# Consequences of Protected Areas for Household Forest Extraction, Time Use, and Consumption: Evidence from Nepal[1]

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Running Title: Consequences of Protected Areas on Households in Nepal

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**Consequences of Protected Areas for Household Forest Extraction, Time Use, and Consumption: Evidence from Nepal**

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Abstract: Many forest protected areas (PAs) are located in developing countries, where forests are a major source of food and fuel. Thus, biodiversity conservation may have unintended consequences on welfare of people in local communities. To explore this issue, we examine the effects of the new PAs in Nepal established during 1995-2003. Using the Nepal Living Standard Survey collected in 1995/1996 and 2003/2004, we evaluate the effects of these new PAs on household consumption, wood collection, and time use. Our estimates suggest that the establishment of PAs reduce the average wood collection by 20% to 40% compared to the period prior to PA establishment, with greater impact when PAs are strictly managed. We find evidence that households adjust to the new PAs with at least modest shifts to fuel purchased in market but not by using fuel conserving stoves, and that PAs are ineffective when climate makes fuelwood for heating essential or if households are in regions with large dependence on wood as a fuel. Finally, while wood collection reductions could lower household welfare, we find no evidence that PAs trigger either large decreases or increases in total consumption or consumption of food.

Keywords: Protected Area, Land Conservation, Impact Evaluation, Nepal, Biodiversity
JEL Codes: Q560, Q570, Q580.

## 1. Introduction

Protected areas (PAs)—places with legal restrictions on resource extraction—play a pivotal role in biodiversity conservation. Although protected areas have long been used in natural resource management, recent environmental movements have accelerated growth in the area under protection (Phillips, 2004). According to the World Database of Protected Areas, about 15% of the world's land is currently under some level of "protection".[2] Because tropical developing countries are the habitat for many endangered and threatened species, many recent PAs are located in poor regions of the world (World Bank, 2018).[3] However, households living near forests in developing countries traditionally extract and depend on resources from the landscape (e.g., firewood, honey, and herbal medicines). An important unintended consequence of PAs in developing countries may be welfare loss for these households if people need to reallocate their efforts and consumption choices after a sudden ban on resource access (Sims, 2010). This paper therefore studies how PAs affect forest extraction, time allocation, and total consumption of the surrounding people. Understanding these outcomes and the mechanisms that drive them may help us to better design new suites of PA policies for sustainable resource management.

A large body of literature in environmental economics spatially analyzes forest cover to understand the impacts of PAs. In general, estimated impacts show that PAs help protect biodiversity (Andam et al., 2008; Ferraro & Hanauer, 2011; Nelson & Chomitz, 2011; Shah & Baylis, 2015; Sims, 2010). Using landscape-level pixel data, these papers examine the effects of PAs on deforestation and find that PAs help to reduce deforestation. When PAs succeed in protecting biodiversity conservation, people in communities nearby may be negatively affected

[2] See also http://www.protectedplanet.net/.

[3] According to the World Bank Open Data development indicators (2018) on the terrestrial protected areas, higher income countries have 15.1% of land under protection, and lower income countries have 15.9% area under protection.

by PA restrictions depending on how much those people depend on forest resources. Given the success of PAs on biodiversity outcomes, it becomes important to understand the effects of PAs on surrounding people.

Environmental economists and policymakers are concerned about whether establishment of forest PAs causes an adverse impact on human welfare by restricting forest resources or reduces poverty by generating ecotourism activities (Adams et al. 2004; Barrett et al. 2011; Baylis et al., 2015; Miteva et al., 2012; Pullin et al., 2013; Alix-Garcia et al., 2014; Ferraro and Pattanayak, 2006; Wilkie et al., 2006). A growing body of literature has analyzed regional-level poverty data to determine the impacts of PAs. The first papers to discuss the impacts of PAs on human welfare are qualitative analyses based on cross-sectional case-specific data (Cernea & Schmidt-Soltau, 2006; Pattanayak et al., 2003; Brandon et al., 1998; Bookbinder et al. 1998; Foerster et al. 2011). These studies show that PAs have diverse impacts, depending on household characteristics. However, these studies, due to the settings and availability of data, do not estimate the the causal effects of PAs on the households.

The first research that uses a careful identification strategy to determine the economic impact of PAs on neighboring communities is Andam et al. (2010). The authors use census-tract level poverty index data from 1973 and 2000 for Costa Rica, and they use the poverty headcount ratio at the sub-district level (share of the population with monthly household consumption below the poverty line) from the 2000 census for Thailand. Using a matching with difference-in-difference (DID) method, the study shows that poverty is non-negatively correlated with established PAs. However, the measurement of poverty used in the study is an average score over several decades that gives only a relative ranking of the areas. Following this study, several other studies use similar econometric approaches and find that PAs have mixed impacts on the regional-level poverty index in different countries (Canavire-Bacarreza & Hanauer, 2013; Miranda et al., 2014; Sims, 2010; Clements et al. 2014, 2015; Ferraro and Hanauer, 2011; Pfaff et al. 2014). Finally, Yergeau et al. (2017) develop a theoretical macro model of the effects of conservation on aggregate social welfare that includes an agricultural productivity model. However, a complementary analysis of the impacts of PAs with household-level information is needed in order to design better policy instruments for future conservation and follow-up compensation tools to accompany that conservation.

This paper is grounded in a household-level theoretical framework. The predictions derived from economic theory are ambiguous. Resource restrictions associated with PAs will hamper the daily livelihoods of forest-dependent local communities, but ecotourism driven by PAs could increase household wellbeing by changing the local labor market and introducing new income sources. Even without changes in the local labor market, the impact of PAs on time spent gathering wood and working outside the home will depend on details of the situation. We take a quasi-experimental approach to studying the actual impacts of PAs on households in rural Nepal. We estimate the impact on wood collection, time spent gathering wood, and a consumption measure of household well-being. We investigate the channels through which households in villages near new PAs react to such establishment: changes in labor supply, market fuel purchases, stove choice, migration, and establishment of community forests for resource use. We estimate how the effects of a PA depend on the stringency of the management system, a feature that is rarely examined in the literature. Finally, we estimate how the effects of PAs depend on local conditions such as season and geography that influence baseline household wood collection and elasticity of demand for wood.

To examine the effects of PAs on household activities, we choose to use data from Nepal because that country has both newly established PAs with different management strategies and multiple years of detailed household-level economic information. Excessive dependency on daily firewood collection from forests has been documented as one of the biggest drivers of deforestation and environmental degradation in Nepal (Baland et al., 2010; Soussan et al., 1995). We use Nepal's household survey data for the years 1995/1996 and 2003/2004. During this time span, the Nepalese government introduced several new PAs with various management stringency. We collect detailed information about the geographical locations of these "treatment" PAs and the villages in the household survey. We compare household wood-collection and other activities in villages close to PAs to villages farther away from new PAs.

The ideal experiment to estimate the impacts on households would randomly assign PAs to some communities and not to others and then compare activities across the communities. However, PAs are not randomly allocated across space. Forest landscapes with richer biodiversity may be more likely to be protected. Also, governments may choose to invest in protecting landscapes that have better prospects for being popular tourist spots. Thus, unobserved community characteristics (e.g., forest dependency or site selection) may be a source of bias in OLS estimates of the effects of PA designation on household activity. Our empirical research strategy accounts for such possible selection processes and provides causal identification of the PA impacts. The political economy of forest management in Nepal suggests that state-level decisions about PA siting are exogenous to individual-level choice behavior. Thus, this paper's strategy for identifying PA impacts relies on the fact that a household's exposure to PAs varies with time and region. We use a difference-in-difference (DID) estimator that controls for systematic variation of the presence of a ban on resource access over time and across regions.

To evaluate the impacts of PAs and to understand thoroughly the potential bias arising from the non-random setting, we carry out the analyses with three different control groups. First, a control group of "never treated" communities that are near forests but not near any PAs that were ever established forms our primary basis for comparison. Second, we use a control group of "treated earlier" villages near PAs that were established prior to the study period. Third, we use another control group of "treated later" villages located near PAs that were established after the study period. These "treated earlier" and "treated later" villages may be more similar in unobserved characteristics to villages that are treated during our study period than villages that are never near a protected area. We inspect a district-level welfare variable and district-level forest cover data over time to confirm similarity of the pre-treatment welfare trends in the treatment and main control group.

We find evidence of a significant reduction in the amount of firewood collected near PAs. Our estimates suggest that between 1996 and 2003, households' firewood collection decreased by at least 20% to at most 40% in areas close to PAs, and the impacts are much stronger for strict forms of PAs than for new conservation areas that allow some community use of forest resources (buffer zones). However, in contrast to other research, we do not find any robust accompanying impact on the real per-capita total or food consumption expenditures. We do not find evidence that PAs have consistent effects stove choice, migration, or time use such as wood collection time, labor supply, or unemployment rate, but we do find that a new PA can drive more households to purchase fuel from the market to substitute for the wood they cannot collect. Finally, we find that the effects of PAs vary significantly according to geographical and seasonal factors that affect household demand for fuelwood.

## 2 Background

Our choice of the case for this study is driven by the need to find a place with both newly established PAs and multiple years of detailed household-level economic information. We also sought a site in which wood collection was a serious problem for forest health. Traditional forest dependency, establishment of new PAs in recent years, and detailed household-specific forestry information in the Nepal Living Standard Survey (NLSS) all make Nepal a good choice for this study. Nearly two-thirds of the Nepalese people still use firewood as a primary source of cooking fuel. According to the Nepalese census, 70.50% of total population used firewood in 2001, and in 2011, 66.14% of people still used firewood.[4] A question in the 1996 census showed that households collect firewood only for home consumption, but the scale of firewood use is large enough that it is one of the biggest drivers of forest degradation in Nepal (Baland et al., 2010). The establishment of PAs has the potential to alter this forest use by reducing the size of the forest available for extraction. Indeed, discussions between government and local people about firewood use and conservation area loss spurred the establishment of BZ and CF institutions in the late 1990's (Nagendra et al., 2005).

