Environment and Development Economics (2021), **26**, 151–168
doi:10.1017/S1355770X20000303

RESEARCH ARTICLE

Hidden welfare effects of tree plantations

Gustavo Anríquez Nilson,[1,2,3] Gabriela Toledo Roman,[1,3]* and
Rodrigo Arriagada Cisternas[2,3,4,5,6]

[1]Department of Agricultural Economics, Pontificia Universidad Católica de Chile, Santiago, Chile; [2]Center for Socioeconomic Impact of Environmental Policies (CESIEP); [3]Center for Intercultural and Indigenous Research (CIIR); [4]Department of Ecosystems and Environment, Pontificia Universidad Católica de Chile, Santiago, Chile; [5]Center for Applied Ecology and Sustainability (CAPES) and [6]Centre for Climate and Resilience Research, CR2, University of Chile, Santiago, Chile
*Corresponding author. E-mail: gabitoledo@gmail.com

(Submitted 16 January 2019; revised 17 February 2020; accepted 16 May 2020; first published online 6 August 2020)

Abstract
Subsidies to promote tree plantations have been questioned because of negative impacts of the forestry industry. Quantitative evidence on the socioeconomic impacts of afforestation subsidies or of tree plantations is elusive, mainly due to data scarcity. We assess the overall impact of a tree plantation subsidy in Chile, using our original 20-year panel dataset that includes small area estimates of poverty and the subsidy assignment at the census-district scale. We show that forestry subsidies – on average – in fact, do increase poverty. More specifically, using difference in difference with matching techniques, and instrumental variables approaches, we show that there is an increase of about 2 per cent in the poverty rate of treated localities. We identify employment as a causal mechanism explaining this finding, since we found a negative effect of tree plantations on employment, and therefore, on poverty. We suggest reassessment of the distributional effects of the forest subsidy and forestry industry.

Keywords: afforestation subsidies; impact evaluation; poverty; tree plantations

JEL classification: Q23; Q56; I32

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 per cent, and the global value of trade of the industry grew at an average of 6.6 per cent per annum (Food and Agriculture Organization, 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 socioeconomic 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 Food and Agriculture Organization through technical assistance and credits (Cossalter and Pye-Smith, 2003; Bull *et al.*, 2006).

The consolidation of these 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 weak institutional frameworks 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; Fonseca, 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 (Williams *et al.*, 2003; Schirmer, 2007). 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.*, 2016), but the identification of a causal link between the expansion of plantation forestry and higher poverty is still an unanswered question. In general, natural and planted forests tend to correlate spatially with poverty (Müller *et al.*, 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 a work related to the study presented here, Andersson *et al.* (2016) shows that poverty positively correlates with tree plantations in Chile. They do so by running panel regressions of poverty of 180 southern municipalities, from four years of the 2000s, on municipal control variables and the share of land area covered with tree plantations. However, without dealing explicitly with the endogeneity of poverty and tree plantations, they fail to establish causality or causal mechanisms. Causal mechanisms are key to understanding the relationship between a public policy, afforestation subsidies, and the socioeconomic conditions of local people.

As we are unaware of other studies that measure the socioeconomic 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 the socioeconomic impact of PES programs is also surprisingly scarce (Suich *et al.*, 2015), we refer here to studies that have examined poverty impacts of PES programs using impact evaluation techniques. The conclusions emerging 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 *et al.*, 2015). If these programs involve high initial pecuniary and non-pecuniary costs, and there are scale economies – usually accompanied by 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. Arriagada *et al.* (2015) also analyzed this same program and concluded that it had no measurable impact on incomes, but Robalino and Villalobos (2015) found that the program does increase wages in areas visited by tourists. The Mexican conservation PES program was reviewed by Alix-Garcia *et al.* (2015) and they found 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 that is 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) who identify positive employment opportunities in the tourism industry in Costa Rica as 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 with land use change, such as: changing employment opportunities, environmental degradation that could hinder the harvest of environmental goods and services, migration, and land redistribution, among the most salient (Angelsen and Wunder, 2003; Sunderlin *et al.*, 2008; Lambin and Meyfroidt, 2010).

We present here the results of a quantitative impact evaluation of an afforestation subsidy program on poverty. There are several reasons that make the Chilean afforestation subsidy program, studied here, internationally relevant. The Chilean Decree Law 701 (DL 701) program is presently the largest afforestation subsidy scheme of its type in the world, 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, such as Ecuador and Uruguay. Moreover, the turn of the century has seen a rise in social and ethnic conflict in the Chilean regions where the program evolved, which calls for a more scientific approach to measuring 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.

