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The Net Effect of Concessions on Forest Loss

Quasi-Experimental Evidence from Mexico

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The Net Effect of Concessions on Forest Loss:
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Abstract: Rapid deforestation remains a pressing problem in much of the global South and has severe environmental and socioeconomic consequences. Policies aimed at addressing this problem have historically focused on 'land sparing': prohibiting some or all extractive activities in specified locations, such as protected areas. An alternative approach is 'land sharing': improving supervision and management of extractive activities. Timber extraction can be managed in ways that significantly reduce forest loss, by, for example, relying on selection logging instead of clearcutting and extending logging rotations. Probably more important, forests that are well managed for extraction may discourage illegal logging and land-use change. Hence, in principle, forest concessions can reduce as well as encourage forest loss, and their net effect is an empirical question. Limited rigorous evidence is available to measure net effects. We use remotely sensed forest loss panel data, detailed information on hundreds of forestry concessions, and quasi-experimental methods (matched difference-in-differences) to measure the net effect of concessions on forest loss in Mexico. Results from an initial analysis indicate that although we test for a variety of temporal and subgroup effects, we are unable to reject the null hypothesis that concessions have no net effect on forest loss. It is important to emphasize that these results are preliminary and subject to revision.

Keywords: concession, permit, deforestation, land sharing
JEL codes: Q23, Q56, Q57

1. INTRODUCTION

Forests provide critically important local and global ecosystem services: they sustain livelihoods, regulate surface water flows, recharge aquifers, cycle nutrients, harbor biodiversity, and help mitigate climate change (Seymour and Busch 2016). As a result, persistently high rates of forest loss in the global South have severe environmental and socioeconomic consequences (Lewis et al. 2015; Harris et al. 2012; Gibson et al. 2011). Policies aimed at addressing deforestation have historically focused on "land sparing": prohibiting some or all extractive activities in specified locations, such as protected areas and land enrolled in payments for environmental services initiatives. An alternative approach is "land sharing": improving supervision and management of extractive activities. Supervision can be handled by the state through forestry concessions and by third parties through eco-certification and sustainability standards.

At first blush, the notion that improving the management of timber extraction could be an effective means of conserving forests may seem implausible. But in fact, timber in developing countries can be harvested in ways that significantly reduce forest loss, by, for example, relying on selection logging instead of clearcutting, carefully planning skid trails, and extending logging rotations (Griscom et al. 2017). Probably more important, forests that are well managed for extraction may discourage illegal logging and land-use change, which are major drivers of forest loss in developing countries. Hence, forest concessions can reduce forest loss as well as encourage it, and their net effect is an empirical question. Although rigorous evidence on the effects of all types of forest conservation policies is limited, we know the most about land-sparing approaches, particularly protected areas (Miteva et al. 2012; Seymour and Busch 2016). The evidence on land-sharing policies, by contrast, is much thinner: we know relatively little about the net effects of forestry concessions on forest loss.

This paper aims to help fill that gap. We use quasi-experimental methods to identify the effect of forestry permits (concessions) on forest loss in Mexico. We use Hansen et al. (2013) fine-scale annual data to measure forest loss (our outcome), official records of hundreds of forestry permits (our treatment), and a rich set of spatial data to control for potential confounders, including socioeconomic factors (crop and meat prices, population density, tenure, opportunity costs, and protection status), climatological factors (temperature and rainfall), and geophysical factors (slope, elevation, directional orientation, historical forest loss, distance to population centers, and above-ground biomass). Our identification strategy combines matching with generalized difference-in-differences models (Imbens and Wooldridge 2009; Ho et al. 2007). We use propensity score matching to identify forest management units (FMUs) lacking permits that are similar to FMUs with permits in terms of characteristics that affect forest loss (e.g., slope, elevation, distance to cities). Then we fit difference-in-differences panel data models that generate estimates of average treatment effect on the treated (ATTs) by, in effect, comparing annual changes in forest loss on FMUs with permits and matched FMUs without them. Results from an initial analysis indicate that although we test for a variety of temporal and subgroup effects, we are unable to reject the null hypothesis that concessions have no net effect on forest loss. It is important to emphasize that these results are preliminary and subject to revision.

The remainder of the paper is organized as follows. The next section presents background on forests and forestry permits in Mexico. Section 3 describes our empirical approach, and Section 4 discusses our data, sample, and variables. Section 5 presents an analysis of the primary assumption unpinning our identification strategy. Section 6 presents our results. And the last section sums up and concludes.

2. BACKGROUND

Mexico's forests, more than half of which are relatively undisturbed old-growth, extend 65 million hectares, one-third of the national territory (FAO 2011). Fifty-five to 80 percent of the forest area is managed by several thousand communal FMUs, a legacy of the agrarian reform that accompanied the Mexican revolution (FAO 2011; Madrid et al. 2010; Bray and Merino Perez 2002). The two principal types of communal FMUs are *comunidades,* which are indigenous communities with historical ties to land, and *ejidos*, which are groups of peasants granted land through the reform process. Most of these communal FMUs, particularly the smaller ones, lack the technical resources often used in sustainable forest management (Anta Fonseca 2006).

Historically, deforestation and forest degradation have been severe problems in Mexico. Between 1990 and 2000, clearing of all types of forests averaged more than one-half of 1 percent per year and caused the seventh-highest net annual forest loss of any country in the world (FAO 2011). During the same period, clearing of primary forests averaged more than 1 percent per year (FAO 2011). Deforestation and forest degradation have contributed to a host of local and global environmental problems, including soil erosion, aquifer depletion, diminished biodiversity, and global warming (Cervigni and Brizzi 2001). Although deforestation at the national level has slowed significantly since 2000, rapid forest cover loss continues to plague some regions (Madrid et al. 2010).

As in many countries, Mexico's system of forest regulation emphasizes permits and management plans. To extract timber, FMUs, including *comunidades* and *ejidos*, are required to obtain permits from state offices of the National Environment Ministry (Secretaría de Medio Ambiente y Recursos Naturales, SEMARNAT). That, in turn, requires that they develop forest management plans, typically with the assistance of a consulting forester. Among other things, permits specify the amount, type, and location of trees extracted each year and the silvicultural system used to do so. State offices of the National Environmental Attorney General (Procuraduía Federal de Protección Ambiente, PROFEPA) have responsibility for monitoring compliance with SEMARNAT permits. However, during our 2001–2012 study period, particularly the early years, funding and manpower allocated to that task were insufficient (OECD 2003). Partly as a result, illegal logging and land-use and land-cover change, including that driven by criminal syndicates, were significant problems (Reforestamos Mexico 2012; Vidal et al. 2013).