Prior to 1950, forests were under the control of local communities. In 1957, the government nationalized forest land, which created open access resources and exacerbated degradation because of ineffective and corrupt governance. In 1967, Nepal introduced a special forest protection act to enable forest conservation.

Today, Nepal has a complex structure of different levels of protection, although the forest cover loss is still very high in some areas. Nepalese forest cover declined at an annual rate of 2.7% between 1947 and 1990 and then at an annual rate of 1.23% between 1990 and 2010. Since

[4] Data extracted from the Nepalese census available in IPUMS. The closest substitute for firewood is dung (11.95% in 2001 and 12.55% in 2011).

1973, Nepal has established twenty PAs, comprised of ten national parks, three wildlife reserves, six conservation areas, and one hunting reserve. Together, those PAs cover 18% of the country's land. The Nepalese government began to engage the army in national park management in 1975. The Royal Nepalese Army is responsible for guarding the national parks and enforcing the regulations (Allendorf, 2007). Thus, there is meaningful enforcement of conservation (Yergeau et al., 2017; Heinen and Shrestha, 2006).

The International Union for Conservation of Nature (IUCN) divides PAs into six categories with respect to management objectives.[5] During the period 1996–2003, Nepal added three new PAs in IUCN categories I–IV (NPs and CAs) and six new PAs in IUCN category 6 (BZs). Aside from these PAs, all other forest land falls under the National Forest System. Community and leasehold forestry areas are also included in the National Forest System. Figure 1 shows a map of the PAs in Nepal, with treatment areas indicated; Table A1 of the Appendix lists information about the current structure of PAs in Nepal.

However, continuous degradation around PAs forced the Nepalese government to arrange a new system in 1996: the Buffer Zone Forest Management system. A Buffer Zone (BZ) is an area surrounding the core park to integrate conservation and development. BZs were designed to reduce human-wildlife conflict in Nepal. According to the law that created BZs in 1996, local communities close to PAs could benefit from park revenues (30 to 50 percent) (Bajracharya, 2004; Keiter, 2014; Nagendra et al., 2005). BZ policy allows some, though not unlimited, harvest of fodder; under selective criteria it also gives right to limited withdrawal or collection. BZs are generally monitored by the hired forest guards. Revenue is shared with people near BZs where

[5] Category I includes strict nature reserves and wilderness areas, Category II includes national parks, Category III includes natural monuments and natural landmarks, Category IV includes wildlife reserves and wildlife sanctuaries, Category V includes protected landscapes/seascapes, and Category VI includes managed resource protected areas.

income mostly come through tourists, supplemented by membership fees and sale of forest products (Nagendra et al., 2005). Table A1 provides the size of the BZs in the study areas.

In a related policy the government has, since the late 1980's, gradually handed national forests over to local communities based on forest management plans devised by the District Forest Office and the local population. Any forest-using community can apply for a parcel of forest land in exchange for a promise to reinvest 30–40% of the revenue in the resulting community forest (CF) every year. A CF needs to be approved by the Forest Department as a user group to work collectively under community forest law. Harvest of grasses, fallen wood, and leaf are generally permitted. CFs are monitored by local volunteers. CF members have a limited degree of control on the management system (Nagendra et al., 2005).

## 3. Conceptual Framework

We develop primary hypotheses regarding the effects of PAs on household behavior (wood collected and time spent on wood collection) and total consumption expenditure by drawing on household models in previous research. Several researchers have developed household utility models of forest resource users in developing countries (Bardhan & Udry, 1999; Albers and Robinson, 2007, 2015). We draw on a simple model from Bode et al. (2014) that is particularly relevant to our empirical questions in which households maximize utility by making two types of choices. Households divide their time between gathering a resource (in our case, wood) from a forest for use by the household itself and working outside the home for wages. They choose exactly where in the forest to go to collect wood in order to minimize the time cost of gathering a given amount of wood. They can spend their wage earnings on a substitute for the forest good or a bundle of other goods such as food. Households take market prices, wage rates, and the spatial distribution of forest resources as given. Additionally, the path that households can take in the forest may be constrained by the presence of PAs, causing the minimum time needed to gather a given quantity of wood to increase for all quantities.

The model in Bode et al. (2014) generates several testable hypotheses regarding the effects of a newly established PA, assuming wages and prices remain constant. Specifically, the model predicts that a new PA will cause households to gather less wood from the forest, to consume less wood in total, and to have lower utility overall; we will test those predictions, using consumption as a partial proxy for well-being. The impact of a PA on time spent gathering wood is theoretically ambiguous in the model of Bode et al. (2014) itself, because the household will choose to gather less wood, but the time needed to gather any particular amount of wood is greater. This paper will shed light on whether PAs in Nepal increase or decrease time spent gathering wood.

New PAs in Nepal take two different forms. National Parks (NPs) or Conservation Areas (CAs) are under IUCN category 1 and 2 and are stringently protected. The second type of new PA is a BZ which is established around previously protected NPs. BZs are less stringently protected under IUCN 6 category; some income generating activities are allowed in them (Budhathoki, 2004). We hypothesize that the impact of a BZ on firewood collection is smaller than the impact of a NP because BZs are less restrictive. We also hypothesize that establishment of a BZ will have a less negative effect on household welfare than a NP because of the income associated with BZs; indeed, the impact of a BZ on welfare could even be positive.

The household model makes clear that other behaviors and outcomes could be affected by PAs. First, in the model of Bode et al. (2014) with a well-functioning market for alternatives to wood, households are likely to increase purchases of those market fuel alternatives in response to a new PA; we test that hypothesis in our setting of rural Nepal where markets for forest goods and wood alternatives are limited. Second, because PAs alter the marginal product of time spent gathering wood, we hypothesize that household labor supply (both numbers of hours and employment status) may change, especially for women who are the primary wood collectors. Third, new technology could be adopted. More than 95% of Nepal's rural people use either mud stoves or open fireplaces. Though both these types of stoves use firewood to produce energy, a mud stove is environmentally more efficient, so a household could adapt to a PA by switching to a market fuel or an energy-efficient wood stove. Fourth, people in a household may choose to migrate to another location if a PA drives household utility falls below a certain level. Fifth, households may do more to organize CFs as an alternative source of firewood, since access to a CF is likely to reduce the negative impacts of PAs (Ostrom, 2008) and the Nepalese government will allow any group of people living near a forest to form a CF if they apply to district administration We test whether PAs drive any of those five types of changes in rural Nepal.

Finally, the effects of PAs on wood collection are likely to vary with village characteristics that influence the level and elasticity of wood demand. We hypothesize that the absolute quantity of wood collection reduction will vary among geographic regions. The hill area has the densest forests and greatest baseline use of collected fuelwood; this could mean that households in the hills are able to decrease wood collection more than people in other areas without hardship, or it could mean that those households resist change because their lifestyles are adapted to heavy use of collected wood. We also expect that people may be less likely to abide by fuelwood collection restrictions during cold months when wood is critical for home heating. Thus, our final regressions will test for treatment heterogeneity across region and season.[6]

[6] We also hypothesized that treatment effects might be higher if households had extensive access to livestock (since dung is a common substitute for wood as a fuel) and in places with many community forests before the PA was

## 4 Data

We collect information on PA locations and establishment years from the World Database on Protected Areas (2011). We use that to define which villages fall into each of the treatment and control groups. Our other primary data source is the Nepal Living Standard Survey (NLSS) collected by the Nepalese government with the assistance of the World Bank. In its construction, this dataset is similar to the well-known Living Standard and Measurement Survey collected by the World Bank. The dataset is nationally representative and has detailed information on the collection of goods from forests, including the time spent on collection, the amount collected, and the types of the forests. NLSS also includes a community survey that includes information on distance to forests. Basic data are collected for each individual in the household. In this study, we focus on only rural Nepal, as the forest-goods demand structure is different in urban areas (Baland et al., 2010).

We use the first two waves of the NLSS, collected in 1995/1996 and 2003/2004. NLSS-1's sample frame was taken from the 1991 census, and NLSS-2's sample frame was taken from the 2001 census. NLSS-1 and -2 follow the same survey stratification: they divide Nepal into ecological zones of mountains, hills, and low land. The probability sampling units (village wards) were selected from those ecological zones. We use NLSS-provided sampling weights whenever necessary in the analysis. In total, NLSS-1 interviewed 3388 households and NLSS-2 interviewed 3912 households. NLSS has both repeated cross-section and panel data. As the number of households in the panel is small (though nationally representative), we utilize it only for a robustness check for the repeated cross-sectional analysis. The Central Bureau of Statistics provides some other household-level datasets including NLSS-3 collected in 2010, but the

established so they had access to an alternative. No results supported those hypotheses, so the analyses are not reported here to reduce tables.

change in sampling strategy in NLSS and change in the political system prohibited us from using those in a causal identification framework.

The dependent variables that come from the NLSS include quantity of firewood collected, firewood collection time, expenditure per capita, food expenditure per capita, fuel choice, labor supply, number of migrants, employment status, and type of stove used. We also use NLSS data to create a proxy for membership in a CF equal to a binary dummy for whether a household gathers wood from a CF (Baland et al., 2010).

