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Essays on forest conservation policies, weather and school attendance

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J'ai dit rapidement,
en mêlant un peu les mots
et en me rendant compte de mon ridicule,
que c'était à cause du soleil.
Il y a eu des rires dans la salle.

L'étranger, Albert Camus

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Introduction

One of the most pressing environmental challenges for the 21st Century is the loss of biodiversity and land degradation (UNEP, 2012). Forests cover approximately 30% of the total land area in the World (FAO, 2016) and provide environmental services essential for climate regulation, water and raw materials provision, soil quality, biodiversity, and cultural value.

Even though the annual rate of deforestation has slowed in the last 15 years, still 3.3 million hectares of forest disappear every year (FAO, 2016), an area approximately equivalent to the size of Belgium. Drivers of deforestation are diverse and include both natural causes and land use change for economic activities. Land use change has a direct effect on climate change, as forests play a critical role absorbing carbon dioxide.

In recognition of the importance of the environmental services provided by the forests, countries have implemented several policies aimed at protecting the natural resources within their borders. The first two chapters of this thesis are devoted to studying the effectiveness, and unintended or side effects of two very popular forest conservation policies.

In particular, the traditional and still leading policy is to set aside area for conservation purposes. At present, as much as 12% of the terrestrial area of the World is under protection according to the World Database on Protected Areas (2012). For such a non-negligible investment of resources, it is of particular interest to understand how effective is this policy in stopping deforestation and forest degradation, and how to improve its design and implementation.

The first chapter of my dissertation Heterogeneous Local Spillovers from Protected Areas in Costa Rica (with Juan Robalino & Alexander Pfaff) offers a contribution to the literature estimating the impact of protected areas (PAs) on preventing tropical deforestation. It extends previous work by looking at how the establishment of national parks affects land use change in the neighboring private land. This is a relevant question as most analyses to date examine the realized deforestation impacts of PAs only within their borders, generally finding reduced deforestation effects. However, spillovers can significantly reduce or enhance net effects of land-use policies.

Using data from Costa Rica, we confirm previous results finding zero spillover effects on average. Further, we extend the analysis by showing that protected areas can increase deforestation in adjacent private land when the returns to agriculture are high, but we also show that alternative activities, such as tourism, can block these leakage effects. This result is relevant in terms of policy as it offers a guideline of where complementary conservation efforts might be most effective. For example, stronger monitoring and enforcement, or additional incentives such as payments for environmental services could neutralize the negative spillover of protected areas on private land.

A second policy for forest protection that is becoming increasingly popular is forest certification. Certification is a voluntary policy instrument intended to solve a market failure posed by information asymmetries. The idea is that producers who meet stringent environmental standards or rules (set by an external party) are allowed to label their products in the marketplace and potentially achieve greater market access and receive higher prices for their products. As consumers rely on the signal provided by the labels, it is relevant to assess whether certification is associated with more sustainable forest management. But because this is a voluntary program, it is difficult to quantify the effects of this policy due to selection bias. Indeed, the evidence offered by the literature is inconclusive.

The second chapter Has forest certification reduced forest degradation in Sweden? (with Anna Nordén & Jessica Coria) estimates the effects of the two major forest certification schemes in Sweden, FSC and PEFC, on environmental outcomes during the forest management for non-industrial forest owners. Forestry is a key economic activity in the Swedish economy, as 47% of its territory is covered by productive forests, and Sweden holds a top place within World leading exporting countries in the forest industry. At the same time, approximately half of its productive forest is certified and it has the largest total area of certified forest in Western Europe (UNECE/FAO, 2012).

The contribution of this paper is to estimate whether certification leads to a more sustainable forest management. We focus on individual forest owners, who hold 50% of the total forest area in Sweden. Furthermore, we look at the key environmental components of the certification standard: the preservation of high conservation value areas, the facilitation of regeneration conditions, and setting aside area exclusively for conservation purposes. We compare how certified forest owners achieve these outcomes in relation to comparable un-certified forest owners.

Our findings indicate that certification has not improved any of the three evaluated environmental outcomes. Furthermore, we find no differences between the FSC and PEFC schemes. Our findings suggest that for forest certification to have an effect, the standards should be tightened and the monitoring and enforcement of forest certification schemes strengthened.

These two chapters share not only the intention to contribute to the public debate about what works and how conservation policies could be improved, but also they have in common the empirical methods utilized to tackle the research questions. Both papers evaluate a policy ex-post, and hence rely on quasi-experimental methods for impact evaluation. In both cases, in order to quantify the effect of the policy on the outcome of interest, we compare observations that have been exposed to the treatment with an unexposed (but otherwise similar) group of observations. The identification assumption is that the observed characteristics included in the analysis as control variables successfully help us to rule out rival explanations. We rely on previous literature to identify relevant confounders and discuss the plausibility of this assumption.

A second major challenge for the society at present is adaptation to climate change. Recent empirical evidence indicates sizable economic losses of higher-than-normal temperatures (Burke et al., 2015). However, how individual decisions are affected by weather and how these mechanisms translate into aggregate economic loss is still largely unknown, and not accounted for in the models that estimate the social cost of carbon.

Children may be especially harmed by extreme temperatures because they are less likely to manage their own heat risk and may have fewer ways to avoid the heat (Zivin and Shrader, 2016). Indeed, recent evidence shows that cognitive abilities are compromised at higher temperatures (Wargocki and Wyon, 2007; Zivin et al., 2015; Park, 2016).

In the third chapter of my dissertation, Effects of weather on daily school attendance decisions and academic performance I look at how high-frequency weather variation affects school attendance, a key input for academic achievement. Part of what motivated me to start this project is the striking correlation between academic achievement and weather conditions in my home country Costa Rica. I illustrate this correlation in Figure 1, which shows a map with academic achievement for high schools, together with the maps of temperature and precipitation. It is evident how the schools with the lowest academic performance tend to be located in the warmest and more humid areas. Many factors could explain this pattern, including historical dynamics, infrastructure, and demographics. This paper attempts to isolate the effect of weather, as an external factor out of the control of the students, on school outcomes that affect their human capital formation.

To this end, I exploit the variation in hourly weather conditions to explain the variation in individual-lecture attendance decisions. Student attendance is at the core of any model explaining academic achievement, and the literature reports non-negligible absenteeism rates especially among pupils coming from underprivileged households (Ready, 2010). Even when factors such as socioeconomic background, regional differences, and students ability are important determinants of school attendance, this cross-sectional variation is less useful in explaining short term individual dynamics. Following each students attendance decisions throughout the academic year and matching this information with the weather conditions at the time and place of the absence is a novel approach that allows me to rule out these slow-changing factors.

I find that school attendance decreases with precipitation and with every additional degree for students exposed to temperatures higher than 26°C. A higher absenteeism at high temperatures is consistent with a heat stress mechanism, by which the students adjust their activities to avoid the heat. Furthermore, I show that higher absenteeism is associated with lower academic performance.

Together these results suggest that weather can have a direct and instantaneous effect on human capital formation. Relatively small adjustments such as climate control technologies and schedule design could increase attendance significantly. In addition, these results inform about classic issues of economic development and especially the role of geographic features in inuencing development paths. Given this relationship and a scenario of warmer and more extreme weather events, regional gaps in schooling outcomes might not close in the future.

Studying high-frequency attendance decisions is a relevant matter in itself. The education literature typically studies school enrolment decisions, partly because this information is what is usually available in household surveys (Orazem et al., 2004). Also, often schools merely report annual aggregated drop-out rates, despite the fact that teachers are commonly required to take attendance in every lecture. Complete and systematic attendance records are an essential input to properly monitor and understand the causes of high daily school absenteeism.

Better quality in the attendance information, together with additional information would allow a more comprehensive analysis of the mechanisms behind the main results found in this study. In particular, information at the household level, such as socioeconomic background and geographic location, as well as further school-level information would allow disentangling mechanisms such as the role of the infrastructure, school administrative capacity, and current schooling policies, such as cash or in kind transfers (food and transportation) as incentives for neutralizing the effects of weather on human capital formation.

Finally, a more extensive time framework would allow an analysis of what these short run weather effects could represent in terms of the costs of future climate change. Incorporating adaptation, intensification, and general equilibrium effects is crucial to develop a better understanding of the relationship between weather variation and human capital formation (Dell et al., 2014).

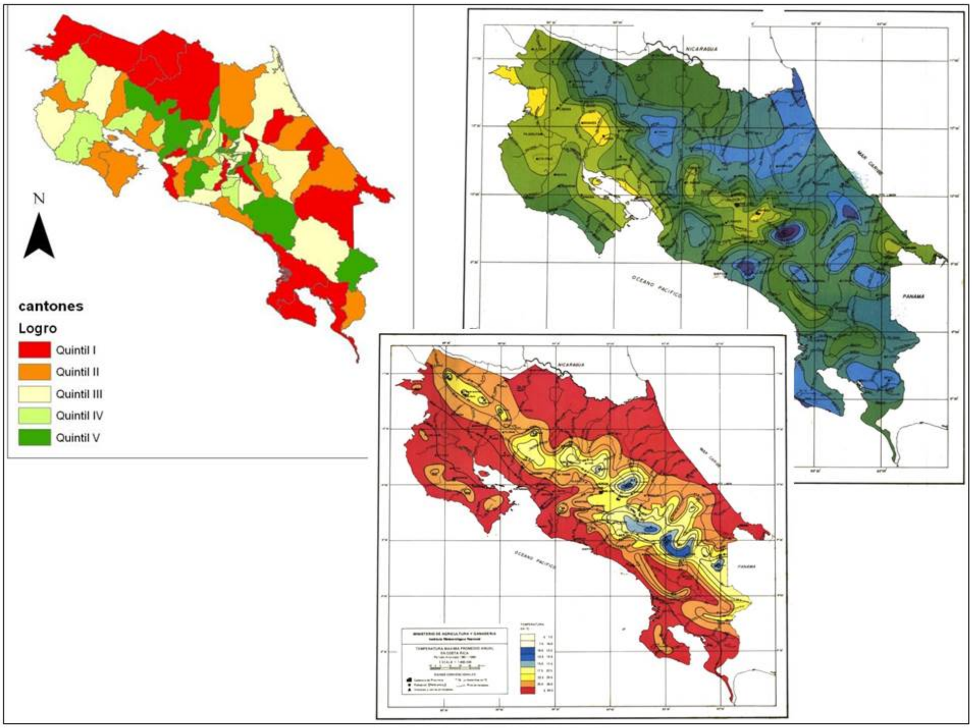

Figure 1. Top left: Map of school academic achievement by quintiles at county level for period 2005-2011. In red: group of counties in the lowest fifth of the academic achievement distribution, where on average 30% of the students passed the grade. In green: over 80% success rate (PEN, 2014). Top right: map of annual average precipitation for period 1961-1980. Blue represents more than 6500mm per year, and yellow less than 1500mm per year (IMN, 2017). Bottom: map of maximum annual temperature for period 1961-1980. Red represents higher than 30C, and blue 10-12.5C (IMN, 2017).

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Chapter I

Heterogeneous Local Spillovers from Protected Areas in Costa Rica*

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(Forthcoming in *Journal of the Association of Environmental and Resource Economists)*

Abstract

Spillovers can significantly reduce or enhance net effects of land-use policies, yet there exists little rigorous evidence concerning their magnitudes. We examine how Costa Rica's national parks affect forest clearing nearby. We find that average deforestation spillovers are not significant in 0-5km and 5-10km rings around parks. However, this average blends multiple effects that are significant and vary in magnitude across the landscape, yielding varied net impacts. We distinguish the locations with different net spillovers by their distances to roads and park entrances – both of which are of economic importance given critical local roles for transport costs and tourism. We find large and statistically significant leakage close to roads but far from the park entrances, which are areas with high agricultural returns and less influenced by tourism. We do not find leakage far from roads (lower agriculture returns) or close to park entrances (higher tourism returns). Finally, parks facing higher levels of deforestation threat show greater leakage.

Keywords: Protected Areas, National Parks, deforestation, conservation, spillover effects, impact evaluation, Costa Rica

JEL codes: Q23, Q24, Q28, Q57, O13

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1. Introduction

Protected areas (PAs) cover 12% of the earth's surface (WDPA 2012) and are the leading policy to reduce deforestation. Thus, an understanding of all their impacts on deforestation is important for future conservation policy (see Brunner et al. 2001, Andam et al. 2008, Sims 2010, Pfaff et al. 2009, Joppa and Pfaff 2010a, Blackman et al. 2015, Robalino et al. 2015). Most analyses to date examine the realized deforestation impacts of PAs only within their borders. However, it is well known that net forest impacts of PAs can depend significantly on PA impacts outside their borders[1].

There are numerous hypotheses about how protected areas might affect nearby rates of deforestation. Some argue that land-use restrictions could displace development to unprotected areas nearby (Wu 2000, Leathers and Harrington 2000, Wu 2005, Fraser and Waschik 2005, Armsworth et al. 2006, Robalino 2007, Alix-Garcia et al. 2012). Just the expectation alone of future expansion of land-use restrictions could lead landowners nearby to deforest, in order to lessen the chance of any such new restrictions (Newmark 1994, Fiallo and Jacobson 1995). These hypotheses suggest that PAs could increase the rates of deforestation in nearby areas. If the magnitude of such impacts in nearby areas were large, spillovers could fully offset deforestation reductions in PAs.

However, parks might instead decrease the deforestation in nearby areas. Protection could generate incentives for eco-tourism activities near parks, which increase the returns of forest conservation outside of, but near, PAs. In addition, some have argued that PAs increase environmental awareness (Scheldas and Pfeffer 2005). There is evidence showing that deforestation decisions of neighbors in Costa Rica reinforce each other, so that private land-use choices to conserve forest can shift incentives for nearby private land use towards additional forest conservation (Robalino and Pfaff 2012). Should PAs generate such spillovers to private land-use choice, then their conservation impacts may be underestimated.

Empirical estimates of local forest spillovers of parks could reflect combinations of such multiple land-use interactions. Further, the magnitude and sign of the net spillovers may vary across space generating heterogeneous spillover effects. Our hypothesis is that PAs increase net nearby deforestation in one location but lower it in another, and that both effects are hidden

[1] Spillovers generated by other environmental policies, such as a carbon tax, have been documented (Baylis et al. 2014).

behind an average zero effect. Indeed, a large enough single PA could even generate different net effects around its border.

We examine deforestation spillovers from Costa Rica's national parks from 1986 to 1997, the most recent time period during which deforestation rates in Costa Rica were significant. To go beyond prior empirical work (see Andam et al. 2008), we distinguish the forest locations that are near PAs by distances to the nearest road and nearest park entrance, both of which are economically important. We expect the existence and intensity of spatial spillovers to vary over space, given that the relevant economic mechanisms are likely to be affected by both transport costs and the proximity to tourism.

High-resolution spatial data for forest parcels allows controlling for parcel characteristics, which are important predictors of both park siting and deforestation patterns, according to recent literature on PA impacts. Parcels in protected areas differ significantly, on average, from forest left unprotected; Joppa and Pfaff (2009) show this globally. Thus, the forest parcels near PAs also are likely to differ, in relevant characteristics, from unprotected forest parcels to which they are compared in order to estimate spillovers[2].

We employ matching and regression methods to address the resulting potential biases. In environmental economics, matching strategies have been used for some time for evaluations concerning, e.g., effects of air quality regulations upon environmental outcomes (Greenstone 2004) and economic activities (List 2003). More recently, they have been applied to identify the causal effects of land-use restrictions and conservation policies on environmental and socioeconomic outcomes (see Andam et al. 2008, Joppa and Pfaff 2010b, Sims 2010, Arriagada et al. 2012, Ferraro et al. 2011, Alix-Garcia et al. 2012, Pfaff et al. 2009, Carnavire-Bacarreza and Hanauer 2013, Robalino and Pfaff 2012, Robalino and Villalobos 2015, and Robalino et al. 2015).

We start by estimating average impacts for all parcels near PAs. As in Andam et al. (2008), we find that, on average, there are no significant deforestation spillovers for the entire rings of forest immediately surrounding Costa Rica's national parks. However, we argue that this finding hides multiple heterogeneous spillover effects that result from differing influences of

[2] Governments pursue specific objectives when siting PAs. They might minimize conflicts with advocacy groups, or target impact (i.e., additionality) by choosing higher deforestation threats (as suggested in Pfaff and Sanchez 2004), or maximize environmental benefits conditional upon impact (Costello and Polasky 2004, e.g., extend a large literature).

transport costs and tourism. Thus, for further testing of varying spillover effects, we separate forests near protected areas using distances to roads, a factor associated with transport costs, and distances to entrances, a factor associated with tourism activities.

We find a 10% increase in deforestation close to roads, when far from the entrance within a 0-5km ring from the border of the PA. Areas within the inner ring, i.e. the closest forest, unaffected by tourism and with high agricultural returns seem to capture pressure emanating from inside the parks. In locations far from an entrance in the more distant 5-10km rings around the borders of PAs, we find no impact on deforestation rates.

Where tourism should have its greatest influence, e.g. locations close to park entrances in a 0-5km ring, we find no leakage at all, even close to roads. Yet, moving 5-10km out from the park entrance close to roads, we do find an 8.8% increase in the rate of deforestation. For this second ring, tourism may well not increase private forest returns and it is even possible that it raises returns to clearing for complementary development, such as hotels for those who pay to see forest at the park entrance.

We also test whether the deforestation pressure faced by a park affects leakage. For this, we look at a park's differing characteristics that are relevant for deforestation, and find different leakage levels. Leakage is higher when the opportunity cost is high in low tourism areas. Leakage is significant far from the entrance of flatter parks, which tend to be subject to high deforestation pressure. In parks with steep land, this is not the case. Smaller protected areas, that tend to be in high deforestation pressure areas, generate leakage. Yet, large protected areas, that tend to be in lower pressure areas, do not.

These results show not only the potential importance of spillovers in evaluating PA impact[3] but also the value of delineating specific mechanisms that are likely to underlie spillovers. These mechanisms help to predict the expected spillover effect for a given location. Looking only at impacts inside PAs can be misleading if PAs have positive or negative spillovers in nearby forests, as in Costa Rica. Such information is highly relevant in defining the optimal siting of PAs and other complementary policies.

In Section 2, we provide some background concerning both forest conservation in Costa Rica and the estimation of spillovers. We present a simple theoretical framework and a literature review of park leakage and spillover effects in Section 3. In Section 4, we present our data and

[3] Ando and Baylis (2014) have also noted this issue.

empirical approach. We present our results in Section 5 and, finally, we present our conclusions in Section 6.

2. Background

2.1. Deforestation in Costa Rica

While deforestation rates fell significantly by the end of the 1990s, during the late 1970s and early 1980s Costa Rica had one of the highest deforestation rates in the world (Sanchez-Azofeifa et al. 2001). For example, between 1976 and 1980 the deforestation rate was 3.2 percent per year (FAO 1990), but between 1986 and 1997, the deforestation rate outside protected areas was only about one percent per year (Pfaff et al. 2009). Multiple factors help to explain the observed drop in deforestation rates between time periods.

One set of factors concerns economics. For instance, beef prices fell while ecotourism activity rose. The profitability of other traditional Costa Rican agricultural products, such as coffee and bananas, also helps to determine where deforestation will occur. Profitability of these agricultural products is greatly affected by transport costs. Hence, roads are an important factor determining deforestation across landscapes, as confirmed empirically in other countries by Chomitz and Gray (1996), Pfaff (1999) and Pfaff et al. (2007). Naturally, another set of key factors involves state interventions. Lower deforestation might result from conservation efforts, including the implementation of PAs, with impact inside and outside their borders.

2.2. Conservation in Costa Rica

The area under protection in Costa Rica rose greatly between the 1950s and 1990s. Protected areas now cover 25% of the country. The largest category of protected areas is national parks, covering 10% of the country (Pfaff et al. 2009). One characteristic of national parks is that they receive visitors, which in turn generates related economic activities, such as rapidly increasing ecotourism. By 1995, tourism was the country's main source of foreign revenue (Inman et al. 1997) and a significant number of foreign visitors have ecotourism as their main objective, a goal which includes visiting parks.

The public decisions to establish PAs responded to multiple public and private objectives. For instance, the first conservation effort in Costa Rica took place in 1955 with a law that decreed as protected the entire area within 2km of the crater of any volcano. By 1977, with forest cover reduced to 31% of the territory, the National Park Authority (Servicio de Parques Nacionales) considered the establishment of new PAs an urgent matter. New protected areas were established in order to protect representative portions of all life-zones and all major ecosystems (Boza, 2015). To this end, the goal was to protect at least 5% of the territory.

The creation of many additional national parks was justified by this goal, yet the specific characteristics of each area differ (Boza 2015). For example, high recreational, cultural and historical value motivated the foundation of Santa Rosa National Park in 1971, while some of the biggest national parks were created to conserve geologic formations, flora and fauna, habitats and ecosystems, microclimates, life-zones, watersheds and aquifers (Rincón de la Vieja in 1973, Chirripó and Corcovado in 1975, and La Amistad in 1982). Other explicit motives for PAs include preventing the commercial and private exploitation of natural resources (Corcovado in 1975). Braulio Carrillo was created in 1978 to block expansion of agricultural and real estate activities following ongoing urban growth and the construction of a major road. Finally, some PAs were established to protect specific species, such as the coral reefs in Cahuita in 1970, the turtles in Tortuguero in 1975 or the birds in Palo Verde in 1982 (Boza 2015).

Given these explicit conservation goals, the state also needed to take into account the opportunity costs of PAs. As noted in Pfaff et al. (2009), these opportunity costs could guide protection away from development. In Costa Rica, PAs are located farther from San José, farther from national and local roads and on steeper lands compared to unprotected forests. They are also on lower productivity lands (Andam et al. 2008). These characteristics are associated both with high costs of transport and agricultural production.

Still, parks in Costa Rica have reduced deforestation significantly on average, even if variably so and on the whole by a smaller magnitude than many might assume (Andam et al. 2008). Around 2% of the forest inside protected areas would have been deforested between 1986 and 1997 without protection, though effectiveness of the protection depends on location, and hence on land characteristics (Pfaff et al. 2009). PAs close to roads, close to San Jose and on flatter land avoided significantly more deforestation (Pfaff et al. 2009). What remains undocumented to date is the impact of protection on neighboring forest.

3. Theoretical Framework & Prior Evidence

3.1. Simple Model of Park Leakage and Spillovers

Following Robalino (2007), we use a von Thünen framework to describe the effects of protection on deforestation in unprotected land. In Figure 1, all units of land are presented, in decreasing order, by relative profitability of clearing. The curve of relative profitability of clearing is denoted by R_a. As long as clearing profits are positive, i.e., in $[0, f]$, the land will be deforested. Forest will remain when returns are lower than 0, beyond f.

If a park is implemented in the interval $[0, p]$, we assume that deforestation cannot occur within that interval. This is an assumption justified by exceptionally low clearing within Costa Rica's PAs. If a park is stablished in an area where the agricultural profits are positive, as within this interval, agriculture production will be reduced and prices of agricultural goods will increase. This will lead to increases in rents in each location (Robalino 2007). This increase in agricultural rents is shown by the curve R'_a. Thus, deforestation will take place in the interval $[p, f']$. The interval $[f, f']$ would not have been deforested without the presence of the park. This is one form of "leakage."

On the other hand, if the presence of a park increases tourism activities, then the profits from keeping land forested rises to R_f. We assume that the park entrance is located at p. Returns from keeping the forest will decrease as the distance to the entrance of the park increases[4]. In this case, deforestation would not occur in some locations beyond f due to the increases in forest profits. Agricultural products will decrease once more and agricultural rents will increase again, R''_a. The result will be that deforestation will not occur in $[f, f'_t]$, from where the park is located to where forest returns equal agricultural returns. This reduction in deforestation would not have occurred had the park not brought tourism. Parks raising tourism activities create a "halo" in unprotected areas. In the figure, we also see that deforestation occurs in the interval $[f'_t, f'']$. Two effects of opposite signs are taking place. The net effect depends on the magnitude of the increase in forest profit, on the magnitude of the final increase in agricultural rents at f, and on the difference in the slopes between agricultural and forest returns.

[4] We assume forest returns fall faster than agricultural returns. If this is not the case, no leakage will take place close to entrance at any distance. This will be tested in the empirical section.

This simple model has four empirical implications. First, if there are no alternative activities that increase forest returns due to park implementation, increased deforestation outside PAs will occur. Protected areas without tourism generate deforestation outside. Second, the locations where such increases in deforestation will take place are the most profitable remaining land to deforest. Lands with low profits will remain unaffected. Third, if the park increases forest returns, the effects on deforestation outside the PA are ambiguous. Profits favor forest close to the entrance of the park, but also favor agriculture far from the entrance of the park. The sign of the overall effect depends on the magnitude of the increase of forest returns, on the magnitude of the increase in agricultural returns, and on how fast they decrease from the entrance of the park and from the market.

3.2. Previous Empirical Evidence of Deforestation Leakage

As noted above, various hypotheses exist for how parks might affect deforestation in nearby areas. They involve environmental awareness (Scheldas and Pfeffer 2005), displacement of deforestation toward nearby areas (Cernea and Schmidt-Soltau 2006), preemptive clearing to prevent a future expansion of land restrictions (Newmark 1994, Fiallo and Jacobson 1995), and changes in market prices, which could have local and global effects (Armsworth et al. 2006 and Robalino 2007).

However, empirical analysis of spillovers, in particular from parks, has been exceptionally limited. Globally, due to changes in market prices, restrictions on timber harvest in one region are expected to increase timber harvest in other regions (Sohngen et al. 1999). There is also evidence of large leakage effects from the Conservation Reserve Program involving direct payments to farmers in the United States. For every 100 hectares retired under the program, 20 hectares were converted to cropland outside of the program (Wu 2000). Other papers have also shown evidence of leakage in forest carbon sequestration (Murray et al. 2004, Chomitz 2007 and Sohgen and Brown 2004).

For Mexico, there is evidence of leakage from the national ecoservice-payments program (Alix-Garcia et al. 2012). Landowners who had enrolled some land in the program increased deforestation on other property holdings. This effect is stronger in poorer municipalities and with those given less access to commercial banks, where credit constraints are higher (Alix-Garcia et al. 2012).

In Costa Rica, park deforestation spillovers have been explored on average (Andam et al. 2008). Average net effects on nearby forests were seen to be insignificant (Andam et al. 2008), a result that we confirm. Yet, as we show in this paper, averages can mask significant leakage effects in certain particular areas − especially where small changes in deforestation incentives could induce clearing activity, such as forests close to roads within areas where the returns to forest due to tourism are low.

4. Data & Empirical Approach

4.1. Data

Using the spatial detail offered by high-resolution data in a GIS (Geographic Information System), we randomly drew 50,000 points, one per km^2, from across Costa Rica as our units of analysis.

4.1.1. Forest & Sample

We use forest-cover maps for 1986 and 1997 to determine the deforestation during 1986 to 1997. The maps were derived from Landsat satellite images with a 28x28m resolution. They distinguish forest from non-forest and mangroves. Developed by the Tropical Science Center from aerial and satellite pictures, they indicate forest presence or absence at each point. To study deforestation, we eliminate points with uncertain presence of forest (leaving 47,241 points; see Table 1). We also drop 2,864 observations covered by clouds or shadows. We analyze points under forest in 1986 (42% or 21,087)[5].

[5] Observations covered by clouds and shadows, and, where presence of forest is uncertain, in principle might generate sample bias. Clouds and shadows are associated with wetter forests. The uncertainty of forest presence is associated with drier places due to the colors of the trees that cannot be distinguished from other land uses. This issue, which is faced by all analyses using satellite data, affects 11% of our sample. However, we are not aware of any reason for which the economic mechanisms outlined in Section 3 would not hold also in these areas. This is certainly something that should be explored empirically in future research.

Our focus is non-protected private forest. Thus, we drop all points inside parks and in public areas where government chooses land use, leaving 9,480 observations. Finally, because an important variable is the distance to park entrances via roads (calculated as the distance from the closest road segment to the park entrance), we also dropped 466 observations located farther than 5km from the closest road segment[6]. The number of forest observations remaining is 9,014. Our dependent variable is whether a forest point in 1986 had been cleared by 1997.

4.1.2. National Parks & Nearby Areas

Maps of all protected areas (PAs) in Costa Rica were digitalized by the GIS Laboratory at the Instituto Tecnológico de Costa Rica. We focus on national parks because they cover the most area and they are the strictest type of protection that allows tourism. All PAs included in the analysis were created before 1986. We drop all other types of PAs and all points within a PA, to analyze only neighboring areas. To determine which points are the neighbors of national parks, we compute the linear distance from each forested point to each national park, and take the minimum distance. This criterion defines our "treated group."