3. EMPIRICAL APPROACH

The principal challenge to identifying the effect of forestry permits on deforestation is the usual one in impact evaluation: controlling for observed and unobserved confounding factors. For example, as we shall see, in our sample, compared with never-permitted FMUs (FMUs that did not have a

permit at any point in our 2001–2012 study period), ever-permitted FMUs (FMUs that did) tended to have lower opportunity costs of conserving forests and to be outside protected areas—observed characteristics one would expect to be positively correlated with deforestation. Probably more important, permitted FMUs likely have unobserved features that affect deforestation, including relatively high levels of management skill. Failure to control for such observed and unobserved confounding factors risks conflating the causal effects of permits with the effects of FMUs' preexisting characteristics. To control for such endogeneity, we use FMU-level 2001–2012 panel data along with a combination of fixed effects—that is, generalized difference-in-differences—and propensity score matching.

Our two-way fixed effects model is specified as

$$Y_{it} = \gamma_i + \delta_t + D'_{it-z}\beta_1 + X'_{it-z}\beta_2 + \varepsilon_{it} \qquad (1)$$

where i indexes FMUs, t indexes years, z indexes temporal lags, Y is the percentage of the FMU deforested, γ are FMU fixed effects, δ are year fixed effects, D is a vector of contemporaneous and lagged dichotomous dummy variables indicating that a permit was in force, X is a vector of time-varying control variables, β are parameters or vectors of parameters to be estimated, and ε is an error term. The parameters in β_1 measure permits' net effect on forest cover change—formally, the average treatment effect on the treated (ATT). The FMU-fixed effects control for unobserved time-invariant FMU heterogeneity. The year fixed effects control for unobserved temporal effects that affect all FMUs in the study area, including changes in forest policy and international prices of timber. We omit time-invariant control variables because they are perfectly correlated with the FMU-fixed effects. We estimate Equation (1) using ordinary least squares (OLS) and cluster standard errors at the FMU level.

Although our fixed effects model should control for unobserved time-invariant heterogeneity that affects the levels of deforestation, it would not control for unobserved heterogeneity that affects trends in deforestation. Therefore, the primary identifying assumption is that absent concessions, these trends would be the same for both the treatment and the control groups. Section 5 presents a test of this assumption using prepermit forest loss data.

In addition to fixed effects, we control for confounders by using matching to "preprocess" our data (Imbens and Wooldridge 2009; Ho et al. 2007). That is, we identify a matched control group of never-permitted FMUs that are similar to the treatment group of ever-permitted FMUs in terms of observed characteristics that may explain forest loss. We then drop unmatched control FMUs from the regression sample and use OLS to estimate Equation (1). Combining nonparametric matching with standard parametric regression typically generates treatment effects estimates that are more robust to misspecification and omitted variables bias than does parametric regression alone (Imbens and Wooldridge 2009; Ho et al. 2007; Ferraro and Miranda 2017).

We use propensity scores for each FMU—the probability of obtaining a permit predicted by a probit regression—to match ever-permitted and never-permitted FMUs (Rosenbaum and Rubin 1983). Propensity scores can be interpreted as weighted indices of the characteristics that explain treatment—here, permits. We implement propensity score matching as follows. First, we use a cross-sectional probit model to estimate propensity scores for each FMU. The model is specified as

$$\Pr(D_{ij} = 1 | W_{ij}) = F(W'_{ij}\psi_j) \qquad (j = 1, 2, \ldots 5) \qquad (2)$$

where j is an index of the five regions of Mexico (defined below), D is a binary variable indicating whether an FMU obtained a permit in any year from 2001 to 2012, F is the standard normal cumulative distribution function, and ψ is a vector of regression coefficients. Next, we create control groups of never-permitted FMUs by matching ever-permitted FMUs with never-permitted FMUs on the basis of propensity scores. We match ever-permitted FMUs to never-permitted FMUs in the same region using nearest-neighbor 1-to-1 matching without replacement (Cochrane and Rubin 1973). Finally, we drop all unmatched control FMUs and then estimate Equation (1). Again, we cluster standard errors at the FMU-level.

4. DATA

4.1. Sources

Our data are drawn from four sources (Appendix Figure A1). The first is cadastral data (ownership boundaries) for most of Mexico, comprising more than 640,000 private, communal, and state property polygons (RAN n.d.). The second is annual 2000–2012 forest loss data derived from high-resolution (30m×30m) Landsat satellite images for all of Mexico (Hansen et al. 2013). The third is a compendium of 9,837 forest permits issued by SEMARNAT state offices for the 16 federal entities with significant forest area (INECC 2013).[1] Consisting of information on silvicultural practices, harvest areas, and permitted harvest volumes, these data were compiled by digitizing original paper copies of permits on file in SEMARNAT state offices. The final source is a variety of data sets used to construct the FMU-level control variables described below.

Although permits sometimes apply only to that part of an FMU designated as a forest harvest zone, our analysis is at the FMU level. The main reason is that we need to compare outcomes for FMUs with and without permits, and those without permits do not have designated harvest zones. An advantage of using FMUs as spatial units of analysis is that we control for spatial spillover effects that occur when forest management permits on one part of an FMU spur deforestation on other parts.

4.2. Sample

We limit the sample to FMUs in the 16 federal entities that have significant forest cover—the entities for which we compiled permit data (Figure 1). In addition, we include only FMUs with communal tenure—that is, *ejidos* and *comunidades*. The reason is that our cadastral data do not include the names of private properties, information needed to associate FMUs with permits. As noted above, estimates of the share of Mexico's forestland that is managed by communal FMUs range from 55 to 80 percent.

[1] The 16 entities are Campeche, Chiapas, Chihuahua, Distrito Federal, Durango, Estado de México, Guerrero, Jalisco, Michoacán, Morelos, Oaxaca, Puebla, Queretaro, Quintana Roo, Veracruz, and Yucatan.

We used our permit database to determine which communal FMUs in our 16-state study area were treated—that is, had permits during our 2001–2012 study period. We manually associated permits with cadastral polygons, using FMU names and locations (state and *municipio*). Of the 2,246 communal FMUs in our study area that are represented in our permit database, we successfully matched 1,360 (60 percent) to cadastral polygons. We dropped 226 FMUs for which matches were inconclusive.

We also dropped 580 treated FMUs that were issued permits prior to 2002. The purpose was to ensure that we had pretreatment outcome (forest loss) data for all treated units in the regression sample, a feature that improves the reliability of treatment effect estimates in panel data models (Ferraro and Miranda 2017; Cook et al. 2008). Finally, we dropped the 12th and final year of our 2001–2012 panel on forest loss because we lacked data for two of our time-varying covariates (crop and meat prices) for that year.

At the end of the day, our regression sample comprised 15,849 FMUs, of which 771 are ever-permitted and 15,078 are never-permitted (Figure 1). From these cross-sectional data, we created a 12-year unbalanced panel spanning 2001 to 2011. The panel contained 174,293 FMU-years, 8,480 of which were treated (under permit) and 165,813 were untreated.