The hypothesis we test in this paper is that the broad land use change promoted by this large-scale public program increased poverty in the areas where it thrived. By converting low productivity agricultural areas into tree plantations, there was a negative employment effect, i.e., there was an employment opportunity cost: forestry created fewer jobs than those lost by the alternative activities in those same areas. Also, the lower employment generated promotes out-migration, leaving behind those poorer households that do not have the human and physical assets to facilitate out-migration. Below we prove the hypothesis that the public program did in fact cause higher poverty, and also show that the negative employment effect is likely the acting mechanism through which this public program increased poverty in southern Chile. 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 remainder of the paper is organized as follows. Section 2 describes the evaluated public program, while section 3 provides a description of data and methods used in this program evaluation. Sections 4 and 5 describe the main results in terms of poverty impact of the plantation subsidy program using non-parametric program evaluation methods and the instrumental variables approach. These results are followed by an analysis of possible causal mechanisms in section 6. Section 7 provides some concluding remarks.

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

Region	Area & value[a]	Subsidized area and funds disbursed period				Total tree plantation area	Area with subsidy support (%)
		1976–81	1982–92	1993–2002	Total 1976–2002		
Maule	Area	25,412	117,064	75,651	218,127	265,047	82.3
	US$	N/A	37,089	24,659			
Biobío	Area	124,162	160,038	77,008	361,208	437,184	82.6
	US$	N/A	63,871	31,576			
Araucanía	Area	30,752	108,571	99,836	239,158	271,230	88.2
	US$	N/A	37,526	41,672			
Los Riós & Los Lagos	Area	10,497	53,087	54,803	118,387	154,760	76.5
	US$	N/A	16,152	19,647			
Country	Area	208,654	472,572	376,027	1,057,253		
	US$	N/A	168,736	160,436			
Study area as % of country	Area	91.5	92.8	81.7			
	US$	N/A	91.6	73.3			

Source: Instituto Forestal (2009), and authors' calculations.
[a]Area is in hectares; values are in thousands of 2010 US$.

2. The afforestation subsidy program and poverty in Chile 1982–2002

In Chile, the DL 701 program, rolled out in 1974, established subsidies of up to 90 per cent of total costs as cash-back for plantations and forest management expenses on certified forest soils. These lands had to be certified by an authorized agronomist as land that could not be used for intensive agriculture or livestock rearing without soil degradation. Any landowner with soils that met these technical criteria could apply and there were no limits until 1998. However, the program unavoidably promoted concentration of resources. The large sunk costs involved, as money is returned after 18 months approximately (Arriagada and Anríquez, 2013), and the significant scale economies involved in the establishment and management of tree plantations, inevitably acted as barriers for the participation of smaller scale landholders. The program also provided additional financing for administration costs of the tree plantations, and additional funding for supplementary soil protection investments, such as dune control and infiltration ditches in high slope lands.

Another important aspect is that the private forestry industry started in earnest, together with this program. By 1976, there were about 300 thousand hectares 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 (Instituto Forestal, 2009). The study area covers the center-South regions of Chile and includes the contiguous regions of Maule, Biobío, Araucanía, Los Ríos and Los Lagos, which captured 87 per cent of the national area subsidized by the program between 1976 and 2002. Given the high level of state financing for these plantations, 83 per cent 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, the impacts must be interpreted in light of the fact that the underlying causal mechanisms are related to the forest plantations engendered by the program.

https://doi.org/10.1017/S1355770X20000303 Published online by Cambridge University Press

Table 2. Comparison of poverty rates and number of poor people in the study area and subsamples of interest

			1982[b]	1992	2002
National[a]	Total	%	52.6	32.9	18.7
		#	5,959,441	4,390,507	2,905,424
	Rural	%	55.4	33.9	20.0
		#	1,115,539	737,881	406,983
Study Area Regions[b]	Total	%	62.7	39.7	22.2
		#	1,993,006	1,428,218	857,200
	Non-treated districts	%	61.9	38.6	21.1
		#	1,906,896	1,128,586	589,678
	Treated districts	%	64.6	42.7	25.1
		#	64,942	287,442	267,522

[a] National Household Survey (CASEN) 1992, 2003, and micro simulations for 1982.
[b] Poverty micro simulations.

