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Evidence on Wealth-Improving Effects of Forest Concessions in Liberia

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Abstract

The effects of resource-led development on local people's wellbeing are disputed. Using four rounds of Demographic and Health Survey data in Liberia, we find that households living closer to active forest concessions achieved a higher asset-based wealth score compared to those living farther away. These wealth-improving effects did not stem, however, from the direct employment effects of concessions. Rather, evidence suggests that indirect general equilibrium effects related to demand for goods and services and increased employment in allyear and non-subsistence jobs are the main channels. Our study underlines potential wealthimproving effects of resource-led development in poor countries, thereby contributing to the literature on wellbeing impacts of resource-led development on local people.

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JEL: *O13, O20, Q23, Q56, R20*

Keywords: *forestry concessions; wealth; impact evaluation; general equilibrium; Liberia*

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Evidence on Wealth-Improving Effects of Forest Concessions in Liberia

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The number of land concessions for natural resource extraction has increased significantly in the past decade (Borras Jr et al. 2011). Land investors with concessions have substantial incentives to use land intensively so as to profit from extractive products such as timber or minerals. Host countries can benefit from new investments that stimulate employment, enable knowledge and technology spillovers, increase exports and demand for products, and induce economic growth and improvements in local income (Collier and Dercon 2014; Deininger and Xia 2016). Indeed, international organizations such as the World Bank and the United Nations encourage natural resource development through capital inflows into the natural resource sector so as to promote economic development and reduce poverty (Asiedu and Lien 2011).

Many studies have investigated whether and how abundant natural resources and resource-led development can lead to structural transformation of African agrarian economies and to economic development, often using data on macroeconomic indicators at the national level (e.g., Sachs and Warner 1995; Sachs and Warner 2001; Isham et al. 2003; Sala-i-Martin and Subramanian 2013). They have found that successful development through natural resource extraction depends on such key factors as the quality of institutions that regulate and impact the process of economic development and distribution, management of commodity price volatility, e.g., indexed contracts, monetary policy on exchange rates, and prices of non-traded goods, e.g., input costs and wages, used to produce tradable goods (Sachs and Warner 2001; Mehlum et al. 2006; Frankel 2010).

The local welfare effects of natural resource development within a country and the channels through which such effects unfold, however, have only recently started to receive attention (Cust and Poelhekke 2015; Jung 2018). Within-country studies are especially useful because they enable identification of the channels through which wealth and wellbeing impacts from concessions are transmitted. Examples of such channels include government spending of revenues from concessions, infrastructure development, and local economic characteristics, e.g., labor market and prices of goods and services. Improved understanding of the role of these channels can help with policy design as it can reveal subnational heterogeneity in outcomes. Fortunately, increased availability of micro-level socioeconomic and geographic data enables examination of variations in outcomes at the level of households and the local economy (Van Der Ploeg and Poelhekke 2017).

In this paper, we investigate changes in local household wealth in Liberia, and the extent to which such changes can be attributed to natural resource concessions in Liberia's forest sector. We examine potential causal mechanisms that account for the effects of forestry concessions. We focus in particular on changes in the labor market because increased local employment in concessions is a mechanism for enhanced income, assets, and wellbeing. Our empirical strategy exploits heterogeneity in exposure to and timing of concessions and uses matching and event-study specifications with fixed effects estimation methods. We use four rounds of secondary household data (2007, 2009, 2011, and 2013) from the Demographic and Health Surveys (DHS). We focus on the effects of one type of forestry concession on wealth, i.e., private use permits (PUPs), because of their full implementation on the ground for a relatively short period of time and the availability of DHS data before and after the creation of PUPs. We measure the net impact of logging concessions using an asset-based wealth indicator, assuming proximity to concessions is the critical determinant of the average effects of concessions on households.

Our investigation of local wealth impacts of forestry concessions in Liberia fills an important gap and contributes to a growing literature on impacts of natural resource-led economic development, particularly in poor countries. Liberia is one of the poorest countries in the world with about 54% of its population below the national poverty line in 2014 (World Bank 2018). At the same time, the economy is heavily dependent upon the extraction of natural resources. Approximately 45% of its total land is governed through natural resource concession arrangements, including for minerals, oil, and forests (Balachandran et al. 2012; World Bank Group 2012). Investigating whether such investments can generate positive economic impacts and identifying the mechanisms that lead to such impacts can provide useful input to policy design for Liberia, but also for other poor countries that have resource-dependent economies and which rely on concessions for resource extraction.

Available empirical evidence of the welfare impacts of resource-led development in poor- and middle-income countries is mixed. For example, mining activities in Peru appear to have had positive impacts on consumption, poverty rates, and literacy rates at the district level (Loayza et al. 2013), and also on income at the household level through the government's procurement policy to buy local inputs (Aragón and Rud 2013). Other studies have found negative socioeconomic outcomes, such as increased inequality and conflicts and decreased productivity because of pollution (Aragón and Rud 2016; Kotsadam and Tolonen 2016). Studies of the impacts of forestry concessions are rare and usually use qualitative methods (McCarthy 2010; Lescuyer et al. 2012; Sikor 2012). One study (Ross 2001) shows how timber politics and rent-seizing politicians have driven appropriation of public resources and led to "natural resource curse" outcomes in Southeast Asian countries. Some quantitative studies have focused on the effects of certification efforts that impose stricter sustainability standards on concessions to evaluate their impacts on socioeconomic and environmental outcomes. The literature on certification has not arrived at a consensus regarding the effects of certified and non-certified concessions, which may well be context dependent. Evidence of both increases and decreases in deforestation exists, with some studies finding economic and health benefits associated with certified concessions (Blackman and Rivera 2011; Miteva et al. 2015; Brandt et al. 2018).

We find that households living closer to boundaries of implemented logging concessions experienced an increase in their asset-based wealth scores compared to those living farther from concession boundaries. These findings withstand multiple checks for effects of model specification and robustness. We find that people working in the manual labor sector that could have benefited from concessions did not achieve higher wealth scores. Rather, our analyses suggest increased economic activities and demands for local goods and services to be a major driver of the increased wealth. This finding is supported by our observation of structural changes in major occupational categories for households situated near concessions, such that working-aged men and women (15-49) near concession boundaries worked more in the agricultural and manual labor sectors compared to those living farther away. We also find that more skilled (educated) households achieved higher wealth scores, an indication of indirect impacts of concessions. Evidence from our analysis suggests that working-aged men and women in all sectors living near concession areas have more secure all-year and non-subsistence employment, which is a possible mechanism for encouraging consumption of goods and services around the concession area.

Our study advances the existing literature on resource-led development by examining the indirect impacts of resource-led development efforts, by focusing in particular on the wealth improving general equilibrium impacts of concessions. These effects are difficult to pinpoint in country-level macroeconomic changes because such indirect effects are most visible in areas close to logging, and likely become imperceptible in country-level analyses of average economic effects. Estimates of these outcomes, therefore, require an analytical approach and data that are spatially explicit, and where the causal analysis is undertaken at the subnational level. Similarly, the effects we identify are difficult to pinpoint through qualitative case studies that focus primarily on the logging sector and logging-related changes in employment, incomes, and welfare (Jung 2018). Kotsadam and Tolonen (2016), Aragon and Rud (2013), and Loayza et al. (2013) have found that mining operations can increase wealth through positive impacts on income, consumption, literacy, and changes in occupational structure. Our results highlight the significance of the indirect impacts and backward linkages (Hirschman 1958) through which concessions stimulate the local economy and improve wealth, even in the absence of direct benefits for concession employees, i.e., increased wealth from employment by concessions. Taken as a whole, these results constitute important evidence that can help structure the wealth-improving effects of resource-led development policies in resource-rich developing countries such as Liberia, where 45% of the land is under forestry, agriculture, and other natural resource concessions.

1. BACKGROUND

1.1. Context

Forest concessions have been an important form of forest governance in the 20th and 21st centuries, along with decentralized and community-based governance and market-incentive-based governance (Agrawal et al. 2008). In Liberia, one of the poorest countries in the world, the total area of forestry concessions is equivalent to 24% of the estimated forest area (10,073 km^2) (Balachandran et al. 2012; World Bank Group 2012). The history of land deals in Liberia goes back to the early 1800s when the American Colonization Society (ACS) obtained a significant amount of land in Liberia to relocate freed black slaves in the country (Beyan 1991). Liberia's civil war (1983 to 2003) was partly financed by extractive resources such as diamonds and timber, leading the United Nations to ban imports of Liberian diamonds and timber in 2001 and 2003, respectively. The ban on timber import was lifted in 2006 after Ellen Johnson-Sirleaf's election as the new president. One of her first acts in office in 2006 was to cancel all forest concessions signed by the former president Charles Taylor.

The government of Liberia passed the National Forest Reform Law in 2006. Under the new law, forests can be used under four types of contracts: Forest Management Contract (FMC), Timber Sales Contract (TSC), Community Forestry Management Agreement (CFMA), and Private Use Permit (PUP) (Table A1 in the Appendix). All the contract types except PUPs are lease agreements between private investors/groups/communities and the Government of Liberia (GoL) through its Forestry Development Authority (FDA). Owing to the lack of specific regulations for the sustainable management of PUP concessions, many PUP agreements were forged or misused and violated the National Forestry Reform Law, according to the report of the Special Independent Investigating Body (SIIB) established by the GoL. The explosion of PUPs followed by protests from civil society led the FDA to issue a moratorium on PUPs in 2012. Some PUP operations continued until shortly after 2012 (SIIB 2012), but the president's executive order No. 44 reconfirmed a moratorium on PUPs in January 2013. Currently, only FMCs and TSCs are in operation and all PUPs are regarded as illegal.

1.2. Identification of Impacts - Private Use Permits (PUPs)

We focus on the estimation of the impacts of PUPs on wealth because of a unique setting that supports our empirical identification strategy.[1] The implementation of PUP concessions can be described as a "temporary shock," which provides us with a useful natural experimental setting in which to analyze the impacts of this shock on local livelihoods. According to our Liberian in-country partner, Sustainable Development Institute (SDI), most PUPs had been fully implemented on the ground over a relatively large area during a short period of time between 2009 and 2013 while other concessions, such as FMCs and TSC, were implemented only partially. For example, only two out of seven FMCs were partially in operation in December 2012.[2] The concession area for each of 10 TSCs was 5,000 ha, whereas PUP areas ranged up to 80,000 ha with the average > 40,000 ha. These statistics suggest that the investments in TSCs and FMCs were smaller and their effects less detectable. Therefore, compared to FMCs and TSCs, the full implementation of PUPs makes their effects more detectable. Their short-lived nature supports their interpretation as a "temporary shock" because the livelihoods of people living in and around the concessions mostly depend on small-scale primary economic activities, including agricultural production.

