, provided some guidelines and priorities for further research and organised a conference on acupuncture research in 1973. In June of 1972, the American Society of Anesthesiologists issued a press release on acupuncture warning that "the safety of American medicine has been built on the scientific evaluation of each technique before it becomes a widely accepted concept in medical practice."[667] In September of 1972, the Food and Drug Administration (FDA) held a meeting to discuss the regulatory status of acupuncture instruments; legalising needles and stimulators as investigational devices.[668] Several state medical and dental boards, as well as individual physicians, issued position statements and testified that acupuncture should be

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[666] *U.S. Relations with the People's Republic of China*, CIS-NO: 72-S381-8, Committee on Foreign Relations. Senate, Hearing, June 24-25-28-29, July 20, 1971, 118, "Dr. Galston: "I have spoken with several people who have expressed to me skepticism, who have implied that the patients were hypnotized, that drugs were secretly administered..."
The Chairman: "You mean they seriously suggest that the Chinese put this show on just to impress you as a visitor..."
Dr Galston: Some people have suggested exactly that..."""
Tkach, "I Have Seen," 50-6 and E. Grey Dimond, "Acupuncture Anesthesia: Western Medicine and Chinese Traditional Medicine," *Journal of the American Medical Association* 218 (1971): 1558-63, confirm the existence of these doubts, which they claim their observations disproved.

[667] Committee on Science and Technology, United States-China Science Cooperation Hearings before the Subcommittee on Science, Research and Technology U. S. House of Representatives, May 7,8,10; June 22, 1979 (U. S. Government Printing Office Washington: 1979), 226: testimony of M. T. Jenkins.

[668] "Report on Acupuncture: NIH/FDA Meeting, September 22, 1972, Bonica Papers, MS C 118, Box 66, Folder 12, Darling Library, UCLA.

legalised only as an experimental procedure, to be practiced only within a coordinated research initiative. In February of 1974, the American Medical Association (AMA) appointed an Ad Hoc Committee on Acupuncture and issued a position statement calling for more and better studies of acupuncture.[669] The Committee on Scholarly Communication with the PRC, formed by the National Academies of Science as a result of Kissinger's negotiations in Peking, sponsored a delegation for the study of acupuncture anaesthesia in 1974.[670]

Why was the evaluation of acupuncture apparently taken so seriously? I suggest there were four reasons motivating the different actors who took part in this response: the pressure of popular demand for acupuncture; the opportunity to build Chinese-American relations through its study; the ambition to constitute a specialised field of pain research and treatment; and the fear that acupuncture would spread extensively beyond medical control.

Americans who saw in acupuncture the promise of relief for their chronic pain demanded information about, and access to, this new therapy. According to DeWitt Stetten Jr., director of the National Institute of General Medical Science (NIGMS): "The patient population with pain that did not respond to ordinary treatments, created pressure to do something, demanded that something be done on acupuncture. They made their pressure felt on the White House and the various medial societies..."[671] This "population" had learned about acupuncture in articles that appeared nearly daily in American newspapers.[672] Pressured by Congress, the NIH had appointed the Ad Hoc Committee on Acupuncture. Congressional

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[669] "AMA Grams," *Journal of the American Medical Association* 227, no. 12 (1974): 1393, during its January meeting the AMA board of trustees authorised the establishment of a 5 member committee to study and report on the state of acupuncture.
"Ad Hoc Committee on Acupuncture, the Brown Palace Hotel, Denver, September 12-13, 1974," Bonica Papers, MS C 118, Box 66, Folder 22, Darling Library, UCLA.

[670] American Acupuncture Anesthesia Study Group (AAASG), *Acupuncture Anesthesia in the People's Republic of China a Trip Report of the American Acupuncture Anesthesia Study Group, Submitted to the Committee on Scholarly Communication With the People's Republic of China* (Washington: National Academy of Sciences, 1976).

[671] Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1974, House of Representatives, Part 4- NIH*, (US Government printing office, Washington DC: 1973), 794.

[672] Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1974, House of Representatives, Part 4- NIH*, (US Government printing office, Washington DC: 1973), 794.

representatives, apparently concerned to show their constituents that their interest in acupuncture was being taken seriously, continued to badger NIH officials: "All we want to know today," warned Congressman Flood during a hearing on the appropriation of funds to the NIH, "is that you are not fooling about this."[673] Congressman Magnuson's interrogation of Stetten was direct: "What are you doing about the acupuncture? Any research on it?... The American people want to know, and they want your people to give us advice, otherwise, somebody's going to get hurt in this process."[674] The tone of this exchange explains the rapidity of the NIH response, and their concern to get quick answers, but also, as we will see, a reluctance to fund large-scale and long-term research on acupuncture and the treatment of chronic pain.

The organisation of delegations to visit China and study acupuncture was a means of establishing scholarly exchange and communication between the U. S. and the PRC. Acupuncture was only one of several topics investigated by the delegations sponsored by the Committee on Scholarly Communications or organised by American medical societies, which also included pharmacology and public health. To obtain permission to enter the PRC, these groups had to be officially sponsored by an American and Chinese organisation. The American interest and Chinese pride in acupuncture seemed to make it a natural topic to include in these exchanges. As the chair of the Committee on Scholarly Exchange with the PRC—a committee created following Kissinger's negotiations in Peking—the study of acupuncture would allow American researchers to "interact with their Chinese colleagues at the research level. This is one of the few fields in which a kind of mutual study and learning is possible."[675]

But why did researchers want to participate in these exchanges? How do we explain that a researcher like John J. Bonica, who apparently had no prior

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[673]Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1974, House of Representatives, Part 4- NIH*, (US Government printing office, Washington DC: 1973), 794.
[674]Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1974, House of Representatives, Part 2,* (US Government printing office, Washington DC: 1973), 2045.
[675] Emil L. Smith to Bonica, January 24, 1974 , Bonica Papers, MS C 118, Box 67, Folder 5, Darling Library, UCLA.

interest in China or Chinese medicine, made repeated attempts to obtain permission to go witness acupuncture in the PRC before he was allowed to join a delegation sponsored by the Committee on Scholarly Communication?[676] Isabelle Baszanger has drawn connections between Bonica's desire to go to China and his ambitions to create a specialised field of pain research and treatment. She has suggested that the spotlight on acupuncture, and, by extension, on the problem of pain, gave "a useful impetus" to Bonica's project of creating "a world of pain."[677] I agree with Baszanger that it was no coincidence that Bonica, now memorialised as the "founding father" of the modern "pain movement," was actively involved in various initiatives on acupuncture. I will examine this involvement in more detail, and show how his efforts to promote a rigorous evaluation of acupuncture's pain-relieving efficacy were also meant to draw attention to the problem of pain, stimulate the development of pain research and claim special expertise for pain researchers.

Bonica's appointment as the chairman of the NIH Ad Hoc Committee yoked the study of acupuncture to the emerging field of pain research and treatment. This appointment indeed deserves some attention. Bonica was neither a specialist in Chinese medicine or surgical anaesthesia. Instead, his reputation was based on the development of regional anaesthetic techniques and the development of a new model for the treatment of chronic pain. Describing his strategy for creating the Committee, Stetten explained: "We get together a group of scientists, presumably people who know what controlled experiments are. At my suggestion, Dr. Marston invited Dr. John Bonica as chairman of this committee. He is the chairman of the Department of Anesthesiology at the U of WA in Seattle. He runs in addition a very large and successful research clinic for chronic pain."[678]

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[676]John Bonica, chairman of the NIH Ad Hoc Committee on Acupuncture had been trying to get into China since at least October of 1972, see: "Doctors Eye China Visit," *New Haven Register* (Oct 4 1972) in Bonica Papers, MS C 118, Box 67, Folder 9, Darling Library, UCLA.
[677] Isabelle Baszanger, *Inventing Pain Medicine From the Laboratory to the Clinic* (New Brunswick, N.J: Rutgers University Press, 1998), 63.
[678]Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1974, House of Representatives, Part 4- NIH*, (US Government printing office, Washington DC: 1973), 793-4.

Bonica's model of the multidisciplinary pain clinic was closely tied to his concept of chronic pain. In a 1953 manual on pain management, Bonica had begun to argue that persistent pain should be approached as a target of treatment, rather than as a symptom of an underlying condition.[679] Bonica explained that pain persisting beyond an initial period of treatment and healing (usually defined as six months) shifted from its original function as a symptom of injury or disease to become a pathology in itself. Pathological pain no longer served a useful purpose as a warning signal or protective mechanism. Chronic pain was a multi-dimensional condition affecting the "whole person," and always involving some psychological component. Its treatment thus required the combined expertise and knowledge of a multi-disciplinary team that included psychologists and psychiatrists, within a setting dedicated to this problem. While Bonica's manual helped articulate a conceptual model of chronic pain, in 1960 he was able to concretise a clinical model for its treatment when he became Director of the Department of Anesthesia at the University of Washington. From 1962, Bonica began to campaign for support from NIH and dedicated himself to the establishment of a "world of pain": the expansion of specialised chronic pain treatment facilities, the creation professional pain networks, the recognition of chronic pain as a serious national health problem, and increased funding for pain research.[680]

By 1972, it seems that Bonica finally began to get the attention of the NIH. The same year as he was appointed to the Ad Hoc Committee on Acupuncture, he was also invited to a symposium on trauma organised by the National Institute for General Medical Sciences (NIGMS), and was promised funding for a symposium on pain (this never came through).[681] It is difficult to know how, exactly, Bonica's achievements and ambitions motivated his appointment to the Ad Hoc Committee on Acupuncture. What is clear, however,

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[679] John J. Bonica, *The Management of Pain With Special Emphasis on the Use of Analgesic Block in Diagnosis, Prognosis, and Therapy* (Philadelphia: Lea & Febiger, 1953); Baszanger, *Inventing Pain Medicine*, 26-31

[680] Baszanger, *Inventing Pain Medicine,* 65

[681] Baszanger, *Inventing Pain Medicine*, 65.

is that he seized the occasions offered by this and other official positions in acupuncture initiatives to pursue his ambitions for the advancement of pain research and treatment as an effective and legitimate field within American medicine. He did this in several ways.

First, Bonica used the opportunity of public addresses and interviews to link the need for a proper evaluation of acupuncture to the importance of the problem of chronic pain. For example, in the testimony he prepared for hearings on acupuncture legislation in the State of Washington, Bonica introduced the issue of chronic pain as "a serious national health problem."[682] Offering a potential solution to such a serious problem, acupuncture deserved serious evaluation, particularly because chronic sufferers, desperate for relief, were vulnerable to the false promises of quacks and entrepreneurs. As Bonica affirmed during the discussion at an NIH conference on acupuncture research: "I really sense a great need for these studies. There is tremendous public interest in acupuncture, and many people in severe pain view it as their last hope. Is it truly helpful or is it a false hope... I sincerely hope we will mount serious and well-controlled trials..."[683] In addition, Bonica told a journalist: "American medicine is particularly eager to assess acupuncture because present methods cannot relieve the CP which many patients endure (...) Unfortunately there are some who are ready to exploit the public..."[684] Until its efficacy was determined, Bonica added in a radio interview, "unscrupulous practitioners may exploit this interest [in acupuncture] to the detriment of the American people."[685] Bonica also explained that a proper evaluation of acupuncture's efficacy would require more research on pain. In his summary statement on the NIGMS conference on acupuncture research, Bonica explained that "one of the serious problems in evaluating

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[682] Acupuncture notes, ms., n.d., in "Acupuncture Legislation, Washington State, 1974," Bonica Papers, MS C 118, Box 66, Folder 20, Darling Library, UCLA.
[683] Howard P. Jenerick, ed. *Proceedings of the NIH Acupuncture Research Conference* (Washington, D. C., US Department of Health, Education and Welfare: 1973), 135.
[684] Al Dieffenbach, "Doctors mull long-range research on acupuncture," *Seattle Times* (Feb 11, 1973) in Bonica Papers, MS C 118, Box 41, Folder 27, Darling Library, UCLA.
[685] "Transcript" in Bowen I. Hosford to John J. Bonica, Apr 25, 1973, Bonica Papers, MS C 118, Box 21, Folder 21, Darling Library, UCLA: Enclosed is also a transcript of an interview between "NIH announcer" and members of the AD Hoc Committee including Bonica, that was to be broadcast "on radio stations around the country."

acupuncture for the relief of pain is that pain itself is a complex phenomenon which also should receive greater scientific research efforts,"[686] a point he also emphasised in interviews and conference discussions.[687] Throughout his discussion of acupuncture, Bonica emphasised the need for specialised expertise on pain. He presented his own credentials in acupuncture matters with respect to his longstanding interest in pain and its treatment.[688]

Bonica also used his influence in the selection of the membership for committees and conferences to begin developing professional networks between researchers with an interest in pain. When Bonica sought to obtain permission for the entry into China for a small delegation to study of acupuncture, the travel companions he chose were similarly invested in the field of pain research and in the psychological dimensions of pain. The names he suggested were Patrick Wall and Ronald Melzack, authors of the groundbreaking gate-theory of pain and respected authorities on the psychology and neurophysiology of pain; Donald Katz, an anaesthesiologist with experience using hypnosis as a surgical anaesthetic and in neurophysiologic research; and Wilbert Fordyce, a colleague in the pain clinic and expert in the behavioural aspects of chronic pain. These researchers were also members—probably because they were invited by Bonica—of the NIH Ad Hoc Committee on Acupuncture. However, other members of the Committee who were more interested in acupuncture itself or in physiology were not suggested.