[Insert Figure 1 here]

4.3. Variables

Table 1 lists the variables in the regression analysis, including their names, definitions, units, sources, spatial scales, and years.

[Insert Table 1 here]

Our dependent variable, *percent cleared*, is the percentage of the total land area of the FMU cleared each year from 2001 to 2011. It is derived from fine-scale Landsat satellite images (Hansen et al. 2013).

Our four principal treatment variables—*permit all years*, *permit early*, *permit anticipatory*, and *permit year n*—are 0/1 dummy variables. As discussed below, most are used in separate models. Each variable aims to capture a slightly different temporal effect. *Permit all years* is equal to one if a permit was in effect in year t (the current year). It aims to capture the average annual effect of a permit on forest loss during all years that the permit was valid, including years when the original permit was renewed. By contrast, *permit early* is equal to one only during the first five years of the first permit period. Premised on the idea that the early permit period is most likely to affect forest loss, it aims to pick up the permit's average annual effect over the course of the first five years of the permit period. *Permit anticipatory* is equal to one if the initial permit was awarded in any of the two years after t. Premised on the idea that FMUs may change forest management and land use in anticipation of receiving a permit, it aims to pick up average annual anticipatory effects during the two years before the first permit is awarded. *Permit year n* is equal to one if the initial permit was awarded n years before or after t. Unlike the first three treatment variables, which reflect average effects over several years, these variables aim to capture single-year effects of the initial permit. For

example, *permit year –3* is equal to one if the initial permit was awarded three years before t and aims to capture only the effect of that event in that year. Finally, *permit expired* is equal to one in all years following the expiration of the last permit. It aims to control for any effects that permits may have after their expiration.

We use a rich set of time-varying and time-invariant covariates to control for confounding factors (along with FMU and year fixed effects). These covariates reflect various socioeconomic, climatological, and geophysical features of FMUs. We include the time-varying covariates in our fixed effects panel data regressions (Equation 1) and the time-invariant covariates in the cross-sectional probit regression used to generate propensity scores that, in turn, are used to match ever-permitted FMUs with never-permitted FMUs. We use six time-varying covariates: *crop price* (index of crop prices), *meat price* (index of meat prices), *population density*, *population indigenous* (the fraction of the population speaking an indigenous language), *temperature*, and *precipitation*. We use 11 time-invariant covariates: *ejido tenure* (identifies *ejidos* versus *comunidades*), *opportunity cost* (annual gross revenue from agriculture and ranching), *historical temperature* (50-year average), *historical rainfall* (50-year average), *protected area* (identifies FMUs that overlap with federal protected areas), *altitude*, *aspect* (directional orientation), *slope*, *size* (surface area of the FMU), *travel time to city*, *distance to clearing* (average distance to nearest cleared pixel in 2000), and *carbon* (total above-ground carbon stock).

4.4. Summary statistics

Among the 804 new permits issued during our 2002–2010 study period, we find substantial variability in the number of new permits issued across states and over time during our study period (Appendix Table A1). The states with most new permits were Durango (174), Chihuahua (94), Oaxaca (88), Guerrero (87), and Puebla (62). The total number of permits issued per year ranges from 81 to 110 for the first seven years of the panel and then falls to 55 and finally two for the last two years. The median length of a first permit is 10 years.

[Insert Table 2 here]

Table 2 presents variable means for all 174,293 FMU-years (representing15,849 FMUs) in our full regression sample and for subsamples of 8,480 permitted FMU-years (representing 771 ever-permitted FMUs) and 165,813 unpermitted FMU-years (reflecting 15,078 never-permitted FMUs). The table also presents, for each variable, both standardized bias (the variance normalized difference in means for these subsamples) and results from difference-in-means tests. We discuss summary statistics for the matched sample in Section 6.1 below.

For the full unmatched sample, the mean of *percent cleared* is 0.185, which indicates that on average, two-tenths of 1 percent of each FMU was cleared each year between 2002 and 2011. For an average-sized FMU in our unmatched sample (2706 hectares), that implies a forest loss of 5 hectares per year. The mean of *permit all years* is 0.027, which implies that 3 percent of the observations in our sample were treated in any given year.

The difference-in-means tests indicate statistically significant differences between the ever-permitted and never-permitted subsamples for every variable. Most notably, the means of the

outcome variable, percentage cleared, are significantly different: on average, the percentage of ever-permitted FMUs cleared (0.129) was slightly more than two-thirds that of never-permitted FMUs (0.187), hinting that permits may be associated with reductions in forest loss. However, this difference obviously does not control for differences in preexisting characteristics of FMUs that affect forest loss and therefore may be spurious. As just noted, these differences are significant. We discuss them below when we present the results of the probit model used to generate the propensity scores. Our empirical strategy aims to control for these observed characteristics, as well as unobserved time-invariant characteristics.

Fixed effects models like ours identify treatment effects by exploiting within-group (here, within-FMU) temporal variation and ignore between-group static variation. Therefore, to be estimable, they require significant within-group variation (Greene 2008). Appendix Table A2 presents statistics measuring overall, within-group, and between-group variation for our outcome and treatment variables: percentage cleared and *permit all years*. For both variables, within-group variability is in fact significant.

5. COMMON TRENDS ASSUMPTION

As noted above, the primary identifying assumption for a difference-in-differences model is that absent treatment, trends in the outcome would be the same for the treatment and control groups. We test whether this common trends assumption holds in two pretreatment periods (e.g., Alix-Garcia and Sims 2017). Because our annual forest cover change data (Hansen et al. 2013) begin in 2001, we use Mexican statistical agency fine-scale land-use maps for 1976, 1993, and 2000 (Velasquez et al. 2010) to calculate changes in the FMU-level percentage forest cover in the intervals between those years. Figure 2 summarizes average changes over the two intervals for the matched sample. Trends for treatment and control groups appear to be quite similar.

[Insert Figure 2 here]

To test that observation, we regress changes in percentage forest cover onto *permit ever*, an ever-certified treatment dummy variable. We fit separate models for the first pretreatment period (1976–1993), the second period (1993–2000), and both periods (1976–2000), using both the full sample (n = 15,849) and the matched sample (n = 1,542). In addition, we fit separate models with and without time-invariant control variables (Appendix Table A3).

In general, the results support the common trends assumption. In 10 of the 12 regression models, we are not able to discern a difference in the average change in outcomes for our treatment and control groups that is statistically significant at the 5 percent level or higher. Moreover, both models in which *permit ever* is statistically significant use the full (versus matched) sample; this variable is not significant when we use the matched sample.

