Challenges in attributing avoided deforestation to policies and actors: lessons from provincial forest zoning in the Argentine Dry Chaco

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Abstract

Rigorous impact assessments test for causal effects of interventions on outcomes of interest. When findings of such assessments become part of political and scholarly controversies, they can be interpreted in unintended ways. The value of the ensuing debate is enhanced by a shared understanding of key concepts, methodological approaches, and evaluative criteria. Here we illustrate the importance of such shared understanding by example of a recent controversy surrounding the estimated impacts of decentralized zoning on deforestation in a major agricultural frontier, the Argentine Dry Chaco. In a recent analysis, we concluded that provincial zoning plans had significantly reduced deforestation in three provinces; critics suggest it had not. In attempting to resolve this debate, we identify six areas in which shared understanding can support more productive interaction. These include: (1) the distinction between impact and other measures of effectiveness, (2) an appreciation of recent advances in methods for causal inference, (3) the distinction between effective and perfect enforcement, (4) the challenge of attributing impacts to mechanisms and actors, (5) transparency in standards used to judge the desirability of observed outcomes, as well as (6) caution in the generalization of findings to other geographies.

1. Introduction

Conservation is shaped by conflicts of interest (McShane et al., 2010). Whether a community protects a sacred forest, local fisheries devise harvest rules, or national agencies adopt conservation laws, the conservation of an environmental good implies that at least one actor will not use the good in ways it would otherwise have liked to. For that reason, the design, implementation, and evaluation of conservation policies is often accompanied by debate and controversy, with interest groups drawing from different sources of evidence to advocate for specific goals or means. Scholars interested in the effectiveness and impacts of conservation policies can find themselves implicated in such controversy – intendedly and unintendedly – when research findings enter the public sphere and are interpreted by colleagues, politicians, and the media (e.g., Brandt et al., 2016; Karsenty et al., 2017). Such debate can be valuable if participants share an understanding of the conceptual and analytical dimensions at hand; its usefulness can be constrained by disagreement on key dimensions of the issue, the misinterpretation of methods and inferential statements, or the application of different standards to judge observed outcomes.

A recent debate on the impacts of decentralized land use zoning on deforestation in the Argentine Dry Chaco, a globally relevant agricultural frontier (Baumann et al., 2016; Bucher and Huszar, 1999) illustrates these dimensions of scientific controversy. In 2007, Argentina passed a Forest Law which required provinces to establish land use zoning for their remaining native forests (García Collazo et al., 2013; Gautreau et al., 2014). Because provinces had substantial leeway in the implementation of the law (Gautreau et al., 2014; Gobbi, 2015) and faced strong incentives to implement zoning plans that would not inhibit agricultural expansion, skeptics believe that the policy did little to affect deforestation. However, using a rigorous empirical estimation strategy, we estimated that provincial land use zoning did reduce deforestation significantly in each of the three provinces that accounted for more than three quarters of pre-law deforestation: Salta, Santiago del Estero, and Chaco (Nolte et al., 2017a).

Within one year of publication of our analysis, three studies appeared in the peer-reviewed literature with findings that seem to contradict our own. Of main interest here is the response by Volante and Seghezzo (2018) who, “emphatically rebut […] the alleged causal relationship between deforestation trends and decisions made by subnational administrations”, offering numerous reasons for their disagreement (see below). They also suggest that our analysis had “negative political consequences at the local level” and was used by “governmental officials […] to publicly justify their past and present policies”. Their rejection of a causal effect of zoning on deforestation seems to be supported by two empirical studies of deforestation patterns in individual provinces. Ceddia and Zepharovich (2017) find that the Forest Law was “not effective at slowing down deforestation and habitat loss” in Salta. Camba Sans et al. (2018) suggest that the Forest Law was “ineffective for avoiding deforestation in categories of high conservation value” in Santiago del Estero.

These perspectives provide an opportunity to enrich the scholarly debate on the effectiveness of conservation policies in general, and of decentralized zoning in the Argentine Dry Chaco in particular. Reporting "positive" causal impacts of conservation policies raises legitimate concerns when the stringency and implementation of such policies leaves much to be desired from the viewpoint of social and environmental advocates: notably, large-scale deforestation in the Argentine Dry Chaco continued, and a substantial proportion of that deforestation was not in compliance with the Forest Law. However, rigorous evidence should not be dismissed in the heat of the argument. While the responses do offer valuable insights that complement and enhance our understanding of the observed impacts, they also contain inaccuracies or misinterpretations that stand in the way of a shared perspective.