By investigating the impacts of PUPs, we were able to exclude wealth impacts through the government's involvement and focus on direct and indirect wealth impacts from concessions. Governments can use instruments such as taxes, stumpage fees, and other revenues from concessionaires to improve public infrastructure in affected villages. However, the PUPs did not require a bidding process or land rental fees required for other types of concessions. Logging companies needed to present the consent of the original land title holders to obtain PUP concessions and use the land for logging operations. Companies located profitable logging sites on their own and negotiated with land title holders that could be individuals or communities. FDA did not have any specific standards or administrative procedures for PUP approvals, except for the broader national forestry reform law through which the government defined areas suitable for all types of forest concessions. Some requirements relevant to the sustainable use of forests included the presentation of business

[1] The following information in this section is based on the report by SIIB (SIIB 2012) and the information obtained from field visits in 2017. A field visit was done in January 2017 to understand the nature and impacts of forestry and other types of concessions by conducting meetings with different stakeholders including government officials in concessions and forestry related institutions and NGOs including Sustainable Development Institute (SDI) and focus group interviews with people from affected communities.

[2] In many cases, FMCs were not profitable for the companies. The large area contains not only commercially viable high-value timber area but also areas with low-value timber and the requirement of rotation makes it harder for the companies to make it profitable (SIIB 2012).

and land management plan along with environmental impact assessment and written social agreements between the land owner and the company defining benefits and access rights for local people. However, many PUPs were issued without the FDA carefully investigating relevant documents. Where documents contained social provisions, payments to communities were often in abeyance and the provisions of services such as schools or clinics did not occur (SIIB 2012). Documents submitted in support of the PUP applications were often forged and negotiations with communities occurred only sporadically. This also means that livelihood benefits might have been undermined because of the lack of negotiations with and provisions for local people living around the concessions. Therefore, we should expect few or no direct wealth impacts through such mechanisms as the provision of services by concession holders, or wealth transfers by the government to local communities.

Our estimates of impacts can be interpreted as measures of the net impacts of PUPs on wealth through other direct mechanisms, such as increased employment opportunities, or indirect mechanisms such as changes in economic conditions. We discuss the detailed channels in the following section.

1.3. Channels of Wealth Impacts

We expect labor market impacts of PUPs to be one of the main channels for effects on local people's wealth. Our theoretical framework for local economic impacts of labor supply and demand shocks draws on Rosen (1979) and Roback (1982; 1988), where the elasticities of labor supply and the supply of non-traded goods are determining factors for how new economic shocks affect local people. More recently, Moretti (2011) relaxed assumptions about the elasticity of local labor and housing supply to show that the welfare impact of local labor market shock depends on the relative magnitude of elasticities of local labor and housing supply. Our basic model builds on these models to illustrate how forestry concessions might affect the local labor market, in turn affecting the local economy and household well-being.

We define an economy that produces a vector of tradable and non-tradable goods using skilled and unskilled labor and fixed inputs such as land. For simplicity, we assume that households are not restricted in the amount of land that they can use for production.[3] Households can supply both skilled and unskilled labor as wage laborers or can produce agricultural or non-agricultural goods and services with a concave production function given the amount land for production. Their indirect utility function with usual properties, $V(y, P; \boldsymbol{Z})$,

[3] The arrival of PUP concessions might decrease the amount of land that households have access to, which will likely to negatively affect their production.

depends on income (*y*) and prices of a vector of tradable and non-tradable goods (*P*) with other household specific preferences (***Z***). The income includes wage income and income from own production of agricultural or non-agricultural goods and services. We assume that the supply of skilled labor is relatively inelastic owing to low labor mobility and that the supply of both traded and non-traded goods has a low elasticity. This is a plausible assumption given that most PUPs are located in remote areas, implying high transportation costs for the supply of goods and labor. The average distance from PUP operation areas to cities with populations over 8,625[4] in 2008 was around 50 kms (Table 1). In addition, roads around PUP areas are typically unpaved and become impassable during the rainy season. Remoteness and high transportation costs likely make the supply of goods and labor more inelastic, implying that agricultural products and other manufactured goods will have similar characteristics as non-tradable goods. (Aragón and Rud 2013). We also assume that formal rental markets do not exist. Our field observations indicate that the concept of charging rent for temporary housing is not common in rural areas. This means that the housing supply for outsiders depends on social relationships which are not difficult to create because local residents allow guests to stay in their homes without charging rent. The negative impacts of an increase in housing prices on local residents are, therefore, likely to be restricted.

The arrival of PUP concessions will first directly increase the demand for both skilled and unskilled manual labor for PUP operations. We can expect that manual labor forces directly employed by PUPs will likely gain more income, *y*, therefore increase in $V(y, P; \boldsymbol{Z})$ with all else constant. However, we expect this employment effect to be less substantial compared to other factors given that logging concessions often outsource skilled labor (Bacha and Rodriguez 2007) as confirmed in our field observations and focus group interviews.

Second, the PUP might indirectly affect local employment in the rest of the tradable and non-tradable sectors, and also have general equilibrium effects on local prices. We expect that increased economic activities and the numbers of people around PUPs increases the demand for agricultural and manufactured goods and services, which increases employment and income of labor forces, *y*, in all sectors. However, sectors that produce goods that people prefer and on which they spend a greater share of income will experience a larger effect. It is likely that skilled labor will benefit more because the elasticity of the skilled labor supply is likely to be lower than that of unskilled labor supply. The increase in income, *y*, might increase the households' indirect utility, $V(y, P; \boldsymbol{Z})$, depending on the changes in the prices of tradable and non-tradable goods, *P*.

[4] One standard deviation of the population size in all towns in Liberia from the Census data in 2008

Increases in demand for agricultural and manufactured goods might increase their prices, *P*, undermining the increased utility from the higher income. Increases in prices may be mitigated by increased production of those goods if the elasticity of labor supply is large and more people can easily be engaged in the production of them. However, it is also possible that this increase in the prices of goods and the lack of labor supply might be mitigated by PUP operations, because these investments can improve access to more remote communities. In this case, an increase in prices and income might be limited.

Given these interactions, we hypothesize that the overall wealth impacts from PUP operations largely depend on the relative magnitudes of direct and indirect employment effects, changes in the wage rates, and the general equilibrium effect on prices of agricultural and non-agricultural goods and services resulting from an increase in demand for local agricultural and manufactured goods. We expect to observe heterogeneous impacts among workers in different occupations and between skilled and unskilled workers, owing to the varying degree of direct and indirect employment and general equilibrium impacts by industry.

2. DATA

To evaluate the impacts of forestry concessions (PUPs) on wealth, we use publicly available data sets, including boundaries of logging and other types of concession and data on wealth outcomes and other socioeconomic and biophysical variables. Among the set of boundaries of PUP concessions that come from Global Forest Watch, we used 39 PUPs that match with documents from the government of Liberia (SIIB 2012). We use AidData (Bunte et al. 2018) and data from our in-country partner, SDI, to define and control for the distance to other types of concessions. Wealth outcomes, household characteristics, and biophysical variables mainly come from Liberia DHS (LDHS). Two of the LDHS were conducted in 2007 and 2013, and two rounds of its shorter version, the Liberia Malaria Indicator Survey (LMIS), were carried out in 2009 and 2011. LMIS focuses more directly on health indicators related to Malaria, but we use common variables available in all four data sets and present results using all datasets so as to increase the number of observations for matching estimations and to test for our identifying assumptions. However, we also use additional variables that are only available in LDHS 2007 and 2013 for robustness checks and to investigate heterogeneous impacts and the potential causal mechanisms through which PUPs generate wealth impacts.

The sampling strategy for LDHS 2007 is different from that for LDHS 2013, MIS 2009, and MIS 2011. The LDHS in 2007 uses a sampling framework based on the 1984 Population Census. The other datasets are based on the 2008 Population Census. This results in differences in the number of regions used for stratification of enumeration areas (EAs) and in the classification of urban and rural areas, explained in detail in the LDHS documentation on the DHS website[5]. Because of differences in the sampling strategy in LDHS 2007, we run our models with and without using LDHS 2007. The differences are insignificant for the inferences we draw and describe in this paper.

Since all PUP contracts were implemented between 2009 and 2012, we use the LDHS 2007 and LMIS 2009 data sets for the pre-concession period (baseline) data and the LMIS 2011 and LDHS 2013 data for the post-concession period. We note that the timing of the post-concession DHS 2013 data is after the moratorium on logging concessions and the President's executive order. However, we expect DHS 2013 data to reflect the impacts of PUP concessions given that PUP operations continued shortly after 2012. Our measures represent short-term impacts of concessions owing to the short duration of PUPs and the timing of our DHS data. Our unit of observation is household and we use geo-referenced location information for clusters of households (i.e., 20-30 households) in the LDHS and LMIS. Some of them do not represent exact locations because of spatial masking with perturbations of 2, 5, and 10 kms for confidentiality. The perturbations are restricted such that clusters remain within the district[6] to which each cluster originally belongs. Clusters in urban and rural areas are randomly displaced up to 2 kms and 5 kms, respectively, and randomly selected 1% of rural clusters are displaced by up to 10 kms. We use this location information to determine household distance from concession boundaries. We assume that location perturbations are randomly assigned to clusters, and account for the locational uncertainty through a selection of distance thresholds and sensitivity analyses of their impacts on results. Figure A1 in the Appendix shows the distribution of clusters in each of LDHS and LMIS data sets and locations of PUPs.

Our main outcome variable of interest is wealth scores from LDHS and LMIS data. The wealth score is readily available in the LDHS and LMIS data sets and is based on asset indices using principal components analysis (PCA). It considers assets such as radio, television, computer, and housing characteristics such as electricity connections, toilet system, and floor and roofing materials. All types of assets and quality of dwelling variables used for the construction of the wealth score are listed in Table A2 in the Appendix. The asset-based wealth

[5] LDHS 2013 sampling is similar to LMIS 2009 and LMIS 2011 sampling except that urban/rural classification is updated and six regions having been contracted to five regions by not having Monrovia as a separate region, which does not matter in our case since we do not have household observations located in Monrovia in our analyses.