Several explanatory variables come from the NLSS. We use the interview month mentioned in the NLSS survey to create a dummy for the season in which the survey was conducted. The NLSS identifies which villages are in the lowland, hill, and mountain regions. We also use several other control variables derived from the NLSS (with coefficient results not reported in tables): household size, age and literacy of household head, and number of households in a village.

All control groups and variables used in the analyses are defined in Table 1.

## 5 Estimation and Identification Strategy

This section explains our strategy for generating plausible estimates of the causal effects of PAs in Nepal. We use a DID approach exploiting two sources of variation in order to construct sound estimates of the counterfactual outcomes: the distance from each household to the nearest PA and changes in the treatment and outcome variables over time.

### *5.1 Empirical framework*

The unit of observation is a household. Our main outcome variables of interest are the amount of firewood collected, the time spent collecting firewood, and per-capita consumption expenditures. The main independent variable is a policy indicator that equals one if the household is "treated" by being near any newly established PA and zero otherwise. In our main analyses, we define "near" as "within 20 kilometers". NLSS data is not geocoded, so we use the names of the villages provided by NLSS to map them with PAs, using the distance from each village to the border of the nearest PA. The treatment group consists of 372 households in 1995/1996 and 540 households in 2003/2004. In robustness checks we change the definition of proximity to PAs to see how that affects the results.

Using the potential outcome framework, our empirical strategy is to compare changes in outcomes in regions with newly established PAs to changes in outcomes in other areas where similar types of forest livelihood exist. The basic regression framework we use is the standard form of DID regression. Following Imbens & Wooldridge (2009) and Angrist & Pischke (2015), we use a repeated cross-sectional data based DID method where the composition of treatment and control group is unchanged:

$$Y_{ist} = \alpha + \beta D_s + \delta Post_t + \rho(D_s * Post_t) + \gamma X_{ist} + \varepsilon_{ist} \quad (1)$$

where *i* indexes the individual, *s* indexes the group (treated village), and *t* indexes time. $D_s$ is the treatment dummy and is equal to one if a household is in a village *s* that is in the treatment group. $Post_t$ equals one if an observation is in the after-treatment period and zero otherwise. The policy dummy, $(D_s * Post_t)$, is an interaction of the treatment dummy and the post-period dummy, and is equal to one if a household is in a village *s* that is in the treatment group after the establishment of a new PA. The impact coefficient, $\rho$, will capture the impact of PAs. The potential outcome framework works for repeated cross-section data as long as the composition of the group is same (Imbens and Wooldridge, 2009). As the Nepal NLSS is a nationally representative sample based on the population census, it is reasonable to use our repeated cross-section sample for a difference-in-difference analysis.

The vector of covariates is $X_{ist}$ . In our preferred specification, this will include the other important household- and village-level variables that can have effect on the outcomes: household head's education level and age, the household size, the number of households in the village. We also control for the ecological belt of the country (hill, mountain, or low land) in case that is correlated with unobservable factors.

The main empirical challenge is to find a suitable counterfactual - a measure of what would have happened to the households if they had not been subjected to a nearby PA. To identify the counterfactual, we need communities near forests that are comparable to the treatment group in the covariates. Matching with observed covariates cannot solve the problem, as "forest dependency" and "PA site/location choice criteria" are not observed in the dataset. To reduce the bias potentially introduced by these unobservable differences in forest livelihood across the treated and untreated group, we construct three separate control groups that face different level of bias. We construct a main control group of "never treated" households (designated as control group A) who live near a forest, but not near any PA site. To determine the distance from the forest, we use the NLSS community survey question for "distance to forest", where the distance is measured in units of time in hours. Households in control group A are defined in two ways: they live at most six hours one way from a forest, and they live at least 40 km away from all PAs established before December 2017. Control group A consists of 276 households in 1996 and 660 households in 2003. Distance is self-reported by the village head and therefore might include a measurement error.

Another concern is site selection bias (Allcott, 2015). The “never treated” control may have a flatter trend of wood collection than the communities living around the treatment PAs if we suspect that PAs have been preferentially established in areas where firewood collection has been increasing more rapidly than the areas around non-PA forests. Another possible source of bias is that people might be reluctant to report the actual quantities of firewood collection in the survey, if they know that they are not supposed to be collecting in the park. They may know the patrolling schedule too and can just steal otherwise (Bajracharya, 2004). Such unobserved difference would yield overestimates of the impacts.

To evaluate robustness to such potential biases, we use two other control groups. A “treated earlier” group (control group B) is comprised of households within 20 km of any PA established before 1995. We exclude the PAs established before 1995 that were extended to BZs during the study period. Villages in areas near previously established PAs may have similar features to areas near our treatment PAs. Similarly, we use a third control group of people living near PAs that were established only after our study period (in December 2009 and January 2010) designated as the “treated later” control group, or control group C. A comparison of the results obtained using these three control groups can yield bounds on the treatment effect if the conventional adjustments in pre-treatment covariates fail to remove all bias (Rosenbaum, 1987; Meyer, 1995) and if we suspect the presence of differential trends between the treatment and control groups. The construction of three control groups is summarized in Table 1.

Recall that Nepal has different categories of PAs. We divide these into two groups: one for CAs and NPs and another one only for BZs. We merge CAs and NPs together into a status designated NP because both of these have similar strict protection and villagers face same constraints from them. To understand the different impacts of these types of PAs, we use a multiple-group DID regression framework, which is a slight modification of equation (1):

$$Y_{ist} = \alpha + \delta Post_t + \beta_1 BZ_s + \beta_2 NP_s + \rho_1(BZ_s * Post_t) + \rho_2(NP_s * Post_t) + \gamma X_{ist} + \varepsilon_{ist} \quad (2)$$

*BZ* is equal to one if the household is in a village that is near a BZ and zero otherwise, and *NP* is equal to one if the household is in a village that is near a NP or CA and zero otherwise. Following the conceptual framework, we expect that $\rho_1$ and $\rho_2$ will be negative for the fuelwood collection dependent variable and that $|\rho_1| < |\rho_2|$.

Finally, we analyze potential heterogeneous treatment effects using variation in geography and in the season during which the survey was conducted. For that purpose we use the regression below where "H" denotes the heterogeneity, and $\delta_1$ and $\delta_2$ capture how heterogeneity affects the average treatment effect:

$$Y_{ist} = \alpha + \delta Post_t + \beta_1 BZ_s + \beta_2 NP_s + \beta_3 H + \rho_1(BZ_s * Post_t) + \rho_2(NP_s * Post_t) + \gamma_1(BZ_s * H) + \gamma_2(NP_s * H) + \delta_1(BZ_s * Post_t * H) + \delta_2(NP_s * Post_t * H) + \theta X_{ist} + \varepsilon_{ist} \quad (3)$$

### *5.2 Identifying Assumptions*

At a minimum, we need unconfoundedness to establish causality. Given our assumption that the treatment effect does not vary over households, unconfoundedness, or the conditional independence assumption, is equivalent to independence of the treatment assignment and error, conditional on covariates. This assumption cannot empirically be tested, but we employ an indirect strategy to explore it. We conduct a placebo test by pretending to estimate the causal effect of the new PAs in our study period on a treatment group that is known not to have been affected by them – the households near the PAs that were established in the period 2009–2010, long after our study period. These "false" or experimental households should not have been affected by the PAs that were established during the study period (1996–2003).

We also need a stronger assumption than unconfoundedness. The DID result can be interpreted as the causal effect of PAs only under the assumption that in the absence of a PA the increase in outcomes would not be systematically different in these two groups. In other words, there need to be parallel trends in the outcome variables. Unfortunately, Nepal does not provide any national household-level data prior to 1996. The Demography and Health Survey 1987 is the only pre-1995 household survey provided by the Nepal Central Bureau of Statistics, but this is only a focus group study. Moreover, the Demography and Health Survey 1995 does not provide any geographic information owing to privacy concerns. In the absence of any pre-baseline data, it is impossible for us to check the parallel trend assumption by gathering data for a longer period.

However, to develop some intuitive evidence regarding whether trends in conditions in those villages are parallel to each other, we look at three sets of data over time. First, we employ the lifestyle data from the community questionnaire. NLSS asks the community head whether the village has been on an upward welfare trend over the last five years. In 1996, 86% of the control group and 88% of the treatment group confirmed that their village was in an upward trend. In 2003, these numbers went down to 68% and 71% for the control and treatment groups, respectively, because of the nationwide economic crisis. Although this welfare trend is a self-reported qualitative measurement, the similarity between the treatment and control groups by time shows a similar trend for welfare. Second, we present the district-level human development index (HDI) in Figure 4 to obtain a crude idea about the pre-treatment welfare trend. Comparing with all control groups, we see that HDI shows a similar trend in the pre-treatment period. Third, we present district-level forest cover data that show similar trends among groups in the pre-treatment period in Figure 5; the treatment PA forests were more deforested than control group A but less deforested than control groups A and B, and the trend of deforestation is similar for all groups.

The next requirement for a valid DID analysis is that the support for the distribution of the conditioning covariates in the treatment group should overlap with the support for the distribution of these covariates in the comparison group. Economic theory helps us classify which variables need to be balanced, based on their role in the theoretical model of household behavior. The model says that a household's treatment status and corresponding reaction depend on the distance to a forest, household size, and household-specific demographic characteristics and asset levels. As PA site selection may depend on the possibility of revenue generation, we can proxy for it with variables such as distance to market, slope, and population size. Tables A3 through A8 in the Appendix present the covariate balance at the village level and at the household level for each of the control groups. Many things are balanced; however, we see that the treatment group was always collecting more firewood than the control groups during the pre-treatment period. Additionally, Nepal is geographically very dispersed, which makes the district-level maximum elevation factor imbalanced. As a robustness check, we run an analysis in which the mean elevation is balanced and find this correction does little to the results.