In what follows, we attempt to reconcile the diverging views on the impacts of provincial zoning in the Argentine Dry Chaco. In doing so, we identify several lessons for empirical scholars engaged in debates on impacts of conservation policies. These lessons are about: (1) the difference between policy "impact" and other measures of effectiveness, (2) recent advances in methods to identify the causal impact of policies in the presence of other spatial and temporal factors that influence outcomes (confounders), (3) the distinction between perfect and effective enforcement, (4) the challenge of attributing impacts to actors, (5) standards used to judge the adequacy of impacts, and (6) the generalizability of findings. We discuss these issues for the case of regulatory land use policies, yet many of these lessons are applicable across policy domains.

2. Discussion

2.1. Distinguishing "impact" from other measures of policy effectiveness

Impact evaluations measure the causal effect of interventions on outcomes. "Impact" is defined as the difference between outcomes observed in the presence of the intervention and outcomes that would have been observed in its absence (Ferraro, 2009). This counterfactual definition of impact is arguably identical to that of a "causal effect", and used across domains of social intervention, including health (Habicht et al., 1999; Pullin, 2001), education (Slavin, 2002), finance (McKenzie, 2010) and development (Baker, 2000). It is also implicit in statements that claim whether or not an intervention "affected", "reduced/increased", "avoided" or "slowed down" an outcome of interest. Impact is thus a continuous measure: it can be small or large, and its estimated effect can be statistically significant or not.

Estimating impact requires making assumptions about *counterfactual* outcomes, which are never observed and can only be inferred. Recent years have seen an increase in the application of rigorous impact assessment methods that infer counterfactual outcomes of land use policies through advanced statistical methods (Andam et al., 2008; Börner et al., 2016; Miteva et al., 2012; Nolte et al., 2013). Such rigorous evaluations can be quite demanding in terms of system understanding, data, and methods. Firstly, most approaches depend on the existence of suitable control observations (comparable units that were not affected by the treatment in question) and are thus not applicable in contexts where such controls do not exist. Secondly, analysts need to understand the dynamics of both the outcome (e.g., deforestation) and the intervention (protection) to identify variables that affect both (cf. 2.2. Estimating policy impacts). Thirdly, successfully controlling for those variables requires data for both treated and untreated units, before the treatment was assigned. Fourthly, analysts and their audience need to be familiar with the statistical approaches used to infer impact. Combined, these demands can stand in the way of rigorous impact assessment in many real-life contexts (Margoluis et al., 2009).

In the absence of rigorous impact assessments, policy analysts often resort to methods that do not involve counterfactuals. Such methods might also rely on measurements of observable outcomes, but they differ in the standard against which such outcomes are compared. For instance, a common conceptualization of “effectiveness” refers to the extent to which certain goals or standards have been reached (Nathan and Pasgaard, 2017). Selecting such goals or standards is inevitably a social and subjective process (cf. 2.5 Judging policy impacts). If this standard corresponds to a legal rule, the analysis becomes an assessment of regulatory compliance. The works by Volante and Seghezzo (2018), Ceddia and Zepharovich (2017), and Camba Sans et al. (2018) all contain language that infer an ineffectiveness of the Forest Law from an observed lack of compliance (Table 1): in all provinces, deforestation occurred in zones which, according to the national Forest Law, as well as selected provincial interpretations of it, should not have allowed any deforestation ("red" and "yellow" zones).

Compliance analysis is useful when analysts wish to highlight discrepancies between legal goals and observed outcomes. However, compliance is rarely a good proxy for the impact of a regulatory policy: it does not attempt to measure whether or not a policy made a difference. A regulatory policy may have an impact even if some level of illegal activity persists (cf. 2.3 Attributing deforestation reductions to mechanisms). Similarly, compliance does not imply impact: when protected areas are placed in areas of low conversion risk, their impact might be negligible, even if compliance is perfect (Nolte et al., 2013). As our interest lies in the measurement of impact, we focus on the difference between deforestation observed in reality and deforestation that we estimate would have occurred in the absence of the Forest Law (Nolte et al., 2017a).

2.2. Estimating policy impact in the presence of spatial and temporal confounders: did decentralized zoning reduce large-scale deforestation?