Next, we use this distance to the park to distinguish three sets of observations (see Table 1). First, we consider the 1,253 forested points that are within 5km of the nearest park border (Ring 1). Second, we consider the 1,486 forest points that are between 5km and 10km from the nearest park border (Ring 2). Every treated observation will be drawn from these two sets of observations. Finally, we obtain 6,275 observations that are over 10km from a national park (far from parks). For each test that we perform, we use this last set of observations as controls.

We define the set of observations that are "proximate" to the entrance of a national park as those with an along-the-roads distance of less than 20km from the nearest park entrance. Within Rings 1 and 2, we split the treated observations into close to entrance (503 observations in Ring 1 and 408 observations in Ring 2) versus far (750 observations in Ring 1 and 1,078 observations in Ring 2).

Finally, we distinguish closer versus farther than 1km from a national road for each ring separately. In particular, we distinguish the following four groups: i) close to both entrance and road (125 in Ring 1 and 92 in Ring 2); ii) far from entrance but close to road (84 in Ring 1 and

[6] We feel this is the best approach but emphasize that including those observations has no effect on our core results.

190 in Ring 2); iii) close to entrance but far from road (378 in Ring 1 and 316 in Ring 2); and iv) far from both entrance and road (666 in Ring 1 and 888 in Ring 2). All are compared with the untreated points. Of the 6,275 observations 10 km or farther from national parks, 1,136 observations are located close to national roads, while 5,139 are located far from national roads[7].

4.1.3. Parcel Characteristics

We used spatially specific information stored and manipulated within a GIS to obtain characteristics that are helpful in finding untreated points that are similar to the treated. These improved comparisons allow us to better estimate the impacts. We obtained measures of slope, precipitation, elevation, and distances to both rivers and key ocean ports. We also computed distances to San José, population centers, sawmills and schools. Finally, we computed the fraction of forest in 1986 at the census tract level, as a measure of forest stock in the neighborhood.

4.2. Empirical Approach

In order to determine the impact of national parks on deforestation rates in neighboring areas we must answer the question: "What would the neighboring deforestation rate have been had a park not been established nearby?" The simplest estimation strategy to answer this baseline question is to consider the average deforestation rate in untreated forest points, an estimator known as the "naïve" estimation (Morgan and Winship 2014). In our case, this would imply comparing deforestation rates in Rings 1 and 2 with deforestation rates beyond 10km of national parks. This approach is relatively common (Joppa and Pfaff 2010a list some examples) but clearly inadequate if the treatment group and the untreated group differ in terms of characteristics that also affect deforestation rates.

Table 2 shows such differences. Compared to controls, parcels within 0-5km of the nearest national park have steeper slopes, more precipitation, higher elevation, higher census-tract share of forest in 1986, and longer distances to roads, rivers, cities, coasts, sawmills and

[7] We also dropped 119 observations that are within the 20km distance via roads but farther than 10km from parks linearly. These observations would have entered the control group but they might be contaminated by the treatment effect as they are close to a park entrance via roads.

schools. In sum, Ring 1 points are more remote and likely to face less deforestation pressure than the average unprotected forest parcel beyond 10km from a PA (column 1). Ring 2 also differs from unprotected forest far from PAs but is less remote than Ring 1. The location of these groups of observations can be seen in Figure 2.

Table 2 also suggests that the national parks blocked deforestation in Ring 1, but may have increased it in Ring 2. However, such differences in the observed deforestation rates might be caused by the differences in land characteristics and not by proximity to parks. We use matching and regression analysis to compare treated to similar untreated points which do not differ in average land characteristics.

Matching selects the most similar untreated observations as controls. The deforestation rate in the control group is the estimate of what would have happened in areas near parks without the parks. Compared to standard regression, which can be employed after matching, this method imposes fewer assumptions for the functional form that relates land characteristics and deforestation (Rubin 2006). For example, if the treated observations tend to be far from roads, the estimated treatment effect is likely to depend on the functional form assumed for distance to roads (e.g. linear or log-linear). Matching directly reduces the difference in distance to roads between treated and untreated, as shown below, which thereby reduces the effect of functional-form assumptions on the estimates.

One important condition within matching is that the characteristics of the parcel we use reflect park siting determinants and, therefore, characteristics of surrounding areas. As documented in Pfaff et al. (1997) and discussed in Section 2.2, parks tend to be located in relatively remote areas with relatively low opportunity costs. We control for distance to the capital city, roads, towns, and sawmills to account for these opportunity costs. As we also discussed in Section 2.2, all volcanos are protected. Thus, we control for altitude, which in Costa Rica is highly correlated with the presence of volcanos. Additionally, we control for precipitation that, combined with altitude, determine the type of ecosystems and the presence of flora and fauna. As we also mentioned in Section 2.2, flora and fauna are important factors in determining park location. However, we acknowledge that there could be factors that affect park location which are missing from the available variables in the data, such as quality of institutions, as we discuss in Section 6.

Matching requires a definition of "similar." One is the distance in the characteristics' space between any two points[8], known as Covariate Matching (Abadie and Imbens 2006). One advantage of this strategy compared to other matching estimators is that the standard errors are consistently estimated (Abadie and Imbens 2006). In Table 3, we show the number of covariates that are different between treated and untreated groups both before and after matching, using a 5% significance level. Covariate matching reduces the number of unbalanced covariates for each test we perform[9].

In sum, we aim at testing the impact of park proximity on nearby private forest. Our counterfactual in all cases is what would have happened in those private locations if the park had not been implemented. We estimate these effects using the observed deforestation rate for the most similar unprotected forest far from parks. For each ring of private forest near a park, we test overall and heterogeneous effects by considering i) forest close to and far from park entrances, ii) forest close to and far from roads, and iii) all these heterogeneous effects for parks with different characteristics, always separately testing Ring 1 and Ring 2.

5. Results

5.1. On Average, No Significant Local Spillovers from Protected Areas

We test first whether there are deforestation spillovers on average near national parks. The naïve estimator (first two columns in Table 4) reflects different mean deforestation rates for treated and untreated observations[10]. Lower deforestation rates are found in the 5 km ring, in particular close to park entrances and far from roads. In the second ring, overall we find higher deforestation far from park entrances, although this difference is not statistically significant. Still, as noted, land characteristics can explain variations in deforestation rates between the treated and the untreated observations. Thus, from this alone we cannot conclude that parks cause these differences in deforestation rates.

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[8] $((x_1-x_2)' \, V \, (x_1-x_2))^{1/2}$ where x_1 and x_2 are land characteristics for any two observations and V is a positive-definite weight matrix.

[9] We also tested Propensity Score Matching but CVM achieved better balances. Thus, we choose to focus on covariate matching.

[10] We use clustered standard errors in all Naïve estimates.

We include land characteristics in estimations using ordinary least squares (OLS) and covariate matching (CVM) to isolate the effect of nearby parks. For the OLS specification, we estimate the average treatment effect for all observations, as well as the average treatment effect on the treated, which is directly comparable with the CVM estimator. We clustered the standard errors at the census tract level[11], and for CVM we also present the robust standard errors proposed by Abadie and Imbens (2006). We do not find any significant effects in either ring, whether or not we distinguish subsets by the distance to entrances or roads. This zero effect result confirms previous average spillover estimate for Costa Rica (Andam et al. 2008) and is robust to the estimation strategy.

However, this zero average effect might blend effects of different significance, magnitude and even sign. As discussed, national parks might reduce deforestation in nearby areas under some conditions, yet raise it under other conditions. Thus in principle, the average findings in Table 4 could be the result of blending overlapping and offsetting heterogeneous effects.

5.2. Heterogeneous Local Spillovers per Returns from Agriculture & Tourism

We expect greater deforestation leakage as the difference between the returns to agriculture and to forest conservation increases. A powerful determinant of agricultural returns is the distance to the nearest road. A powerful determinant of complementary touristic activities, which can raise returns to forest, is likely to be proximity to the entrance of the park. This section describes Table 5, which combines these factors.

We expect more deforestation in locations near parks when not affected by tourism, while at the same time close to roads. We find large and significant leakage effects under exactly these conditions in Ring 1, within 5km of parks (see first row under Ring 1 columns). This leakage result is robust to the different strategies used, and represents a statistically significant magnitude between 8.59 and 14.67%.

Moreover, the forces generating that leakage seem to be absorbed in the initial ring around the parks. In the upper row under Ring 2 columns we show that there are no significant effects in Ring 2, even when close to roads (low transport costs) and far from entrances (low

[11] To estimate the clustered standard errors for the CVM estimator, we run a regression with the treated and matched control observations, following Alix-Garcia et al. (2012).

tourism). In the second row of Table 5, we show that no impacts are found far from roads in either ring.

We might also expect that even for Ring 1 close to roads, leakage could be offset by tourism. Table 5 shows this result in the third row under Ring 1 columns. However, if we remain close to roads but move away from the entrance (third row under Ring 2 columns), we again see some evidence of leakage. These spillover estimates are large and significant increases in deforestation rates, with magnitudes that range from 6.32% to 16.82%. This effect could reflect other elements of tourism mechanisms, such as complementary hotel infrastructure.

In sum, we split the sample of forest areas near parks into subsets, using proxies for factors that are likely to be correlated with the returns to agriculture and tourism. We find that leakage from parks can be significant when close to roads. Tourism can reduce leakage, but impacts are not fully eliminated, as increases in deforestation simply take place farther away.

5.3. Robustness

In Table 6, we test whether the results are sensitive to the choice of the thresholds defining close and far from roads and park entrances. If we move the threshold that defines proximity to roads by 50 meters, our results do not change. Within Ring 1 far from park entrances, effects are large and significant when close to roads using this definition as well (Panel A, first two columns, first three rows). If we change the definition of proximity via roads to park entrances by 1km, again we still find large and significant effects (Panel A, from the third to the sixth column, first row). The results still hold even when we combine these tests (Panel A, from the third to the sixth column, first to third rows).

We perform the same tests for Ring 2 close to the entrance (Panel B). Changes in the definition of proximity to park entrances do not affect the results. When we increase the definition of proximity to roads by 50 meters, results do not change. However, significance is lost when we test reductions of 50 meters in the definition of proximity to roads. This might be explained simply by a reduction in the sample size. The sign of the effect is still positive and magnitudes are high.

We also test proximity to park entrances as a continuous variable. Using the same samples we have used in the previous robustness tests, we use a continuous distance variable from the park instead of a discrete treatment variable. As expected, deforestation spillovers fall as distance from the park increases, both for Ring 1 far from the entrance but close to roads (see Panel A, seventh and eighth columns, first row), and for Ring 2 close to entrances (see Panel B, seventh and eighth columns, fourth row). We also tested different distances to roads (see seventh and eighth columns, second, third, fifth and sixth rows). Only for the robustness test of proximity to roads in Ring 1 far from the entrance does the continuous treatment lose significance for clustered standard errors. However, we still get the same magnitude and sign.

5.4. Heterogeneity by Park Characteristics

Finally, we test whether these local spillover effects vary when the park characteristics differ. For instance, steeper parks facing lower clearing pressure might have different spillover effects than parks on relatively flat lands. Larger and smaller parks might also have different spillovers. These are empirical questions. In theory, the magnitude and sign of impacts will depend on how much productive land is protected, on the characteristics of nearby land, and on the presence of tourism.

In the first two columns of Table 7, we present the estimates of these heterogeneous effects for places where the conditions generate more leakage, which is far from the entrances and close to roads in Ring 1. Flatter parks have higher leakage effects (Panel A). We also find that smaller parks have higher leakage effects than larger parks (Panel B). As documented in Pfaff et al. (2009), smaller parks tend to be located in high deforestation threat areas. Taken together, these results are consistent with the model presented in Section 3, as flatter and smaller parks tend to have higher opportunity costs and greater levels of deforestation threat.

Close to roads and close to the entrance in Ring 2 are conditions where we also found leakage (see columns 3 and 4 in Table 7), although perhaps for different reasons, such as complementary tourism infrastructure. For this leakage location, we find opposite results compared to Ring 1 far from entrances, where a tourism mechanism is irrelevant. Here, larger parks with steeper lands have higher leakage effects than smaller and flatter parks (Panels A and B). Significant tourism infrastructure might play a greater role for these parks. The entrances of the larger parks with steeper lands tend to be found in spots with relatively easier access.

Therefore, these areas have higher opportunity costs than other spots within those parks[12]. This could explain why we find higher leakage in Ring 2 near park entrances than in Ring 1 far from the entrance. Such within-park differences are smaller in small parks.

Older parks also differ from newer ones in terms of local deforestation spillovers (Table 7, Panel C). As explained in Section 2.2, older parks protect volcanos (e.g. Poás and Irazú) and other areas with high recreational, cultural and historical value (e.g. Santa Rosa and Manuel Antonio). Tourism activities are highly consolidated all around old parks. We even have negative coefficients, though statistically insignificant, far from the entrance in Ring 1. However, we do find leakage in Ring 2 close to entrances for older parks, a result that is again consistent with considerable tourism infrastructure. These are areas located at some distance from those parks, where deforestation do not spoil tourism directly yet providing easy access to parks. In contrast, newer parks generate significant leakage effects for Ring 1, when close to roads and far from the entrance.

6. Discussion

Motivated by the observation that spillovers can significantly reduce or multiply the effects of land conservation policies, we empirically examined how national parks in Costa Rica affect the deforestation rates in forested lands near them. We used the most similar parcels that are far from parks as counterfactual comparisons in order to estimate spillover impacts. We employ the definition of similarity embedded in covariate matching, which generated the best balance of treated and controls across the parcel characteristics.

We significantly extended the existing literature by using economic rationales concerning the returns from agriculture and tourism as a basis for splitting nearby forested lands into subsets. We expect these groups to have different net returns to forest (versus clearing) and thus to differ in the magnitude and sign of the spillover. There are multiple possible mechanisms by which land-use interventions could affect the factors that determine the net returns to forest clearing. Considering them yielded various theoretical predictions, which we validated empirically.

[12] Indeed, a simple test showed that when inside the park, land near entrances is significantly closer to roads than land far from the entrances.

On average, we found insignificant net spillover effects within both 0-5km and 5-10km of parks, when controlling for land characteristics using matching and regression methods. However, averages blend heterogeneous spillover park impacts for different subsets of nearby forested lands, defined according to the distance to roads (critical for agricultural returns) and to park entrances (critical for tourism and thus forest returns). Spillovers close to park entrances are insignificant – in areas associated with higher tourism − but we found large increases in deforestation (around 9%) near roads in the areas less exposed to tourism. Further, we again find leakage when moving away from the entrances towards areas where the immediate tourism returns are lower and the returns to clearing for agriculture and for tourism infrastructure increase. When looking across all the parks, these heterogeneous spillover impacts results are quite robust.

We further extend existing literature by separating parks into groups in multiple ways that may meaningfully characterize different settings that could raise or lower such significant spillovers. For instance, the leakage effect that we found far from entrance near roads is higher if the parks in question are in lower slopes (higher opportunity cost) forest land. Also, older and larger parks, associated with higher tourism, generate more leakage near entrances but in Ring 2.

As discussed in the empirical strategy, identification relies upon successfully controlling for all characteristics that are correlated with both park location and deforestation. However, one set of characteristics that is not available in our data concerns the organizational capacity and political capital of communities and their leaders. Communities with strong organization capacity and political capital might affect the decision process for park location, and orient that process towards either tourism or agricultural employment. To the extent that such factors are not correlated with deforestation, these omitted variables do not bias our estimates. However, they could in principle affect the rate of tourism and agricultural development, and thus affect deforestation. If other covariates do not capture some of these effects, our estimates could be biased. The sign of the bias will depend on whether strong communities will attract parks and tourism or reject parks in favor of agriculture. For our period of analysis, agriculture was an important source of income for rural Costa Rica while tourism was not. Thus, parks may be near communities unable to advocate effectively for development (which yields deforestation). That would bias impact estimates for spillovers towards not finding increases in deforestation near parks.

Without additional information, unfortunately we cannot comment on how enforcement affects spillover effects. However, we do not expect protection enforcement to vary substantially by type of protected area in the case of Costa Rica. Still, this is a dimension to be explored in future spillovers work, given the importance of the variation in enforcement across types of protected areas found in other leading tropical forest countries (see Joppa and Pfaff 2010a, Nelson and Chomitz 2011, Pfaff et al. 2013, Pfaff et al. 2015a,b).

We acknowledge the fact that even though we are using state-of-the-art measures of deforestation, better metrics of forest loss are needed to detect forest degradation. Binary measures of deforestation indicate the presence of forest, but there could be some underlying forest loss. When metrics of forest degradation can be utilized, estimates of carbon leakage will be improved, which will be highly relevant in the context of REDD policies. Similarly, better land-use data is needed to avoid having observations with uncertain presence of forest due to clouds or shadows. Future research should consider testing, if results would change significantly if parcels covered by clouds and in places where the presence of forest are included in the analysis.

Additionally, we have considered only the protected areas. There are multiple other interventions in land management that may well generate significant spillovers from private behavior. These changes in private behavior may affect other private behavior, shifting regional equilibria (Robalino and Pfaff 2012). For instance, payments for ecosystem service programs are growing rapidly. These payments could influence land use outside the program through various mechanisms, such as the new option value of possible future payments. Certification of logging concessions, for instance by the Forest Stewardship Council, is another type of intervention that is growing rapidly and may feature spillovers through mechanisms such as the optimization across multiple concessions by logging firms.

Protected areas, which as in Costa Rica or the Brazilian Amazon can be interventions on a very large scale, could have additional spillover effects on optimal public behavior of other parts of the governments. Herrera (2015), for instance, finds that the establishment of protected areas affects both private migration and the location of new roads. That possibility raises the potential for game-theoretic interactions between agencies pursuing very different frontier agendas.

We find evidence of heterogeneous spatial spillovers that depend on the location of roads and the presence of tourism. Therefore, it is critical to include the impacts outside the boundaries of an intervention when designing future policy, particularly as the attention to the impact evaluation of conservation and development interventions increases. While clearly it was natural and understandable to start with impacts inside those boundaries, improving our understanding of spillovers can help us develop more accurate estimates of program impact, and help protected area managers be more vigilant of where deforestation is likely to be displaced.

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Table 1. Forest and Sample

	Number of observations	Percentage
Observations (total)	**50000**	**100**
Drop if there was no forest in 1986	23290	46.58
Drop if it is not private land	11607	23.21
Drop if undefined distance by roads to parks	466	0.93
Drop if uncertain about presence of forest	2759	5.52
Drop if there are clouds or shadows	2864	5.73
1986 private forest observations for analysis	9014	18.03
Ring 1: 0-5Km	**1253**	**100.00**
Close to the entrance	*503*	40.14
Close to national roads	125	9.98
Far from national roads	378	30.17
Far from the entrance	*750*	59.86
Close to national roads	84	6.70
Far from national roads	666	53.15
Ring 2: 5-10Km	**1486**	**100.00**
Close to the entrance	*408*	27.46
Close to national roads	92	6.19
Far from national roads	316	21.27
Far from the entrance	*1078*	72.54
Close to national roads	190	12.79
Far from national roads	888	59.76
Beyond 10km	**6275**	**100.00**
Close to national roads	1093	17.42
Far from national roads	5063	80.69
Dropped+ if close to the entrance (less than 20km through roads)	119	1.90

+We consider observations beyond 10km as untreated. However, if they are relatively close to the entrance (less than 20km through roads), they might be contaminated.

Table 2. Land Characteristics & Group Mean Differences

	Untreated	Treated 0-5 km		Treated 5-10 km	
	Mean	Mean	t-stat[1]	Mean	t-stat[1]
Dependent Variable					
Deforestation rate	13.42	10.61	-2.70	14.87	1.47
Control Variables					
Slope (percentage)	44.85	64.93	7.66	55.01	4.19
Precipitation (mm)	3.30	3.73	15.15	3.67	14.02
Elevation (m)	0.35	0.75	27.08	0.43	6.53
Dist. to local roads (Km)	0.78	1.01	8.34	0.99	8.15
Dist. to national roads (Km)	3.90	4.35	3.96	3.69	-2.11
Dist. to rivers (Km)	1.42	1.61	4.80	1.25	-4.83
Dist. to capital city (Km)	105.70	104.01	-1.14	116.42	7.81
Dist. to Pacific coast (Km)	52.30	50.45	-1.44	55.70	2.80
Dist. to Atlantic coast (Km)	110.23	104.99	-2.49	96.75	-6.79
Dist. to towns (Km)	2.82	3.36	9.51	3.10	5.49
Dist. to sawmills (Km)	18.34	22.28	11.55	22.06	11.49
Dist. to schools (Km)	15.21	14.32	-2.98	13.37	-6.58
Percentage of forest 1986	52.17	58.86	9.08	55.05	4.15

[1] Test of means against untreated.

Table 3. Matching Balances – Number of Statistically Different Covariates at 5% Significance Level before and after Matching

	Pre-match	**After CVM**
Ring 1	11	2
Close to entrance	10	0
Close to roads	6	0
Far from roads	10	0
Far from entrance	13	2
Close to roads	5	0
Far from roads	12	2
Ring 2	12	0
Close to entrance	9	0
Close to roads	5	0
Far from roads	10	0
Far from entrance	9	0
Close to roads	5	0
Far from roads	12	0
Ring 1 and 2	11	2
Close to entrance	9	0
Close to roads	6	0
Far from roads	10	0
Far from entrance	11	2
Close to roads	5	0
Far from roads	10	2

Table 4. Initial Estimates of National Park Impact on Nearby Deforestation

	Naive[1]		OLS[2]		ATT with OLS[1]		Covariate Matching[2] with Abadie & Imbens (2006) s.e.		Covariate Matching[2,1]	
	Ring 1	Ring 2	Ring 1	Ring 2	Ring 1	Ring 2	Ring 1	Ring 2	Ring 1	Ring 2
	0-5 Km.	5-10 Km.	0-5 Km.	5-10 Km.	0-5 Km.	5-10 Km.	0-5 Km.	5-10 Km.	0-5 Km.	5-10 Km.
Overall effect	**-0.0280***	**0.0145**	**0.0071**	**0.0199**	**0.0192**	**0.0206**	**0.0079**	**0.0080**	**0.0073**	**0.0079**
	[0.015]	[0.017]	[0.013]	[0.015]	[0.013]	[0.015]	[0.011]	[0.011]	[0.017]	[0.018]
Far from park entrance	**-0.0137**	**0.0255**	**0.0186**	**0.0211**	**0.0231**	**0.0205**	**-0.0001**	**0.0059**	**0.0029**	**0.0066**
	[0.019]	[0.020]	[0.016]	[0.018]	[0.015]	[0.017]	[0.014]	[0.013]	[0.024]	[0.021]
Close to park entrance	**-0.0515*****	**-0.0173**	**-0.0017**	**0.0065**	**0.0233**	**0.0144**	**0.0081**	**0.0186**	**0.0083**	**0.0166**
	[0.018]	[0.024]	[0.016]	[0.023]	[0.016]	[0.021]	[0.013]	[0.018]	[0.019]	[0.027]
Far from roads	**-0.0424****	**0.0034**	**0.0038**	**0.0148**	**0.0189**	**0.0157**	**0.0070**	**0.0065**	**0.0061**	**0.0069**
	[0.017]	[0.018]	[0.013]	[0.017]	[0.013]	[0.016]	[0.011]	[0.012]	[0.018]	[0.020]
Close to roads	**0.0387**	**0.0579***	**0.0280**	**0.0420**	**0.0390**	**0.0413**	**0.0435**	**0.0235**	**0.0421**	**0.0227**
	[0.038]	[0.032]	[0.040]	[0.033]	[0.038]	[0.032]	[0.027]	[0.028]	[0.040]	[0.034]

***, ** and * represent significance at 1, 5 and 10% level respectively. Standard errors in squared brackets. [1]Clustered standard errors at the census tract level. [2]The control variables used are shown in Table 1. Distances as controls are in logs.

Table 5. Additional Matching Estimates of National Park Impact on Nearby Deforestation

	Ring 1: 0- 5 Km						Ring 2: 5 - 10 Km					
	OLS	OLS (ATT)	CVM	CVM Trimmed[a]	OLS after CVM	OLS after CVM Trimmed[a]	OLS	OLS (ATT)	CVM	CVM Trimmed[a]	OLS after CVM	OLS after CVM Trimmed[a]
	Clustered[1] standard errors		Robust standard errors		Clustered[1] standard errors		Clustered[1] standard errors		Robust standard errors		Clustered[1] standard errors	
FAR FROM ENTRANCE												
Close to roads												
Effect	**0.0859**	**0.0761***	**0.1039****	**0.1327*****	**0.1086***	**0.1467****	**0.0269**	**0.0256**	**-0.0145**	**-0.0313**	**-0.0135**	**-0.0254**
	[0.067]	[0.046]	[0.043]	[0.048]	[0.060]	[0.074]	[0.042]	[0.040]	[0.035]	[0.042]	[0.047]	[0.057]
Far from roads												
Effect	**0.0110**	**0.0209**	**-0.0114**	**-0.0289**	**-0.0088**	**-0.0250**	**0.0192**	**0.0189**	**0.0072**	**-0.0217**	**0.0087**	**-0.0211**
	[0.017]	[0.016]	[0.015]	[0.019]	[0.025]	[0.029]	[0.019]	[0.018]	[0.014]	[0.015]	[0.023]	[0.025]
CLOSE TO ENTRANCE												
Close to roads												
Effect	**-0.0008**	**0.0142**	**0.0161**	**0.0187**	**0.0151**	**0.0175**	**0.0632**	**0.0738****	**0.0882****	**0.1682****	**0.0825***	**0.1318**
	[0.041]	[0.037]	[0.030]	[0.046]	[0.044]	[0.069]	[0.047]	[0.036]	[0.041]	[0.074]	[0.042]	[0.085]
Far from roads												
Effect	**0.0084**	**0.0154**	**0.0125**	**0.0310***	**0.0121**	**0.0307**	**0.0060**	**0.0066**	**0.0083**	**-0.0149**	**0.0075**	**-0.0153**
	[0.019]	[0.016]	[0.014]	[0.016]	[0.019]	[0.019]	[0.030]	[0.025]	[0.019]	[0.021]	[0.031]	[0.024]

***, ** and * represent significance at 1, 5 and 10% level respectively. Standard errors in squared brackets. [1]Clustered standard errors at the census tract level.
[a]Using only observations in the interval of 0.1 and 0.9 of the propensity score as suggested by Crump et al. 2009

Table 6. Distance and threshold robustness tests

	Discrete treatment						Continuous treatment	
	CVM	OLS after CVM	CVM	OLS after CVM	CVM	OLS after CVM	OLS after CVM	
	Robust	Clustered[1]	Robust	Clustered[1]	Robust	Clustered[1]	Robust	Clustered[1]
Panel A	**Ring 1: 0-5Km**		**Ring 1: 0-5Km**		**Ring 1: 0-5Km**		**Ring 1: 0-5Km**	
FAR FROM ENTRANCE	More than 20km in roads		More than 21km in roads		More than 19km in roads		More than 20km in roads	
Close to roads (1km)	0.1039**	0.1086*	0.1051**	0.1097*	0.1019**	0.1061*	-0.0442**	-0.0442*
	[0.043]	[0.060]	[0.044]	[0.062]	[0.042]	[0.059]	[0.021]	[0.026]
Close to roads (1.05km)	0.0956**	0.1013*	0.0921**	0.0995*	0.0970**	0.1025*	-0.0341*	-0.0341
	[0.040]	[0.055]	[0.041]	[0.056]	[0.039]	[0.054]	[0.020]	[0.024]
Close to roads (0.95km)	0.1036**	0.1084*	0.1020**	0.1080*	0.1023**	0.1066*	-0.0438**	-0.0438
	[0.044]	[0.063]	[0.044]	[0.064]	[0.042]	[0.061]	[0.022]	[0.028]
Panel B	**Ring 2: 5-10Km**		**Ring 2: 5-10Km**		**Ring 2: 5-10Km**		**Ring 2: 5-10Km**	
CLOSE TO ENTRANCE	Within 20km in roads		Within 21km in roads		Within 19km in roads		Within 20km in roads	
Close to roads (1km)	0.0882**	0.0825*	0.0788*	0.0728*	0.0814**	0.0757*	-0.0701***	-0.0701***
	[0.041]	[0.042]	[0.041]	[0.042]	[0.041]	[0.042]	[0.021]	[0.026]
Close to roads (1.05km)	0.0910**	0.0851**	0.0865**	0.0804**	0.0867**	0.0808**	-0.0650***	-0.0650**
	[0.038]	[0.040]	[0.038]	[0.040]	[0.039]	[0.040]	[0.020]	[0.026]
Close to roads (0.95km)	0.0639	0.0598	0.0589	0.0543	0.0561	0.0519	-0.0823***	-0.0823***
	[0.041]	[0.040]	[0.042]	[0.040]	[0.041]	[0.040]	[0.021]	[0.028]

***, ** and * represent significance at 1, 5 and 10% level respectively. Standard errors in squared brackets. [1]Clustered standard errors at the census tract level.

