Hidden Welfare Effects of Tree Plantations

G. Anriquez; G. Toledo; R. Arriagada

Pontificia Universidad Católica de Chile, Departamento de Economía Agraria, Chile

Corresponding author email: gustavo.anriquez@uc.cl

Abstract:

Subsidies to promote tree plantations have been recently questioned because of potential negative social and environmental impact of the forestry industry. Quantitative evidence on the socioeconomic causal impacts of afforestation subsidies or of tree plantations is elusive, mainly due to data scarcity. We assess the overall impact of such a subsidy in Chile by using an original 20 years panel data set that includes small area estimates of poverty and relate it to the subsidy assignment at census-district scale. We show, with a battery of impact evaluation techniques, that forestry subsidies -on average- do, in fact, increase poverty. More specifically, using difference in difference with matching techniques, and instrumental variable approaches we show that there is an increment of about 2% in the poverty rate of treated (with subsidized tree plantations) localities. We also identify a causal mechanism by which tree plantations induce higher poverty, which is a negative effect on employment. Our research indicates the existence of negative welfare effects of the afforestation subsidy on local populations suggesting a reassessment of the distributional effects of the subsidy and the industry.

Acknowledegment:

JEL Codes: Q56, I32

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Keywords: Afforestation subsidies; impact evaluation; poverty.
JEL Codes: Q23, Q56, I32

Hidden Welfare Effects of Tree Plantations

1. Introduction

Tree plantations have been expanding since the 1950s following a strong growth in the demand for pulpwood, timber, and firewood. During the two decades between 1990 and 2010, the global area under tree plantations grew by 50%, and the global value of trade of the industry grew at an average of 6.6% per annum (FAO 2016). While there is good information on the spectacular expansion of the industry, less is known about the side effects of this expansion, especially its associated socio-economic impacts. The sector, usually perceived as a viable industry to foster an export base for developing countries, has generally received support from local governments in the form of subsidies and tax breaks, and from development agencies like the World Bank and the FAO through technical assistance and credits (Bull et al. 2006; Cossalter and Pye-Smith 2003).

The consolidation of these, usually monoculture, tree plantations, while generally succeeding in developing an export sector, has faced mounting criticism regarding unaccounted environmental and social costs derived from extensive land use change (Carrere and Lohmann 1996; Bull et al. 2006). These negative impacts are believed to be more severe in developing countries where a weak institutional framework sanctions abuses. Certification programs may appease these negative impacts, but these programs are voluntary and many times fail to improve social and environmental indicators (Elliott 2000; Araújo 2008; Zhao et al. 2011). Local perceptions of the industry, sometimes very negative, are correlated with the scale of the industry, and the distribution of land ownership that sustain the plantations (Schirmer 2007; Williams, Nettle, and Petheram 2003). Rural strife against large forest plantations has manifested in conflict for water, land, and employment conditions (Gerber 2011; Kröger 2014).

In spite of the fact that most of the expansion of the sector has been supported by direct public subsidies (Bull et al. 2006), clear evidence of a causal relation between plantation subsidies (or the monoculture forests they promote) and negative environmental and social impacts highlighted above is still lacking. The literature presents evidence that forest plantations and poverty correlate (Ruiz et al. 2004; Andersson et al. 2015), but this evidence does not amount to the identification of a causal link between the expansion of plantation forestry and higher poverty. In general, natural and planted forests tend to correlate spatially with poverty (Müller, Epprecht, and Sunderlin 2006; Sunderlin et al. 2008), because forests remain or expand in low-opportunity costs areas where poverty, although at a lower density, tends to be higher. In Chile, for example, (Andersson et al. 2015) show that municipalities with higher tree plantation coverage display higher poverty; while in China (Ruiz et al. 2004), forest income is correlated with the livelihoods of the poorest.

As we are unaware of other studies that measure the socio-economic impact of forestry promotion policy, we briefly review a related policy, Payment for Environmental Services (PES), which has been formally evaluated in the literature. PES programs and plantations forestry subsidies are not completely comparable, but both promote environmental outcomes, and represent public pecuniary costs. Although the literature regarding socio-economic impact of PES programs is also surprisingly scarce (Suich, Howe, and Mace 2015), we refer here to studies that have examined poverty impacts of PES programs using impact evaluation techniques. The conclusions emanating from these studies is that the socioeconomic outcomes of PES programs generally depend on the degree of social targeting and the causal mechanisms involved (Suich, Howe, and Mace 2015). If these programs involve high initial pecuniary and non-pecuniary costs, and there are scale economies, usually accompanied with credit market failures, as is usually the case with tree plantation subsidies programs in developing countries; then the expected outcome should be increased income inequality instead of broad welfare improvements (Muhammed et al. 2008; Deng et al. 2010). Robalino et al. (2014) examine the poverty impacts of the well-known Costa Rican conservation PES scheme. They conclude that the PES program has had an insignificant impact on poverty, but it appears to marginally decrease poverty in high slope areas, and marginally increase poverty in low slope areas. The Mexican conservation PES program was reviewed by Alix-Garcia, Sims, and Yañez-Pagans (2015), and demonstrated that while the program had been partially successful in reducing deforestation, there was an insignificant impact on welfare. The authors attribute this outcome, to high entry costs into the program, and the cost efficiency paradox of avoided deforestation; i.e. the most likely PES program participants have the lowest opportunity costs for their holdings, and the highest likelihood of conserving.

Given that social outcomes of PES programs are context specific (i.e. is the forest most at risk owned by the poorest?), it is necessary to move beyond causality into identifying the causal mechanisms or drivers that determine the outcomes of these policies. An important step in that direction is given by Ferraro and Hanauer (2014) that identify positive employment opportunities in the tourism industry in Costa Rica, the driver behind a positive impact of PES on social indicators. Moving back into subsidized afforestation, we can identify *a priori* which could be the drivers of welfare impacts, all associated to land use change, like: changing employment opportunities, environmental degradation that could hinder the harvest of environmental goods and services, migration, land redistribution, among the most salient (Angelsen and Wunder 2003; Lambin and Meyfroidt 2010; Sunderlin et al. 2008).