Inferring policy impact from observational data requires particular care when outcomes are influenced by factors other than the policy of interest that also co-vary across space and time. Such spatio-temporal "confounders" are present in most landscapes where conservation policies are implemented. For the Argentine Dry Chaco, Volante and Seghezzo (2018), Camba Sans et al. (2018), and Ceddia and Zepharovich (2017) identify several factors that complicate assessments of policy impacts. Deforestation can vary across space as a function of agricultural productivity, accessibility, distance to previous deforestation, and the willingness of local stakeholders to protect forests (Gasparri et al., 2015; Volante et al., 2016). It can vary across time as a function of global agricultural prices, exchange rates, macro-political change, and climate variability (Bravo et al., 2010; Piquer-Rodríguez et al., n.d.; Richards et al., 2012; Siegert et al., 2001). As a result, deforestation rates fluctuated across the Argentine Dry Chaco even before the adoption of the Forest Law (Baumann et al., 2016; Vallejos et al., 2015).

Recent advances in empirical methods have strengthened our ability to infer causal policy impacts in the presence of such confounders (Ferraro and Hanauer, 2014a). In our analysis of the Argentine Dry Chaco, we combine two methods – paired matching and differences-in-differences (DID) – to isolate the effects of stricter zonation on deforestation from confounding effects (Table 1). Each method has distinct advantages, and their combination is particularly powerful.

Paired matching is a quasi-experimental technique that identifies pairs of treated and untreated observations (in our case, properties in stricter vs. less strict zones) that were similar in terms of *observable* confounders (i.e., confounders for which analysts were able to obtain data) before the treatment occurred (Ho et al., 2007). Paired matching attempts to emulate a random experiment: if treatment and control groups were not systematically different *before* the treatment, differences in observed outcomes *after* treatment can be assumed to be the causal effect of the treatment itself.

In practice, analysts rarely observe (i.e., obtain data for) all relevant confounders. For instance, unobserved differences in the willingness of local stakeholders to protect forests can affect the likelihood of both stricter protection and deforestation, and thus bias findings. Paired matching cannot remove unobserved heterogeneity. However, DID can control for unobserved confounders as long as their effects are time-invariant, i.e., their influence on the outcome does not change over time. DID infers policy impact from the differences in outcomes between treated and control observations before and after the assignment of treatment. In other words, if the difference in outcomes between treatment and control groups *changes* after the treatment, this “difference in differences” can be interpreted as the impact of policy. DID not only controls for time-invariant unobservables that might differ systematically between groups; it also reduces threats to inference that stem from unobserved confounders that vary over time, but whose effects on outcomes do not vary systematically between groups. Combining paired matching and DID provides a powerful inferential strategy to test for policy impacts in the presence of both observed and unobserved confounders (Brandt et al., 2015). In our analysis, we apply this approach to data from 30,126 parcels in three provinces to estimate the impact of stricter zonation (“red” or “yellow”) on parcel-level deforestation rates, using parcels in less strict zones as controls.

Responses to our paper largely ignore the inferential properties of this approach. Volante and Seghezzo (2018) and Camba Sans et al. (2018) claim that we do not control for time trends. Yet we do: our DID estimates reduce, if not eliminate, the risk of temporal confounders. Volante and Seghezzo (2018) suggest that we do not account for the contagiousness of deforestation in the Argentine Dry Chaco. Yet we do: one of the observable variables we balance through matching is the level of nearby deforestation that occurred prior to the adoption of the Forest Law. Volante and Seghezzo (2018) also suggest that we "zon[ed] land units prior to actual land use planning". This reflects a misreading of placebo tests that we conduct to ensure that deforestation rates and trends on matched control and treatment units did not differ prior to the assignment of treatment. A failure of such a test would indicate the presence of unobserved bias between matched groups, and thus cast doubt on the success of paired matching. The fact that we do not find significant differences in pre-treatment deforestation trends bolsters the strength of our inference.

Closer attention to inferential methods allows scholars to engage more productively with research findings. Our approach offers critics at least three points of leverage against our conclusions, none of which have been comprehensively addressed:

1. Matching-based approaches have the shortcoming of allowing causal inference only for treatment and control units that are considered comparable. Non-comparable units that diverge too much in terms of key confounders are dropped. The fact that, on average, we found controls for only 71% of treated

units implies that our analysis remains silent about policy impacts for a substantial part of the landscape. If excluded properties differed systematically with respect to the impact of zonation, this could affect the validity of our findings. However, precisely due to the absence of such comparable units, it is difficult to test for the absence of this risk without extrapolation, which requires additional assumptions about the functional relationships between treatment, outcome, and confounding variables (Morgan and Winship, 2007).