## 6 Results

In this section, we present the results of our average treatment effect estimates from regression equations (1), (2) and (3) and a series of robustness and placebo tests.

### *6.1 Basic Average Treatment Effects*

We begin by estimating a specification equivalent to regression equation (1) for the actual experiment using repeated cross-section data. As the sampling procedure is exogenous, regression estimates are not weighted using sampling weights (Solon et al., 2013). Table 2 presents the results of analyses using each of the three control groups. Recall that the coefficients for the policy variable (post*treatment) are the impacts of PAs on the surrounding households.

Column 1 shows that proximity to PAs causes average firewood collection to decrease; this result is significant in the analyses for all three control groups. The result with the primary "never treated" control group (A) implies that firewood collection drops by 0.106 bhari per day (a bhari is a basket that people can carry on their backs, supported by a brace). For the alternative control groups, "treated earlier" (B) and "treated later" (C), per-day firewood collection decreases by 0.0921 bhari and 0.0996 bhari, respectively. This core treatment effect is stable; it appears that comparing treatment PA communities to non-PA forest communities does not overestimate the impacts.

The regressions in columns 2, 3, and 4 do not find robust and significant effects of protected areas on wood collection time or expenditure. The treatment coefficient for wood collection time is always positive, but it is only significant for control group C and then only at the 10% level. The effects of PAs on expenditure per capita (total and on food alone) are if anything positive, but the only statistically significant result is for total expenditure per capita with control group A.

We have a natural "placebo" experimental group of households living near PAs that were established in 2009 and 2010. This placebo treatment group should not have any change in firewood collection behavior during our study period as those PAs were created after the study period. This placebo treatment group is control group C in the main analysis. When we estimate equation (1) with the placebo treatment group and either control group A or B, there is no significant change in firewood collection, as can be seen in Table 3. This implies that the average treatment effect we find for the actual treatment group is not due only to a differential trend; otherwise, we might also see a significant non-zero impact for this placebo treatment group.

### *5.2 Average Treatment Effects of Two Types of PAs*

We now estimate the impacts of different kinds of protection areas with regression equation (2). Tables 4, 5, and 6 present the results for control groups A, B, and C respectively. We find further evidence that protected areas reduce wood collection in column (1) of all three tables. The treatment coefficient for the strict NP/CA category of PAs is robustly negative and significant; NP/CA type PAs are estimated to reduce firewood collection by 0.151, 0.164, and 0.179 bhari per day when using control groups A, B, and C, respectively. BZ protected areas have significant and negative coefficients for control groups B and C (though not A). As hypothesized, BZ protected areas lead to smaller reductions in wood collection than strict PAs; the coefficients on the BZ treatment variable are only -0.06 and -0.07 for control groups B and C – less than half the magnitude of the effects estimated for strict PAs.

Consistent with the results in Table 2, we find no evidence that either NP/CA nor BZ areas have robust statistically significant effects on per capita expenditure or time spent collecting firewood. Dividing the treatments up by type does not reveal an effect that was obscured in the earlier pooled analysis.

### *5.3 Robustness Checks and Magnitudes*

We investigate the robustness of the estimated impact of PAs on firewood collection in a number of alternative specifications using control group A; these results are presented in Table

A8 of the Appendix. First, on the premise that people in low lands may have different firewood demands than in hills and mountains (Baland et al., 2010), we estimate regression Equation 2 after dropping low land areas (Panel A). In Panel B, we estimate the results after balancing the mean elevation of the treatment and control groups; extreme elevations have been removed from this analysis. Overall, the results are quite robust. Table A9 of the Appendix also presents the result of changing the definition of treatment to a proximity of 10 km to a PA (rather than 20 km). For strict PAs, this does not cause any meaningful change in the results. The treatment effect for BZs using control group A is now also significant and negative, but the effect of BZs is still estimated to be much smaller than for strict PAs. Finally, to complement our main analyses of repeated cross-section data we estimate equation (1) on the actual NLSS panel data with 84 observations for the treatment group and 84 observations for the control group. The results in Table A11 of the Appendix are similar to the repeated cross-section results; PAs are estimated to reduce firewood collection by 0.204 bhari per day but have no significant impact on collection time.[7]

To understand policy significance, we must evaluate the magnitude of the estimated effects of PAs on wood collection. The unit of measurement for firewood collection is the bhari, which is a local Nepalese measurement unit. We can convert bhari to kilograms using conversion rates available in NLSS; one bhari equals 33.28 kg. A simple calculation thus predicts an average household will collect 1 to 6 kg less firewood per day when a PA is established nearby. On average, these villages have around 150 households, so an average village collects at least 150 kg less firewood every day because of a PA. Table 7 calculates the percentage changes in fuelwood consumption implied by the statistically significant results of Tables 2, 4, 5, and 6. In general,

[7] Only 30 households in this panel live near strict PAs, which prevents us from estimating Equation 2.

PAs reduce wood collection by about 30%. When broken up by type, the results imply that strict PAs can reduce collection by 33-40%, and more accommodating BZ arrangements appear only to cut wood extraction by 20-26%.

### *5.4 Mechanisms and Heterogeneity*

The conceptual framework identified several other behaviors and outcomes that could be affected by PAs. Migration and creation of CFs could mitigate negative impacts of PAs on household well-being. Increased fuel purchases and stove switching could help mitigate the decrease in utility associated with reduced access to self-collected fuelwood. Finally, PAs could affect labor supply in several ways; time allocation might shift from wood collection to labor supply as PAs alter the productivity of time spent on collection, and changes in tourism could improve work opportunities for people. Tables 10, 11, and 12 present DID regressions for each of the control groups to explore the impacts of PAs on market purchases of fuel, labor supply and employment, outmigration from a village, adoption of a more efficient stove, and formation of a CF.

The first column of the three tables shows results of a linear probability model estimating the impacts of PAs on whether a household purchases fuel from the marketplace. We find that strict PAs do increase the probability that a household buys fuel instead of only using fuel they collect themselves, but BZ areas have no such effect. These results hold for all control groups. The NLSS data reveal that only 2% of the people in our data use market stoves, but those rates appear to be increased by creation of national parks that cut off wood collection opportunities.

None of the other results are highly robust across control groups. The coefficient for strict PA introduction is always positive in the regressions for female labor supply and whether the household purchases a more fuel efficient stove, but those coefficients are only significant for one of the control groups. More collaborative BZ type PAs have negative coefficients for female and male labor supply in most of the regressions, but those findings are only sporadically significant. Finally, the estimated effects of PAs on migration, development of CFs, and unemployment are even less robust – the coefficients have inconsistent signs and are rarely significant.

Our last regressions explore how the impacts of PAs on wood collection vary with season and with geographic region. These are triple-difference-in-difference regressions. Households in control groups B and C ("treated before" and "treated after") are only located in a few parts of the country, and thus those control groups do not have sufficient orthogonal variation in all the variables to support the triple diff approach. Hence, Table 11 only presents results for control group A ("never treated.")

We look first at columns (2) and (4) which estimate how the impact of PAs vary with the season in which the survey was administered. The omitted category of season is Summer. The coefficients on the baseline impact variables are still negative and significant; during summer, PAs on average reduce wood collection by 0.149 bhari per day, and when we estimate the effects separately for NP/C and BZ areas, the decreases are 0.206 and 0.139, respectively. The Rainy season and Autumn do not have different effects. However, the interactions between Winter and the treatment variables are positive and significant. PAs cause essentially no net reduction of wood collection in the cold season.

Nepal has three geographically distinct areas: mountain, hill and terai (low land). The results in columns (2) and (4) of Table 11 estimate how the effects of PAs on wood collection vary between those regions. The omitted geographical category is mountain, so the significant baseline treatment effects indicate that PAs reduce wood collection in mountain areas by 0.155

bhari overall, and by 0.213 and 0.115 bhari for NPs and BZs, respectively. The interaction terms for "Lowland" are not significant, so the effect seems to be the same in the lowland areas as in the mountains. However, the interaction coefficients are positive and significant for the "Hill" variable in both regressions; the net impact of PAs on wood collection is essentially zero in the hill region.

## 7 Conclusions

We find evidence that establishment of PAs in Nepal led to a decrease in firewood collection, and thus firewood consumption, in households that live near the PAs. Our estimates indicate that PAs caused household firewood collection to fall by 20% to 40% (Table 7); the size of effect depends on the restrictions placed on the area. This decrease in wood collection may be reflected in Figure 2, where we see evidence from satellite data that forest cover did indeed increase in several of the treatment PAs after PA establishment.[8] PAs appear to be effective in accomplishing their primary goal of slowing forest degradation.

Despite the sizable decrease in wood collection, we find no evidence that the establishment of PAs affected per capita consumption of the people who live near them. In the short time period of our study, PAs have not inflicted great harm nor has establishment of an ecotourism industry led to great gains for people close to new PAs. These results are robust to different alternative specifications and survive internal validity tests, falsification tests, and inclusion of a wide range of control variables.

Finally, we find two significant types of heterogeneity in the effects of PAs on wood collection. Results show strong seasonal variation in the impact of PAs; during the cold season, households may be gathering wood from areas that are supposed to be off limits to mitigate what

[8] Forest cover loss data was extracted from Hansen et al.'s (2013) dataset (Figure 2).

would otherwise be real hardship from lack of fuel for heating. PAs also have little impact on wood collection behavior in the hill region where people previously used the most wood. It may be difficult to effect change in forest extraction activity when people and their culture are adapted to heavy forest use.