We present here what we believe to be the first formal impact evaluation of possible socio-economic impacts (poverty in this case) of an afforestation subsidy program. There are several reasons that make the Chilean afforestation subsidy program, studied here, internationally relevant. The Chilean Decree Law 701 (DL 701) program has been the largest afforestation subsidy scheme of its type, both in terms of funds disbursed, as well as period covered, more than 40 years (Bull et al. 2006). Also, the model of this program has been exported to other countries in the continent, like Ecuador and Uruguay. Moreover, there is ongoing social conflict in the regions where the program evolved, which calls for a more scientific approach to measure the existence or not, of alleged negative social impacts. Also, adding more policy relevance to the analysis, we move beyond measured impact towards identifying causal mechanisms.

In what follows we provide an assessment of the impacts on poverty of the Chilean afforestation subsidy program, the DL 701, using impact evaluation methods. The next section of the paper describes data and methods used in the program evaluation. The third section describes the main results in terms of poverty impact of the plantation subsidy program using non-parametric program evaluation methods. These results are followed by an analysis of possible causal mechanisms, and concluding remarks.

2. Data and Methods

The DL 701 program, rolled out in 1974, established subsidies of up to 90% of total costs as cash-back for plantations and forest management expenses on certified forest soils, defined as land that could not be used for intensive agriculture or livestock rearing without soil degradation. Anyone could apply and there were until 1998 no limits. However, the program unavoidably promoted concentration of resources. Large sunk costs involved as money is returned after 18 months approximately (Arriagada and Anríquez 2013), and significant scale economies involved in the establishment and management of tree plantations, inevitably acted as barriers for the participation of smaller scale land-holders. Another important aspect is that the private forestry industry started in earnest, together with this program. By 1976, there were about 300 thousand ha of tree plantations in the country, most of it on state-owned land, while by 2002 there were 1.6 million ha, almost all held by private owners (INFOR 2009). The study area covers the center-South regions of Chile and includes the contiguous regions VII, VIII, IX, X, and XIV, that captured 87% of the national area subsidized by the program between 1976 and 2002. Given the high level of public support for these plantations, 80% of tree plantations established over the study area after 1976 received subsidies (see Table 1). Thus, although we present here an evaluation of a particular public program, impacts must be interpreted under the light that the underlying causing mechanisms are related to the forest plantations engendered by the program.

2.1 Data

The data comes from two main sources. First, we use a spatially-explicit database of beneficiaries at census-district level. The program's database originally only identifies the municipality of the beneficiary property, so we used the property ID numbers to carefully map properties in space. Second, we built a detailed Chilean "poverty map," or small-area estimates of poverty (see Elbers, Lanjouw, and Lanjouw (2003)). Specifically, three poverty maps were prepared, for 1982, 1992, and 2002, using the national demographic censuses of those years, and the Chilean household survey CASEN (National Socio-Economic Characterization) of 1987, 1992, and 2003. Since poverty estimations, made at the household level, using the small-area estimates are noisy, a minimum household count is required to reduce the variance of these estimates. For this study, census districts with less than 140 households where merged in databases and maps, to ensure a minimum district size. This merging of areas was also carried out by merging changing districts over time to reconstruct spatial aggregates (census districts) that could be followed across time. Thus, we end up with 835 census districts, our unit of analysis, from the 120 municipalities that comprise the study area.

The approach followed here is to measure the impact in terms of poverty that the afforestation subsidy (the DL 701) had on districts receiving the program using program evaluation techniques. The main methodological approaches chosen are the difference in difference with matching estimator (DiD with matching), and an instrumental variables (IV) approach. These methods assume, as the bulk of the program evaluation techniques do, that participation in the forest subsidy program, or treatment, is binary. However, at the scale the program is studied here, participation is not binary: an infinitesimal area of the district may be affected by the program, or most of it. The impacts of the program will most likely depend on the intensity of this treatment. To apply the impact evaluation methods, we divide districts into treated if they had more than 5.7% of its area affected by the program, and not treated otherwise. The threshold chosen, 5.7%, is approximately the mean share of district area covered by the subsidy, but given the naturally skewed distribution of the program, treated districts are 231, or about 28% of the sample. The choice of treatment cutoff is certainly arbitrary, and the effects of this choice is explored with attention below.

2.2 Difference in difference

Given the two possible outcomes for a unit, $Y(1)$ if unit receives treatment and $Y(0)$ if it does not, and an observed realized uptake $d = 1$ if the program is taken, and 0 otherwise, the program evaluation challenge is to deal with the problem of an unobserved counterfactual. That is, usually we want to estimate average treatment effects (ATE), $E[Y(1) - Y(0)]$, but in reality we only observe $E[Y(1)|d = 1]$ and $E[Y(0)|d = 0]$. In this study we estimate the average treatment on the treated or $ATT = E[Y(1) - Y(0)|d = 1]$, and where we lack the unobserved counterfactual $E[Y(0)|d = 1]$. With adequate availability of information before and after treatment for treated and untreated population, one popular method to estimate the ATT is the difference in difference estimator, $DiD = E[Y(1)|t = 1, d = 1]$- $E[Y(1)|t = 0, d = 1] - \{E[Y(0)|t = 1, d = 0] - E[Y(0)|t = 0, d = 0]\}$. In words, the estimator is the difference of the change over time between treated and untreated. This estimator may be easily calculated non-parametrically calculating sample means of outcome variable before and after, for treated and untreated population. The exact same estimator can be obtained parametrically estimating the following regression:

(1) $$y_{it} = \alpha + \beta t + \gamma d + \delta \cdot d \cdot t + \varepsilon_{it}$$

where ε_{it} is a mean zero iid econometric error, t is time (0,1), given the data setup, $(\alpha, \beta, \gamma, \delta)$ is the vector of coefficients to be estimated, and $\hat{\delta}$ is the DiD estimator. Note that this equation can be expressed in time difference, i.e. a cross-section, as:

(2) $$\Delta y_{it} = \beta + \delta \cdot d + (\varepsilon_{it} - \varepsilon_{it-1})$$

which estimates the exact same $\hat{\delta}$, but with a different standard error. Equation (2) is useful as we explore econometric alternatives. Equation (1) shows why the DiD estimator is popular, it controls for persistent differences between groups, observable and unobservable, and it also controls for time-specific shocks that affect both the treated and untreated. However, it is important to understand how the counterfactual is built in this model. The counterfactual is implicit in the assumption that the trajectory of the impact variable (Y) is the same for the treated and untreated, also known as parallel trends assumption ($E[\varepsilon \cdot d \cdot t] = 0$), and therefore the difference in the differences can be attributed to the treatment.