2. Our impact estimates could be biased in the presence of unobserved (i.e., unmatched) variables whose effect on deforestation varied systematically between stricter vs. less strict zones, but only *after* treatment occurred (otherwise, we would observe different pre-treatment deforestation rates and trends), and *not* as a result of such treatment (otherwise, it would count as a policy impact). We do not know whether such a variable exists. The presence of smallholders and/or indigenous groups raised by Volante and Seghezzo (2018) might be a candidate. However, it is not clear what factors other than the Forest Law and its zonation would have conferred these groups increased leverage against deforestation, and only so after zonation was implemented. Another possible candidate might be external random variation in large-scale contiguous natural events such as fires (Boletta et al., 2006; Bravo et al., 2010) that may disproportionately affect parcels in one zone. Whether such large-scale fires occurred to an extent that affected results remains to be studied.

3. The potential presence of leakage – a policy-induced displacement of

deforestation from one geography to another (Atmadja and Verchot, 2012; le Polain de Waroux et al., 2017) – inhibits our ability to quantify the *net* impacts of the Forest Law on deforestation. Firstly, stricter zonation might have displaced deforestation to less strict zones within our study area, which could result in an overestimation of differences between treatment and control groups. While we tested for localized leakage by ignoring controls units that are situated too close (<10km) to treated units (which did not affect findings), it is more difficult to control for long-distance leakage within a study region. Secondly, the Forest Law might have displaced deforestation to locations outside our study region. We agree with Volante and Seghezzo (2018) that it is possible that some of the reduction in deforestation observed in stricter zones led to a displacement to other provinces like Formosa, or to neighboring countries like Paraguay and Bolivia. In sum, while our analysis leaves us confident that stricter zones lowered deforestation as compared to less strict zones, it does not allow us to separate net reductions from displacement effects, which would be required to quantify overall impact. For that reason, the exact quantity of net deforestation reduction has to remain elusive. However, emerging evidence suggest that such leakage is unlikely to offset impacts entirely: one recent study estimates that Argentina's regulations displaced only 6.8% of deforestation to Paraguay and Bolivia (le Polain de Waroux et al. 2016); another study finds no evidence that regulation displaced deforestation across South America's deforestation frontiers (le Polain de Waroux et al., 2017).

2.3. Attributing deforestation reductions to mechanisms: what role did enforcement play?

When causal impacts of a policy are identified, analysts often wish to explain *why* such impact was observed. Recent work has showcased rigorous methods that allow to test for the existence of causal mechanisms through which a policy affected outcomes (Ferraro and Hanauer, 2014b; Flores and Flores-Lagunes, 2009). We did *not* conduct such analysis before suggesting that the observed impacts of zonation on deforestation were likely a result of increases in regulation and enforcement capacity. Instead, we based our proposition on a diverse range of anecdotal evidence, collected over 13 weeks of field research and 122 interviews by the three first authors, that enforcement capacity appeared to have increased across the three provinces studied. Among other things, we noticed increased remote detection capabilities, higher reported frequencies of field visits, higher legal levels of sanctions, and an increase in the numbers of sanctions issued.

Volante and Seghezzo (2018) reject the assertion that enforcement led to significant deforestation reductions. They join Ceddia and Zepharovich (2017) and Camba Sans et al. (2018) in highlighting that the enforcement of the Forest Law has been “ineffective” and “poor” across the Dry Chaco, as evidenced by high levels of illegal deforestation (i.e., deforestation surpassing legal property-level restrictions) and deforestation in strictly protected zones. Illegal forest conversion is indeed widespread in the Dry Chaco. In Santiago del Estero and Salta, more than 50% of deforestation occurred in zones were deforestation was supposedly prohibited (Camba Sans et al., 2018; Ceddia and Zepharovich, 2017). Yet, impacts of enforcement can co-exist with illegal activity, as enforcement does not have to be perfect to achieve a deterrent effect (Robinson et al., 2010). A recent example is the Brazilian government's massive crackdown on illegal deforestation in the Amazon region, which achieved a 70% reduction in forest loss, in spite of illegal forest conversion continuing throughout the region (Assunção et al., 2013; Börner et al., 2015; Godar et al., 2015).