Bonica failed to obtain permission for this particular group to visit China. In 1974, however, a specialised group was formed to investigate acupuncture in China: the American Acupuncture Anesthesia Study Group (AAASG). Bonica was asked for his opinion on the selection of members for this group, and his

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[686] John J. Bonica, "Summary Statement on NIGMS Acupuncture Research Conference, Feb 28-Mar 1 1973," in *Proceedings of the NIH Acupuncture Research Conference*, ed. Jenerick, (Washington, D. C., US Department of Health, Education and Welfare: 1973), vi.

[687] ""Transcript" in Bowen I. Hosford to John J. Bonica, Apr 25, 1973, Bonica Papers, MS C 118, Box 21, Folder 21, Darling Library, UCLA: In which Bonica is reported to have said: "I think some of the painful conditions are so complex and it will take several years before we get any useful information that will suggest trends."

[688] "Acupuncture" notes, ms., n.d., in "Acupuncture Legislation, Washington State, 1974," Bonica Papers, MS C 118, Box 66, Folder 20, Darling Library, UCLA

recommendations can, similarly, be read as a wish-list for an all-star pain research cast. They included the neurophysiologists Edward Perl, who had done "outstanding work on pain;" Fred Kerr, who was involved in the neurosurgical relief of pain; Ronald Dubner, who would become a leading authority on dental pain; Vernon Mountcastle, "one of the most respected neurophysiologists with an interest in pain in the U. S.;" and Arthur Taub, the director of a pain diagnostic and treatment unit at Yale University School of Medicine. Bonica also recommended psychologist Richard Chapman, "one of the brightest young psychologists doing serious research on pain," and anaesthesiologist Ephraim Siker, who had experience in analgesic evaluation.[689] Many of Bonica's suggestions (Chapman, Taub, Dubner, Kerr, Siker) were followed, while a few others who would become respected authorities on pain research were added to the list: Jerome Modell and Kenneth Casey. The priority of the selection committee was clearly to prioritise expertise in pain research and treatment.[690]

In addition to those who were recruited by Bonica and by others to participate in the evaluation of acupuncture, some researchers also took advantage of opportunities acupuncture offered to conduct their own trials on chronic pain sufferers. A handful of research grants were awarded by the NIH for acupuncture studies, and there were also opportunities to present results of trials on chronic pain in conferences, workshops, and in the news media. Some who had already established pain treatment facilities set up trials of acupuncture, thus showing that these could become useful research settings.

For various reasons, then, the membership of the NIH Ad Hoc Committee on Acupuncture, the AAASG, and of the NIGMS conference on acupuncture research brought together individuals who would, just a few years later, be at the forefront of the institutionalisation of pain research. They would participate in

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[689] Bonica to Denise Emery, February 1, 1974, Delegation to the People's Republic of China: Acupuncture Membership, General, 1974, National Academy of Sciences Archives (NASA):
[690] Emil L. Smith to Bonica, January 24, 1974, Bonica Papers, MS C 118, Box 67, Folder 5, Darling Library, UCLA: According to Smith, chairman of the Committee for Scholarly Communication, the objectives of the AAASG would be to "interact with their Chinese colleagues at the research level. This is one of the few fields in which a kind of mutual study and learning is possible. It is, therefore, important that the delegation include members who have an experimental background on the mechanism of pain."

meetings (World Congresses on Pain), professional organisations (the International Association of the Study of Pain and the American Pain Society),and publications (*Pain*) and contribute to the expansion of pain clinics. Acupuncture evaluation may have offered some of these researchers their first opportunity to work together as pain experts.

Whether or not these pain researchers hoped, one day, to offer their patients the option of acupuncture therapy, they unanimously insisted on the need for the most rigorous evaluation process. If acupuncture was to be made widely accessible, it should be approved by pain experts using the trusted methods of scientific medicine. If it was proved to be ineffective, chronic pain patients should be protected from false—and potentially expensive—hopes, while increased funding should be channelled into pain research in search of more effective solutions. Indeed, Bonica, often joined by colleagues, did not seem to lose any opportunity—from the reports of his visit to China to his recommendations to the NIH and his comments in various meetings—to call for more, well-controlled trials of acupuncture.[691]

The repeated appeals for caution, patience, and further research formulated by pain experts were also mobilised by the spokespersons of professional organisations. For them, calling for a rigorous evaluation of acupuncture was a means of lobbying for legislation that would keep acupuncture under the control of state medical and dental licensing boards. Some of these boards, as well as national organisations, were active in pushing for legislation that would define acupuncture as the practice of medicine, and thus stipulate that acupuncture could only be practiced by, or under the supervision of, a qualified medical professional. The FDA had taken early legislative measures in defining acupuncture apparatus—needles and stimulators—as experimental devices. This implied that any device sold in interstate commerce would have to display a

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[691] For example: "An eye on the needle: a suggested approach to acupuncture,"1972, Bonica Papers, MS C 118, Box 66, Folder 6, Darling Library, UCLA; "Addition by Dr Bonica to Acupuncture Legislation," Draft 4, December 6, 1973, Bonica Papers, MS C 118, Box 66, Folder 11, Darling Library, UCLA; Bonica, "Acupuncture Anesthesia in the People's Republic of China: Implications for American Medicine," *Journal of the American Medical Association* 229, no. 10 (1974): 1317-25.

warning label, and that the following conditions would be imposed on its use: The device should be used by qualified physicians or dentists, under a research protocol which had been reviewed by a peer committee on human research, and on subjects from who informed consent had been obtained.[692] Until states regulated acupuncture practice, however, the FDA ruling would have little impact. Many state societies and boards issued position statements, while some took more direct action either by proposing their own restrictive bills on the practice of acupuncture, or by speaking against regulations they saw as overly lax.

Though some attempts were made to support the restriction of acupuncture on the grounds of safety, the more common and seemingly more persuasive strategy was to argue that its efficacy had not been proven. The statements of researchers like Bonica were used to support this position, while Bonica was also invited to testify in hearings on proposed acupuncture bills in the State of Washington. Some of those who lobbied for this legislative position may have honestly hoped that acupuncture would one day be proved effective and legalised as a legitimate therapy. However, the immediate consequence of such legislation was to enable tight control over its practice. The definition of acupuncture as an experimental therapy usually entailed specifying the conditions under which it could be practiced: in appropriate research settings, such as teaching hospitals; under the supervision of a medical research; and according to an appropriate research protocol.[693]

For the boards, such legislation meant restricting the practice of acupuncture, but for researchers it was also an opportunity to spell out the conditions for a proper evaluation of its efficacy. Bonica, for example, proposed, in an addition to a State of Washington bill, that, "in order to obtain meaningful

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[692] "Report on acupuncture- NIH/FDA Meeting, September 22, 1972," Bonica Papers, MS C 118, Box 66, Folder 12, Darling Library, UCLA.

[693] See, for example: James N. Benedict, Albert J. Pirro and Joseph R. Pisani, "Acupuncture: The Practice of Medicine?" *Albany Legal Review* 38 (1974): 633-90: On July 26 1972, the New York State Board of Medicine issued this statement: "Acupuncture is not an accepted medical procedure in the S of NY at the present time. The NYSBM regards acupuncture involving the human body as an *experimental* procedure." The board recognizes the need for further research and "believes [acupuncture research] should be performed only in medical centers and teaching hospitals which have committees on human research. This would provide the necessary peer review of protocols and appropriate monitoring of acupuncture studies."

data, it will be necessary to have a uniform protocol, a special record keeping system, and a central agency for the collection and analysis of the data." In addition, controlled clinical trials "required adhering to strict scientific principles of:" controlling, blinding, and statistical analysis.[694] Thus, Bonica saw legislation as a potential means of guiding and standardising the evaluation of acupuncture. Another anaesthesiologist, M. T. Jenkins, had testified before the Texas Board of Medical Examiners in 1974 that acupuncture should be defined an investigational procedure, to be investigated only by full-fledged members of an Acupuncture Study Group, organised by each medical school in the state, under a uniform protocol. The investigator would then submit her data for review, collation, and statistical analysis to a "special record-keeping system."[695]

Because of various political and professional reasons—Congress' responsibility to the American people, Chinese-American relations, the constitution of a "world of pain" and the protection of the practice of medicine—the evaluation of acupuncture became a serious and visible issue, leading to the creation of forums in which researchers could discuss how best to judge the effects of acupuncture on pain.

No precise answers, however, came out of these discussions. Researchers usually recommended, rather vaguely, that well-controlled experiments should be conducted. When they did specify further *how* these should be conducted, they mentioned placebos, blinding, and statistical analysis, sometimes adding large-scale coordination, uniform protocols, as well as centralised data collection and analysis. These conditions are hardly surprising if we consider that controlled clinical trials had become standard for the evaluation of drug therapies, and, in the early 1970s, were being extended to new therapies such as surgery.

If researchers were rather vague about the designs of experiments on acupuncture, they were quite specific about the reasons why experimental

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[694] "Addition by Dr Bonica to Acupuncture Legislation, Draft 4, December 6, 1973," Bonica Papers, MS C 118, Box 66, Folder 11, Darling Library, UCLA.

[695] Committee on Science and Technology, *United States-China Science Cooperation Hearings before the Subcommittee on Science, Research and Technology U. S. House of Representatives, May 7,8,10; June 22, 1979* (U. S. Government Printing Office Washington: 1979), 223-6: testimony of M. T. Jenkins.

technologies were necessary, and about what experimental technologies should "do" with respect to eliminating certain kinds of error and bias. These researchers were, at the same time, insisting that it was impossible to determine whether acupuncture was effective in relieving pain *without* these technologies. This necessity, as we will see, did not go undisputed. Many argued that it was possible, on the basis of individual, unmediated experiences of pain and suffering, to tell whether acupuncture worked.

As Harry Marks has shown, trust in the methodological devices of controlled clinical experimentation—placebo-controlling, randomization, blinding, and statistical analysis—was promoted by emphasising the untrustworthiness of certain figures—pharmaceutical firms, nurses, and even investigators. Therapeutic reformers insisted that these "figures of mistrust" were threats to the objective evaluation of therapeutic efficacy, on which both good medical practice and professional autonomy rested.[696] In the context of the acupuncture debate, however, these "controls" were given new meanings. They were endowed, for example, with the power to protect therapeutic evaluation against the influence of new figures of mistrust: "brainwashed" Chinese patients and doctors; desperate and gullible chronic pain sufferers; charismatic and enterprising acupuncturists; the pervasive influence of the media; and, to a certain extent, medical researchers themselves, especially if they were not experts on pain.

Medical researchers appealed to the need for large-scale, well-controlled clinical experimentation by describing the ways in which these figures—by their interests or susceptibilities—could distort the evaluation of acupuncture. In pronouncing these figures biased, researchers were, at the same time, rejecting certain forms of evidence (anecdotal, testimonial, individual, observational) in favour of others (experimental, clinical, controlled, statistical). Researchers justified mistrust in these figures with reference to the particular nature of pain, especially of chronic pain. Hence, the necessary conditions for objective

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[696] Harry Marks, "Trust and Mistrust in the Marketplace: Statistics and Clinical Research, 1945-1960," *History of Science* 38 (2000): 343-55.

evaluation described by members of acupuncture committees and study groups. The need for certain criteria to control the evaluation of acupuncture was closely connected to a portrayal of pain, and its relief, as an experience that was open to certain kinds of social, mental, physical and even political influences. These criteria had implications in deciding who should, and more importantly, who should not, have authority in judging the pain-relieving efficacy of acupuncture.

7.2 Pain, Objectivity, and Figures of Mistrust in the Acupuncture Debate

In calling for more controlled evaluations of acupuncture's analgesic efficacy, researchers were criticizing the reliability of various different sources and forms of evidence. They rejected the "anecdotal evidence" provided by non-experts—journalists and physicians—in the first reports from China. When "experts" such as Bonica and the members of the AAASG reported on their own trips to China, they complained that the political and cultural conditions under which acupuncture was practiced in China made both Chinese patients and doctors unreliable indicators of acupuncture's efficacy. In addition, their lack of access to information and results from properly controlled trials made it impossible for them to make their own judgments. Bonica declared that only well-controlled trials on American patients could resolve the question of acupuncture's efficacy. Even Americans, however, had been exposed to potentially biasing influences: media hype; the wide publicity given to "testimonial" evidence; and the promises of acupuncturists. In addition, given the nature of chronic pain, their suffering was, in some measure, amplified by the psychological consequences of this condition, and thus susceptible to the psychological modulation of strong suggestive influences.[697] The subjectivity and openness of pain made individual experience an unreliable indicator of analgesic efficacy. The evaluation of this experience had to be subjected to the control of certain psychological influences,

[697] It is important to note that the "psychological" maladjustement that accompanied chronic pain was believed by many pain experts to be a result, rather than a cause, of persistent bodily suffering.

and compared to multiple other experiences. Finally, as we have seen, knowledge about pain was said to be helpful in the evaluation of acupuncture's efficacy. Researchers with knowledge about pain were encouraged to get involved in the evaluation of acupuncture, while calls were made for more funding for research on pain.