The international debt crisis of 1982 hit Chile particularly hard. That year the country experienced negative GDP growth of −13 per cent. However, three years later the economy started a strong growth period, strengthened later by the return to democracy and social peace in 1989. Overall, the economy grew on average 5.3 per cent over the 1982–92 decade, and 5 per cent over the 1992–2002 decade. This strong performance of the macroeconomy translated into fast poverty reduction, as shown in table 2. Nationally, poverty fell 37 per cent, from 53 to 33 per cent over the 1982–92 decade, and decreased at an even higher rate (43 per cent) over the 1992–2002 decade, from 33 to 19 per cent. Poverty rates in the study area are higher than national averages, as table 2 shows. The analysis includes the poorest region in the country, Araucanía, and coastal areas of the Maule, Biobío regions that also have among the highest poverty rates in the country.

3. Data and methods

The data comes from two main sources. First, we use a spatially-explicit database of beneficiaries at the census-district level.[1] The program's database originally only identified 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 *et al.*, 2003). Poverty maps have been used in the literature in the context of environmental policy evaluation (Sims, 2010; Gibson, 2018). Three poverty maps were prepared for this study, for the years 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.[2] Since poverty estimates – 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 were merged in databases and maps, to ensure a minimum district size. This merging of areas was also carried out by merging districts that atomized over time to reconstruct spatial aggregates

[1] A census district is smaller than a municipality, the smallest administrative unit, and corresponds to the area a census enumerating team would cover within a day.

[2] More information regarding these three different poverty maps is available in the online appendix, table A9.

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 (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 per cent of their area affected by the program, and not treated otherwise. The threshold chosen, 5.7 per cent, is approximately the mean share of district area covered by the subsidy, but given the naturally skewed distribution of the program, the treated districts are 231, or about 28 per cent of the sample. The choice of treatment cutoff is certainly arbitrary, and the effects of this choice are explored with attention below.

3.1. Difference in difference with matching

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. In this study we estimate the average treatment on the treated (ATT), 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 DiD estimator,

$$\begin{aligned}\text{DiD} \equiv E[Y(1)|t = 1, d = 1] - \; & E[Y(t = 0, d = 1] \\ - \{E[t = 1, d = 0] - \; & E[Y(t = 0, d = 0]\}.\end{aligned}$$

Put into words, the estimator is the difference of the change over time between treated and untreated. This estimator may be calculated non-parametrically with sample means, or the same estimator can be obtained parametrically estimating the following regression:

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

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:

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

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 (γ), observable and unobservable, and it also controls for time-specific shocks that affect both the treated and untreated (β). The counterfactual, however, 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 DiDs 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 referred to in different ways 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, $E[d = 1, X] = E[X]$. As highlighted by Heckman *et al.* (1997), 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 $E\,[Y(0)|d, X] = E[Y(0)|X]$; that is, a less restrictive version of the reduced 'partial' ignorability assumption, $Y(0) \perp d|X$.

In this study we use two types of matching techniques: PSM; and Genetic Matching, suggested by Diamond and Sekhon (2012). Genetic Matching is both a distance metric, a generalization of the popular Mahalanobis distance, and an algorithm which 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 a 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 impact 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.

4. 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 consider 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 *et al.*, 2015). However, here the unit of analysis is the census-district area. The afforestation subsidies promote the establishment of tree plantations in areas where land is less productive and more degraded (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 over two decades (1982 to 2002), one would expect that the poorer areas reduced poverty levels more than the better-off areas. 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 (area convergence, i.e., poorer areas growing faster than wealthier ones, has generally been confirmed in Chile (see Anríquez and Fuentes, 2001). Over longer periods of time, migration can cloud these predictions, because it is not usually the poorest who migrate – in the case of Chile, see Coeymans (1983) and Aroca and Hewings (2002) – 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.[3] Then, we include variables that describe the socioeconomic context of the district: mean years of schooling of households, household size, population density, demographic dependency, percentage of urban population, and the share of households with employment in the primary sector. Nevertheless, the most important aspect to control is land quality, because this is a major driver of participation, and also outcomes. This is why we include variables that physically describe the districts: district size, road density, distance to nearest port, and proportion of the district area with high slope, greater than 10 per cent, lands. The latter is an exogenous trait that we believe to be a reasonable proxy for unobserved land quality. Table A2 in the online appendix shows that pre-matching covariates significantly differ among treated and non-treated districts, with the treated group being poorer, less educated, with a higher percentage of rural area as well as being closer to ports, among other differences.