[6] Second administrative level unit next to county, the first administrative unit. Liberia is composed of 5 regions with three counties in each region: North Western (Bomi, Grand Cape, Gbarpolu), South Central (Montserrado, Margibi, and Grand Basa), South Eastern A (River Cess, Sinoe, Grand Gedeh), South Eastern B (River Gee, Grand Kru, Maryland), and North Central (Bong, Nimba, Lofa).

score has been found to reasonably approximate income and consumption of households (Wagstaff and Watanabe 2003; Filmer and Scott 2012[7]). The wealth score can also be seen as a relatively more cost-effective and objective measure of wealth than other measures of material well-being such as income or consumption. Enumerators can observe and record possession of assets while indices for income and consumption can be affected by potentially biased interviewee reporting. We compare differences in wealth scores from 2007 and 2009 to 2011 and 2013 between control and treatment groups as specified in the following section.

We use the location of households to calculate the Euclidean distance from a cluster of households to the closest towns with a population size over 8,625. We also use the 2007 road network data from United Nations Missions in Liberia (UNMIL) to calculate the total length of roads within 5 km buffers of household clusters. Forest cover data in 2000 is used to calculate the average percentage of forest cover within 5 km buffers of household clusters (Hansen et al. 2013). The definition and descriptive statistics of all variables are presented in Table 1.

3. EMPIRICAL STRATEGY

We first pre-process the data using a matching method (Ho et al. 2007a) in order to reduce model dependence and control for observable characteristics affecting proximity to concessions and wealth. Then we use an event-study specification that generalizes difference-in-differences (DID) regression using matched observations with time and county-fixed effects to control for any-year events and county-level unobservables that can confound the impacts of concessions. Lastly, we use additional control variables and pseudo-panel estimation methods to control for other observables and unobservables at the defined cohort level and to check the sensitivity of results.

3.1. Selection on Observables

Because our analysis is based on non-experimental data, it is critical to control for factors that determine both the location of forestry concessions and household wealth. Our interest is to estimate the impacts of concessions on the wealth of those who are living in and near concessions (average treatment effect on the treated - ATT). Unbiased estimates of ATT require the following unconfounded or ignorability assumption:

$$(1) \quad \mathrm{E}\,[Y_{0it} |\, X_{it}, P_{it} = 1] = \mathrm{E}\,[Y_{0it} |\, X_{it}, P_{it} = 0]$$

[7] See Filmer and Scott (2012) for an introduction of studies that used asset indices for the measurement of household economic status. They analyze cases when asset indices can generate different ranking results compared to when using expenditure data. The rankings will likely to be more different under the cases of larger transitory shocks to expenditure, higher chance of random measurement errors, and a high proportion of individually consumed goods over total expenditures.

where Y_{0it} indicates the potential wealth outcome without the treatment for household i in time period t. This assumption means that the participation in the treatment is independent of potential wealth outcomes without participation, controlling for biophysical and household characteristics variables X_{it}. Conditional on X_{it} variables, P_{it} is assumed to be uncorrelated with households' initial wealth. We assume that households do not move into concession areas after controlling for all covariates, X_{it}. In other words, we assume that household and biophysical characteristics, X_{it}, as well as other county-specific characteristics before concession contracts have been made, are major determinants of households' relocation decisions to move into concession areas, and other factors do not affect their decisions to move into or near concession areas.

We control for the factors that might affect the selection of logging concession sites as well as households' wealth outcomes and their location decisions with respect to concession boundaries by using those factors as matching covariates or as control variables in ordinary least square (OLS) regressions. We find that the amount of forest area, density of infrastructure such as roads, and distance to a major city to be major factors in determining the location of logging concessions and households/towns and contribute to households' wealth (Laporte et al. 2007; Ferretti-Gallon and Busch 2014). Concessionaires would be interested in areas with dense forest cover because of their higher productivity, as well as places with good road infrastructure for transportation of logs. Likewise, the forest cover and road networks affect the mobility of households and access to forest resources that are assumed to affect wealth outcomes and their decision for migration.

We do not have any information on households' relocation decisions or relationships between concessions and households in the LDHS and LMIS surveys. However, we find, based on the Core Welfare Indicator Survey in 2010, that 77% of households were displaced because of the war since 1990, of which 92% have returned to their place of origin. Among households that have not returned to their place of origin, the reasons for not coming back to the place of origin vary, but economic reasons (e.g., no work opportunity, lack of funds to return) seem to be the main causes. Therefore, we also control for household characteristics, such as the number of household members who are under 5 years old and household head's age and sex, which have been found to affect households' economic outcomes (Bardhan and Udry 1999; Glewwe 2002; Fisher 2004). Household characteristics also affect household labor allocation decisions, which are also a major determinant of household migration decisions (Lucas 1997). Therefore, we use these observable biophysical and household characteristic variables to control for the selection of logging sites, households' decision on where to locate, and their wealth.

3.2. Selection on Unobservables

The main criticism of the matching methods and cross-section estimators is that they do not control for unobserved characteristics that can potentially affect both the location decisions of households (i.e., decision to move near concession areas) and the outcome of interest (i.e., wealth). We address this by matching observations within the same county to minimize the impact of unobserved characteristics specific to a county and by using county fixed effects in OLS regressions. Since these precautions cannot eliminate the possibility that unobserved characteristics of households affect outcomes, we first use the Rosenbaum test[8] to assess how sensitive our results are to unobservable characteristics. Secondly, we also use pseudo-panel estimation method to control for any other cohort-specific unobservables (see discussion in the *Additional Controls and Pseudo-Panel Approach* section 3.6).

3.3. Determinants of Impacts - Proximity to Concessions

We assume that proximity to concession areas is the major determinant of whether or how much a household is affected by logging concessions. Logging concession areas are mostly located in remote areas where households' mobility is restricted by high transportation costs. The roads are often not well connected to major cities, and walking is the most common mode of transportation in Liberia. Therefore, it is likely that households that live closer to concession areas have better access to economic opportunities and are affected by increased economic activities brought by the operation of logging concessions.

We define the treatment group as clusters of households that are within 5 kms[9] of the concession boundaries. The control group is defined as those clusters outside of the 5 kms buffer from concession boundaries but within 10 kms of the concession boundaries. We use rather conservative distance thresholds in determining affected households by using all observations within 10 kms from concession boundaries and those within 5 kms as the treatment group. There are several reasons that justify such an approach. First, the Household Income and Expenditure Survey (HIES) 2014 data indicates that 75% of rural Liberians commute less than one hour on foot (See the Figure A2 in the Appendix), which would be approximately 5 kms, assuming a person will walk about 5 kms or a little less in an hour. Second, we assume that the impacts of forestry concessions are much smaller or negligible for

[8] If we assume that two matched individuals with similar observed covariates are different only by the difference in unobserved factors in their odds of being affected by logging concessions, the Rosenbaum test measures how big the difference in unobserved factors should be to make the estimated ATT insignificant (Rosenbaum 2002).

[9] The distance that locations of household clusters in the DHS data are randomly masked, 2km, 5 km, or 10 km (1%), for confidentiality reasons.

households living more than 5 kms from concession boundaries. This is a plausible assumption given that PUPs were implemented for only a short time period, 1-3 years. This short duration of PUPs is likely to make the impacts less visible to the households located farther away from concession boundaries. Third, we would like to better capture general equilibrium impacts of concession operations, the major channel of effects as discussed in the section 1.3, by considering communities in the immediate vicinity of PUP concessions. Our underlying assumption is that the impacts of increased demand for non-tradable goods will be higher and more visible in communities in closer proximity, i.e., within 5 kms, from concession boundaries compared to those further away. This assumption is likely to hold because most labor hired by concession managers comes from outside and few of these outside staff travel far beyond the concession boundary. These factors lower the impacts of concessions on households located farther from concession boundaries. Fourth, we restrict the control samples to be within 10 kms from concession boundaries to use the control households that are as similar to the treatment households as possible. It is more likely for households that are farther away from concession boundaries to have systematically different observable and unobservable characteristics compared to those closer to concession boundaries (treatment group) in terms of their means of livelihoods and access to forests. We test the sensitivity of our main results by first changing the threshold to 10 kms as an upper limit of the distance that households are affected by PUP concessions and treating households outside 10 kms but within 20 kms as the control group. We also use continuous distance to PUPs to check the robustness of our results using the pseudo-panel approach.

We evaluate the possible impacts of other types of forestry, agricultural, and mining concessions by excluding observations that are within 5 kms or 10 kms from those of active concessions between 2007 and 2013 and by using the distance to other concessions as one of the control variables. We use information from SIIB on active forestry concessions (SIIB 2012); from our in-country partner, SDI, on active agricultural concessions; and from AidData (Bunte et al. 2018) on active mining concessions. As a result of this procedure, 21% (40%) and 34% (49%) of treatment and control group observations, respectively, have been dropped using 5 kms (10 kms) as the distance threshold that divides between control and treatment groups. However, we find that our main findings do not change even if we include these observations.

3.4. Impact Estimation – Matching on Observables

We first conduct a simple t-test of differences in the asset-based wealth score to compare the difference in average wealth outcomes between control and treatment groups before and after concession contracts, without conditioning on any covariates. Then, to condition on possibly confounding variables, we use Mahalanobis matching and event-study specification methods to explore potential causal relationships between forest concessions and wealth outcomes (Abadie 2005; Smith and Todd 2005; Stuart 2010).

The matching approach involves pairing each observation in the treatment group to similar observation(s) in the control group based on household and biophysical characteristics and comparing the value of the wealth score of the treatment and control households. Our identification assumption is that the wealth score of matched households in the control group is an estimate of the wealth score of households in the treatment group had they not been in or near concessions after controlling for household and biophysical characteristics. We use seven observable household characteristics and biophysical variables that were discussed above for matching: household head's sex and age, the number of household members under five, distance to roads and town, distance to other types of active concessions, and forest density.

We first use Mahalanobis distance matching, which performs well when there are fewer (e.g., less than eight) variables and covariates are normally distributed (Rubin 1979; Gu and Rosenbaum 1993; Stuart 2010). Since we match by more than one continuous variable, we correct for the bias that remains after matching by estimating and adjusting the differences in matched control and treatment households for the differences in covariates when calculating potential outcomes (Abadie et al. 2004; Abadie and Imbens 2012). We estimate heteroskedastic-robust asymptotic variance (Abadie et al. 2004; Abadie and Imbens 2006), which relaxes the constant variance assumption conditional on treatment and covariates X_{it}. We also use the caliper method after matching to limit the maximum distance between matched pairs and improve the balance of covariates between control and treatment groups (Cochran and Rubin 1973). We exclude 25%[10] of "bad" matches with the highest covariate distance between control and treatment groups from the pool of observations in the treatment group. By doing so, we increase the covariate balance between control and treatment group observations in order to satisfy our identification assumption. We acknowledge that this has implications for the interpretation of our results because it pertains only to those households that remain after the exclusion of bad matches, and households like them. Therefore, we present results from matching both with and without calipers for our overall impact assessments to test whether the results are consistent.