The first doctors and journalists who wrote about acupuncture in China emphasised the visual nature of their encounter with the technique and its wondrous effects. These observers circulated evocative photographs and verbal images of patients' behaviours under acupuncture anaesthesia, describing what they had seen, and sometimes heard, inside the operating room. "I'm writing only about what I actually saw," cautioned Walter Tkach in *Today's Health,* and concluded that "seeing is believing."[698] "I Have Seen the Past and It Works," was the title of Samuel Rosen's article in the *New York Times.*[699] Galston and Singer assured they had "personally witnessed the incisions made and operations performed."[700] They also saw the insertion of a few slim needles into strategic points on patients' bodies, and that patients were chattering, sipping juice, eating fruit, and clutching little red books[701] while one "lay on his stomach with a vast gaping hole in his back, through which you could see the gasping of the remaining lung..."[702] Another patient's surgeon "picked up [her] beating heart and held it in his hand."[703] They saw enough to ascertain that patients were conscious, that they had really been operated on and that they appeared to be pain-free. But, their critics soon began to ask, could this apparent efficacy be an illusion? Might

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[698]For example, W. Tkach, "I Have Seen," 50-6; *U.S. Relations with the Poeple's Republic of China*, CIS-NO: 72-S381-8, Committee on Foreign Relations. Senate, Hearing, June 24-25-28-29, July 20, 1971, 119: testimony of Galston.

[699] Samuel Rosen, "I Have Seen the Past and It Works," *New York Time* (1 November, 1971), 41, writes: "What I have to tell is, I know, not going to be believed. I know this because a Chinese surgeon, chief at the major metropolitan hospital in Canton, told me that he had not believed it himself -- until he had seen it many times over."

[700]E. Singer and A. W. Galston, "Education and Science in China," 15-23.

[701]Collections of quotations by Chairman Mao that were distributed during the Cultural Revolution. To observers, they could be taken as signs of the pervasiveness of political ideology in Chinese society.

[702]Reston, "A View," 3.

[703] Topping, "Chinese Use Acupuncture,"10. Early reports written by physicians (Dimond, Rosen, Tkach), contained similar graphic imagery.

residual pain, or non-acupunctural sources of relief, have remained invisible to these observers?

Many invisible forces, American critics suggested, could have created a false appearance of efficacy. At first, it seems, some suspected a trick had been played on naïve American visitors. Perhaps patients had been secretly medicated or hypnotised before the American observers arrived.[704] But soon, such intentional and straightforward deception on the part of the Chinese was no longer mentioned, at least not in published statements.[705] Yet, critics continued to suspect that a more subtle form of deception was taking place. A recurring hypothesis was that acupuncture worked, or appeared to work, in China because of the social, political, and cultural conditions peculiar to the Chinese people. This hypothesis took several variations. Some critics supposed that a cultural tendency towards stoicism made Chinese patients less likely to express pain than American patients under the same conditions, because they were less sensitive, ready to accept more discomfort without suffering, or simply inexpressive.[706] Indeed, references to Chinese stoicism, or to culturally-conditioned responses to pain, were not infrequent in discussions about the evaluation of acupuncture.[707] Many others

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[704]See n.9 above.

[705]In private exchanges, some doctors were less diplomatic.D. Effler to J. J. Bonica, August 19, 1974 Bonica Papers, MS C 118, Box 66, Folder 10, Darling Library, UCLA: Effler writes: I will try to speak with utmost frankness...Medicine in China is playing a desperate game of catch-up, and the majority of papers that I read were obvious propaganda by the central government and scarcely qualified as scientific in nature..."

[706] Harold R. Isaacs, *Scratches on our Minds: American Images of China and India* (New York: The John Day Company, 1958). Isaacs' sociological investigation into American attitudes towards the Chinese traces a long history of Western representations of Chinese and Asian people as being stoical. Isaacs tells this history as an oscillation of positive and negative representations back to the eighteenth century. The positive attributes given to the Chinese were those of Pearl Buck's novels: peaceful, hardworking, endurant of hardships. But there was also another set of images of cruelty and barbarism, of the Chinese as "binders of women's feet" and "torturers of a thousand cuts" whose indifference to pain was "nerveless." Thus, there was a "vast lore" attributing to the Chinese an "absence of nerves", which was either a tribute to their strength and endurance or a way of dehumanizing the Chinese on the basis of their apparent disregard for pain, be it their own suffering or that inflicted on others

[707]"Doctors Fear Quackery: Acupuncture Hit," *Washington Star*, (Aug 4, 1974) A-1 and A-8 in Delegation to the PRC: Acupuncture, General, 1973-76, NASA, reports on the trip of an AMA delegation to China. The delegation statement reportedly specified "In the opinion of three Western trained surgeons and one anesthesiologist who are members of our delegation, it seems unlikely that acupuncture analgesia will be widely accepted by Western patients, who tend to have relatively lower pain thresholds than do their stoic Oriental brethren."

supposed that a potent ideological indoctrination, apparent in the pervasive presence of Maoist ideology, produced a belief in acupuncture that was so strong it provided Chinese patients with the relief of surgical pain.[708] They noted that Chinese patients were often found clutching "little red books"—collections of the citations of Chairman Mao—and exclaiming "long live Chairman Mao!" on the operation table.[709] At least one author likened this phenomenon to hypnotism, and pointed out that hypnotism could effectively block pain, while others saw it as a type of placebo effect.[710] It was also suggested, more rarely, that patients did not say they had felt some pain for political reasons. [711]

Kenneth McCracken, "After China Visit- Dr Kerr Reports Acupuncture Benefit" *Rochester Post-Bulletin* Thurday June 6 1974, Delegation to the PRC: Acupuncture, General, 1973-76, NASA: "They are indeed stoical people," Dr Kerr said, "but when there is no response from even involuntary muscle to severe trauma to tissue as in major surgery, it's hard to believe something isn't happening"; "Americans Study Acupuncture as Anesthetic" *Gainesville Sun* (June 28, 1974) in Delegation to the PRC: Acupuncture, General, 1973-76, NASA: Walter Modell is quoted to have said: "An important question which remains to be answered is whether or not the Chinese, in general, have greater tolerance to pain than Americans"; "Acupuncture (Editorial)," *Journal of the American Medical Association* 223, no. 1 (1973): 77-8, "Of course, there are skeptics who have sent letters to THE JOURNAL and to *American Medical News* denouncing acupuncture aneshtesia as a hoax. Most of them believe that Chinese stoicism plus hypnosis are what makes acupuncture anesthesia seem to work."

[708] "Acupuncture: When You Need a Little Needling," *New York Times,* (2 May 1971), sec. Science/Medicine, p. E7: "While Western physicians are apt to be highly skeptical of acupuncture, many believe that it should not b dismissed out of hand. Some have suggested that acupuncture works because of its patients' faith in it, that it is a form of psychosomatic medicine..."

[709] "Inside Look at Chinese Medicine" *American Medical News* (Oct 11 1971) in Bonica Papers, MS C 118, Box 41, Folder 17, Darling Library, UCLA, in which Dimond reported that "after the patient's neck was sewn up, he sat up with a smile. He picked up the little red book of Chairman Mao's thoughts, waved it, and said: 'Long Live Chairman Mao. Welcome to our American friends..."; Reston, "A View": "One troubling diversion in all this for a visitor is that the impressive objective evidence of the medical uses of acupuncture is always mixed up here with subjective psychiatric and even ideological explanations. For example, all the patients we saw on the operating table were clutching their little red books of Chairman Mao Tse-Tung's philosophic and moral teachings. And the doctors and surgeons, after participating in the operations, were explaining that the success of this system depended importantly on trust between doctor and patient and on a common faith in 'Mao Tse-Tung thought.'" Rosen, "I Have Seen," 41, described patients leaving "the opearting room or the dentist's chair alert... and waving his 'Quotations from Chairman Mao Tse-tung.'"

[710] W. S. Kroger, "Acupuncture Analgesia: It Explanation by Conditioning Theory, Autogenic Training, and Hypnosis," *American Journal of Psychiatry* 130, no. 855-60 (1973); Kroger, "Hypnotism and Acupuncture," *Journal of the American Medical Association* 220 (1972): 1012-13; Kroger, "The Scientific Rationale for Acupunctural Analgesia," *Psychosomatics* 14 (1973): 191-4.

[711] Bud Gordon, "Why Some People Feel Pain More than Others," *National Enquirer* (1974) Bonica Papers, MS C 118, Box 66, Folder 31, Darling Library, UCLA: reports Bonica's words as: "Some showed facial expressions, shivering, groaning, and other signs that they felt pain.... But

These various hypotheses were made plausible was a view of pain as a bodily experience that was particularly open to social influences, which acted on experience through the medium of a malleable mind. The influences did not necessarily produce bodily changes, yet they were strong enough to make normally excruciating surgical procedures relatively painless. Such explanations were also made more believable by an American view of Chinese communist society, and of a Chinese cultural essence, which together produced a relationship between body, mind, and society that was radically different from the American one. The Chinese were portrayed as being particularly susceptible to the influence of collective ideology but relatively indifferent to individual bodily experience.

Similar ideas informed the complaints of visiting "experts"—such as Bonica and the members of the AAASG—in their reports on the conditions under which they observed acupuncture in China. In their reports and publications, these experts, for example, commented on the political origins of acupuncture anaesthesia, a practice born from a directive issued by Mao himself and revived in the fervour of the Cultural Revolution.[712] A journalist reported, for example, that "Bonica [had] suggested that Chairman Mao-Tse Tung's strong advocacy of the technique and the political and religion popularity of Mao might have something to do with acupuncture's success."[713] The AAASG report even suggested that "the desire of some patients to withhold evidence of pain cannot be entirely

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they denied they felt pain because they'd been indoctrinated to believe that acupuncture is painless."

[712] Bonica, "Acupuncture Anesthesia in the People's Republic of China: Implications for American Medicine." *Journal of the American Medical Association* 229, no. 10 (1974): 1318, tells how acupuncture was reintroduced during the Cultural Revolution as part of Mao's movement to fully integrate Western and traditional medicine. Almost all the articles he had read on acupuncture "emphasized the fact that Acupuncture Anaesthesia is the product of Chairman Mao"s genius and policies. After reading all this, I agree with Geiger... that in China, Acupuncture Anaesthesia is more than a medical and scientific subject, but one which involves national pride, health policy and political implications."

AAASG, *Acupuncture Anesthesia*, 23, "The role of peer acceptance and national pride on the part of the patients were believed to be important factors by some of our colleagues..."

[713] N. Rosenberg, "Acupuncture in Doctor's Grip," *Milwaukee Journal* (Feb 12 1973) [Box 67, Folder 9] John J. Bonica Papers, Manuscript collection number 118, Louise M. Darling Biomedical Library, History & Special Collections Division, University of California, Los Angeles.

discounted."[714] The influence of factors such as national pride on the apparent efficacy of acupuncture was impossible to detect through observation, or even physiological measurements, under uncontrolled conditions.

These experts also complained that they had access to insufficient information about acupuncture to make informed judgments about the causes of its efficacy. They were disappointed not to be able to find out more about how patients were selected and prepared to undergo surgery under acupuncture anaesthesia. "To the best of the study group's knowledge," the members of the AAASG wrote tentatively, "coercion is not employed," while they were only "relatively certain" that "no effort to condition the patient or to induce hypnosis or posthypnotic suggestion was made."[715] They also suspected that doctors and scientists did not, for political reasons, reveal the whole truth about the extent to which acupuncture was practiced, nor their real opinions about its efficacy. Bonica, for example, made some calculations indicating that acupuncture was used much less frequently as a surgical anaesthetic than had been reported by the authorities.

Finally, Bonica and the members of the AAASG repeatedly pointed out the absence of controlled clinical trials in China.[716] Bonica also noted that the Chinese disregard for placebo-controlling and statistical analysis extended more broadly than the evaluation of acupuncture. For example, Bonica underlined passages about the deficiencies in Chinese mechanisms of therapeutic evaluation in his fellow delegates' report of their trip. In Bonica's copy of Myron E.