The results of the different estimates of the poverty impact of the Chilean afforestation subsidy program are presented in table 3. 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 per cent higher in districts that had at least 5.7 per cent of their area subsidized by 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 per cent 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 of table 3 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 covariates to the DiD regression. The use of baseline covariates (i.e., not contaminated by response to treatment) is used in randomized control trials 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 table 3). The vector of exogenous and baseline (1982) variables includes: distance to port, district area, rural population (percentage), and high

[3] A detailed description of all variables used is available in table A1 in the online appendix.

Table 3. Summary of estimated ATT impact under different matching and IV techniques

Period 1982–2002		Post-matching regressions	
ATT Identification Method	Non-Parametric DiD	DiD	DiD + baseline covariates
Simple DiD	0.0137** (0.041)[a]		
DiD with PSM matching	0.0233*** (0.006)[b]	0.0233*** (0.009)[c]	0.0216*** (0.008)[c]
DiD with Genetic Matching	0.0207*** (0.003)[d]	0.0173*** (0.000)[c]	0.0178*** (0.000)[c]
IV treatment effect		0.0299*** (0.009)[c]	0.039** (0.011)[c]
Period 1982–1992		Post-matching regressions	
ATT Identification Method	Non-Parametric DiD	DiD	DiD + baseline covariates
Simple DiD	0.0134 (0.122)[a]		
DiD with PSM matching	0.0165* (0.084)[b]	0.0165** (0.025)[c]	0.0167** (0.024)[c]
DiD with Genetic Matching	0.0144* (0.068)[d]	0.0187** (0.001)[c]	0.0146*** (0.000)[c]
IV treatment effect		0.0414*** (0.001)[c]	0.0525*** (0.000)[c]

Notes: p-values in parentheses. *** = 99% confidence, ** = 95%, * = 90%.
Genetic matching estimates obtained using the 'R' package available from Sekhon (2011).
[a] Using non-parametric difference in difference standard error.
[b] Using Abadie and Imbens (2006) standard error.
[c] Derived from bootstrapped standard errors, 1,000 iterations.
[d] Coefficient and *p*-value of ATT calculated with genetic matching weighting matrix.

slope area (percentage). The post matching covariate balance tables are presented in the online appendix (tables A3 and A4), together with the overall balancing tests (table A5). 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. In the case of PSM, all but one variance were balanced. In spite of this, normalized differences (by the root of sum of variances) were all within a tight 7 per cent bound, which is why overall balancing tests were handily passed. Overall the PSM achieved slightly lower normalized differences.

Remarkably, all estimators indicate that the ATT is significant, and it moves within a narrow window between 1.7 and 2.3 per cent.[4] The second panel of table 3 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, since the rotation of plantations takes at least 12 years and can last 25 years. Furthermore, the number of treated districts is lower, as the program has been in constant expansion,

[4] Although not shown in table 3, following the advice of an anonymous referee, we also test if there is a significant ATT on FGT(1) and FGT(2). In both cases there is a significantly positive ATT (lower, of course). These results suggest that the program also has a distributional effect that needs to be further studied.

https://doi.org/10.1017/S1355770X20000303 Published online by Cambridge University Press

which reduces the sample size and precision of the estimated 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 3 estimates ATT with treatment defined at 5.7 per cent of district area covered, we explore ATT results if treatment was defined at 3.2 and 8.2 per cent of area covered, i.e., ± 2.5 per cent, in the online appendix tables A6 and A7. 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 per cent of area covered by subsidy. With treatment defined at 3.2 per cent of area, the ATT estimates range between 1.4 and 2.3 per cent, while ATT estimates moves between 1.4 and 2.1 per cent when treatment is defined at 8.2 per cent of area.