[10] We also used one standard deviation (S.D) of distance from the mean distance as a maximum distance threshold and find that further trimming by excluding 25% of bad matches achieves better balance in terms of differences in means and improvements in the variance ratio than using one S.D from the mean as the threshold.

We calculate normalized differences in means as well as ratio of variances for all covariates between treatment and matched control group households to check the balance among covariates. We calculate the normalized differences in means by dividing the difference in means between treatment and control groups by the square root of the sum of treatment and control groups' variances (Stuart 2010). We also draw quantile-quantile (QQ) plots for each continuous covariate variable to visualize distributions.

3.5. Event-Study Specification

After matching, we use the event-study framework that generalizes the DID estimation method to allow the wealth impacts of PUPs to vary by the number of years before and after PUP implementation and estimate the changes in wealth relative to the baseline period (Jacobson et al. 1993; Bailey and Goodman-Bacon 2015).

We estimate the following regression:

$$(2)\quad Y_{it} = \theta_{ij} + D_{it} + G_{it} + \sum_{k=2009}^{2013} \tau_t D_t(t = k)G_{it} + \delta \boldsymbol{X_{it}} + \varepsilon_i$$

where Y_{it} is the wealth score of a household *i* at time period *t*; θ_{ij} is a county dummy variable that a household *i* belongs to county *j*; D_{it} is a time indicator variable, which is 1 if $t = k \in \{2009, 2011, 2013\}$ and 0 otherwise for a household *I* (*t*=2007 omitted); G_{it} is a treatment dummy variable equal to 1 if the location of the cluster in which a household is included is within 5 kms or 10 kms of the concession area and 0 otherwise in time period *t*; $\boldsymbol{X_{it}}$ is a vector of other covariates; ε_i is an error term that is assumed to be independent of both G and D. The county dummy variable, θ_{ij}, controls for any fixed county-specific differences in wealth for households. The time-indicator variable, D_{it}, controls for any time-specific events that affect wealth scores in year *t*.

We run the estimation first using all observations and second using only observations that have been matched, because this makes the outcomes less dependent on parametric specifications of the model and reduces bias from model misspecification or from potentially omitted variables (Ho et al. 2007b; Blackman et al. 2015). This estimation method enables us to control for county- and time-specific unobservables as well as time-varying covariates and investigate the causal link between concessions and wealth score. Our variable of interest τ_t when $G_{it} = 1$ allows us to test the impacts of concessions over time relative to the baseline year 2007. Those of τ_t when $G_{it} = 1$ measures the difference in wealth score for households that live within 5 kms or 10 kms of concession area relative to the wealth score for households living farther away than 5 kms or 10 kms but within 10 kms or 20 kms, respectively, of concession area in a given year *t* relative to that of the base year 2007.

3.6. Additional Controls and Pseudo-Panel Approach

We use additional control variables and a pseudo-panel approach to check the robustness of our estimation results. Our control or conditioning variables for matching and event-study estimations above are based on commonly available variables in the four rounds of 2007, 2009, 2011, and 2013 DHS data sets. Some socioeconomic variables that might be important in determining households' relocation or wealth are only available for certain years. We use two additional household production- and cost-related variables that are directly linked to wealth, ownership of livestock and bank account, to test if the results show consistent patterns. We use two comprehensive versions of DHS 2007 and 2013 that have both variables and apply the same event-study estimation method after pre-processing of data using Mahalanobis with a caliper matching method.

Despite care in the identification strategies we use, it is possible that individual households' wealth scores are correlated with household-level shocks and unobservables. This concern can be addressed by having household fixed effects using panel data. However, because our data is a repeated cross-section, we adopt a pseudo-panel approach with cohorts to estimate fixed effects models (Deaton 1985). The pseudo-panel approach can be as good as or at times more advantageous than panel data with nonrandom attrition and panel conditioning (Deaton 1985; Zwane et al. 2011). In order to get consistent estimates from the pseudo-panel approach, grouping variables need to be exogenous, time-invariant, and available for all household in the data (Verbeek 2008). We use household head's sex, birth year, and the region as grouping variables, with birth year divided by quartile, variables commonly used in the literature (e.g., Bernard et al. 2011; Pless and Fell 2017). Owing to the variation in cohort size between years, which leads to heteroscedasticity, we weigh the observations using the inverse of the square root of cohort size (Dargay 2007; Pless and Fell 2017).

Specifically, we aggregate observations into cohorts and estimate the following model.

$$(3) \quad \bar{Y}_{ct} = \theta_c + D_{ct} + \sum_{k=2009}^{2013} \beta \overline{dist}_c D_t(t = k) + \delta \overline{\mathrm{X}}_{\mathrm{ct}} + \varepsilon_i$$

where θ_c is the cohort-level fixed effect that controls for time-invariant cohort level unobservables and D_{ct} represents time-fixed effects at the cohort level; $\bar{Y}_{ct}$ is the average wealth score value of all Y_{it}'s within cohort *c* in period *t*; $\overline{dist}_c$ is the average distance of household locations to the nearest PUP concession boundaries, where $dist_c \leq 10\ km$, within cohort *c* interacted with the time dummy variable; other control variables also have been averaged at the cohort level $\overline{\mathrm{X}}_{\mathrm{ct}}$ in each time period t. We test the robustness of the results by estimating the above equation with added interaction terms between the average distance to other concessions and year dummies, assuming that the impacts of other concessions might vary by year.

3.7. Heterogeneous Impacts and Mechanisms

While the unit of the previous analyses is the household, we use the DHS individual men and women's survey modules that are available only in 2007 and 2013 to investigate how welfare impacts vary by occupation and education level. Heterogeneous impacts by occupation provide us an overview of how PUPs have affected different sectors, which we divide into three major occupation categories: agriculture, sales, and manual labor. We also test whether wealth impacts of PUPs have been more pronounced for skilled labor or for unskilled labor by using the education level of individuals as a proxy for skilled and unskilled labor forces. We divide the observations into three levels of education: no education, some education, and above median years of education (3 years).

To explore potential mechanisms that drive our previous results, we test changes in the occupational structure, employer type, and employment status by using occupation categories in the individual men and women's survey. The occupation categories of sales, agriculture, and manual labor represent more than 85% of employed men and women in our matched households. We first estimate the impacts of PUPs on the changes in the probability that certain types of jobs and payment types might appear in villages closer to the concession boundaries in order to estimate potential causal mechanisms that PUPs have affected wealth. Then we estimate the impacts of PUPs on the probability of being employed by family or other employers and of being employed all year and seasonally or occasionally to investigate potential causal mechanisms. We use the same variable and models that are used in estimating heterogeneous impacts.

We include in our analyses only men and women with similar household characteristics, i.e., members of the matched household from the previous analyses. We exclude about 2% of observations that are not usual residents of villages in order not to confound our estimation results by temporary migrants within villages.

4. RESULTS

4.1. Overall Impacts

The simple t-test of differences in means of wealth scores of control (non-impacted) vs. treatment (impacted) groups is consistent with the argument that PUP concessions have not decreased the wealth status of households in the treatment group that lives within 5 kms or 10 kms of concession boundaries compared to the control group households that live farther away than 5 kms and 10 kms, but within 10 kms and 20 kms of concession boundaries, respectively (Table 2). In the baseline period, the wealth score of the treatment group is lower by 0.04 and 0.09 than that of the control group, respectively for the 5 kms and 10 kms thresholds. In the post-concession period, the wealth score of the treatment group is higher by the same amount using 5 kms as a threshold and lower by 0.06 using 10 kms as a threshold, compared to the wealth score of the control group. A comparison of the wealth score of treatment and control households between baseline and the post-concession period shows that the average wealth of both control and treatment groups increased significantly ($p<0.01$) by 0.08-0.16 from baseline to the post-concession period. These comparisons of wealth score from baseline to the post-concession period support the argument that concessions have contributed to increases of household wealth.

After matching with a caliper, we dropped 319 and 382 observations that correspond to 25% bad matches from the treatment group in the baseline and post-concession periods, respectively[11]. The standardized differences between matched control and treatment observations for most of the variables were lower after matching with a caliper (Table A3 in the Appendix), indicating improved overall covariate balance. This is true for both the baseline and post-concession periods. The variance ratios also show improvements in household's sex, age, distance to town, and distance to other concessions (Table A3 and Figure A3 in the Appendix).

The impact estimation of PUPs using DID after the Mahalanobis distance with or without a caliper matching estimator consistently shows overall positive impacts of PUPs on the wealth score (Table 2). Using different thresholds between treatment and control groups does not change the pattern of PUPs' positive impacts. When comparing each household in the treatment group to a household in the control group with similar biophysical and household characteristics, the DID results show no significant differences at 5% level of significance

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[11] Our consistent results using matching without and with a caliper (Table 4) mitigate concerns about our results being driven by the remaining observations that may not be representative. We dropped observations from the treatment group that have a lower number of household members who are under five and younger household heads, higher road density, lower forest density, closer proximity to towns and other types of concessions in order to increase the covariate balance between control and treatment groups and satisfy our identification assumptions.

(without a caliper) or wealth-improving impacts by 0.09-0.10 (with a caliper) using 5 kms or 10 kms as a threshold dividing treatment and control groups (Table 2).

The event-study estimates controlling for time- and county-specific effects with Mahalanobis matching and a caliper (Figure 1) show that the treatment group had a significantly higher wealth score by 0.22 in 2013. The effect size of the estimated value of 0.22 using the 5 kms distance threshold with matching with a caliper is 0.44.[12] This means that the wealth score of the average household in the treatment group is 0.44 standard deviations above the average household in the matched control group after controlling for all the covariates used in the regression. The significant wealth impact of PUPs in 2013 but not in 2011 may reflect the full operation of PUP concessions during 2011-2012 before the president's 2013 executive order No. 44 declaring a moratorium on PUPs. The insignificant treatment impact in the year 2009 provides evidence in favor of similar pre-treatment trends in wealth score between control and treatment groups, validating the estimation results for 2011 and 2013. Our analyses show consistent patterns with and without pre-processing of data using Mahalanobis matching with or without a caliper (Table A4 in the Appendix) in which the treatment group has a significantly higher wealth score compared to that for the control group by 0.16-0.22 as a result of PUPs in 2013, but not in 2011.