6. RESULTS

6.1. Propensity-score regression

Results from the probit model used to generate propensity scores confirm what our summary statistics suggest: our treatment, forestry permitting, is not randomly assigned among FMUs in our regression sample. Most of our time-invariant control variables are statistically significant (Table 3). FMUs with permits tended to have lower opportunity costs of conserving forests; to be outside protected areas; and to be cooler, drier, lower, more steeply sloped, larger, farther from cities, closer to cleared areas, and more carbon dense. All of these characteristics are typically correlated with our outcome, forest loss (Busch and Ferretti Gallon 2017). Hence, it is important to control for them in estimating treatment effects.

[Insert Table 3 here]

Matching based on the propensity scores generated by Equation 2 significantly improves covariate balance. Before matching, covariate means for treatment and control subsamples are significantly different at the 5 percent level for all 12 time-invariant covariates (Table 2). The median standardized bias for these covariates is 28 percent. After matching, covariate means are significantly different at the 5 percent level for nine of the 12 time-invariant covariates. However, the mean standardized bias is just 5 percent. Note that our identification strategy does not depend on matching alone and therefore does not require that treatment and matched control subsamples have the same average characteristics. Rather, as discussed above, we use matching to "preprocess" the data with which we estimate our difference-in-differences models.

6.2. Main results

We estimate two-way fixed effects models for both the unmatched sample (Model 1) and the matched sample (Model 2) using the treatment variable *permit all years*. In effect, we test the hypothesis that on average for our entire 16-state study area, the acquisition of a permit affects the average rate of forest loss over the entire period during which the permit is valid. The results do not support that hypothesis: *permit all years* is not statistically significant in either model (Table 4).

[insert Table 4 here]

Since we are not able to discern a statistically significant effect of permits on deforestation, it is important to explore our regression models' statistical power. To facilitate that discussion, it is helpful to first consider how to interpret our treatment effect estimates (the point estimates of coefficients on *permit all year*) and how to gauge their economic significance. Given the definition of our outcome variable, these estimates indicate the average annual effect of a permit on the percentage of the FMU cleared. For example, a statistically significant effect of 0.100 would indicate that on average, a permit increases the percentage of the FMU cleared each year by one tenth of a percentage point.

Two statistics derived from our estimated treatment effects can help elucidate their economic significance. The first is the treatment effect divided by the counterfactual annual deforestation rate

(the average predicted outcome with all treatment variables set equal to zero), which for the unmatched sample is 0.185 (and is roughly equal to the average outcome in our regression sample; Table 2). To continue with the above example, a statistically significant coefficient of 0.100 normalized in this way would indicate that permits increase annual forest loss by 0.100/0.185 = 54 percent. The second statistic is the estimated treatment effect multiplied by the average size of the FMUs in our regression sample (2,706 ha for the unmatched sample and 7,660 ha for the matched sample), which yields the average number of hectares of forest lost or gained each year as a result of a permit. Continuing with the above example, an effect of 0.100 multiplied by that factor would indicate that on average, acquiring a forestry permit spurs an additional 271 to 766 hectares of forest loss per year.

To assess the power of our regressions, we compute minimum detectable effects (MDEs) for each of our models (Models 1 and 2, Table 4) and we interpret them using the two statistics described above. A MDE is the smallest true absolute value of the treatment effect that has at least an X percent chance of producing a statistically significant estimate, given the size and variability of the study sample (i.e., the smallest true absolute value of the treatment effect for which there is less than a 1–X percent chance of making a Type II error; Bloom 1995). It can be calculated as a simple multiple of the estimated standard error of the treatment effect. Following convention (Dong and Maynard 2013), we use X equal to 80 percent. In addition, we allow for a two-sided hypothesis test and a 5 percent significance level (equivalently, a one-sided test at the 2.5 percent significance level). The MDE for Model 1 (unmatched sample) is 0.0192 and that for Model 2 (matched sample) is 0.0234 (Table 4). MDEs expressed as percentages of the counterfactual deforestation rate range from 10 percent to 18 percent and imply changes in deforestation of 1 to 2 hectares per year for the average FMU.

Hence, our MDE estimates generally imply we can be confident that for our entire 16-state study area, the acquisition of a forestry permit does not cause average annual changes in forest loss greater than one-tenth to one-fifth of the baseline rate of deforestation, which for the average-sized FMU translates into changes greater than 1 to 2 hectares per year. Our models would be able to identify such effects 80 percent of the time.

6.3. Robustness checks

To check the robustness of our national-level results, we estimate additional models that search for effects specific to certain geographic areas, certain years of a permit's validity, or longer time horizons, and for effects that might be affected by temporal spillover. All of these robustness checks comport with the results reported above: on average, permits do not have a discernible effect on forest loss.

6.3.1. Regional-level results

In principle, forestry permits could have heterogeneous effects on forest loss that depend on geography. We test for such effects in five regional subsamples defined by aggregations of federal entities: Yucatan (Campeche, Yucatan, and Quintana Roo), South (Chiapas, Guerrero, Oaxaca, Puebla, and Veracruz); Central (Distrito Federal, Estado De Mexico, Morelos, and Queretaro), North (Chihuahua and Durango), and Pacific (Jalisco and Michoacán). For simplicity, we use

matched subsamples for each region. For the regional subsamples, we match ever-permitted FMUs to never-permitted FMUs in the same region.

We are not able to discern a statistically significant effect of permits on forest loss in any of the five regional subsamples (Table 5). For two of the three regions—Yucatan and South—the statistical power of our models is sufficient to rule out effects of modest size. In these regions, MDEs expressed as a percentage of the counterfactual rate of forest loss range from 22 to 27 percent, which for the average-sized FMUs in these regions translates into changes in forest loss of 2 to 30 hectares. For the remaining regional subsamples—Central, North, and Pacific—MDEs expressed as a percentage of the counterfactual rate of forest loss exceed 100%.

[insert Table 5 here]

6.3.2. Early effects

In principle, permits could have largest effects when first acquired because, for example, harvesting timber in a virgin forest requires constructing logging roads and landings. To test for such effects, we fit a model (Model 8) in which the treatment variable is *permit early,* a binary dummy variable equal to one in each of the first five years after a permit is first awarded. This model would shed light on the average annual effect of a permit during that period. This variable is not statistically significant (Table 6). We get the same result if *permit early* is defined to equal one only for the first two years after a permit is awarded (Model 9).

[insert Table 6 here]

6.3.3. Anticipatory effects

In principle, permits could have effects just before they are awarded because, for example, managers preemptively harvest (illegally) to avoid the monitoring that accompanies permits and/or because as noted above, harvesting in a virgin forest requires constructing logging roads and landings. To test for such effects, we fit a model (Model 10) in which the treatment variables are *permit all years* and *permit anticipatory*, a binary dummy variable equal to one in each of the first two years before the permit is first awarded. This model sheds light on the average annual effect of a permit in those two years. *Permit anticipatory* is not statistically significant (Table 6).