The question of unobserved traits biasing results, however, lingers. Of particular concern is unobserved land quality, which could be driving both high participation in the program and poor performance in poverty alleviation. Although we obviously cannot observe the unobserved, we can assess how much bias driven by unobserved traits would be necessary to render estimated impacts insignificant. This is, in essence, the approach proposed by Rosenbaum's (2002) sensitivity to hidden bias analysis. Rosenbaum's sensitivity analysis is built on the parameter Γ, which is the log of the odds ratio between matched pairs that is due to unobserved bias. For example, when $\Gamma = 1$, there is no unobservable bias, but if $\Gamma = 1.3$, then hidden bias explains a 30 per cent differential in the odds of being assigned into treatment. As Rosenbaum (2002: 107) suggests, 'Γ is a measure of the degree of departure from a study that is free of hidden bias'. Using as the benchmark the PSM ATT estimates (row 2 in table 3), the sensitivity analysis suggests that at about $\Gamma = 1.5$ ($\Gamma = 1.6$), the upper bound of the significance level reaches the 5 per cent (10 per cent) benchmark. Alternatively, at about $\Gamma = 1.5$ ($\Gamma = 1.6$), the lower bound of the confidence interval of the estimated ATT reaches 0 at the 5 per cent (10 per cent) significance level. Thus, the estimated ATT is reasonably robust to hidden bias for an economic/environmental observational study.

5. An instrumental variables approach to measuring 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, such as: 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 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:

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

$$y_1 = \mu_1 + g_1(x) + u_1, \tag{4}$$

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where μs are parameters, $g_i(x)$ 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:

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

We follow Wooldridge (2010) and assume that $g_0(x) \equiv x\beta_0$, and that $g_1(x) - g_1(x) \equiv [x - m_x]\delta$, where m_x is the vector of means (expected values), and δ is 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.[5] Under these assumptions we have the following econometric specification:

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

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 the 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 $(1, x, d, d[x - m_x])$ and the vector of instruments $(1, x, \hat{d}, \hat{d}[x - m_x])$, 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 misspecification 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 with (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 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; that is, $(u_1 - u_0) \perp d$ does not hold – what the literature calls 'unobservable heterogeneity' or 'essential heterogeneity'. Heckman *et al.* (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: $E[x, z] = a + b\ p(x, z)$, with parameters a, b and propensity score $p(x, z)$. Standard tests on the significance of higher order polynomials coefficients indicate 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 Wooldridge (2010) suggests that misspecification of the treatment probability model is not crucial for this IV estimation technique, we believe we have a good vector of instruments, which we test. Both distance to the pulp mill as well as rainfall

[5] Another possibility is to follow the suggestion of Heckman *et al.* (2006) and estimate $d1-0x$.

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 where 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. That is, means difference tests fail to reject the null hypothesis that poverty is equivalent in areas closer to and farther from the mills, and between areas with higher and lower precipitation. We are on the right track, but these are not formal IV tests. In table 4, using a reduced form model for poverty, we test the validity of instruments. In the first column, we explore the plausibility of the reduced form model, which explains poverty with a set of standard correlates, mean income, and it dispersion (Theil Index), socio-demographic indicators (schooling, household size, employment in the primary sector, population density and urban population), area bio-physical characteristics (district size, road density, distance to nearest port, and high slope area), and municipality and time fixed-effects. Ignoring the known endogeneity of the first regressor, district subsidized area (percentage), the rest of the explanatory variables have signs and significance as expected. However, we do not have instruments for this panel data model, because new wood pulp mills were constructed during the 1990s, which means that we can no longer ascertain that the changing (over time) distance to pulp mills is exogenous to poverty.

Hence, we take the first difference of poverty by district, and estimate a cross-section reduced form model, where all regressors are at their baseline levels (table 4, column 2). Since we have two instruments, we test for obvious violations of the assumption $(z, v) = 0$, with the Sargan-Hansen overidentifying restriction test, which is not rejected with p-values in the 0.65–0.34 range (columns 3 and 4 respectively). 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 Kleibergen (2007) F-statistic from the first stage, and compare it to the Stock and Yogo (2005) IV-Bias tables to assess the maximum potential bias imposed by our chosen instruments. We find that the instrument vector is reasonably strong: the Wald tests on parameter significance are off by only 10–15 per cent. For example, the 5 per cent 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 per cent rejection rate, the 'true' critical level would be at most 4.42 (3.84×1.15). The implicit Wald statistic, estimated to be above 19.4 for the subsidized area in table 4 (columns 3 and 4), handily surpass this more stringent critical level. In conclusion, instruments are sufficiently strong, and results are very strong (complete IV test results are included in the online appendix, table A8).

