Direct and spillover effects of productive development programs

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PhD in Economics Thesis

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April 2020

Universidad de San Andrés

Chapters

I. The Causal Effects of Regional Industrial Policies on Employment: A Synthetic Control Approach*

Abstract

Industrial policies affecting entire sectors in regions, provinces, or districts can account for large portions of sub-national government spending. Yet because of the methodological challenges related to the identification of a counterfactual when a single unit is treated, the causal effects of these policies on the growth of the industry, or specifically on employment, are seldom identified. We adopt a Synthetic Control Method (SCM) approach to analyze the long-term impact on employment of the Tourism Development Policy (TDP) implemented by the Argentinean province of Salta. We find an 11 percent average annual impact over 10 years on employment in the hospitality sector, which translated in an accumulated impact of 1,376 formal jobs in the tourism value-chain. We also find that this growth did not happen at the expenses of other industries and that TDP generated positive inter-industry employment spillovers/externalities. For each job created in the tourism value-chain, an additional job was created in the rest of the provincial economy, which resulted in a total creation of 2,750 formal jobs. Our results are robust across a series of placebo tests and sensitivity checks and are consistent among alternative synthetic control units.

JEL Classification: C81, E24, H40, J48, O25, R58.

Keywords: Regional industrial policy, employment, synthetic control method, policy evaluation, tourism.

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*The final version of this study was published in Regional Science and Urban Economics (2017) and coauthored by Victoria Castillo (OEDE), Lucas Figal Garone (UdeSA-IDB), Alessandro Maffioli (IDB), and Lina Salazar (IDB). We wish to thank Alberto Abadie, Sergio Ardila, María Laura Lanzalot, Héctor Malarin, David McKenzie, Oscar Mitnik, Giovanni Peri, Carlo Pietrobelli, Ricardo Quiroga, Maria F. Rodrigo Lopez, Stuart Rosenthal, Ernesto Stein, two anonymous referees and the participants at seminars at the Inter-American Development Bank, the Argentinean Ministry of Labor, Employment, and Social Security, the Workshop on Impact Evaluation in Practice in Santiago-Chile (CEIP 2014), and the Workshop on Impact Evaluation of Productive Development Policies in Peru 2015 for useful comments. We would to especially thank Martin Rossi and Rodolfo Stucchi for their guidance throughout the research. We are also grateful to the Ministry of Culture and Tourism of the Province of Salta, especially to Ana Cornejo. The usual disclaimers apply. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors. The views expressed in this paper are the authors' and do not represent those of the Inter-American Development Bank, the Inter-American Investment Corporation, the Argentinean Ministry of Labor, Employment, and Social Security or the Ministry of Culture and Tourism of the Province of Salta.

1 Introduction

In the last decades, industrial policy has been absent from the economic policy debate. Memories of failed import substitution policies, with disappointing consequences for public finance, kept policy-makers from even contemplating industrial policy as a viable option. However, since the global crisis of 2008-2009, interest in industrial policy has re-emerged in developed and developing economies alike, particularly at the sub-national level. Given the outstanding results achieved by the early Asian Tigers of South Korea and Taiwan, industrial policies have not only been reconsidered, but even advocated by scholars such as Philippe Aghion, Ricardo Hausmann, Dani Rodrik, and Joseph Stiglitz (Aghion *et al.*, 2011; Hausmann & Rodrik, 2006; Rodrik, 2004; Stiglitz *et al.*, 2013). And when, in 2010, the free-market champion and former EU Commissioner for Competition Policy Mario Monti stated that "Industrial policy is no longer taboo",[1] it was clear that policy-makers had altered their perspective as well. Industrial policy was back on the public policy agenda.[2]

Policies focused on local production systems, industrial districts, networks, clusters, and regional innovation systems, with a strong emphasis on improving regional competitive advantage, have emerged as a new style of policy-making. Due to dramatic job losses after the crisis of 2008-2009, the United States and Europe introduced measures to support strategic industries (Kline & Moretti, 2013).[3] An increasing number of developing countries, particularly in Latin America, have also introduced strategic development plans targeting specific industries in certain regions,[4] and programs to support industry clusters and value chains focusing on specific local industries (Crespi *et al.*, 2014; Maffioli *et al.*, 2016).

Like other large-scale economic policies, industrial policies are often implemented at the regional or provincial level and determine how significant portions of federal and/or sub-national government budgets are allocated. This is because policy-makers view regional industrial policies (RIPs) as important instruments to boost job creation and productivity-based growth.

In terms of job creation, three questions are particularly relevant in the context of RIPs: (i) What is the causal effect of RIPs on employment of the target region-industry?; (ii) Does the increase in employment in the target region-industry due to the RIPs come at the expenses of other industries or as an increase of total (regional) employment?; and, (iii) Does the RIP generate positive inter-industry employment spillovers/externalities i.e. the increase in total employment is larger than the increase in the employment in the target region-industry? To date, however, little empirical

[1] See "The Global Revival of Industrial Policy: Picking Winner, Saving Losers." *The Economist* (Aug 5, 2010).

[2] For the purpose of this study, "industrial policy" is a policy that directs public investments to specific industries in a given economy.

[3] For instance, the U.S. government and many individual state governments have spent roughly \$95 billion a year on regional development policies targeting specific industries (Kline & Moretti, 2013).

[4] See, for example, the Sector Funds Program in Brazil, the experience of CORFO and SERCOTEC in Chile, and the initiatives introduced in Mexico by the CONACYT and in Argentina by the MINCYT.

evidence has been produced to answer these questions, and few studies have properly dealt with the methodological challenges related to the identification of the causal effects of RIPs.

Three issues make the evaluation of RIPs particularly challenging. First, RIPs are usually implemented at the aggregate level, affecting a single industry within a region, province, or district. This implies that all individuals or firms that belong to or are related to the treated industry within the government's zone of influence are in some way affected by the intervention. Second, RIPs often target high-growth-potential industries, which are also commonly characterized by externalities and agglomeration economies, making indirect effects an important issue to be considered when estimating a proper counterfactual (Angelucci & Di Maro, 2016). Finally, RIPs usually comprise a bundle of policy instruments, including business support, tax incentives, infrastructure development, and institutional strengthening. These intrinsic characteristics of the RIP often leave the researcher with only one (aggregate) treated unit. In this context, pure time series or before-after analysis of the impacts would be clearly contaminated by changes other than those induced by the RIPs.

To answer the aforementioned questions and address the empirical challenges, this paper proposes the application of the synthetic control method (SCM) approach to identify the causal effects of a RIP. As a case study, we examine the Tourism Development Policy (TDP) implemented in the Argentinean province of Salta. The SCM, developed by Abadie & Gardeazabal (2003) and extended in Abadie *et al.* (2010), is an econometric technique used to devise data-driven comparative case studies. Specifically, we use a combination of other Argentinean provinces to construct a "synthetic" control that resembles Salta's tourism industry before the TDP and produces a counterfactual of what would happened in the absence of the TDP.

The TDP case is a relevant for two reasons. First, the government of Salta designed the TDP to boost job creation in the province. Second, the TDP followed an integrated, large-scale approach to tourism development that included upgrading tourism and transport infrastructure, restoring cultural heritage, strengthening institutions, and launching national and international promotional campaigns. The plan required public-private partnerships and a long-term commitment by the provincial government.

For our analysis we use data from 1996 to 2013 consisting of monthly information on different economic sectors at the provincial level. The data enable identification of the effects of the TDP in a ten-year window following its implementation and, more importantly, the creation of a counterfactual based on eight pretreatment years. The data also allow us to control for relevant confounders and seasonality and enable us to implement a battery of placebo studies and robustness checks.

Our main results show that, after the TDP was implemented, employment in the hospitality sector in Salta increased by an average of 11 percent per year, for an overall impact of around 114 percent (750 new formal jobs), between 2003 and 2013. When considering the tourism value-chain (including the hospitality sector), employment increased by an average of 2.2 percent per year i.e. an accumulated impact of 1,376 formal jobs. Additional analyses show that the TDP not only did not crowd-out employment in other industries but also generated positive inter-industry employment spillovers/externalities. We find that for each job created in the tourism value-chain, an additional job was created in the rest of the provincial economy, which resulted in a total increase of 3,750 new formal jobs due to the TDP. These results are robust across a series of placebo tests, robustness checks and different synthetic control groups.

This paper contributes to the existing literature in several ways. First, to the best of our knowledge, this is the first paper that examines the long-term causal effects of a large-scale RIP with only one treatment unit. The closest studies are related to a broader literature that evaluates business support policies and place-based interventions.[5] This contribution is particularly relevant to the debate on the effectiveness of tourism policy.[6]

Second, this paper is also among the first applications of SCM to assess the impact of an economic development policy.[7] Until now, SCM has been used to evaluate the effect of the introduction of reforms, events, and specific policies.[8] SCM and the exhaustive empirical exercises presented in Annex C can be very useful for the evaluation of a variety of policies with dual focus (location and industry), such as other RIPs, cluster development programs, value chain programs, and other regional and urban development policies and reforms.

Finally, the study contributes to the debate on the design of tourism policies in developing countries. As pointed out by Crotti & Misrahi (2015), identifying priorities, upgrading infrastructure, calibrating fiscal incentives and executing national and international marketing campaigns are among the key tasks necessary to succeed in developing the tourism industry. The TDP offers a successful case study of this integrated approach.

The rest of the paper is organized as follows. Section 2 discusses the rationale behind tourism policies, the background of the TDP, and a simple framework to motivate our empirical analysis. Section 3 presents the empirical methodology, and Section 4 describes the dataset and the sample. Section 5 presents the results. This section is followed by a set of placebo and robustness tests in Section 6. Section 7 explores other characteristics of the impact of the TDP, and Section 8 concludes.

[5] See, for instance, Criscuolo *et al.* (2012); Freedman (2015); Kline & Moretti (2013); Romero (2009). For a detailed review and analysis of place-based policies see Neumark & Simpson (2015) and the references cited therein.

[6] The few studies that have attempted to identify impacts in this area use simulation models (Ashley & Mitchell, 2009). These approaches, however, do not directly address causality and often fail to provide convincing evidence of the policy's net effects.

[7] Gathani *et al.* (2013) and Barone *et al.* (2016) are probably the studies closest to an application of SCM to an economic development policy.

[8] California's tobacco control program (Abadie *et al.*, 2010), trade restrictions (Garcia Lembergman *et al.*, 2015), a mileage tax for trucks (Luechinger & Roth, 2016), economic liberalization processes (Billmeier & Nannicini, 2013), terrorist conflicts and crime (Abadie & Gardeazabal, 2003; Gautier *et al.*, 2009; Pinotti, 2015), catastrophic natural disasters (Barone & Mocetti, 2014; Cavallo *et al.*, 2013), German reunification (Abadie *et al.*, 2015), energy policies (Ando, 2015; Munasib & Rickman, 2015) and childcare (Bassok *et al.*, 2014), and spillovers from universities (Bonander *et al.*, 2016; Liu, 2015).

2 Background

2.1 Tourism, employment, and policy justification

Tourism is one of the world's largest industries, particularly in terms of employment. According to the World Tourism Organization (WTO), in 2013, the tourism industry provided one out of every 11 jobs in the world, represented 9 percent of the world's GDP (direct, indirect, and induced impact), and generated 6 percent of the world's exports (WTO, 2014b). Annual international tourist arrivals worldwide jumped from 25 million in 1950 to more than one billion in 2013. Also in 2013, international arrivals in developing countries outnumbered those in developed economies.

Although tourism has always been considered a significant contributor to growth and economic development,[9] expanding tourism is not a development objective per se. The benefits of expanding this industry come from its positive impacts on foreign exchange earnings through tourism receipts, economic growth, and job creation (Scheyvens, 2012).

One of the main reasons of interest towards tourism in developing countries is that it generates both formal and informal employment (Sinclair, 1998). Expanding the tourism sector creates three types of employment−direct, indirect, and induced.[10] Tourism is a diverse and labor-intensive industry, and thus an effective generator of a wide range of employment opportunities (Telfer & Sharpley, 2015). Furthermore, tourism employs more women, young people, and people with low educational attainment than most industries, fostering an environment of inclusiveness and empowerment for vulnerable groups (UNDP, 2011). In addition, given its low barriers to entry, tourism provides investment opportunities for entrepreneurs to start small-scale firms and hire workers.