Table 7. Splits by park characteristics: size and years after implementation

Close to Roads	CVM	OLS after CVM	CVM	OLS after CVM
	Robust (1)	Clustered[1] (2)	Robust (3)	Clustered[1] (4)
	Ring 1: 0-5Km		Ring 2: 5-10Km	
	FAR FROM ENTRANCE		CLOSE TO ENTRANCE	
All sample	0.1039**	0.1086*	0.0882**	0.0825*
	[0.043]	[0.060]	[0.041]	[0.042]
Panel A: by slope				
Steeper 50%	**0.0182**	**0.0206**	**0.1422***	**0.1187**
SE	[0.061]	[0.083]	[0.075]	[0.074]
Num treat obs	42	42	45	45
Flatter 50%	**0.1246****	**0.1575**	**-0.0144**	**-0.0071**
SE	[0.059]	[0.099]	[0.038]	[0.041]
Num treat obs	42	42	47	47
Panel B: by size[2]				
Larger 50%	**-0.0049**	**0.0136**	**0.2027*****	**0.1900*****
SE	[0.059]	[0.084]	[0.076]	[0.071]
Num treat obs	42	42	42	42
Smaller 50%	**0.1726*****	**0.1758****	**0.0186**	**0.0182**
SE	[0.060]	[0.088]	[0.042]	[0.038]
Num treat obs	42	42	50	50
Panel C: by years after implementation[3]				
Older 50%	**-0.0764**	**-0.0572**	**0.1508*****	**0.1392****
SE	[0.054]	[0.080]	[0.058]	[0.054]
Num treat obs	42	42	52	52
Newer 50%	**0.2295*****	**0.2312*****	**-0.0281**	**-0.0292**
SE	[0.065]	[0.087]	[0.061]	[0.055]
Num treat obs	42	42	40	40

***, ** and * represent significance at 1, 5 and 10% level respectively. Standard errors in squared brackets. [1]Clustered standard errors at the census tract level. [2] We use the median for the split. Larger parks are those with more than 12 thousand hectares and smaller parks those with less than 12 thousand hectares. [3] We use the median for the split. Older parks were established before 1975 and newer parks were implemented between 1975 and 1986.

Figure 1

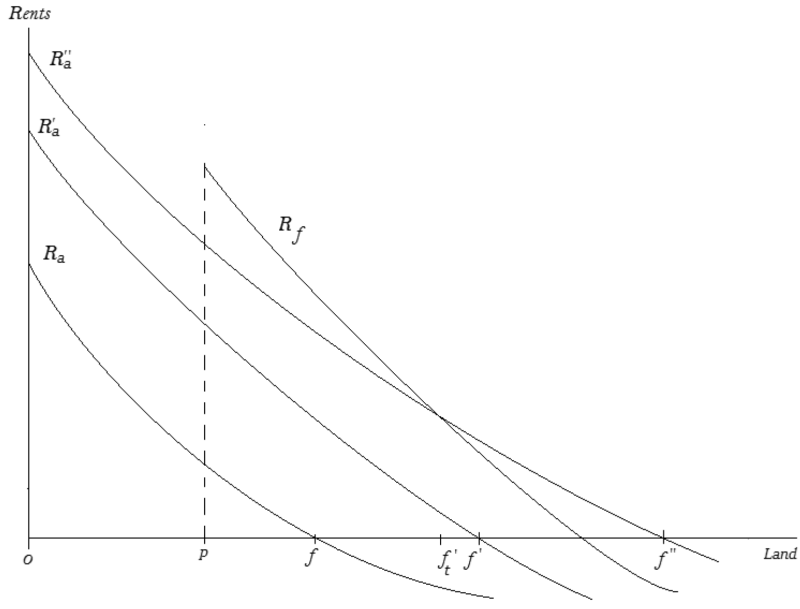
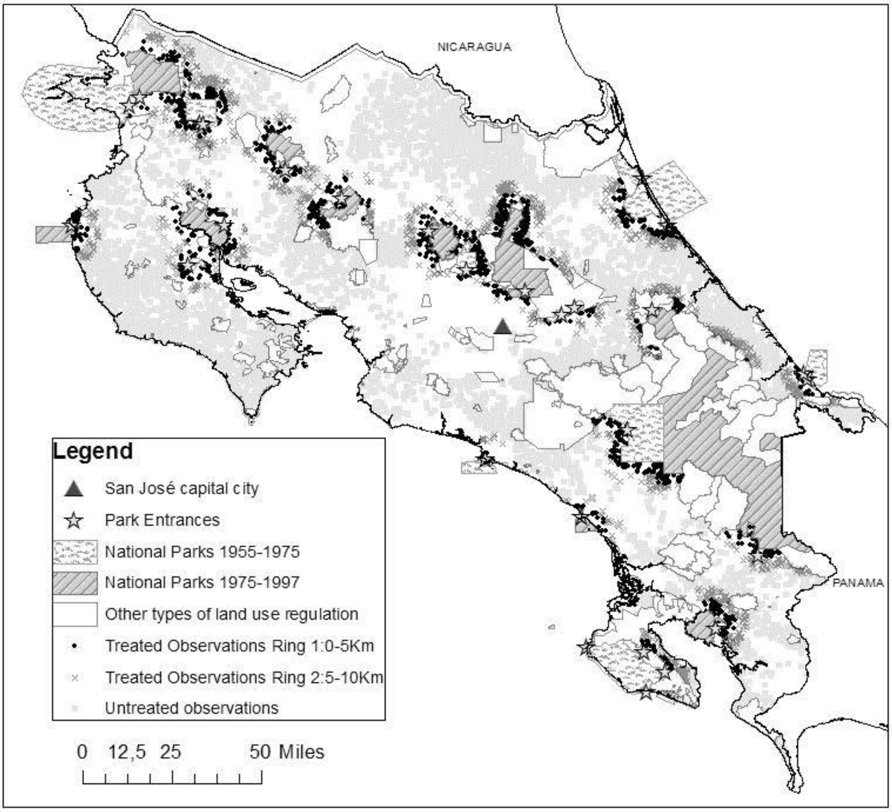


Figure 2


NICARAGUA
PANAMA
Legend
San José capital city
Park Entrances
National Parks 1955-1975
National Parks 1975-1997
Other types of land use regulation
Treated Observations Ring 1:0-5Km
Treated Observations Ring 2:5-10Km
Untreated observations
0 12,5 25 50 Miles

Chapter II

Has forest certification reduced forest degradation in Sweden?

Laura Villalobos[1,2], Jessica Coria[1] and Anna Nordén[1,3]

Abstract

Voluntary forest certification is an increasingly popular tool to promote sustainable forest management. Certification allows producers who meet stringent environmental standards to label their products and potentially achieve greater market access and receive higher prices for their products. The voluntary nature of certification programs implies, however, that it is difficult to determine the effects of forest certification due to selection bias. This paper contributes to the impact evaluation of forest certification by estimating the effects of certification of non-industrial private forest owners on forest degradation in Sweden – one of the countries with the largest total area of certified forests. We rely on official forest inventory data at the plot level, information on certification status, and standard impact evaluation methods to identify the causal effect of certification on three environmental outcomes aimed at preserving areas of special significance for biodiversity conservation. Our results indicate that certification has not halted forest degradation in that it has not improved any of the environmental outcomes. Our findings suggest that, for forest certification to have an effect, the standards should be tightened and the monitoring and enforcement of forest certification schemes strengthened.

Keywords: forest certification, impact evaluation, sustainable forest management, FSC, PEFC.
JEL Codes: L15, Q12, Q23, Q28.

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1. Introduction

Accelerated losses of old-growth forest and intensive timber production have serious consequences for biodiversity conservation due to the loss of habitats (Folke et al. 2004). Deforestation and forest degradation also contribute to climate change as they release between 10% and 15% of global human-induced greenhouse gas emissions (Van der Werf at al. 2009). Given a global deforestation rate of about 13 million hectares per year, and an unknown but likely higher rate of forest degradation, increasing efforts to maintain forests and their biodiversity through improved forest management is an important contemporary issue for several United Nations (UN) conventions. For example, the UN Convention on Biological Diversity sets a global target for restoration of at least 15% of degraded ecosystems by 2020, and the UN Framework Convention on Climate Change proposes to recover degraded forests as carbon sinks (see e.g., Rametsteiner and Simula 2003, and Thompson et al. 2013).

Forest certification is a voluntary, market-driven instrument whereby an independent third party (called a certifier or certification body) assesses the quality of forest management in relation to a set of predetermined standards and gives written assurance that a product or process conforms to the requirements specified in the standards (Rametsteiner and Simula 2003). The general objective of forest certification is thus to provide information to consumers about the quality of forest management in areas from which traded wood products are sourced.

Producers who meet stringent environmental standards can then label their products in the marketplace, allowing them to potentially achieve greater market access and receive higher prices for their products. An environmentally sensitive consumer base should create incentives that reward certified producers, encouraging non-certified producers to seek certification and its market benefits (Auld et al. 2008). In this way, certification directs demand away from uncertified forests towards products that meet rigorous management criteria.

Forest certification has generated considerable attention as means to reverse deforestation and forest degradation by promoting improved environmental and social outcomes in forest management criteria (Blackman and Rivera 2011, and Romero et al. 2013). As a consequence, the global area of certified forest has been rapidly growing. For instance, the global area of certified forest increased from 18 million ha in 2000 to some 438 million ha in 2014.

About 90 percent of the total area certified in 2014 is in the temperate and boreal climatic domains, although there has also been growth, albeit at a slower pace, in the tropics and subtropics (FAO 2015).

The voluntary nature of forest certification implies, however, that environmental benefits from a certification scheme may be limited if only producers who are already meeting environmental standards opt into certification. Hence, to properly assess the effectiveness of forest certification, we need to account for this selection problem. Unfortunately, due to the lack of suitable information, the evidence regarding the impact of forest certification on environmental outcomes using rigorous impact evaluation methods is very limited and finds mixed results. For example, some studies indicate that forest certification has reduced deforestation in Indonesia (Miteva et al. 2015) and Chile (Heilmayr and Lambin 2016). In contrast, no evidence of certification reducing deforestation has been found for Mexico (Blackman et al. 2015), Peru (Rico Staffron 2015), and Cameroon (Panlasigui 2015).

This paper investigates the effects of the two main certification schemes (i.e., the Forest Stewardship Council, FSC, and the Programme for the Endorsement of Forest Certification, PEFC) on forest degradation in Sweden – the country with the largest total area of certified forest in Western Europe (UNECE/FAO 2012). A key feature of our research is that unlike previous studies which use remote sensing data sources, we use detailed forest inventory data of non-industrial private forest owners at the plot level both before and after felling. This ground information is able to capture subtle changes in the amount and composition of the forest in small areas with higher precision than remote data sources, thus providing more precise estimates of the effects of forest certification (Vincent 2016).

Furthermore, in contrast to previous studies analyzing the effects of forest certification on the rates of deforestation, we focus on the effects on avoided degradation. Sweden's boreal forest has experienced radical changes caused by humans since the beginning of industrialization. Ecological structures such as large, dead, and deciduous trees have been removed from the forest, and natural processes have been suppressed, leading to ecosystem degradation. Current evidence indicates the need for restoration because the intensification of forest management to enhance wood production has reduced forest biodiversity and resilience (Gauthier et al. 2015, and Nordlind and Östlund 2003). Our study focuses on the effects of forest certification on avoided decrease in forest quality with respect to the initial condition – an outcome whose assessment calls for observations on the ground and that relates directly to forest management practices and to the certification standards. Moreover, we look at the behavior of private non-industrial forest owners, who are the base of the supply chain in Sweden with 50% of the total forest area and 60% of the total annual yield in Sweden (Swedish Forest Agency 2014).

We investigate three key environmental outcomes on which certifications are expected to have an impact. First, we look at the effect of certification on the preservation of environmentally important areas during felling, a criteria that is at the core of the environmental principles of the certification standards (FSC 2009, 2010 and 2013). Second, we look at the number of trees and high stumps remaining in the plots 5–7 years after felling. High stumps have many important functions, as food resource, habitat or shelter (Söderström 2009). Because of the ecological value of these remainders, the certification standard encourages forest owners to leave both living wood and high stumps after clearing. Finally, we look at the certifications' requirement to set aside at least 5% of the total forest land for conservation purposes. Set aside areas are considered a cost-efficient way to conserve biodiversity in managed and fragmented forest landscapes because they provide increased structural variation and availability of habitats and substrates and improve species' dispersal abilities and long-term survival in the forest landscape (Timonen et al. 2011, and Wikberg et al. 2009).

The joint consideration of all these environmental outcomes delivers a more complete assessment of the overall environmental impact of certification. To this end, we combine Swedish forestry inventory databases with information on certification status collected through a phone survey specifically for the purpose of this research. We analyze this information using standard impact evaluation methods to identify the causal effect of certification on the outcomes.

We found that 64% of the inspected plots do not comply with environmental considerations, implying that most sensitive habitats are not saved during the felling. Since certified forest owners receive a price premium and improved market access, we expect certification to provide further incentives to comply with the environmental considerations. Therefore, the questions we address in this paper are whether forest certification provides additional incentives to i) reduce the rate of non-compliance, ii) decrease the magnitude of non-compliance, and iii) increase the area set aside for conservation purposes.

Our results provide no evidence that certification improves environmental performance. Certified plots are not significantly more likely to preserve these areas or to increase the magnitude of the areas that are saved. Interestingly, we found no statistical differences between the environmental performance under the FSC and PEFC schemes. Our findings suggest that if forest certification is to have an effect, the stringency of standards should be increased, and the monitoring and enforcement of forest certification schemes should be strengthened.

The paper is organized as follows. Section 2 briefly describes the forest sector and the certification schemes in Sweden. Section 3 presents the data and Section 4 the empirical strategy. Results are presented in Section 5. Finally, Section 6 discusses some policy implications and concludes the paper.

2. Background

Forestry is a key sector in Sweden. Mainly covered by boreal forests, 57% of the land area is productive forest, where climate and soil conditions favor growth in the south of the country. Indeed, forestry is an economic activity that represents around 11% of Swedish total employment and exports (Royal Swedish Academy of Agriculture and Forestry [KSLA] 2015). Norway spruce and Scots pine are the dominant species with 78% of the standing volume, and the harvest's final use is mainly pulp, paper and sawn timber production. Most of these products are exported, giving Sweden a top place within World leading exporting countries in the forest industry (KSLA 2015). Mechanized clear cutting is the dominant management system, resulting in an evenly-aged forest, and it is typically subcontracted. Harvest is followed by compulsory reforestation, which is mainly done by manual planting (also subcontracted) or by natural regeneration (KSLA 2015).

There are several stakeholders in the sector, including around 330,000 private owners who are the base of the system as they own half of the productive forest. The other major category is private sector companies, with 25% of the productive forest (Swedish Forest Agency 2014). It is common for individual owners to supply industrial enterprises and to adhere to a forest association, where they obtain advisory services and representation in the forest policy (KSLA 2015).

The timber market in Sweden is less regulated than in most other countries and the forest management is based on a “freedom with responsibility” and “soft governance” policy that includes wide-ranging discretion for forest owners to manage their forests (KSLA 2015). Still, conservation targets are a priority policy issue. The Swedish Forestry Act states that environmentally important areas must not be damaged or destroyed during felling.

In particular, sensitive habitats, unusual trees and shrubs, and buffer zones have to be kept intact. Sensitive habitats are areas with high natural values deviating from the evenly aged production forest. Unusual trees and shrubs are those that have had time to develop some form of natural value, for example older, slow-growing, large or rare trees. Buffer zones and riparian zones are areas important for the conservation of species diversity in the forest floor, wetlands, lakes, and streams. The Swedish Forest Agency provides forest owners with detailed information about the definition of these categories, including for example illustrative pictures to facilitate their identification on the ground (Swedish Forest Agency 2010).

Two certification schemes operate in Sweden. The Forest Stewardship Council (FSC) certification was launched in 1998 when a working group comprising different forest stakeholders, including the NGO Swedish Society for Nature Conservation, introduced a national standard based on the international FSC guidelines. In contrast, the Programme for Endorsement of Forest Certification (PEFC) started in 2000 driven by private forest owner associations (see Johansson and Lidestav 2011). FSC initially targeted large-scale forest companies while PEFC focused on small-scale private forest owners. However, at present, both standards certify any scale of operations from a minimum of 0.5 ha and are very similar in terms of requirements.

It is estimated that 50% of all productive forest land is certified under FSC and 48% under PEFC (FSC Sweden 2014). It is possible to hold both certifications simultaneously. Important for our research is that all major large-scale companies are FSC certified, making it difficult to identify the impact of certification on these contracts due to the lack of a control group. Therefore, our analysis focuses on non-industrial private forest owners, of whose land only around 17% is certified (Johansson and Lidstav 2011).

Adherence to forest certification is voluntary and the only eligibility requirement is to have productive forest with management purposes. However, the forest owners face transaction costs associated with the certification process. Information collected through a phone survey suggests that costs range between a one-time payment of around € 1,900 and an annual fee of € 210.[i] Because the scale of operations is typically small for non-industrial private forest owners, they opt for group certificates to reduce transaction costs[ii]. It is through this scheme that timber suppliers for companies, associations, and larger private owners are certified. In all cases, the certification is valid for 5 years after which renewal is possible upon request.

Whereas FSC and PEFC Sweden set the standards, in practice seven certifiers manage the certification. These certifiers are authorized by accreditation organizations, named Accreditation Services International (ASI) for FSC and Swedish Board for Accreditation and Conformity Assessment (Swedac) for PEFC. The certifiers are responsible for monitoring compliance with the standard and are themselves inspected by the accreditation organizations. For the group certificates, the lead contract holders also monitor their respective members through annual spot checks.

The accreditation organizations, too, make random spot checks of certified forest every year to verify that the certification standards are followed. In case of violation, a Corrective Action Request (CAR) is issued, allowing for up to 12 months to remedy a small deviation and up to 3 months for a large one. After this, if the CAR is not attended to, the forest owner loses the certificate. Uncorrected or serious violations of a single member within a group certificate leads to its exclusion from the certificate. A report by WWF (Hirschberger 2005) analyzed the public reports of the CARs over the period 1997–2005, and concluded that most CARs in Sweden concerned environmental issues, and most of the major transgressions concerned the failure to leave biodiversity trees and dead wood, as well as the lack of conservation of habitats and biotopes.

Timber prices vary greatly depending on factors such as tree species, timber quality, infrastructure, and geographical location. Although precise statistics are hard to obtain, explorative figures suggest that certified timber has a price premium of up to 5%, according to figures reported by the Swedish Forest Agency (2014) and publicly available information in the websites of some forest owners associations and industrial companies.[iii] Indeed, a meta-analysis of the consumers' willingness to pay for certified forest products find an average price premium of 12.2% (Cai et al. 2013). Forest owners may also view certification as a way to establish a competitive advantage in the forest product marketplace. Certification may create opportunities to access new markets that favor certified forest products. For example, green building and publishing companies give preference to certified wood products and these markets are growing in popularity (European Comission 2011). Indeed, demand for certification is also driven by legislation like the Lacey Act (US) and FLEGT (Europe) that stipulate that only certified timber can be traded on these markets.

3. Data

This section presents the data sources and sample size for each of the three environmental components of the standard under analysis. In each case, we describe in detail the outcomes, the definition of the treatment, and the control variables and present the corresponding descriptive statistics.

3.1 Environmentally important areas

In Sweden, all forest owners must submit a notification form to the Swedish Forest Agency before felling. On average, the agency receives 40,000 notifications from non-industrial private forest owners per year. From this pool, the agency selects a random sample of plots for ground inspection.[iv] During this unannounced field visit, inspectors conduct a forest inventory of the plot. One growing season (around one year) after the felling, the agency returns to the same plot and conducts another inventory in order to assess the new conditions. The information collected along this process is condensed in a dataset called Polytax 0/1, where 0 stands for the data collected before the felling and 1 for the data collected one year after.

The main purpose of Polytax 0/1 it to assess the environmental considerations taken during the felling. It includes precise measures of the environmentally important areas defined by the Swedish Forestry Act and required by the certification standards. We define the total environmentally important area as the sum of the areas under sensitive habitats, buffer zones, and unusual trees and shrubs. [v] As these areas are measured both before and after clearing by the inventory, we can observe the magnitude of the reduction in the total environmentally important area for each plot.

From this measure, we define our outcome variables. Firstly, we classify the plots depending on their compliance status. A plot is in compliance if all of its environmentally important area was maintained during the felling. In contrast, a plot is not compliant if there is a reduction in the environmentally important area. Hence, the non-compliance rate is defined as a categorical variable taking the value 1 for any positive reduction in the relevant area and 0 if there is no change. Secondly, we look at the magnitude of the damage in hectares, measured as the difference in total environmentally important area before and after clearing[vi]. Finally, we consider the magnitude of the damage in relative terms, i.e., as a share of the total environmentally important area before clearing. This is important because there could be substantial variations in the magnitude of the environmentally important areas across plots. Our hypothesis is that if forest certification promotes a more sustainable management of the forest, both the compliance rate and the magnitude (in absolute and relative terms) of environmentally important area left after clearing should be larger (and positive) for certified forest owners.

We base our analysis on the plots included in the Polytax 0/1 during the period 1999–2011 for non-industrial private forest owners under the category of regeneration felling. The total number of plots inspected during this period and under this category is 3,037. In Table 1, we describe this sample, after excluding 1% of the observations due to missing or invalid information on key variables. The average plot size is 6.7 ha, of which 10.5% is environmentally sensitive area. This small proportion is not surprising as most of these areas are mainly productive forests. Furthermore, we observe that on average the area of environmentally important areas decreases by roughly 30% after clearing.

Interestingly, the share of the sample that does not comply with the requirement of protecting all environmentally important area is 64%, causing a damage of 0.2 ha. This is not a negligible amount if we consider that the Forest Agency receives around 40,000 notifications of felling every year, which results in over 8,000 ha of environmentally important areas being cleared annually. If we consider the non-compliers only, we find that this group clears on average 42% of the initial environmentally important area. Table 1 also presents some basic demographic characteristics of the forest owners of the plots included in the sample. The average plot is legally under the property of a 60 years old male, who lives in or close by the plot, and makes counseled decisions regarding the forest management.

For the purpose of our analysis, a key variable missing in this dataset is whether the plot has adhered to a certification scheme. This missing information is what has prevented Polytax 0/1 from being used in previous analyses of the impacts of the certification program in Sweden. To collect such information, we conducted a phone survey[vii] where forest owners were asked about their certification status, date and type of certification, and participation in forest associations (see survey questionnaire in the Appendix). In addition, we asked forest owners for their main motivation for being or not certified, information we present in Table A1 in the Appendix. The fact that 45% of the respondents declared that they obtained the certification because it is economically viable is consistent with the benefits expected from the certification, i.e., price premium and access to markets.

We successfully collected information for 1,412 plots (response rate 45%) through the phone survey.[viii] From this sample we obtained 763 observations with complete information regarding the certification status, and other key variables[ix]. In Table 1 we compare this sub-sample with the Polytax 0/1, in order to assess whether it is representative of the entire sample of inspected plots. We include both the mean value for each characteristic and a statistical test for the difference in means with respect to the Polytax 0/1. We observe that the average plot is identical in both samples in terms of size, environmentally important areas, and non-compliance rate and magnitudes. The plots are also balanced in terms of geographic location, as we find no systematic differences in the proportion of observations per county between the sub-sample and the Polytax (not shown). This is reassuring that the plots in our sample mimic the population of inspected plots at least in the characteristics relevant for our analysis.

However, as it might be expected from a survey conducted by phone, we do find differences in the demographics of the forest owners. For example, in our survey, the respondents are on average 1.3 years older compared to the Polytax sample, and we obtained a fewer proportion of female forest owners. To the extent that certification status depends on demographics, these differences could introduce some bias to our sample. For example, if female forest owners are more likely to be certified than males, our sample could misrepresent the share of certified forest owners in Polytax 0/1. We correct for selection bias by including the variables that control for sample selection. In addition, in Section 5.4.1 we present a robustness test where we show that the results are robust to a Heckman (1979) sample selection model.

Using the information on the type and date of certification, we classify the observations that were certified at the moment of clearing into four treatments and one control group. Treatment 1 is the most general and includes all plots with at least one certification at the moment of clearing, regardless of type and whether they add the other certification after felling (247 observations). We also look at the effect of each standard independently by defining Treatments 2 and 3. Treatment 2 includes plots that were FSC certified at the moment of clearing that were not certified according to PEFC after felling (53 observations). Similarly, Treatment 3 includes plots that were PEFC certified at the moment of clearing, with no FSC after felling (75 observations). Finally, Treatment 4 contains plots holding both certifications at the moment of clearing (82 observations). The control group comprises plots not certified during the entire period of analysis (516 observations). In Table 1 we can see that the rate and magnitude of non-compliance is lower for certified plots. The rest of the statistical analysis is devoted to analyze whether this difference is statistically different from zero and whether it can be attributed to the certification.

(INSERT TABLE 1)

We describe further our final sample in Table 2, where we present the descriptive statistics of the set of control variables that will be useful in estimating the causal effect of certification. These are characteristics that could explain the quality of the forest management and be correlated with the certification status. In particular, we account for geographic characteristics of the plots, including the plot area, soil quality, density of roads in the municipality, and county. In a fairly homogeneous landscape as the Swedish case, these variables are expected to capture most of the variation in the productivity of the plots and their connectedness to the markets.

In addition, we include characteristics of the forest owners that could determine their certification status, including participation in forest associations, and demographics such as education level, gender, age, experience, and how involved the forest owner is in the forest management (measured by whether he/she lives close to the plot and by whether he/she makes decisions on his/her own as opposed to with peers). These variables are expected to capture differences in the level of environmental awareness and access to information. Note that participation in forest associations reduces transaction cost and up-front payment to become certified, and hence it is expected to play an important role in the probability that a forest owner gets certified.

Slightly over one third of the forest owners in our sample reached up to high school and most of them have at least 6 years of experience managing the forest. In terms of participation in forest associations, 40% of the sample does not belong to any association, whereas 23% belong to the Southern forest owners' association and 16% to the Middle forest owners' association. Most of the plots have medium soil quality, which is a measure of the forest growth rate per unit of time.

(INSERT TABLE 2)

3.2 Trees and high stumps left

Our analysis is based on Polytax 5/7 for the period 1992-2010. This dataset is similar to Polytax 0/1 but includes a different sample of randomly chosen plots and its main purpose is to assess the regeneration conditions. In this case, field visits are made 5 years after the felling in the south and 7 years in the north of Sweden. Polytax 5/7 includes measures of two additional outcomes that are relevant in terms of ecological value, which allows for a more comprehensive analysis of the certification effects. Our outcome variables correspond to the number of trees and high stumps left (per cleared hectare) and the corresponding probabilities of non-compliance. As the certification requires leaving at least 10 trees and 3 high stumps, we define non-complying plots as those with less than these amounts of trees per cleared hectare.

The Polytax 5/7 sample comprises 2,616 observations. In Table 3, we describe this sample, after excluding 1% of the observations due to missing or invalid information on key variables. The average plot size is 6.7 ha, the same size as the average plot in Polytax 0/1. On average, forest owners leave 8.8 trees and 1.4 high stumps left per hectares of cleared forest, which is less than the numbers required by the certification. Also, the rate of non-compliance with the expected density is 71% for trees and 82% for high stumps. These rates of non-compliance are by all means high but consistent with reports indicating that failure to leave biodiversity trees and dead wood is the most common transgression of certification standards (see Hirschberger 2005). Non compliers leave only 3.3 trees and 0.5 high stumps per hectare, on average. Notably, the profile of the forest owner is consistent with the Polytax 0/1.

Through the phone survey, we successfully collected information for 1,231 plots (response rate 47%). After removing observations lacking information regarding certification status or felling year, and outliers and missing values, we obtain a final sample of 1,065 observations. In Table 2, we compare our final sample with the characteristics of the Polytax 5/7. Similarly to the Polytax 0/1 analysis, we found that our final sample resembles the population of inspected plots in terms of size, and non-compliance rate and magnitude. However, once again we find differences in the demographics of the forest owners, which is not surprising as the sampling procedures for both databases are similar. Interestingly, the direction of the differences is analogous to the Polytax 0/1, as for example there are fewer females and older owners in our sample.

Similarly to the previous analysis, we classify the observations according to their certification status at the time of clearing. Due to a small sample size in each of the categories previously defined, in this case we define only one treatment and one control group[x]. The treated group corresponds to all plots ever certified (619 plots). This slightly modifies the interpretation of the results as we cannot control for the type of certification. In the control group, we include the 446 plots that were not certified during the period of analysis.