6.3.4. Single-year effects

Thus far, our models aim to identify average annual effects over a span of years, either the entire duration of the permit, including renewals *(permit all years)*, early years of the permit *(permit early)* or the two years before a first permit is awarded *(permit anticipatory)*. These models may miss effects that tend to occur only in certain years before or after the award of a permit. To test for such effects, we estimate a model with a vector of binary treatment variables indicating that a permit was awarded n years ago—that is, for *permit year n,* where n ranges from −8 to 8. Again, we find that none of the treatment effects are statistically significant (Figure 3).

[insert Figure 3 here]

6.3.5. Expired permits

In principle, our results could be biased if the effect of a permit persists even after a permit expires because, for example, management practices instituted while the permit was in force are retained after it expires. In that case, supposedly untreated FMU-years are actually treated, a form of temporal spillover. To control for such effects, we fit a model (Model 11) that drops all observations after permits expire. There are 411 such observations. Here, too, *permit all years* is not statistically significant (Table 6).

6.3.6. Longer-run effects

Our fixed effects models measure the effect of permits on annual changes in deforestation, which may be small even though the cumulative effects over multiple years may be larger. To test for such effects, we fit a model in which we aggregate single years into three-year epochs (Model 11). Hence, rather than average annual effects, these models measure three-year epochal effects. Again, *permit all years* is not statistically significant (Table 6).

7. DISCUSSION

We have used rich spatial panel data on forest loss, forestry permits, and dozens of potential confounding factors along with matched difference-in-differences models to measure the effect on forest loss of permits in Mexico. We tested for a variety of temporal and subgroup effects but were unable to reject the null hypothesis that forestry permits have no additional effect on forest loss. Calculations of MDEs indicate that for our national-level models, that finding is unlikely to be driven only by a lack of statistical power: our models have the power to identify effects on the order of one-tenth to one-fifth of the counterfactual deforestation rate. Hence, this initial analysis suggests that in Mexico, legally permitted forest extraction does not raise or lower rates of forest loss relative to what they would have been otherwise. However, it is important to emphasize once again, that these results are preliminary. That said, the broad tentative conclusion is that in Mexico, while land sharing may not prevent additional forest loss, neither does it spur such loss.

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Table 1. Variables (†time varying)

Variable	Description	Units	Source	Scale	Years
OUTCOME					
percent cleared†	Percentage FMU area cleared in year t	[0-100]	Hansen et al. (2013)	30m	2001–2012
TREATMENT					
permit all years†	Permit in effect in year t?	0/1	INECC	FMU	2001–2012
permit early†	Permit awarded ≤ 5 years before t?	0/1	INECC	FMU	2001–2012
permit anticipatory†	First permit awarded 1 or 2 year after t?	0/1	INECC	FMU	2001–2012
permit year n†	First permit awarded in year t–n?	0/1	INECC	FMU	2001–2012
permit expired†	Permit expired and not renewed in year t?	0/1	INECC	FMU	2001–2012
permit ever	Permit in any year?	0/1	INECC	FMU	2001–2012
CONTROLS					
Socioeconomic					
cop price†	Value weighted index of crop prices	pesos	SAGARPA	municipality	2001–2011
meat price†	Value weighted index of cow and sheep meat prices	pesos	SAGARPA	municipality	2001–2011
population density†	Mean persons per km^2	persons/km2	SAGARPA/INEGI	municipality	2001–2012
population indigenous†	Fraction pop. > 5 yrs. speaking indigenous language	[0-1]	INEGI	municipality	2001–2012
ejido tenure	*Ejido* (versus *comunidad*) tenure?	0/1	INECC	FMU	2001–2012
opportunity cost[a]	Annual gross revenue from agriculture and ranching	($ pesos/ha)	SAGARPA/INEGI	FMU/municipio	2010
protected area	In federal protected area?	0/1	WDPA	1:50000–1:1,000,000	1917–2010
Climatological					
temperature†	Mean temperature in year t	ºK	NASA (2001)	1 km	2001–2012
rainfall†	Mean annual rainfall in year t	mm	Huffman et al. (2012)	25 km	2001–2012
historical temperature	Average annual temperature	ºC*10	Hijmans et al. (2005)	30 arc-sec	1960–1990
historical rainfall	Average annual precipitation	mm	Hijmans et al. (2005)	30 arc-sec	1960–1990
Geophysical					
altitude	Mean altitude above sea level	m	Farr et al. (2007)	15m	2006
aspect	Mean directional orientation	º	Farr et al. (2007)	15m	2006
slope	Mean percentage slope	%	Farr et al. (2007)	15m	2006
size	FMU surface area	ha	RAN (undated)	FMU	undated
travel time to city	Mean travel time to nearest city pop. > 50K	km	Nelson (2008)	30 arc-sec	2000
distance clearing	Mean distance nearest cleared pixel in year 2000	m	Hansen et al. (2013)	30m	2000
carbon	Total above-ground carbon stock	tons/ha	Cartus et al. (2014)	30m	circa 2006

FMU = forest management unit; INECC = Instituto Nacional de Ecología y Cambio Climático; INEGI = Instituto Nacional de Estadística e Informática; SAGARPA = Secretaría de Agricultura, Ganadería, Desarrollo Rural, Pesca y Alimentación; WDPA = World Database on Protected Areas.

[a] Computed from SAGARPA and INEGI data on hectares devoted to crops and pasture (FMU level), average yields (muncipio level), and average crop prices (municipio level).