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[714]AAASG, *Acupuncture Anesthesia*, 24. In their original manuscript of the report, the members of the AAASG had expressed their doubts slightly differently: "With regard to motivational factors, the possibility could be considered that the desire of the patients to present this authentic Chinese discovery in the best possible light, while at the same time preserving 'face', might be sufficiently compelling factors for them to endure pain stoically. This possibility cannot be discarded lightly, especially when we review our records and find notes to the effect that the patient moaned softly, winced, grimaced, clenched fists or squirmed, yet when questioned directly after the operation, stated that he/she felt no pain. This should not be construed as deception but related to cultural and social factors which are important to the Chinese people." See, "NEJM: m/s review "acupuncture hypalgesia...PRC", 1975, Bonica Papers, MS C 118, Box 66, Folder 35, Darling Library, UCLA
[715]AAASG, *Acupuncture Anesthesia*, 23.
[716]Bonica, "Therapeutic Acupuncture in the People's Republic of China: Implications for American Medicine," *Journal of the American Medical Association* 228, no. 12 (1974): 1544-51: "Unfortunately, no clinical trials have been done, and all of the reports claiming high percentage of efficacy of acupuncture in relieving pain are anecdotal."AAASG, *Acupuncture Anesthesia.*

Wegman's report on the use of traditional medicines, the following was underlined: "...unfortunately, the reaction against critical statistical analysis that took place during the cultural revolution is impeding proper evaluation of such experiments..." In his copy of Lynchcott's report on child development, Bonica had apparently drawn attention to the following passage: "I am left with the impression that the Chinese have put a very low priority, if any, in modern systems of collecting biostatistical information for the country at large. I would hasten to add, however, that I am sure they have the expertise and organization to accomplish this." These markings, which correlate with Bonica's own comments,[717] seem to suggest that he shared his colleague's impression that the Chinese did not share Americans' understanding and practices of objectivity.[718]

Various factors thus conspired, according to American medical researchers, to make it impossible to untangle material causes of pain relief from psycho-cultural and psycho-social ones on the basis of observations in China. The view of Chinese society reflected in both general news coverage and discussions of acupuncture anaesthesia was one in which individual actions were indissociable from the thick ideological web that synchronised the actions of each of its parts.[719] The nature of pain made patients' experiences of relief under acupuncture open to the influence of this collusion, regardless of whether it was considered to be the result of voluntary consensual complicity, unconscious

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[717]See, for example: "John J. Bonica notes on trip to China, 1973," Bonica Papers, MS C 118, Box 66, Folder 52, Darling Library, UCLA, Bonica wrote: "Question about clinical trials- they indicated that they do use controls and study groups but the answer was very vague and it is doubtful that this is the case." Al Dieffenbach, "Doctors mull," Bonica Papers, MS C 118, Box 41, Folder 27, Darling Library, UCLA: "Unfortunately, [Bonica] said, the ban on publishing in China has left no recent body of scientific reports that might have served as a basis for the expected American study of the procedure."

[718] Leo Orleans, "China's Statistics: the System and Its Problems," *Public Data Use* 1 (1973): 17-?, in Bonica Papers, MS C 118, Box 66, Folder 49, Darling Library, UCLA Bonica also kept in his papers this article about the different cultural and political attitude of the Chinese towards exact quantification and the establishment of a viable statistical system.

[719]This image is present in W. S. Kroger, "The Scientific Rationale," 191-4, in which the author compares Chinese society to Skinnerian operant conditioning: "There are the cultural factors of a more than 3000 year belief system which in a regimented society, readily bring about compliant behavior without *overt* cooperation being necessary. Such compliance will be obtained in Western society only if strong reward inducements are offered such as those outline in Mao's *new thought directives*. Also, too, Mao is regarded as a deity and his words are accepted as gospel.... Mao's sociopolitical exhortations which are part of Chinese life induce an exquisite receptivity to mold the thinking of the masses."

brainwashing or totalitarian control. Chinese physicians were similarly caught within the tight social net, unable to think for themselves or to reveal that they did.[720]

Given widespread remarks on the limited utility of Chinese statements, and of observations made in China, it is not surprising that the Ad Hoc Committee on Acupuncture recommended that acupuncture should be experimented in priority on American subjects.[721] American chronic pain sufferers, however, had their own biases requiring careful control through the use of experimental devices. In addition, the nature of their pain sometimes created difficulties for the implementation of trials and the interpretation of results.

Americans had easy access to descriptions of, and opinions about acupuncture, which were widely diffused through the news media. Some researchers complained that it was impossible to recruit naïve subjects, and even that "publicity given by press, radio and television to acupuncture in the United States has preconditioned our population to the same extent as people in China who believe in acupuncture because of their cultural heritage."[722] Furthermore,

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[720]Boyce Rensberger, "U. S. Doctors Are Skeptical of Acupuncture in Treatment of Purely Physical Diseases," *New York Times* (7 October 1971), 42, reports the opinion of several American physicians (Henry Beecher,Vernon Mountcastle, Janet Travell) who express doubts about whether Chinese physicians are telling the truth about acupuncture. Reston, "Now, About," 1: "While I have no way of knowing the validity of the reports, the faith even of the professionally qualified doctors at the AI hospital is impressive. Maoism itself has obviously become an infectious disease, even among many of the well-educated urban citizens who had a hard time during the Cultural Revolution."

[721]"An eye on the needle: a suggested approach to acupuncture,"1972, Bonica Papers, MS C 118, Box 66, Folder 6, Darling Library, UCLA.

[722]While testing of acupuncture in American patients had initially been seen as a way to control for psychological factors, namely Chinese ideological influences, this view did not hold for long. The following view was quite common: E. R. Kepes, M. Chen, M. Schapira, "A critical evaluation of acupuncture in the treatment of chronic pain," *Unpublished,* n.d. in Bonica Papers, MS C 118, Box 66, Folder 46, Darling Library, UCLA: "In order to do research on new therapeutic modalities, one has to separate the bias on the part of the therapist and the bias of the paitnet from the actual effect of a new method. This is difficult to do with acupuncture. The publicity given by press, radio and television to acupuncture in the United States has preconditioned our population to the same extent as people in China who believe in acupuncture because of their cultural heritage. We have met very few patients who did not believe in its efficacy before their first experience with acupuncture treatment."
See also: RJ Beebe, TW Andersen, HM Perkins, VA Hospital and U of Florida Coll of Med "Preliminary findings with acupuncture treatment of pain" in *Proceedings of the NIH*, 1-2:

these overly enthusiastic subjects were "failures from conventional medicine," and were thus more likely to be sensitive to the "novelty effect" of acupuncture, which might heighten the placebo effect.[723] The definition of acupuncture as experimental made it difficult, according to ethical guidelines, to recruit subjects who were not refractory to other therapies.[724] Some researchers argued this factor required additional controls, while others argued that any significant relief obtained with acupuncture, even if it fell "within the placebo range," might be a sign of success in a patient population that had found no relief elsewhere.[725]

Researchers also expressed the need to control for acupuncturists' expertise and charisma. Experienced acupuncturists could not be "blinded" to the use of "placebo acupuncture," and some preferred to use trained technicians to deliver test therapies, despite the difficulties this involved and the arguments made for the importance of skill in acupuncture efficacy.[726] Others suspected acupuncturists of infectious enthusiasm, even if they spoke no English, which made it difficult to protect double or even single blind controls.[727] Some investigators even conducted trials comparing the efficacy of Asian-looking and Caucasian-looking acupuncturists.[728]

The vulnerability of American sufferers to various influences—media publicity, acupuncturists' beliefs, and the hardships of their own suffering—

Despite results indicating that acupuncture was effective for the relief of chronic pain, the authors called for caution in part because: "the patients' expectations were high due to the widespread publicity."

[723] *Proceedings of the NIH*, 130-131.

[724] F. F. Foldes in *Proceedings of the NIH*, 135.

[725] *Proceedings of the NIH*, 130: "Dr Foldes: The overall results...seem t be surprisingly close to the placebo effect... Dr Katz: I would object to an overall comparison of percent effectiveness...Many of these patients in the acupuncture trials are failures from conventional therapy. Dr Moore: I would underscore that particular point. Our patients come to the pain clinic with intractable pain."

[726] Gerald L. Looney, "Response of Osteoarthritis to Acupuncture" in *Proceedings of the NIH*, 18.

[727] Gene M. Smith, "Acupuncture and experimentally-induced ischemic pain," in *Proceedings of the NIH*, 64: "Even though the acupuncturist does not speak English this does not preclude the possibility of bias, since there are many sources of non-verbal signalling available." The acupuncturist appeared "more expectant, more interested" in true than placebo trials, thus making it difficult to "blind" the study.

[728] E. R. Kepes, M. Chen and M. Schapira, "A critical evaluation of acupuncture in the treatment of chronic pain," *Unpublished,* n.d. in Bonica Papers, MS C 118, Box 66, Folder 46, Darling Library, UCLA, one of the questions this study aimed to answer was: "Do the therapeutic results differ when acupuncture is administered by Chinese or Caucasian physicians?"

justified the need for well-controlled experiments, and for additional research on the possible presence of phenomena such as hypnotism or responses to the appearance of the acupuncturist. In addition, this image undermined the value of the "testimonial" evidence these sufferers presented in letters and legislative hearings. It was not so much that patients themselves could not be trusted to be truthful about their experience, but that individual experience could not be trusted. To become collectively valid, this experience had to be multiplied, compiled, and compared under the supervision of a medical researcher, preferably one who knew something about pain. Large numbers were imperative. The purpose of amassing and comparing data was not just to increase the level of certainty in evaluating acupuncture; it was a necessary means of controlling for the power, and malleability, of the mind in experiencing pain.

7.3 Experience *versus* Experimentation: the Challenge to Medical Control

While the popular response to acupuncture had reportedly stimulated a demand, through Congress and the NIH, for the serious medical evaluation of acupuncture, many grew impatient with, disappointed in or suspicious of the medical establishment's response. The authority of medical researchers' ideal evaluation through controlled experimentation was challenged. Sufferers, who had experienced the pain-relieving effects of acupuncture and who believed in the validity of their individual experience, testified in the media and in legislative hearings that acupuncture worked. They also criticised the medical "takeover" of acupuncture's evaluation, pointing out that the insistence on the exclusivity of controlled experimentation was just another means of keeping acupuncture within medical control. These claims were diffused widely with the help of journalists and editors, who published individual testimonies in newspapers; pro-acupuncture lobbyists, who arranged demonstrations and testimonies at legislative hearings, and also wrote to newspapers; and occasionally family members, who wrote letters about their kin's' experiences with acupuncture.

The mass media was an active player in the acupuncture debate. In a month-long survey of how frequently acupuncture was making the news, Bonica had reportedly collected 1,100 newspaper clippings.[729] A computerised search of the archives of the *New York Times* and the *Wall Street Journal* using ProQuest confirms that articles, letters, and even short poems on acupuncture were printed by the hundreds between 1971 and 1974. The television and radio listings in these newspapers also indicate an omnipresent discussion of the acupuncture topic.

Information about acupuncture, medical experts' opinions, research efforts, and regulatory debates was easily accessible to many Americans. The media also provided a channel for expressions of mistrust towards medical interests in, and methods of, evaluating acupuncture, and to praise for its efficacy. Testimonies of dramatic recoveries from disabling pain were reported in journalists' coverage of legislative hearings, interview citations, and in the letters sent by sufferers and their families to the editor of various newspapers.[730] Newspaper articles also provided evidence of popular enthusiasm by relating the success of the acupuncture business.[731] Indeed, "extravagant media attention" would, in the 1980s, be accused of having distorted the real extent of acupuncture's limited practice in the early 1970s.[732] The media also related doctors' fears of uncontrolled spread of quackery and their concerns about the unethical exploitation of sufferers' desperation and gullibility, and traced the

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[729] Al Dieffenbach, "Doctors mull long-range research on acupuncture," *Seattle Times* (Feb 11, 1973) in Box 41, Folder 27; Bonica to John Andes, December 11, 1972 in Bonica Papers, MS C 118, Box 66, Folder 31, Darling Library, UCLA, in which Bonica reports that he had collected "hundreds of clippings" on acupuncture.

[730] For example: Boyce Rensberger, "Acupuncture 'Wonderful' to a Patient," *New York Times,* (May 31, 1972), 43; S. M. Gottlieb, "An Acupuncture Testimonial (letter to the editor)," *New York Times,* (July 6 1974), 16; M. Schumach, "Council Delves Into Acupuncture," *New York Times* (December 8, 1972), 49; "Hearings Started on Acupuncture: Witnesses Tell State Panel the Therapy Cured them after Doctors Failed," *New York Times* (March 13, 1973), 82.

[731] "Acupuncture Patients Fear Ban," *New York Times* (Sep 5 1972), 1; "Acupuncture Clinic Here Closes Down,"*New York Times* (July 20, 1972), 29.

[732] Ginger McRae, "A Critical Overview of U. S. Acupuncture Regulation," *Journal of Health Politics, Policy and Law* 7, no. 1 (1982): 163: "If acupuncture seemed rampant and ubiquitous, it was because of extravagant media attention and the tendency of the medical profession to exaggerate the dimensions of acupuncture practice."

evolution of medical research efforts.[733] Articles, many bearing titles apparently in competition for the most imaginative "needling" puns[734], were not only records, but also vehicles, of rising tensions between a pro-acupuncture lobby and the medical establishment. Articles such as "Doctors Fear Quackery: Acupuncture Hit,"[735] "A Doc Deflates Acupuncture,"[736] "MD's Criticism of Acupuncture Disputed"[737] may even have exaggerated the animosity between lay acupuncture supporters and their medical critics.

Nevertheless, these sources—along with letters sent directly to researchers and records of legislative and congressional hearings—do seem to reflect a real tension between elite researchers and those who believed there was sufficient evidence of efficacy to make acupuncture therapy immediately and widely accessible. Researchers' appeal to the authority of experimentation—in which individual subjectivity was interrogated under controlled conditions and then submerged in large numbers of data—was opposed by sufferers' appeal to the authority of individual experience—in which their own bodily knowledge was more valuable that the knowledge of experts. Medical institutional responses to acupuncture were also accused of bearing the biases of the medical profession, whose professional interests and biomedical close-mindedness would preclude a fair evaluation.