Despite the substantial positive effects of tourism on employment creation, economic growth, and foreign currency receipts, the sector has only recently gained relevance in the public policy debate (Hawkins & Mann, 2007; OECD, 2010). Thus, an important question to be addressed is, to what extent public intervention to promote tourism is justified.

As pointed out by Winters *et al.* (2013), the justification for public intervention in tourism is twofold. First, the economic benefits of tourism are unlikely to be realized at a socially optimal level if investment is left solely to the private sector. In fact, because of geographic proximity and industry complementarities, agglomeration economies and externalities are prevalent in the tourism industry.[11] Under such conditions, investment

[9] Abundant work in economics has emphasized the link between tourism, growth, and economic development. See Sharpley & Telfer (2014) on theoretical and empirical research in this literature.

[10] Direct employment is related to direct expenditure on goods and services by tourists. It refers to employment in hotels, restaurants, transportation, and tour operators, among others. Indirect employment refers to jobs created in sectors that provide goods and services to affected firms (backward linkages), such as food suppliers, merchants, and mechanics. Induced employment refers to the additional jobs resulting from the effects of the tourism multiplier, i.e., from spending the income earned by tourism business owners and employees outside the tourism industry (Dwyer *et al.*, 2004b).

[11] By definition, the tourism industry is geographically concentrated because of its dependence on

decisions become interrelated, and the profitability of a particular investment becomes a function of other complementary investments.[12] Without proper coordination among investors, the market would fail to assign resources optimally.[13]

Second, public intervention in tourism has been justified from a poverty-alleviation perspective. Particularly, as mentioned above, local tourism policy can be used as an important instrument to boost job creation. Many developing countries are endowed with natural, cultural, and historical resources that, with proper coordination and planning, can form the core of a profitable and sustainable tourism industry, generating jobs and incomes for the local population (Scheyvens, 2002).[14]

Other types of market imperfections, such as labor market frictions, can also justify regional or local tourism policies. As pointed out by Neumark & Simpson (2015), one of these imperfections is the spatial mismatch that generates mobility constraints, particularly for low-skilled workers.

2.2 Salta's Tourism Development Policy

Following the economic collapse of 2001, the Argentinean tourism industry gained relevance. The steep devaluation of the peso was expected to increase both domestic and international tourism, as it significantly reduced the cost of Argentinean destinations relative to international locations. Under this assumption, the forecasts for medium and long-term growth in tourist arrivals in the early 2000s were overly optimistic.

In this context, the government of Salta, a province in the northwest of Argentina (see Annex B), decided to implement a set of policy interventions to support tourism expansion, which together comprised the Salta's Tourism Development Policy (TDP). The expansion of Salta's tourism industry was expected to contribute to the revitalization of the post-crisis economy and boost local employment. The TDP was launched in June 2003 with the approval of the first loan for tourism development received by the province from a multilateral organization.

The TDP was designed and implemented as a coordinated set of interventions meant to produce a structural change in the tourism industry. The investments were made gradually over the 2003-2010 period and required a high degree of coordination and collaboration frameworks that fostered public-private partnerships.

the natural or cultural attractions of a specific area. In addition, the strong complementarities among services and products boost the effects of externalities, making coordination among local agents even more important.

[12] On this topic see the seminal work by Rosenstein-Rodan (1943).

[13] For instance, hotel owners may underinvest in accommodation capacity knowing that returns on their investment depend on the investment decisions of restaurant owners and other local investors in recreational activities. Similarly, public investment in complementary infrastructure, such as roads, water and sanitation, and public lighting, may also be hampered by the lack of coordination with the private investment needed to generate an adequate flow of visitors. For a review on coordination problems in development, see Hoff (2000). On clusters and coordination failures, see also Rodriguez-Clare *et al.* (2005).

[14] There is broad consensus regarding tourism's potential to alleviate poverty, particularly in developing countries (see, for example, Ashley & Mitchell, 2009; Scheyvens, 2012).

The TDP was based on three pillars. The first was the construction and modernization of tourism and transport infrastructure, including highways to access Salta City and the main tourist destinations, an international airport, and bus terminals, as well as the restoration of the province's historical and cultural heritage.

The second pillar consisted of tax credits for the construction, expansion, and remodeling of hotels and other lodging establishments. The availability of new accommodations, resorts, and other tourism facilities gave the province a competitive advantage. This policy instrument was instrumental in meeting the growing demand for lodging. It also created a conducive environment for firms wishing to do business in this sector.

The third pillar was institutional strengthening, including additional funding for the Tourism Secretariat, the creation of a public-private Provincial Tourism Council, and the launch of an integrated national and international promotion campaign. By making clear that the sector was a high priority, the government could channel funds to the TDP and coordinate the actors and resources necessary to develop the industry. The public-private synergies proved pivotal, as they funded the integrated policy.

Finally, a fundamental feature of the TDP was its partnership with the Inter-American Development Bank (IDB), which provided the first multilateral loan to the Province of Salta in support of a specific industry. The IDB's involvement was a turning point for Salta's tourism policy, because it provided funding for key components of the TDP and it made a long-term commitment to support the development of the provincial tourism industry.

2.3 A simple framework and expected impact

The TDP was designed and implemented as a coordinated set of interventions in the tourism industry. As such, the program aimed at simultaneously boosting the demand and expanding the supply of tourism services in Salta, with the final goal of creating new employment opportunities.

For this reason, we focus our analysis on the TDP's effects on employment. We first look at the effect on employment in the hospitality sector, which includes hotels, campgrounds, and other establishments providing lodging. We prioritize this measure because hospitality is the most representative sector of the tourism industry and, thus, the one that could more clearly reflect a structural change induced by the TDP (WTO, 2014a).

The conceptual framework for the interpretation of the TDP's impact must therefore consider both the demand and the supply sides of the labor market. Following Hamermesh (1986, 1993) and Kadiyali & Kosova (2013), we can describe the labor demand as a function of the wage rate, non-labor input prices, the price of outputs, the average industry-specific level of technological/production efficiency, and output demand shifters. In a context of tourism industry expansion, these output demand shifters are mainly the number of visitors, the average daily expenditure per tourist, the average number of overnight stays per tourist, and other aggregate demand shocks. On the supply side, the labor supply can be defined as a function of the wage rate, the level of labor mobility, the level of human capital, and other aggregate shocks.

The TDP was designed to activate various shifters of the demand of labor in the tourism industry. Through the infrastructure upgrade and the promotional campaigns, the TDP was expected to increase the number of visitors of the province (extensive margin). In addition, the TDP aimed at increasing the value of the tourism-related public goods, recreation activities, and natural and cultural heritage attractions. This should lead to the growth of both daily tourism expenditure and number of overnight stays (intensive margin). Finally, the TDP had also the objective to foster the supply of tourism services through the provision of fiscal incentives for the construction, expansion, and remodeling of hotels and other establishments and through a series of coordination activities. All these elements were expected to produce a significant increase in the labor demand by the tourism industry.

Despite its focus on tourism, the TDP was meant to boost the overall employment of the province, beyond the tourism and its related industries. That is, the expectation was that the increased demand and supply of tourism services would have benefited other local industries, either by direct and indirect spending or via multiplier effects, with limited or more than compensated crowding-out effects (Gretton, 2013; Kadiyali & Kosova, 2013; Vanhove, 2005). These are potential negative effects that may take place in the presence of significant factor supply constraints of labor, capital, and land (Banerjee *et al.*, 2015; Buiter, 1976).

In terms of employment, potential negative effects might occur if the increased labor demand in the tourism sector results in higher wages and ends up diverting supply of labor from other sectors. In that case, tourism employment would grow at the expenses of a reduction in employment in other industries and would be accompanied by a general increase in wages (Todaro, 1969). However, because of the minimum wage regulation applied to all industries and the high unemployment in Salta, the increased labor demand in tourism is unlikely to cause significant pressure on wages and a consequent diversion of labor supply from other sectors.[15] In this context, the increased tourism labor demand should more likely result in a reduction of the general unemployment without significant negative effect on other industries' employment.

Similarly, we can also expect that the positive effects from increased demand of output from other sectors dominate any potential negative effects due to the pressure on other input prices (i.e. cost of capital or land), or the reduced competitiveness in export and import-competing markets through exchange rate appreciation. The former expectation is consistent with the Salta's economy being characterized by low capital intensity and high land availability. The latter with the reduced influence that Salta's tourism inflows can have on the exchange rate and the competitive devaluation that was taking place at that time in Argentina. As a result, the TDP should result in a significant overall increase in employment, above and beyond the tourism industry.[16]

[15] In Salta, the unemployment rate was around 30 percent of the economically active population (Argentina National Population, Households, and Dwelling Census, 2001), and the labor informality rate was around 50 percent in 2001-2002 (Ministry of the Interior and Transportation).

[16] As pointed out by Banerjee *et al.* (2015), to assess the net impact of tourism investment, country and region contexts are critical, especially the consideration of factor supply constraints, domestic

In addition, as pointed out by Moretti (2011), "big push"-type policies, such as the TDP, have the potential to start an agglomeration process that can ultimately shift a certain regional or provincial industry from a bad equilibrium (small agglomeration, low productivity, low employment) to a good equilibrium (large agglomeration, high productivity, high employment). In other words, the TDP could have substantial and long-lasting effects on the equilibrium level of tourism activity and employment in Salta.

3 Identification Strategy

As mentioned in Section 1, the identification of the impacts of the TDP is challenging. Pure time series or before-after analysis of the impacts would be contaminated by changes other than those induced by the TDP. To address this challenge, we use a SCM, an empirical approach developed by Abadie & Gardeazabal (2003) and extended in Abadie *et al.* (2010). A synthetic control is a weighted average of the available control units, constructed to approximate the most relevant characteristics of the treated one. In our case, the SCM is used to estimate the counterfactual situation of Salta in the absence of the TDP by looking at the tourism employment trend in an artificial province (i.e., synthetic Salta).

We observe $J+1$ provinces over T periods. Among these, only Salta was exposed to the intervention of interest. The J remaining provinces serve as potential controls. This set of control units is conventionally called the "donor pool." Our sample includes a number of pre-intervention periods, T_0, as well a number of post-intervention periods, T_1, with $T = T_0 + T_1$. In this context, it is useful to think in terms of potential outcomes in a panel setup. The treatment effect for Salta at time $t = T_0 + 1, ..., T$ is defined as

$$\tau = Y_{St}(1) - Y_{St}(0) = Y_{St} - Y_{St}(0) \tag{1}$$

where $Y_{St}(1)$, $Y_{St}(0)$ are Salta's potential outcomes with and without treatment, respectively.[17] We aim to estimate the vector $(\tau_{ST_0+1}, ..., \tau_{ST})$, that is, the impacts of the TDP over time. Because $Y_{St}(1)$ is observed, to estimate τ_{St} we just need to estimate $Y_{St}(0)$, that is, the contrafactual trajectory of tourism employment in Salta without the TDP.

Suppose a general model for the potential outcomes of all provinces. The observed tourism employment for province i at time t is

$$Y_{it} = Y_{it}(0) - \tau_{it} D_{it} \tag{2}$$

where $i = 1, ..., J+1$ and D_{it} takes the value of one when $i = S$ and $\tau > T_0$. Following Abadie *et al.* (2010) we express $Y_{it}(0)$ using a linear factor model

$$\begin{aligned} Y_{it}(0) &= \delta_t + \nu_{it} \\ Y_{it}(0) &= \delta_t + \theta_t X_i + \lambda_t \mu_i + \varepsilon_{it} \end{aligned} \tag{3}$$

capacity to service the tourism sector, and the macroeconomic and fiscal policy environment (Dwyer *et al.*, 2000, 2003, 2004a).