This study has policy implications for the design of conservation instruments. First, the results show that PAs can result in reduced forest-good collection without necessarily having a significant negative effect on total household consumption. However, that total consumption variable may not reveal some actual negative consequences of reduced wood collection because of the non-monetary nature of the forest good consumption. While we find that PAs spur some market fuel purchases, most households in Nepal are not shifting toward market substitutes for self-harvested wood as suggested in many theoretical models (Bode et al., 2015). This pattern may be driven by low access to market, and may yield hardship to families who must make do with less fuel. Efforts to improve household access to alternative fuel sources and stoves may increase social well-being in villages near PAs in rural Nepal.

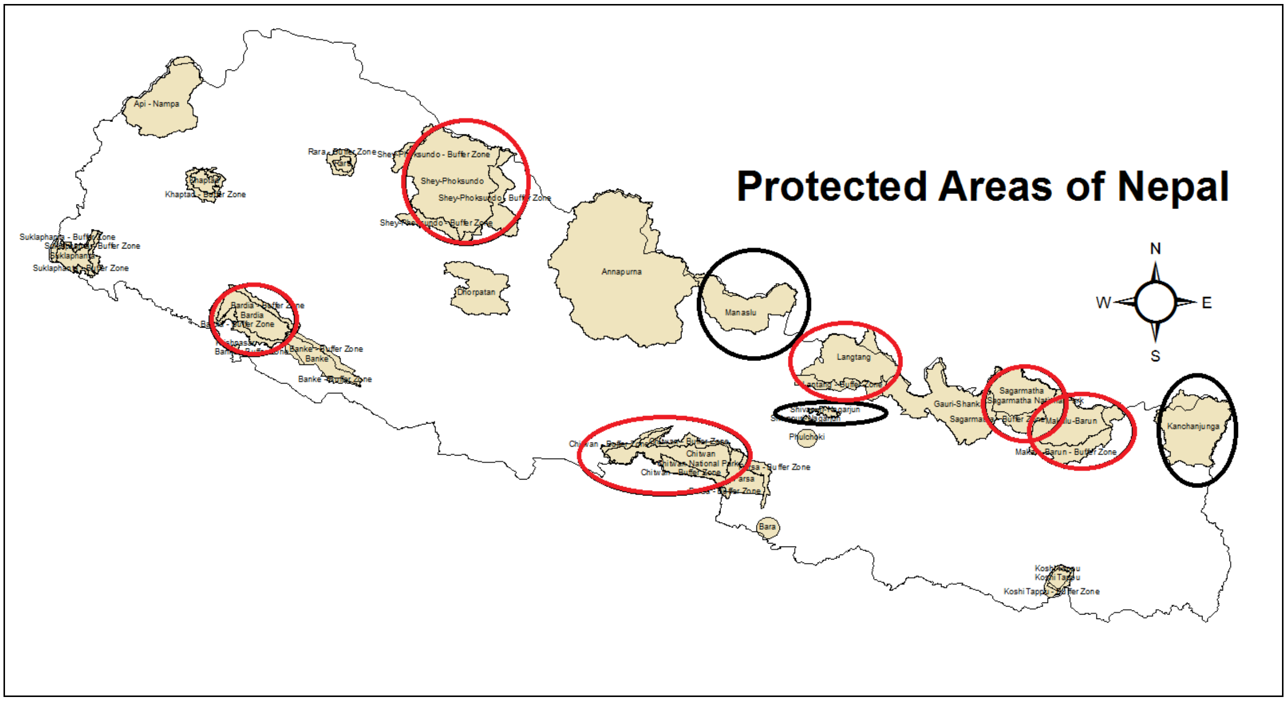
Second, more stringent conservation measures can indeed produce greater reductions in forest exploitation. Results in this paper suggest that the strict PAs produce greater reductions in forest degradation than areas with more lenient arrangements. Conservation planner should be cautious in deploying flexible PAs like Nepal's Buffer Zones if major reduction in wood collection is needed to accomplish a time-sensitive conservation objective.

Third, planners should not assume that PAs will attract ecotourism that will immediately make local people better off; we find no evidence of that occurring in Nepal. It is possible that these results are Nepal-specific and cannot be generalized to other situations with different political economies of forest management. However, disentangling the direct impacts and market consequences of PAs is particularly important in light of IUCN's recent advocacy of the Payment for Ecosystem Services in Nepal (Paudel et al., 2015).

Fourth, planners should expect that the effects of PAs will vary with the elasticity of forest-product demand in different areas and climatic conditions. One cannot expect people to freeze in winter just to obey restrictions on fuelwood collection, and behavior change may be slow in places where households have few substitutes and are accustomed to making heavy use of the forest products planners hope to put off limits.

Our study has several limitations. First, it does not include indirect (displacement) impacts of tourism on the labor market outcomes. Employees in the tourism sector may be temporary migrants and omitting this spillover may underestimate the overall welfare impacts. However, NLSS data do not allow us to check for this possibility. Second, PA-based infrastructure development may take longer than the duration of our study to influence local labor markets. Tracking these PA communities for a longer period of time will help distinguish steady-state equilibria from transitional impacts. Finally, this paper only estimates average treatment effects and does not identify distributional inequities in the impacts of PAs. The poorest cohorts may bear the largest adverse impact from PAs if they depend on the forest for basic survival. Answering these questions is beyond the scope of this paper, but future research could use the establishment of PAs to understand these effects.

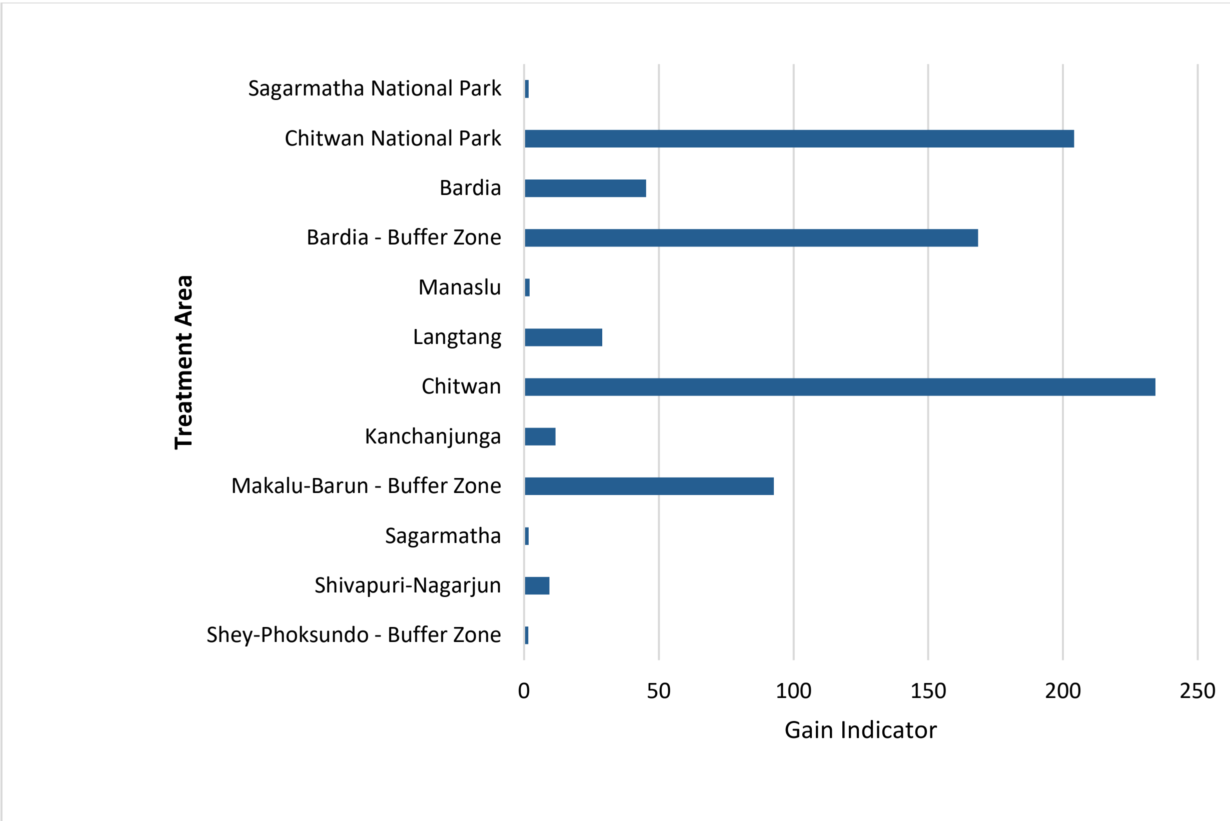
**Figure 1: Protected Areas of Nepal**




Note: Treatment areas are circled (PAs established in 1996-2003), BZ in red circle and NP/CA in black circle.