Conclusively assessing the role of enforcement in reducing deforestation in the Argentine Chaco requires not only a robust and spatially consistent estimate of enforcement activity, but also a model of how such activity translates into a deterrent effect (Nolte, 2016). In the absence of such estimates, anecdotal evidence from field research suggests that land managers adopt a wide range of strategies to respond to regulation, from "waiting and seeing", to pre-emptive deforestation, to the adoption of silvo-cultural practices associated with a slow, steady loss of forest cover over time. A recent study also finds that enforcement significantly deterred land investments, and thus deforestation, by large agricultural companies across the Dry Chaco (le Polain de Waroux et al., 2016)

Even if the deterrent effect of current enforcement was found to be negligible, other mechanisms could explain the observed impacts of regulatory zoning on deforestation. For instance, a mere expectation of *future* enforcement likely affects the net present value of future returns of agricultural production and, by extension, returns to forest clearing. Similarly, expectations about the development of production-friendly environments in less strict zones (e.g., clustering of agricultural services) can pull agricultural investments away from more regulated zones (Garrett et al., 2013). Identifying the relative importance of these different causal mechanisms in reducing deforestation would be an g subject of future research.

2.4. Attributing deforestation reductions to actors: what can be inferred about the willingness of provinces to reduce deforestation?

Environmental policy making is a political struggle in which multiple interest groups attempt to influence decisions towards their preferred outcomes, sometimes through unobserved channels of influence. Deducing the specific causal influence of any actor group from a mere observation of adopted policies and impacts can be a challenging, if not impossible, endeavor (Nolte et al., 2017b). In-depth research into the political ecology of decision making provides illuminating clarifications and insights (Seghezzo et al., 2011). While providing conclusive evidence of the willingness and ability of actors to influence a policy outcome will remain challenging, advances in qualitative methods offer some promising avenues (Brannstrom, 2011).

Provincial zoning in the Argentine Dry Chaco was shaped by intense struggles between the interests of agricultural industry, smallholders, indigenous groups, environmental activists, and the national government (Gautreau et al., 2014; Gobbi, 2015; Seghezzo et al., 2011). Given the difficulty in reconstructing these complex interactions *a posteriori*, we decided to keep inferences about the specific role of the final decision makers – provincial governments – in swaying decisions towards stricter or less strict zonation outside the scope of our analysis. When we ask whether our findings can serve as an indicator of provincial "willingness and ability" to reduce deforestation, we make no conclusive statements, but instead acknowledge the challenges of isolating the role of provincial priorities from those of other actors. Indeed, provincial zoning plans would likely look different if the national government, indigenous groups, and environmental activists had not actively advocated for more forest conservation (Volante and Seghezzo 2018). However, jurisdictions with similar levels of economic dependence on agriculture tended to favor agricultural expansion over conservation, as observed in neighboring Paraguay, Bolivia, and Formosa (Nolte et al., 2017b). More research is needed to better characterize the role of provincial governments in negotiating these trade-offs between competing interests.

2.5. Judging policy impacts: how much deforestation is acceptable?

If decentralized zoning reduced deforestation, but illegal deforestation continued, should the zoning plans be judged as a success or failure? The answer to this question depends on the standard against which outcomes are compared. Scholars and practitioners can resort to a wide range of analytical devices to suggest or define such standards – including cost-benefit analysis, environmental valuation, participatory deliberative institutions, moral reasoning, reference to legal or political goals, and, of course, personal opinion (Spash and Vatn, 2006). Different stakeholders are likely to set different standards, ranging from any deforestation being interpreted as a failure to any reduction in deforestation being interpreted as a success.

As a group of authors, we share a concern for the ecological and social impacts of the large-scale land use change in the Argentine Dry Chaco. However, our opinions differ, or are ambiguous, when it comes to the desirability of specific deforestation outcomes in the Argentine Chaco. We thus refrained from setting such standards (for instance, we do not refer to policy outcomes as “success” or “failure”), but instead focused on whether or not decentralized zoning can reduce deforestation in a major agricultural frontier. In doing so, we may have implicitly set a standard that any change from the status quo is a potentially *interesting* outcome. In any case, what we do reject is the assertion that the only valid interpretation of remaining (and illegal) deforestation is that of a failure of the Forest Law, given that the law did have a measurable effect towards reaching its goal.