The parallel trend assumption may not hold in the case studied here. Areas with subsidized plantation forests are located in zones that are generally poorer, with lower population density, and lower general levels of human capital. The poverty trend in these areas is not likely to be similar to urban areas with lower poverty, higher levels of human capital, and no subsidized afforested area. This is why we make use of another bedrock of the program evaluation literature, propensity score matching (PSM). The method suggested by Rosenbaum and Rubin (1983) relies on bypassing the selection (into treatment) bias by finding a condition that guarantees independence between outcome Y's, and actual treatment. This is called differently in the literature: unconfoundedness, ignorability (of treatment assignment), or selection based on observables, formally: $Y(0), Y(1) \perp d|X$. That is, outcomes are independent from treatment assignment given a vector of observables X. This guarantees that we can estimate the unobserved counterfactual, $E[Y(0)|d = 1, X] = E[Y(0)|X]$. As highlighted by Heckman, Ichimura, and Todd (1998), the idea underlying unconfoundedness, is that at a given level of $X = x$, there are units that participate and that do not, and that for those units (given that they are equivalent on observables), outcomes are not correlated with participation. As we focus on the ATT estimator, we need a lighter condition than full ignorability, which is conditional mean independence $E[Y(0)|d, X] = E[Y(0)|X]$, which is a less restrictive version of the reduced "partial" ignorability assumption, $Y(0) \perp d|X$.

In this study we use two type of matching techniques, PSM, as suggested by Rosenbaum and Rubin (1983), and Genetic Matching by Diamond and Sekhon (2012). Genetic matching is both a distance metric, a generalization of the popular Mahalanobis distance, and an algorithm, that searches for best weights for the generalized Mahalanobis distance proposed (see details in Diamond and Sekhon (2012)). According to simulations performed by these authors, Genetic Matching can achieve better balance of covariates, not only of means, but overall distributions of covariates between treated and controls. For simplicity, we make one-to-one matches using the nearest neighbor approach with replacement, which reduces bias, compared to matching on several neighbors, at the cost of decreased efficiency. Unobservables that determine participation, and the probability of participating in the program, may remain after matching, and as unobserved dimensions are likely not balanced by the matching techniques proposed. However, unobservables only bias the estimated treatment effect if they are correlated with outcomes. This possibility cannot be rejected *a priori*, and is investigated below. Once all treated districts have been matched, we estimate the DiD treatment (ATT) with the matched sample, more confident that the parallel trends assumption is likely to hold.

3. The impact of afforestation subsidies on poverty

Before delving into the estimation of the poverty impacts of the Chilean forest subsidy program, it is useful to think what are *a priori* the biases that selection into treatment may promote. At the individual beneficiary level, it is more likely that better educated and wealthier households overcome the bureaucratic obstacles to obtain the subsidy, and have the personal financial support to make the initial investments before receiving the subsidies as has been shown in the case of PES (Alix-Garcia, Sims, and Yañez-Pagans, 2015). However, here the unit of analysis is the census district area. The afforestation subsidies promote the establishment of forest plantations in areas where land is less productive (i.e. higher slope, poor quality soils, etc.), and therefore they are likely to be more prevalent in areas with higher poverty rates, but also with lower population density, and poverty density. Moving into the change in the poverty rate between two decades (1982 and 2002), one would expect that the poorer areas reduced poverty levels more than better-off area. This would be the natural result if poverty fell at the same rate in all regions, but also because there is a tendency for areas to converge economically. Over longer periods of time, migration can cloud these predictions, because it is not usually the poorest who migrate; and therefore, poverty may increase simply as a result of the better-off migrating.

The vector on which unconfoundedness is built must contain, ideally, all variables that are correlated with participation and outcomes. First, we include variables that describe the initial poverty levels: the poverty rate, the district mean per-capita income, and income inequality as measured by Theil index[1]. Then, we include variables that describe the socio-economic context of the district: mean years of schooling of households, household size, population density, demographic dependency, urban population (% of the district), and the share of households with employment in the primary sector. Finally, we include variables that physically describe the districts, district size, road density, distance to nearest port, and proportion of the district area that is classified as with high erodibility.

The results of the different estimates of the poverty impact of the Chilean afforestation subsidy program are presented in Table 2. The first number presented 0.014 is the simple *DiD* estimate of poverty impact, i.e. the naïve estimate. It says that the change in the poverty rate between 1982 and 2002, was 1.4% higher in districts that had at least 5.7% of its area subsidized with the afforestation program in 2002. Given that over this period the country experienced pronounced growth and poverty reduction, in practice it means that in the treated districts poverty fell less (1.4% less) than in districts that did not receive the afforestation program, and this difference is statistically significant. If the assumption of parallel trends is valid, this would be a sound estimator of ATT.

The second and third row present the estimates of ATT, using the *DiD* with matching method. The first column shows the non-parametric matching estimator, while the second column shows the post-matching regression *DiD* estimator. In the third column, we add baseline co-variates to the *DiD* regression. The use of baseline covariates (i.e. not contaminated by response to treatment) is used in randomized control trials (RCTs) to improve efficiency and test power, particularly when sample size is at a premium. We follow the same strategy in the third column. If the baseline covariates are working adequately, the ATT point estimator should not change, but the efficiency should improve (as is the case in the table). The vector of exogenous and baseline (1982) variables includes: distance to port, district area, rural area, and high erodibility area. The post matching covariate balance tables are presented in the supplementary material (Tables A2 and A3), together with the overall balancing tests (see Table A4). In brief, very good balancing was attained with both methods; for the PSM, covariates in quadratic form were required. Balancing of all means was achieved with both methods, and with both methods all but two variances were balanced. In spite of this, normalized differences (by the root of sum of variances) were all within the 10% bound,

[1] Detailed description of all variables used available in the supplementary material, Table A1.

which is why overall balancing tests where handily passed. Overall the Genetic Matching achieved slightly lower normalized differences.