Table 2. Summary statistics

Variable	Unmatched Sample: Mean all	Unmatched Sample: Mean ever-permitted	Unmatched Sample: Mean never-permitted	Unmatched Sample: SB[a]	Unmatched Sample: t-test[b]	Matched Sample: Mean all	Matched Sample: Mean ever-permitted	Matched Sample: Mean never-permitted	Matched Sample: SB[a]	Matched Sample: t-test[b]
OUTCOME										
percent cleared†	0.185	0.129	0.187	-14.800	***	0.130	0.129	0.132	-0.500	
TREATMENT										
permit all years† (0/1)	0.049	1.000	0.000	-	-	0.500	1.000	0.000	-	-
CONTROLS										
Socioeconomic										
cop price† (pesos/ρ)	1.577	1.739	1.569	11.500	***	1.825	1.739	1.910	-11.600	***
meat price†(pesos/ρ)	1.643	1.680	1.641	2.300	**	1.655	1.680	1.630	2.900	*
population density† (pop/km2)	1.548	0.628	1.595	-24.100	***	0.721	0.628	0.815	-4.700	***
population indigenous† (%/Ψ)	0.128	0.150	0.126	10.700	***	0.164	0.150	0.178	-12.900	***
ejido tenure (0/1)	0.898	0.811	0.903	-26.500	***	0.793	0.811	0.774	10.500	***
opportunity cost[b] (pesos/ha/μ)	8.880	3.954	9.132	-71.900	***	4.050	3.954	4.145	-2.600	***
protected area (0/1)	0.070	0.067	0.070	-1.500		0.072	0.067	0.077	-4.500	**
Climatological										
temperature† (°K/δ)	1.512	1.494	1.513	-105.800	***	1.495	1.494	1.496	-12.000	***
rainfall† (mm/ρ)	1.299	1.140	1.307	-29.300	***	1.140	1.140	1.140	0.000	
historical temperature (°C/Ψ)	2.109	1.754	2.127	-83.200	***	1.763	1.754	1.772	-4.100	***
historical rainfall (mm/Ψ)	1.010	0.945	1.013	-13.900	***	0.937	0.945	0.930	2.900	**
Geophysical										
altitude (m/ρ)	1.118	1.674	1.090	71.600	***	1.673	1.674	1.673	0.100	
aspect (°/Ψ)	1.742	1.764	1.740	7.500	***	1.764	1.764	1.763	0.300	
slope (%/μ)	8.974	8.989	8.973	17.000	***	8.988	8.989	8.987	1.500	*
size (ha/ρ)	2.706	8.187	2.426	43.900	***	7.660	8.187	7.133	8.000	***
travel time to city (km/Ψ)	1.826	2.672	1.783	48.100	***	2.742	2.672	2.812	-7.600	***
distance clearing (m/Ψ)	1.054	2.176	0.996	33.200	***	2.304	2.176	2.433	-7.200	***
carbon (tons/μ)	1.480	2.482	1.428	116.700	***	2.434	2.482	2.386	10.700	***
MedBias				27.900					4.600	
Number of FMU-years	174293	8480	165813			16961	8481	8480		
Number of FMUs	15849	771	15078			1542	771	771		

a. Standardized bias (SB) is the absolute value of the difference in means of the ever-permitted and never-permitted subsamples as a percentage of the square root of the average variance of these subsamples.
b. Test of null hypothesis that ever-certified and never-certified subsamples have equal means.

***, **, * = significant at 1, 5, 10% level; μ=10, Ψ=100, ρ=1,000, δ=10,000

Table 3. FMU-level cross-sectional propensity score probit results; dependent variable is SEMARNAT permit in any year 2002–2011; marginal effects [s.e.]

Variable	Marginal effect
Socioeconomic	
ejido tenure	-0.0040
	[0.0028]
opportunity cost	-0.0014***
	[0.0002]
protected area	-0.0271***
	[0.0046]
Climatological	
historical temperature	-0.0649***
	[0.0066]
historical rainfall	-0.0099***
	[0.0026]
Geophysical	
altitude	-0.0228***
	[0.0035]
aspect	0.0039
	[0.0029]
slope	0.0669**
	[0.0331]
size	0.0006***
	[0.0001]
travel time to city	0.0030***
	[0.0005]
distance clearing	-0.0007**
	[0.0003]
carbon	0.0236***
	[0.0016]
Observations	15,849

Log likelihood	-2357.3486
Pseudo R2	0.2353

***, **, * = significant at 1, 5, 10% level.

Table 4. FMU-level panel data two-way fixed effect regression results; dependent variable is percentage FM cleared in year t = 2001–2011; treatment is SEMARNAT permit [s.e.]

	Model	**1**	**2**
	Sample	**Unmatched**	**Matched**
permit all years		-0.0001	0.0036
		[0.0069]	[0.0083]
Counterfactual[a]		0.1847***	0.1295***
		[0.0002]	[0.0023]
MDE		0.0192	0.0234
MDE/Counterfactual		0.1041	0.1806
Observations		174,293	16,961
R-squared		0.007	0.011
Number of FMUs		15,849	1,542

All models include FMU-fixed effects, year-fixed effects, and standard errors clustered at the FMU-level. Models with matched control observations use probability weights.
MDE = minimum detectable effect (Bloom 1995)
[a]Predicted outcome with all treatment variables is set equal to zero, computed using delta-method.
***, **, * = significant at 1, 5, 10% level.

Table 5. FMU-level panel data two-way fixed effect regression results for regional subsamples; matched samples only; dependent variable is percentage FMU cleared in year t = 2001–2011; treatment is SEMARNAT permit [s.e.]

Model	**3**	**4**	**5**	**6**	**7**
Region	**Yucatan**	**South**	**Central**	**North**	**Pacific**
permit all years	-0.0149	0.0072	-0.0101	-0.0005	-0.0037
	[0.0626]	[0.0125]	[0.0124]	[0.0072]	[0.0285]
Counterfactual	0.6526***	0.1595***	0.0145***	0.0183***	0.0653***
	[0.0190]	[0.0034]	[0.0028]	[0.0020]	[0.0086]
MDE	0.1751	0.0349	0.0346	0.0201	0.0798
MDE/Counterfactual	0.2684	0.2186	2.3828	1.0987	1.2224
Average annual forest loss	0.6458	0.1577	0.0090	0.0185	0.0595
Observations	1,254	6,885	638	5,324	2,860
R-squared	0.152	0.016	0.030	0.009	0.011
Number of FMUs	114	626	58	484	260

All models include FMU-fixed effects, year-fixed effects, and standard errors clustered at the FMU-level. Models with matched control observations use probability weights.
Regions: Yucatan (Campeche, Yucatan, and Quintana Roo), South (Chiapas, Guerrero, Oaxaca, Puebla, and Veracruz); Central (Distrito Federal, Estado De Mexico, Morelos, and Queretaro), North (Chihuahua and Durango), and Pacific (Jalisco and Michoacán).
MDE = minimum detectable effect (Bloom 1995).
[a]Predicted outcome with all treatment variables is set equal to zero, computed using delta-method.
***, **, * = significant at 1, 5, 10% level.

Table 6. Robustness checks: FMU-level panel data two-way fixed effect regression results; matched samples only; dependent variable is percentage FMU cleared in year t = 2001–2011; treatment is SEMARNAT permit [s.e.]