In spite of our attempts to refrain from judging the desirability of policy outcomes, Volante and Seghezzo (2018) mention that “government officials in at least one province took advantage of the overall message […] to publicly justify their past and present policies with respect to deforestation and related law enforcement”. The two news articles cited to corroborate their claim actually reproduce our findings correctly. However, it is certainly possible that officials may have referred to our work as a proof of success of their policies, which would not be the first time that “academic analysts

[…] find their scholarship appropriated in unexpected ways" (Jasanoff, 1996). In fact, readers might have interpreted our findings as an *implicit* judgment of provincial actors and policies. Such misinterpretations are difficult to avoid altogether but could have been forestalled by being more explicit about what we did and did not say: specifically, that we did not confer praise or blame to any particular actor group or action taken.

2.6. Generalizing insights to other contexts

Our findings have been criticized for being affected by selection bias in the choice of our study area (Volante and Seghezzo, 2018). We chose our study area based on relevance and data availability. Salta, Santiago del Estero, and Chaco accounted for 79% of forest loss observed in the Argentine Dry Chaco before 2007, with the remaining 21% distributed across nine provinces. Parcel data for Formosa covered only a fraction of the territory, most of which was situated in the Humid Chaco, making it impossible to conduct analyses of the same level of rigor as we did for the other three provinces. Whether or not Argentina's Forest Law affected deforestation in provinces and ecosystems other than those studied thus requires further analysis. Pending such work, the assertion that increasing deforestation in Formosa implies the absence of an effect of Formosa's zoning plan is premature, as deforestation in that province had been following an upward trend before 2007. Meanwhile, other omitted provinces had declining deforestation trends (e.g., Córdoba). Whether or not such trends were an impact of land use zonation can only be inferred from analyses that control for spatial and temporal confounders.

To which extent is our main finding – that decentralized land use zoning can reduce deforestation – generalizable to locations beyond the Argentine Chaco? We do not make strong claims to this effect. Prior evidence about the effects of decentralization on forests remains inconclusive. Studies have found decentralization to increase deforestation (Burgess et al., 2011), to have no effect on forest loss (Andersson and Gibson, 2007; Pfaff et al., 2012), to produce similar forest outcomes as state-led conservation (Somanathan et al., 2009), to reduce deforestation in some locations and time periods but not others (Lund et al., 2015; Santika et al., 2017; Wright et al., 2016), or to reduce deforestation in the short run (Blackman et al., 2017). Synthesis across studies is difficult due to considerable variations in examined administrative levels (states, districts, municipalities, villages, indigenous communities), types of decentralization (subdivision of districts, creation of federal vs. state parks, decentralization of forest management rights, communal titling of land, etc.), and in the rigor of analytical methods used to identify causal effects. Few studies have analyzed the effects of participatory land use zoning processes mandated by a higher administrative level but whose implementation was determined by a lower level that *already had* the legally recognized right to define and allocate land rights. Policies with such a specific administrative setup are likely infrequent. However, the growth management acts passed in several U.S. states in the 20^{th} century might provide a set of related examples, as they also obligated communities to engage in land use planning (Meyer et al., 2012). One study from Oregon finds that this approach provided “a measurable degree of protection to forest and agricultural lands" (Kline, 2005), which aligns with our findings.

3. Conclusion

Informed scholarly debate is a key ingredient of the scientific enterprise. We identify six areas where a shared understanding of concepts, methods, and perspectives could foster a more insightful appreciation of apparently contradictory research findings. Firstly, counterfactual reasoning needs to be a key ingredient of any inference of policy impact, whereas alternative approaches, such as studies of compliance, can provide important complementary perspectives. Secondly, methods to infer policy impacts in the presence of spatial and temporal confounders have advanced considerably; literacy in such methods and attention to detail can inform a productive critique. Recent advances in methods have also increased our ability to identify causal mechanisms; in the absence of their use, the identification of the relative importance of specific linkages between policies and impacts is empirically challenging. Thirdly, enforcement does not have to be perfect to have a deterrent effect, though assessments of compliance will likely continue, as they are easy to communicate and frequently offer environmental advocates political leverage. Fourthly, ascribing policy outcomes to individual actors or groups is a challenging endeavor, which will likely entail a non-negligible degree of speculation. Fifthly, whether an observed policy outcome is a success or failure depends on the reference of the observer; the setting of such a standard is inevitably subjective, but contributors can forestall criticisms by identifying their own upfront. Finally, caution should be exercised when generalizing findings beyond a given study area; where possible, assumptions regarding the generalizability of findings should be tested empirically.