Remarkably, all estimators indicate that the ATT is significant, and it moves within a narrow window between 1.6 and 2.4%. The second panel of Table 2, shows the *DiD* estimates for poverty change between 1982 and 1992. Two things are different in this case, in the first place less time has elapsed for whatever poverty impact of plantation forests to manifest itself. Consider that impacts are likely to be slow, as is the rotation of plantations 12-25 years. Furthermore, the number of treated districts is lower, as the program has been in constant expansion, which reduces the sample size to estimate with precision the ATT. Given these considerations, the results are not surprising, the ATT for the differences between 1982 and 1992 is estimated marginally lower with all methods, and precision is also lower.

However, the significant treatment effects estimated may be conditioned to the definition of treatment: by choosing a binary treatment with a threshold which by chance detects treatment effects. We explore this possibility by analyzing the sensitivity of the treatment effects to different subsidy area coverage binary thresholds. While Table 2 estimates ATT with treatment defined at 5.7% of district area covered, we explore ATT results if treatment was defined at 3.2 and 8.2% of area covered, i.e. +/- 2.5%, in the supplemental tables A.4 and A.5. The ATT estimates are also significant at these different thresholds. Estimates are more precise, both in intra-method and inter-method variability, when treatment is defined at 8.2% of area covered by subsidy. With treatment defined at 3.2% of area, the ATT estimates range between 0.8 and 2.3%, while ATT estimates move between 1.7 and 2.1% when treatment is defined at 8.2% of area.

4. An Instrumental Variables Approach to Measure the impact of afforestation subsidies on poverty

The different *DiD* matching estimators provide very strong evidence that the public afforestation program had as an unforeseen side-effect an increase in poverty. However, these estimators leave behind some questions, like: are there unevenly distributed unobservable area characteristics which are driving the results? and is the binary simplification of a continuous treatment forcing the results found? As a second examination of these relevant questions, we attempt to identify program impact effects using instrumental variables (IV). To introduce the IV econometric approach, let us start with the Roy model that econometrically describes outcomes for treated, y_1 and untreated, y_0 individuals:

(3) $$y_0 = \mu_0 + g_0(\mathrm{x}) + u_0$$

(4) $$y_1 = \mu_1 + g_1(\mathrm{x}) + u_1.$$

where μ's are parameters, $g_i(\mathrm{x})$ functions of a vector of observables x, and u_i are deviations from the mean outcome, i.e. $E[u_i|x] = 0$. Given that the expected value of outcomes is $E[y] = d\, y_1 + (1-d)\, y_0$, we can express the expected model as:

(5) $$y = \mu_0 + g_0(x) + d(\mu_1 - \mu_0) + d[g_1(x) - g_1(x)] + u_0 + d(u_1 - u_0).$$

We follow Wooldridge (2010), and assume that $g_0(x) \equiv \mathrm{x}\beta_0$, and that $g_1(x) - g_1(x) \equiv [\mathrm{x} - \mathrm{m_x}]\delta$, where $\mathrm{m_x}$ is the vector of means (expected values), and δ, a vector of coefficients to be estimated. These are not crucial assumptions, they represent the standard econometric practice of linearizing unknown functions; also they could be tested, or flexible linear functional forms could be used if nonlinearities were deemed relevant[2]. Under these assumptions we have the following econometric specification:

(6) $$y = \mu_0 + \mathrm{x}\beta_0 + d(\mu_1 - \mu_0) + d[\mathrm{x} - \mathrm{m_x}]\delta + \nu,$$

where $\nu = u_0 + d(u_1 - u_0)$. This model can be estimated, but it suffers from two serious known problems related to the evaluation of programs.

To treat problems separately, assume for a moment that $u_1 = u_0$. The first problem, which has already been confronted above, is the lack of a "counterfactual," which here is manifested as $E[u_0|x, d] \neq 0$, that is, the treatment d is endogenous. This is a dummy endogenous variable model which can be treated with instrumental variables. Thus, if we have an adequate set of instruments z such that $\mathrm{Cov}(z, d) \neq 0$, and $\mathrm{Cov}(z, u_0) = 0$, we can estimate predicted probabilities $\hat{d}$, using a probit or logistic model on x, z, and estimate model (6), using the vector of observed $(1, \mathrm{x}, d, d[\mathrm{x} - \mathrm{m_x}])$, and the vector of instruments $(1, \mathrm{x}, \hat{d}, \hat{d}[\mathrm{x} - \mathrm{m_x}])$, by 2SLS. This method, as Wooldridge (2010) shows, has a first stage predictor of participation probability that is asymptotically efficient among the class of IV estimators where the IVs are functions of (x, z); and the IV-2SLS model is robust to mis-specification of the first stage model $Pr(d = 1|x, z)$. Having estimated model (6) by IV-2SLS, treatment effects can be calculated: $ATE = (\widehat{\mu_1 - \mu_0}) + [x - m_x]\hat{\delta}$, and $ATT = (\widehat{\mu_1 - \mu_0}) + \{d[x - m_x]\hat{\delta}\}_{d=1}$.

The second problem of (6) occurs when there is selection or sorting based on unobservables. We may have $E(u_1 - u_0) \neq 0$, let us call it the gain of treatment on

[2] Another possibility is to follow the suggestion of Heckman, Urzua, and Vytlacil (2006) and estimate $d(\beta_1 - \beta_0)\mathrm{x}$.

unobservables, and still estimate model (6). In this case, the non-zero mean error translates into a biased estimate of the intercept $\widehat{\mu_0}$ in (6), but the rest of the estimators can be recovered adequately. We face a problem when there is selection into treatment based on this gain, i.e. $(u_1 - u_0) \perp d$ does not hold, what the literature calls "unobservable heterogeneity" or "essential heterogeneity". Heckman, Urzua, and Vytlacil (2006) propose a method to estimate treatment effects under such a model, but they also propose a method to test the presence of essential heterogeneity. The authors show that under the null of no essential heterogeneity, outcome is linear on the propensity score: $E[Y|x,z] = a + b\,p(x,z)$, with parameters a, b and propensity score $p(x,z)$. Standard -tests on the significance of higher order polynomials coefficients suggest that the relation of the propensity score with outcomes of our data is not quadratic or cubic, which suggests that the IV approach described by (6) is suitable.