[17] Hereafter, "S" indicates the Province of Salta.

where δ_t is a vector of common time-specific effects (factors) with constant individual effects (factor loadings) across provinces, and ν_{it} is an error that can be divided into a vector of relevant observed predictors for tourism employment X_i −time invariant or time varying, and pre- or post-treatment as long as they are not affected by the policy, a vector of unknown time-specific parameters θ_t, a province-specific unobservable μ_i, an unknown common factor λ_t, and an unobserved transitory shock at the provincial level ε_{it} with zero mean for all i conditional on (δ_t, X_i, μ_i).[18]

As defined above, synthetic Salta is a weighted average of the provinces in the donor pool. That is, synthetic Salta can be represented by a $(Jx1)$ vector of weights $W = (w_1, ..., w_J)'$ such that $w_i \geq 0$ for all $i \neq S$ and $w_1 + ... + w_J = 1$. Each value of the vector W represents a potential synthetic control for Salta, that is, a particular weighted average of control provinces. Using the linear factor model just described, Abadie *et al.* (2010) prove that if the number of pre-intervention periods in the data is large relative to the scale of the transitory shocks and, we can choose w^* such that

$$\sum_{j=1}^{J} w_j^* Y_{jT_0} = Y_{ST_0} \quad \text{and} \quad \sum_{j=1}^{J} w_j^* X_j = X_S, \text{then} \tag{4}$$

$$\widehat{\tau} = Y_{St} - \sum_{j=1}^{J} w_j^* Y_{jT} \tag{5}$$

is an unbiased estimator of τ_{St} for $t \,\epsilon\, \{T_0 + 1, ..., T\}$, that is, the impact of the TDP. As in the case of a common lagged dependent variables model, the identifying assumption in the SCM is independence of treatment status and potential outcomes conditional on a lagged outcome variable and other observable confounders.[19]

Since condition (4) can hold exactly only if $Z_S = (Y_{ST_0}, X_S)$ belongs to the convex hull of $Z_j = \{(Y_{11}, X_1) ... (Y_{JT_0}, X_J)\}$, in practice, W^* is estimated in a non-parametric fashion and is selected so that condition (4) holds approximately. Abadie & Gardeazabal (2003) and Abadie *et al.* (2010) propose choosing W^* as the value of W that minimizes the distance

$$\|Z_S - Z_j W\| \, \nu = \sqrt{(Z_S - Z_j W)' V (Z_S - Z_j W)} \tag{6}$$

where V is a symmetric and positive semidefinite matrix that reflects the relative importance assigned to each employment predictor, including pretreatment employment.

[18] Notice that, while the traditional differences-in-differences (fixed-effects) model would restrict the impact of unobservable province heterogeneity to be constant over time −i.e. $\lambda_t = \lambda$ for all t−, the factor model presented allows the impact of these confounding unobserved characteristics to vary with time. We can think, for instance, of λ_t as the devaluation in Argentina in 2002 (common shock across provinces) and μ_i as the heterogeneous impact of the peso devaluation on province i according to its tourism potential. See Bai (2009) for panel data models with interactive fixed effects.

[19] See Dehejia & Wahba (1999) for an example of matching strategies based on lagged dependent variables. See also Chapter 5 in Angrist & Pischke (2008).

Although this inferential procedure is valid for any choice of V, the choice of V influences the mean squared prediction error (MSPE) of the estimator, that is

$$MSPE(Y) = \frac{1}{T_0}\sum_{t=1}^{T_0}[(Y_{St} - \sum_{j=1}^{J} w_j^*(V)Y_{jt})^2] \tag{7}$$

To assign larger weights to variables that have large predictive power on tourism employment, we choose V^* as the value of V that minimizes $MSPE$ for tourism employment in the entire pretreatment period.[20] The weights for the synthetic control are then given by $W^* = W^*(V^*)$. In other words, we minimize equation (7), for $W^*(V)$ given by equation (6).[21]

Overall, the synthetic control algorithm estimates the missing counterfactual for Salta ($Y_{St}(0)$) as a weighted average of tourism employment for provinces in the donor pool. The weights are chosen so that pretreatment values of tourism employment and covariates of synthetic Salta are, on average, similar to those of real Salta. Then, if real Salta and synthetic Salta have similar behavior over the extended pretreatment period, a discrepancy in tourism employment following the intervention is interpreted as having been produced by the intervention itself, that is, as a causal effect of the TDP on tourism employment.

4 Data and Sample

This analysis uses a monthly sector-level panel dataset at the provincial level for the period 1996-2013. The data were collected by the Observatory of Employment and Entrepreneurial Dynamics (OEDE) at the Ministry of Labor, Employment, and Social Security of Argentina.[22] Salta's TDP began in June 2003, providing almost 7.5 years (89 months) of pre-intervention data. The sample period begins in 1996, the year when the OEDE started collecting these data, and ends in June 2013, the last year of complete information. This period amounts to a decade of post-treatment analysis, which is a reasonable period to predict and measure the effect of this policy.

The list and description of all variables used in the empirical analysis are provided in the data appendix, along with data sources. The outcome variable is employment in the "Hotel and Other Accommodation Establishments" sector (3-digit SIC sector) as a proxy for tourism employment. For the pretreatment covariates, we rely on a standard set of tourism employment predictors: employment, number of firms, average wage, average size of firms, average age of firms, GDP, informality, population, population with university level, road paving and public lighting (see Annex A for details).

[20] We follow Abadie & Gardeazabal (2003), Abadie *et al.* (2010), and Billmeier & Nannicini (2013).

[21] We use the *synth()* routine developed by Abadie *et al.* (2011). Specifically, we use the data-driven, fully nested optimization procedure that searches among all (diagonal) positive semidefinite V-matrices and sets of W^*-weights for the best fit between Salta and a convex combination of the control units (i.e., the synthetic Salta) in terms of the pretreatment values of the outcome variable.

[22] Given the confidentiality of the data, the estimations were conducted following the OEDE microdata policy, which implies working in situ under the supervision of its staff and with blinded access to sensible information.

Because synthetic Salta is constructed as a weighted average of potential control provinces, it is important to exclude from the donor pool those provinces that were subject to structural shocks in tourism employment. For this reason, those provinces that implemented another large-scale tourism policy during the period under study were not included in the donor pool.[23]

Finally, to minimize bias caused by interpolating across provinces with very different characteristics and with outcomes driven by a different structural process, we also discarded Buenos Aires, the Autonomous City of Buenos Aires, and Córdoba.[24] Therefore, the final donor pool includes the remaining 19 provinces: Catamarca, Corrientes, Chaco, Chubut, Entre Ríos, Formosa, Jujuy, La Pampa, La Rioja, Mendoza, Misiones, Neuquén, San Juan, San Luis, Santa Cruz, Santa Fé, Santiago del Estero, Tucumán, and Tierra del Fuego.[25]

5 Results

5.1 On the mechanisms of TDP impact

Although a causal assessment of the specific mechanisms that led to TDP's effects is beyond the scope of this study, we then explore potential channels through which the policy was expected to trigger growth in the tourism industry and therefore boost employment.[26] The TDP was expected to strongly increase the number of tourists, especially international tourists, as well as their daily expenditure and overnight stays.

Since 2002, the number of tourism arrivals in Salta sustainably increased (extensive margin). As shown in Figure 1a tourism arrivals tripled in the post-policy period.[27] In particular, this increase was lead by the air arrivals to the Salta International Airport. Indeed, while in 2003 air arrivals represented 33 percent of total arrivals, in 2011 it represented 50 percent. This was also accompanied by a large increase in the hotel occupancy rate. Figure 1 in Annex B shows that, for the 2004-2011 period, the occupancy rate in Salta increased around 90 percent with respect to 2004, the best performance in this indicator among all tourist destinations in Argentina.

Finally, another relevant mechanism is the intensity of tourism activity per visitor (intensive margin). Figure 1b shows the trend in the average daily expenditure and the number of overnight stays per tourism visitor. As expected, both tourism indicators increased in the post-TDP period. This increase is related to the greater variety and higher quality of the tourism services made available by the TDP. That is, the change in the intensity of the tourism activity was driven by both longer stays and more and higher-quality tourism options.

[23] This is the case of the province of Río Negro. Río Negro received three IDB programs (2003, 2005 and 2006) to support the tourism industry.

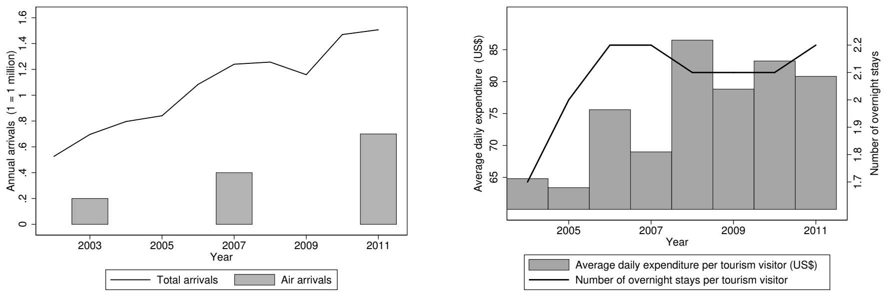
[24] These provinces are outliers and highly cyclical in terms of tourism employment.

[25] Our results are robust to the inclusion of all discarded provinces as well as other tourism employment predictors.

[26] Our main limitation for analyzing causality regarding the mechanisms through which the TDP had effects is the lack of adequate data on tourism-related indicators before the TDP and for the remaining provinces.

[27] Own elaboration based on data from the Ministry of Culture and Tourism of the province of Salta.

Figure 1: Mechanisms of the TDP impact.



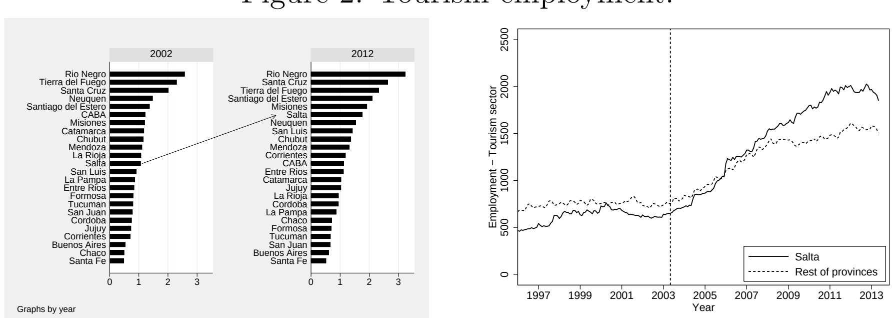
(a) Annual visitor arrivals. (b) Expenditure and overnight stays.

5.2 The impact of TDP on tourism employment

Following Sinclair (1998), we first assess the relevance of the TDP's results by looking at the evolution of tourism employment as a share of total employment in Salta relative to the other Argentinean provinces. Figure 2a shows that between 2002 and 2012, this ratio almost doubled in Salta, increasing from around 1 percent to 2 percent. The comparison with other provinces confirms that this growth was due to a real structural change for Salta. In fact, while in 2002, Salta was 12th out of 24 provinces in terms of its share of total tourism employment, ten years later Salta had climbed up to the sixth position.

Before estimating the weights for the synthetic Salta using the SCM, Figure 2b plots the employment trends in tourism in Salta and the population-weighted average of the rest of the provinces in the donor pool. The figure shows that the entire donor pool would not be a suitable comparison group for Salta. In fact, even prior to TDP implementation, the time series of tourism employment in Salta differed from that of the rest of the country. While pretreatment trends in employment are somewhat similar, after June 2003, trends began to diverge drastically, pointing to a potential impact of the policy.

Figure 2: Tourism employment.



(a) Share in total employment. (b) Trends.

Table 1 displays the weights for each donor province in synthetic Salta from the SCM estimation. The reported weights indicate that tourism employment in Salta in the pre-policy period is best reproduced by a combination of Jujuy, Santa Fé, Tucumán, Formosa, and Neuquén. Intuitively, these weights are quite reasonable. The algorithm constructed a synthetic Salta from a combination of some neighboring provinces with similar development indicators and tourism dynamics (Jujuy, Tucumán, and Formosa), and some provinces with a more relevant and high-potential tourism industry (Santa Fé and Neuquén).

Next, we use the estimated weights to obtain synthetic Salta and compare it to real Salta in pretreatment characteristics. The results displayed in Table 2 show that synthetic Salta is very similar to real Salta in all covariates used in the estimation. By contrast, the simple weighted average of all provinces in the country and the provinces in the northwest region, where Salta is located, would not provide a suitable control group.

Table 1: Province weights in the synthetic Salta.

Province	**Weights**
Buenos Aires	-
Autonomous City of Buenos Aires	-
Catamarca	0
Córdoba	-
Corrientes	0
Chaco	0
Chubut	0
Entre Ríos	0
Formosa	0.114
Jujuy	0.393
La Pampa	0
La Rioja	0
Mendoza	0
Misiones	0
Neuquén	0.064
Río Negro	-
San Juan	0
San Luis	0
Santa Cruz	0
Santa Fé	0.222
Santiago del Estero	0
Tucumán	0.207
Tierra del Fuego	0

Table 2: Employment predictor means before TDP.