The Rosenbaum test value of 1.1-1.2 for matching estimators using 5 kms or 10 kms threshold (Table 2) implies that matched households with the same observed covariates would have to differ in terms of unobserved covariates by a factor of 1.1-1.2 (10-20%) in order to invalidate the inference of the lower wealth score of households located within, or within 5 kms or 10 kms of, concession boundaries. The low Rosenbaum test values do not mean that unobservable characteristics which may confound the results are necessarily present, but it is a useful proxy to test how likely it is that unobservables might invalidate the results from matching estimation. The relatively low value of Rosenbaum test in the pre-concession period indicates that unobservables are more likely to invalidate the significant differences in wealth score between control and treatment groups. The relatively high value of Rosenbaum test in post-concession period (matching with a caliper) suggests robustness in our results to unobserved covariates in the post-concession period where the treatment group has higher wealth scores.

The estimates from the event-study specification (Table A4 in the Appendix) also show how household and biophysical characteristics are associated with the wealth score in Liberia. Households that have a male household head and fewer children under 5 have a higher wealth

[12] The effect size is calculated by dividing the coefficient estimate by the standard deviation of the event specification model's error term.

score, consistent with the expectation that such households have increased the chance of higher household income. Biophysical characteristics indicate that households that have access to more roads and that are located in less forest-dense areas and closer to towns but further away from other types of concessions have higher wealth scores. Having access to infrastructure that is likely to improve the wealth of households can be represented as having lower forest density and/or higher road density.

4.2. Robustness Checks

The results presented above suggest that local people living closer to PUP boundaries have gained greater wealth as compared to people living farther away from PUP boundaries. In this section, we analyze if our results are robust to potential sources of bias.

The coefficients for the interactions of treatment and the year dummy in 2013 have the same signs and similar magnitudes as those without the two additional control variables (0.19 and 0.16 using 5 and 10 kms thresholds, respectively, in Table A4 in the Appendix). The household characteristics and biophysical variables also show consistent patterns with the ownership of a bank account being significantly associated with higher wealth score.

Given the cross-sectional nature of our data, our estimation results can still suffer from household-specific unobservables that might drive results showing positive impacts of PUPs. The results of our pseudo-panel approach, grouping households by household head's sex, birth year, and region, and using cohort fixed effects, indicates consistent patterns with matching and event-study estimation results (Table 3). Specifically, we find that the wealth score decreases by 0.04 for every 1 km increase in the distance of a household from PUPs. Households that have more access to roads have lower wealth scores. Another potential source of bias is the perturbation of the household locations by 2 and 5 kms with up to 10 kms for 1% of observations. Our consistent results from using different thresholds of 5 kms and up to the maximum distance of 10 kms, which is the maximum distance of the perturbation, in delineating treatment and control groups reduces concerns about positional uncertainty confounding the results.

4.3. Potential Mechanisms Leading to Heterogeneous Impacts

Using data on individual men and women for 2007 and 2013 of the matched households, we find some evidence regarding the heterogeneity in wealth gain by occupation and education levels (Table 4). Our estimates do not show distinct wealth gain patterns for most of the job categories, except for people in the sales sector at a 10% level of significance. The insignificant impacts of PUPs on people in the manual labor sector likely reflect limited direct employment effects of PUPs on manual labor, as suggested by empirical evidence that logging concessions outsource much of their skilled labor needs and tend to employ limited numbers of local manual labor for low and medium level positions (Bacha and Rodriguez 2007). Although statistically significant at $p<0.1$, we find that people in the sales sector who are living within 5 kms from PUPs may obtain a greater wealth score (by 0.35) compared to those outside 5 kms but within 10 kms from PUPs.

Our tests of changes in the occupational structure help us understand the heterogeneity in wealth gain for different occupation categories (Table 5). We find that the probability of a woman or man at working age to be in the agricultural or the wage labor sector increased by 5% and skilled manual laborers increased by 2%. Other occupation categories do not show any significant changes. This indicates that the number of people working in the agricultural and skilled manual labor sectors within 5 kms from PUPs has increased significantly more than that outside of 5 kms but within 10 kms from PUPs. This might explain why the wealth score for people in the agricultural sector or manual labor sector has not changed significantly (Table 4), despite the potential increase in the demand for agricultural and other goods and services. The increase in the agricultural and skilled manual labor forces might have reduced the wage for those employed in those sectors and the prices of their products might not have increased owing to the increased production. On the other hand, we do not find significant changes in employment in the sales sector despite the expected increase in the demand for goods and services. This might also explain why we find evidence for a significant increase in wealth score of people in the sales sectors and living within 5 kms from PUPs (Table 4).

We also find that people with some education or above median years of education and living within 5 kms from PUPs gained greater wealth than people with the same level of education but living outside of 5 kms but within 10 kms from PUPs, while there were no significant differences for people with no education (Table 4). If we assume that education level distinguishes skilled from unskilled labor, this result indicates that the net positive effect of increased employment and local prices resulting from changes in labor and non-tradable goods demand for skilled labor might have been higher than that for unskilled labor.

We also observe changes in the employer type in our treatment villages. The number of people working for others increased by 4%. All-year employment increased by 25%, with a decrease of seasonal or occasional employment by 21% in the villages within 5 kms from PUPs compared to the villages farther away from PUPs (Table 5). These increases in the non-subsistence employment and all-year employment might have enabled household members to increase consumption of tradable and non-tradable goods by securing their employment, which might have induced the increase in wealth scores for households living in those villages due to the increased economic activities and wealth in the area.

4.4. Alternative Mechanism

The above results show evidence of increased asset-based wealth score for households that are affected more by the PUP concessions – the channels for the increase in wealth scores turn out to be increased economic activities and employment. It is possible that these results are capturing households that migrate from areas within 10 kms of the concession boundaries to areas within 5 kms of concession boundaries. We checked against this possibility by excluding 2% of observations that were not usual residents of included villages. This step does not preclude that other migrant households may still affect observed changes in wealth.

Since DHS data does not contain information on migration status of respondents, we address this concern indirectly by testing whether PUP concessions have changed observable characteristics of working age men and women living within 5 km of concessions compared to those that are farther away. We use age, sex, education, and religion (Christian and Muslim) of working age men and women as indicators and estimate the same DID regression.

We do not find any significant differences in changes in sex, education, and religion of working age men and women between control and treatment groups at $p<0.05$ (Table 6). The sex of an individual is different at 10% level of significance, indicating more women in the treatment area. Although this might raise a concern on selective migration into the treatment area, we find this less of a concern since it is usually men who migrate into a newly developed area (Ratha et al. 2011). Further, the same test using information on household heads available for all four rounds of DHS shows that there is no significant change in sex. This mitigates concerns about in-migration driving our results.

5. DISCUSSION AND CONCLUSION

Natural resource concessions are widespread as a means to drive forestry and other natural resources development, such as mining. They leverage external investments, and have the potential to contribute to the economic development of economically poor but resource rich countries in the Global South. Our analysis of the impacts of a specific form of timber concessions – PUPs - show that there is no statistically significant ($p<0.05$) evidence of negative impact of forestry concessions on the asset-based wealth score in Liberia. Our results are different from studies that find negative effects of concessions on wealth of local people

(Lanier et al. 2012; Richards 2013; Shete and Rutten 2015). Rather, our results, in some contrast to a raft of case-based literature on extractive concessions, provide evidence that PUP concessions had a positive effect on household asset-based wealth scores in Liberia. These results are consistent with several other studies using quantitative methods that find some positive outcomes of concessions by providing increased economic opportunities to local people (Loayza et al. 2013; Aragón and Rud 2013; Baumgartner et al. 2015; Kotsadam and Tolonen 2016).

We find that an indirect increase in economic opportunities, resulting from logging activities through PUPs, might have played a critical role in increasing households' wealth. Most PUP concessions had been fully implemented on the ground before they became illegal. The full implementation of PUPs is likely to have boosted increased economic activities, and demands for local goods and services by logging workers as well as by local people. The effect size for the households within 5 km from concession boundaries compared to those outside of 5 km but within 10 km from concession boundaries is estimated to be 0.44. This is equivalent to approximately over 60 percent of the control group being below the average person in the treatment group.

We find higher employment in the agricultural and manual labor sector. The increase in wages for employees in those sectors and prices of their products might have been limited owing to the higher supply of labor and increased production. This might explain why we do not find differential wealth increases for people in the agricultural and manual labor sector. We also find that people with any education or those with above-median years of education gained higher wealth scores compared to people with no education. Further, more people have been employed in all year in non-subsistence jobs during post-concession periods compared to pre-concession periods in the villages closer to PUPs. This potentially indicates more secure employment, likely encouraging an increase in the consumption of goods and services in the area. Overall, our results suggest that, on average, the positive impacts of increased economic opportunities through increased demand and flow of goods and services outweigh the effects of reduced access to natural resources owing to tenure change, at least in the short term.

Our analysis of PUPs enables us to isolate the net wealth impacts of this form of logging concessions in Liberia by excluding other mechanisms such as the increased provision of services by the government or by concession holders. The estimated impacts should nonetheless be interpreted with some caution as there may be an upper bound on what can be achieved through an instrument such as the PUPs. It is probable that PUPs are located in well-suited places that are likely to result in higher investment returns for logging companies when compared to less-suited places. Also, logging companies could have overinvested for concession operations considering the short duration of the PUP concessions and the contracted terms of between 11 and 30 years. Concession owners may not have been able to anticipate the presidential moratorium. Also, most concessions were owned and/or operated by larger companies which have generally higher fixed costs and skill levels needed for creating the infrastructure for operating the logging concession.

The survey data are well aligned in time to provide observations of the wealth score before and after concession dates. Concerns about possible effects of the perturbation of cluster locations by 2 kms and up to 10 kms for households is mitigated by the fact that our results are consistent and significant across different specifications using different thresholds. This suggests that the main results are unlikely to change had we used the exact location of households. Further, we expect to observe stronger positive impacts with higher t-values if we had exact locations because the estimated coefficient and the t-statistics could have been biased downward due to the measurement error in the distance-to-concessions variable. Although the matching with and without caliper estimates have relatively low Rosenbaum values of 1.1-1.2, our results from robustness checks including the pseudo-panel approach and additional controls show consistent patterns, reducing concerns about effects of unobservable characteristics of households.

The generalizability of our findings about the impacts of PUPs may raise concerns because PUPs are a special case of concessions and have different characteristics compared to other concessions. We suggest that this specific feature of PUPs, in fact, provides a stronger test of the short-term effects of concessions in resource-rich, poor contexts. Our findings about the lack of direct positive employment impacts on wealth, despite intensive extraction of natural resources within the short period of time, and about the importance of indirect economic opportunities arising from concessions helps bring together the results highlighted in other studies of concessions. Qualitative case studies tend to focus on the direct impacts of concessions and identify these to be minimal or negative. More quantitative studies, on the other hand, have often highlighted positive impacts of concessions. Our analysis suggests that on the average concessions can have positive effects, even as the channel for these effects is likely to be the indirect economic opportunities created by concessions. These results also underscore the need for institutional mechanisms that governments can use if concessions are to support higher employment of local labor or improved provision of services.