Legislative hearings were the main sites of competition between experience and experimentation as sources of evidence for acupuncture's efficacy. Testimonies and demonstrations were admitted to legislative hearings on

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[733]For example, see: "Medical Panel Asks Acupuncture Study to Evaluate Merits," *New York Times* (March 3, 1973), 8; Lawrence K. Altman, "Interest in Acupuncture Rises in U. S.: Doctors Test Old Chinese Technique," *New York Times* (June 12, 1972), 1; Boyce Rensberger, "U.S. to Evaluate Acupuncture for Safety and Effectiveness," *New York Times,* (July 28, 1972), 1.
[734]"Acupuncture: When You Need a Little Needling," *New York Times* (May 2, 1971), E7; R. R. Leger, "It May Needle Some, But Acupuncture Is On the Way in Nevada," *Wall Street Journal* (April 17, 1973), 1; "Acupuncture Craze Gets Sharp Poke in Medical Report," *Wall Street Journal* (June 18 1974), 21; "A Needle for the Doctors," *Wall Street Journal (*Apr 11 1972), 22; C. K. Dorland, "Pointed Replies (letter to the editor*),"* *Wall Street Journal,* 14.
[735]Bonica papers, in *Washington Star*, Sunday Aug 4, 1974, A-1 and A-8 in NASA: Delegation to the PRC: Acupuncture, General, 1973-76.
[736] E. Nelson, *Daily News,* (June 18, 1974) in Bonica Papers, MS C 118, Box 66, Folder 31, Darling Library, UCLA.
[737] A. H. Kao, *Honolulu Advertiser*, (Mar 22 1974) in Bonica Papers, MS C 118, Box 66, Folder 31, Darling Library, UCLA.

acupuncture bills, as well as some congressional hearings. In a New Jersey hearing, a man was said to have testified while carrying his discarded crutches and back brace.[738] A Nevada bill was approved after weeks of demonstrations on hundreds of patients by a Hong Kong acupuncturist, including twenty legislators, according to one account.[739] One demonstration was even conducted during the hearings of a congressional sub-committee on Chinese-American Cooperation, using as its experimental subject, its chairman Mr. Brown.[740]

Legislators seem to have been more willing than medical authorities to consider types of evidence other than clinical trial results.[741] Medical researchers, as we have seen, testified that the only possible source of valid information about acupuncture's efficacy was a coordinated set of well-controlled experiments. They urged that "testimonial" and "anecdotal" evidence should be purged from the acupuncture policymaking process. Yet, individuals—often backed by more influential acupuncture enthusiasts—persisted in presenting their own bodies as evidence for the efficacy of acupuncture. According to some observers, it worked. In a 1982 article presenting a "Critical Overview of U. S. Acupuncture Regulation," Ginger Mac Rae argued that "laws favoring the nonphysician acupuncturist generally are the fruit of activity by persistent and vocal state lobbies comprising nonphysician acupuncturists, patients who have benefited

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[738] W. H. Waggoner, "Hearings Started on Acupuncture: Witnesses Tell State Panel the Therapy Cured Them After Doctors Failed," *New York Times* (March 13, 1973), 82; W. H. Waggoner, "Chief Medical Officer, in Shift, Backs Acupuncture by Nondoctors," *New York Times* (May 8, 1974), 96.

[739] Associated Press, "Acupuncture Law in Nevada," *New York Times* (April 21, 1973), 25; Leger, "It May Needle Some, 1; UPI, "Nevada legislature Backs an Acupuncture System," *New York Times* (April 10, 1973), 40; UPI, "Acupuncture in Nevada," *New York Times* (March 11, 1973), 28. This last one says that Hong Kong acupuncturist was invited because officials from the Senate Committee on Health and Welfare said they had requested a demonstration of acupuncture. Another article ("It May Needle Some") stated that the demonstrations were organised by New York attorney and real estate developer Arthur Steinberg who had been leading an active campaign to publicize and legalize the practice of acupuncture.
See also: W. M. Edwards, "Acupuncture in Nevada," *The Western Journal of Medicine* 120 (1974): 507-512; F. M. Anderson, "Instant Acupuncture for Nevada (editorial)," *Western Journal of Medicine* 120 (1974): 487-8.

[740] Committee on Science and Technology, *United States-China Science Cooperation Hearings before the Subcommittee on Science, Research and Technology U. S. House of Representatives, May 7,8,10; June 22, 1979* (U. S. Government Printing Office, Washington: 1979), 196-204.

[741] McRae, "A Critical Overview,"163-96. My impression from reading congressional hearings and news coverage of state hearings tends to confirm this.

from acupuncture treatment and Western physicians well-disposed toward acupuncture." Mac Rae added, in a note, that "patients excel at presenting the kind of 'testimonial and anecdotal' evidence on behalf of acupuncture that is persistently belittled by the medical profession, but is persuasive to legislators."[742]

Sufferers and acupuncture enthusiasts also contested researchers' denial of the validity of observational evidence. A widely diffused Associated Press summary of an article by Bonica on his observations in China that was published in JAMA drew so many angry responses and "poison pen" letters that Bonica felt compelled to issue a statement to correct the misconception that he was condemning acupuncture.[743] In these letters, individuals told Bonica about the relief that they, or their close ones, had obtained from acupuncture; disputed the medical establishment's right to control access to a useful therapy; and questioned doctors' competence in evaluating its efficacy. Anthony Kao, the president of Acupuncture Services, Inc., accused Bonica of racism in implying that the Chinese felt less pain than did Americans, and of protecting the interests of the medical profession.[744] Franz Z. Warren, President of the National Acupuncture Research Society, wrote Bonica an open letter claiming that his overly cautious tone would harm the cause of giving acupuncture a fair trial through well-funded and rigorous research programs. He added that this caution seemed unfounded: "The most confusing aspects of your report were the disparities in what you observed and how you translated your own eye-witnessed findings. If you really did see what you saw, how then interpret it as you did with your statement that acupuncture is no better or worse than hypnosis or placebo..." Warren added: "When has there ever been any incontrovertible evidence in the areas of pain, hypnosis, anaesthesia theory and so on?"[745]

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[742]McRae, "A Critical Overview,"196.
[743] Associated Press, "Report Claims Acupuncture Overrated," in [Box 66, Folder 29]; Bonica to New York Post, Washington Post, Los Angeles Times, June 28, 1974 [Box 67, Folder 9] John J. Bonica Papers, Manuscript collection number 118, Louise M. Darling Biomedical Library, History & Special Collections Division, University of California, Los Angeles.
[744] A. H. Kao, "MD's Criticism of Acupuncture Disputed" *Honolulu Advertiser* (March 22, 1974) in Bonica Papers, MS C 118, Box 66, Folder 31, Darling Library, UCLA.
[745] "An open letter to Dr John Bonica, chairman of the Advisory Committee on Acupuncture for the ASA- his remarks in a recent AMA article contained damaging implications to our society and

As we can see from these responses, there was a readiness not only to assert the value of non-experimental evidence, but also to dispute the legitimacy of medical control over the evaluation of acupuncture. In a hearing on the appropriation of funds to the NIH, in which Congressional representatives were demanding that the NIH take the evaluation of acupuncture seriously, Flood expressed particular concern about an article published in the *Washington Star* entitled "Acupuncture Deserves Wider Trial." "The question," the author declared, "is, how it is to be tested and by whom? And when tested, how valid will be the results? Doctors, of course, say that only they should be involved... " The author concluded convinced that "the biases of the medical profession and its often arrogant pretended omniscience might well botch the job": "As long as M. D.'s hold all the cards, no one will really know what is valuable about acupuncture therapy and what should be rejected."[746] Flood and Magnuson were apparently worried that if NIH didn't act fast on acupuncture, the American public would not only lack reliable evidence about acupuncture, but would likely to lose its trust in medical researchers' willingness and capacity to produce such evidence.[747]

Such expressions of distrust were often directed towards the AMA, and may even have provoked reluctance among AMA representatives to get officially involved in the acupuncture debate, preferring to leave licensing issues to the state boards.[748] By late 1972, the AMA had not yet adopted an official position on acupuncture. When the AMA finally did appoint an Ad Hoc Committee on

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to acupuncture in general, and in the interest of fairness require a printed rebuttal, " in Frank Z. Warren to Bonica, August 8, 1974 in Bonica Papers, MS C 118, Box 66, Folder 29, Darling Library, UCLA. See also: Cecile E. MacTaggart, "Letter to the Editor 1-- No Title," *New York Times* (August 18, 1972), 30, "Is it likely that Dr. Walter Tkach, personal physician to the President of the U. S. would write an article in the July issue of *Today's Health* that was 'purely anectodal'? After all, he saw operations performed in China with his own eyes..."

[746]J. Randal, "Acupuncture deserves Wider Trial," *Washington Star* (June 8, 1972) cited in Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1974, House of Representatives, Part 4- NIH*, (US Government printing office, Washington DC: 1973), 791-2.

[747]Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1974, House of Representatives, Part 4- NIH*, (US Government printing office, Washington DC: 1973), 793.

[748]"Report on Acupuncture: NIH/FDA Meeting, September 22, 1972 in Bonica Papers, MS C 118, Box 66, Folder 12, Darling Library, UCLA.

Acupuncture,[749] in February of 1974, Jenkins expressed concern that the purpose of the committee "could be misinterpreted as protection of the financial interests of physicians," and noted that there was "a danger than an AMA statement would be construed to be self serving."[750] Jenkins may have feared a similar response to the statement issued by the American Society of Anesthesiologists, which called for acupuncture to be submitted to the scientific standards of American medicine. When this statement had been summarised in the *Chicago Sun Times*, one reader promptly responded: "it will be interesting to see how long it takes the medical establishment to either 1) stifle [acupuncture] or 2) monopolise it for the further enrichment of physicians. The first indications may come at San Francisco this month when the AMA stages its annual re-enactment of King Canute's effort to stop the rising tide."[751] Jenkins may also have received letters similar to the one C. R. Wilson sent to Bonica: "Whether the AMA approves of acupuncture or not is unimportant. In fact, if the AMA disapproves, there must be some validity to the treatment. As long as the AMA can keep a closed mind on something which has 5,000 years of operation behind it, it will encourage the unscrupulous, the unqualified and the incompetents to engage in surreptitious practice of acupuncture..."[752]

Historians and sociologists of medicine have described the 1960s and 1970s as a time of "crisis of legitimacy" in American medicine, when distrust of medical authority was expressed in critiques of medical professional power, the emergence of "patient movements," and a growing involvement of the mass media in health issues.[753] This criticism was associated with broader social

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[749] "AMA Grams," *Journal of the American Medical Association* 227, no. 12 (Mar 25) (1974): 1393. During its January meeting the AMA board of trustees authorised the establishment of a 5 member committee to study and report on the state of acupuncture.

[750] "Ad Hoc Committee on Acupuncture, the Brown Palace Hotel, Denver, September 12-13, 1974," in Bonica Papers, MS C 118, Box 66, Folder 22, Darling Library, UCLA.

[751] "Medical group hits acupuncture" *Chicago Sun Times*, (June 3, 1972); "Acupuncture and the doctors" *Chicago Sun Times* (June 7 1972) in Bonica Papers, MS C 118, Box 66, Folder 31, Darling Library, UCLA.

[752] C. R. Wilson to Bonica, June 18, 1974, in Bonica Papers, MS C 118, Box 66, Folder 12, Darling Library, UCLA.

[753] Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, 1982), 379-393; Mike Saks, "Medicine and the Counter Culture,"in *Companion to Medicine in the 20th Century.* eds Roger Cooter, and John Pickstone, (London: Routledge, 2000) 113-23.

movements such as feminism and civil rights activism. Two of its aims were to reclaim lay control over health issues, and to validate sick persons' own knowledge and experience of their bodies.

The debate about acupuncture thus reflects a more widespread willingness to contest biomedical authority, expertise, and exclusivity. While researchers claimed that the subjective and individual nature of pain made it especially difficult to evaluate the effects of acupuncture, for sufferers these effects were obviously and immediately perceptible. They didn't see the need for expensive and complex technologies to tell them what they already knew. In addition, many who had experienced several therapeutic failures had good reason to lose faith in biomedicine and its actors and to be enthusiastic about a therapy that seemed to procure relief.

Pain experts such as Bonica agreed that medicine had failed to satisfactorily address problems of chronic pain, and that pain should be defined as what patients themselves experienced and defined as pain. Indeed, these were crucial cornerstones in his campaign to create a specialised "pain medicine." However, for Bonica, the best collective long-term solution was to improve medicine rather than reject it, by developing, through increased research funding, properly controlled therapeutic experimentation, and by establishing new types of clinical models, a new kind of medicine within medicine.

The failure to concretise Bonica's envisioned program of acupuncture evaluation was probably not directly due to "popular" or "pro-acupuncture" opposition. The AMA may have been reluctant to take a strong public stance on the need for an evaluation program controlled by medical researchers, but this doesn't explain the lack of actual research. The actions of pro-acupuncture lobbies in the legislative arena may have been a more concrete obstacle to the realisation of researchers' ambitions, in that it impeded rulings that may have provided authority and financial support for the establishment of coordinated research programs at the state level.[754] I suggest, however, that acupuncture research faced

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[754] Jackson W. Riddle, "Report of the New York State Commission on Acupuncture," *American Journal of Chinese Medicine* 2 (1974), 317-318: the draft of proposed legislation included, section

more immediate and practical obstacles arising from tensions and gaps within the medical establishment, which prevented the allocation of sufficient resources for the implementation of valid and large-scale clinical trials.