	Salta		**Average of rest of**	
	Real	**Synthetic**	**Provinces**	**NOA**
Tourism sector level				
Employment	617	615	750	459
Number of firms	77	75	93	46
Average Wage	510	512	557	515
Average size of firms	8	8	8	10
Average age of firms	7	8	8	7
Log of GDP	17	17	17	17
Province level				
Log of Employment	11	11	12	11
Log of Number of firms	9	9	9	8
Average Wage	608	645	664	619
Average size of firms	11	11	9	11
Average age of firms	12	12	12	13
Log of GDP	22	22	23	22
Informality	0.52	0.49	0.46	0.52
Log of Population	13	13	14	13
University level	0.02	0.02	0.02	0.02
Road paving	0.52	0.54	0.59	0.49
Public lighting	0.85	0.85	0.84	0.82

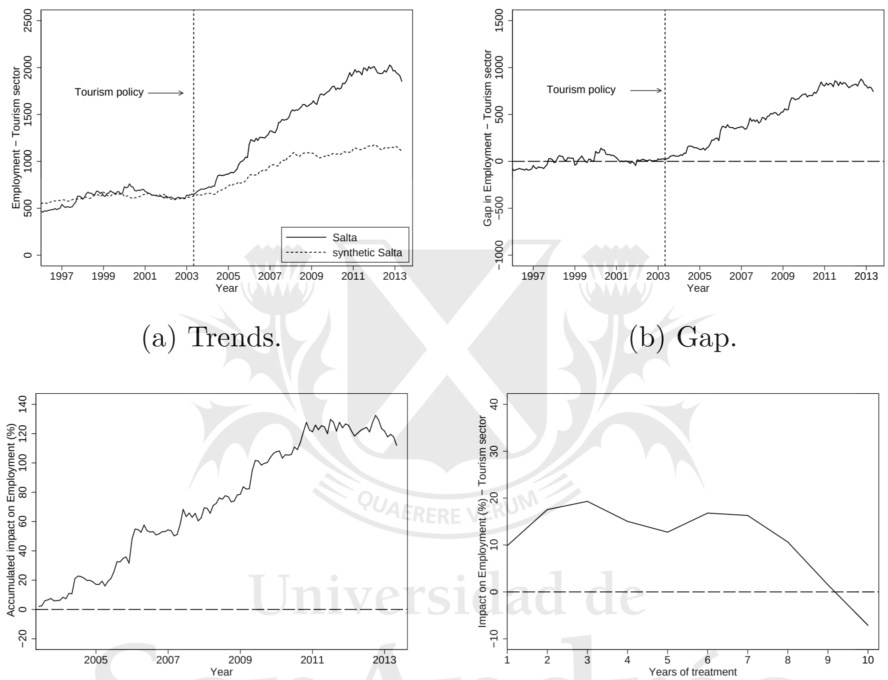
Note: Employment, number of firms, average wage, average size of firms, and average age of firms are averaged for the January1996-May2003 period (for both the tourism sector and province level). GDP is averaged for the 1993-1998 period. Informality is measured in 2002-2003, and population, university level, road paving and public lighting are measured in 2001.

Figure 3a displays the tourism employment trajectory for real Salta and its synthetic counterfactual from 1996 to 2013. Tourism employment in synthetic Salta closely resembles the real Salta's trend during the entire pre-policy period, especially in the months before the TDP began, further confirming the validity of the generated counterfactual. The estimate of the impact of the TDP on tourism employment in Salta is given by the difference between real Salta and its synthetic counterpart after policy implementation. From this date onward, the two lines diverge noticeably. The discrepancy between the two lines suggests a large positive effect of the TDP on tourism employment.

Figure 3b plots the gap in tourism employment between real and synthetic Salta. The magnitude of the estimated impact of TDP is substantial. Between 2003 and 2013, tourism employment increased by an average 11 percent per year due to the TDP, for an accumulated impact of 114 percent from the May 2003 baseline level (Figure 3c).

Since the growth in tourism employment in the period was 184 percent, the estimated impact implies that around 62 percent of this growth was due to the TDP. In terms of job creation, the magnitude of the impact is approximately 750 new formal jobs. In dynamic terms, Figure 3d shows that the magnitude of the average annual impact increased during the first years of treatment, followed a relatively constant path between the fourth and seventh years, and decreased in the last years of analysis (2010-2013) until it disappeared.

Figure 3: The impact of TDP on tourism employment: Salta vs. synthetic Salta



(a) Trends.

(b) Gap.

(c) Accumulated impact by month.

(d) Average impact by year.

6 Placebo and Robustness Tests

To confirm that the gap shown in Figure 3b is the true causal effect of the TDP, we need to conduct inference and provide evidence of the validity of synthetic Salta as a counterfactual. In comparative case studies such as this analysis, large sample inferential techniques are not well suited because of the small sample size of the dataset. Therefore, following Abadie & Gardeazabal (2003) and Abadie *et al.* (2010, 2015), we apply exact inferential techniques, similar to permutation tests, to conduct inference. By systematizing the process of estimating the counterfactual of interest, the SCM enables us to conduct a series of placebo tests and falsification tests.[28] Specifically, we use three versions of placebo tests: provinces, sectors, and in-time placebo.

The idea behind these placebo tests is that the inherent validity of the results obtained would be limited if the SCM also estimated large effects when iteratively

[28] See Angrist & Krueger (1999) and DiNardo & Pischke (1997) for applications of similar falsification tests.

applied to non-treated provinces, non-treated economic sectors, or to different dates of the intervention. In other words, our confidence in the large impact of the TDP on tourism employment in Salta would be undermined if this estimated effect fell inside the distribution of placebo effects or if the in-time placebo test generated impact in the pre-policy period. Using *p-values* computed under random permutations of the units (or starting dates) assigned to treatment, we can compare the placebo effects and the estimates for Salta's tourism employment.

We also perform four additional robustness checks: (i) the dependence of the results on a particular (positive weighted) control unit or a group of positive weighted donors, (ii) the exclusion of nearby provinces, (iii) the choice of V weights, and (iv) the combination with Differences-in-Differences. Finally, given the dual focus of the evaluated policy −one specific sector in one province− we construct an alternative synthetic trajectory of tourism employment in Salta using a combination of other sectors from different provinces.

Overall, the purpose of these exercises is to assess whether the gap in tourism employment might be caused by other external factors rather than the TDP or biased due to inter-province or other type of spillover effects. The results of these exercises C confirm that our main results are robust across the placebo tests and sensitivity checks and are consistent among alternative synthetic control units. In fact, the strong similarity of the results obtained through the different specifications provides robust evidence that the SCM is correctly isolating the effects of the policy. This also allows us to discard the hypothesis that these effects are overestimated (underestimated) because of potential negative (positive) spillovers. Moreover, in all cases, the synthetic control units produce counterfactuals that clearly contain more information than a simple extrapolation of Salta's pre-intervention trend. For example, the weighted control units clearly capture the cyclicality that a true counterfactual should be expected to pick up.

7 Exploring other Aspects of TDP Impact

7.1 Number of firms and average wage in tourism sector

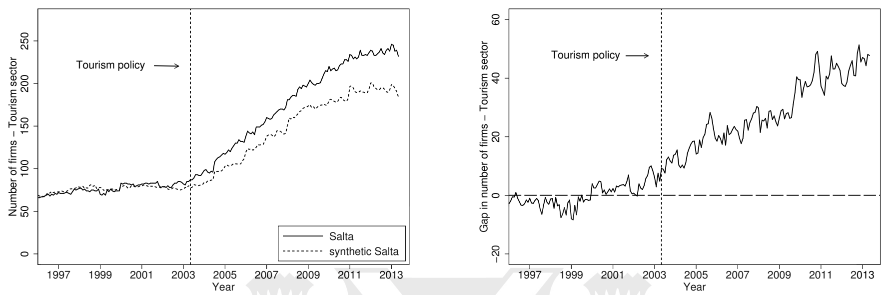
On the supply side, in addition to analyzing changes in employment, we also explore the impact of the TDP in the number of establishments offering hospitality services. Figure 4a clearly shows that, after being stagnant at around 60 units for several years, the number of hospitality establishments almost quadrupled since the beginning of the TDP, reaching 240 units in 2013. However, after constructing Salta's synthetic counterpart, we obtained that only 28 percent (50 firms) of the total change is due to the TDP (Figure 4b). In other words, the TDP increased the number of establishments by around 10 percent per year.[29]

Second, we analyze wage dynamics in Salta's tourism industry. As mentioned in Section 2.3, given the characteristics of the local labor market, we do not expect any pressure on wages. Nevertheless, in the tourism sector, where the construction of hos-

[29] By applying the placebo test, we obtained that this effect became statistical significant at 10 percent.

pitality infrastructure takes time to materialize, it may be possible that a short term effect on wages (and prices) would have occurred given that the supply may react more slowly than the demand of tourism services. This dynamic seems to be revealed by Figure 5a, which shows the evolution of the ratio between the average wage in the tourism sector and the average wage of other sectors in Salta. The average wage ratio increased after 2004 but then decreased gradually to its previous level.

Figure 4: Number of firms: Salta vs. synthetic Salta



(a) Trends. (b) Gap.

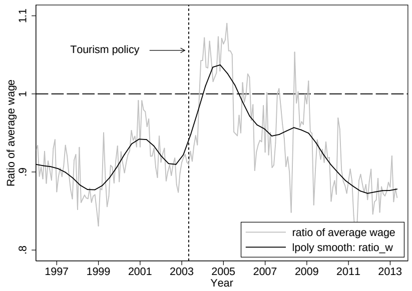
We then estimate the impact of TDP on wages applying the SCM. Figure 5b displays the trend in average wages in Salta and synthetic Salta. The synthetic counterpart follows a very similar pattern to that of Salta. Both trends are mainly driven by the inflationary period that began after the devaluation of the peso in 2002. Another interesting feature is the occurrence of seasonal peaks in wages. The SCM does a good job of capturing this seasonality.

When we take a closer look at the gap in average wages (Figure 5c), we find a similar result as the one shown in Figure 5a: a small impact in the short and medium term that disappears in the long-term. This confirms that while the TDP had a significant and long lasting effect on employment it had no long-term effects on wages in the tourism industry. This result is also consistent with the fact that the tourism sector employs a relatively large portion of low-skilled and part-time workers paid at a minimun wage. In fact, Figure 5d shows how the average wage in the tourism sector in Salta is close to the minimum wage, and between 10th and the 50th percentile of the average wage distribution among sectors.

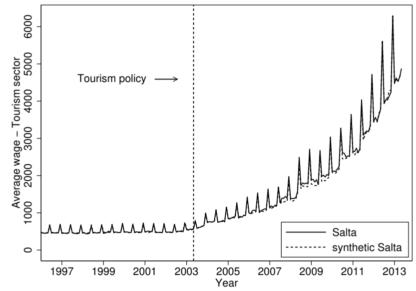
7.2 Tourism value-chain and provincial economy

Having identified robust positive effects on hospitality employment in Salta, another question is whether the TDP affected employment in tourism-related sectors. For this, we replicate our estimation using SCM in the tourism value-chain (including the hospitality sector). According to the WTO (WTO, 2014a), the tourism value-chain includes the following sectors: accommodation for visitors, food and beverage serving activities, railway, road, water and air passenger transport, transport equipment rental, travel agencies and other reservation services activities, cultural and entertainment activities, sports and recreational activities, retail trade of country-specific tourism characteristic goods, and other country-specific tourism characteristic activities.

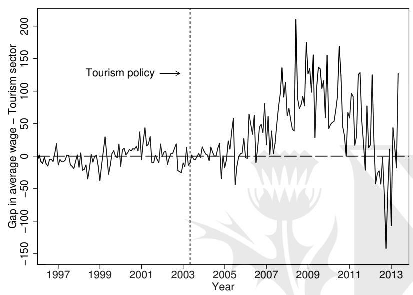
Figure 5: Average wage.



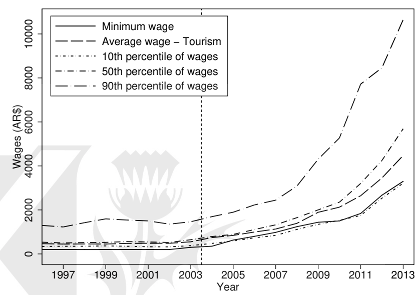
(a) Ratio.