(INSERT TABLE 3)

3.3 Set-aside areas

The database utilized in our study corresponds to the set-aside area survey conducted by the Swedish Forest Agency. In the survey, a random sample of small and medium-size forest owners is selected for a phone survey asking specifically about forest land voluntarily set aside for conservation purposes. Even if this data has been collected several times since 1996, we focus on the survey conducted 2009–2010 because it is the only survey that has included the certification status of the forest owner.

As mentioned above, the units of observation are small and medium-size forest owners (including social and local associations, as for instance, municipalities, foundations, religious communities, and economic associations). The survey includes information on the municipality in which the land is located, whether there are voluntary set-aside areas, the size of voluntary set-aside areas, whether the forest owner is certified, and, if so, for how long. We complemented this dataset with information from the Swedish Forest Agency on the total area that the forest owner owns within a municipality. With this information, we define the share of total land that is set aside voluntarily for conservation purposes. We then compare whether the average shares of set-aside areas differ between certified and non-certified forest owners.

Unfortunately, the data in this survey does not include sufficient information on other characteristics of the land or the forest owners for us to use as control variables. Fortunately, location allows us to control for some of the geographical variation, and size of the plot allows us to control for variations in the opportunity cost of setting aside areas for conservation purposes, as opportunity cost might vary with scale. Nonetheless, our results should be interpreted with caution, since the observed differences in the share of set-aside area could be explained by other characteristics omitted from this analysis due to lack of information.

The dataset includes 327 small and medium-size private forest owners who participated in the survey. We exclude forest owners with missing information or no set-aside areas, for a final sample size of 283 (86%) observations with valid information on the variables of interest. Of those, 95 observations do not hold a forest certification, 57 are certified by FSC, and 131 are certified by PEFC. Table 4 presents descriptive statistics for the relevant variables. We observe that on average, forest owners set aside 8.8% of their forested area for conservation purposes, which is higher than the 5% required by the standard. Moreover, approximately 60% of our sample set aside more than 5%, while 40% set aside less than required by the standard. We look at the differences in the share of set aside areas between certified and uncertified in Section 5.3.

(INSERT TABLE 4)

4. Empirical Strategy

Our treatment assignment is not random since the certification program is voluntary. In this case, the challenge when trying to identify the causal effect is that if certified plots are systematically different to the uncertified group we cannot attribute all the potential differences in the outcomes to the certification status. Rather, these differences could be explained by other factors, commonly referred to as confounders.

Tables 5 and 6 present the normalized differences in covariates between the control and treated groups for the Polytax 0/1 and 5/7 datasets, respectively. For each characteristic, the normalized difference is defined as the difference in averages by treatment status, scaled by the square root of the sum of the variances. The normalized differences provide a scale and sample size free way of assessing overlap, and compared to the t-statistic, this metric is more useful for assessing the magnitude of the difference between the groups (Imbens 2015). As a rule of thumb, values above 0.25 indicate that the difference between the groups is substantial and hence linear regression methods tend to be sensitive to the specification (Imbens and Rubin 2007). We also report the t-statistic for the difference in means as a reference.

In Panel A of Table 5, we observe that even when the groups are statistically different in many of the characteristics, the magnitude of the difference is substantial only for the covariates related to the geographic characteristics of the plots and participation in forest associations. In particular, there are substantial differences in the quality of the soil, and there is lower road density in the control group. From this analysis, we can conclude that because some of the covariates differ substantially between the treated and control groups, the conventional OLS analysis could be sensitive to specification and outliers (Imbens 2015). Figure A1 in the Appendix shows the location of the certified and uncertified plots in Sweden.

(INSERT TABLE 5)

Similar patterns can be seen for Polytax 5/7 (see Table 6), where the magnitude of the difference is substantial only for the covariates related to road density and the participation in forest associations.

(INSERT TABLE 6)

To account for the potential selection bias, we first fit an OLS regression in which we control for observed heterogeneity by including the covariates:

$$Y_{ijt} = \beta_1\, Treatment_i + \sum_{l=1}^{L} \boldsymbol{\alpha_l Z_{il}} + \sum_{m=1}^{M} \boldsymbol{\delta_m X_{im}} + \gamma_j + \eta_t + u_{ijt},$$

where Y is the outcome for the i-th plot located in the j-th county felled in year t. For the analysis of the environmentally important areas, we test different definitions for the outcome: the cleared area (measured in absolute and relative terms) and the non-compliance rate. As the cleared area follows a log-normal distribution, we also show the results with the inverse hyperbolic sine transformation as outcome (see footnote V). Note that by defining the cleared area in relative terms, we include only those plots with a positive initial value of environmentally important areas. For the analysis of the number of trees and high stumps left after felling, we define the dependent variable as number of trees left per cleared hectare, and we also consider the non-compliance rates.

Both in the case of the analysis of environmentally important areas and in the case of the number of trees and high stumps left, the coefficient of interest is β_1, which measures impact of certification on the outcome. $Treatment$ is a dummy variable taking the value 1 if the plot is certified and otherwise 0 according to the previously defined treatment groups. $\boldsymbol{Z}$ is a vector of L forest owner characteristics, and $\boldsymbol{X}$ is a vector of M plot characteristics. γ_j, η_t are county and felling year fixed effects respectively, and u_{ijt} is the error term. We estimate this model with OLS, where β_1 captures the average treatment effect, but we also present the average treatment effect on the treated (ATT) with OLS following Wooldridge (2010). The ATT estimator finds the average treatment effect for the certified plots and is useful for comparing the results with the matching estimator (see below).

In addition to the OLS estimator, we use matching to construct a control group that mimics the treated group in all relevant observable characteristics. This matched control group is intended to resemble the counterfactual, i.e., what would have happened had the treated group not received the certification. One advantage of matching over OLS is that the results are less sensitive to the specification of the functional form (see, e.g., Imbens 2015).

Following Imbens (2015), we first pre-process the data in order to obtain a more balanced sample. This will ensure that the results are more robust with any estimator (see, e.g., Rosenbaum and Rubin 1983; Imbens 2015). We trim the sample based on the propensity score matching estimated using one neighbor, a caliper of 0.01 and all covariates listed in Tables 5 and 6 in addition to county and year fixed effects. We choose this caliper value to select the observations that are closest in terms of propensity score in order to reduce the selection bias (Caliendo and Kopeinig 2008). In trimming the sample, we also drop treatment observations whose propensity score is higher than the maximum or less than the minimum propensity score of the controls.

This procedure dropped 54 (22%) and 66 (11%) treated observations for the Polytax 0/1 and Polytax 5/7 respectively[xi]. In both cases, observations with a propensity score higher than 0.94 were dropped in order to ensure overlap (see Figures A2 and A3 in Appendix). For the main results we present the results for matching with trimming, but in Tables A2 and A3 we show the results for the full sample as robustness test, as well as for alternative methods for trimming the sample and different values of the caliper.

Although in estimating the selection into the treatment model we are primarily interested in obtaining the propensity score values to trim the sample, the results can shed some light on what explains the certification decision. We present the coefficients for treatment assignment in Table A4 in the Appendix, where we show that there is substantial geographic and time variation in the certification rates. In particular, certification is more likely in southern counties and in more recent years. In addition, participation in forest association is an important predictor with members having 23% higher probability to be certified compared to non-participants.

We then use bias adjusted covariate matching (CVM) on the new sample obtained after trimming to estimate the average treatment on the treated (ATT) (Imbens 2015). For the CVM we consider one neighbor, and observations are matched using the diagonal matrix of the inverse sample standard errors of the covariates. By using bias adjusted matching, the estimator will remove some of remaining bias that could result from having unbalanced covariates after matching through a regression on the same set of covariates (Abadie and Imbens 2012). Compared to other matching estimators, CVM has the advantage that the standard errors are consistently estimated (Abadie and Imbens 2006).

Matching as a strategy to control for covariates is motivated by the assumption that conditional on observed characteristics, the potential outcomes are independent of the treatment assignment. We verify the plausibility of the unconfoundedness assumption in Table A6 in the Appendix. Also, we verify the sensitivity of our results to the presence of a potential unobserved factor by estimating the Rosenbaum bounds (Rosenbaum 2002) in Table A7.

5. Results

In this section we report the results of our analysis for each of the three environmental outcomes under evaluation. We start by presenting the effects of the certification on the environmentally important areas. For this first component, we first describe the effects of certification for the whole sample and then by certification status. Next, we present the results for the number of trees and high stumps. We close by presenting the analysis of the set-aside areas and running robustness checks.

5.1 Forest Certification and Conservation of Environmentally Important Areas

Tables 7 and 8 present the main results for the environmentally important areas. In Table 7 we address the question: Does forest certification affect the probability and magnitude of compliance with preservation of environmentally important areas during the felling? In columns we compare the results obtained with different estimation strategies, and in rows we specify different definitions for the outcome and for the set of control variables included in the model.

In the first column and row of Table 7, we show the difference in means for the non-compliance rate between the treatment and the control. We observe that certified plots have a 3.7% lower probability of non-compliance compared with non-certified plots, but this difference is not statistically significant. Similarly, we present this test for the other outcomes and observe that, although certified plots have a smaller damaged area both in absolute and relative terms, these differences are not statistically different from zero either.

In fact, when we account for the spatial and temporal variability, we find higher but non-significant non-compliance rates and magnitudes in the certified group. This result holds when we add additional observed characteristics that could confound the effect, when computing the average treatment effect on the treated (ATT), and with matching (Columns 2 to 5). In Table A2 we show that these findings are robust whether we trim the sample or not, and to the caliper used to pre-process the data. We note that the magnitude of the coefficients is small in all estimation methods. Hence, even in a scenario of lack of statistical power to detect an effect given the sample size, the magnitude of the effects suggest a small difference between treated and control groups.

(INSERT TABLE 7)

In Table 8, we look at the effects of each certification standard separately and test whether there are any differences between them. We also test whether holding both certifications simultaneously has an effect on the outcomes. We compare the treatment effects obtained by OLS including all covariates. We find the same results: there is no evidence that FSC or PEFC certification plots decrease the probability or the magnitude of non-compliance. Also, we observe no difference between the labels.

(INSERT TABLE 8)

Thus, our analysis shows no evidence to support the hypothesis that forest certification increases the preservation of environmentally important areas during felling. Furthermore, we find no difference between certification labels. This might not be a surprising result since the certification requirements by FSC and PEFC are rather similar and the requirements of both FSC and PEFC coincide with those established by the Swedish Forestry Act. This raises, however, questions regarding the value-added of multiple certifications and label competition for overall environmental protection. Further, there is the question of the overall compliance with the Swedish Forestry Act and of the reliance on certification schemes to provide additional economic incentives to comply with environmental regulations. Our results show that such incentives are marginal and ineffective.

5.2 Forest Certification and the Number of Trees and High Stumps Left

To analyze the effects of forest certification on the number of trees and high stumps left, we conduct a similar analysis as in the previous section. We present the results in Table 9, where we present the results for the number of trees left and the non-compliance rates with both OLS and matching estimates on the full and trimmed sample. A simple difference in means test shows that more trees are left and non-compliance is lower in certified plots. However, these differences vanish once we account for the spatial and time variation. When controlling for additional covariates and computing the matching estimators, we consistently find zero effect of certification on the outcomes.

(INSERT TABLE 9)

Together, these results suggest that there is no evidence that certification has a significant effect on the non-compliance rate or on the number of trees or high stumps left. This is a disappointing result since scientific studies have shown that there is no generally accepted single threshold value for the proportion of deciduous trees required for conserving viable populations of species (Johansson et al. 2013). Nevertheless, most studies suggest thresholds that are much higher than those established by the Swedish Forestry Act and certification schemes (see e.g., Butler et al. 2004 and Ranius 2006). The lax requirements by certification schemes and the lack of compliance with them can have significant detrimental effects on the survival of species dependent on continuous input of dead wood at stand level. Moreover, our results suggest that, in order to mitigate the effects of forest management on biodiversity and improve the situation for threatened forest species, the levels in the Swedish Forestry Act and certification standards need to be adapted to the current knowledge on habitat demands and thresholds for demanding species.

5.3 Forest Certification and Set-Aside Areas

Finally, our results show that, on average, certified owners set aside 8.8% of the land. This share is higher than the 5% required by the certification schemes, but is not statistically different to the 7.3% left by non-certified forest owners (see Table 10). Furthermore, we explore whether there are differences between certification types. FSC-certified forest owners set aside 0.9% more area than non-certified forest owners; the corresponding figure for PEFC-certified forest owners is 2.8%. However, these differences are again not statistically significant at conventional levels - even when we control for geographic location by including the forest area and county fixed effects - pointing to a lack of incremental effect of forest certification on the size of the areas set aside for conservation purposes. This is - again - a disappointing result since fragmentation is a major reason for the declining biodiversity in forest ecosystems (Haddad et al. 2015). Besides decreasing habitat area, fragmentation results in increasing isolation of suitable habitats, increasing exposure to negative edge effects and reducing the provision of forest ecosystem services (see, e.g., Chaplin et al. 2015).

(INSERT TABLE 10)

5.4 Robustness Checks

5.4.1 Sample selection bias

We test whether selection bias affects our results as roughly half of the sample did not complete the survey through which we obtained the forest owners' certification status. To account for the systematic differences in the demographic characteristics of the forest owners in our sample with respect to the original Polytax 0/1 and 5/7 datasets, we use the two-stage Heckman model (Heckman 1979) both for Polytax 0/1 and Polytax 5/7. We first use the entire sample of forest owners to estimate the probability that the survey is completed. The dependent variable takes the value of 1 if the survey was completed and zero otherwise. As explanatory variables, we include demographic variables that could explain the response rate and that are available for the entire sample, i.e., gender, age, and owner present.

In the second stage, we correct for selectivity bias by including the inverse Mills ratio obtained from the first stage in the deforestation equation, all control variables related to the geographic characteristics, participation in forest association, education, experience as forest owner, and county and year fixed effects. The assumption is that the variables gender, age, and owner only affect the outcomes through their role in the sample selection, conditional on the variables included in the second stage. This is a reasonable assumption, as we are already controlling for characteristics that determine environmental awareness and access to information such as education, experience and participation in forest associations.

We present the results in Table A5, where we show that both for Polytax 0/1 and 5/7 the Heckman selection model finds an effect of the certification almost identical to the OLS with all covariates (Column 3 in Table 7 and 9). Furthermore, in all cases the lambda parameter is not significantly different from zero, which suggests that unobserved factors that make participation in the survey more likely are not associated with forest management.

5.4.2 Unconfoundedness

Following the test proposed by Imbens (2015), we perform an analysis of the plausibility that our matching method meets the unconfoundedness assumption. We test whether the method results in a sample that is as good as random conditional on the covariates. In Table A6 we present the treatment effect for three "pseudo-outcomes": area of the plot, gender, and age. We follow the same procedure as presented in Section 4.1 for the core results. First, we pre-process the sample matching on the propensity score obtained with the remaining covariates, and then we use covariate matching (CVM) on the trimmed sample to obtain the average treatment effect of the treated, which is known a priori to be zero. For the Polytax 0/1 analysis, we observe that we cannot reject the null hypothesis of zero effect on all the pseudo-outcomes with the full sample, whereas with CVM after trimming all three "pseudo causal effect" are not statistically different from zero. This result indicates that trimming the sample indeed makes the results more robust to the unconfoundedness assumption. For the Polytax 5/7 analysis we find that the unconfoundedness assumption is plausible both for the full and trimmed sample.

5.4.3 Sensitivity test for selection on unobserved factors

We report the Rosenbaum bounds (Rosenbaum, 2002) for hidden bias in Table A7, following the routine for binary outcomes proposed by Becker and Caliendo (2007). This analysis shows how strongly must an unobserved factor affect the selection into the treatment in order to invalidate the matching estimates. When gamma is equal to 1, we are under the no hidden bias scenario and the test statistic shows that there is no effect of certification on the non-compliance rate. If we have a negative unobserved selection, so that compliers do not get certified, our estimated treatment effect underestimates the true treatment effect. In this case the Qmh statistic is too low and should be adjusted upwards.

Table A7 shows the Mantel-Haenszel statistic for the assumption of under-estimation of treatment effect and the corresponding significance level. Overall, we can see that our analysis is fairly robust, as at relatively small values of gamma the result of zero impact holds. For the analysis of the effect of the certification on the non-compliance with the environmentally important areas (Polytax 0/1), our estimates are robust to unobserved factors that increase the odds of certification by a factor of 2.2. Similarly, our analysis of the effects of certification on the compliance with the number of trees left per cleared hectare is robust to unobserved factors that increase the odds of certification by a factor of 1.8.

6. Conclusions

Reconciling timber production with biodiversity protection in private forest is a challenge since the supply of biodiversity usually goes unrewarded by markets, and protection of biodiversity comes at an opportunity cost to forest owners. This makes it unlikely to achieve biodiversity protection in the absence of further incentives to compensate forest owners for the potential productivity losses. Forest certification has been proposed as an alternative to provide assurance to a mass of environmentally concerned consumers that certified forest products come from a forest management that maintain and/or enhance biodiversity protection.

In this paper, we investigate the effects of the two main forest certification schemes in Sweden, FSC and PEFC, on three key environmental outcomes embedded in the certification standards, namely environmentally important areas preserved during felling, number of trees and high stumps left after felling, and area set aside for conservation purposes. These environmental outcomes are relevant to avoid forest degradation and to ensure sustainable management of the boreal forests. Our main result is that, compared to the performance of similar non-certified forest owners, certification has not led to any additional improvements in these outcomes, and hence it has not contributed to reducing forest degradation in Sweden.

We conclude that although certification as a sustainable forest management policy is rewarded with price premiums and improved market access, certified forest owners are not significantly more likely to preserve areas of high conservation value compared to similar non-certified forest owners. Furthermore, there is no difference between the certification schemes, which is not surprising given the similarity of the standards. In contrast, the geographic location of the plots, soil productivity, and participation in forest associations seem to be key factors explaining compliance with certification standards and the selection into treatment.

Our results are robust to the model choice and to various alternative definitions of the treatment and outcome variables. They contribute to the evidence that forest certification is generally not associated with increased environmental benefits (see e.g., Blackman et al. 2015, Rico Staffron 2015, and Panlasigui 2015). Nevertheless, in contrast to previous studies that have focused on deforestation, our study provides evidence of the lack of effect of certification on avoided degradation *vis-à-vis* the performance of comparable non-certified forest owners.

Furthermore, while previous studies have analyzed the effects of certification in the context of developing countries, we focus on the case of Sweden, a developed country with the largest total area of certified forest in Western Europe. We acknowledge that national forest certification standards are the result of voluntary negotiations among stakeholders with different goals and power, and, hence, are context-dependent, because countries and regions have different forest–industrial regimes. Nevertheless, the fact that empirical evidence has shown that the effects on certification are limited both in the context of developing and developed countries raises concerns about the role of forest certification as a tool to promote sustainable forest management practices.

Our results indicate that, if forest certification in Sweden is to have an effect, it needs to become more stringent, not only when it comes to the standards but also in terms of monitoring and enforcement, because neither certified nor non-certified owners are in compliance with the environmental outcomes studied in this paper.

Indeed, even though identifying the mechanisms that explain our results goes beyond the scope of our analysis, the high rates of non-compliance might be the result of the lack of clear definitions and quantifiable measures in the Swedish Forestry Act regarding what constitute sustainable forest management. Although the Swedish Forestry Act states that preservation of natural and environmental values should be prioritized to the same extent as forest production values, the lack of clear and quantifiable measures makes it difficult for the certifiers to implement standards that are stringent enough and legitimized by society.

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Table 1: Polytax 0/1. Descriptive statistics

	All sample N=3005				Final sample N=763		Certified N=247	Non-certified N=516	FSC[a] N=53	PEFC[b] N=75	Both N=82
	Mean	Std. Dev.	Min	Max	Mean	Difference (All-Final)	Mean	Mean	Mean	Mean	Mean
Plot characteristics											
Area requested for felling (ha)	6.705	8.231	0.500	120.000	6.480	0.302	5.752	6.828	7.766	5.672	5.037
Environmentally important area											
Before clearing (ha)	0.706	1.695	0.000	39.500	0.673	0.045	0.502	0.755	0.788	0.447	0.415
After clearing (ha)	0.497	1.393	0.000	39.500	0.474	0.031	0.341	0.537	0.422	0.305	0.319
Non-compliance rate (0/1)	0.641	0.480	0.000	1.000	0.649	-0.010	0.623	0.661	0.755	0.560	0.598
Environmental damage (ha)	0.209	0.849	0.000	21.915	0.199	0.014	0.161	0.217	0.366	0.142	0.096
Damage/area before clearing	0.288	0.349	0.000	1.000	0.273	0.019	0.266	0.277	0.342	0.301	0.209
Non-compliers only											
Environmental damage (ha)	0.326	1.042	0.002	21.915	0.307	0.026	0.258	0.329	0.485	0.254	0.161
Damage/area before clearing	0.420	0.350	0.000	1.000	0.395	0.034*	0.397	0.394	0.454	0.473	0.320
Forest owner characteristics											
Female (0/1)	0.242	0.428	0.000	1.000	0.166	0.101***	0.121	0.188	0.132	0.107	0.134
Single decision maker (0/1)	0.273	0.446	0.000	1.000	0.642	-0.495***	0.587	0.669	0.585	0.627	0.646
Age (years)	59.337	13.003	23.000	94.000	60.308	-1.302***	59.364	60.760	58.698	58.680	60.512
Owner lives near forest plot (0/1)	0.780	0.415	0.000	1.000	0.811	-0.042**	0.838	0.798	0.925	0.827	0.805

Notes: "All sample" refers to the complete sample of plots under the category of non-industrial private forest owners that are randomly selected by the Swedish Forest Agency for forest inventory and monitoring both before and after clearing during the period 1999-2011. We excluded 1% of the observations due to missing or invalid information in any of the variables presented in this table. "Final sample" comprises the plots for which the certification status could be verified. FSC[a] includes plots certified with FSC before clearing but excludes plots that in addition obtained PEFC certification after clearing (20 observations). PEFC[b] includes plots certified with PEFC before clearing but excludes plots that in addition obtained FSC certification after clearing (17 observations). p<0.10, ** p<0.05, *** p<0.01

Table 2: Descriptive statistics of covariates, final sample.
Polytax 0/1

Variable	Mean	Std. Dev.	Min	Max
Forest owners' characteristics				
Female (0/1)	0.166	0.373	0.000	1.000
Single decision maker (0/1)	0.642	0.480	0.000	1.000
Age (years)	60.308	11.390	28.000	90.000
Education up to Highschool (0/1)	0.364	0.482	0.000	1.000
Up to 3 years of higher education (0/1)	0.157	0.364	0.000	1.000
Up to 4 years of higher education (0/1)	0.185	0.388	0.000	1.000
Other education (0/1)	0.037	0.188	0.000	1.000
Medium level of expericence (6 to 40 years) (0/1)	0.767	0.423	0.000	1.000
High level of experience (more than 40 years) (0/1)	0.180	0.384	0.000	1.000
Owner lives in or closeby the forest plot (0/1)	0.811	0.392	0.000	1.000
Plots' characteristics				
Area of the plot (ha)	6.480	8.102	0.500	64.500
High soil quality (0/1)	0.282	0.450	0.000	1.000
Medium soil quality (0/1)	0.655	0.476	0.000	1.000
Forest associations				
Sothern (0/1)	0.227	0.419	0.000	1.000
Middleforest (0/1)	0.163	0.369	0.000	1.000
Northforest (0/1)	0.072	0.259	0.000	1.000
North forest owners' (0/1)	0.087	0.281	0.000	1.000
Other (0/1)	0.043	0.204	0.000	1.000
None (0/1)	0.409	0.492	0.000	1.000
Municipality characteristics				
road density (m/km2)	782.302	327.601	59.695	1704.137

Table 3: Polytax 5/7. Descriptive statistics

	All sample (Polytax 5/7) N=2593				Final sample N=1065		Certified N=619	Non-certified N=446
	Mean	Std. Dev.	Min	Max	Mean	Difference (All-Final)	Mean	Mean
Plot characteristics								
Area requested for felling (ha)	6.734	8.105	0.500	94.200	6.786	-0.088	6.364	7.371
Number of trees left per hectare of cleared forest	8.813	12.182	0.000	180.534	0.697	0.142	9.356	7.859
Number of high stumps per hectare of cleared forest	1.427	2.296	0.000	19.111	0.812	-0.109	1.662	1.255
Non-compliance rate (trees left)	0.708	0.455	0.000	1.000	8.729	0.019	0.661	0.747
Non-compliance rate (high stumps)	0.822	0.382	0.000	1.000	1.492	0.017	0.790	0.843
Non-compliers only								
Number of trees left per hectare of cleared forest	3.350	2.965	0.000	9.976	3.374	-0.040	3.256	3.519
Number of high stumps per hectare of cleared forest	0.553	0.881	0.000	2.989	0.557	-0.007	0.595	0.508
Forest owner characteristics								
Female (dummy)	0.240	0.427	0.000	1.000	0.176	0.110***	0.168	0.186
Single decision maker (dummy)	0.290	0.454	0.000	1.000	0.638	-0.591***	0.601	0.691
Age (years)	59.957	12.971	10.000	99.000	60.023	-0.112	60.047	59.989
Owner lives near forest plot (0/1)	0.802	0.399	0.000	1.000	0.823	-0.037**	0.832	0.812

Notes: All sample refers to the complete sample of plots under the category of non-industrial private forest owers that are randomly selected by the Swedish Forest Agency for forest inventory and monitoring both before and after clearing during the period 1999-2011. We excluded 1% of the observations due to missing or invalid information in any of the variables presented in this table. p<0.10, ** p<0.05, *** p<0.01

Table 4: Descriptive statistics set-aside areas

Variable	Obs	Mean	Std. Dev.	Min	Max
All sample					
Set aside area (ha)	283	148.456	879.565	0.400	12588.000
Productive forest (ha)	283	1117.629	3432.936	7.000	38728.000
Set aside area (%)	283	0.088	0.121	0.000	0.987
Obs with 5% or less of set aside area	113	0.028	0.015	0.000	0.050
Obs with more than 5% of set aside area	170	0.128	0.142	0.050	0.987
Any certification					
Set aside area (ha)	188	118.198	510.169	0.500	5343.000
Productive forest (ha)	188	1036.766	2230.048	7.000	16145.000
Set aside area (%)	188	0.096	0.123	0.003	0.987
Non-certified					
Set aside area (ha)	95	208.334	1340.774	0.400	12588.000
Productive forest (ha)	95	1277.653	5042.108	16.000	38728.000
Set aside area (%)	95	0.073	0.116	0.000	0.867
Certified FSC					
Set aside area (ha)	57	145.027	281.847	0.600	1378.000
Productive forest (ha)	57	1869.737	3115.004	7.000	16145.000
Set aside area (%)	57	0.083	0.058	0.004	0.242
Certified PEFC					
Set aside area (ha)	131	106.525	582.855	0.500	5343.000
Productive forest (ha)	131	674.328	1592.852	8.000	12883.000
Set aside area (%)	131	0.101	0.142	0.003	0.987

Table 5: Means, t-test stat, and normalized difference in covariates between the control and treated groups. Before and after matching. Polytax 0/1 dataset.

	Mean: Non-certified	Mean: Certified	Difference: t-stat	Difference: Normalized
A. Before Matching				
Number of observations	516	247	.	.
Female (0/1)	0.188	0.121	-2.432	-0.129
Single decision maker (0/1)	0.669	0.587	-2.147	-0.119
Age (years)	60.760	59.364	-1.622	-0.088
Education up to Highschool (0/1)	0.347	0.401	1.427	0.079
Up to 3 years of higher education (0/1)	0.157	0.158	0.033	0.002
Up to 4 years of higher education (0/1)	0.186	0.182	-0.129	-0.007
Other education (0/1)	0.045	0.020	-1.890	-0.097
Medium level of expericence (0/1)	0.756	0.789	1.044	0.057
High level of experience (0/1)	0.182	0.174	-0.273	-0.015
Owner lives in or closeby the forest plot (0/1)	0.798	0.838	1.342	0.072
Area of the plot (ha)	6.828	5.752	-1.855	-0.097
High soil quality (0/1)	0.341	0.158	-5.490	-0.292
Medium soil quality (0/1)	0.581	0.810	6.306	0.340
Forest associations (0/1)	0.477	0.830	8.928	0.491
Road density (log)	6.439	6.779	8.162	0.446
B. After Matching				
Number of observations	102	193	.	.
Female (0/1)	0.127	0.135	0.176	0.015
Single decision maker (0/1)	0.598	0.611	0.222	0.019
Age (years)	58.392	59.218	0.581	0.051
Education up to Highschool (0/1)	0.392	0.378	-0.232	-0.020
Up to 3 years of higher education (0/1)	0.098	0.155	1.446	0.121
Up to 4 years of higher education (0/1)	0.216	0.171	-0.908	-0.080
Other education (0/1)	0.039	0.026	-0.592	-0.053
Medium level of expericence (0/1)	0.716	0.777	1.135	0.099
High level of experience (0/1)	0.176	0.181	0.104	0.009
Owner lives in or closeby the forest plot (0/1)	0.853	0.839	-0.308	-0.026
Area of the plot (ha)	6.210	5.932	-0.280	-0.025
High soil quality (0/1)	0.216	0.192	-0.481	-0.042
Medium soil quality (0/1)	0.716	0.772	1.038	0.091
Forest associations (0/1)	0.657	0.793	2.408	0.212
Road density (log)	6.608	6.712	1.828	0.161

Notes: Counties for which normalized difference > 0.25 before matching: 2; after matching: 0. Total number of counties: 21

Table 6: Means, t-test stat, and normalized difference in covariates between the control and treated groups. Before and after matching.
Polytax 5/7 dataset.