Economic development through natural resource concessions can be crucial in countries such as Liberia which have high poverty and are resource rich. Liberia itself has approximately

45% of its land under natural resource or agricultural concessions and over 56% of people identified as poor (Balachandran et al. 2012). Additional research is necessary to examine how forestry concessions generate wealth impacts through other channels such as infrastructure development, tenure change, and environmental degradation, all known to affect wealth (Jacoby 2000; Grieg-Gran et al. 2005; Bacha and Rodriguez 2007; Jacoby and Minten 2009; Rist et al. 2012). It is also possible that in the long run, positive livelihood impacts of concessions will be offset by negative environmental impacts caused by degradation of the environment by logging activities. The asset-based wealth score we use in this study only captures one of the many dimensions of households' wellbeing. Further research on pathways of and impacts through logging concessions will help increase confidence in how our findings relate to findings of more qualitative studies that focus on specific direct channels contributing to change in poverty and wealth. Further research is also needed to verify the long-term impacts of concessions.

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Table 1. Variable Descriptions, Means, and Standard Deviations (S.D.)

Variables	Description	Mean (S.D.) Total	Control group	Treatment group
		N=2,508	N=1,401	N=1,107
Wealth score	Composite asset index, 1 being the lowest to 4 being the highest	-0.54 (0.56)	-0.49 (0.65)	-0.52 (0.61)
Household characteristics				
Under5	The number of household members who are under 5 years old (no.)	1.13 (1.17)	1.13 (1.15)	1.13 (1.16)
Hheadsex	=1 if the head of the household is male and =0 otherwise	0.72 (0.45)	0.67 (0.47)	0.70 (0.46)
Hheadage	The age of household head (years)	43.24 (15.23)	43.91 (14.73)	43.53 (15.01)
Livestock	=1 if the household owns any livestock and =0 otherwise	0.52 (0.50)	0.52 (0.50)	0.52 (0.50)
Bank	=1 if the household owns a bank account and =0 otherwise	0.06 (0.24)	0.05 (0.22)	0.06 (0.23)
Biophysical characteristics				
Road	The length of roads within 5 kms buffer from where a household is located (km)	19.54 (10.53)	16.19 (9.20)	18.07 (10.10)
Forest	Average percentage of forest cover in 2000 within 5 kms buffer from where a household is located (percent)	69.51 (6.29)	71.72 (5.39)	70.48 (6.01)
Town	Distance from a cluster of households to the closest towns over population of 8,625 in 2008 (km)	47.15 (21.22)	46.88 (19.29)	47.03 (20.39)
Othrconcess	Distance from a cluster of households to the closest other concessions (other forestry, agriculture, mineral, and mining) (km)	27.19 (17.58)	26.91 (14.43)	27.07 (16.26)

Note. The values have been calculated using DHS 2013 data with treatment group defined as households within and within 5 kms of the concession boundaries and control group as those outside of the 5 kms buffer from concession boundaries but within 10 kms from the concession boundaries.

Table 2. The Differences in Wealth Score Between Control and Treatment Groups

	t-test			Matching estimator			
	Average (PUP)			Mahalanobis – w/o a caliper		Mahalanobis – w/ a caliper	
Year	Control	Treat	Difference	Difference	Rosenbaum test (γ)	Difference	Rosenbaum test (γ)
Baseline (2007,2009)							
Within 5 kms	-0.46	-0.68	-0.22^{***} (0.02)	0.05^{**} (0.02)	1.1	0.02 (0.03)	
Within 10 kms	-0.50	-0.59	-0.09^{***} (0.02)	0.02 (0.03)		0.01 (0.05)	
Post-Concession (2011,2013)							
Within 5 kms	-0.38	-0.55	-0.17^{***} (0.02)	0.05 (0.03)		0.09^{***} (0.03)	1.1
Within 10 kms	-0.41	-0.46	-0.06^{**} (0.02)	0.07^{**} (0.03)	1.1	0.09^{***} (0.03)	1.2
Differences in average							
Within 5 kms	0.08^{***} (0.02)	0.13^{***} (0.02)		0.01 (0.03)		0.10^{***} (0.03)	
Within 10 kms	0.09^{***} (0.02)	0.13^{***} (0.02)		0.06^{*} (0.03)		0.09^{***} (0.03)	

Note. The average differences in wealth score are calculated using the t-test and Mahalanobis nearest neighbor matching with and without a caliper (excluding 25% of observations with the highest distance). * p<.1. ** p<.05. *** p<.01

Table 3. The Impacts of Private Use Permits on Wealth Score Using Pseudo-panel Method – Robustness Check

Dependent variable:	Wealth score	
Treatment impact by 1km distance away from concessions		
2009	-0.00 (0.01)	-0.01 (0.02)
2011	-0.00 (0.02)	-0.00 (0.02)
2013	-0.04*** (0.02)	-0.04*** (0.02)
Household and biophysical characteristics		
Under5	0.11* (0.06)	0.12* (0.062)
Road	0.02*** (0.01)	0.02*** (0.01)
Forest	-0.00 (0.01)	-0.01 (0.01)
Town (10 kms)	-0.01 (0.00)	-0.00 (0.00)
Distance to the nearest PUP	0.01 (0.01)	0.02 (0.01)
Othrconcess	-0.00 (0.01)	-0.01 (0.01)
Cohort fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Distance to other concessions × Year	No	Yes
R^2	0.54	0.56
N	160	160

Note. Pseudo-panel estimation results using household head sex, birth year, and region as cohorts. * p<.1. ** p<.05. *** p<.01.

Table 4. Heterogeneous Impacts of Private Use Permits on Wealth Score by Occupation

Dependent variable: wealth score						
	Occupation			Education level		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Agriculture	Sales	Manual	No education	Primary education.	Secondary education
Treatment impact						
2013	0.04	0.35*	0.45	0.03	0.21**	0.22**
	(0.08)	(0.19)	(0.39)	(0.11)	(0.08)	(0.10)
R^2	0.28	0.49	0.44	0.35	0.35	0.36
N	1762	298	185	1166	1884	1314

Note. Heterogeneous impact estimation results by occupation and education level using difference-in-difference estimation methods and 5 kms threshold that divides control and treatment groups. The full set of control variables includes: Age, Sex, Education (years), Christian, Muslim, No. of household members, women, living children in the household, Livestock and Bank ownership, Road (km) and Forest (percent) density, distance to Town (km). County and forestry concession-fixed effects are not shown in the table. Clustered standard errors (S.E) at the concession level. * p<.1. ** p<.05. *** p<.01.

Table 5. Potential Mechanisms: Changes in Occupational Structure, Employment, and Payment Type

	Occupation						Employer type		Employment seasonality	
	Ag - self employed	Ag - employed	Sales	Manual - skilled	Manual-unskilled	Unemployed	Work for family	Work for someone else	All year	Seasonal or occasional
Treatment impact										
2013	-0.07	0.05***	-0.06	0.02***	0.02	-0.00	-0.15*	0.04**	0.25***	-0.21***
	(0.07)	(0.02)	(0.05)	(0.01)	(0.05)	(0.11)	(0.09)	(0.02)	(0.07)	(0.07)
N	2929	2330	2737	2808	2583	2778	1185	877	2368	3050

Note. The probability of changes in the occupational structure, employer type, and employment seasonality using probit difference-in-difference estimation methods and 5 kms threshold that divides control and treatment groups. The same set of control variables in Table 4 has been used. The treatment impact has been calculated following Puhani (2012). County and forestry concession-fixed effects are not shown in the table. Clustered standard errors (S.E) at the concession level. * p<.1. ** p<.05. *** p<.01.

Table 6. Changes in Demographic Characteristics

	Individual data (2007, 2013)					Household head data (2007, 2009, 2011, 2013)		
	Age	Sex	Education	Christian	Muslim	Age	Sex	Education
Treatment impact								
2013	-0.15	-0.06*	-0.30	0.03	-0.03	1.23	-0.02	-0.15
	(0.87)	(0.03)	(0.42)	(0.05)	(0.03)	(0.86)	(0.05)	(0.17)
R^2	0.03		0.07					0.07
N	3140	3140	3140	3102	2364	2967	2967	2330

Note. The difference in changes in age and education has been calculated using difference-in-differences estimation methods and the probability of changes in sex, Christian, and Muslim using probit estimation methods, where control variables include biophysical characteristics. The treatment impact has been calculated following Puhani (2012) except for the Age and Education variables that are not binary. The models used the 5 kms threshold that divides control and treatment groups. County and forestry concession-fixed effects are not shown in the table. Clustered standard errors (S.E) at the concession level. * p<.1. ** p<.05. *** p<.01.