(b) Trends - Salta vs. synthetic Salta



(c) Gap - Salta vs. synthetic Salta.



(d) Minimum wage.

Figures 6a and 6b represent the evolution of the employment trajectory and gap between Salta and its synthetic counterpart in the tourism value-chain, respectively. In general, the SCM algorithm matches well the pretreatment employment trends in this sector. Between 2003 and 2013, employment increased by an average 2.2 percent per year due to the TDP, for an accumulated impact of 22 percent from the May 2003 baseline level. In terms of job creation, this implies a net creation of 1,376 formal jobs, from which almost the 50 percent comes from the hospitality sector.

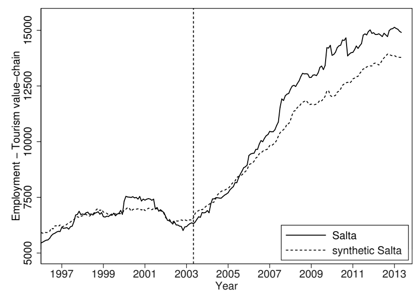
We then analyze the impact of TDP on the overall employment of the province of Salta. Figure 6c shows a positive effect of the TDP on the employment of the Salta economy compared with the synthetic unit. While the employment gap prior to the TDP tends to be around 0, after the TDP the gap started to be positive (Figure 6d). This gap corresponds to a TDP impact of around 0.5 percent per year between 2003 and 2013 i.e. an accumulated impact of 5 percent. This implies that, in total, 2,750 formal jobs were created in the province of Salta due to the TDP. Both the effects on the tourism value-chain and overall economy tend to appear mainly in the medium and long term.[30]

The estimated TDP impact on the overall economy has two important implications. First, it confirms that the increase in employment in tourism and its related sectors did not come at the expenses of (crowd-out) other sectors but an increase of total provincial employment. Second, the overall effect is larger than the tourism value-

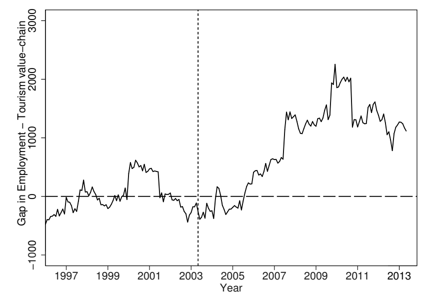
[30] By applying the placebo tests, we obtain that these effects are statistical significant at 10 percent or 5 percent, depending on the test. Results are robust to different SCM specifications and tests.

chain effect, pointing out that inter-industry employment spillover (crowding-in) effects actually occurred. Indeed, for one job created in the tourism value-chain, an additional job was created in the rest of the economy. This is highly consistent with the input-output matrix of Argentina, which estimates an employment multiplier of around two for the tourism industry.[31]

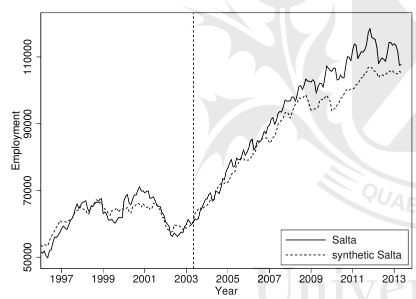
Figure 6: The impact of TDP on employment: Salta vs. synthetic Salta



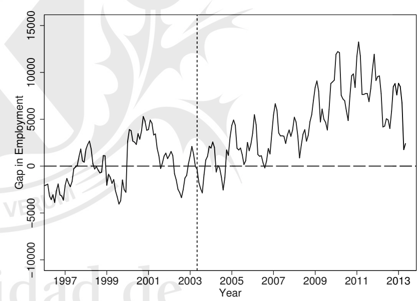
(a) Trends in tourism value-chain



(b) Gap in tourism value-chain



(c) Trends in the overall economy



(d) Gap in the overall economy

Finally, in terms of overall average wage, we find a negative effect of the TDP, particularly in the medium and long-term (see Annex D Figure 1). This effect is again consistent with the hypothesis that TDP may have mostly created relatively low skill jobs for unemployed workers willing to accept wages close to the legal minimum. Over time, this constant increase in the portion of low wage jobs may have induced a growth of the average wage slower than the counterfactual trend without these new jobs.

8 Conclusion

A true revival of industrial policies has occurred. After falling out of favor for many years, a new type of industrial policies is now being globally implemented by governments to foster growth and sustain job creation, particularly at the sub-national level. In this context, many countries and regions have adopted regional policies focused on the tourism industry. In this case, governments have acknowledged the need to play an active role in the development of this industry, which is often plagued by coordination failures and requires the provision of several public goods. In addition,

[31] The last complete input-output matrix for Argentina refers to 1997, INDEC (2001).

the ability of tourism to generate employment opportunities has made its development particularly attractive to developing countries endowed with natural, historical, and cultural resources. However, despite the renewed acceptance of this industrial policies, old issues related to their design and evaluation persist and, to date, few studies have attempted to identify their causal effects on growth and employment.

This study contributes with a rigorous analysis of the causal effects of a regional industrial policy on employment. Applying a SCM approach to the tourism development policy of the Salta province in Argentina, we find strong effects on the employment in the hospitality sector. Specifically, we find an average annual impact of 11 percent over the period 2003-2013. This corresponds to an accumulated impact of approximately 750 new formal jobs since the baseline date in May 2003. In addition, our findings show that this effect increases to 1,376 formal jobs when considering the entire tourism value-chain. Given the scope of the policy, these direct effects are not surprising. However, a key question—as should be for any industrial policy—is then whether these direct effects came at the expenses of other industries. In this case, we find that the positive inter-industry employment spillovers/externalities clearly more than compensated any potential crowding our effects. That is, our results show an increase in total employment—equal to 2,750 formal jobs—that clearly exceeded the direct effect on the tourism industry. That is, for each job created in the tourism value-chain, one additional job was created in the rest of the provincial economy.

Our findings confirm that well-designed and opportunely implemented regional industrial policies can effectively achieve important structural effects and boost job creation in developing regions. In the specific case of tourism, fostering coordination and overcoming financial, infrastructure, and institutional bottlenecks are key to the success of policies in this sector. Indeed, the integrated approach adopted in Salta made it possible to overcome various bottlenecks, activate several drivers of the tourism demand, while simultaneously support the supply side. All these elements allowed to initiate a process of agglomeration that is reflected in the significant results achieved over a ten-year period.

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A Data Appendix

Source I: Observatory of Employment and Entrepreneurial Dynamics (OEDE) at Ministry of Labor, Employment, and Social Security in Argentina, 1996-2013 (province − 3-digit SIC sector level).

- **Employment:** number of formal employees. Frequency: Monthly.
- **Number of firms.** Frequency: Monthly.
- **Average wage:** ratio of the sum of monthly wages of formal employees to number of formal employees. Frequency: Monthly.
- **Average size of firms:** ratio of number of formal employees to number of firms. Frequency: Monthly.
- **Average age of firms.** Frequency: Annual.

Source II: Argentina National Population, Households, and Dwelling Census, 2001 (province level).

- **Log of population:** logarithm of total population aged 14 and older.
- **University level:** share of population aged 20 and older with university level completed in the total population.
- **Road paving:** share of households with access to at least one paved road in the census area in the total households.
- **Public lighting:** share of households with access to public lighting in the census area in the total households.

Source III: Ministry of the Interior and Transportation, 1993-1998 (province − 3-digit SIC sector level).

- **Gross Domestic Product (GDP).** Frequency: Annual.

Source IV: Permanent Household Survey, National Statistical and Census Institute, 2003 (province level).

- **Informality rate:** share of employees aged 18 and older without pension contributions.

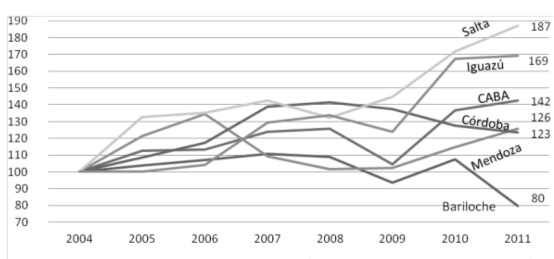
B The Province of Salta

The Province of Salta is in the northwest of Argentina (NOA). It has an area of 155,488 km^2 −6 percent of the nation's land mass−and it borders six Argentinean provinces and three countries (Chile, Bolivia, and Paraguay). In 2001, its population was about 1 million−3 percent of Argentina's total population−with an average population density of seven people per km^2 and an urbanization rate of 78 percent.[32] Salta was one of the least developed provinces in the country. Primary and its complementary industries were the main economic activities. Per capita GDP in 2001 was US$4,000, about half that of the country as a whole (US$7,500).[33]

Despite its stagnant economy, Salta's natural beauty and cultural heritage have made it a tourist destination. The diversity of its natural resources ranges from the Andean highland plateau (the "Puna") and the Chaco forests to the subtropical forest in the Yungas Biosphere Reserve. The uniqueness of its landscapes, characterized by colorful hillsides, ravines, mountain peaks, volcanoes, and salt flats, can be appreciated in its numerous protected areas, covering about 18 percent of its territory. Salta is also known for winery tours through the world's highest vineyards. This unique feature led to the construction of the Grape and Wine Museum (Museo de la Vid y el Vino), located in the tourist city of Cafayate.

Salta's vast cultural heritage includes native and aboriginal communities, colonial and archaeological sites, and cave paintings. The province offers internationally recognized attractions, such as the monumental Train to the Clouds (Tren a las Nubes), one of the highest railways in the world, and the prestigious Museum of High Altitude Archaeology (MAAM). Finally, Salta's privileged location magnifies its tourism potential. Considered the main port of entry to the NOA region and sharing borders with Chile, Bolivia, and Paraguay, the province offers convenient access to regional circuits (i.e., *Qhapaq Ñan* and the Great Inca Road) that have become popular among international tourists.

Figure 1: Hotel occupancy index (2004 = 100) - Main tourist destinations in Argentina.



Source: "IERAL-Fundación Mediterránea" based on the Hotel Occupancy Survey (INDEC).

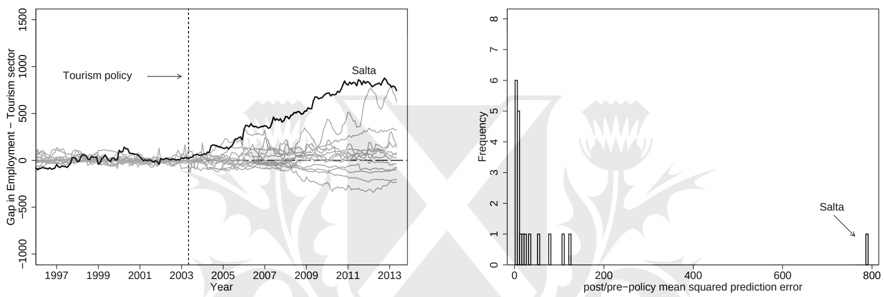
[32] Argentina National Population, Households, and Dwellings Census, 2001.
[33] National Statistical and Census Institute (INDEC).

C Placebo and Robustness Tests

C.1 Placebo of provinces

As in classical permutation tests, the intervention was reassigned to units that were not exposed to the intervention. That is, we iteratively apply the SCM to every other control province, shifting Salta to the donor pool. Ideally, the estimated effect in real Salta should be larger than the estimated effect for any other province not exposed to the TDP.[34] Figure 1a displays the results for this placebo test. Comparing against the distribution of gaps for the 12 remaining untreated provinces, the gap between Salta and synthetic Salta appears highly unusual. In fact, the positive effect in Salta is by far the largest of all.

Figure 1: Placebo of provinces



(a) Placebo gaps (b) Ratio of post/pre-policy MSPE

In this context, *p-values* can be constructed by computing the proportion of estimated placebo gaps that are greater or equal to the estimated gap for Salta. Formally,

$$p - value = \Pr\left(\hat{\tau}^{PL} > \hat{\tau}_S\right) = \frac{1}{J+1}\sum_{i=1}^{J+1} I\left(\hat{\tau}^{PL}_{iT} \geq \hat{\tau}_{ST}\right) \tag{1}$$

where $\hat{\tau}^{PL}_{iT}$ is the estimated gap for the last post-treatment period T when province i is assigned to placebo treatment at the same time as Salta. In our case, given that we use 12 provinces plus Salta, the probability of obtaining a greater or equal effect to the one estimated for Salta is $1/13 \cong 0.076$.