Although Woolridge suggests that mis-specification of the treatment probability model is not crucial for this IV estimation technique, we believe we have a good vector of instruments, which we fully test. Both distance to the pulp mill, as well as rainfall, are very likely to be highly correlated with tree plantations, and hence to the public subsidy scheme, but not highly correlated with poverty. Pulp mills were established in the late 1960s covering the region were plantations flourished (near Arauco and Constitución), located near sparsely populated rural areas, they correlate with poverty in the area; but there are many sparsely populated, and poor rural areas, far from the mills.

We test successfully that poverty balances effectively between areas that are closer and farther than 60 km from the closest mill, and between areas that receive more and less than 1,744 mm of yearly rainfall. We are in the right track, but these are not formal tests. In Table 3, using a structural model for poverty, we test the validity of instruments. First notice, that in the first column, the OLS estimates show that poverty is explained by a set of standard correlates, and ignoring our endogenous first regressor, the rest of the explanatory variables have signs and significance as expected. The first aspect of a good instrument, $\mathrm{Cov}(z,v) = 0$, is tested with the Sargan-Hansen overidentifying restriction test, which is not rejected with p-values in the 0.61 - 0.87 range. In other words, the instrumental z vector is correctly excluded from the model in Table 3, both in the poverty equation in levels (columns 1-3) and in differences (columns 5-6). The second aspect of good instruments, that they are "sufficiently" correlated with the endogenous variable, to limit the potential bias due to efficiency loss of the 2SLS estimator, is harder to test. We use the (Cragg and Donald 1993) F-statistic from the first stage, and compare it to the (Stock and Yogo 2002) IV-Bias tables to assess the maximum potential bias imposed by our chosen instruments. We find that in both levels and differences, the Wald tests on parameter significance are off by about 30%. For example, the 5% significance level Wald test critical-value for the significance of a given parameter would be 3.84, but given the bias imposed by these instruments, to obtain the correct 5% rejection rate, the "true" critical level would be about 4.99 (3.84*1.3). However, the 2SLS parameters estimated in Table 3 handily surpass this more stringent critical level, the significance Wald test is in fact above 20 in columns 3, 5 and 6. In conclusion, instruments may not be as strong as ideally desired, but results are very strong (complete IV test results included in supplementary materials).

The impact of district area subsidized by the afforestation program on poverty, shown on Table 3, does not identify any traditional treatment effect of the program evaluation literature. Nonetheless, the results in the table are meaningful and key to validate this assessment. First, it deals with the endogeneity of program participation and poverty, and estimates a positive impact of area afforested with the subsidy and poverty. Also, it removes the problem of binary treatment allocation, and estimates that given sample averages, increases in area afforested by the program increases poverty.

Having validated the instruments proposed, we estimate the model presented in (6), which estimates a *DiD* IV ATT estimator that is presented in Table 2. The instruments used, are all the variables included in Table 3, plus the excluded instruments, rainfall and pulp mill distance. In the second column in the IV row of Table 2, only the program participation binary is used, while in the third column we include, as the covariates vector, the same baseline covariates used in the non-parametric approaches above; that is, distance to port, district area, rural area, and high erodibility area. Unlike the non-parametric approaches above, the inclusion of baseline covariates is expected to change the ATT estimates given that they add heterogeneous response of the treated based on covariates, i.e. $\{d[x - m_x]\hat{\delta}\}_{d=1}$. When considering the whole period, 1982-2002, the results presented in Table 2, show that the IV approach provides remarkably consistent (with non-parametric approaches) estimators of ATT, both in levels and significance.

5. Discussion: identifying causal mechanisms

We have shown with a battery of impact evaluations methodologies, and different assumptions that the Chilean afforestation subsidy program has *caused* increased poverty in the areas where it thrived. This outcome, while relevant to the current discussion on the impacts of tree plantations on territories, is not very informative for the policy design or reform of the program. We need to delve into the mechanisms which cause poverty in order to understand how the program is affecting social outcomes, how it can be improved or whether it is socially unsustainable. We can sketch a framework that links socio-economic outcomes with the program. Although we are evaluating the policy, a subsidy, causal mechanisms must be related to either the direct payments distributed over the territory, or the tree plantations engendered by the program. Obviously, not all tree plantations spawned from this program, but as we showed above the overwhelming majority of the area did. The direct financial transfers, may have had a poverty decreasing impact if they were received, at least in some proportion, by the poor. This is not a very likely outcome of the program in the first stage we study, because the subsidy requires large upfront expenditures, clearly out of budget for the poor, and there were, at least until the 1998 program reforms, no public programs to help with either loans or transaction costs. The tree plantations, on the other hand, have a direct socio-economic impact, by creating employment, both in the plantations themselves, and in up- and down-stream activities.

The employment creation is always an aspect highlighted by industry boosters; however, the true employment effect is a net impact between the employment created by the sector, and the opportunity cost of employment in other activities that could have developed where the forests grow. This net effect needs not be positive. To extract the net employment effect, one must compare employment outcomes between comparable areas, in other words with an adequate “counterfactual”, which is what program evaluation methods achieve by making comparisons precisely with equivalent districts.

Another important mechanism mediating the poverty effect of the program is migration. Associated to employment outcomes clearly, migration and out-migration, in the case of these rural communities we are focusing on, can have an effect in poverty if those migrating have a different poverty profile than those that stay. Usually, it is not the poorest that migrate, because those that are most poor do not have the backing to invest in the pecuniary and non-pecuniary costs associated with migration. Thus, increased out migration may have a poverty (rate) increasing effect if those migrating are predominantly non-poor. Finally, the tree plantations themselves, may have a poverty reducing effect if they are rich in non-timber forest products (NTFP), and the poor have open or partial access to these resources. Chilean tree plantations, which are mostly mono-cultures of *pinus radiata* and *eucalyptus globulus* are not rich in NTFP; and furthermore, tree plantations strengthen private land rights, which tends to reduce the area of open access resources for NTFP collection and cattle grazing.