Model	**8**	**9**	**10**	**11**	**12**
Robustness check	**Early effects 5 years**	**Early effects 2 years**	**Anticipatory effects**	**Exclude expired permits**	**Longer-run effects**
permit early	0.0033	0.0001			
	[0.0087]	[0.0088]			
permit all years			0.0060	0.0044	-0.0146
			[0.0129]	[0.0088]	[0.0248]
permit anticipatory			0.0047		
			[0.0158]		
Observations	15,439	13,341	16,961	16,550	6,168
R-squared	0.011	0.010	0.011	0.012	0.029
Number of FMUs	1,542	1,542	1,542	1,542	1,542

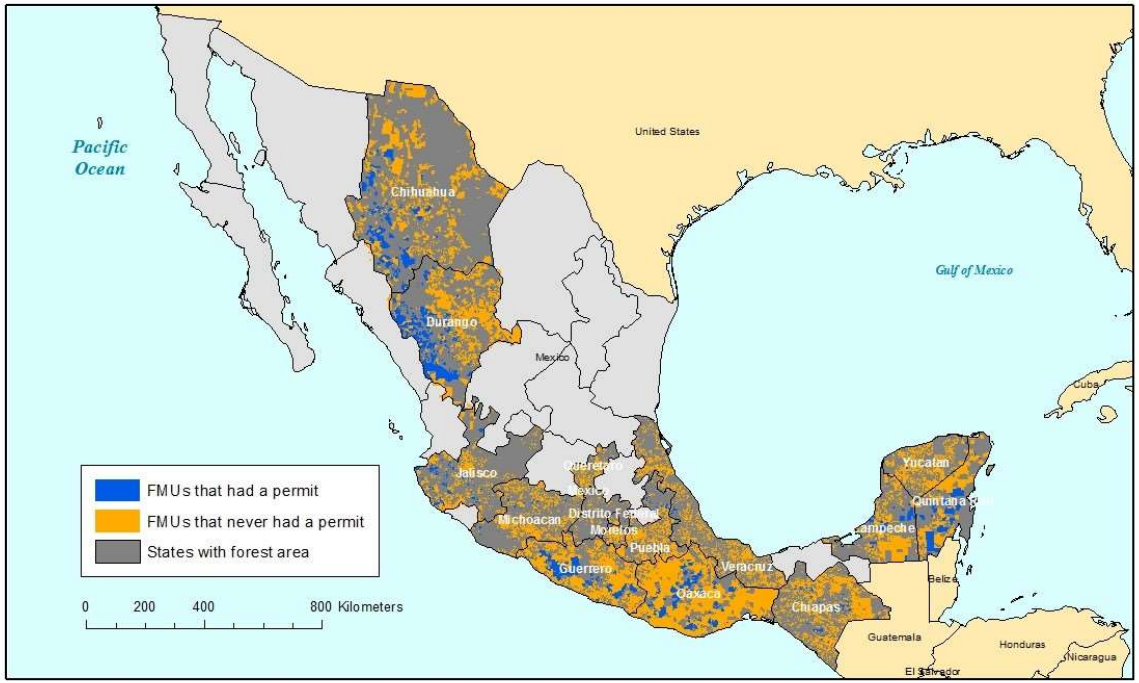

Figure 1. Study area and regression sample
(FMU = forest management unit)

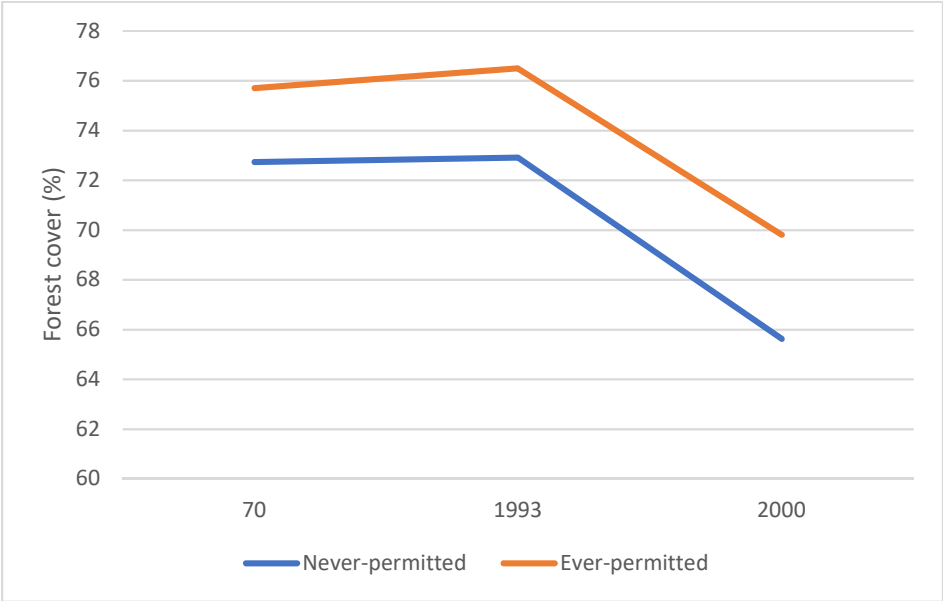

Figure 2. Pretreatment trends in forest cover for treated and control units are similar: average 1976, 1993, and 2000 percentage forest cover for ever-permitted subsample (blue) and matched never-permitted subsample (orange).

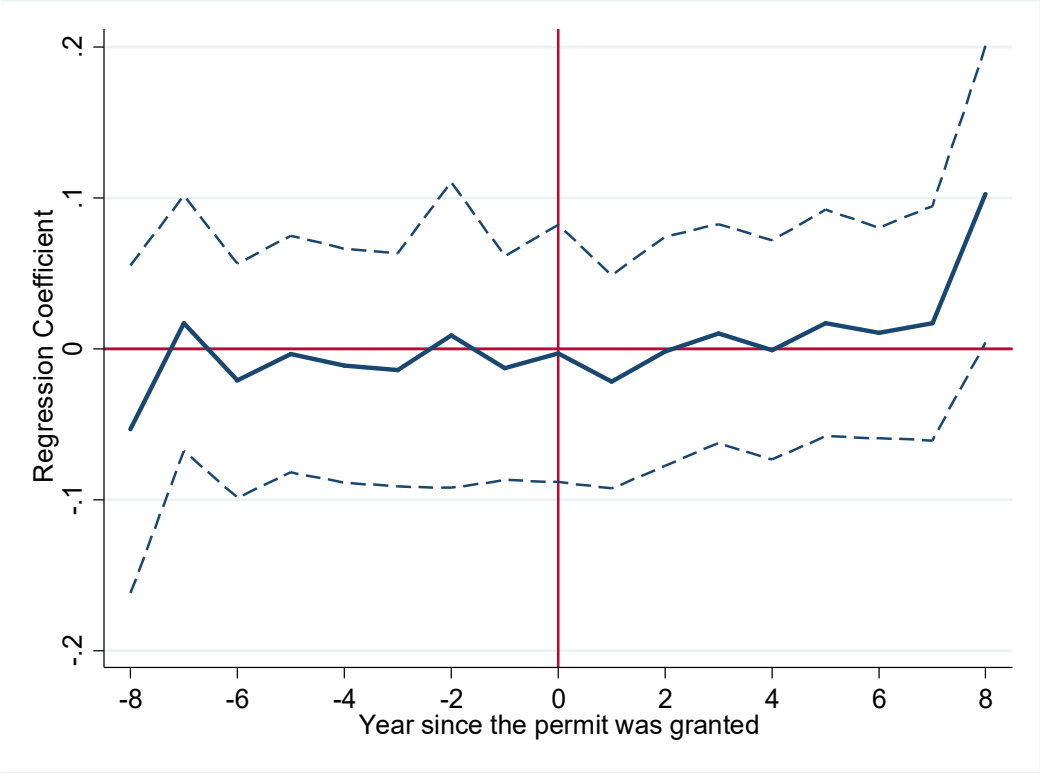

Figure 3. Estimated coefficients for single-year lagged permit dummy variables: FMU-level panel data two-way fixed effect regression; dependent variable is percentage FM cleared in year t = 2001–2011; treatment is SEMARNAT permit; matched control FMUs