	Mean: Not certified	Mean: Certified	Difference: t-stat	Difference: Normalized
A. Before Matching				
Number of observations	446	619	.	.
Female (0/1)	0.186	0.168	-0.759	-0.033
Single decision maker (0/1)	0.691	0.601	-3.019	-0.132
Age (years)	59.989	60.047	0.077	0.003
Education up to Highschool (0/1)	0.444	0.433	-0.356	-0.016
Up to 3 years of higher education (0/1)	0.132	0.152	0.906	0.040
Up to 4 years of higher education (0/1)	0.146	0.174	1.267	0.055
Other education (0/1)	0.045	0.034	-0.893	-0.040
Medium level of expericence (0/1)	0.769	0.763	-0.249	-0.011
High level of experience (0/1)	0.159	0.202	1.799	0.078
Owner lives in or closeby the forest plot (0/1)	0.812	0.832	0.851	0.038
Area of the plot (ha)	7.371	6.364	-1.754	-0.078
Forest associations (0/1)	0.433	0.798	11.392	0.497
Road density (log)	6.441	6.671	6.957	0.307
B. After Matching				
Number of observations	201	553	.	.
Female (0/1)	0.199	0.179	-0.613	-0.036
Single decision maker (0/1)	0.647	0.626	-0.533	-0.031
Age (years)	60.557	59.964	-0.573	-0.034
Education up to Highschool (0/1)	0.468	0.447	-0.511	-0.030
Up to 3 years of higher education (0/1)	0.119	0.146	0.986	0.056
Up to 4 years of higher education (0/1)	0.139	0.159	0.683	0.039
Other education (0/1)	0.040	0.029	-0.698	-0.042
Medium level of expericence (0/1)	0.776	0.779	0.095	0.006
High level of experience (0/1)	0.174	0.181	0.213	0.012
Owner lives in or closeby the forest plot (0/1)	0.811	0.826	0.482	0.028
Area of the plot (ha)	7.213	6.422	-0.923	-0.058
Forest associations (0/1)	0.582	0.774	4.797	0.284
Road density (log)	6.522	6.647	2.927	0.174

Notes: Counties for which normalized difference > 0.25 before matching: 0; after matching: 0. Total number of counties: 21

Table 7: Effects of a forest certification on environmentally important areas. Different model specifications in columns and different outcome specifications and covariates in rows.
Polytax 0/1 dataset

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Difference in means	OLS (year & county FE)	OLS (all covariates)	ATT with OLS (all covariates)	ATT with CVM on trimmed sample
Non-compliance rate	-0.037	0.013	0.010	0.012	0.072
Standard error	[0.037]	[0.042]	[0.045]	[0.052]	[0.086]
Observations	763	763	763	763	295
R-squared	0.001	0.134	0.161	0.21	
Area cleared (ha)	-0.057	0.043	0.038	0.059	0.017
Standard error	[0.069]	[0.093]	[0.086]	[0.059]	[0.115]
Observations	763	763	763	763	295
R-squared	0.001	0.029	0.091	0.157	
Area cleared (IHS)	-0.040	0.007	0.000	0.031	-0.018
Standard error	[0.025]	[0.033]	[0.032]	[0.037]	[0.057]
Observations	763	763	763	715	295
R-squared	0.003	0.041	0.109	0.163	
Area cleared (%)	-0.011	0.019	0.023	0.031	0.075
Standard error	[0.028]	[0.033]	[0.035]	[0.037]	[0.086]
Observations	715	715	715	715	273
R-squared	0.000	0.067	0.085	0.163	

Notes: For simplicity we present the linear probability model (OLS), but the results also hold with non-linear probability models (Probit). All covariates include all variables listed in Table 2 and county and year fixed effects. Variables used for the matching: all variables listed in Table 5 and county and year fixed effects. Bias adjustment in CVM includes all covariates and county and year fixed effects. Covariate matching includes one neighbor and estimates the average treatment effect on the treated. The sample includes 21 counties and 13 years. Area cleared (%) is the change in the environmentally important areas divided by the initial value. Reported standard errors are Robust (White, 1980) for OLS, and Abadie and Imbens (2006, 2012) standard errors for covariate matching. IHS: Inverse Hyperbolic Sine Transformation. ATT: average treatment on the treated. CVM: covariate matching. FE: fixed effects. OLS: ordinary least squares. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Effects of certification on environmentally important areas by certification label. Polytax 0/1 dataset.

	(1)	(2)	(3)	(4)
Dependent variable	**FSC=1, Uncertified=0**	**PEFC=1, Uncertified=0**	**Both FSC and PEFC=1, uncertified=0**	**FSC=1, PEFC=0**
Non-compliance rate	0.055	-0.026	0.015	0.073
Standard error	[0.070]	[0.059]	[0.075]	[0.115]
Observations	569	591	598	128
R-squared	0.149	0.178	0.154	0.501
Area cleared (ha)	0.193	0.007	0.058	0.316
Standard error	[0.264]	[0.057]	[0.073]	[0.265]
Observations	569	591	598	128
R-squared	0.097	0.089	0.088	0.519
Area cleared (%)	0.063	0.042	0.003	-0.025
Standard error	[0.058]	[0.051]	[0.052]	[0.085]
Observations	538	551	560	119
R-squared	0.113	0.106	0.096	0.529

Notes: All models are OLS including all covariates.

Table 9: Effects of certification on environmental outcomes 5–7 years after clearing. Polytax 5/7 dataset.

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Difference in means	OLS (year and county FE)	OLS (all covariates)	ATT with OLS (all covariates)	ATT with CVM on trimmed sample
Trees left/ha	1.498**	0.679	0.888	-0.778	-2.198
Standard error	[0.691]	[0.828]	[0.894]	[1.062]	[1.451]
Observations	1,065	1,065	1,065	1,065	754
R-squared	0.004	0.146	0.161	0.216	
High stumps left/ha	0.407***	0.237	0.106	0.183	0.126
Standard error	[0.143]	[0.149]	[0.156]	[0.210]	[0.261]
Observations	1,065	1,065	1,065	1,065	754
R-squared	0.007	0.155	0.169	0.200	
Non-compliance rate					
Trees left/ha	-0.086***	-0.052*	-0.056*	-0.015	0.038
Standard error	[0.028]	[0.030]	[0.033]	[0.042]	[0.053]
Observations	1,065	1,065	1,065	1,065	754
R-squared	0.008	0.110	0.125	0.157	
High stumps left/ha	-0.053**	-0.024	-0.000	-0.012	0.020
Standard error	[0.024]	[0.025]	[0.027]	[0.036]	[0.045]
Observations	1,065	1,065	1,065	1,065	754
R-squared	0.004	0.124	0.136	0.168	

Notes: Non-compliance is defined as plots with less than what is requested by the certification standards: 10 trees and 3 high stumps per cleared hectare. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Effects of certification on share of set-aside areas

Variable	(1) Any certification	(2) Only FSC	(3) Only PEFC	(4) Any certification	(5) Only FSC	(6) Only PEFC
Certified (dummy=1 if yes)	0.0224	0.0096	0.0280	0.0203	0.0112	0.0272
	[0.015]	[0.016]	[0.018]	[0.017]	[0.019]	[0.020]
Area of productive forest (thousand ha)				0.0047**	0.0023	0.0059**
				[0.002]	[0.002]	[0.003]
Constant	0.0731***	0.0731***	0.0731***	0.0406	0.0491	0.0789
	[0.012]	[0.010]	[0.014]	[0.043]	[0.043]	[0.055]
Control variables included	NO	NO	NO	YES	YES	YES
Observations	283	152	226	283	152	226
R-squared	0.008	0.002	0.011	0.061	0.100	0.085

Notes: Standard errors in brackets. Control variables include county fixed effects. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX

Figure A1: Location of certified and uncertified within Sweden.
Polytax 0/1

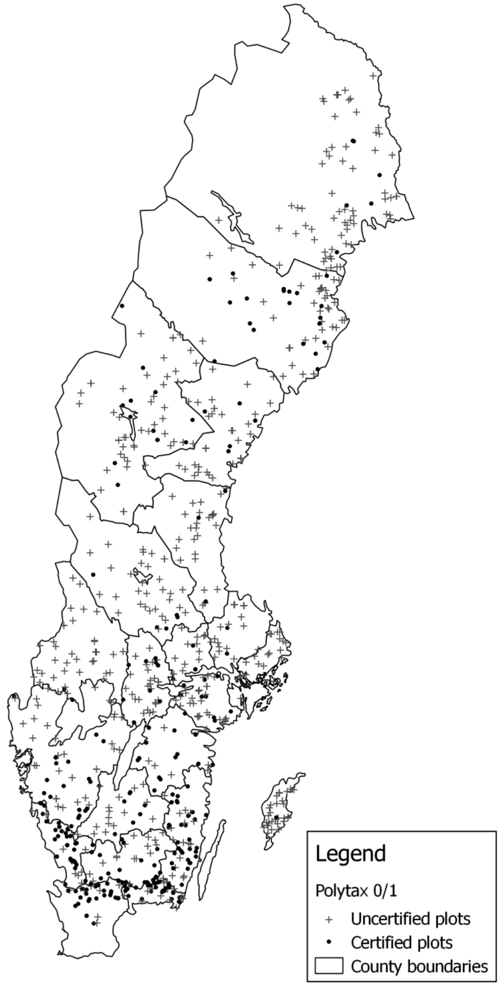

Figure A2: Histogram of the propensity score by treatment status
Polytax 0/1

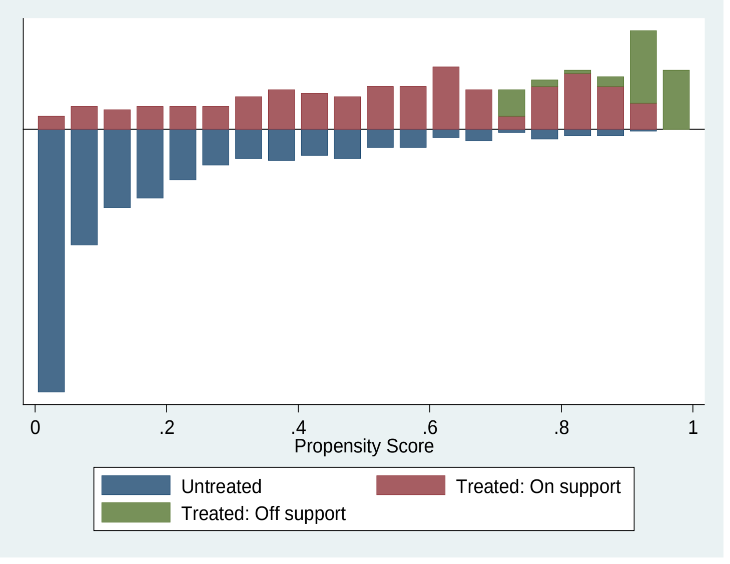

Figure A3: Histogram of the propensity score by treatment status
Polytax 5/7

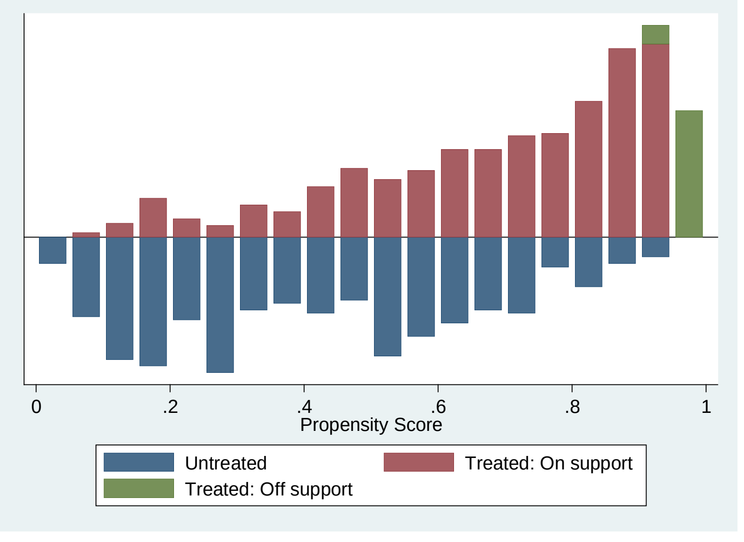

Table A1: Main motivation behind (non)certification:

What is your main reason for not choosing to certify your forest? (mark only one option)	Freq	Percent	Cum
1: No, or not enough, information about certification	101	20	20
2: Too expensive, not economically viable	39	8	27
3: Demands too tough, cannot live up to them, do not want to create lock-in effects	25	5	32
4: Not good for the environment, does not lead to sustainable forestry	7	1	33
5: Do not know anyone who is certified	7	1	35
6: Too small acreage	68	13	48
7: Other	207	40	88
8: Do not know	60	12	100
Total	514	100	
What was your main reason for becoming certified? (mark only one option)			
1: Good PR	19	3	3
2: Economically viable	293	45	48
3: Reasonable demands that are possible to fulfill	24	4	51
4: Good for the environment, leads to more sustainable forestry	153	23	75
5: Know someone / some certified	10	2	76
6: Other	119	18	95
7: Do not know	36	6	100
Total	654	100	

Table A2: Robustness test on the trimming method. Polytax 0/1 dataset.

	Non-compliance rate	Area cleared (ha)	Area cleared (IHS)	Area cleared (share)
Full sample (N=763)				
OLS	0.01	0.038	0.000	0.023
	[0.045]	[0.086]	[0.032]	[0.035]
ATT with CVM	0.042	0.028	-0.003	0.014
	[0.059]	[0.083]	[0.040]	[0.043]
No caliper (N=718)				
OLS	0.012	0.032	-0.004	0.018
	[0.045]	[0.086]	[0.032]	[0.035]
ATT with CVM	0.037	0.021	-0.013	0.011
	[0.060]	[0.087]	[0.042]	[0.044]
Caliper 0.1 (N=718)				
OLS	0.012	0.032	-0.004	0.018
	[0.045]	[0.086]	[0.032]	[0.035]
ATT with CVM	0.037	0.021	-0.013	0.011
	[0.060]	[0.087]	[0.042]	[0.044]
Caliper 0.02 (N=710)				
OLS	0.013	0.030	-0.005	0.018
	[0.045]	[0.087]	[0.032]	[0.035]
ATT with CVM	0.066	0.025	-0.013	0.015
	[0.060]	[0.088]	[0.042]	[0.042]
Caliper 0.01 (N=690)				
OLS	0.016	0.035	-0.004	0.022
	[0.045]	[0.088]	[0.033]	[0.036]
ATT with CVM	0.059	0.029	-0.011	0.016
	[0.059]	[0.087]	[0.039]	[0.042]

Table A3: Robustness test on the trimming method. Polytax 5/7 dataset.

	Trees left/ha	High stumps left/ha	Trees left (compliance rate)	High stumps left (compliance rate)
Full sample (N=1065)				
OLS	0.888	0.106	-0.056*	-0.000
	[0.894]	[0.156]	[0.033]	[0.027]
ATT with CVM	-2.095*	-0.149	0.057	0.074*
	[1.147]	[0.241]	[0.043]	[0.039]
No caliper (N=986)				
OLS	0.839	0.117	-0.054	0.000
	[0.894]	[0.156]	[0.033]	[0.027]
ATT with CVM	-2.417*	-0.041	0.032	0.054
	[1.292]	[0.239]	[0.046]	[0.039]
Caliper 0.1 (N=986)				
OLS	0.839	0.117	-0.054	0.000
	[0.894]	[0.156]	[0.033]	[0.027]
ATT with CVM	-2.417*	-0.041	0.032	0.054
	[1.292]	[0.239]	[0.046]	[0.039]
Caliper 0.02 (N=986)				
OLS	0.839	0.117	-0.054	0.000
	[0.894]	[0.156]	[0.033]	[0.027]
ATT with CVM	-2.417*	-0.041	0.032	0.054
	[1.292]	[0.239]	[0.046]	[0.039]
Caliper 0.01 (N=984)				
OLS	0.843	0.117	-0.054*	0.000
	[0.894]	[0.156]	[0.033]	[0.027]
ATT with CVM	-2.386*	-0.035	0.030	0.051
	[1.290]	[0.239]	[0.046]	[0.039]

Table A4: Determinants of certification (marginal effects after probit)
Dependent variable: Certified plot=1; Uncertified plot=0. Polytax 0/1 dataset.

	Marginal effect	Std. Err. (Delta method)	z	P>|z|
Female (0/1)	-0.121	0.036	-3.350	0.001
Single decision maker (0/1)	-0.054	0.028	-1.920	0.054
Age (years)	-0.002	0.001	-1.310	0.189
Education up to Highschool (0/1)	0.060	0.037	1.630	0.104
Up to 3 years of higher education (0/1)	0.056	0.043	1.290	0.198
Up to 4 years of higher education (0/1)	0.044	0.044	1.010	0.313
Other education (0/1)	-0.026	0.073	-0.350	0.723
Medium level of expericence (0/1)	0.082	0.075	1.090	0.277
High level of experience (0/1)	0.060	0.087	0.690	0.492
Owner lives in or closeby the forest plot (0/1)	0.025	0.037	0.690	0.492
Area of the plot (ha)	0.001	0.002	0.710	0.478
High soil quality (0/1)	-0.089	0.075	-1.200	0.230
Medium soil quality (0/1)	-0.080	0.085	-0.940	0.347
Forest associations (0/1)	0.236	0.026	9.020	0.000
road density (log)	0.048	0.049	0.980	0.328
Stockholms County	0.000	(omitted)		
Uppsala County	-0.035	0.123	-0.290	0.774
Södermanlands County	0.084	0.100	0.840	0.401
Östergötlands County	0.359	0.100	3.590	0.000
Jönköpings County	0.213	0.101	2.110	0.035
Kronobergs County	0.293	0.096	3.050	0.002
Kalmar County	0.303	0.089	3.420	0.001
Gotlands County	-0.155	0.135	-1.150	0.250
Blekinge County	0.365	0.103	3.550	0.000
Skåne County	0.475	0.106	4.480	0.000
Hallands County	0.433	0.094	4.600	0.000
Västra Götalands County	0.174	0.086	2.020	0.044
Värmlands County	-0.004	0.099	-0.040	0.966
Örebro County	0.077	0.103	0.750	0.456
Västmanlands County	0.120	0.117	1.030	0.304
Dalarnas County	0.036	0.093	0.390	0.695
Gävleborgs County	-0.031	0.098	-0.320	0.752
Västernorrlands County	0.002	0.089	0.020	0.984
Jämtlands County	0.104	0.071	1.470	0.142
Västerbottens County	0.172	0.069	2.480	0.013

To be continued in next page…

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	Marginal effect	Std. Err. (Delta method)	z	P>|z|
2000	-0.148	0.153	-0.970	0.332
2001	0.020	0.136	0.150	0.884
2002	0.089	0.133	0.670	0.505
2003	0.183	0.133	1.370	0.170
2004	0.177	0.133	1.330	0.184
2005	0.213	0.142	1.500	0.133
2006	0.288	0.128	2.250	0.025
2007	0.267	0.126	2.120	0.034
2008	0.238	0.126	1.880	0.060
2009	0.282	0.125	2.250	0.025
2010	0.274	0.125	2.190	0.029
2011	0.375	0.128	2.940	0.003
2012	0.327	0.150	2.180	0.029

Notes: baseline county: Norrbottens, and baseline year 1999.

Table A5: Heckman sample selection model.

A. Polytax 0/1				
	Non-compliance rate	Area cleared (ha)	Area cleared (IHS)	Area cleared (%)
Certified (0/1)	0.010	0.038	-0.002	0.024
	[0.043]	[0.093]	[0.032]	[0.034]
Selection Equation				
Female (0/1)	-0.329***	-0.329***	-0.329***	-0.318***
	[0.068]	[0.068]	[0.068]	[0.069]
Age (years)	0.008***	0.008***	0.008***	0.008***
	[0.002]	[0.002]	[0.002]	[0.002]
Single decision maker (0/1)	0.122*	0.122*	0.122*	0.116*
	[0.067]	[0.067]	[0.067]	[0.068]
Lambda	0.233	-0.033	0.016	-0.008
	[0.156]	[0.325]	[0.112]	[0.118]
Observations	2,374	2,374	2,374	2,326
Censored obs	1611	1611	1611	1611
Uncensored obs	763	763	763	715
B. Polytax 5/7				
	Trees left/ha	High stumps left/ha	Trees (non-compliance rate)	High stumps (non-compliance rate)
Certified (0/1)	0.834	0.101	-0.055*	0.001
	[0.779]	[0.161]	[0.032]	[0.027]
Selection Equation				
Female (0/1)	-0.320***	-0.320***	-0.320***	-0.320***
	[0.063]	[0.063]	[0.063]	[0.063]
Age (years)	0.002	0.002	0.002	0.002
	[0.002]	[0.002]	[0.002]	[0.002]
Single decision maker (0/1)	0.088	0.088	0.088	0.088
	[0.066]	[0.066]	[0.066]	[0.066]
Lambda	4.595	0.765	-0.042	0.008
	[3.951]	[0.813]	[0.160]	[0.135]
Observations	2,435	2,435	2,435	2,435
Censored obs	1370	1370	1370	1370
Uncensored obs	1065	1065	1065	1065

Table A6: Unconfoundedness assumption.
Polytax 0/1 and Polytax 5/7 datasets

	Full sample			Trimmed sample		
	Plot area	Female	Age	Plot area	Female	Age
Polytax 0/1 Analysis						
certified=1; non-certified=0	1.648**	-0.048	-0.721	0.711	-0.028	-1.423
Standard error	[0.804]	[0.052]	[1.410]	[0.853]	[0.050]	[1.368]
P-value	0.040	0.356	0.609	0.405	0.574	0.298
Observations	763	763	763	690	690	690
Polytax 5/7 Analysis						
certified=1; non-certified=0	-0.182	-0.073	0.475	-1.345	0.017	-0.496
Standard error	[0.893]	[0.048]	[1.251]	[0.957]	[0.045]	[1.295]
P-value	0.839	0.130	0.704	0.160	0.699	0.702
Observations	1,065	1,065	1,065	984	984	984

Table A7: Unobservables: Rosenbaum bounds.
Polytax 0/1 and Polytax 5/7 datasets

	Non-compliance rate (Env important areas) Polytax 0/1		Non-compliance rate (Trees left) Polytax 5/7	
Gamma	Q_mh-	p_mh-	Q_mh-	p_mh-
1	0.257	0.399	0.967	0.167
1.1	0.024	0.490	0.533	0.297
1.2	-0.188	0.575	0.137	0.446
1.3	-0.019	0.508	0.005	0.498
1.4	0.164	0.435	0.340	0.367
1.5	0.336	0.369	0.651	0.258
1.6	0.496	0.310	0.943	0.173
1.7	0.647	0.259	1.218	0.112
1.8	0.790	0.215	1.477	0.070
1.9	0.926	0.177	1.724	0.042
2	1.055	0.146	1.959	0.025
2.1	1.178	0.119	2.183	0.015
2.2	1.296	0.098	2.398	0.008
2.3	1.409	0.079	2.605	0.005
2.4	1.517	0.065	2.803	0.003
2.5	1.622	0.052	2.995	0.001
2.6	1.723	0.042	3.179	0.001
2.7	1.821	0.034	3.358	0.000
2.8	1.915	0.028	3.532	0.000
2.9	2.007	0.022	3.700	0.000
3	2.096	0.018	3.863	0.000

Questionnaire University of Gothenburg

Introduction:

Hello!

(If the person we try to reach is not home, ask if the person you talk to or someone else in the household knows something about the forest. Even forest owners with very small forest holdings should be interviewed.

My name is _____ and I call on behalf of University of Gothenburg who currently carries out a short survey regarding certified forest among forest owners in Sweden.

May I ask you a few short questions regarding forest certification?

Q1. For how long have you been a forest owner?

1: 0-5 years
2: 6–15 years
3: 16–25 years
4: 26–40 years
5: 41-60 years
6: 61 years or longer

Q2. Which forest owner association are you a member of?

1: Södra
2: Mellanskog
3: Norrskog
4: Norra Skogsägarna
5: Other (type):______________________
6: None
7: Don't know

Q3. Is the forest you own wholly or partly certified?

1: Yes, the whole forest
2: Yes, parts
3: No
4: Don't know (To the interviewer: Is there someone else in the household who knows?) If not, go to Q12.

(If 2 in Q3)

Q4. In which municipality/municipalities are the certified forest holdings located? (Open)

(If 1 or 2 in Q3)

Q5. According to which standards is your forest certified? (Multi) Read alternatives

1: FSC
2: PEFC
3: Don't know

(If 1 or 2 in Q3)

Q6. In which year did you certify the forest according to FSC? (Number)

Q6.2. In which year did you certify the forest according to PEFC? (Number)

(If 3 in Q3)

Q7. Which is the <u>main</u> reason behind your choice of not certifying your forest? (Mark one alternative only) Don't read alternatives

1: None or insufficient information about certification
2: Too expensive, not financially profitable
3: Too strict requirements, I can't live up to them, don't want to lock up my forestry
4: Not good for the environment, does not lead up to a more sustainable forestry
5: Don't know anyone who is certified
6: Too small area
7: Other
8 Hesitant, don't know

(If 1 or 2 in Q3)

Q8. Which is the <u>main</u> reason behind your choice of certifying your forest? (Mark one alternative only) Don't read alternatives

1: Good PR for my mission
2: Financially profitable
3: Reasonable requirements, can live up to them
4: Good for the environment, leads up to a more sustainable forestry
5: Know somebody who is certified
6: Other
7: Hesitant, don't know

Q9. Which group certificate are you a part of, that is, through whom is your forest certified? (Mark one alternative only) Read alternatives

1: Through one of the forest associations
2: Through one of the forestry companies (e.g. Sveaskog, Bergvik Skog, Holmen, SCA, Statens fastighetsverk, Stora Enso)
3. Other (type):________________________
4: Don't know

Q10. How do you pay for your forest certification?

1: Lump-sum payment
2: Payment as a member fee/annual fee
3: Hesitant, don't know

Q11. How much do you pay/have you paid? (Number)

If don’t know: 99999

Q12. Who has the main responsibility for decisions regarding your forest holdings? Read alternatives

1: I make the decisions myself
2: I make the decisions together with advisor/manager
3: I make the decisions together with joint owner(s)
4: Advisor/manager makes the decisions
5: Joint owner(s) make the decisions
6: Hesitant, don’t know

Q13. Which is your highest *completed* education?
1: Primary school
2: Secondary education or elementary school
3: Higher education (up to three years)
4: Higher education (up to four years)
5: Other education

These were all the questions. Thank you for your participation!

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[i] Exchange rate €/SEK is 9.15. These estimates are based on 65 valid answers, 50 of which reported having made a one-time payment of 17,421 SEK on average, and the other 15 reported an annual fee of 1,925 SEK on average. These figures must, however, be interpreted with caution since there is great variation in the responses (e.g., for group certificates, the direct costs are determined by each group certificate organization separately and the costs consist of an affiliation fee and a yearly fee). Depending on the size of the forest, the affiliation fee ranges from 550 SEK to 2,500 SEK and the yearly fee from 300 to 3,000 SEK (Prosilva Skogscertifiering AB 2015).

[ii] Small forest owners (with forest properties less than 1000 ha of productive forest land) can be part of a group certificate. The organization in charge of the group certificate is responsible for a large part of the administrative work, which makes it easier for small forest owners to participate. At the moment there are 14 such organizations.