To test employment impacts of the program we use two impact indicators, the employment rate of heads of households within the demographic labor force age window (15 to 64 years old), and the total employment rate for all the population in this same age group. Again, we measure the ATT of districts, defining as treated those with more than 5.7% of district area with subsidized tree plantations, and we use as previously, the *DiD* estimator, and the more suitable *DiD* with matching estimator. Table 4, shows the estimated impacts of the program on the employment indicators. The table shows that both employment of heads of household, as well as the total employment rate, ended up about 2.5 - 3% lower in districts receiving the afforestation subsidy. Thus, we identify a clear poverty causing mechanism of the afforestation subsidy program. It is not that these forests do not create employment, but that they create about 3% less jobs than alternative activities that develop in similar districts, i.e. tourism, agriculture, and livestock rearing.

We also measured possible impacts on migration using municipal-level imputed net-migration rates. Unfortunately, the census only identifies migrants by their municipality of origin. We find that there is a positive correlation between out-migration rates and the program; however, this relationship is not statistically significant. This means that the migration effects do not exist, or they are not sufficiently strong to be statistically detected by the reduced, municipal-level sample size.

6. Conclusions

This paper has shown with a battery of different econometric empirical strategies that the Chilean afforestation subsidy program, one of the oldest and largest programs of its type, has caused increased poverty in the areas where the subsidized forests were established. We show that this result is very robust, with estimates of increased poverty by the program in treated districts between 1.6 and 3%. This is a remarkably small variability as we explore completely different parametric, non-parametric, and instrumental variables approaches. The paper also shows that net employment creation of tree plantations promoted by this subsidy program is lower than area-suitable alternatives. Hence, this negative net-employment effect is identified as a causal mechanism for the increased poverty found. Emigration from treated areas may be another mechanism by which poverty increases, but data limitations that reduce our sample hinder a definite answer on this mechanism. Further studies are required to fully understand other potential causal mechanisms, such as environmental externalities and migration.

This paper has identified an often-ignored negative socio-economic impact of this afforestation subsidy, however, the scope and implications of these results need to be assessed. First, there are likely additional negative impacts on the environmental front. Indeed, monocultures may have a negative effect on biodiversity, water availability, and other ecosystem services. On the other hand, this paper does not present a cost-benefit analysis of the program or the forestry sector that the program promoted. Simple "back of the envelope" calculations would show that these socio-economic externalities do not overwhelm the benefits for the country of the forestry sector's GDP, employment, and foreign exchange generated; even if we value the sector at about 60%, which is the net forest additionality of the program estimated by (Arriagada and Anríquez 2013). Nonetheless, the negative socio-economic impacts uncovered by this study cannot be ignored. Those 2% additional poor, live in low poverty density areas, so they are not many. These poor, on the other hand, are among the poorest in the country, as the poorest and the forest coincide in space.

Currently tree plantations continue expanding in Chile, mainly in indigenous territory exacerbating existing conflicts. In this sense, our results suggest revising the zoning and intensity of tree plantations considering that greater and more robust impact occurs with larger portions of territory with plantations. Countries exploring to copy the Chilean model, or are currently implementing the Chilean model, like Uruguay and Ecuador, should seriously consider compensatory social policies for those most vulnerable and most negatively impacted by the planted forests' growth.

Tables and Figures

Table 1. Evolution of forestry plantations and subsidies in the study area.

	Period 1976-2002		
Region	**Total Forest Plantations (ha)**	**Subsidized land (ha)**	**With Subsidy Support (%)**
Maule	265,047	224,377	84.7
Biobío	437,184	357,369	81.7
Araucanía	271,230	246,817	91.0
Los Ríos y Los Lagos	154,760	121,122	78.3
Total area	**1,179,764**	**949,684**	**80.5%**

Source: CONAF (2015), INFOR (2009)

Table 2. Summary of estimated ATT impact under different matching techniques.

Period 1982-2002		**Post-matching Regressions**	
Matching technique	Non-Parametric DiD	DiD	DiD + baseline covariates
No matching	0.0137** (0.041)[a]		
PSM	0.0159* (0.042)[b]	0.0159*** (0.048)[c]	0.0187*** (0.016)[c]
Genetic Matching	0.0163** (0.002)[d]	0.0230*** (0.000)[c]	0.0238*** (0.000)[c]
IV treatment effect		0.0249*** (0.005)[c]	0.0334*** (0.002)[c]

Period 1982-1992		**Post-matching Regressions**	
Matching technique	Non-Parametric DiD	DiD	DiD + baseline covariates
No matching	0.0134 (0.122)[a]		
PSM	0.0144 (0.149)[b]	0.0144* (0.064)[c]	0.0135* (0.044)[c]
Genetic Matching	0.0175** (0.050)[d]	0.0149** (0.030)[c]	0.0146** (0.029)[c]
IV treatment effect		0.0374** (0.024)[c]	0.0621*** (0.000)[c]

P-values in parentheses, ***= 99% confidence, **= 95%, * = 90%.

[a] Using non-parametric Difference in Difference standard error

[b] Using (Abadie and Imbens 2006) standard error.

[c] Derived from bootstrapped standard errors, 1000 iterations.

[d] Coefficient and p-value of ATT calculated with genetic matching weighting matrix.