The impact of district area subsidized by the afforestation program on poverty, shown in table 4, does not identify any traditional treatment effect of the program evaluation literature, but rather a marginal effect. For example, the coefficient in the third column shows that increasing area subsidized by 1 per cent would on average raise the change (fall) in poverty between 1982 and 2002 by 3.1 per cent. Moreover, the results in table 4 are meaningful and key to validating the proposed IV approach. 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

Table 4. IV estimates of the impact of subsidized afforested area and poverty

	Panel	Cross section		
	District poverty	Poverty change, 1982–2002		
Variables	OLS	OLS	IV 2SLS	IV 2SLS
District area subsidized	0.000193** (8.37×10^{-5})	3.72×10^{-5} (0.000371)	0.0310*** (0.00703)	0.0614*** (0.0131)
Per capita income (spatial lag)	−0.00771 (0.00577)	−0.00847 (0.0104)	−0.0282 (0.0183)	−0.0580* (0.0336)
Theil Income Inequality Index	0.0642* (0.0346)	−0.0865** (0.0359)	−0.0667 (0.0676)	0.00396 (0.131)
Years of schooling HH	−0.0411*** (0.00215)	0.00849** (0.00346)	0.00633 (0.00729)	−0.00342 (0.0155)
Household size	0.0200* (0.0103)	−0.00739 (0.00800)	−0.0599*** (0.0204)	−0.102*** (0.0358)
HH work in primary sector	−0.0566*** (0.0180)	−0.0305 (0.0366)	−0.0222 (0.0635)	−0.00318 (0.101)
Population density	−0.000101** (4.36×10^{-5})	-3.59×10^{-5} (0.000109)	0.000280 (0.000231)	0.000395 (0.000476)
Demographic dependency	0.107 (0.107)	−0.159 (0.106)	−0.397 (0.329)	−0.951 (0.639)
% urban	0.151*** (0.00798)	−0.107*** (0.0157)	−0.123*** (0.0262)	−0.0457 (0.0435)
Constant	0.660*** (0.0899)	−0.126 (0.128)	0.350 (0.316)	1.098* (0.600)
Biophysical characteristics	Yes	Yes	Yes	Yes
Fixed effects	Municipal			
Year effects	Yes			
Observations	2,502	834	834	834
R^2	0.928	0.242		

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

of binary treatment allocation and estimates that, given sample averages, increases in area afforested by the program increase poverty.

Having validated the proposed instrument vector, we estimate the model presented in (6), which estimates a DiD IV ATT estimator that is presented in the fourth row of table 3. The instruments used are all the variables included in table 4, plus the excluded instruments, rainfall and pulp mill distance. In the second column in the IV row of table 3, only the program participation binary is used, i.e., we impose that $\hat{\delta} = 0$ in (6), and there is no treatment heterogeneity. Additionally, in the third column we allow for treatment heterogeneity, adding as the covariates vector, the same exogenous baseline covariates used in the non-parametric approaches above; that is, distance to port, district area, rural population, and high slope 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., $\{d[x - m_x]\hat{\delta}\}_{d=1}$ in equation (6). When considering the whole period, 1982–2002, the results presented in table 3 show

https://doi.org/10.1017/S1355770X20000303 Published online by Cambridge University Press

that the IV approach provides consistent (with non-parametric approaches) estimators of ATT, slightly higher impact in levels and similarly strong significance levels.

6. Discussion: identifying causal mechanisms

Although the econometric evidence seems incontrovertible, it is worthwhile to assess if the result of increasing poverty caused by this tree plantations program is consistent with the qualitative evidence. What were the alternative development paths of areas equally suitable for program treatment but that did not receive an intensive application of the program? We interviewed industry experts and members of the Chilean Forestry Service, and they agreed that the results we presented were credible. The anecdotal evidence suggests that in equally suitable areas that did not receive an intensive treatment of tree plantations, people's livelihoods depend on mixed strategies of agriculture, forestry and low-density extensive livestock rearing. While these mixed livelihood strategies probably generate less income per area than intensive forestry, they allow more people to escape poverty. Tourism and associated services is a development path in some very specific locations, but is not generally an alternative for areas near ports where most treated and control areas are located, which would explain why we do not find the type of positive relation between Protected Areas and income reported by Robalino and Villalobos (2015).

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. In the following, we 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.

First, we analyze the direct impact on the beneficiaries. 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 other non-pecuniary transaction costs. The tree plantations, on the other hand, may have a direct socioeconomic impact, by creating employment, both in the plantations themselves, and on up- and down-stream activities.