[iii] For instance, if we consider that the average price of Norway spruce sawlogs corresponded to 466 SEK/m3, the price premium corresponds to 23.3 SEK/m3 (Swedish Forest Agency 2014). Also, the forest owner association Södra Skogsägarna and the large forest owner Holmen explicitly state the price premium to FSC certified wood on their websites. Södra Skogsägarna pays extra 10 SEK/m3 for wood that is either certified by FSC or PEFC and 20 SEK/m3 if certified by both labels. Holmen pays 10 SEK/m3 for FSC or PEFC certified wood and 20 SEK/m3 for both labels in Östergötland and Småland. In other areas (e.g. Södermanland and Västmanland) Holmen pays 5 SEK/m3. Translated into percentages, the price premium of FSC (of PEFC) certified wood for Södra Skogsägarna ranges between 1.64 and 2.08 percent. The price premium of wood certified according to both FSC and PEFC ranges between 3.17 and 4.17 percent. For Holmen, the respective ranges are between 0.90 and 1.98 percent, and 3.00 and 3.97 percent.

[iv] Over the period 1999–2011, the rate of inspection has ranged from a minimum of 0.4% of all applications in 1999 to a maximum of 1.07% in 2009 (and an average of 0.6%). The Swedish Forest Agency uses a stratified sample based on geographic location to ensure representation of every county, and within strata they choose the sample randomly. This process results in a slight over-representation of forest in southern Sweden.

[v] We convert the number of unusual trees and shrubs into area multiplying by a factor of 0.0025, as suggested by the Swedish Forest Agency.

[vi] Importantly, the distribution of the area cleared defined in hectares is positively skewed. The bulk of the data is concentrated on the left hand side of the probability distribution, with 35% of the observations concentrated at the zero value and another 60% between zero and 1 hectare. As robustness test, we use the inverse hyperbolic sine transformation of the area cleared, defined as: $\log(\mathrm{y} + (\mathrm{y}^2 + 1)^{\frac{1}{2}})$, where y is the area cleared in hectares. Unlike the log transformation, this function avoids eliminating data as it is defined at zero values.

[vii] The survey was conducted by SIFO, between 2014-04-22 and 2014-05-09

[viii] It was not possible to contact the other 28% of the listed forest owners because of several other reasons, including unidentified, wrong, blocked, or non-existent phone numbers.

[ix] In particular, we dropped: i) observations lacking information on certification status or year of certification (183 observations); ii) outliers and observations with missing values on key variables (58 observations); iii) observations with unknown certifier label (226 observations); and iv) plots cleared before the owner obtained the certification (182 observations). However, results hold if we consider plots certified at any time by any label (714 observations), a sample that is comparable with how the Polytax 5/7 was constructed.

[x] For the Polytax 5/7 only for 51 observations we could determine both the certification label and whether the plot was under certification by the time it was cleared.

[xi] In addition, 19 treated observations were dropped in Polytax 0/1 and 15 in Polytax 5/7 due to the empty cells problem. In particular, these are treated observations in counties where there are no untreated observations.

Chapter III

Effects of weather on daily school attendance decisions and academic performance

Laura Villalobos *

Abstract

This paper investigates how weather affects human capital by estimating the impact of meteorological conditions on schooling outcomes across high schools in Costa Rica, a tropical middle-income country. Combining hourly weather records from local weather stations with data on absenteeism at the individual-lecture level, I find higher absenteeism when it rains compared to a no-rain scenario, and a non-linear relationship between absenteeism and temperature. Absenteeism increases with every additional degree for students exposed to temperatures higher than 26°C. Furthermore, I document that non-attendance is associated with worse academic performance. These effects are in line with previous literature showing the negative effects of thermal (heat) stress on human cognitive skills, and suggest that higher temperature and precipitation can hamper human capital acquisition. These results are relevant for policy making, as adaptation strategies such as climate control technologies and schedule adjustments could have significant effects in increasing attendance.

Keywords: School attendance; absenteeism; education; weather; climate change; temperature; precipitation; heat stress; human capital

JEL Classification Numbers: I20; J24; O15; Q54; Q56.

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1 Introduction

Understanding the relationship between climate-related variables and economic loss is a central topic in the climate change economics literature (Pindyck, 2010). There is evidence that economic growth is lower in warmer years (Burke et al., 2015; Dell et al., 2012). The most salient effect of warmer temperatures on production is lower agriculture output (Schlenker and Roberts, 2009), but negative and economically important effects have also been found in non-agricultural sectors (Hsiang, 2010; Somanathan et al., 2015).

One possible explanation is that weather affects human productivity, a result often found in ergonomics and psychology experiments (Seppanen et al., 2006). According to this mechanism, known as "heat (or thermal) stress", high temperatures cause discomfort, fatigue, and even cognitive impairment (Zivin and Neidell, 2014). For example, Somanathan et al. (2015) show that high temperatures decrease worker productivity and attendance in manufacturing plants in India. There is also evidence showing large reductions in labor supply in industries with high exposure to climate, suggesting that workers reallocate their activities to avoid the heat (Zivin and Neidell, 2014).

This paper looks at the micro-fundamentals of how weather can affect the accumulation of human capital. I estimate the effect of short term weather variation on school attendance, and show that higher absenteeism comes at the cost of a lower academic performance. This is relevant as weather could constitute an external factor directly and permanently affecting the human capital formation, an effect that is currently not taken into account in the calculations of the social cost of carbon.

Previous studies show that pupils obtain lower scores when tests are taken in warmer days (Zivin et al., 2015; Park, 2016), but the effects of weather conditions on daily absenteeism remain unexplored even when student attendance is at the core of any model explaining academic achievement (Gottfried, 2009). Indeed, looking at the effect of weather variation on school attendance is a novel approach, as little is know about what determines the individual high-frequency school attendance decision, even when absenteeism is systematically found to be high in various contexts, and it is particularly high for the most vulnerable students (Gottfried, 2009, 2010; Aucejo and Romano, 2016; Goodman, 2014; Hayes and Gershenson, 2016). For example, in this study, I find an absenteeism rate of 14%, which means that in a group of 30 students, 4 pupils are missing for any given lecture on average.

The working hypothesis in this paper is that the instantaneous decision on whether to attend the next lecture is dynamic and influenced by the weather conditions observed within a short time window around the time of the lecture. Weather conditions might determine the attendance decision by affecting the direct and opportunity costs of attendance, the motivation and behavior of the pupils, or their productivity (Zivin and Neidell, 2014; Zivin et al., 2015; Zivin and Shrader, 2016; Baylis, 2015). To this end, I exploit the variation in the attendance decision at the student-lecture level over an entire schooling year on a sample of about two thousand 7-9th grades in six typical high schools located in the main climatic regions in Costa Rica, a tropical middle-income country. What is unique about this dataset is that it has information on the precise date and time of absence. Combined with hourly data on temperature and precipitation, obtained from local weather stations, this dataset allows me to identify the effect of weather conditions on high-frequency individual attendance decisions. Because the data is at the individual-lecture level, I am able to rule other rival explanations such as differences in ability, motivation, teacher effects, seasonality, and fatigue.

In addition, I add evidence to the previous literature that quantifies the effect of attendance on academic performance. In the context of U.S primary and middle schools students, the general result suggests that student absences, and particularly unexcused non-attendances, are associated with modest but statistically significant decreases in academic achievement, although the effect is larger for low-income and low-performing students (Gershenson et al., 2016; Aucejo and Romano, 2016). I confirm these results by finding lower academic achievement at higher values of absenteeism, a relationship that is visible only for unjustified absences.

The results suggest that both higher precipitation and temperature increase absenteeism, and that the effects are non-linear, suggesting that students at warmer and more humid areas are at higher risk and disadvantage. Furthermore, the temperature effect is visible only for unjustified absences, which is consistent with a story where the heat stress is the mechanism, and the students adjust their activities to avoid the heat. In addition to the weather effect, I find that absenteeism is systematically higher in the afternoons, on Fridays, and for the second half of the schooling year, a pattern that is consistent with a fatigue effect.

Back of the envelope estimations suggest that a one standard deviation increment in temperature from the sample mean of 26 degrees Celsius increases absenteeism by 0.4 percentage points, which in turn decreases academic performance by 0.024 of a grade standard deviation. These results are relevant for policy making. Relatively simple adjustments, such as climate control technologies and schedule adjustments, could have significant effects in increasing attendance. In addition, these results relate to classic issues of economic development and especially the role of geographic features in influencing development paths (Dell et al. 2014). Given this relationship and an expected scenario of warmer and more extreme weather events, regional gaps in schooling outcomes might not narrow in the future.

This paper is divided into 6 sections. Section 2 provides a theoretical structure for the hypothesis tested in this paper and summarizes the relevant literature. Section 3 describes the context, and presents the data and the descriptive statistics. Section 4 discusses the empirical strategy, whereas the results are presented and discussed in Section 5. Section 6 concludes.

2 Theoretical framework and related literature

Attendance is an important input in the eduction production function. Following Gottfried (2009), the student achievement is the basis for a model of education production function that can be expressed as

$$A = f(AB, G, F, N, S, T, C) \quad (1)$$

Where A represents student achievement; AB is a vector of student absence indicators; G is the student's own characteristics; F, family characteristics; N neighborhood characteristics; S, school characteristics; T, teacher characteristics; and C, classroom characteristics. According to this model, a student's test score in year t is a function of the influences of all input vectors in that year.

An augmented version of this model, known in the literature as value added model (VAM), considers the achievement in period $t-1$ as a proxy for all historical conditions that explain achievement. Hence, by introducing this lagged variable into the model, only additional current factors that vary conditional on achievement in $t-1$ are necessary to predict achievement at time t.

The first question addressed in this paper is what determines the student absences AB. Previous literature has found that socioeconomic and demographic background is a powerful predictor of absences (Romero and Lee, 2008; Ready, 2010; Goodman, 2014; Gershenson et al., 2016). For example, Gershenson et al. (2016) found that children living in households below the poverty line experienced nearly five more absences per term than their counterparts in households at or above the poverty line.

Even when these factors can explain differences in absenteeism patterns across students, they are less informative about what drives the individual attendance decisions over time since the socioeconomic and demographic background typically varies slowly or is fixed. The working hypothesis in this paper is that the attendance decision is dynamic and ultimately taken within a short time window before the lecture, and that weather conditions influence this decision conditional on time invariant student fixed effects and other controls. In Section 2.1, I outline a model that incorporates these insights and provide empirical evidence that support this hypothesis.

The second question addressed here is what is the effect of attendance on achievement, holding all other inputs constant. Identifying the causal effect of absenteeism on performance is challenging because even when it is possible to control for many observed factors in Equation (1), there are unobserved characteristics that affect both attendance and performance, such as ability and motivation. These unobserved characteristics could vary across pupils, but also for the same individual over time. Section 2.2 summarizes previous findings that account for these factors and Section 5.4 presents the empirical strategy I propose to identify the effects of absenteeism on performance.

2.1 Drivers of absenteeism

Following the model for school attendance decision proposed by Afridi et al. (2016), an enrolled child attends school on a particular day based on the expected benefits and costs of attendance. The benefits could include for example the additional knowledge gained during the lectures that increases the probability of passing the year and improves the labor market outcomes. On the side of the costs, there is a direct cost of attendance (transportation costs) that is relevant for the first and after-lunch lectures[1], and an opportunity cost of the time spent at school.

Afridi et al. (2016) denote the individual average cost and benefit of attendance by μ_i and b_i respectively. These average costs and benefits of attendance could be determined by household or individual characteristics and, like them, I remain agnostic about how these are aggregated within families and the precise role parents have in the attendance decision. They define the cost of attending schools for a given student i on a day d as:

$$c_{id} = \mu_i + \epsilon_d$$

Where ϵ_d represents the idiosyncratic factors that may result in higher or lower costs on a particular day. They assume that the distribution of ϵ over each month has zero mean and is identical across students. Thus a child will attend if $c_{id} < b_i$, so the monthly attendance rate is

$$A_i = F(b_i - \mu_i)$$

Where $F(\cdot)$ is a distribution function. In this paper, I am interested in explaining the role of weather on this high-frequency deviations from the mean benefits and costs of attendance. I assume μ_i and b_i are time invariant, so that the individual attendance decision is dynamic

[1]It is common for students who live close to school to go home for lunch.

and partly determined before the lecture depending on how different is the observed weather compared to the usual conditions, denoted as $\bar{w}$. Hence, for each lecture at time t, the student i will attend if:

$$B(w_{it} - \bar{w}) - C(w_{it} - \bar{w}) > 0$$

Where $B(\cdot)$ and $C(\cdot)$ are the cost and benefit functions that depend on the deviation from the "normal" weather conditions. Note that in this basic model, I assume that only the weather realizations around the time of the lecture determine the current costs and benefits, although it is possible to conceive a more comprehensive model in which the lagged values of weather and/or attendance decisions enter the decision as well.

Both precipitation and temperature might affect the marginal benefits and costs of attending the next lecture through different mechanisms, so the functional form for $B(\cdot)$ and $C(\cdot)$ depends on several factors. For example, higher precipitation could increase the direct cost of attendance, increasing the probability of absenteeism. The strength of this mechanism depends on infrastructure and mobility conditions, distance to school, access to adequate apparel, and the intensity of the weather shock.

Once at school, precipitation could affect the attendance decision through its effects on the side options and the time allocation decisions. For example, Lee et al. (2014) find that bad weather increases individual productivity, by eliminating potential cognitive distractions resulting from outdoor activities. Connolly (2008) shows that men work 30 more minutes and have an average of 25 minutes less leisure on rainy days. Hence, once at school, the cost of attending the next lecture could be lower when it rains. The average effect of the precipitation depends on what effect dominates.

In addition, high temperatures could affect the expected productivity and temper of the students, affecting their behavior. Indeed, Baylis (2015) shows that the hedonic state (or mood) decreases sharply after 21°C, suggesting a strongly lower preference for warmer temperatures compared to moderate ranges, and Zivin and Neidell (2014) find evidence of fewer time allocated to work and outdoor leisure in warmer days.

Also, it is well documented that high temperatures affect human productivity in performing tasks. In a meta-analysis of 24 relevant studies, Seppanen et al. (2006) found that performance at office work environment increases with temperatures up to 21-22°C and decreases above 23-24°C, with the highest productivity at around 22°C. According to this study, at 30°C the performance is around 9% lower. In a non-experimental setting, Somanathan et al. (2015) find that both productivity and attendance at work decrease at high temperatures in the manufacturing sector in India.

Children may be especially harmed by extreme temperatures because they are less likely to manage their own heat risk and may have fewer ways to avoid the heat (Zivin and Shrader, 2016). Indeed, Wargocki and Wyon (2007) found improved test performance of numerical and language-based tests when the temperature was reduced from 25°C to 20°C in Denmark. Zivin et al. (2015) explore the heat stress effect by looking at how temperature affects performance on standardized math tests for children at schools in the United States. They found that performance in math decreases when the test is taken in warmer days and in warmer years, whereas they don't find an effect for reading. Park (2016) also finds that hot exam days reduce test scores and educational attainment by economically significant magnitudes.

These elements of productivity, hedonic state and cognitive effects could enter the students' attendance decision process. If pupils anticipate a lower marginal benefit of attending a lecture, warmer temperatures could cause higher absenteeism. Furthermore, I expect the effects to be non-linear so that the effects are stronger at higher temperatures. Taken together, these results suggest that high frequency variation both in precipitation and temperature could affect the mood, emotions, expected productivity or time allocation decisions, which in turn could affect the probability of attendance. The sign and magnitude of the average effects are ambiguous and call for empirical estimation.

2.2 Absenteeism as an input for academic performance

Previous literature that accounts for confounding factors in Equation 1 has found positive and statistically significant effects of attendance on performance in the context of U.S primary schools. In particular, Aucejo and Romano (2016) finds that a reduction of 10 days of absence would lead to gains of 5.5% in math and 2.9% in reading standardized tests, and that effects are higher for low-performing students. Similarly, Gershenson et al. (2016) finds that one standard deviation increase in absences is associated with decreases in reading and math achievement of 2% to 4% test-score standard deviation, respectively.

In both studies, the average number of absences is around 6 days of school in an academic year of around 180 days. Also, both studies found that the effect of absences on test scores is roughly linear, and higher in magnitude for higher school levels and low-income students. In addition, Gershenson et al. (2016) found that absences similarly reduce achievement in urban, rural, and suburban schools. In a completely different context, Arulampalam et al. (2012) finds that missing class has an adverse causal effect on performance only for higher-performing students in a U.K college.

In Aucejo and Romano (2016), both absenteeism and performance are measured once per academic year, and they obtained data for several years. Their preferred specification controls for student, teacher and school fixed effects, grade, year, free/reduced price lunch status, as well as measures of overall school performance, and peer effects. Their results hold when they redefine the model in terms of a VAM and are robust to the inclusion of student disengagement, and family/health shocks. Similarly, Gershenson et al. (2016) rely on a VAM that controls for within-classroom variation in student absences. Finally, Arulampalam et al. (2012) also accounts for individual unobservable characteristics by exploiting the variation in absences for the same individual across subjects, and their estimation is robust to an IV approach that uses information on the (exogenous) day and time slot of each class as an instrument for absences.

Importantly, instrumenting absenteeism with temperature and precipitation is problematic if these weather variables affect performance directly, as previous evidence suggests (Park, 2016; Zivin et al., 2015), of if weather also determines teacher absences which in turn affects performance (Goodman, 2014). These mechanisms imply that the exogeneity assumption required for an instrument to be valid would be violated. The approach I take in this paper is to first calculate the additional absenteeism caused by weather and then quantify the impact that these missed lectures would have in the academic performance.

3 Background and Data

Costa Rica is a tropical, middle income country with good elementary education coverage, but big challenges persist at the high school level. Only half the population in their early twenties completed high school, which is a lower proportion compared to Latin America (53.5%) and the OECD countries (75%) (OECD, 2013; Bellei et al., 2013). Furthermore, worse performing schools tend to be located in warmer and more humid areas (PEN, 2014). Although many factors could explain this correlation, this is a salient pattern that deserves attention.

Three quarters of all high schools in the country are public institutions, centralized under the Ministry of Education at the highest administrative level, and 27 regional boards at the intermediate level. This study focuses on students at the high school level as they are more independent in their decisions and subject to less control from parents and teachers compared to elementary level students. High schools cover grades 7th to 11th, with expected entry age of 13 and expected graduation age of 17.

The schooling year begins in early February and runs until mid December, with one week break for Easter (mid April) and two weeks of midterm holidays (early July). Early every year, the Ministry of Education establishes the calendar that divides the academic year is divided into three trimesters. At the end of each trimester and for every subject, the student receives a grade in a scale from 0 to 100, which is an weighted average of classwork, homework, evaluations (tests), projects, teacher's concept and attendance[2]. In all cases, attendance accounts for up to 5% of the grade. The student receives a final annual grade per subject, which is a weighted average of the three partial grades, with weights 30% for trimesters I and II and 40% for the last trimester.

Students are sorted into groups of approximately 30 individuals and remain fixed throughout the year. Each group is matched with a weekly schedule that defines the curriculum, which also remains constant for a given school year (see Figure 1). For a given group and year, different subjects are taught by a different teacher. The schooling day typically starts between 7:00 and 8:30 am and finishes between 2:00 and 4:20 pm.

Poor infrastructure is one of the major challenges in Costa Rica, which concerns roads and buildings. In 2011, one third of all public high schools had at least 50% of their classrooms in poor or mediocre condition PEN (2013)[3].

3.1 Data

Weather data come from automatic meteorological stations and are available hourly. These ground weather stations provide highly accurate measurement of the exact weather conditions at each location (Dell et al., 2014; Auffhammer et al., 2013). The location of the weather stations determined the choice of the schools included in the sample. In particular, for each climatic region, and using a geo-coded dataset of all secondary schools in the country (PEN, 2013), I chose the school that is closest to a representative weather station with the most complete data available, as defined by the leading local weather data generating institutions[4]. In case attendance records were not available, the next closest school was chosen. This selection criteria ensures higher precision in the measurement of the weather conditions at each point in time for each school (see Figure 2). This selection criteria allowed me to obtain information from 6 high schools during an entire schooling year each.

The attendance data is taken directly from school records. Teachers must take attendance at the beginning of the lecture and then report it electronically to the administrative staff, who enters the information into a software that contains a personal file for each student.

[2]Unfortunately, detailed disaggregation of each student values for each component of these grades is not available. In this respect, this analysis deviates from previous literature that uses standardized math and reading tests. Moreover, partial grades could reflect a more comprehensive definition of learning outcomes.

[3]Statistics on climate control devices such as air conditioning and fans is not available, but during the data collection I observed that some schools do have air conditioning, but only for the administrative offices, and computer and engineering labs. These special subjects are not included in the analysis because they are offered only in certain schools.

[4]Instituto Metereológico Nacional (IMN), Instituto Costarricense de Electricidad (ICE) and Centro Agronómico Tropical de Investigación y Enseñanza (CATIE).

This reporting system has been implemented gradually at the national level since year 2010, and there has been a learning curve for schools and teachers to adjust to this technology (Sanchez, 2012).

It is mandatory for teachers to take attendance in each lecture, and there are strong incentives for them to comply with the norm. As previously mentioned, attendance accounts for up to 5% of the final grade for each subject. In addition, the number of absences per subject is detailed in the report given to parents quarterly, so it is common knowledge for all interested parties. These rules are set by the Ministry of Education and concern all public institutions.

Importantly, the attendance records include the date of the absence and the subject. This information allows me to follow each individual's attendance decision for each lecture, every day, during the entire schooling year. Combined with the weekly schedule for each group, it is also possible to merge attendance records with the weather conditions at the specific place and time (1-hour window before the lecture).

I focus on 7th to 9th grades because the curriculum for the 10th and 11th grades varies substantially between technical and academic schools. I dropped the 5% of the sample comprised by repeaters, drop-outs, or transfers and consider only core subjects which are mandatory for all students in a group and common across schools.

On average, a student has 83 days of lecture per subject per year, with some subjects having heavier weight on the curriculum such as Math and Spanish, for example (see Table 1). Each lecture lasts for 40 minutes and lectures are grouped in blocks of two or three lectures with a break in between. Per week, the average student receives between two and five 40-minute lectures per subject. Furthermore, for a given day, students take between one and three 40-minute lectures per subject.

Absences are divided into justified absences, unjustified absences and escapes. According to the regulation, an absence or tardiness is justified by a *force majeure* beyond the students' control that prevents her from attending school. This would include sickness, an accident, illness or death of close relatives, and other motives validated by the teacher or corresponding authority. An unjustified tardiness shorter than 10 minutes counts as half an unjustified absence, and if longer than that it counts as a full unjustified nonattendance.

3.2 Descriptive Statistics

The schools included in the sample have between 525 and 750 pupils each, which is representative of the national average (PEN, 2013)[5]. After removing 10th, 11th and 12th

[5]On average, diurnal non-rural public high schools in the country have around 600 students.

grades, as well as dropouts, repeaters and transfers, I obtain a sample of 1951 students in 94 groups (see Table 2).

As mentioned, taking attendance is required. However, in practice I observe incomplete records. One anecdotal explanation for this is that some teachers do take attendance but do not include the information into the software. Indeed, I find that on average teachers provide electronic daily attendance records for around 30% of the lectures (see Table 2) with great variability across subjects and classes, suggesting that some teachers are more habituated to take and report attendance. To account for this variability in the attendance taking decision, I control for teacher-classroom fixed effects in the empirical strategy and I further address this potential selectivity bias in Section 5.3.2.

On average, students receive 4.6 different subjects a day, 2.6 of which are main subjects. And on average, attendance is taken in one of those main lectures every day. The absenteeism rate is 14%, which means that in a group of 30 pupils, on average 4 students are absent every lecture. Furthermore, 86% of absences are unjustified.

The average temperature for the hours in which students are in school is 26°C, with a maximum of 38 and a minimum of around 11. These figures vary across schools as they are located in different regions. Figure 3 shows the distribution of the weather variables by school. There is considerable overlap in the temperature distribution across schools. Also, precipitation frequency and intensity patterns vary across schools, with some areas presenting little but constant amounts of rain, and others facing substantial rainfall in a given moment of the day.

4 Empirical strategy

My objective is to estimate the causal effect of weather conditions on attendance, for which I specify the econometric model as follows:

$$Y_{isjt} = W_{st}\beta + \alpha_{ij} + \theta_t + \sigma_{dow} + \rho_w + \epsilon_{isjt} \tag{2}$$

The dependent variable Y is dichotomous and takes the value one if the student i in school s was absent from a lecture in subject j at time t, and zero if the student was not reported as absent. I only include lectures for which attendance was taken. The vector W_{st} contains the weather variables for school s at the time of lecture t, and includes precipitation and temperature. Following previous literature, I expect the effects to be non-linear. Hence, I include temperature as a second degree polynomial, measured in degrees Celsius. To allow for a discrete effect (rain vs no rain) as well as for a magnitude effect (little vs heavy rainfall), I include a dummy variable equal to 1 for positive rainfall values, in addition to a second degree polynomial for the continuous variable, measured in mm. β is the coefficient vector of interest, and measures the effect of weather on attendance.

I assume that the weather variables are exogenous, i.e., uncorrelated with the error term in the model. Thus, I assume that there is no reverse causality, no bias posed by omitted variables, and no measurement errors in the explanatory variables. The fact that I observe the same individuals repeatedly helps me to rule out cross-sectional confounders that could correlate with weather and explain schooling outcomes, such as school quality or infrastructure.

The identification comes from the variation in attendance for each individual within each subject. By including individual-subject fixed effects α_{ij}, I control for time invariant individual characteristics that explain absenteeism patterns such as ability, motivation, income, and parents' characteristics. Controlling for these factors is relevant as previous literature consistently shows that absenteeism correlates with demographic variables (Romero and Lee, 2008; Ready, 2010; Goodman, 2014; Gershenson et al., 2016). Also, this specification controls for different individual attendance patterns across subjects, perhaps in response to students' preferences or ability for different subjects. Importantly, as for a given student each subject is taught by a different teacher, this specification also controls for teacher-classroom effects. This accounts for the teachers' quality and preference for taking and reporting attendance.

I include fixed effects for the order of the lecture within the day θ_t because this is a time variant characteristic that is relevant for absenteeism and correlated with weather. For example, absenteeism tends to be consistently higher for the afternoon lectures. This pattern could be explained by a fatigue effect, that would be observed regardless of the weather conditions. As temperature is always higher in the afternoon compared to the morning lectures (see Figure 4), this variable absorbs some of the potential intra-day effect of weather on absenteeism.

In addition, I control for time varying factors by including week fixed effects ρ_w. These fixed effects capture seasonality in the absenteeism patterns that are common to all students. For example, absenteeism could be typically high right before or after holidays or low during exams period. Furthermore, they capture seasonality in economic cycles that could affect absenteeism. In Section 5.3, I show that the results hold with date fixed effects as well.

Other omitted time-variant individual characteristics are not expected to bias the results, given the high frequency of the data. Still, I control for the day of the week fixed effects, σ_{dow}, because these could explain attendance patterns that are common for all individuals. Note that even if this control variable is not strictly necessary for the weather coefficients to be unbiased, it could increase the precision of the estimates as they help to explain the attendance patterns.

Importantly, the weather variables are fixed for a given point in time for all individuals in one school. For this reason, I cluster the standard errors at the school-date level to account both for the grouped structure of the weather variables and for the potential correlation in individual error terms within the same school at a given point in time[6].

5 Results

Table 3 presents the main results. For comparison purposes, Column 1 shows the effects of temperature and precipitation in a linear specification, before adding additional control variables. Under this baseline specification, absenteeism is 2.5 percentage points higher when it rains compared to no rain, and 0.5 percentage points higher for each additional temperature degree. Both weather variables are highly statistically significant.

However, if there are other factors that explain absenteeism and correlate with weather, these estimates would be biased. For example, attendance could be systematically lower in some regions, or for certain students, due to characteristics that relate to weather such as access to infrastructure and economic activities. Column 2 accounts for these time-invariant differences that could result in different attendance patterns across regions or individuals.

Notably, Column 2 shows results of the within estimator. Hence, the coefficients can be interpreted as the effect of deviations from the individual-subject means. The results suggest that the probability of being absent increases when it is warmer than usual at the time of the lecture. Furthermore, the point estimates are quite stable compared to the unconditional estimates, suggesting that cross-sectional unobserved heterogeneity does not account for the positive average effect of precipitation and temperature on absenteeism.

Columns 1 and 2 show that absenteeism increases when it rains and with higher temperatures, and that this relationship is robust to time-invariant characteristics, as well as exogenous time-variant student and school characteristics. Column 3 rules out that these effects are driven by seasonal patterns by controlling for week fixed effects.