Table 3. IV Estimates of the impact of subsidized afforested area and poverty

	Panel			Cross Section		
	District Poverty	District Poverty	District Poverty	Poverty Change 1982-02	Poverty Change 1982-02	Poverty Change 1982-92
Variables	OLS	IV 2SLS Pooled	IV 2SLS Random Effects	OLS	IV 2SLS	IV 2SLS
District area subsidized	0.000180** (8.28e-05)	0.00156 (0.00142)	0.00499*** (0.000928)	0.000242 (0.000426)	0.0314*** (0.00699)	0.0608*** (0.0129)
Per capita Income (spatial lag)	-0.00796 (0.00582)	-0.00983 (0.00640)	-0.0151** (0.00586)	-0.0156 (0.0102)	-0.0421** (0.0181)	-0.0822** (0.0326)
Theil Income Ineq. Index	0.0640* (0.0346)	0.0697** (0.0334)	0.0862** (0.0348)	-0.0969*** (0.0353)	-0.0881 (0.0682)	-0.0377 (0.131)
Years of schooling HH	-0.0411*** (0.00216)	-0.0390*** (0.00294)	-0.0335*** (0.00259)	0.00978*** (0.00339)	0.00851 (0.00741)	0.000259 (0.0156)
Household Size	0.0201* (0.0103)	0.0208** (0.00979)	0.0266*** (0.0101)	-0.00881 (0.00773)	-0.0611*** (0.0203)	-0.100*** (0.0354)
HH work in primary sector	-0.0567*** (0.0179)	-0.0420* (0.0246)	-0.00542 (0.0203)	-0.0326 (0.0352)	-0.0291 (0.0617)	-0.0187 (0.0996)
Population density	-0.000103** (4.40e-05)	-9.65e-05** (4.13e-05)	-8.64e-05* (4.58e-05)	-2.49e-05 (0.000107)	0.000259 (0.000239)	0.000309 (0.000487)
Demographic dependency	0.105 (0.107)	0.128 (0.101)	0.184* (0.102)	-0.124 (0.105)	-0.351 (0.334)	-0.879 (0.637)
% urban	0.151*** (0.00795)	0.157*** (0.0102)	0.165*** (0.00795)	-0.110*** (0.0154)	-0.130*** (0.0254)	-0.0584 (0.0419)
Constant	0.671*** (0.0917)	0.648*** (0.0893)	0.577*** (0.0892)	-0.0573 (0.127)	0.502 (0.314)	1.369** (0.582)
Biophysical characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Municipal	Municipal	Municipal			
Year effects	Yes	Yes	Yes			
Observations	2502	2502	2502	834	834	834
R-squared	0.928			0.266		

Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4. Difference in Difference with Matching ATT estimates of impact of DL701 on different measures of employment

Period 1982-2002		**Post-matching Regressions**	
Matching technique	DID	DID	DID + baseline covariates
Employment rate (Head of the household)			
Baseline – No matching	-0.085*** (0.00)		
PSM	-0.0293 (0.131)	-0.0293** (0.032)	-0.0331** (0.012)
Employment rate (total population)			
Baseline – No matching	-0.067*** (0.00)		
PSM	-0.0249* (0.085)	-0.0249** (0.016)	-0.0247** (0.012)

P-value in parentheses, ***= 99% confidence, **= 95%, * = 90%.

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Supplementary Material for “Hidden Welfare Impacts of Tree Plantations”

Table A. 1. Data and sources

Variable	Description	Source
Outcome Variable		
Poverty	Headcount ratio of individuals below the poverty line.	Poverty Microestimations: 1982, 1992, 2002.
Treatment variable		
% subsidized land	Share of district area subsidized with at least 1 component of the program - accumulated	District mapping from the subsidy database (aggregated for years: 1982, 1992, 2002).
Confounders		
Theil Index	Income distribution index	Poverty Microestimations: 1982, 1992, 2002.
Income	Log of the income and spatial lag of the log of the income (District average)	
Schooling years of the HH	Mean years of schooling of adults in the HH, district average	Population Census 1982, 1992, 2002.
Demographic dependency	Percentage of household members out of labour force over the total household members (District average)	
Family Size	Household size (District average)	
Urban area	Percentage of urban inhabitants (District average)	
% who work in agriculture (district)	District share of household heads employed in agriculture	
Population density	District number of people per hectare	
Distance to pulp mill	Distance from the district centroid to the closest pulp mill (KM)	Own calculation based on National Statistics Institute Cartography: 1982, 1992, 2002.
Distance to port	Distance from the district centroid to the closest port (KM)	Own calculation based on National Statistics Institute Cartography: 1982, 1992, 2002.
Precipitation	Accumulated annual precipitations interpolated to districts (5 years average centered in 1982, 1992 and 2002)	Own calculation based on historical registers of Ministry of Environment.
District area	District area (ha)	National Statistics Institute : 1982, 1992 y 2002
Road density MOP	District road density (km/km2)	Ministry of Public Works, 2002
High erodibility surface (%)	District high or very high erodibility surface (%)	Ministry of the Environment, National Committee of the Environment, scale 1:250.000. 2002.

Table A. 2. Post-Matching Covariate Balance (PS Matching).

Variable	Treated	Control		t-test		V(T)/	Normalized
	Mean		%bias	t	p>t	V(C)	difference[a]
Poverty 1982	62.2%	61.7%	6.3	0.96	0.338	1.07	0.05
Per Capita Income	9.301	9.297	1.7	0.32	0.746	0.93	0.02
Theil Index	0.735	0.731	5.4	0.69	0.487	1.2	0.04
Years of schooling HH	3.810	3.701	6.8	1.3	0.194	0.97	0.07
Demographic dependency (dependent over total)	0.405	0.407	-6.8	-0.99	0.322	1.16	-0.06
Distance to the closest port	75576	78810	-7.3	-0.99	0.323	0.94	-0.06
Urban area % of the district	0.220	0.179	10.2	1.48	0.14	1.12	0.08
High-erodibility (%) area,	0.075	0.082	-2.1	-0.45	0.653	0.93	-0.03
Road density	0.095	0.093	7.2	1.02	0.31	0.94	0.06
HH work in agriculture (district average)	0.493	0.499	-2.5	-0.37	0.714	1.15	-0.02
Population density	0.804	0.843	-0.2	-0.12	0.901	1.75*	-0.01
Household Size	4.927	4.895	7.9	0.93	0.352	0.83	0.05
District area	16411	18819	-7.5	-1.46	0.146	0.61*	-0.08

* if variance ratio outside [0.81; 1.24]

[a] Imbens and Rubin (2007) suggest as a rule of thumb a normalized difference less than one quarter

Table A. 3. Post-Matching Covariate Balance (PS Matching).