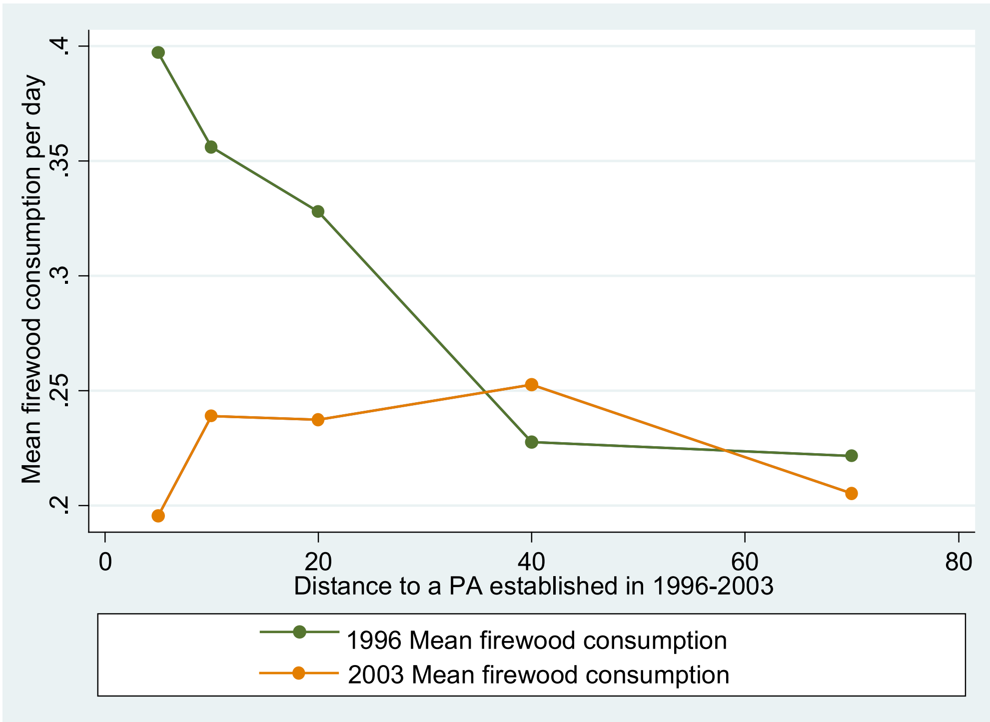
Source of shape file: World Database of Protected Area, Available at: www.protectedplanet.net.

**Figure 2: Forest Cover Gain in Treatment Protected Areas (2001-2014)**



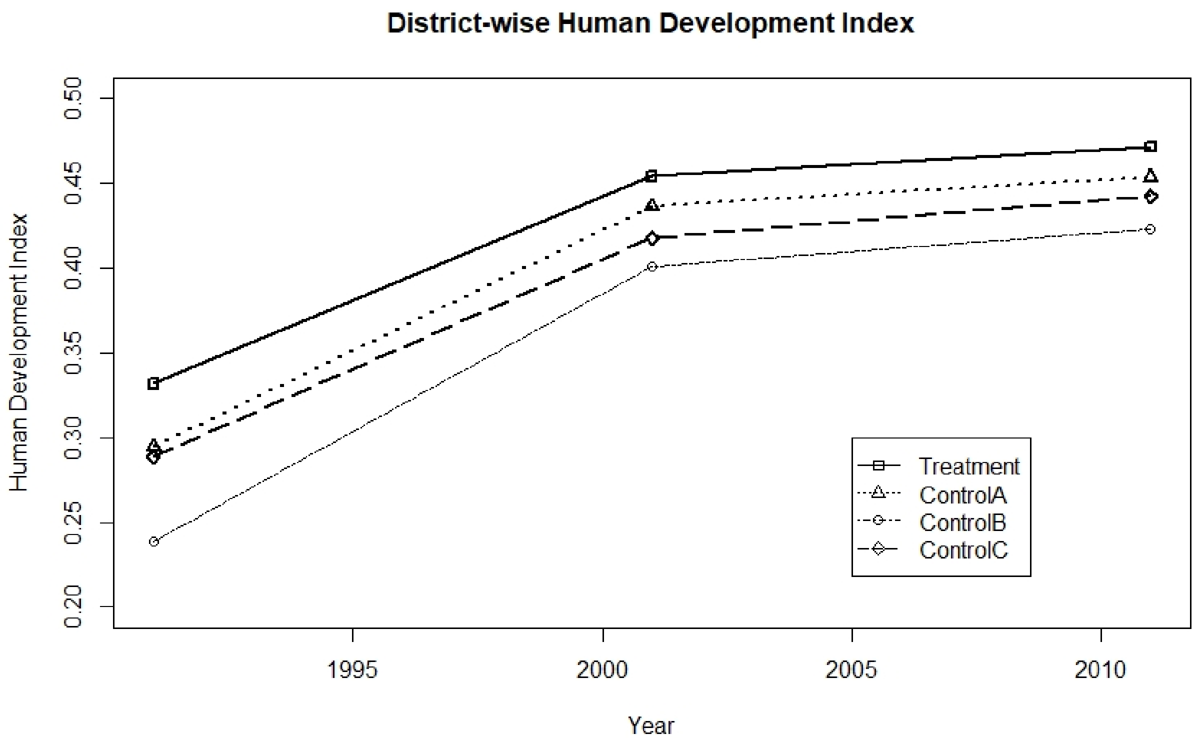

Note: These data were derived from Global Forest Change (2000 – 2014). The bars show forest gain during the period 2000–2014, defined as the inverse of loss, or a non-forest to forest change entirely within the study period. (Hansen et al., 2013).

**Figure 3: Correlation between Distance to PA and Forest-Good Collection**




Note: This graph shows the time series variation in the amount of collection against the distance to the nearest PA. The units of the vertical axis for firewood consumption is bhari/day.

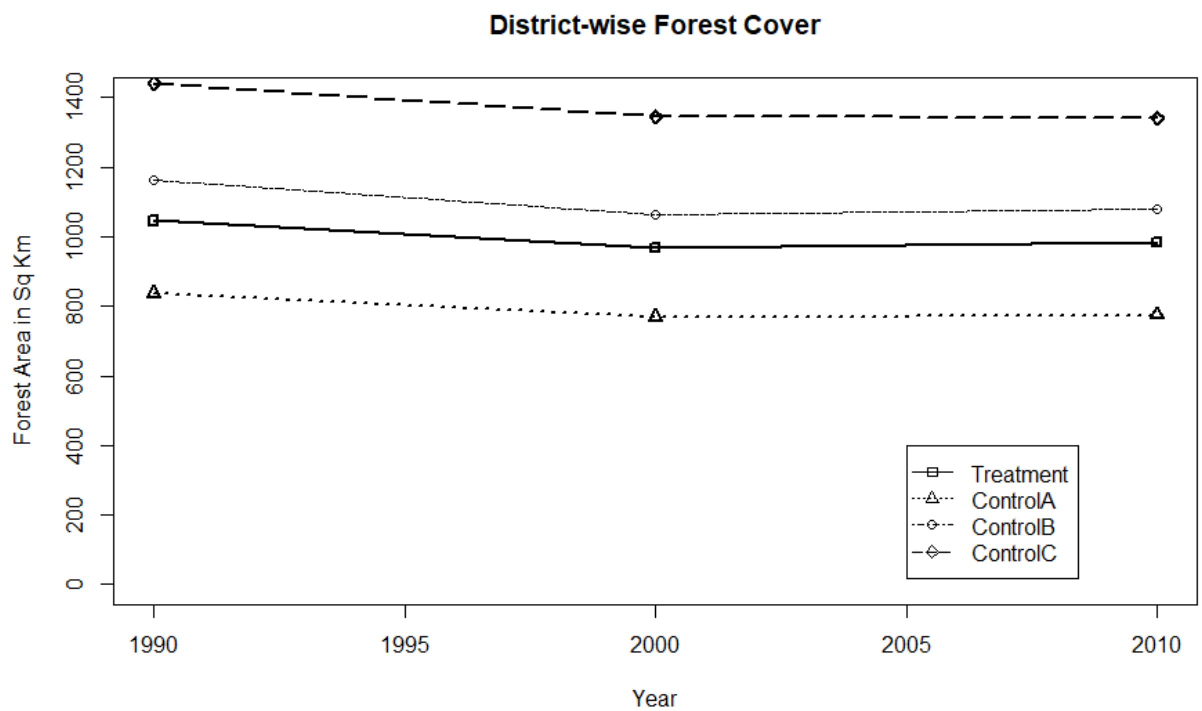
**Figure 4: District-Level Human Development Index[a,b]**




[a] The Human Development Index is a composite statistic of life expectancy, education and per capita income indicators Source: UNDP Human Development Index Report (2011).

[b] The control groups here are "never treated" (A), "treated earlier" (B), and "treated later" (C).

**Figure 5: District-Level Forest Cover Area[a,b]**




[a] Forest cover area was extracted from Landsat satellite database from ICIMOD and serves as a proxy for forest quality.

[b] The control groups here are "never treated" (A), "treated earlier" (B), and "treated later" (C).

**Table 1: Control Group and Variable Definitions**

| Observation Group or Variable | Definition |
|---|---|
| Treatment group[b] | Villages within 20 km of PAs established in 1996-2003 |
| "Never treated" control group[b] | Villages around forest but not within 40 km of any PA site |
| "Treated earlier" control group[b] | Villages within 20 km of PAs established before 1996 |
| "Treated later" control group[b] | Villages within 20km of PAs established after 2003 |
| Firewood collected | Quantity of firewood collected by household (bhari/day) |
| Firewood collection time | Average time spent collecting firewood each day (hours/day) |
| Expenditure per capita | Total consumption per person per year (Nepalese Rupee) |
| Food expenditure per capita | Food consumption per person per year (Nepalese Rupee) |
| Market fuel? | Dummy for fuel purchased from market |
| Female labor supply | Hours of labor supplied per day by female household head |
| Male labor supply | Hours of labor supplied per day by male household head |
| # of migrants | Total number of migrants from household that year |
| Stove choice? | Dummy for whether chose an energy-efficient wood stove |
| Community Forest? | Dummy for having access to a community forest |
| # Unemployed | Number of unemployed persons in the household |
| Season dummies | Survey conducted in Rainy, Autumn, Winter, or Spring season |
| Geography dummies[b] | Household in Mountain, Hill, or Lowland area |
| Household size | Number of people in household |
| Age | Age of household head in years |
| Literacy | Is household head literate? |
| Number of households | Number of households in village |

[a] Data source is Nepal Living Standard Survey (NLSS) unless indicated otherwise

[b] Data sources are NLSS and World Database on Protected Areas (2011).