Column 4 includes the order of the lecture within the day as a control variable, another characteristic that explains attendance patterns and correlates with weather. The point estimates are lower compared to the unconditional effects, but still both are positive and statistically significant. On average, the probability that a student is absent is 1.1 percentage points higher when it rains, and an additional temperature degree increases absenteeism by

[6]Results are similar if I cluster the standard errors at the school-date-hour level, or if I account for serial correlation by using two-way clustered standard errors at the school-date and individual level (see Section 5.3.1).

0.2 percentage points.

Columns 1 to 4 show a linear model for the response of absenteeism to weather. However, as previous literature suggests, the relationship is likely to be non-linear: an additional degree could be more harmful at higher temperatures. Column 5 accounts for non-linearities in both temperature and precipitation. The dummy variable for precipitation captures the discrete jump in the absenteeism function between zero rain and any positive amount of rain, whereas the continuous part captures the intensity effect. This specification also includes the full set of control variables.

The estimated coefficients imply that the probability of absenteeism is 1.5% higher for moderate rainfall compared to zero rain, and that there is a convex (U-shape) relationship for both precipitation and temperature. These curves are plotted in Figure 5 and 6. Figure 5a shows the linear prediction of absenteeism as a function of temperature. The figure shows that at moderate temperatures between 12-26°C the absenteeism rate remains close to the sample average of 14%, whereas attendance decreases at higher temperatures. The marginal effects plotted in Figure 5b show that this increase in the absenteeism rate is statistically significant above 26 degrees.

Together these results suggest that when the "normal" temperature at which students are exposed is already relatively warm (higher than 26°C, the average of the sample) an additional temperature degree increases their likelihood of being absent[7]. In addition, absenteeism increases with precipitation compared to a no-rain scenario, but small amounts of rainfall increase attendance.

In particular, a one standard deviation increment in temperature from the sample mean increases the absenteeism rate by 0.4 percentage points. Considering that the main subjects (math, science, and Spanish) comprise 200 lectures a year, this effect represents an additional 8 missed lectures per year and subject.

Figure 6a shows that more rainfall decreases absenteeism, supporting the hypothesis of low opportunity cost of attending lectures when it's raining once at school. However, for abundant precipitation absenteeism increases again suggesting an extreme weather shock effect, although the precision of the estimates decreases considerably for the upper part of the precipitation distribution.

[7] In a non-linear model with individual fixed effects, the coefficients can no longer be interpreted as reflecting effects of deviations from the individual mean, but instead recover a "global" non-linear relationship, where the marginal effect on Y depends on the value of X. In section 5.3.1, I estimate the hybrid model proposed by McIntosh and Schlenker (2006), in order to test for the "within non-linearity", i.e., to see whether the marginal effects on Y also depend on how much X is deviated from the individual mean:

$$y_{it} = \beta_1 x_{it} + \beta_2 x_{it}^2 + \beta_3 (x_{it} - \bar{x}_i)^2 + c_i + \epsilon_{it}$$

5.1 Fatigue effects

The coefficients from the control variables (lecture order, day of the week, and week of the year) are interesting in themselves. They show patterns in attendance that are assumed common to all students in the sample. Figure 7 plots the coefficients from the main specification. Absenteeism is higher on Fridays, and increases as the day goes by, as well as in the second half of the schooling year. This is evidence that absenteeism is consistent with fatigue effects.

5.2 Heterogeneous effects

Based on the preferred specification (Column 5 in Table 3), this section looks at the effects by gender and by type of absence.

5.2.1 Effects by gender

Figure 8 shows the relationship between temperature and absenteeism by gender. Although the non-linear result holds in both cases, there are subtle differences in the responses across genders. The effect for males is stronger and their absenteeism increases at a lower temperature threshold compared to females. The marginal effect is already statistically significant at 24 degrees for males, whereas for females the effect becomes significant only after 28 degrees. These results suggest that, compared to males, females tolerate better higher temperatures.

5.2.2 Effects by type of absence

The type of the absence can reveal some information about the mechanisms behind the main effects. As previously mentioned, justified absences cover cases that are out of the student's control such as sickness, an accident, or the decease of a relative. Although extreme weather events or shocks could explain these events (Baez et al., 2010), justified absences are less likely to be caused by "normal" variation in the meteorologic conditions. Consistent with findings in other contexts, Table 2 shows that a large proportion of the absences are unjustified, and hence potentially avoidable (Gershenson et al., 2016; Gottfried, 2009).

Figure 9 replicates the same results as in Figure 5b dividing the analysis by absence type. The Figure clearly shows that all the effect of temperature on absenteeism comes from the discretionary absences. This finding is consistent with a story where the heat stress is mechanism driving absenteeism, where students re-schedule their activities to avoid the heat.

5.3 Robustness checks

The main result so far suggests that absenteeism is when it rains and at the upper part of the temperature distribution. This section tests the robustness of this non-linear effect by allowing alternative specifications for the functional form. Also, this section addresses the potential sample selection bias that could arise from incomplete attendance records.

5.3.1 Specification tests

This section presents additional results in order to test for the robustness of the main effects presented in Column 5 of Table 3. First, I test whether the results are sensitive to the choice of the standard errors estimator. In Column 1 of Table 5, I present the results clustering the standard errors at the school-date-hour level, which is the unit of observation for the weather variables. In addition, Column 2 shows the results with two-way clustered standard errors at the school-date and individual level. This specification controls for potential serial correlation in the error term. The two-way clustering yields virtually the same standard errors.

Second, I test whether the response to temperature and precipitation is also non-linear in deviations from the individual means. According to McIntosh and Schlenker (2006), in the presence of these "within non-linearities", the estimates in Column 5 of Table 3 will be biased. To test for the robustness of the main specification, I estimate the hybrid model proposed by McIntosh and Schlenker (2006) to test whether the "within non-linearity" is statistically different from zero. This model includes two squared terms for each weather variable: the demeaned squared variable to measure "global" non-linearities and the squared demeaned variable to measure "within-group" non-linearities (see Footnote 7). Intuitively, the global effects capture the response in absenteeism as we move across climatic regions, whereas the within-group effects capture the non-linear response to deviations from the "normal" weather conditions for any given climatic region. Column 3 in Table 5 shows that there are no significant effects for the within non-linearities, suggesting that the model is well specified and that non-linearities in the data are the result of moving along the temperature and precipitation distributions.

Third, I test whether the results hold if instead of week fixed effects I include date fixed effects, a more demanding specification that controls for daily unobserved factors that could explain attendance and performance. Column 4 shows that the results are very close to the week specification, which is reassuring that the effects are not explained by seasonality.

Fourth, in order to rule out that the results are driven by the effect of one school only or by extreme values, I exclude the observations in both ends of the temperature distribution so that there is overlap for at least two schools (between 15.5-34.9°C, see Figure 3a). These estimates are presented in Column 5, where again the results remain very close to the main specification.

Finally, I use two alternative specifications to capture the non-linear effects. Following previous literature (Zivin et al., 2015), I define a variable containing the number of degrees by which the temperature exceeds 21°C, with values below 21°C assigned a value of 0. This piece-wise linear function allows the effects to be non-linear around a relevant threshold. The 21°C cut-off was chosen because the quadratic specification showed a minimum effect around this temperature, but also in order for the results to be comparable to previous findings[8].

Table 5 shows the results, which are qualitatively identical to the quadratic specification. An additional degree at temperatures higher than 21°C increases the probability of absenteeism by 0.3 percentage points. The results for precipitation remain stable as well. As an additional test, I use a nonparametric specification which is also standard in this literature. In this model, the temperature enters as a full set of indicator variables for every 2°C, with the 20-22°C bin as reference category. Results are shown in Figure 10, where again we see the non-linear relationship between absenteeism and temperature, with statistically significant effects after 27°C.

Taken together, these tests suggest that the results are robust to alternative estimators of the standard errors, to the exclusion of extreme temperatures, and are not driven by the choice of the functional non-linear form, but that both precipitation and an additional degree at warmer temperatures increases absenteeism.

5.3.2 Incomplete attendance records

As attendance records are incomplete, a concern is whether the sample of lectures for which attendance is observed is as good as random. Figure 11 shows the temperature distribution of the group of lectures for which attendance is observed compared to the group with missing information. Although both distributions look fairly similar, a formal Kolmogorov—Smirnov test rejects the null hypothesis of equal distribution functions (Combined K-S test: D=0.0196, P-value=0.003). Figure 11 shows that indeed attendance is taken more frequently at lower temperatures.

The direction of the potential bias in my main results depends on the correlation between the error term in the student attendance decision and the error term in a model determining the teacher's probability of taking attendance. For example, teachers attendance is an unobserved factor m that is missing in the individual attendance decision (Equation 2) and that explains the teacher's probability of taking attendance.

[8] Average temperature in this study is 26°C which is comparable with the 22.77°C in Zivin et al. (2015).

I argue that the correlation between teachers and students attendance is likely to be positive, for example if weather affects both groups similarly. Given that I observe that attendance is taken less often at high temperatures, this positive correlation implies that my estimates are a lower bound of the true effect. The intuition is that the missing observations are likely to be bunched at high temperatures and high absenteeism rates. Similar case happens if students are more likely to skip a lecture when they predict that the teacher won't take attendance.

In order for my results to be a spurious relationship between absenteeism and temperature, it would have to be the case that teachers systematically take more attendance when students are absent AND temperatures are low, or when absenteeism is low AND temperature is high, or both cases combined. This would be the case for example if teachers decision on taking attendance depends on the number of students that are missing AND this decision is sensitive to the weather conditions.

To account for this potential sample selection bias, I estimate a two stage sample selection model (Heckman, 1979). The first stage in the Heckman model includes the entire sample of scheduled lectures in order to estimate the probability that attendance is taken for a given lecture and classroom. The dependent variable takes the value 1 for all lectures in which attendance records are observed, and zero for all scheduled lectures with no attendance information. The probability of observing attendance depends on the average absenteeism rate computed for each classroom and date, and the interaction of this variable with both temperature and precipitation. I also include the same set of control variables and functional form as in the attendance decision (Equation 2) because the probability of taking attendance can also be related to weather conditions, vary across the day of the week, lecture order and date. Then, I compute the Mills ratio and include it in Equation 2 as an additional covariate.

Table 6 presents the results. Column 1 replicates the mains results (Column 5 of Table 3) for comparison purposes. Column 2 presents the selection equation. The probability of observing attendance is lower when it rains compared to no rain, and negative for temperature. Also, the more students there are absent, the lower the probability of attendance being taken, which is consistent with the story of students and teachers absenteeism being correlated. Finally, both interaction effects are positive and statistically significant for temperature, suggesting that conditional on absenteeism being high, teachers take attendance more frequently when the temperature is high.

Finally, the corrected coefficients for the attendance equation are presented in Column 3. Firstly, note that The Mills ratio in the attendance equation is positive and statistically significant, rejecting the null hypothesis of no selection bias. Furthermore, the magnitude of the sample bias corrected coefficients for the weather variables are very similar, and if anything stronger, to the uncorrected specification, confirming that the main results are not driven by sample selection bias. Figure 12 confirms that the marginal effects with this corrected specification remain identical to the main results presented in Figure 5b.

5.4 Effects of absenteeism on academic performance

This section investigates the relationship between absenteeism and academic performance. I combine the quarterly partial grades with the number of absences per trimester in order to quantify the conditional correlation of absenteeism and academic performance. The unit of observation is the student-subject-trimester[9].

Following Aucejo and Romano (2016), I standardized the grades at the school-level-subject-trimester. When I don't observe any absence for a given student-subject-trimester, I assume that the individual was not absent, hence the variable number of absences per trimester takes the value zero. I report the descriptive statistics both with and without this assumption, and the results are similar in both cases (results available upon request).

Table 7 shows the descriptive statistics of the sample. There are 1229 students, 7 subjects and 3 trimesters. There are some student-subject missing observations, so on average a student is observed in 19 subject-trimesters, and there are 7881 student-subjects, for a final sample size of 23075.

The average grade is 74 on a scale from 1-100 points, and the number of absences and tardies is 2.13 per individual-subject-trimester. This average of the quarterly number of absences and tardies is a lower bound of the true value, given that some of the missing values, and indeed also some of the observed positive values, are underreported. Indeed, if I only consider the observations with positive number of absences during a trimester, the number of absences increases to 5 lectures per student-subject-trimester. The lower bound suggests that a student will miss 6.4 lectures per subject per year, whereas with the conditional mean the annual figure is 15 absences. These figures are consistent with the sample in Gershenson et al. (2016) where the average is between 6 and 8 absences and tardies per year, and between 7 and 12 absences for students in households below the poverty level.

As mentioned earlier, most of the absences are unjustified. In addition, there is substantial variation in absences both between and within students, suggesting that some pupils tend to be more systematically absent than others, and that a given student tends to skip more lectures in some subjects than in others.

Because I observe three rounds of grades and absenteeism for each individual and subject,

[9]The sample comprises the following schools and years: Central Valley (2014), North (2014), Central Pacific (2014), South Pacific (2015). I include students in grades 7-9 who completed the academic year. The subjects are: Science, Civic Education, Spanish, Social Studies, French, English, and Math.

the identification strategy comes from variations in attendance across trimesters for the same student and subject. The following model exploits this within individual-subject variability:

$$G_{igjq} = \beta_0 + \beta_1 A_{igjq} + \alpha_{ij} + \gamma_q + \delta_{gq} + \epsilon_{igjq} \quad (3)$$

The dependent variable is the (standardized) partial grade that student i in group g obtained for subject j by the end of trimester q. A_{igjq} is the total number of absences during the corresponding subject and period. By including individual-subject fixed effects α_{ij}, this specification accounts for unobserved characteristics across individuals, but also controls for variation in individual ability, effort, and motivation across subjects. This is particularly relevant as I am including courses that might require a different set of skills (Arulampalam et al., 2012). For example, if pupils tend to prefer science over math, they could perform better at the former than at latter, even if they attend both subjects equally.

Also, I include trimester fixed effects γ_q to control for seasonal patterns both in attendance and performance that are common to all individuals, as well as for the length of each period. For example, by the third trimester pupils could exhibit a fatigue effect that affects both attendance and performance. In addition, I include group-trimester fixed effects δ_{gq} to account for peer-effects, including average absences and performance of the group. The coefficient of interest is β_1, which measures the correlation between the number of absences A and academic performance, net of seasonal, peers and individual-subject effects. I cluster the standard errors at the group level, to allow for intra-class correlation as well as serial correlation of the error term.

However, the estimator of β_1 could still be biased if there are time-variant characteristics that simultaneously explain attendance and performance across trimesters. For example, students' engagement may not be fixed over time or family/health shocks could affect absences and test score performance (Aucejo and Romano, 2016). Also, students who obtained a low grade early in the schooling year might be discouraged and decide to skip lectures more often, which in turn reflects in an even lower grade later on. To address this concern, I also present the results of a value-added model (VAM) that includes performance in the previous period. Because OLS estimates of this dynamic panel model are biased, I also present the results following the method proposed by Anderson and Hsiao (1982). In this method, the twice-lagged grades serve as instrument for the first-differenced lag grade. These estimates are comparable with the results in Gershenson et al. (2016).

Importantly, the quarterly number of absences per subject could be underestimated due to the fact that not all teachers take and report attendance in every lecture. As discussed before, the probability of taking attendance seems to be teacher-specific. Hence, this measurement error is less problematic within a given subject, as the teacher remains constant. In addition, there could be a time trend in the effort of taking attendance, which is captured by the trimester fixed effects. As a result, I don't expect that the measurement error in the absenteeism records introduces a significant bias.

As I observe one year of data per individual and students remain in one school throughout the year, this specification implicitly includes the set of control variables listed in Equation (1), and it is similar to the one that has been used in previous studies (Aucejo and Romano, 2016; Gershenson et al., 2016; Arulampalam et al., 2012). In particular, this specification accounts for year, school, family, level, classroom, teacher, student and peers fixed effects. Also, by including the lagged grade, this specification is also comparable to the value-added models (VAM) of student achievement implemented in Aucejo and Romano (2016) and Gershenson et al. (2016).

Table 8 shows the results. Panel A reports the effects of the aggregate number of absences and tardies, whereas Panel B reports these effects separately. In Column 1, I present the unconditional effect of absenteeism on performance. Missing an additional 40-minutes lecture decreases the partial grade by 7% of a standard deviation. Column 2 accounts for the temporal, spatial and subject, and peers variation by controlling for school, subject, group, trimester and group-trimester fixed effects. The coefficient is 8%, suggesting that the average effect is robust to the institution, group composition, time of the year, and the discipline.

On the contrary, Column 3 shows that individual unobserved characteristics account for a substantial part of the effects. Once these factors are taken into account, the effect of absenteeism on performance decreases to 3% of a standard deviation. Column 4 controls for differences in the ability and motivation across subjects for the same individual, and the coefficient holds constant.

Columns 5 to 7 consider a value-added model (VAM), where I control for time-variant individual characteristics by including the lagged grade. Note that the number of observations decreases as only the last two trimesters are included in the sample as a result of including the lagged grade. Column 5 reports the OLS estimates, where we can see that the grade obtained in the previous period is positively correlated with the current grade, and the effect of absenteeism is still negative, statistically significant and fairly similar to the ones reported in Column 3. Finally, columns 6 and 7 show that the coefficient remains stable under the fixed effects and the Anderson and Hsiao estimators for the VAM.

In Panel B, we can see that both types of absences and tardies have a negative effect. However, the effect is statistically significant only for the unjustified absences in the static models, and for both the unjustified and the tardies in the VAM. This is consistent with previous literature that shows that having a higher proportion of unexcused absences to total absences has a stronger negative effect on the academic achievement, compared to having a higher proportion of excused absences (Gottfried, 2009; Gershenson et al., 2016). An plausible explanation for this result is that unjustified absences may arise from school disengagement. Thus, lower motivation induces absenteeism, and both of these effects reinforce each other to reduce performance. However, to the extend that the results in Column 7 control for disengagement, the zero effect of the justified absences could be explained by a compensatory behavior in which teachers (or classmates) provide additional assistance to mend the justified absences.

Together, these results show that missing an additional 40-minutes lecture lowers performance by a magnitude of around 3-4% of a standard deviation and that most of the effect comes from unjustified absences. The next question that naturally arises is whether the effect is linear along the performance distribution. For example, high absenteeism could be more hurtful for low performing students. To test this, I run a quantile regression analysis based on the specification of Column 4 in Table 8[10]. Figure 13 shows the point estimates and standard errors of the effect of absenteeism for different values of the academic performance distribution. This Figure suggests that absenteeism hurts performance of all students. In addition, the point estimates suggest that the negative effect is stronger at the lowest-performing students, but these differences are not statistically different from zero. This result differs from previous results for primary schools in the U.S that find that absences are less harmful for high-achieving students (Aucejo and Romano, 2016; Gershenson et al., 2016).

The magnitude of the overall effect is remarkably high compared to both Aucejo and Romano (2016) and Gershenson et al. (2016). One reason could be that absenteeism is a more relevant input in the education function for secondary schools. Alternatively, the fact that this study looks at the effect on the partial grades as opposed to tests could explain this larger effect.

6 Discussion and Conclusions

Education is a leading determinant of economic growth, employment, and earnings in modern knowledge-based economies (Woessmann, 2016). Because school attendance is a key input in the learning production function, understanding what drives absenteeism has relevant policy implications.

[10] The regressions include student-subject and trimester fixed effects and were estimated following Baker et al. (2016) and Powell (2016). As currently this routine does not allow for factor operators, I excluded from the model the group-trimester fixed effects. As the core results are not sensitive to the exclusion of these variables, I expect the quantile results to be robust to this specification as well. Table results are available upon request

This paper contributes to the literature that investigates how weather affects attendance for students at the high school level of a middle-income country, where major education challenges persist. I found that both precipitation and heat increase the absenteeism rate, and that the effects are non-linear. These results are robust to sample selection bias correction, functional form specification, and grouped and serial correlation in the error term. Also, I found that weather conditions affect both males and females, although males are slightly more sensitive to heat. In addition to the weather conditions, a strong fatigue effect also explains the attendance patterns.

This paper also contributes to the literature that quantifies the effect of attendance on the academic performance. Consistent with previous literature, I found harmful effects of absenteeism on academic achievement. In particular, an additional absence is associated with a decrease of 0.03 grade standard deviation. Connecting these effects, I find that a one standard deviation increment in the temperature from the sample mean decreases the grade by 0.024 of a standard deviation.

This type of analysis also is useful in informing about classic issues of economic development and especially the role of geographic features in influencing development paths (Dell et al., 2014). Given this relationship and a scenario of warmer and more extreme weather events, regional gaps in schooling outcomes might not close in the future.

These results also offers an alternative but complementary explanation for previous findings in Zivin et al. (2015) and Park (2016). If higher temperatures induce absenteeism, the result of lower performance for tests taken in warmer days/years could be also explained by poor learning induced by low attendance. Together these results suggest that higher temperature and precipitation instantaneously translates into human capital loss. Heat not only has a direct and instantaneous effect during a task performance, but it could also be the result of lower accumulated learning.

These results indicate that students at warmer and more humid areas are at higher risk of school failure, as well as regions with warmer expected weathers as predicted by climate change scenarios. However, while there are a number of large scale projects that can help reduce climate change or increase adaptation in the long term, we need to design policies for immediate behavioral changes to prevent this lower accumulated learning. Policy interventions such as installing air-conditioning could be highly beneficial, as well as better planning and scheduling of the academic activities.

Further analysis is required to address related important questions. First, it is relevant to understand the role of these effects for the labor market outcomes, and how they reflect on aggregated economic growth. A second question is what do these short run weather variation effects represent in terms of the costs of future climate change. Incorporating adaptation, intensification, and general equilibrium effects is crucial to develop a better understanding of the relationship between weather variation and human capital formation (Dell et al., 2014; Zivin et al., 2015). Finally, it is important to understand what is the role of current schooling policies, such as cash or in kind transfers (food and transportation) as incentives for neutralizing these effects of weather on human capital formation. Although additional information is required for it to be possible to address this question, the answers could be useful in providing better information to decision makers.

Figure 1: Example of a week schedule for a given group

MINISTERIO DE EDUCACIÓN PÚBLICA

Sección: 7 – 1 Profesor: Aula:

	Hora	L	K	M	J	V
1	8:30 – 9:10	Cívica-Montoya		Matemática-Nelson	Inglés Conversacional A Jorge-B Johel	Ciencias-Everardo
2	9:10 – 9:50	Cívica-Montoya	Francés-Paul	Matemática-Nelson	Inglés Conversacional A Jorge-B Johel	Ciencias-Everardo
	9:50 – 10:00	Recreo				
3	10:00 – 10:40	Inglés-Helen	Español-Anabelle	Matemática Fortalecimiento-Alejandro	Inglés Conversacional A Jorge-B Johel	Francés-Paul
4	10:40 – 11:20	Inglés-Helen	Español-Anabelle	Inglés-Helen	Inglés Conversacional A Jorge-B Johel	Francés-Paul
	11:20 – 12:00	Almuerzo				
5	12:00 – 12:40	A Animación Sarita-B Ideando vacante#2	Estudios sociales-Montoya	Religión-Rosa Nelly	Matemática-Nelson	A Animación Sarita-B Ideando vacante#2
6	12:40 – 1:20	A Animación Sarita-B Ideando Vacante#2	Estudios sociales-Montoya	CienciasA y S-Everardo	Español-Anabelle	A Animación Sarita-B Ideando vacante#2
	1:20 – 1:30	Recreo				
7	1:30 – 2:10	Estudios sociales-Montoya	Matemática-Nelson	Educación física-Sandra y Alexis	Ciencias-Everardo	A Animación Sarita-B Ideando vacante#2
8	2:10 – 2:50	Estudios sociales-Montoya	Matemática-Nelson	Educación física-Sandra y Alexis	Ciencias-Everardo	A Animación Sarita-B Ideando vacante#2
	2:50 – 3:00	Recreo				
9	3:00 – 3:40	Guía Sarita	Inglés Conversacional A Jorge-B Johel		Música-Alejandro	Español-Anabelle
10	3:40 – 4:20	Orientación-Elena	Inglés Conversacional A Jorge-B Johel		Música- Alejandro	Español-Anabelle

Notes: In columns: days of the week from Monday to Friday. In row lectures by hour. In cells: the subject and the teacher.

Figure 2: Map with climatic regions and schools location (stars)

Table 1: Number of 40-minute lecture per week per subject

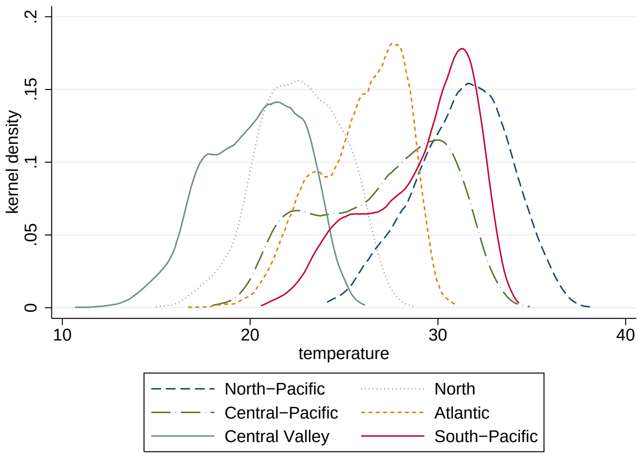
Level	Science	Civic education	Social Studies	Spanish	English	Math	French
7 grade	5	2	4	5	3	5	3
8 grade	5	2	4	5	3	5	3
9 grade	5	2	4	5	3	5	3

Table 2: Descriptive Statistics

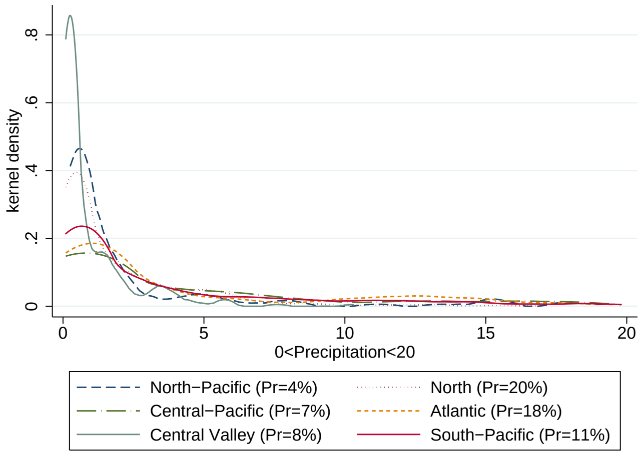
	All Sample	North-Pacific	North	Central Pacific	Atlantic	Central Valley	South Pacific
Year	2014-15	2014	2014	2015	2014	2014	2015
Sample size							
Total population (number of students in all grades)	3791	750	722	525	752	605	437
Number of students 7th, 8th, 9th grades	2668	586	416	380	542	390	354
Number repeaters, dropouts, changed school	788	195	155	97	113	78	150
Number of students in the sample	1951	391	261	283	500	312	204
Students in the sample/students 7th, 8th, 9th grades	73%	67%	63%	74%	92%	80%	58%
Number of groups	94	19	13	12	20	12	18
At group-subject level							
Share of lectures with attendance in the year (average)	31%	35%	37%	17%	21%	35%	40%
Number with share of attendance taken 50%	76	17	29	2	6	0	22
At group-day level (averages)							
Number of different subjects a day	4.640	4.863	4.369	4.074	5.200	4.441	4.894
Number of different subjects a day in main subjects	2.613	2.958	2.415	2.463	2.660	2.593	2.591
Number of main subjects with attendance taken a day	1.076	1.378	1.277	0.506	0.939	1.211	1.142
At date-lecture level							
Absenteeism rate (average)	14%	14%	17%	22%	11%	8%	19%
Share of absences that are unjustified (average)	86%	93%	86%	94%	88%	73%	79%
At weather station level							
Mean Temperature (Degrees Celsius)	26.25	31.26	22.75	27.06	25.88	20.22	29.07
Max Temperature (Degrees Celsius)	38.10	38.10	28.70	34.90	31.00	26.10	34.30
Min Temperature (Degrees Celsius)	10.70	24.10	15.00	18.00	16.70	10.70	20.60
Temperature (Standard Deviation)	4.73	2.52	2.30	3.47	2.31	2.53	2.85
Precipitation (mm)	0.37	0.15	0.44	0.39	0.97	0.08	0.52
Probability of Precipitation	10%	4%	20%	7%	18%	8%	11%

Notes: Weather values between 7:00 and 16:00. Statistics for students in 7th, 8th, 9th grades unless otherwise specified

Figure 3: Weather variables by school



(a) Temperature during school hours



(b) Precipitation during school hours [0-20mm]

Table 3: Effects of temperature and precipitation on the probability of absenteeism