7.4 A Divided House of Pain: the Challenge from Within

I have argued in previous chapters that the successful implementation of pain-measuring technologies usually depends on the use of certain social and material resources that include money, labour, authority, agreement, and the standardisation of definitions and techniques. Was there a lack of crucial resources for the implementation of technologies to evaluate the pain-relieving effects of acupuncture?

NIH officials asserted, in their response to the inquiries of Congressional representatives, that "the deficit here, sir seems not to be the lack of funds, but the lack of fundable research applications."[755] Researchers' own discussions of the difficulties they, and others, faced in designing and implementing acupuncture trials lead me to ask two questions: Why were good experimental designs for the evaluation of acupuncture so hard to come by? Was there really enough money?

In 1974, the NIH had reportedly given out five grants for acupuncture research amounting to "in excess" of $200,000.[756] Perhaps, this could have produced some relatively persuasive evidence about therapeutic efficacy in a field in which researchers had already agreed on what was a good experiment. This was *not* the case either for acupuncture or chronic pain. While some researchers complained that NIH funding for acupuncture evaluation was inadequate and

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6: "the sum of two hundred fifty thousand dollars ($250,000), or so much thereof as may be necessary is hereby appropriated to the state boards for medicine and dentistry…in carrying out the provisions of this act." These provisions included the creation of state-wide mechanism of data collection and evaluation to determine the efficacy of acupuncture.

[755] Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1975, House of Representatives, Part 7*, (US Government printing office, Washington DC: 1974), 2045.

[756]Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1975, House of Representatives, Part 7*, (US Government printing office, Washington DC: 1974), 2045.

inaccessible,[757] I would argue that this was especially the case if we consider the amount of work that was needed to achieve some agreement on methodological details or, as some others suggested, to establish some centralised mechanism with sufficient authority to impose a standard protocol on a large scale. To this suggestion, Bonica had replied: "The committee considered this and did recommend a national collaborative effort wherein the study designs are similar and coordinated. I'm not sure this can be brought off in the face of current funding."[758]

The novelty of acupuncture, and important ways in which it was different from the drug therapies for which clinical study designs had been refined, made it difficult to agree on a standard design. Acupuncture itself was difficult to standardise as a therapy because, according to many, it required skill, individualisation, and patient participation (for locating appropriate spots). There were difficult questions to answer, on which researchers disagreed, about how to deliver placebo acupuncture and how to achieve double-blind conditions. This was illustrated in a "controversial correspondence about double-blind studies of acupuncture" that took place over nearly two years through letters to the editor of JAMA.[759] A parallel exchange of letters debated the importance of skill and of

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[757]Department of Labor and Health, Education and Welfare, *Hearings before a Subcommittee of the Committee on Appropriations FY 1975, House of Representatives, Part 7*, (US Government printing office, Washington DC: 1974), 256, Statement of Dr Herman Platt: "I don't know the precise figure for acupuncture research funding was with respect of the recent NIH involvement. However, it must be a mere pittance because I have known a number of these researchers and there is no way that they can conduct their work in a meaningful way."; Committee on Science and Technology, *United States-China Science Cooperation Hearings before the Subcommittee on Science, Research and Technology U. S. House of Representatives, May 7,8,10; June 22, 1979* (U. S. Government Printing Office Washington: 1979), 205: Testimony of R. Coan, "Ertel: Does anyone finance your study, such as the NIH or the National Science Foundation, or any other groups at this time? Coan: I'm glad you asked that question. No is the naswer. I was told by Dr. Harold Jenerick, who has something to do with the Institute of General Medical Science of NIH that my chances of getting funds to do this would be 1 in 5,000; that is, to do acupuncture research."

[758] Bonica in *Proceedings of the NIH*, 137.

[759] B. D. Adler, "Acupuncture (Letter to the Editor)," *Journal of the American Medical Association* 222, no. 7 (1972): 833, called for well-controlled double-blind studies of acupuncture: in half of the patients, the needles would be placed in their proper position, but the other half would have needles placed in the approximate location, but a short distance away from the approved site(...) By this method, the role of suggestion can be easily determined."
L. C. Mark, "Acupuncture and Suggestion (Letter to the Editor)," *Journal of the American Medical Association* 223, no. 8 (1973): 922, replied to Adler that placebo-acupuncture was

exact needle placement in obtaining acupuncture effects.[760] Similar issues came up at the NIGMS Conference on Acupuncture Research in 1973.[761]

While disagreements about acupuncture certainly posed an obstacle to the rapid and cheap implementation of a successful program of evaluation, my focus in this section will be on the issue of pain-measurement. I will suggest that the novelty and instability both of chronic pain as an object, and of the professional networks concerned with its study, can explain why such a program was never realised.

We've already seen that discussions about the evaluation of acupuncture were used as an opportunity to draw attention to the problem of chronic pain.

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impossible since the correct placement of the needles required the collaboration of the subject in identifying distinctive sensations. Nevertheless, "despite the inapplicability of the simplistic double-blind approach, meticulous scrutiny by careful investigators should provide the basis for a reasoned appraisal."
E. Y. M. Chein and A. K. Schapiro, "Evaluation of Acupuncture (Letter to the Editor)," *Journal of the American Medical Association* 224, no. 11 (1973): 1533-4, suggested that electrically stimulated needles could be made to elicit such sensations regardless of the correct placement of the needles.

[759] L. C. Mark, "Double-Blind Studies of Acupuncture," *Journal of the American Medical Association* 225, no. 12 (1973): 1532, replied to Chein and Schapiro that needles first had to be placed, before they could be stimulated, and this first step could not "be counterfeited."
Chein and Shapiro, "A Mini-Symposium on Acupuncture (Letter to the editor," *Journal of the American Medical Association* 227 (1974): 1122, continued to insist that controlling for the "powerful placebo effect" of acupuncture was essential, and proposed some new solutions. Perhaps two true acupuncture points could be chosen, but one would be irrelevant to the condition being treated and thus serve as the placebo. To make this double-blind, however, naive acupuncture technicians would have to be briefly trained, and only naive subjects recruited. Such a design, if replicated and repeated in different conditions and by different investigators, could establish a "powerful case... for the efficacy of acupuncture."

[760] R. Macintosh, "Acupuncture (Letter to the Editor)," *Journal of the American Medical Association* 226, no. 11 (1973): 1360, "A few weeks ago I expressed the opinion that in acupuncture for surgery, success depends on the suggestibility of the patient and not on the skill of the acupuncturist in placing the needles accurately. My belief is strengthened by the contradictions in the accounts of thyroidectomy by recent visitors to China." Macintosh is referring to variations in needle placement in different accounts of acupuncture surgeries.
T. O. Cheng, " A Mini-Symposium on Acupuncture (letter to the editor)," *Journal of the American Medical Association* 227 (1974): 1122, in response to Macintosh, Cheng maintained that exact point does not have to be the same on each patient, because different points are interconnected. Argues that it is necessary to understand the principles on which the points are based to practice acupuncture effectively. "Success fo acupunctural anaesthesia indeed depends on the skill of the acupuncturists in accurate placement of the needles, and not on the suggestibility of the patient."
John R. Tack., "A Mini-Symposium on Acupuncture (letter to the editor)," *Journal of the American Medical Association* 227 (1974), also in response to MacIntosh: "Dr MacIntosh seems to imply that the effect is due to suggestion because it can be achieved by placing the needles in a wide variety of points. Erythromycin can be given topically, orally, or intravenously. Shall we attribute its effect to suggestion also?"

[761] For example: Looney in *Proceedings of the NIH*, 13-18.

Indeed, Bonica's position as chair of the NIH Ad Hoc Committee was probably instrumental in defining acupuncture as a therapy for chronic pain, and thus also defining chronic pain as a specific type of condition. First, Bonica and his committee's recommendations prioritised the evaluation of acupuncture's effects on pain rather than other conditions said to be responsive to the therapy, such as deafness and paralysis. In addition, they subdivided research priorities into two categories: anaesthesia, and chronic pain. Various types of conditions, such as migraine, cancer pain and various forms of arthritis, were newly assembled under a common label. There had been little previous discussion, however, about how what these conditions had in common, and little or no research specifically funded as primarily investigating chronic pain.[762]

Researchers from different areas—psychiatry, anaesthesiology, rheumatology, etc.—who may not have been accustomed to working together were brought to work on this poorly defined common object. As we've also seen, the constitution of committees and conferences was used as a means of creating new professional networks, but these newly formed networks had had little chance to stabilise. Researchers had had little time and few organised opportunities to discuss how they might define or measure chronic pain, or to identify or create a mechanism to standardise measurement practices.

It is true that previous initiatives had succeeded in effectively implementing clinical trials to determine analgesic efficacy. As we saw in the previous chapter, however, the trials sponsored by the Committee on Drug Addiction (CDAN) and the Veterans' Administration (VA) required long-term funding and close coordination in order to produce consistent and reliable results. Another cooperative clinical trial project, which was suggested as a model for acupuncture evaluation, was that developed by the Arthritis Foundation and the American Rheumatism Association. In 1958, the ARA had created a Committee on Diagnostic and Therapeutic Criteria and a Cooperating Clinics Committee (CCC) to resolve problems of precision and quality in arthritis drug evaluation. From 1959, the CCC began conducting multi-site controlled trials of arthritis

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[762] Baszanger, *Inventing Pain Medicine*, 65.

treatments, and was able to demonstrate that previous uncontrolled had led to false impressions of therapeutic efficacy for some drugs, such as indomethacin. The use of multiple sites enabled the selection of sufficiently large and uniform samples of subjects and long-term, careful observation of each subject that were necessary to overcome the variability of rheumatic diseases and perform the "detailed objective assessments required" made such conditions indispensable. Each study was performed under the same protocol, and data was analysed centrally.[763] Like the CDAN and VA trials, the CCC trials had required central coordination and concerted work on a specific target of measurement in order to produce valid results. No large institution was interested in overseeing, funding, and coordinating a long-term evaluation of acupuncture.

Bonica's and others' appeals for more research on pain can be seen as a way of saying that the current state of knowledge and agreement about chronic pain was not sufficient to resolve the question of acupuncture's efficacy. Of course, Bonica also had his own agenda in calling for more funding for pain research. Yet his position seems to be confirmed by the lack of agreement about what chronic pain was, and how it could be measured, that was clearly evident in the proceedings of the NIGMS Acupuncture Research Conference. In 1973, most of the researchers who had an interest in acupuncture got together to present their findings and discuss further research. These researchers came from various disciplines and research areas, bringing different ideas about pain-measurement from sensory psychology, rehabilitation medicine, arthritis research, analgesic assessment, anaesthesiology and psychophysics to bear on the evaluation of acupuncture. Almost every trial they reported used a different method for measuring improvements in pain due to acupuncture. In the discussion of each presentation, the various methods used to rate pain and its relief were frequently criticised. In addition, it was not clear, given the particularly tenacious nature of subjects' pain, how much relief should be considered significant. Given, also, the

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[763]Department of Labor, Health and Welfare, *Hearings of the Senate Subcommittee on Heath of the Committee on Labor and Public Welfare on the National Arthritis Act*, (U. S. Government Printing Office, Washington D. C.: 1974), 535, annex: "A History of the Cooperating Clinics Committee.

susceptibility of pain to placebo effects, what was a rate of effectiveness "within the placebo range" supposed to mean?[764]

Four different types of pain-measuring instruments were used in the acupuncture trials described at the conference, or suggested in discussion: 1) Physiological tools that measured autonomic somatic responses to strong stimulation such as skin temperature, skin resistance, blood pressure, evoked potentials; 2) psychophysical tools that measured sensory thresholds, such as the dolorimeter; 3) self-reporting rating tools that presented a series of quantifiable options that subjects could choose to rate their impression of suffering or relief; and 4) behaviour-rating tools used by observers or subjects to record the frequency of certain types of "pain behaviours" such as time spent lying down, complaining, or limping.

Each of these tools was associated with different models of pain and of objectivity. Physiological instruments could give very precise quantitative measurements and offered a high level of mechanical objectivity, since they did not require the participation of either the subject or the observer to produce numbers. Their disadvantage, however, is that they did not actually measure pain; what they measured was a bodily response to stimulation and its perception. The discussions reported in the conference proceedings show that meeting participants disagreed about what these indicators meant, in part because of a lack of knowledge about pain. For example, the use of evoked potential responses to measure the outcome of acupuncture in one study was questioned as to how it was "a real measurement of pain," to which the investigator responded that they "didn't say it is a measure of pain relief."[765]

Psychophysical instruments also offered a fairly high level of quantitative precision, and partial mechanical objectivity, since they required subjects to make sensory judgments. We have already seen, in Chapter 5, that the use of

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[764] *Proceedings of the NIH*, 130: " Dr Foldes: The overall results...seemed to be surprisingly close to the placebo effect... Dr Katz: I would object to an overall comparison of percent effectiveness...Many of these patients in the acupuncture trials are failures from conventional therapy. Dr Moore: I would underscore that particular point. Our patients come to the pain clinic with intractable pain."