**Table 2: Impacts of Protected Areas[a, b]**

| Control group A | (1) Wood Collected | (2) Wood Collection Time | (3) Expenditure/ Capita | (4) Food Expenditure/ Capita |
|---|---|---|---|---|
| Post*Treatment | -0.106** | 0.149 | 3,644** | 772.5 |
| | (0.0425) | (0.893) | (1,735) | (591.0) |
| Treatment | 0.104*** | 0.113 | -964.0 | 428.0 |
| | (0.0360) | (0.758) | (782.0) | (495.2) |
| Post | -0.00237 | -1.179* | 2,737*** | 2,017*** |
| | (0.0328) | (0.710) | (969.9) | (480.8) |
| Constant | 0.174*** | 5.659*** | -4,704* | 2,720*** |
| | (0.0458) | (1.088) | (2,573) | (841.4) |
| Observations | 1,719 | 1,597 | 1,819 | 1,819 |
| R-squared | 0.172 | 0.073 | 0.168 | 0.263 |
| **Control group B** | | | | |
| Post*Treatment | -0.0921*** | 0.0311 | 2,067 | 272.2 |
| | (0.0335) | (0.723) | (1,447) | (505.3) |
| Treatment | 0.120*** | -0.149 | 1,231** | 1,196*** |
| | (0.0255) | (0.591) | (577.2) | (335.9) |
| Post | -0.0122 | -1.128* | 4,662*** | 2,526*** |
| | (0.0197) | (0.583) | (852.8) | (301.5) |
| Constant | 0.177*** | 5.067*** | -4,168* | 1,734** |
| | (0.0397) | (0.710) | (2,504) | (763.2) |
| Observations | 1,267 | 1,250 | 1,491 | 1,491 |
| R-squared | 0.179 | 0.075 | 0.225 | 0.348 |
| **Control group C** | | | | |
| Post*Treatment | -0.0996** | 1.311* | 1,583 | 1,114 |
| | (0.0424) | (0.773) | (1,903) | (738.1) |
| Treatment | 0.111*** | -1.039 | 335.1 | 374.8 |
| | (0.0317) | (0.655) | (1,134) | (551.9) |
| Post | -0.00160 | -2.393*** | 4,935*** | 1,598*** |
| | (0.0306) | (0.711) | (1,799) | (592.7) |
| Constant | 0.215*** | 5.753*** | -4,851 | 2,499** |
| | (0.0477) | (0.994) | (3,110) | (1,087) |
| Observations | 1,061 | 1,051 | 1,180 | 1,180 |
| R-squared | 0.165 | 0.103 | 0.216 | 0.296 |

[a] Results are from DID regressions. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Variables are defined in Table 1.
[b] Every regression has a set of controls. The household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls are dummies for the geographic belts (mountain, hill or low land.)

**Table 3: Placebo Test[a]**

| | Quantity of Firewood Collected | |
|---|---|---|
| Control group[b] | A | B |
| Post*Placebo Treatment | 0.0124 | -0.00919 |
| | (0.0338) | (0.01911) |
| Placebo Treatment | -0.00562 | 0.02102 |
| | (0.0271) | (0.01443) |
| Post | -0.0214 | -0.00037 |
| | (0.0209) | (0.00965) |
| Constant | 0.181*** | 0.15798 |
| | (0.0191) | (0.01137) |
| Observations | 1,625 | 1,017 |
| R-squared | 0.052 | 0.067 |
| Controls[c] | Y | Y |

[a] The placebo treatment group is defined as households near the PAs established in 2009-2010, far later than our study period. Results are from DID regressions. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Variables are defined in Table 1.

[b] Control groups are "never treated" group A and "treated earlier" group B.

[c] Household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls include dummies for the geographic belts (mountain, hill or low land.)

**Table 4: Impacts of Different Types of Protected Areas, Control Group A[a]**

| | (1) Wood Collected | (2) Wood Collection Time | (3) Expenditure per Capita | (4) Food Expenditure per Capita |
|---|---|---|---|---|
| Post*NP/CA | -0.151** | -0.866 | 3,482 | 373.6 |
| | (0.0623) | (1.637) | (2,990) | (1,070) |
| Post*BZ | 0.0228 | -0.313 | -129.0 | -517.4 |
| | (0.0551) | (0.620) | (1,599) | (785.2) |
| Post | -0.0689 | -0.877* | 4,593*** | 2,678*** |
| | (0.0526) | (0.464) | (1,408) | (750.4) |
| NP/CA | 0.0209 | 0.840 | 639.0 | 568.5 |
| | (0.0459) | (1.621) | (1,347) | (890.7) |
| BZ | -0.0646 | 0.517 | -1,961** | -141.3 |
| | (0.0433) | (0.576) | (890.6) | (664.0) |
| Constant | 0.317*** | 5.272*** | -5,580** | 2,600*** |
| | (0.0493) | (0.717) | (2,565) | (951.9) |
| Observations | 1,719 | 1,597 | 1,819 | 1,819 |
| R-squared | 0.184 | 0.076 | 0.180 | 0.259 |
| Controls[b] | Y | Y | Y | Y |

[a] The control group is “never treated” A. Results are from DID regressions. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Variables are defined in Table 1.

[b] Household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls include dummies for the geographic belts (mountain, hill or low land).

**Table 5: Impacts of Different Types of Protected Areas, Control Group B[a]**

| VARIABLES | (1)<br>Wood<br>Collected | (2)<br>Wood<br>Collection<br>Time | (3)<br>Expenditure<br>per Capita | (4)<br>Food<br>Expenditure<br>per Capita |
|---|---|---|---|---|
| Post*NP/CA | -0.164*** | -0.433 | 2,110 | 6.648 |
| | (0.0528) | (0.962) | (2,006) | (691.4) |
| Post*BZ | -0.0568* | 0.288 | 1,661 | 301.2 |
| | (0.0339) | (0.777) | (1,648) | (602.7) |
| Post | -0.0145 | -1.115* | 4,602*** | 2,510*** |
| | (0.0194) | (0.581) | (828.0) | (286.9) |
| NP/CA | 0.208*** | 0.0447 | 3,266*** | 1,976*** |
| | (0.0363) | (0.896) | (906.8) | (532.8) |
| BZ | 0.0802*** | -0.240 | 556.3 | 915.8** |
| | (0.0271) | (0.612) | (606.9) | (367.6) |
| Constant | 0.180*** | 5.214*** | -5,077** | 1,528* |
| | (0.0356) | (0.704) | (2,394) | (808.8) |
| Observations | 1,267 | 1,250 | 1,491 | 1,491 |
| R-squared | 0.209 | 0.079 | 0.234 | 0.354 |
| Controls[b] | Y | Y | Y | Y |

[a] The control group is "treated earlier" B. Results are from DID regressions. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Variables are defined in Table 1.

[b] Household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls include dummies for the geographic belts (mountain, hill or low land.)

**Table 6: Impacts of Different Types of Protected Areas, Control Group C[a]**

| VARIABLES | (1) Wood Collected | (2) Wood Collection Time | (3) Expenditure per Capita | (4) Food Expenditure per Capita |
|---|---|---|---|---|
| Post*NP/CA | -0.179*** | 0.754 | 1,637 | 842.5 |
| | (0.0645) | (0.964) | (2,340) | (912.4) |
| Post*BZ | -0.0722* | 1.504* | 1,602 | 1,212 |
| | (0.0408) | (0.823) | (2,041) | (782.6) |
| Post | 0.00415 | -2.334*** | 4,643*** | 1,571*** |
| | (0.0316) | (0.708) | (1,708) | (581.0) |
| NP/CA | 0.195*** | -0.558 | 1,746 | 881.1 |
| | (0.0416) | (0.911) | (1,380) | (712.2) |
| BZ | 0.0699** | -1.245* | -775.2 | 71.21 |
| | (0.0349) | (0.677) | (1,103) | (559.5) |
| Constant | 0.222*** | 5.833*** | -5,177* | 2,471** |
| | (0.0437) | (0.998) | (3,082) | (1,110) |
| Observations | 1,061 | 1,051 | 1,180 | 1,180 |
| R-squared | 0.197 | 0.107 | 0.223 | 0.299 |
| Controls[b] | Y | Y | Y | Y |

[a] The control group is "treated later" group C. Results are from DID regressions. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** fpp<0.05, * p<0.1. Variables are defined in Table 1.

[b] Household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls include dummies for the geographic belts (mountain, hill or low land.)

**Table 7: Calculations of Percentage Change in Fuelwood Consumption**

| Control | Type of policy | Table with results | ATE[a] | Baseline (bhari/day)[b] | % Change[c] |
|---|---|---|---|---|---|
| A | Both | Table 2 | -0.106 | .328 | 32% |
| A | Just NP/CA | Table 4 | -0.151 | .447 | 33% |
| A | Just BZ | Table 4 | NA | .2926 | NA |
| B | Both | Table 2 | -0.0921 | .328 | 28% |
| B | Just NP/CA | Table 5 | -0.164 | .447 | 37% |
| B | Just BZ | Table 5 | -0.0568 | .2926 | 20% |
| C | Both | Table 2 | -0.0996 | .328 | 30% |
| C | Just NP/CA | Table 6 | -0.179 | .447 | 40% |
| C | Just BZ | Table 6 | -0.0722 | .2926 | 26% |

[a]ATE is the average treatment effect.

[b] Baseline is the average household consumption of fuelwood in treated villages reported in the Appendix. NA indicates result is not significant.