Variable	Treated	Control	%bias	t-test		V(T)/	Normalized
	Mean			t	p>t	V(C)	Difference[a]
Poverty 1982	62.2%	61.7%	-3.6	-0.57	0.567	1.13	-0.03
Per Capita Income	9.301	9.297	3.7	0.76	0.45	1	0.04
Theil Index	0.735	0.731	1.8	0.23	0.815	1.14	0.01
Years of schooling HH	3.810	3.701	-7.1	-1.4	0.162	1.07	-0.08
Demographic dependency (dependent over total)	0.405	0.407	4.1	0.65	0.517	1.45*	0.04
Distance to the closest port	75576	78810	-2.6	-0.38	0.706	1.05	-0.02
Urban area % of the district	0.220	0.179	1.5	0.21	0.834	1.02	0.01
High-erodibility (%) area,	0.075	0.082	-9.9	-1.83	0.068	0.57*	-0.10
Road density	0.095	0.093	-6.3	-0.93	0.351	1.02	-0.05
HH work in agriculture (district average)	0.493	0.499	-2.5	-0.37	0.715	1.03	-0.02
Population density	0.804	0.843	-2.3	-1.37	0.17	0.93	-0.08
Household Size	4.927	4.895	-3.4	-0.45	0.653	1.22	-0.02
District area	16411	18819	-3.1	-0.46	0.644	0.28*	-0.03

* if variance ratio outside [0.80; 1.25]

[a] Imbens and Rubin (2007) suggest as a rule of thumb a normalized difference less than one quarter

Table A. 4. Covariate Balance Tests

Sample	Ps R2	LR chi2	p>chi2	Mean Bias	Median Bias	B	R	%Var
Unmatched	0.329	369.97	0	45.4	46.4	136.1*	0.26*	86
Matched (PSM)	0.01	7.99	0.89	3.3	3	24.1	0.84	21
Matched (Genetic Matching)	0.027	24.11	0.03	4	3.4	39.0*	0.87	23

* if B>25%, R outside [0.5; 2]

Table A. 5. Summary of estimated ATT impact under different matching techniques (treatment defined at 3.2% of district area covered by the subsidy)

Period 1982-2002		**Post-matching Regressions**	
Matching technique	Non-Parametric DiD	DiD	DiD + baseline covariates
No Matching	0.0139** (0.031)[a]		
PSM	0.0116 (0.018)[b]	0.0116** (0.114)[c]	0.0083** (0.026)[c]
Genetic Matching	0.0148*** (0.003)[b]	0.0222*** (0.000)[c]	0.0226*** (0.000)[c]
IV treatment effect		0.0242** (0.005)[c]	0.0331*** (0.008)[c]
Period 1982-1992		**Post-matching Regressions**	
Matching technique	Non-Parametric DiD	DiD	DiD + baseline covariates
No matching	0.0098*** (0.0082)[a]		
PSM	0.0163 (0.1179)[b]	0.0163 (0.068)[c]	0.0142** (0.044)[c]
Genetic Matching	0.0097 (0.3195)[d]	0.0192*** (0.003)[b]	0.0216*** (0.001)[b]
IV treatment effect		0.0307* (0.000)[c]	0.0586*** (0.000)[c]

p-values in parentheses, ***= 99% confidence, **= 95%, * = 90%.

[a] Using non-parametric Difference in Difference standard error

[b] Using (Abadie and Imbens 2006) standard error.

[c] Derived from bootstrapped standard errors, 1000 iterations.

[d] Coefficient and p-value of ATT calculated with genetic matching weighting matrix.

Table A. 6. Summary of estimated ATT impact under different matching techniques (treatment defined at 8.2% of district area covered by the subsidy)

Period 1982-2002		**Post-matching Regressions**	
Matching technique	Non-Parametric DiD	DiD	DiD + covars 1982
No matching	0.0137** (0.006)[a]		
PSM	0.0169** (0.029)[b]	0.0169*** (0.072)[c]	0.0132*** (0.026)[c]
Genetic Matching	0.0111** (0.034)[d]	0.0188*** (0.000)[b]	0.0209*** (0.000)[b]
IV treatment effect		0.0277* (0.002)[c]	0.0522** (0.002)[c]
Period 1982-1992		**Post-matching Regressions**	
Matching technique	Non-Parametric DiD	DiD	DiD + covars 1982
No matching	0.0078 (0.008)[b]		
PSM	0.0800 (0.480)[b]	0.0800 (0.220)[c]	0.0391 (0.191)[c]
Genetic Matching	-0.001 (0.929)[b]	0.0064 (0.413)[b]	0.0077 (0.321)[b]
IV treatment effect		0.0317 (0.018)[c]	0.0673 (0.000)[c]

p-values in parentheses, ***= 99% confidence, **= 95%, * = 90%.

[a] Using non-parametric Difference in Difference standard error

[b] Using (Abadie and Imbens 2006) standard error.

[c] Derived from bootstrapped standard errors, 1000 iterations.

[d] Coefficient and p-value of ATT calculated with genetic matching weighting matrix.

Table A. 7. Econometric joint validation of instruments (pooled estimation)

Test	Stat
Underidentification test	10.478
p-val	0.005
Weak identification test (Cragg-Donald Wald F statistic)	5.016
ref Kleibergen-Paap rk Wald F Statistic	5.526
ref Stock & Yogo (2005): 10% maximal IV size	19.93
15% maximal IV size	11.59
20% maximal IV size	8.75
25% maximal IV size	7.25
Hansen J statistic (overidentification test of all instruments)	0.263
Chi-sq(1) P-val =	0.6081

Table A.8. Econometric joint validation of instruments (Change on poverty vs covariates)

Test	Stat
Underidentification test	26.129
p-val	0
Weak identification test (Cragg-Donald Wald F statistic)	6.416
ref Kleibergen-Paap rk Wald F Statistic	13.683
ref Stock & Yogo (2005): 10% maximal IV size	19.93
15% maximal IV size	11.59
20% maximal IV size	8.75
25% maximal IV size	7.25
Hansen J statistic (overidentification test of all instruments)	0.026
Chi-sq(1) P-val =	0.8729