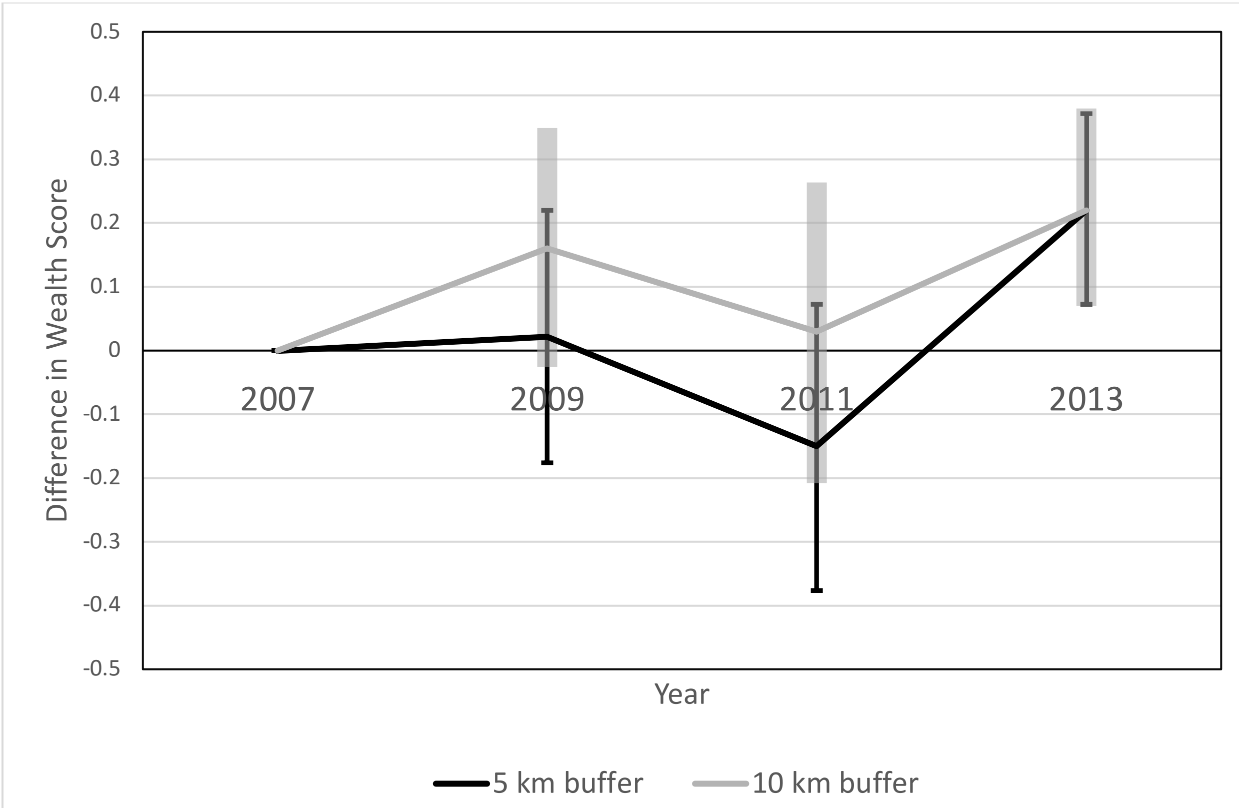


Figure 1. The difference in wealth score between control and treatment groups and confidence intervals ($p<0.05$) from 2007 to 2013 after controlling for household and biophysical characteristics, year and county specific effects using 5 kms buffer (black) and 10 kms buffer (grey) from matching (with a caliper) and event-study specification estimation results

Appendix

Table A1. Characteristics of Different Types of Concessions in Liberia

Type	Size	Lease term	Government involvement	Regulations	Number and total area
Forest management contract (FMC)	50,000-400,000 ha	Long-term lease agreements around 25 years	Yes	Agreed terms and conditions conforming to regulations such as Liberia Code of Forest Harvesting Practice and Guideline for Forest Management Planning	7, totaling 1,007,266 ha
Timber sales contract (TSC)	Less than 5,000 ha	3 years	Yes		10, totaling 50,000 ha
Community forest management agreement (CFMA)	Less than 50,000 ha	Not more than 15 years	Yes		5 active by 2015 and 116 application s received in 2015
Private use permit (PUP)[a]	5,000 - 80,000 ha	Short-lived due to the moratorium	No, contract between private/com munity landowners and logging companies	No specific regulations	63, totaling 2,532,501 ha

Sources: Forest Development Authority (FDA)'s Regulations to the Community Rights Law with Respect to Forest Lands (2012) and 2015 Annual Report; Special Independent Investigating Body (SIIB) report on the issuance of PUPs (2012); Land Commission of Liberia's report on Land Rights, Private Use Permits and Forest Communities (2012)

[a] Illegal since 2012

Table A2. The list of variables on asset ownership and quality of dwelling that are used to generate the wealth score

Ownership	Quality of dwelling
Bank account	Cooking fuel (electricity, gas, kerosene, coal, charcoal, wood)
Bicycle	Floor material (earth, wood planks, vinyl or asphalt, ceramic/wood tile, cement)
Boat or a canoe	Lighting fuel (electricity, battery, solar, kerosene, oil/lantern, lamp, gas, candles, firewood)
Car/truck	Roof material (natural, metal, asbestos, cement, tarp)
Chairs	Toilet facility (flush, latrine – shared/private)
Computer	Wall material (mud/stick, straw, mud blocks, brick, cement blocks, various recycled)
Cupboard	Water source (piped into home/yard, piped public source, tube/borehole well, protected/unprotected well, spring, surface source, truck)
Electricity	The number of household members per sleeping room
Generator	
Mattress (not made of straw or grass)	
Mobile telephone	
Motorcycle/scooter	
Radio	
Refrigerator	
Sewing machine	
Table	
Television	
Watch	

Table A3. Standardized Differences and Variance Ratios

Variable	Baseline (2007,2009)				Post-concessions (2011,2013)			
	Standardized differences		Variance ratio		Standardized differences		Variance ratio	
	Raw	Matched	Raw	Matched	Raw	Matched	Raw	Matched
Under5	-0.01	0.04	1.01	1.09	0.02	0.03	0.94	1.08
Hheadsex	0.06	0	0.94	1	-0.03	0	1.02	1
Hheadage	-0.06	0.02	0.86	1.07	0.04	0.01	0.95	1.11
Road	-0.37	-0.12	0.83	1.18	-0.35	-0.05	0.95	1.11
Forest	0.22	0.11	0.77	1.37	0.29	-0.02	0.77	1.24
Town	0.46	0.03	0.52	0.90	0.13	0.04	0.70	1.08
Othrconcess	-0.24	-0.04	0.93	0.81	-0.02	-0.09	0.76	0.81

Note. The standardized differences and variance ratio values are calculated between control and treatment groups before and after matching for baseline (2007 and 2009) and for post-concessions (2011 and 2013), using Mahalanobis matching with a caliper, observations within 5 kms as a treatment group and a control group within 10 kms from concession boundaries.

Table A4. The Impacts of Private Use Permits on Wealth Score

	No pre-processing		Mahalanobis – without a caliper		Mahalanobis – with a caliper		Mahalanobis – with a caliper	
Threshold	5 kms	10 kms	5 kms	10 kms	5 kms	10 kms	5 kms	10 kms
Treatment impact								
2009	0.11 (0.08)	0.12 (0.090)	0.11 (0.074)	0.13 (0.081)	0.022 (0.10)	0.16^{*} (0.095)		
2011	0.13 (0.12)	0.09 (0.10)	0.13 (0.11)	0.11 (0.093)	-0.15 (0.11)	0.032 (0.12)		
2013	0.18^{**} (0.073)	0.21^{***} (0.078)	0.20^{***} (0.072)	0.20^{***} (0.074)	0.22^{***} (0.076)	0.22^{***} (0.079)	0.19^{***} (0.069)	0.16^{**} (0.072)
Year dummy								
2009	0.19^{**} (0.086)	0.19 (0.12)	0.17^{*} (0.093)	0.34^{***} (0.12)	0.12 (0.10)	-0.076 (0.19)		
2011	0.094 (0.077)	0.13 (0.12)	0.084 (0.10)	0.29^{**} (0.14)	0.044 (0.096)	0.0095 (0.23)		
2013	0.19^{***} (0.058)	0.22^{**} (0.086)	0.16^{*} (0.086)	0.24^{***} (0.080)	0.16^{*} (0.086)	-0.070 (0.17)	0.11 (0.086)	-0.13 (0.15)
Household and biophysical characteristics								
Under5	-0.01^{*} (0.01)	-0.02^{**} (0.01)	-0.01^{*} (0.01)	-0.01^{*} (0.01)	-0.02 (0.01)	-0.01 (0.01)	-0.03^{**} (0.01)	-0.01 (0.01)
Hheadsex	0.04^{**} (0.02)	0.04^{*} (0.02)	0.03^{*} (0.02)	0.04^{*} (0.02)	0.03 (0.03)	0.010 (0.028)	0.02 (0.03)	-0.02 (0.03)
Hheadage	-0.00 (0.00)	-0.00^{**} (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Road	0.01^{***} (0.00)	0.01^{***} (0.00)	0.01^{***} (0.00)	0.01^{***} (0.00)	0.00 (0.00)	0.01^{***} (0.00)	0.00 (0.00)	0.01^{***} (0.00)
Forest	-0.02^{***} (0.00)	-0.00 (0.00)	-0.01 (0.01)	-0.01^{***} (0.00)	-0.02^{*} (0.01)	-0.02^{***} (0.01)	-0.02 (0.01)	-0.01^{*} (0.01)
Town	-0.00 (0.00)	-0.01^{***} (0.00)	-0.00 (0.00)	-0.00^{***} (0.00)	-0.00 (0.00)	-0.01^{***} (0.00)	-0.00 (0.00)	-0.01^{**} (0.00)
Othrconcess	0.01^{***} (0.00)	0.00^{*} (0.00)	0.01^{***} (0.00)	0.01^{**} (0.00)	0.01^{**} (0.00)	0.00 (0.00)	0.01^{*} (0.00)	0.00 (0.00)
Livestock							0.07^{*} (0.04)	0.04 (0.04)
Bank							0.77^{***} (0.20)	0.96^{***} (0.13)
R^2	0.21	0.18	0.14	0.21	0.16	0.24	0.23	0.29
N	6099	7298	3966	4654	2967	3136	2278	2292

Note. Event study specification estimation results with and without pre-processing (using Mahalanobis nearest neighbor matching with or without a caliper - excluding 25 percent of observations with the highest distance) of the data. County-fixed effects are not shown in the table; Clustered standard errors (S.E) at the location cluster level. * p<.1. ** p<.05. *** p<.01.