To obviate the need to choose a cut-off for the exclusion of ill-fitting placebo runs, we look at the distribution of the ratios of post/pre-policy MSPE. A large post-policy MSPE is not indicative of a large effect if the estimated counterfactual does not closely reproduce employment in tourism prior to the policy. Figure 1b reports the distribution of post/pre-policy ratios of MSPE for Salta and 19 provinces. Salta clearly stands out as the province with the highest MSPE ratio. For Salta, the post- policy MSPE is almost 800 times larger than the pre-policy MSPE. Because this test includes 20 provinces, if one were to assign the policy at random in our data, the probability of obtaining a post/pre-policy ratio as large as Salta's would be $1/20 \cong 0.05$.[35]

[34] We exclude provinces that had a pre-policy MSPE of more than 20 times Salta's.

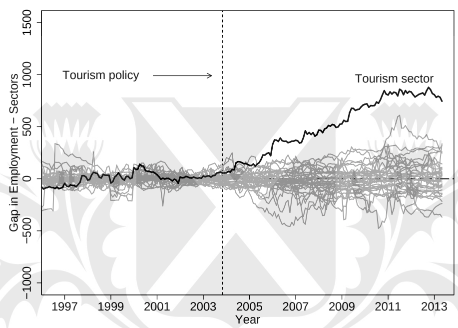
[35] Both test levels obtained are similar to those typically used in conventional tests of statistical significance.

C.2 Placebo of sectors

In the second test, the SCM was iteratively applied to every other sector using our donor pool of provinces to construct the synthetic counterpart. The idea is to discard the hypothesis that the growth in tourism employment in Salta is the result of overall employment growth within the province. If this hypothesis were true, then we should find similar gaps for other sectors.

Figure 2 displays the results for this placebo test for the tourism sector and 35 untreated sectors. The gap for the tourism sector appears highly unusual. In fact, the probability of obtaining a greater or equal effect to the one estimated for the tourism sector is $1/36 = 0.028$.

Figure 2: Employment gap in Tourism sector and placebo gaps in 36 sectors in Salta



C.3 In-time placebo

Another way to conduct a placebo test is to randomly reassign the time when the intervention took place (Heckman & Hotz, 1989; Bertrand *et al.*, 2002). Ideally, no impacts will be found in the pretreatment period. To construct *p-values*, and given that the frequency of our outcome variable is monthly, we can choose, for instance, a 24-month window after a placebo starting date to compare the estimated gaps.[36]

Figure 3a displays the results of applying SCM using a set of pretreatment dates (i.e., our placebo dates). We find no evidence of diverging trends between Salta and synthetic Salta in a two-year window of placebo months. We find consistent evidence that synthetic Salta predicts very well the trends of tourism employment for Salta over the entire pretreatment period (January 1996-May 2003). This result is maintained despite the lower pretreatment information on predictors that SCM uses to predict.

Because the TDP started in June 2003, to conduct inference, we can then use each of the 87 pretreatment months as placebo dates of the beginning of the policy and iteratively apply the SCM to Salta.[37] Figure 3b reports the gaps using all pretreatment months considered plus June 2003.

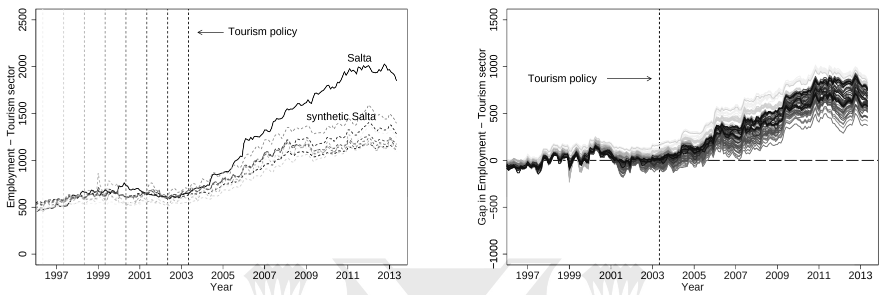
The darkest gaps of Figure 3b correspond to placebo estimates computed using a starting date closer to the actual one. As expected, these gaps are quite similar to

[36] Results are robust to different choices of this time window.

[37] We do not use the first two months (January and February 1996) as placebo months because we need at least two pretreatment periods to apply the SCM.

the one obtained in our main estimation. On the other hand, the lightest gaps, that use a starting date farther away from the true one, slightly overestimate the impact. This is probably related to the fact that the algorithm uses fewer years of pretreatment information in those cases. Finally, the intermediate grey lines represent gaps that use information near the 2001 crisis as the last period of information. As expected, these gaps tend to slightly underestimate the impact of the TDP.

Figure 3: In-time placebo: Salta vs. synthetic Salta



(a) Trends. (b) 87 placebo gaps.

Nevertheless, in all cases, synthetic Salta fits well to real Salta in the actual pre-treatment period, generating no gap in this timespan. Furthermore, the estimated gaps after June 2003 are similar to the gap estimated using the actual starting date of the TDP. If a month is chosen randomly, the probability of obtaining, after two years of a placebo starting date of the TDP, a greater or equal effect to the one estimated using the month when the policy actually started is $1/88 \cong 0.011$.

C.4 Leave-out tests

In this test, we first iteratively apply the SCM to Salta, omitting in each iteration one of the provinces that received a positive weight. Second, this exercise is extended to the rest of the provinces in the donor pool. Finally, we iteratively apply the SCM first omitting the two provinces with highest weights, then the three provinces with highest weights, and so on.

Figure 4 displays the results of this leave-out test. This figure shows that results are robust to the exclusion of any positive or non-positive weighted province from our donor pool as well as to the exclusion of the groups of positive weighted provinces.

C.5 Excluding Salta's nearby provinces

One of the main concerns regarding the main estimation is the fact that the SCM may overestimate (underestimate) the effects on tourism employment due to negative (positive) spillovers produced by the TDP on Salta's nearby provinces. That is, our estimation might be biased due to inter-province spillover effects. Although in Section 6 we show that this is not actually the case, we run the same SCM specification but excluding from our donor pool all of Salta's nearby provinces, that is, Catamarca, Chaco, Formosa, Jujuy, Santiago del Estero, and Tucuman.

Figures 5a and 5b present the results. As expected, we find an impact on tourism employment equal to the one obtained in our main estimation. This finding reinforces the hypothesis that the benefit enjoyed by Salta due to the TDP is not biased by potential spillover effects to nearby provinces. Moreover, it signs that these spillovers did not in fact occur.[38]

Figure 4: Leave-one-out distribution of the synthetic control for Salta.

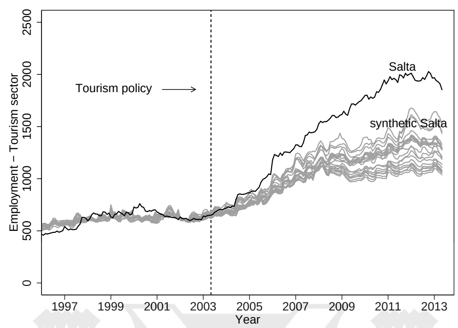
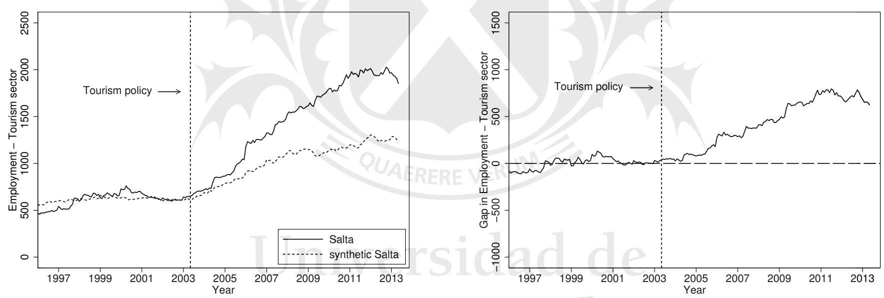


Figure 5: Tourism sector: Salta vs. synthetic Salta without nearby provinces.



(a) Trends. (b) Gap.

C.6 Cross-validation procedure to choose V weights

We then check the sensitivity of the results to the V weights. To do this we divide the pretreatment period originally used to identify the V^* matrix of weights into an initial training period and a subsequent validation period. Then, using predictor data in the training period, the V weights were chosen to minimize the MSPE of the outcome variable in the validation period. Finally, with these latter V weights and the predictors observed in the validation period, we estimate a synthetic Salta.[39] This cross-validation procedure allows us to test the robustness of the estimated gap to different choices of V weights while testing how well the synthetic control fits Salta over different validation periods.

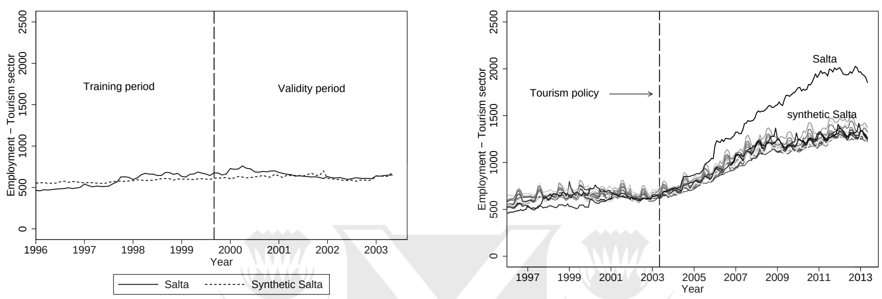
Figure 6a shows an example of the first stage of this procedure. Using the first half of the pretreatment period, we obtain the V weights and construct a synthetic control

[38] We also apply the same SCM specification to each of Salta's nearby province, excluding Salta from the donor pool. Consistent with our main results, we find that the effects on tourism employment in Salta's nearby provinces are not statistically significant different from zero.

[39] This cross-validation procedure is applied by Abadie *et al.* (2015).

that minimizes the MSPE in the second half, the validation period. As shown in this figure, the synthetic control provides a good fit for employment trends in tourism in the validation period. Figure 6b displays the employment trends in tourism for Salta and several versions of synthetic Salta that result from 15 different partitions of the pretreatment period.[40] As shown in this figure, this cross-validation procedure to choose V weights produces results that are almost identical to the results obtained in Section 5. The darker lines correspond to estimates using a longer training period.

Figure 6: Cross-validation procedure to choose V weights



(a) Training and validity period

(b) Trends.

C.7 SCM and Diff-in-Diff

Even though the SCM chooses the optimal weights to minimize the pretreatment MSPE for tourism employment between Salta and its synthetic counterpart, there might still be differences in levels in the pretreatment period. Consequently, to account for this potential problem, we also use a differences-in-differences approach (Diff-in-Diff); that is, we subtract pretreatment differences from post-treatment differences between Salta and synthetic Salta. [41] Then, to obtain the TDP's impact on tourism employment, we compute:

$$\widehat{\beta}_{St} = \left(Y_{St} - \sum_{j=1}^{J} w_j^* Y_{jt} \right) - \frac{1}{T_0} \sum_{t_0=0}^{T_0} \left(Y_{St_0} - \sum_{j=1}^{J} w_j^* Y_{jt_0} \right) \qquad (2)$$

for $t \epsilon \{T_0 + 1, ..., T\}$.

The first term of equation (2) is the difference between Salta and its synthetic counterpart after the TDP, and the second term is the same difference but averaged for the pretreatment period. Note that the second term of the equation approximates zero when the synthetic control unit adjusts better to tourism employment in Salta before the TDP's implementation.

As an additional robustness check, we apply this post-SCM correction to all our results. In practical terms, if the SCM works well, this correction implies only subtracting a small pretreatment average difference between the real unit and its synthetic

[40] Partitions result from setting the threshold in the months of June and December from June 1996 to December 2002.

[41] We follow Garcia Lembergman *et al.* (2015).

counterpart to the effect (gap) estimated through SCM. Results are robust (remains unchanged) to the inclusion of this Diff-in-Diff correction.