[c] % Change is calculated as (ATE/Baseline)*100.

**Table 8: Regressions Evaluating Mechanisms of Change, Control A[a]**

| | Market Fuel? | Female Labor Supply | Male Labor Supply | # of Migrants | Stove Choice? | Community Forest? | Unemployed |
|---|---|---|---|---|---|---|---|
| Post*NP/CA | 0.135** | 1.081* | 0.411 | 0.0953 | 0.319 | -0.127 | 0.0563 |
| | (0.0580) | (0.563) | (0.656) | (0.0697) | (0.212) | (0.0891) | (0.102) |
| Post*BZ | 0.0502 | -0.438 | -1.058* | 0.0347 | -0.0863 | 0.309*** | 0.0618 |
| | (0.0341) | (0.486) | (0.545) | (0.0747) | (0.158) | (0.101) | (0.0967) |
| NP/CA | -0.0252 | 0.131 | 0.446 | -0.138** | 0.0437 | -0.0232 | -0.0418 |
| | (0.0173) | (0.427) | (0.596) | (0.0553) | (0.110) | (0.0678) | (0.100) |
| BZ | -0.0499*** | 0.738* | 0.849* | 0.0146 | 0.0323 | -0.00783 | -0.0854 |
| | (0.0160) | (0.386) | (0.447) | (0.0644) | (0.0913) | (0.0475) | (0.0862) |
| Post | -0.0195 | 2.181*** | 1.372*** | -0.139** | 0.105 | 0.0869 | -0.372*** |
| | (0.0210) | (0.298) | (0.364) | (0.0564) | (0.123) | (0.0552) | (0.0693) |
| Constant | -0.110** | 8.109*** | 6.467*** | 0.249*** | 0.821*** | 0.303*** | 0.309** |
| | (0.0537) | (0.760) | (0.547) | (0.0850) | (0.173) | (0.0895) | (0.142) |
| Observations | 1,818 | 1,595 | 1,818 | 1,468 | 1,817 | 1,819 | 1,741 |
| R-squared | 0.096 | 0.208 | 0.157 | 0.051 | 0.182 | 0.150 | 0.096 |
| Controls[b] | Y | Y | Y | Y | Y | Y | Y |

[a] Results are from DID regressions. Regressions with binary outcome variables use linear probability model. The control group is "never treated" group A. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Variables are defined in Table 1.

[b] Household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls include dummies for the geographic belts (mountain, hill or low land.)

**Table 9: Regressions Evaluating Mechanisms of Change Control B[a]**

| | Market Fuel? | Female Labor Supply | Male Labor Supply | # of Migrants | Stove Choice? | Community Forest? | Unemployed |
|---|---|---|---|---|---|---|---|
| Post*NP/CA | 0.120** | 0.981 | 0.718 | -0.0857 | 0.359* | 0.312 | 0.124 |
| | (0.0503) | (0.672) | (0.696) | (0.0842) | (0.201) | (0.249) | (0.107) |
| Post*BZ | 0.0447* | -0.513 | -0.880 | 0.0391 | 0.0706 | -0.268 | 0.141 |
| | (0.0237) | (0.643) | (0.550) | (0.0958) | (0.118) | (0.172) | (0.0925) |
| NP/CA | -0.832 | -0.0519 | -0.146 | 0.0350 | 0.0105 | -0.100 | -0.338* |
| | (0.550) | (0.633) | (0.103) | (0.0455) | (0.0126) | (0.120) | (0.200) |
| BZ | -0.125 | 0.330 | -0.138 | 0.100 | -0.0109 | -0.0762 | -0.0423 |
| | (0.519) | (0.462) | (0.0854) | (0.0680) | (0.0094) | (0.0714) | (0.119) |
| Post | 2.92e-05 | 2.311*** | 1.191*** | 0.176** | 0.000653 | -0.289** | -0.421*** |
| | (0.0124) | (0.476) | (0.376) | (0.0684) | (0.0873) | (0.119) | (0.0652) |
| Constant | -0.0811* | 9.480*** | 7.648*** | -0.43*** | 1.064*** | 1.863*** | 0.547*** |
| | (0.0463) | (0.946) | (0.692) | (0.0975) | (0.163) | (0.154) | (0.154) |
| Observations | 1,818 | 1,595 | 1,818 | 1,468 | 1,817 | 1,819 | 1,741 |
| R-squared | 0.096 | 0.208 | 0.157 | 0.051 | 0.182 | 0.150 | 0.096 |
| Controls[b] | Y | Y | Y | Y | Y | Y | Y |

[a] Results are from DID regressions. Regressions with binary outcome variables use linear probability model. The control group is "treated earlier" group B. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Variables are defined in Table 1.

[b] Household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls include dummies for the geographic belts (mountain, hill or low land.)

**Table 10: Regressions Evaluating Mechanisms of Change Control C[a]**

| | Market Fuel? | Female Labor Supply | Male Labor Supply | # of Migrants | Stove Choice? | Community Forest? | Unemployed |
|---|---|---|---|---|---|---|---|
| Post*NP/CA | 0.0839* | 0.283 | 2.129*** | -0.169** | 0.255 | 0.153 | 0.0652 |
| | (0.0462) | (0.672) | (0.769) | (0.0782) | (0.225) | (0.315) | (0.119) |
| Post*BZ | 0.00802 | -1.276* | 0.383 | -0.0776 | -0.0876 | -0.496** | 0.0171 |
| | (0.0289) | (0.697) | (0.646) | (0.0860) | (0.176) | (0.236) | (0.0776) |
| Post | 0.0291 | 3.065*** | -0.118 | 0.273*** | 0.130 | -0.0829 | -0.350*** |
| | (0.0256) | (0.543) | (0.505) | (0.0628) | (0.164) | (0.211) | (0.0583) |
| NP/CA | 0.0129 | -0.850** | -1.233** | 0.0750* | -0.115 | 0.0260 | -0.00585 |
| | (0.0189) | (0.396) | (0.571) | (0.0423) | (0.155) | (0.237) | (0.0973) |
| BZ | -0.0164 | 0.0740 | -0.464 | 0.0854 | -0.0710 | 0.374* | -0.0413 |
| | (0.0146) | (0.424) | (0.491) | (0.0547) | (0.121) | (0.189) | (0.0768) |
| Controls | Y | Y | Y | Y | Y | Y | Y |
| Constant | -0.158*** | 10.45*** | 7.832*** | -0.528*** | 0.956*** | 1.425*** | 0.179 |
| | (0.0557) | (0.876) | (0.646) | (0.116) | (0.236) | (0.200) | (0.112) |
| Observations | 1,179 | 1,008 | 1,171 | 1,180 | 1,178 | 1,148 | 1,131 |
| R-squared | 0.111 | 0.254 | 0.133 | 0.347 | 0.143 | 0.198 | 0.099 |

[a] Results are from DID regressions. Regressions with binary outcome variables use linear probability model. The control group is "treated later" group C. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Variables are defined in Table 1.

[b] Household controls are household size, age and literacy of the household head. The village control is number of households. Geographic controls include dummies for the geographic belts (mountain, hill or low land.)

**Table 11: Treatment Effect Heterogeneity[a]**

| | (1) Seasons | (2) Altitude | (3) Seasons | (4) Altitude |
|---|---|---|---|---|
| Post*Treat | -0.149** | -0.155*** | | |
| | (0.0621) | (0.0496) | | |
| Post*Treat*Rainy | 0.0594 | | | |
| | (0.130) | | | |
| Post*Treat*Autumn | 0.153 | | | |
| | (0.114) | | | |
| Post*Treat*Winter | 0.328*** | | | |
| | (0.0776) | | | |
| Post*Treat*Hill | | 0.177** | | |
| | | (0.0770) | | |
| Post*Treat*Lowland | | 0.0499 | | |
| | | (0.0747) | | |
| Post*NP/CA | | | -0.206*** | -0.213*** |
| | | | (0.0751) | (0.0646) |
| Post*BZ | | | -0.139*** | -0.115** |
| | | | (0.0511) | (0.0566) |
| NP/CA*Post*Rainy | | | -0.00348 | |
| | | | (0.152) | |
| NP/CA*Post*Autumn | | | 0.145 | |
| | | | (0.149) | |
| NP/CA*Post*Winter | | | 0.439*** | |
| | | | (0.0789) | |
| BZ*Post*Rainy | | | 0.201 | |
| | | | (0.139) | |
| BZ*Post*Autumn | | | 0.154 | |
| | | | (0.112) | |
| BZ*Post*Winter | | | 0.308** | |
| | | | (0.120) | |
| NP/CA*Post*Hill | | | | 0.273** |
| | | | | (0.109) |
| BZ*Post*Hill | | | | 0.169** |
| | | | | (0.0704) |
| BZ*Post*Lowland | | | | 0.0101 |
| | | | | (0.0796) |
| Constant | 0.109** | 0.262*** | 0.114** | 0.261*** |
| | (0.0545) | (0.0302) | (0.0523) | (0.0293) |
| Observations | 1,509 | 1,719 | 1,509 | 1,719 |
| R-squared | 0.197 | 0.242 | 0.197 | 0.242 |
| Controls[b] | Y | Y | Y | Y |

[a] Triple DID regressions for heterogeneous treatment effects. Standard errors clustered at village level in parentheses. Statistical significance is denoted as *** p<0.01, ** p<0.05, * p<0.1. Regressions use control group A ("never treated") and follow Equation (3). Column1 and Column 2 are the results for the aggregated treatment group. Column 3 and Column 4 are the results for treatment groups with different protection intensities.
[b] Every regression includes the household, village, and geographic controls; columns (1) and (e) also include the season dummy variables as controls.

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