APPENDIX

Table A1. New permits issued, by state and year

	2002	2003	2004	2005	2006	2007	2008	2009	2010	*Total*	Percentage
Campeche	3	4	1	7	2	1	1	3	0	22	3
Chiapas	3	2	2	3	14	14	17	4	0	59	7
Chihuahua	17	5	12	15	15	13	16	2	0	95	12
Distrito Federal	1	0	1	0	0	1	0	0	0	3	0
Durango	15	21	17	15	17	45	23	21	0	174	22
Guerrero	9	10	3	10	16	6	11	2	0	67	8
Jalisco	18	4	5	10	16	10	5	3	0	71	9
Estado De Mexico	1	0	0	1	1	3	10	0	0	16	2
Michoacán	3	13	8	11	6	7	6	5	0	59	7
Morelos	2	0	0	3	0	2	3	0	0	10	1
Oaxaca	11	20	12	11	10	10	9	5	0	88	11
Puebla	17	8	10	6	4	7	5	5	0	62	8
Queretaro	0	0	0	0	1	0	0	0	0	1	0
Quintana Roo	3	13	3	6	4	0	0	2	2	33	4
Veracruz	5	10	7	5	9	0	3	3	0	42	5
Yucatan	0	0	0	0	2	0	0	0	0	2	0
Total	108	110	81	103	117	119	109	55	2	804	100
%	13	14	10	13	15	15	14	7	0	100	

Table A2. Overall, within-group, and between-group variation for outcome and treatment variables (unmatched sample)

Variable	Variation	Mean	s.d.	n
percentage cleared	overall	0.1847	0.5826	174,293
	between		0.3646	15,849
	within		0.4544	
		0.0272	0.1628	174,293
permit all years	overall		0.1301	15,849
	between		0.0979	
	within	0.1847	0.5826	174,293

Overall, between, and within variations are the variances of $(x_{it} - \bar{x})$, $(x_i - \bar{x})$, and $(x_{it} - \bar{x}_i + \bar{x})$, respectively, where $\bar{x}$ is the grand mean.

Table A3. Testing the common trends assumption, FMU-level regression results; dependent variable is change in percentage FMU forested 1976–2000; treatment is indicator variable = 1 if ever-permitted (s.e.)

Model	**A1A**	**A1B**	**A1C**	**A1D**	**A2A**	**A2B**	**A2C**	**A2D**	**A3A**	**A3B**	**A3C**	**A3D**
Period	**'76–'93**	**'76–'93**	**'76–'93**	**'76–'93**	**'93–'00**	**'93–'00**	**'93–'00**	**'93–'00**	**'76–'00**	**'76–'00**	**'76–'00**	**'76–'00**
Control obs.	**all**	**all**	**matched**	**matched**	**all**	**all**	**matched**	**matched**	**all**	**all**	**matched**	**matched**
permit ever	-2.7375***	0.3060	-0.6096	-0.5728	0.0949	-1.4767*	-0.6052	-0.8082	-2.6426***	-1.1707	-1.2148	-1.3810*
	[0.7934]	[0.8339]	[0.8134]	[0.8088]	[0.7551]	[0.7666]	[0.8232]	[0.8034]	[0.8332]	[0.8591]	[0.8498]	[0.8323]
Control variables?	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
FMUs	15,849	15,849	1,542	1,542	15,849	15,849	1,542	1,542	15,849	15,849	1,542	1,542
R^2	0.001	0.028	0.000	0.032	0.000	0.092	0.000	0.068	0.001	0.064	0.001	0.062

***, **, * = significant at 1, 5, 10% level

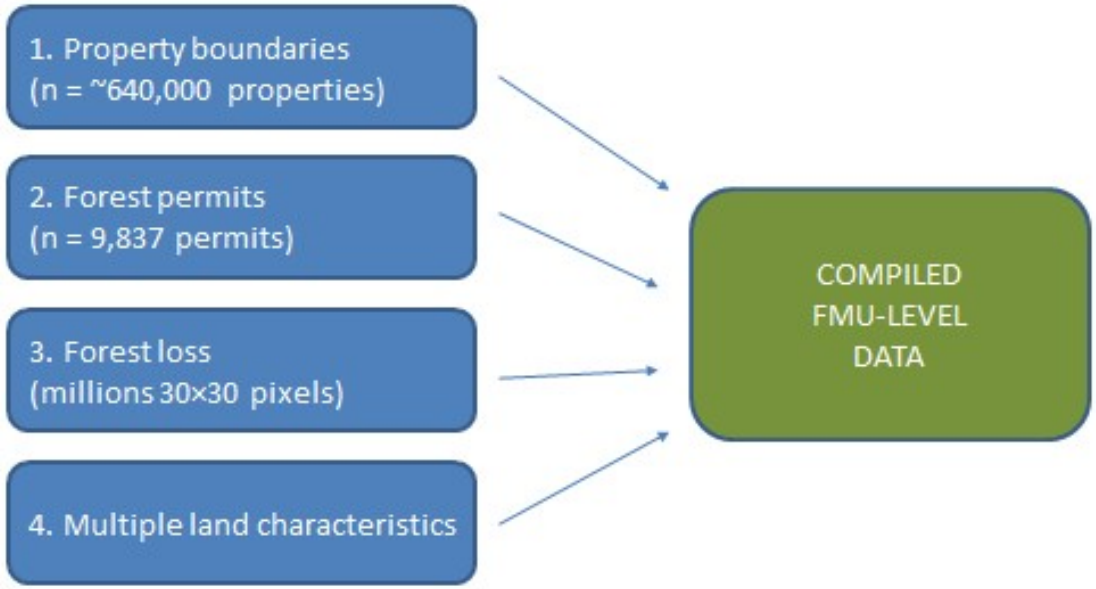

Figure A1. Data assembly