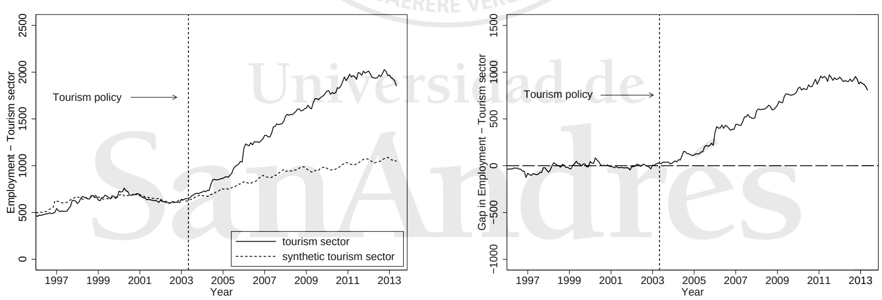
C.8 An Alternative Synthetic Control Group

So far, the empirical analysis has focused on the comparison of tourism employment between Salta and synthetic Salta constructed on a donor pool of non-treated Argentinean provinces (within the same sector). However, another way to construct a synthetic tourism sector for Salta is using other sectors from different provinces as the donor pool. In this case, the number of control units in the donor pool rises considerably (to around 900 sector-province units).[42]

Figures 7a and 7b show the evolution of the employment gap between the tourism sector and this second synthetic tourism sector. This gap corresponds to a TDP impact of around 12.6 percent per year between 2003 and 2013. This alternative synthetic sector is mainly a combination of sectors from Jujuy (other business activities, maintenance and repair of motor vehicles, real estate activities on a fee or contract basis, wholesale, machinery, equipment and supplies, repair of personal and household goods, other mining and quarrying, and activities auxiliary to insurance and pension funding) and Tucumán (manufacture of wood and wood and cork products except furniture, other mining and quarrying, and activities auxiliary to insurance and pension funding).[43]

Figure 7: Tourism sector vs. synthetic tourism sector

- Donor pool of other sectors-provinces-.



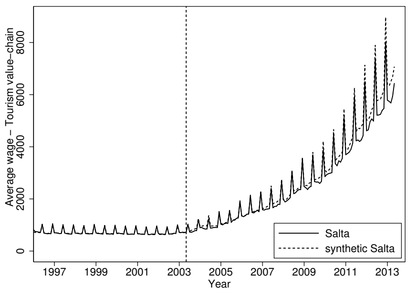
(a) Trends in employment (b) Employment gap

[42] We do not include other sectors from Salta, and the tourism sector and the main tourism-related sectors of other provinces previously discarded.

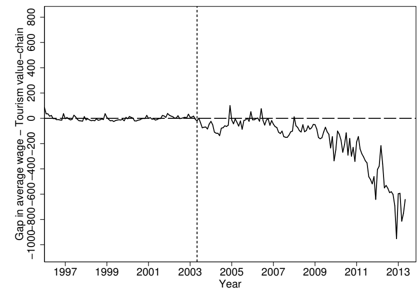
[43] We run the main placebo tests (C.1 tests) for this alternative. We obtained a *p-value* of 0.005 for the case of sector-province donors. For the sake of brevity, we do not present the graphs of these tests in the paper.

D Average Wages

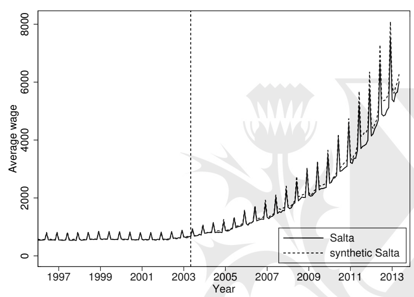
Figure 1: The impact of TDP on average wage: Salta vs. synthetic Salta



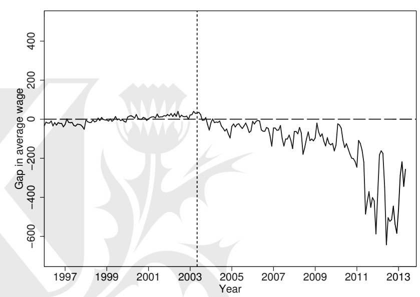
(a) Trends in tourism value-chain



(b) Gap in tourism value-chain



(c) Trends in the overall economy



(d) Gap in the overall economy

II. Knowledge Spillovers through Labor Mobility: An Employer–Employee Analysis[1]

Abstract

Using a 16-year employer–employee panel dataset that contains the entire population of firms and workers in Argentina, this paper provides evidence of the benefits of public support for firm-level innovation for the firms that received support, the workers who were employed by them, and the firms that hired beneficiary workers. The results confirm that participant firms improve their performance and generate valuable productive knowledge, which spills over to workers who directly participated in the program and is diffused through labor mobility to other firms. The worker-level results show that workers exposed to innovation projects receive higher wages. High-skilled workers receive most of the benefits from exposure to innovation, and the wage premium is higher for workers who moved to other firms. At the firm level, the paper provides evidence that hiring workers previously exposed to innovation projects is associated with an increase in firm performance. The findings suggest that labor mobility is an important mechanism for transmitting knowledge between firms.

JEL Classification: D20, H43, J23, J24, J62, O30.
Keywords: Labor mobility, knowledge spillover, R&D, innovation, panel data, Argentina.

[1] The final version of this study was published in *The Journal of Development Studies* (2019) and coauthored by Victoria Castillo (OEDE), Lucas Figal Garone (UdeSA-IBD), Alessandro Maffioli (IDB), Sofia Rojo (Consultant), and Rodolfo Stucchi (IDB). We thank Joshua Angrist, Sebastian Galiani, Hugo Hopenhayn, Oliver Morrissey, Martin Rossi, Ernesto Stein, two anonymous referees, and the participants at the 20th LACEA Meeting, 3rd LACEA Labor Network Meeting, 13th Journees Louis-Andre Gerard-Varet Conference in Public Economics, 7th MEIDE, 6th Bolivian Conference on Development Economics, and at seminars at ILADES and the Inter-American Development Bank for useful comments. Laura Baensch and Maria Laura Lanzalot provided excellent research assistance. The views expressed in this paper are those of the authors and do not represent those of the Inter-American Development Bank Group, its respective Board of Directors or the countries they represent, or the Argentinian Ministry of Labor, Employment, and Social Security. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors. The usual disclaimers apply.

1. Introduction

Knowledge is key to economic growth, at both the micro and macro levels. Endogenous growth theory and many empirical studies have demonstrated how knowledge accumulation boosts growth and productivity in both developed and emerging economies (Romer, 1990; Grossman & Helpman, 1991a, 1991b; Aghion & Howitt, 1992; Parente & Prescott, 1994; Jones, 2005). At the firm level, investments in knowledge and innovation have been shown to have significant positive effects on various measures of performance (Hall & Maffioli, 2008; Hall and Lerner, 2010; Keller, 2010; Doraszelski and Jaumandreu, 2013; Crespi et al., 2014).

Thus, both firms and governments invest in knowledge. Firms adopt different strategies to increase their knowledge base, including by financing research and development (R&D) activities (the “make” strategy) and/or by acquiring knowledge embedded in physical and human capital (the “buy” strategy). Governments typically invest in knowledge as a public good, by funding the generation and diffusion of scientific knowledge and supporting the private sector when market failures lead to sub-optimal private investment in knowledge and innovation.

Knowledge accumulation is intrinsically tied to human capital development. Knowledge is always at least partially embedded within firms’ human capital, particularly among high-skilled workers who more directly participate in knowledge-generation activities. Therefore, the mobility of human capital is one of the main sources of knowledge diffusion between countries, regions, industries, value chains, or firms (Fosfuri et al., 2001; Rao & Drazin, 2002; Kim & Marschke, 2005; Gorg & Strobl, 2005; Moen, 2005; Boschma et al., 2009; Maliranta et al., 2009; Balsvik, 2011; Filatotchev et al., 2011; Stoyanov & Zubanov, 2012; Poole, 2013).[1]

While knowledge diffusion is certainly beneficial and has desirable social returns, it usually has no positive effects – and sometimes even has negative effects – on the returns to the firms that invested in creating the knowledge and building human capital in the first place. For this reason, firms operating in competitive knowledge-based industries often attempt to minimize knowledge diffusion and go to great lengths to retain top talent. For instance, employers may use intellectual property clauses or non-disclosure agreements to prevent employees from sharing any knowledge acquired during their tenure with competitors. Or, as happened in the United States, firms may agree not to hire each other’s employees, an anti-poaching scheme that resulted in Apple, Google, Adobe, and Intel paying a $415 million settlement to tech workers in 2015. In today’s Silicon Valley, the competition for tech talent is so intense that firms offer astronomically high salaries, signing bonuses, and stock options to attract employees, followed by retention bonuses and other perks to get them to stay.

Likewise, since firms cannot monetize the social benefits of knowledge diffusion, market incentives are insufficient to produce socially optimal levels of private investment in knowledge. Public policies to support business R&D and innovation may be justified on these grounds.

If not the original investor, who is realizing the material benefits of knowledge diffusion through labor mobility? Workers can monetize some of the benefits of investments in knowledge by negotiating higher salaries from new employers who are interested in the knowledge they bring. The new employer could also partially monetize the benefits of accessing knowledge at a lower cost if the higher salary they pay to attract talent does not fully capture the value of the knowledge.

In this paper, we study how the generation and diffusion of knowledge affects both firms and workers in Argentina. Using the entire population of firms and employees between 1997 and 2013 and a sample of 669 firms supported by a public innovation promotion program (the Argentine Technology Fund, FONTAR), we examine the program's effects on: (1) the performance of firms generating knowledge (i.e., the firms that participated in the program and invested in innovation); (2) their employees; and (3) the firms that hire employees from the original investors in innovation.

The FONTAR program provides financial support to firms with innovation projects aiming to "generate knowledge that is new to the market"; i.e., knowledge that is valuable to them and potentially to other firms in the Argentinean economy. These projects include the development of new products, devices, materials or services, the construction of prototypes and the implementation of pilot tests. Innovation programs including FONTAR have been widely evaluated and have been proven to be effective at increasing investment in knowledge and enhancing firm performance.[2]

Our analysis confirms that the innovation supported by participation in FONTAR has helped firms improve their performance and generate valuable productive knowledge, which has spilled over to workers who directly participated in the program and has been diffused through labor mobility to other firms.

Regarding effects on employees, we find strong evidence that workers exposed to innovation projects received higher wages. FONTAR workers who stayed at a firm after it participated in the program received higher wages – as did workers who moved to other firms. These findings support the premise that the knowledge acquired through the exposure to innovation was embedded in human capital, had a recognizable market value and might be transferred to other firms. Our results also show that high-skilled workers receive most of the benefits from exposure to innovation, and that the wage premium is higher for workers who moved to other firms.

Finally, we find that companies that hired workers from FONTAR-supported firms significantly improved their performance, which confirms the knowledge diffusion hypothesis. The effects were even larger for firms that hired skilled workers.

To further explore the effects on the performance of receiving firms, we break down the average wage between increases in average wage due to: (1) the hiring of workers with higher wages (changes in skill composition); and (2) the increase in the wages of workers who stay at the firm (changes in marginal productivity). We find that the second term explains most of the increase in the average wage paid by receiving firms, and this effect increases over time. This finding can be interpreted as evidence that the effect of hiring a knowledge carrier on a firm's productivity dominates the change in its skill composition, which reinforces the improvements found in firm performance.

The rest of the paper is organized as follows. Section 2 describes the FONTAR program and its effects on the performance of participant firms. Section 3 discusses a simple framework for labor mobility and knowledge diffusion. Section 4 analyses the effects of exposure to innovation on worker wages. Section 5 shows the spillover effects through labor mobility on the performance of receiving firms. Finally, section 6 concludes.

2. Public support for R&D: the case of FONTAR

2.1. FONTAR

FONTAR began providing financial – reimbursable and non-reimbursable – support to R&D projects in 1995. It was the first program of its kind in Latin America, and despite numerous institutional and economic changes that have occurred over the years, it remains one of the pillars of firm-level innovation support in Argentina.

To select participants for the program, an ad hoc evaluation committee analyzes each firm's proposal, as well as its capacity (i.e., human capital and infrastructure) to carry out the project in a timely manner. The committee assesses the proposals based on their technical quality and feasibility as well as their economic and development impact. Priority is given to projects designed to introduce new innovations to the market in order to generate broader economic impact.[3]

FONTAR finances up to 80% of the costs of R&D projects; funds are disbursed only after the completion of the corresponding stage of the project.[4] This ensures that supported firms undertake approved R&D investments. Projects must be completed within two years and firms cannot have more than one active project at a time.