[765] *Proceedings of the NIH*, 34.

instruments like the dolorimeter for analgesic testing was criticised by Beecher. The use of such instruments for the evaluation of acupuncture was similarly criticised: they focused on the sensory aspects of pain, only one, and not necessarily the most important, of various components, including the emotional and cognitive, which made up the unitary experience of pain. In the NIGMS conference, for example, Chapman remarked: "in recent years, it has become evident that pain should not be studied as a simple sensory experience."[766] The use of such instruments to measure the effectiveness of a therapy for chronic pain—such as acupuncture—was especially difficult to justify for those, like Bonica and many others, who believed that chronic pain was even more psychologically complex than acute pain.

Self-reporting rating instruments, as we have seen in Chapter 6, became standard in the analgesic clinical trials sponsored by the Committee on Drug Addiction and Narcotics during the 1950s and 1960s, and continued to be widely used. They offered little quantitative precision and the numbers they produced were, at the individual level, arbitrary and subjective. When used in a large group of patients, under controlled conditions, they were thought to give reliable and consistent results. They had the advantage of measuring what subjects *felt*, which was, according to some pain researchers, the only legitimate way of measuring pain. For those accustomed to physiological and sensory research, such subjective measures may have seemed absurd. In conference discussions, however, the most vocal critic of such measures for acupuncture evaluation did not oppose them to physiological or sensory instruments, but instead advocated that pain should be measured as a form of behaviour.

The behaviour-rating instruments that were proposed offered no mechanical objectivity, in that they were usually forms or diaries filled out either by observers or patients. They were nevertheless considered by some to be more objective than the self-reporting rating scales because they measured what subjects *did* rather than what they *felt*. More importantly, however, they were justified by a different definition of chronic pain and of therapeutic improvement.

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[766] Richard C. Chapman in *Proceedings of the NIH*, 52.

This conception of chronic pain as a set of learned behaviours was developed by Wilbert Fordyce, a rehabilitation specialist at the University of Washington, where he was also a member of Bonica's multidisciplinary pain clinic as well as of the NIH Ad Hoc Committee on Acupuncture. In the late 60s, Fordyce had developed an innovative and controversial approach to the treatment of pain based on the principles of behaviour-modification. In its most radical formulation, this approach held that chronic pain could be defined as a set of pain behaviours, without reference to physiological disorders. These could be modified by reinforcing activity and discouraging expressions of suffering: limping, sitting, complaining, etc. The outcome of therapy could only be evaluated by measuring changes in observed or self-reported behaviours, not by what a patient might "really" be feeling "inside," nor by somatic changes.[767]

During the NIH conference on acupuncture research, Fordyce often questioned the validity of presenters' self-reported rating scales. For instance, he advised Kepes: "Your criteria are still constrained by verbal reports, since the patients tell you how they feel. I urge that you consider adding some relatively simple behavioral measures of what, in fact, they can do. For example, have them keep diary forms showing the distribution of their time spent reclining, sitting, standing, and walking across a 24-hour period. This is a very valuable tool in evaluating the effects of various modes of therapy."[768] Fordyce similarly prodded his colleague, Richard Chapman, who had used a combination of self-reporting pain scales and psychological tests: "I did not hear you mention behavioral measures of pain. Are you contemplating using any of these?"[769] Chapman, however, urged his colleagues to use multiple measures of pain, and to use available statistical consulting services in order to succeed in "look[ing] at pain in a global fashion" and evaluate it "more meaningfully."[770]

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[767] *Oral History Interview with Wilbert E. Fordyce*, 10 July 1993 (Ms. Coll. no. 127.1), John C. Liebeskind History of Pain Collection, History & Special Collections Division, Louise M. Darling Biomedical Library, University of California, Los Angeles.
[768] *Proceedings of the NIH*, 10.
[769] *Proceedings of the NIH*, 53.
[770] *Proceedings of the NIH*, 137.

There were two sources of tension among the conference participants that seemed to make it difficult to agree on valid measures of pain-relieving efficacy. The first was a lack of common reference points between researchers who had not previously met as "pain researchers." There was also another division between those who had already begun defining themselves as pain experts, but who either believed that chronic pain should be defined and measured according to what patients experienced (the "pain experientalists") and those who asserted that chronic pain was a behavioural issue that should be treated and measured as such. The tension surrounding the acupuncture question foreshadowed the further institutionalisation of this division in pain clinics offering two different types of treatment approaches. A detailed description of these approaches and their implications for the conceptualisation of pain and of suffering persons can be found in Isabelle Baszanger's work.[771]

Given the apparent underdevelopment of, and budding tensions within, pain medicine, it seems obvious that a much more extensive process of consultation and research was necessary in order to evaluate acupuncture—defined as a therapy for chronic pain—than had been envisaged by NIH officials. Coming back to the question of "was there *really* enough money," we have to consider that a program of evaluation that would be recognised as valid first required agreement about basic methodological issues, such as measuring pain and creating placebo-acupuncture. Such agreement—whether obtained by acquiring more knowledge, providing opportunities for communication and discussion among researchers, or the authoritative imposition of a standard protocol—could not be produced without the help of more material resources and time than the NIH provided.

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[771] Isabelle Baszanger, "Pain Physicians: All Alike, All Different," in *Differences in Medicine: Unraveling Practices, Techniques, and Bodies* ed. Marc Berg, and Annemarie Mol (Durham and London: Duke University Press, 1998), 119-143, Baszanger analyses the differences between these approaches at the level of therapeutic practices, noting that one attempts to cure pain through the use of "techniques" and the other instead "manages" pain through behaviour-modification. She does not mention the implications for the evaluation of treatment on the level of experience or of behaviour. A more detailed description of the two treatment approaches can be found in the second part of *Inventing Pain Medicine.*

The NIH initiative may have been more motivated by a concern to show Congress and the public that "something was being done" about the acupuncture question than by a concern to produce knowledge about acupuncture and chronic pain. Though Bonica urged that chronic pain be seen as a serious national health problem, his sense of urgency does not seem to have been widely shared by sponsors, and indeed, this continued to be a poorly funded area of research. Nor did acupuncture carry the promise of great benefit for those who had sufficient resources to mobilise a large-scale evaluation program. Listing the factors that contributed to the failure to resolve controversies about the efficacy of acupuncture, one researcher remarked in 2005, and this was also true in the 1970s: "the relative lack of funding to perform large controlled acupuncture treatment studies as compared to the resources backing the launch of new analgesic drugs should not be forgotten as an additional factor."[772]

The successful evaluation of acupuncture's efficacy indeed seems to have been impeded by a lack of material resources, and by a lack of agreement and authority required to standardise and validate a technology for the measurement of acupuncture's pain relieving efficacy. I have suggested that this lack of material and social resources stemmed, at least in part, from the youth and fragility of pain research. The lack of previously established professional networks, meetings, common definitions, knowledge, and accepted measurement tools or authorities relating to chronic pain made it impossible to launch an immediately successful evaluation program. Such a program would have required, according to most researchers, the large-scale implementation of similar or complementary study designs to produce large amounts of commensurable data. The low priority given to chronic pain as a research topic may have been a reason why NIH, or other institutions, was unwilling to provide researchers will sufficient material resources to create the necessary standardising mechanisms for this. There was another division, however, within the small core of chronic pain experts, which may not have been so easily resolved. Tensions between behaviourist and experientalist chronic pain experts continued to animate debates

[772]Bengt H. Sjolund, "Acupuncture or Acupuncture?" *Pain* 114, no. 3 (2005): 311-12.

about pain treatment, measurement and compensation throughout the 1970s and 1980s.[773]

Conclusion

Although individual patients and doctors surely made up their own minds about whether acupuncture worked, it seems that the question of its pain-relieving efficacy was never formally resolved on a collective level. Yet after 1974, it was no longer a hot issue. The lack of controlled clinical studies of acupuncture was still bemoaned in 1979.[774] Yet no further effort was made to launch any large-scale evaluation program, although individual studies continued to be published. Acupuncture had not, as some pain experts and representatives of professional organisations had feared, slipped far out of their control. As Ginger Mac Rae reported in 1982, acupuncture had failed to flourish; it was still regarded largely as a quack remedy, an option of last resort, to which few Americans had access. Mac Rae suggests that this failure was due to a shortage of services offered, due, at least in part, to restrictive legislation.[775] Medical lobbyists had indeed managed to pass legislation in some states that specified that acupuncture should remain an experimental therapy until there was adequate proof of its efficacy. But perhaps it was not so much the legislation as the lack of acupuncturists (it may not have been as lucrative as some researchers supposed), or the lack of demand (in some areas of the U. S.), that halted the uncontrolled expansion of acupuncture. In addition, some chronic sufferers may have turned towards increasingly numerous pain clinics to seek treatment.[776] Some of these clinics even offered acupuncture,

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[773] United States. Commission on the Evaluation of Pain. *Report of the Commission on the Evaluation of Pain* ed. Kathleen Foley (Washington, DC: U. S Department of Health and Human Services, Social Security Administration, Office of Disability, 1987), see the minority report.
[774] Committee on Science and Technology, United States-China Science Cooperation Hearings before the Subcommittee on Science, Research and Technology U. S. House of Representatives, May 7,8,10; June 22, 1979 (U. S. Government Printing Office Washington: 1979).
[775] McRae, "A Critical Overview,"163-96.
[776] Baszanger, *Inventing Pain Medicine*, 3, notes that before 1960, there were only 3 pain treatment centres and, though there are no figures available for the 1960s, the development was not rapid during that decade. A major impulse was a meeting on pain organised by Bonica in

alongside therapies such as nerve blocks, physiotherapy, electrotherapy and biofeedback.[777]

Newly-defined pain experts also became less dependent on the acupuncture question to draw attention to chronic pain. With the impulse they were given by the acupuncture "episode," they were able to begin, in the mid-1970s, to create their own professional networks through organisations, meetings, and journals. By the 1980s, they had begun participating in serious national and international policy debates on the evaluation of chronic pain as a disability and the treatment of cancer pain.[778]

Among themselves, however, pain experts have continued to perform occasional trials of acupuncture and to argue about the necessary conditions for a valid evaluation. A recent (2005) editorial in *Pain* has continued the long-tradition of pleas for improved trials of acupuncture. The "improvement" described by the author, however, seems to indicate a change in emphasis that is perhaps more in line with how acupuncture is actually used in the treatment of complex pain conditions and complex patients for whom there is no magic solution. Even if acupuncture was sometimes only partly effective, and not for every kind of pain, this was "not an argument against using it" since it was " what we do with both pharmacological and psychological pain management techniques. The crucial question is how to select the patients and the conditions suitable to treat, a problem that is still within the art of medicine..."[779]

Discussions about how to evaluate acupuncture reveal a model of pain as an experience not primarily situated in the body, but in a mind permeable to the influence of social relations, ideas, and psychological predispositions. This fits with the process of psychologisation and disembodiment of pain that I have been tracing in the past three chapters. Since the 1940s, instruments for collecting

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1973. In 1977, date of the first census, there were 327 pain clinics worldwide and at least 60% of these were in the U. S.

[777] Jeanette Ezzo, Brian Berman, Victoria A. Hadhazy, Alejandro R. Jadad, Lixing Lao, and Betsy B. Singh. "Is Acupuncture Effective for the Treatment of Chronic Pain? A Systematic Review," *Pain* 86, no. 3 (2000): 217-25.

[778] U.S. Commission on the Evaluation of Pain, *Report of the Commission*; World Health Organisation, *Cancer Pain Relief* (Geneva: World Health Organisation, 1986).

[779] Sjolund, "Acupuncture," 311-12.

evidence of pain from within the body were increasingly rejected in favour of methods that rested on obtaining psychological, organisational, and statistical control over experimental conditions.

Conclusion

People often look puzzled when they hear that I research the history of pain-measuring technologies, they've looked puzzled: "But *how* do you measure pain?," they asked, exclaiming something about pain being so personal, or subjective, or variable. At first, I thought this was the wrong question. "*Why* has anyone ever wanted or needed to measure pain?," seemed, to me, to be a much more obvious and historically relevant question. I soon realised, however, that the question of *how* was in fact a rather interesting one to ask, and that it was closely connected to the question of *why*. In each of my chapters, I have tried to make links between the reasons behind demands for pain-measuring technologies, the types of resources invested in their creation and implementation, the realities of pain-measuring practices, and, finally, the theoretical and empirical validation of these technologies. In this conclusion, I will further discuss the question: What is the secret of successful pain measurement?

A number of scientists and clinicians, since the 19th century, have searched for the answer in the construction of various types of pain-measuring technologies. They thought the evaluation of pain should be made quantitative because numbers were precise, comparable, communicable and impersonal.[780] However, the value of precision, comparability, communicability and impersonality has taken different meanings depending whether pain was being measured to classify human types or to produce a better analgesic. These pain measurers evidently believed that some sort of technological mediation was necessary to make the evaluation of pain quantitative and objective. They produced various sorts of non-human entities, such as meters, scales, protocols, etc., designed (by humans) and operated (by humans) to reduce or eliminate human variability and error from the evaluation process.