Did provincial governments in the Argentine Dry Chaco implement land zoning plans that inhibited agricultural expansion and reduced deforestation in Salta, Santiago del Estero, and Chaco? Our empirical answer remains cautiously affirmative. We maintain our conclusion that large-scale deforestation in major agricultural frontiers can be slowed down by subnational decisions within a national framework that prescribes processes (here: timelines, types of policy instruments, degree of stakeholder participation, information requirements), not outcomes (such as deterministic rules regarding the size and location of stricter zones, or on the amount of remaining deforestation). We do not judge whether the observed impact is satisfactory, or whether it occurred at an acceptable speed or level of legal compliance. We recognize that illegal deforestation is prevalent across the Argentine Dry Chaco, and that the provincial implementation and enforcement of the Forest Law did not satisfy the expectations of numerous stakeholders, including the national government, indigenous groups, and conservation advocates. We remain cautious about the generalizability of our findings, and certainly do not suggest that provincial zoning will always result in similar reductions. Finally, none of our findings should be construed as a claim that the implemented policy was more effective, efficient, or equitable in avoiding deforestation than any other option. These are important knowledge gaps that future scientific inquiry could, and should, attempt to narrow. More and better research and disciplined controversy will help build a stronger evidence base for effective environmental governance.

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Table 1: Key differences in approach and conclusions of the four studies. All four studies base their quantitative analyses on the same deforestation dataset (Vallejos et al., 2015) and thus cover the same ecoregion (Dry Chaco).

Reference	**Provinces; Unit of Analysis**	**Measure to infer policy effect**	**Inferential method**	**Interpretations regarding policy effect**
Camba Sans et al. 2018	Santiago del Estero; 11,196 parcels*	- Differences in forest loss in stricter vs. less strict zones before vs. after the FL - Compliance	- BACI (before vs. after, control vs. intervention); not controlling for confounders - Compliance with provincial regulations in one sub-zone (empirical analysis)	- “it is difficult to determine the relative contribution of the [FL] in reducing deforestation” - “the zoning policy was ineffective for avoiding deforestation in categories of high conservation value” - “deforested area in [parcels] that did not comply with restrictions […] was higher than for those that did”
Ceddia & Zepharovich 2018	Salta; 6 departments	- Differences in forest loss before vs. after the FL (no comparison of zones) - Compliance	- Panel regression model, using a FL dummy for post-2009 years, controlling for agr. production, pop. density, indig. land - Compliance (citing secondary data on estimated illegal deforestation)	- “[results] point out to the ineffectiveness of the Forest Law in Salta at deterring the process of deforestation and natural habitat loss” - “The fact that a large amount of illegal deforestation occurred […] indicates that the law is ineffective”
Nolte et al. 2017	Salta, Santiago del Estero, Chaco;	- Differences in forest loss on properties in	- Quasi-experimental matching to control for observable confounders (rainfall, soil, accessibility, distance	- “land use plans […] effectively reduced deforestation over counterfactual scenarios, at least in some time periods. These restrictions were effective immediately,

	30,129 parcels	stricter zones vs. similar properties in less strict zones before vs. after the FL	to water, nearby deforestation, forest cover, property size); DID to control for time-invariant unobservable confounders	with measurable impacts within years after the approval of the land use plans" - "provinces with high historical deforestation rates can effectively reduce forest loss if prompted to do so"
Volante & Seghezzo 2018	Salta, Santiago del Estero; no further subdivision	- Differences in forest loss in stricter vs. less strict zones before and after the FL - Compliance	- BACI (before vs. after, control vs. intervention); not controlling for confounders - Compliance with national goal of achieving zero deforestation in red/yellow zones	- "anything more than zero deforestation in yellow and red zones can be interpreted as a failure of provincial governments to enforce this law" - "provincial governments were apparently unable to adequately enforce the mandate of the Forest Law" - "declining trends in the Argentinian Chaco cannot be directly attributed to law enforcement" - "provincial governments were […] unable to […] enforce the Forest Law since deforestation in protected zones continued or even increased […]"

* estimated: no number is provided in original article, but authors use the same cadaster dataset that we use