	(1) OLS	(2) Panel FE	(3) Panel FE	(4) Panel FE	(5) Panel FE
Precipitation (1=YES)	0.025***	0.023***	0.018***	0.011**	0.015***
	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Precipitation (mm/100)					-0.184*
					(0.103)
Precipitation (squared)					0.393*
					(0.236)
Temperature (°C)	0.005***	0.003***	0.004***	0.002**	-0.007*
	(0.000)	(0.001)	(0.001)	(0.001)	(0.004)
Temperature (squared)					0.000**
					(0.000)
Observations	235,904	235,904	235,904	235,904	235,904
R-squared	0.005	0.001	0.008	0.009	0.009
Individual-Subject FE	NO	YES	YES	YES	YES
DOW FE	NO	YES	YES	YES	YES
Week FE	NO	NO	YES	YES	YES
Lect Order FE	NO	NO	NO	YES	YES
Number of individual-subjects		11,719	11,719	11,719	11,719

Notes: Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). The unit of observation is the student-subject. Standard Errors are clustered at the school-date level. Temperature in degrees Celsius. DOW is Day of the Week. ***p<0.01, **p<0.05, *p<0.1

Figure 4: Average Temperature by hour of the day and school

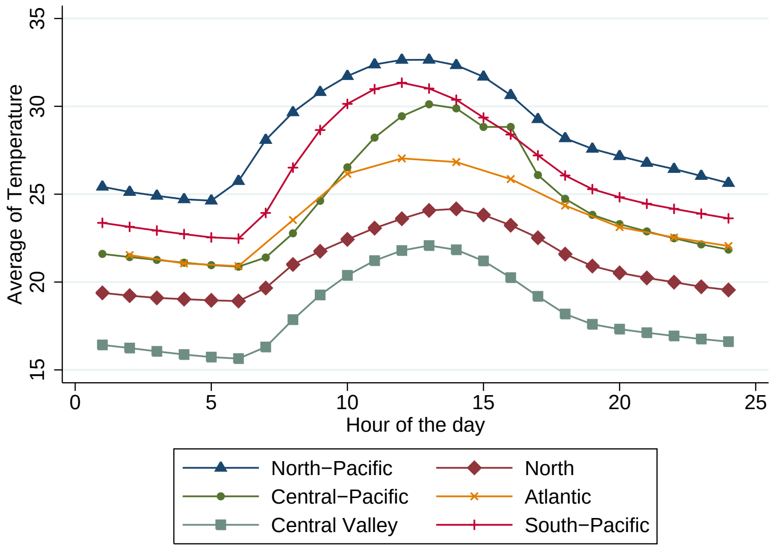
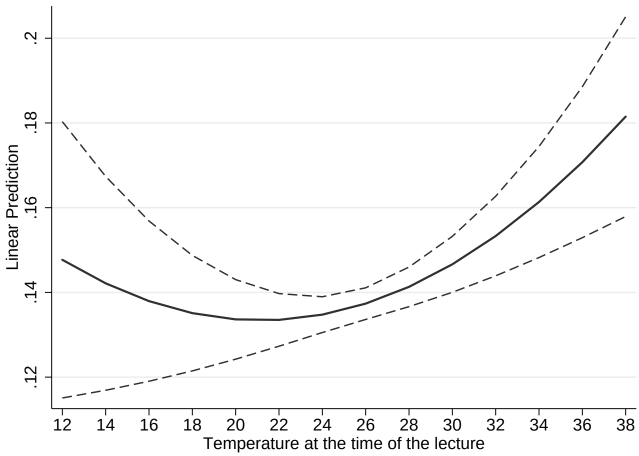
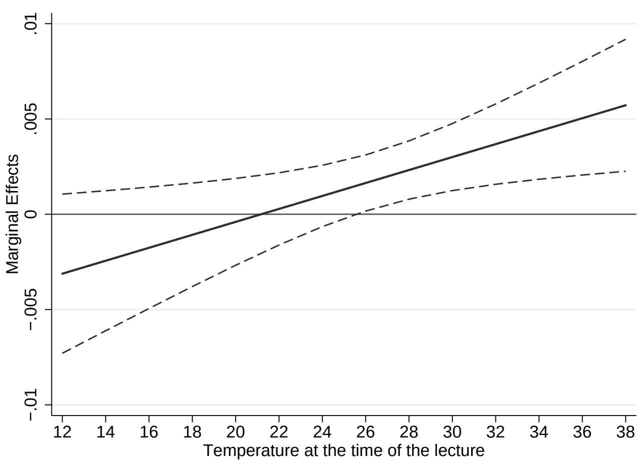


Figure 5: Effects of temperature on Absenteeism



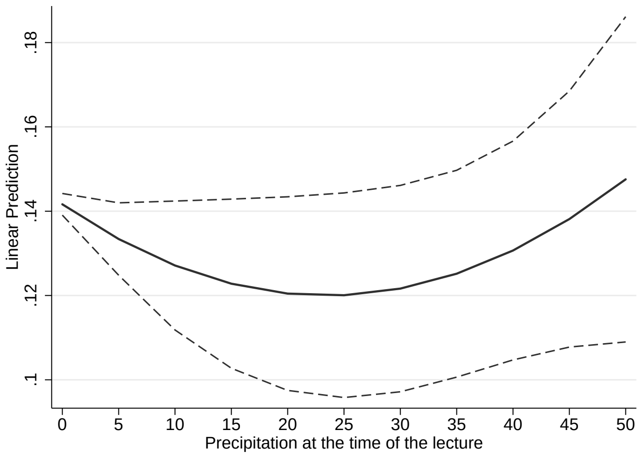
(a) Linear Prediction



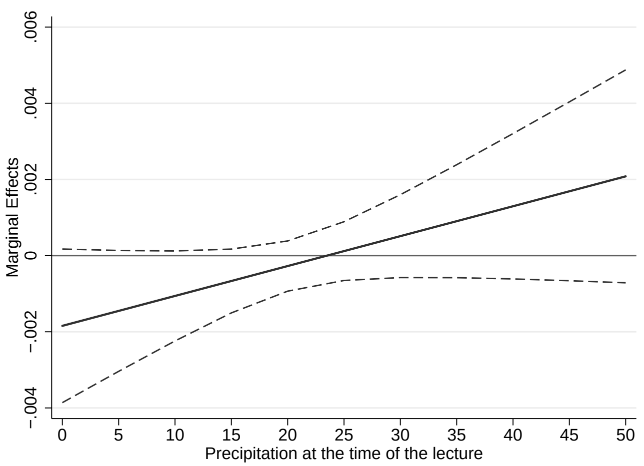
(b) Marginal Effects

Notes: Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). Linear prediction and marginal effects of a regression that has as independent variables: precipitation (0/1), precipitation (mm), precipitation squared, temperature (degrees Celsius), temperature squared, and fixed effects for student-subject, day of the week, lecture order, and week. Standard errors are clustered at the date-school level. Horizontal line at zero, and dashed lines are 95% confidence intervals.

Figure 6: Effects of precipitation on Absenteeism



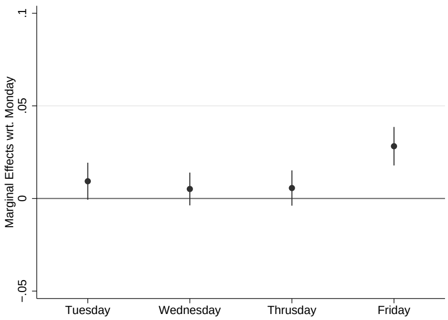
(a) Linear Prediction



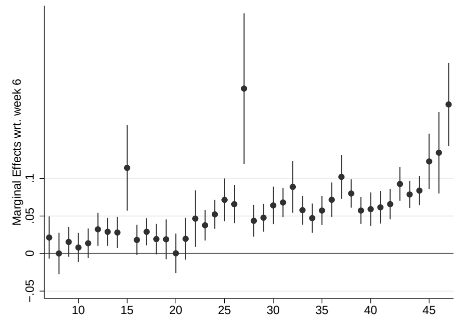
(b) Marginal Effects

Notes: Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). Linear prediction and marginal effects of a regression that has as independent variables: precipitation (0/1), precipitation (mm), precipitation squared, temperature (degrees Celsius), temperature squared, and fixed effects for student-subject, day of the week, lecture order, and week. Standard errors are clustered at the date-school level. Horizontal line at zero, and dashed lines are 95% confidence intervals.

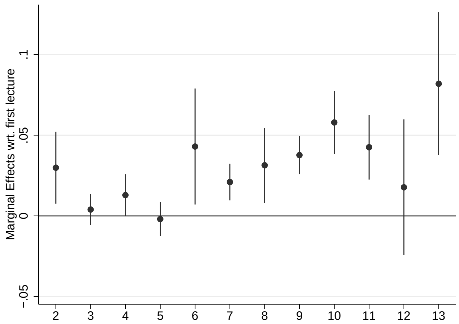
Figure 7: Absenteeism by day of the week, and week of the year



A. Day of the week



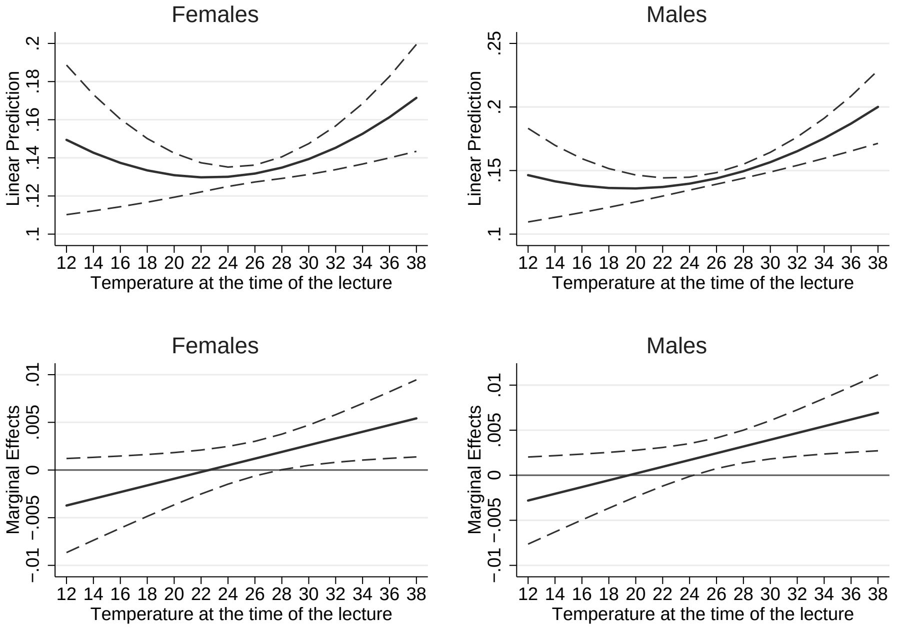
B. Week of the year



C. Lecture order

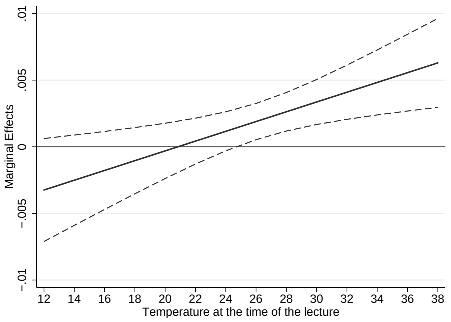
Notes: Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). The regression that has as independent variables: precipitation (0/1), precipitation (mm), precipitation squared, temperature (degrees Celsius), temperature squared, as well as fixed effects for student-subject, day of the week, lecture order, and week. Standard errors are clustered at the date-school level. Vertical line at zero, and horizontal lines are 95% confidence intervals.

Figure 8: Effects of temperature on Absenteeism by gender

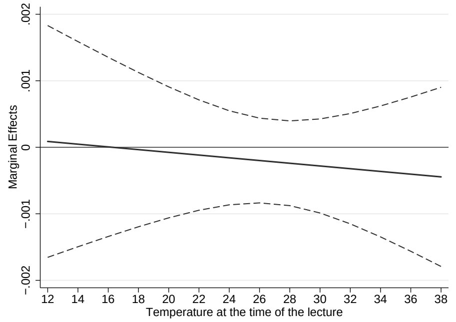


Notes: Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). Linear prediction and marginal effects for separate regressions by gender. Independent variables: precipitation (0/1), precipitation (mm), precipitation squared, temperature (degrees Celsius), temperature squared, and fixed effects for student-subject, day of the week, lecture order, and week. Standard errors are clustered at the date-school level. Horizontal line at zero, and dashed lines are 95% confidence intervals.

Figure 9: Effects of temperature on Absenteeism by type of absence



A. Unjustified absences



B. Justified absences

Table 4: Effects of temperature and precipitation on absenteeism by type of absence

	(1)	(2)
VARIABLES	Unjustified	Justified
Precipitation (1=YES)	0.013**	0.003
	(0.005)	(0.002)
Precipitation (mm/100)	-0.178*	-0.028
	(0.096)	(0.037)
Precipitation (squared)	0.420*	0.010
	(0.219)	(0.087)
Temperature (C)	-0.008**	0.000
	(0.003)	(0.002)
Temperature (squared)	0.000***	-0.000
	(0.000)	(0.000)
Observations	231,288	207,213
R-squared	0.009	0.002
Number of individual-subjects	11,719	11,655

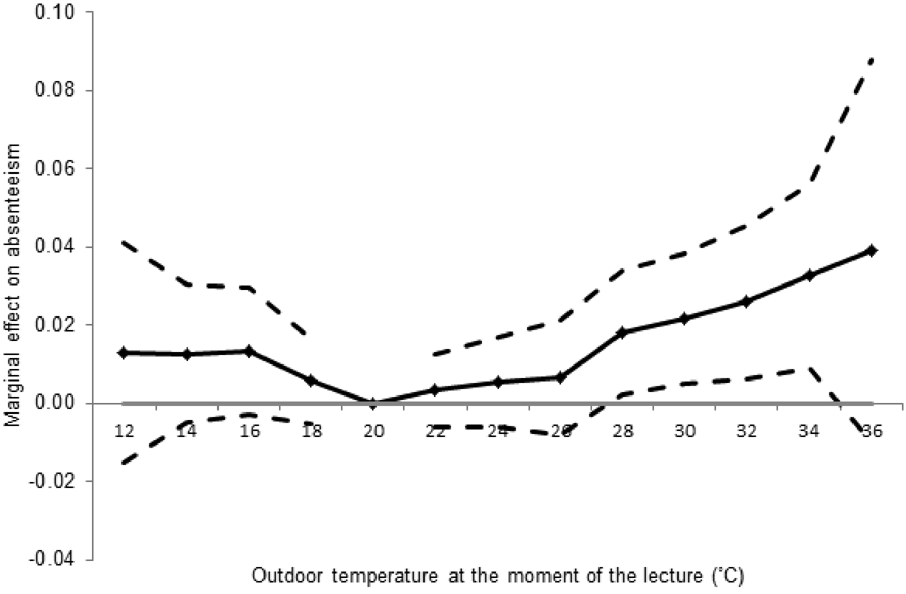
Notes: Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). Marginal effects of two separate regressions, conditioning on type of absence and independent variables: precipitation (0/1), precipitation (mm), precipitation squared, temperature (degrees Celsius), temperature squared, and fixed effects for student-subject, day of the week, lecture order, and week. Standard errors are clustered at the date-school level. Horizontal line at zero, and dashed lines are 95% confidence intervals.

Table 5: Robustness tests

	(1) CE cluster School-date-hour	(2) CE cluster Twoway	(3) Within non-linearities	(4) Date FE	(5) Restricted temperature range	(6) Piece-wise linear function
Precipitation (1=YES)	0.015***	0.015***	0.015***	0.015***	0.015***	0.012**
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
Precipitation (mm/100)	-0.184*	-0.184*	-0.145	-0.173	-0.189*	
	(0.101)	(0.103)	(0.111)	(0.110)	(0.103)	
Precipitation (squared)	0.393	0.393*	-1.719	0.354	0.401*	
	(0.253)	(0.237)	(3.192)	(0.266)	(0.237)	
Temperature (C)	-0.007**	-0.007*	-0.007*	-0.010***	-0.009**	
	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	
Temperature (squared)	0.000***	0.000**	0.000**	0.000***	0.000**	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
Precipitation (Demeaned Sq)			0.000			
			(0.000)			
Temperature (Demeaned Sq)			-0.000			
			(0.000)			
Max[T-21, 0]						0.003***
						(0.001)
Observations	235,904	235,749	235,904	235,904	229,629	235,904
R-squared	0.009	0.009	0.010	0.016	0.009	0.009
Number of student-subjects	11,719	11,564	1,951	11,719	11,719	11,719
Week FE	YES	YES	YES	NO	YES	YES
Date FE	NO	NO	NO	YES	NO	NO

Notes: All columns control for individual-subject FE, DOW FE, and Lecture Order FE. Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). Standard Errors are clustered at the school-date level unless otherwise specified. Two-way clustered standard errors are computed using the routine "ivreg2" in Stata. Column 5 drops the extremes of the temperature distribution and restricts the observations to the temperature range $[15.5 - 34.9°C]$, which is where there is overlap of at least 2 schools (see Figure 3a). ***p<0.01, **p<0.05, *p<0.1

Figure 10: Effects of temperature on Absenteeism, 2-degree temperature bins specification



Notes: Dependent variable=1 if absent and =0 if not absent (conditional on attendance being taken). Marginal effects of a regression that has as independent variables: precipitation (0/1), precipitation (mm), precipitation squared, and n-1 categorical variables, one per each 2-degree temperature bin (the reference bin is 20-22 degrees), as well as fixed effects for individual-subject, day of the week, lecture order, and week. Standard errors are clustered at the date-school level. Vertical line at 22 degrees bin base category, horizontal line at zero, and dashed lines are 95% confidence intervals.

Figure 11: Distribution of Temperature by subsamples

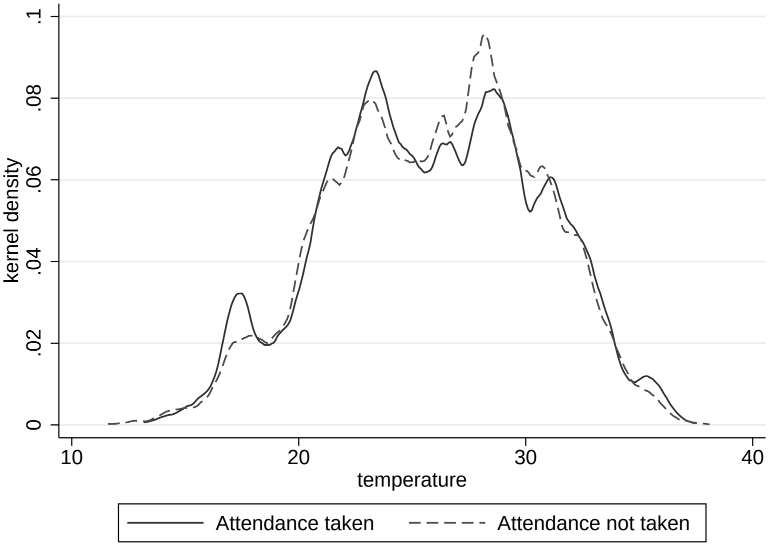
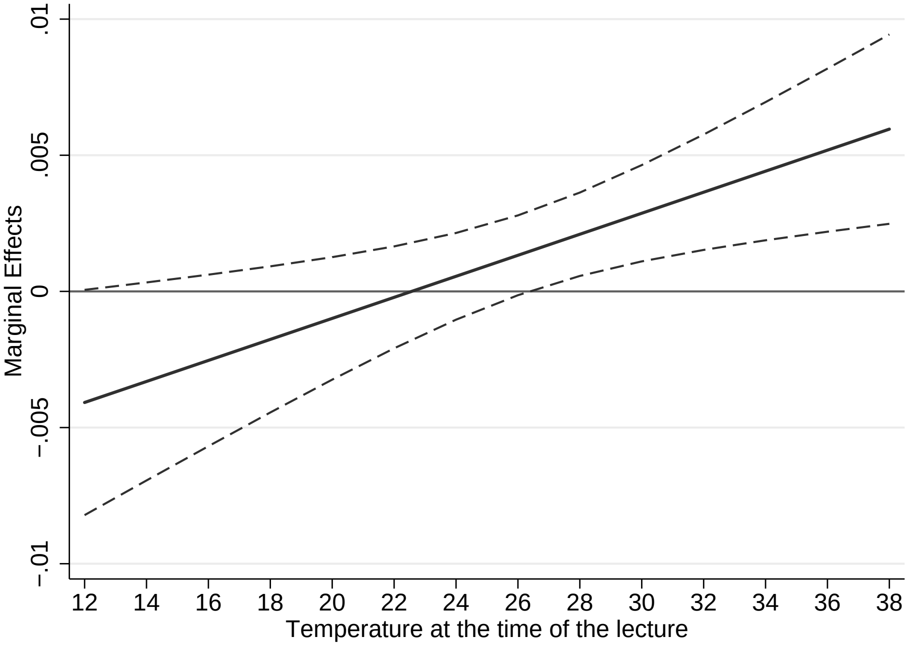


Figure 12: Effects of temperature on Absenteeism, corrected for sample selection



Notes: Selection equation is a Probit model with dependent variable=1 if attendance was taken at the lecture level, and =0 if attendance information is missing. The Probit has as independent variables: precipitation (0/1), temperature (degrees Celsius), the average absenteeism rate by date and classroom, the interaction between the absenteeism rate and temperature, the interaction between the absenteeism rate and precipitation , as well as fixed effects for subject-classroom (which identifies the teacher FE), day of the week, lecture order, and date. Attendance equation model as specified in Column 5 of Table 3 in addition to the Mill's ratio from the first stage. Standard errors are clustered at the date-school level.

Table 6: Sample selection bias test

	(1) Baseline results	(2) Selection (Probit)	(3) Attendance equation
Precipitation (1=YES)	0.015***	-0.051	0.012**
	(0.006)	(0.060)	(0.006)
Precipitation (mm/100)	-0.184*		-0.163
	(0.103)		(0.106)
Precipitation (squared)	0.393*		0.201
	(0.236)		(0.248)
Temperature(C)	-0.007*	-0.003	-0.009**
	(0.004)	(0.008)	(0.004)
Temperature (squared)	0.000**		0.000***
	(0.000)		(0.000)
Average daily attendance rate		-2.528***	
		(0.646)	
Interaction attendance rate x precipitation		0.060	
		(0.263)	
Interaction attendance rate x temperature		0.083***	
		(0.023)	
Mill's ratio			0.097***
			(0.016)
Observations	235,904	382,543	226,941
R-squared	0.009		0.010
Number of individual-subjects	11,719		11,183
Individual-subject FE	YES	NO	YES
Subject-classroom FE	NO	YES	NO
DOW FE	YES	YES	YES
Lect Order FE	YES	YES	YES
Time FE	Week	Date	Week

Notes: See notes in Figure 12

Table 7: Effects of absenteeism on performance. Descriptive Statistics by trimester

	Assuming 0 absences when missing		Conditional on positive absences	
	Mean	SD	Mean	SD
Grade (1-100)	74.21	15.77	70.30	16.39
Net standarized grade	0.00	0.99	-0.19	1.03
Number of absences and tardies	2.13	3.97	5.05	4.75
Std. Dev.				
Between students		2.50		3.46
Within students		3.25		3.53
Between student-subjects		0.87		0.93
Within student-subjects		0.49		0.45
Unjustified absences	1.59	3.45	3.77	4.47
Justified absences	0.34	1.23	0.81	1.80
Tardies	0.20	0.67	0.47	0.97
N Observations	23075	23075	9744	9744
N Students	1229	1229	1109	1109
N subject-trimester (average)	18.78	18.78	8.79	8.79
N Student-subjects	7881	7881	4971	4971
N Trimesters (average)	2.93	2.93	1.96	1.96

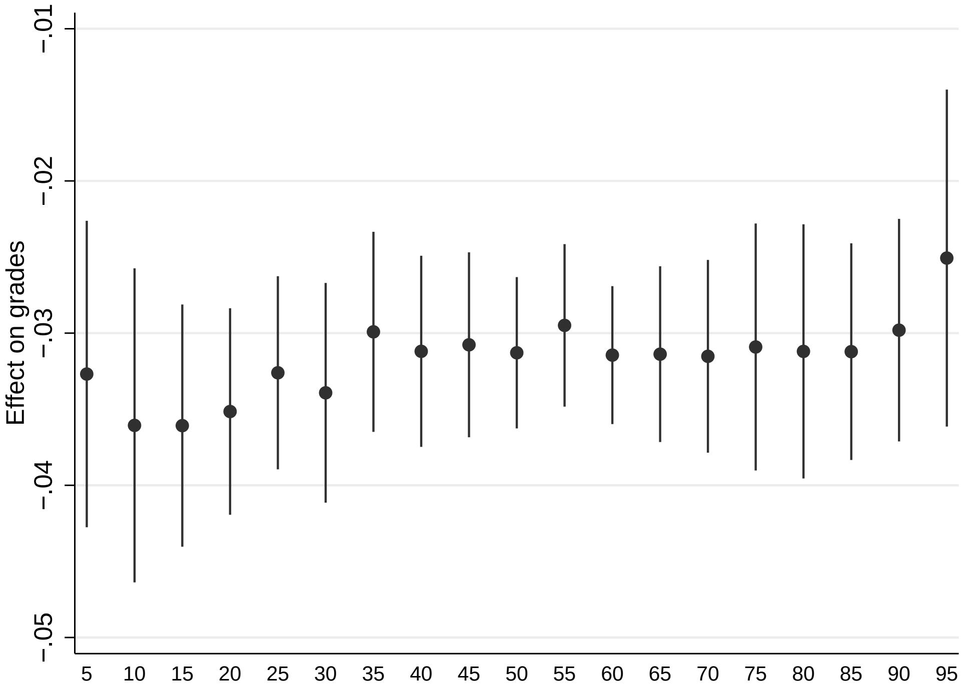
Notes: The grades are standardized to have zero mean and standard deviation equal to 1 at the school-level-subject-trimester level.

Table 8: Effect of absenteeism on academic performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
					Value-added models (VAM)		
	OLS	OLS	FE	FE	OLS	FE	AH
A. Aggregated							
Number of absences	-0.069***	-0.080***	-0.029***	-0.031***	-0.045***	-0.030***	-0.036***
	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
Grade (t-1)					0.593***	-0.387***	0.106***
					(0.014)	(0.015)	(0.034)
Observations	23,075	23,075	23,075	23,075	15,116	15,116	7,339
R-squared	0.077	0.140	0.041	0.057	0.467	0.212	
B. By type of absence							
Number of unjustified absences	-0.087***	-0.094***	-0.034***	-0.036***	-0.053***	-0.034***	-0.043***
	(0.004)	(0.005)	(0.002)	(0.004)	(0.003)	(0.004)	(0.004)
Number of justified absences	0.031***	0.007	-0.006	-0.011*	0.001	-0.010	-0.009
	(0.011)	(0.009)	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)
Number of tardies	-0.069***	-0.074***	-0.013	-0.020	-0.031**	-0.030*	-0.035**
	(0.023)	(0.020)	(0.008)	(0.013)	(0.013)	(0.015)	(0.017)
Grade (t-1)					0.585***	-0.386***	0.102***
					(0.013)	(0.015)	(0.034)
Observations	23,075	23,075	23,075	23,075	15,116	15,116	7,339
R-squared	0.094	0.152	0.043	0.058	0.472	0.214	
School FE	NO	YES	NO	NO	YES	NO	NO
Subject FE	NO	YES	YES	NO	YES	NO	NO
Group FE	NO	YES	NO	NO	YES	NO	NO
Trimester FE	NO	YES	YES	YES	YES	YES	YES
Group-trimester FE	NO	YES	YES	YES	YES	YES	NO
Individual FE	NO	NO	YES	NO	NO	NO	NO
Individual-Subject FE	NO	NO	NO	YES	NO	YES	NO
Grade t-1	NO	NO	NO	NO	YES	YES	YES

Notes: The dependent variable is the trimester grade standardized at the school-level-subject-trimester level. Number of absences per trimester is the number of 40-min lectures missed during the trimester. Standard errors clustered at the school-group level. Zero absences are assumed when no there is no number of absences reported for a given student-subject-trimester, but the results are almost identical without this assumption and are available upon request. AH corresponds to Anderson and Hsiao (1982) estimator. ***p<0.01, **p<0.05, *p<0.1

Figure 13: Effects of absenteeism on academic performance by quantiles of the grades distribution



Notes: Marginal effects of the number of absences on the grade by quantile of the grades distribution (x-axis). Regressions include student-subject and trimester fixed effects and were estimated following Baker et al. (2016) and Powell (2016). As currently this routine does not allow for factor operators, I excluded from the model the group-trimester fixed effects. As the core results are not sensitive to the exclusion of these variables, I expect the quantile results to be robust to this specification as well.

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