What made a pain-measuring technology good? Some of the people who have invented such technologies have explained that the secret in designing a good pain-measuring technology was to correctly identify the "true" nature of

[780] Ted Porter, *Trust in Numbers.*

pain. Ronald Melzack, for example, in an essay on "Concepts of Pain Measurement" published in 1983, explained that a better understanding of the motivational-affective dimension of pain had recently led to the creation of more accurate measuring tools. The previous "sensory approach to pain," however, had "fail[ed] to provide a complete picture of pain processes."[781] The adoption of "more complex models of information processing in the nervous system" and recognition of the "active 'noisy' brain," had led researchers to more fully consider the role of motivational and affective processes that gave rise to pain. A new theory, the "gate control theory" now explained how signals could be "blocked or modulated by cognitive activities," that is, by learning, past experience and thought, before it gave rise to the experience of pain. On the basis of this theory, a new, more effective pain-measuring technology had been developed: the McGill Pain Questionnaire, which used word categories and numbers to rate the sensory, affective and evaluative dimensions of pain.

Before this, however, the Cornell team had insisted that it was crucial, in order to quantify pain, to recognise that outward expressions of pain were the product of two internal processes: "sensation" and "reaction." Pure sensation involved only the sensory, perceptive and discriminative functions of the nervous system, while reaction was cognitive and affective. The reason the dolorimeter succeeded in giving such precise and consistent results was because it was able to isolate the sensory from the emotional components of pain.

Beecher pointed out that the dolorimeter did not produce consistent and accurate results when it was used under appropriate experimental conditions (as defined by Beecher). Discrepancies produced by the dolorimeter, Beecher explained, resulted from the impossibility, in practice, of dividing the pain sensation from the pain reaction. The implication of this reality was that pain was inevitably an idiosyncratic and emotional experience. It was impossible to eliminate subjectivity or to control for variability at the level of the individual, but it was possible to obtain consistency, accuracy and validity at the collective level.

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[781] Ronald Melzack, "Concepts of Pain Measurement," In *Pain Measurement and Assessment,* ed. Ronald Melzack (New York: Raven Press, 1983), 3.

Therefore, in order to measure analgesic effect, it would be necessary to approach pain relief at a population level, using appropriate statistical controls.

Melzack, Hardy-Wolff-Goodell, Beecher and their colleagues each, in their ways, have suggested that once the "true" nature of pain had been apprehended, it became possible to create the technology able to control for bias and variability, and to reliably quantify some essential component(s) of pain and its relief. In addition, however, as they themselves may well acknowledge, knowledge about pain was also often acquired through the use of measuring technologies. I have given examples of this throughout this thesis. Analgesia clinical trials were used to obtain information about how, and why, a certain proportion of the population responded to placebos for the relief of pain, as well as to measure this proportion. The dolorimeter was used to better understand the psychophysical patterns of pain perception. Various algometers were used to find out how biological, social and psychological factors influenced the intensity of pain experience.

The technical capacities of pain-measuring instruments therefore shaped possible ways of thinking about pain: about how pain experience was produced within the body and the mind, and how it was modulated by internal and external factors. While this suggests a more dynamic model of interaction between the technical and theoretical principles of pain-measuring technologies, it also suggests ways in which social and material conditions can enable certain kinds of pain-measuring practices and thus enable ways of thinking about pain. Obtaining control over pain and the conditions in which it was measured depended not only on the technical capacities of the instruments themselves, but the coordination of the actions and abilities of the people who operated them.

As I have also shown in this thesis, and as pain measurers would also probably acknowledge, human effort and judgment were necessary to make pain-measuring technologies work. The instruments did not, by themselves, ensure the precision, consistency and validity of the results they produced. These qualities also depended on the efforts, abilities, attentiveness and knowledge of the individuals who operated these technologies.

As we have seen, the proper functioning of the dolorimeter depended on the ability of subjects to recognise the pain threshold accurately. The analgesic clinical trial depended on the coordination of many actors, and more specifically on the good judgment of observers, statisticians and subject-selectors, as well as the ability of subjects to be decisive, alert to differences in levels of pain intensity, and their willingness to give their pain a number.

Thus, while pain measurers have generally tended to emphasize the theoretical knowledge and technical control necessary for the successful measurement of pain, my own investigation of pain-measuring practices has led me to conclude that creating a valid technology of pain measurement also depends on the success of social processes. These are not separate processes: The technical capacities and theoretical underpinnings of measuring technologies interact dynamically with the social and material conditions of measurement practices. While pain measurers have often neglected, at least in print, to give importance to the broader social and material networks in which certain kinds of "measurable" pain become practicable and valuable, my own interpretation of pain-measuring technologies would be partial if it did not take into account these actors' technical and theoretical explanations of whether, and why, they worked.

When I say that creating a "measurable pain" is, in large part, a social achievement, I mean this in several ways. First, I suggest that the value given to pain measurement by sociological groups, for reasons that are often political, economic, or social, drives the creation of pain-measuring technologies, defines their function, and influences the determination of their validity. In other words, what pain-measuring technologies are considered to be useful for, and by who, may influence whether they will work (and, obviously, whether they will be created in the first place). Second, I would argue that obtaining consistent, accurate and valid measurements of pain, or pain relief, requires social, as much as technical control over the conditions and practices of pain-measurement. That is, to eliminate the influence of irrelevant variables, it is necessary to be able, to some extent, to predict and control people's actions (and thoughts). Finally, the use of pain-measuring technologies has a social impact. They can alter the

relations and the distribution of authority and of resources among people both within the immediate clinical or experimental context, and beyond that context.

When pain measurers report on the design or validity of pain-measuring technologies, they sometimes mention, usually briefly, the utility of these methods. They tell us that some technology may be useful for clinical diagnosis or for therapeutic evaluation. Sometimes they simply let us assume pain-measuring instruments will, in some vague way, contribute to the human struggle against pain. Such omissions seem to imply that the validity of pain-measuring technologies has nothing to do with their purpose. The history of pain-measuring technologies, however, shows that the importance they have been given, for the fulfilment of a specific purpose, can influence their chance of working. When specific technologies were seen to have a greater economic and professional importance, such as the analgesic clinical trials sponsored by CDAN, they were supported with greater social and material resources. As we have seen, in 6, larger grants translated into salaries for full-time observers and fees for statistical consultants who transformed the practice of the clinical trial and made it a much more effective technology. In chapter 7, however, we saw that small and scattered grants for investigations of acupuncture analgesia made it impossible to implement the kind of mechanism necessary to coordinate research on a sufficiently large scale to obtain consistent and comparable results.

In addition, the reason for which pain was being measured also entered into consideration in the evaluation of the validity of a particular technology. For example, algometers were thought to be highly reliable when they were used to measure stable inter- or intraindividual differences in sensitivity in the early twentieth century, but the variation in their results was judged to be too erratic for the purpose of measuring analgesic efficacy in the 1910s. At that time, making analgesic testing objective was not a high priority, and no efforts were invested towards the creation of pain-measuring technologies that were more appropriate for this purpose until the mid 1930s.

We can also look at the evolution of self-reporting pain scales and questionnaires. In the 1950s, these were considered to be valid for the

measurement of therapeutic effect when used within the context of a clinical trial, but do not seem to have been used to evaluate the pain of disease or injury within individual therapeutic encounters. A few decades earlier, such scales might well have seemed absurd to clinicians such as Libman and Behan, for whom the ideal type of technology would bypass patients' own expressions of pain, which could be distorted by emotions, insensibility or conscious lying. It was also unclear that such tools would have been relevant for answering early to mid century questions about the evaluation and treatment of pain. In the last decades of the 20th century, however, self-reporting scales and questionnaires became widely used for clinical pain evaluation, at least in North America. The new uses for which these tools have come to be seen as relevant and valid appear to be linked to the emergence of new kinds of debates about the treatment and compensation of pain, particularly of chronic pain, that have appeared with the institutionalisation of "pain medicine" and the development of a "culture of pain." Within the context of these debates, self-reporting pain scales and questionnaires are seen as contributing relevant information about the effectiveness of different types of pain clinics, the distribution of disability benefits and equitable access to appropriate pain relief. For these purposes, it was important not to eliminate patients' emotions, but to fully integrate them into the evaluative process. In addition, by this time, malingering was widely considered to be rare and insignificant, and thus not an obstacle to giving patients control over the assessment of their pain.

Thus, the social, economic and political function of pain evaluation—whether it be to serve the development of the analgesic drug industry, to detect the presence of a "real" physical disability or to promote equitable access to pain relief—have shaped ideas about how pain should be evaluated, and thus of what pain-measuring technologies should *do*. In addition, the relative importance accorded to such functions influenced the amount of effort, time, money and interest that were invested towards the improvement of pain measurement at particular times. Both types of dynamics suggest links between the historically-specific and socially defined functions of pain-measuring technologies, their design and the judgment of their theoretical and empirical validity.

How exactly does the investment of resources into the development and use of pain-measuring technologies improve their validity? What type of resource did it take to make these technologies work? As I have shown, different types of technologies required different amounts and types of resources in order to produce accurate and consistent results, and did so on different scales. Mechanical technologies such as the dolorimeter, or the fMRI technology I mentioned in the introduction, seem to require mainly material resources to obtain control over the variables that could distort the evaluation of pain, and to increase the precision of measurement. As we have seen, however, instruments such as the dolorimeter worked well when they were used in certain ways, which implies that successful measurement required some control over the behaviours and attitudes of the people who used it. To obtain this control, investigators needed the right type of subject and time for training, in addition to a reliable instrument. Though it was fairly cheap, the dolorimeter seemed to yield consistent results only on a fairly small scale. When more money was invested in analgesic testing, however, it became possible to obtain control on a larger scale. In particular, the hiring of full-time observers and access to large sources of hospital patients made it possible to obtain abundant and comparable data. This kind of large-scale control also made it possible to define pain as an incorrigibly idiosyncratic experience rather than attempt to transform it into something more stable. These case studies suggest that both material and human resources, that is, certain types of people, of labour and authority, were required to make pain-measuring technologies work. In addition, these resources were required not only to obtain technical control but also social control (by predicting and prescribing behaviours) over the conditions and practices of measurement in order to achieve accuracy, consistency and precision.

When it became necessary to validate certain kinds of statements about pain on a larger scale—for instance, that acupuncture was effective in the relief of surgical or chronic pain—it was also necessary to coordinate measurement practices on a larger scale, and thus to obtain access to more resources and authority. For example, elite researchers suggested that the design and results of

individual trials of acupuncture be coordinated at a state and national level in order to accumulate sufficient, and sufficiently comparable, evidence to determine its pain relieving efficacy. It proved impossible, however, to obtain sufficient money and authority to create and implement such mechanisms of control over the design and implementation of technologies of acupuncture evaluation.

When pain-measuring technologies were implemented, they reconfigured the distribution of authority over the evaluation of pain. Both within and beyond the immediate context of measurement, pain-measuring technologies have transformed social relations. Consider the analgesic clinical trial. Its use changed working relations between researchers, hospital staff and patients. It appeared to give patients more authority over the judgment of their own experience of pain, and it is probable that a 1950s surgical patient was more often asked for their opinion about the effectiveness of a treatment if they were participating in a clinical trial than during the course of normal postoperative care. On the other hand, their individual statements would be given little credibility in the final evaluation of the efficacy of a drug. Nurses' and doctors' opinions about the effectiveness of drugs were given no weight in the evaluation process.

The contrast between early and late 20th century technologies, and the authority they were meant to eliminate, is also instructive. Algometers were originally, around the turn of the 20th century, designed to bypass the conscious, wilful and idiosyncratic interpretation of pain by individual patients, and thus to eliminate their authority over the evaluation of pain. The American Pain Society's 1990s campaign of "Pain: The Fifth Vital Sign," however, promoted clinical pain scales as a means of giving every patients' pain a voice, and reducing physicians' discretionary power to decide which patients should be asked about their pain, and how their pain should be taken into consideration.

Certain kinds of technologies also empowered those who controlled their use. For example, responsibility for, and expertise in, the running of analgesic clinical trials benefited the professional status of anaesthesiologists and clinical pharmacologists. As we saw in chapter 7, elite researchers were accused of taking over the evaluation of acupuncture because they insisted on the exclusive validity

of testing technologies such as the analgesic clinical trial. Disputes about whose authority it would be to pronounce acupuncture effective or ineffective—between elite American researchers, various Chinese figures, acupuncturists, legislators and American chronic pain sufferers—may have hampered the ability of these researchers to insist on the validity of any single technology of pain-measurement for the evaluation of this controversial therapy.

The outcome of acupuncture's evaluation—whether it was approved or dismissed— would also have had different implications for each group of actors involved. In the case of CDAN-sponsored trial, however, everyone agreed on the importance of the goal that was pursued through evaluation: the creation and approval of lucrative, safe and effective analgesics. More recently, disagreements over the ways in which social security disability benefits should be distributed to chronic pain sufferers has delayed the introduction of measuring technologies into American disability evaluation.

The close and complex connections between the changing social value of "measurable pain" and the uses and validity of pain-measuring technologies have continued to be evident in recent debates and practices. Beginning in the mid 1970s, the landscape of pain-measurement was dramatically altered. The institutionalisation of "pain medicine" in professional organisations, meetings, journals and clinics created new demands for new types of pain-measuring technologies, new spaces in which they could be circulated, and new conditions for their use. For various reasons, pain-measuring technologies have acquired new uses and have been circulated increasingly widely during the past four decades. I believe that a closer examination of the conditions and consequences of their use can reveal a lot about the issues and practices that affect how sufferers have been managed and treated during this period. My hope is that my thesis has offered new ways of interrogating the issues surrounding the measurement of pain that will be useful for understanding both the narrow and broad implications of these technologies for healthcare professionals and people in pain.

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