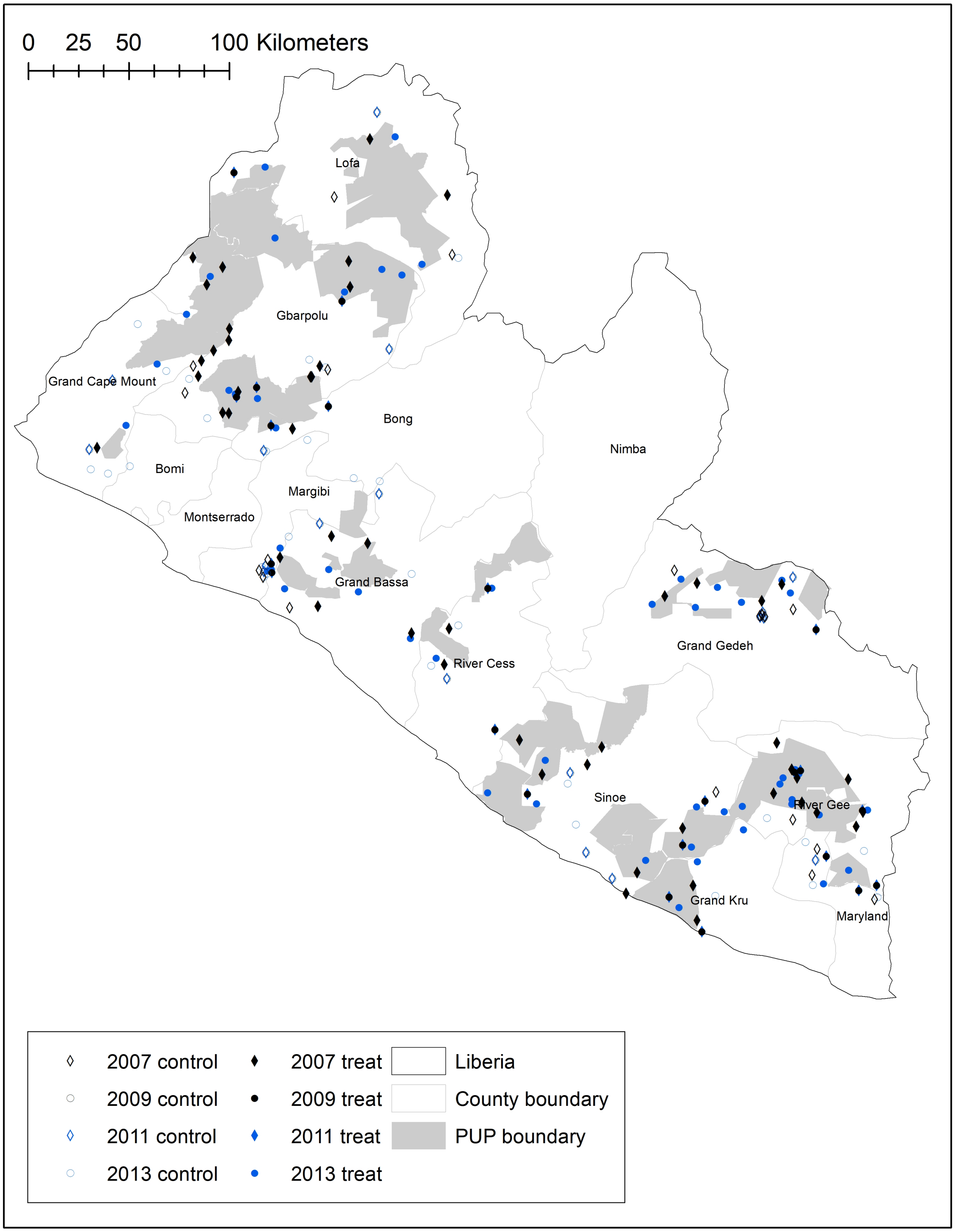


Figure A1. Geographic distribution of private use permits (PUPs) and household clusters (one cluster contains 20-30 households) in control (outside of 5 kms but within 10 kms of PUP boundaries) and treatment groups (within 5 kms of PUP boundaries) in the baseline using 2007 and 2009 DHS data and in the post-concession period using 2011 and 2013 DHS data

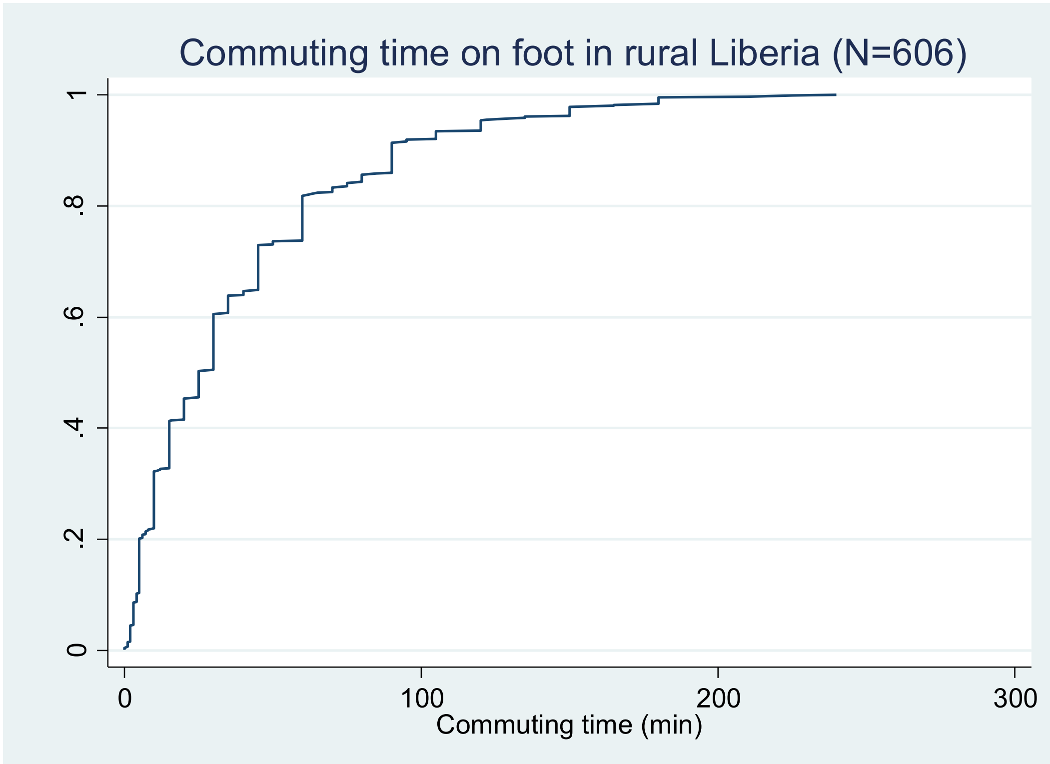
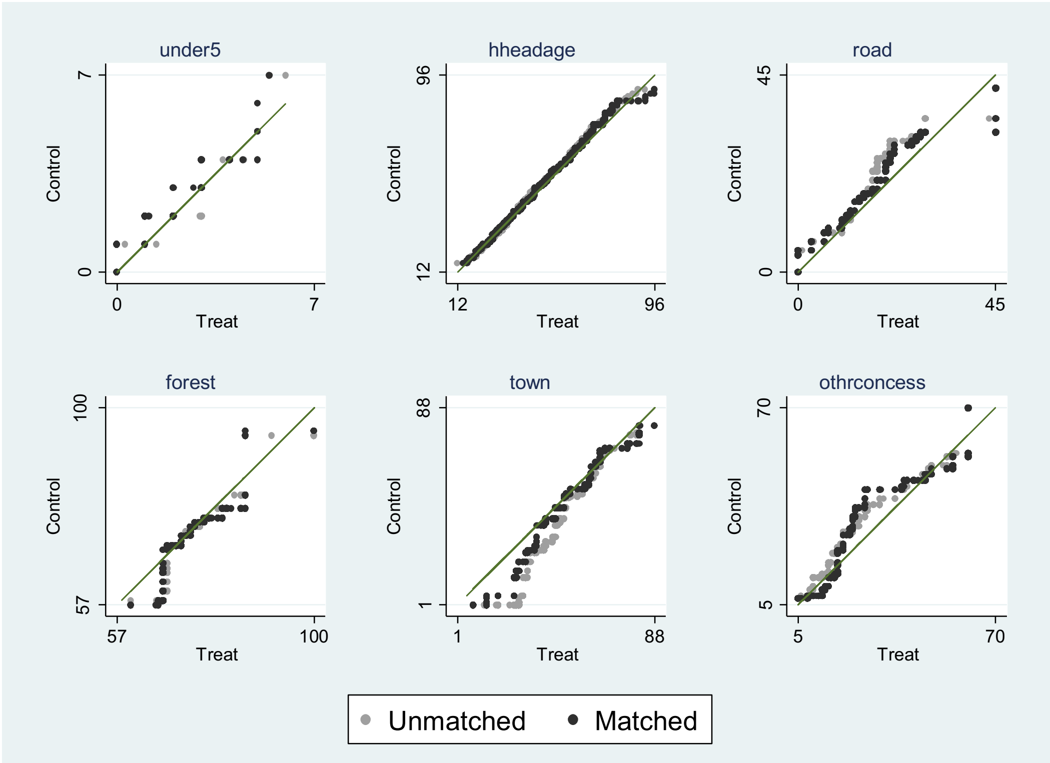


Figure A2. Cumulative distribution of commuting time on foot in rural Liberia from Household Income and Expenditure Survey (HIES) 2014

Panel A. QQ plots of continuous variables in the baseline



Panel B. QQ plots of continuous variables in the post-concession period

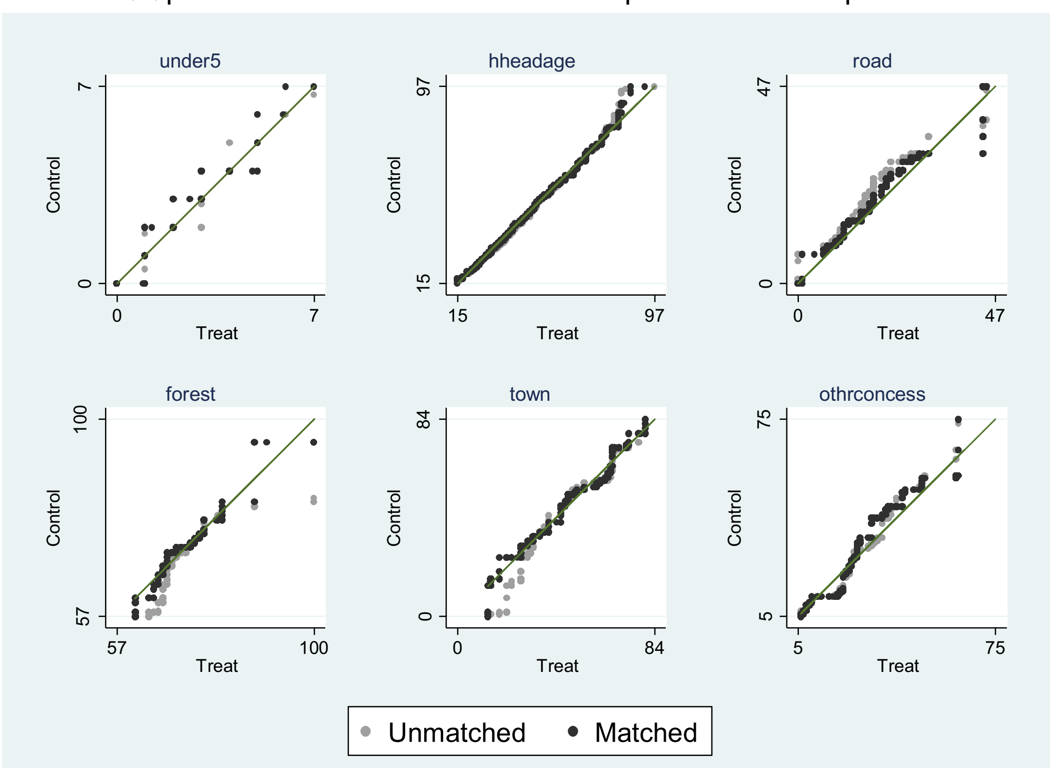


Figure A3. Quantile-quantile (QQ) plots of each continuous covariate before (grey) and after (black) matching