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 tree plantations grow. This net effect need 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 clearly to employment outcomes, migration and out-migration – in the case of these rural communities we are focusing on – can have an effect on 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 the poorest do not have the wealth to invest in the pecuniary and non-pecuniary costs associated with migration. Thus, increased

Table 5. 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	−0.0305 (0.131)	−0.0305** (0.028)	−0.0303** (0.023)
Employment rate (total population)			
Baseline – No matching	−0.067*** (0.00)		
PSM	−0.0202* (0.123)	−0.0202** (0.046)	−0.0184** (0.049)

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

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 (NTFPs), and the poor have open or partial access to these resources. On the contrary, Chilean tree plantations, which are mostly mono-cultures of *pinus radiata* and *eucalyptus globulus* are not rich in NTFPs; and furthermore, tree plantations strengthen private land rights, which tends to reduce the area of open access resources for NTFPs 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 per cent of district area with subsidized tree plantations, and we use as previously the DiD estimator, and the more suitable Did with matching estimator. Table 5 shows the estimated impacts of the program on the employment indicators. Table 5 shows that both employment of heads of household, as well as the total employment rate, ended up about 2–3 per cent 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 per cent fewer 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 with the reduced, municipal-level sample size.

7. Conclusions

The Chilean afforestation subsidy program is one of the oldest and largest programs of its type. This paper shows that this subsidy has caused increased poverty in the areas where the subsidized tree plantations were established. We show, using a battery of different econometric empirical strategies, that this result is very robust, with estimates of increased poverty by the program in treated districts between 2 and 3 per cent. This is a remarkably small variability as we explore completely different parametric, non-parametric and IV approaches. For comparison, Andersson *et al.* (2016) estimate that a 1 per cent increase in tree plantations increases poverty by 0.3 per cent, given poverty during their study period (2001–2011) and area of 18.5 per cent, which means that these authors implicitly estimated a poverty-to-tree-plantation-area elasticity of 0.016. This elasticity appears to be lower than the number we obtain; a discrete change from non-treated to treated increase poverty by 2 per cent, given poverty centered around 40 per cent over this study's period and area, it implies a percentage increase in poverty of 0.05.

We also show 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 through which poverty increases, but data limitations that reduce our sample hinder a definite answer on this mechanism.

This paper has identified an often-ignored negative socioeconomic impact of this afforestation subsidy, which calls for an assessment of the scope and implications of these results. First, there are likely additional negative impacts, such as environmental externalities, monocultures that may have a negative effect on biodiversity, water availability, and other ecosystem services, making local people vulnerable to fires and other events. On the other hand, these results raise the need for a cost-benefit analysis of the whole program. Simple 'back of the envelope' calculations would show that these socioeconomic 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 per cent, which is the net forest additionality of the program estimated by Arriagada and Anríquez (2013). To get an idea of the contribution of the sector to the country's development, consider that over the period of this study forest sector exports grew at a stunning 11 per cent mean annual growth rate. Moreover, while in 1982, forest exports accounted for 9 per cent of the Chilean export basket, by 2002 they accounted for 12.5 per cent. Nonetheless, those 2 per cent additional poor live in low poverty density areas, so they are not many, but are among the poorest in the country, as the poorest and the forest coincide in space. Governments following this afforestation subsidy scheme should consider focalized compensatory policies for those already affected. Cash transfers are a viable alternative, and these can be funded with little distortion with a very small, externality-correcting tax on forest products exports. Also, when cash transfers are not a viable alternative due to institutional limitations and in areas with sufficient population density, guaranteed employment programs such as India's National Rural Employment Guarantee Act (NREGA) program are also a viable alternative.

The negative socioeconomic impacts uncovered by this study are probably behind social conflicts with the forestry industry that evolved in the period following the study period considered in this research. 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 a larger and more robust impact occurs with greater portions of territory with plantations. Countries currently following the Chilean model, such as Uruguay and Ecuador, should seriously consider the distribution aspect of fostering plantations. Finally, the sustained worldwide expansion of tree plantations calls for complementary systematic evidence on other highly planted territories.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/S1355770X20000303.

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Cite this article: Anríquez Nilson G, Toledo Roman G, Arriagada Cisternas R (2021). Hidden welfare effects of tree plantations. *Environment and Development Economics* **26**, 151–168. https://doi.org/10.1017/S1355770X20000303