The fundamental premise underlying R&D subsidies such as those provided by FONTAR is that government intervention can be beneficial if profit-driven actors underinvest in R&D from a social welfare perspective due to the presence of spillovers associated to the 'public good' nature of knowledge (Steinmueller, 2010). If knowledge is in fact a non-rival and non-excludable good,[5] then a firm's rivals may be able to free-ride on its investments. These spillovers may create a gap between private and social returns, and a disincentive to privately invest in knowledge production.

Other market failures, including asymmetric information and uncertainty, affect the financing of innovation activities. R&D projects are different from other investments in three main ways (Hall and Lerner, 2010): (i) the returns on R&D investments are more uncertain and take longer to materialize; (ii) innovators may be reluctant to disclose information about their projects due to the risk of spillovers; and (iii) R&D investments normally involve intangible assets that have very limited use as collateral. For these reasons, firms without deep pockets may find it difficult to access financing for innovation projects, even when these have positive expected private rates of return. Thus, some potentially profitable projects will never be carried out.

2.2 Previous evidence on the effects of R&D subsidies

Most of the empirical literature has measured the impact of R&D subsidies in terms of *input additionality* – i.e., the extent to which subsidies crowd in private R&D investment. The implicit assumption underlying this approach is that, if subsidies are effectively targeted to ease the market failures that affect investments in innovation activities, they will allow firms to pursue projects that they would not have implemented otherwise.

Zuniga-Vicente et al. (2014) conducted one of the most recent and comprehensive reviews of the impact of R&D subsidies on private R&D investments. They document the results of 76 studies carried out at the firm level since the early 1960s. Although the studies are not fully comparable, a general pattern clearly emerges: in 60% of the cases, the crowding-in hypothesis cannot be rejected. The rest of the studies find either crowding-out or non-significant effects (20% each).

As in other regions, most of the studies conducted in Latin America and the Caribbean have evaluated the effect of R&D subsidies on private R&D investment. Summarizing 16 studies in Latin America, Crespi et al. (2014) and Figal Garone and Maffioli (2016) show that in most cases, subsidies stimulated R&D investments.

Although there is less proof of the effect of R&D subsidies on *outputs* – either innovation or productivity – the available empirical evidence tends to confirm that R&D subsidies are effective. Hall and Maffioli (2008) summarized the evidence of the effectiveness of several innovation programs in Latin America and found that firms participating in such programs are able to create new productive knowledge. Beneficiary firms can make investments in knowledge that would not be possible otherwise, which significantly affects their adoption of new products and processes, as well as their overall performance.[6]

Chudnovsky et al. (2006) and Binelli and Maffioli (2007) found that FONTAR had a significant multiplier effect on private investment in R&D. They also found robust evidence that the program has been effective at increasing knowledge and innovation within participating firms, in terms of both process and product innovation. The lack of a sufficiently long panel of data prevented these authors from assessing the program's effect on firm performance.

2.3 Data and descriptive statistics

We use an employer–employee panel dataset that contains annual information for the entire population of firms and employees in Argentina between 1997 and 2013. This dataset was constructed by the Observatory of Employment and Firm Dynamics (OEDE) at Argentina's Ministry of Labor, Employment, and Social Security by combining the social security data of the population of formal firms and their formally employed workers with the administrative records of the General Customs Bureau of the Federal Tax Administration.

This dataset covers the primary, manufacturing, and services sectors, and has firm-level information on age, location, industry, type of corporation, whether a firm is multinational, number of employees, average wages, and value of exports (see Annex A). To assess the effect of FONTAR, we combined this dataset with the program's administrative records, which provide information about the firms that received support between 1998 and 2006 (see Annex A and Table B2 in Annex B for details).

Our dataset contains information on 1,571,969 firms between 1998 and 2013 (10,100,174 firm-year observations). Given that FONTAR targeted small and medium-sized enterprises, we dropped micro and large firms – firms with fewer than five or more than 500 employees. This restriction leaves 255,261 firms and 2,028,334 firm-year observations for analysis (see Table B1 in Annex B). For our study, "Rest of firms" is a pure comparison group of firms that did not participate in FONTAR or hire workers from firms supported by the program.

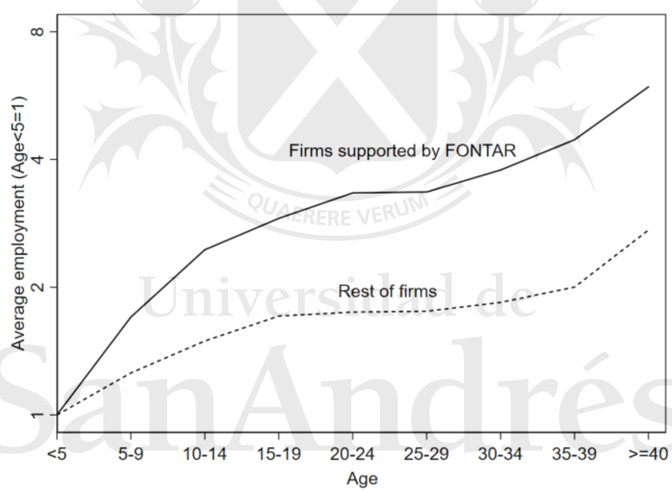
Table 1 reveals that firms supported by FONTAR are, on average, larger, older, pay higher wages, have a higher probability of exporting (and export more), and are more likely to hire workers than other firms in Argentina. This is also clear if we look at their long-term growth. Hsieh and Klenow (2014) found that while in the United States, the average 40-year-old plant employs more than seven times as many workers as the typical plant that has been operating for five years or less, surviving plants in India and Mexico exhibit much slower growth, roughly doubling in size over the same age range. Figure 1 shows similar differences between the dynamics of FONTAR firms and the rest of firms. While the dynamics of employment in

FONTAR firms are close to those of U.S. firms, the rest of firms follow a pattern closer to that observed in India and Mexico.

Table 1. Firm-level descriptive statistics, 1998–2013

	FONTAR firms			Rest of firms		
Variables	Obs.	Mean	SD	Obs.	Mean	SD
= 1 if survives	9,541	0.99	0.09	1,880,016	0.94	0.23
= 1 if exporting	9,541	0.51	0.50	1,880,016	0.06	0.23
Value of exports, if > 0 ('000 US$)	4,862	2,616	11,637	104,699	1,401	20,778
Value of exports ('000 US$)	9,541	1,333	8,409	1,880,016	78	4,914
Number of employees	9,541	72	96	1,880,016	21	36
Age	9,541	22	16	1,880,016	15	15
= 1 if multinational	9,541	0.03	0.17	1,880,016	0.01	0.07

Figure 1. Firm employment by age



2.4 The effects of FONTAR on firm performance

Since firms were selected to participate in FONTAR based on whether their proposed projects aligned with the program's objectives and had the highest probability of success, the better performance found in Table 1 and Figure 1 cannot necessarily be attributed to the firm's participation in the program. Estimating the effect of the program requires controlling for selection bias.

If this bias is related to observable factors, it can be reduced in a simple regression framework by simply including those factors as control variables in the regression. In this case, however, some differences between the groups of firms may also be related to unobservable (or unobserved) factors. A major advantage of using longitudinal firm-level data sets is that it allows us to account for unobservable factors that may affect both the outcome and participation in the program. We use the following fixed-effect linear regression model for the firm-level estimations:

$$Y_{i,p,s,t} = \rho D_{i,p,s,t\text{-}1} + \beta X_{i,p,s,t} + \epsilon_i + \epsilon_t + \epsilon_{p,s,t} + \epsilon_{o,t} + \varepsilon_{i,p,s,t}, \quad (1)$$

where $Y_{i,p,s,t}$ represents the set of outcomes to be considered for firm i, in province p, belonging to industry s, in year t. Firm fixed effects ϵ_i fully absorb any permanent heterogeneity at the firm level, and ϵ_t represents yearly shocks that affect all firms. Regarding the interaction terms, $\epsilon_{p,s,t}$ are province-industry-year effects (i.e., time-specific shocks that affect the outcomes of all firms in province p and industry s), and $\epsilon_{o,t}$ is a vector of two interaction terms that includes the type of corporation-year and multinational-year effects.[7]

$D_{i,p,s,t}$ is a binary variable that takes a value of 1 the year firm i participates in the program and for each subsequent year. Therefore, ρ represents the parameter of interest and captures the long-term effect of participating in FONTAR. Finally, $X_{i,p,s,t}$ is a vector of time-varying control variables at the firm level, and $\varepsilon_{i,p,s,t}$ is the usual error term clustered at the industry level and assumed to be uncorrelated with $D_{i,p,s,t-1}$.[8]

Table 2 shows the estimation of the effects of the FONTAR program on the long-term performance of participant firms.[9] Firms that participated in FONTAR increased their probability of surviving in the long run by 3.2 percentage points. They also improved their probability of exporting by 5.8 percentage points and, if they were already exporting, the value of their exports improved by 34.5%. They also increased employment by 28.4%.[10] The program also had a clear positive effect on wages. Considering the control mean and a maximum post-intervention period of 15 years, we obtain an average annual effect of 1.2% on average wages.

Table 2. Long-term effects of public support to R&D

Dependent variable	= 1 if survives	= 1 if exporting	Exports if value > 0 (in logs)	# of employees (in logs)	Average wage
	[1]	[2]	[3]	[4]	[5]
Average effect	0.032***	0.058***	0.345***	0.284***	392.8***
	(0.003)	(0.016)	(0.065)	(0.047)	(51.04)
Number of observations	1,889,557	1,889,557	109,561	1,889,557	1,889,557
Number of firms	243,445	243,445	20,470	243,445	243,445
R-squared	0.406	0.698	0.795	0.747	0.792
Average of dependent variable in control group (no logs)	0.94	0.06	78,020	21	2,122

*Notes: (a) Estimates of fixed effects model. (b) All regressions include firm, year, province-industry-year, multinational-year and type of corporation-year fixed effects, age and age squared, a dummy variable that takes a value of 1 starting the year after the firm hired low-skilled workers and for all subsequent years, and a dummy variable that takes a value of 1 starting the year after the firm hired high-skilled workers and for all subsequent years. (c) Standard errors clustered at the industry level. (d) ***, **, * statistically significant at 1%, 5%, and 10%.*

These findings confirm that FONTAR support has generated additional efforts to create new and relevant knowledge through innovation projects, which is then reflected in firms' longer-term survival, improved export profiles, and higher growth.

3. Labor mobility and knowledge diffusion: a simple framework

Are FONTAR-supported firms the sole recipients of the benefits of innovation? Since knowledge, particularly when acquired through R&D and innovation, is at least partially embedded in firms' human capital, other firms can benefit by hiring workers from these firms. This is likely the main reason why labor mobility is often identified as one of the most important vehicles through which formalized and tacit knowledge flow throughout a productive system. That is, new workers can bring new valuable knowledge to firms. For the purposes of our study, we rationalize the scenario under which workers exposed to innovation decide to move (stay), and firms decide to hire (retain) them.

Suppose that in period t some firms, denoted by F, participate in a public program that allows them to carry out R&D activities and innovation projects that would not have been feasible otherwise. This program causes employees of F firms, especially high-skilled workers, to absorb knowledge related to the design and implementation of the innovation. If the program supports results in the development of projects that are "new to the market," workers' new knowledge is valuable to both their current employer and the market.

Assume that workers K of firm F acquire level τ of knowledge during the innovation process. In the next period, K workers can either stay with firm F or move to a new firm. Firms that may hire workers from firm F are called R. Given that a firm's knowledge is partially embedded in its human resources, this knowledge is carried to the new workplace when workers move to firm R.[11] Assuming that these workers are at least partially aware of the value of what they have learned during the innovation process, they might seek compensation for this newly acquired knowledge from either their current employer or from the market.[12]

For simplicity's sake, we assume that the workers' utility function depends only on their wages and not on mobility costs, i.e., that they will work for the firm that offers them the highest wage. If the workers negotiate with a new potential employer (denoted firm R), firm R must pay more than firm F, $\omega^F(\tau)$, in order to attract K workers. Assume the value to firm R of hiring K workers from firm F is $f^R(\tau)$. A necessary condition for firm R being willing to hire these workers is $f^R(\tau) - \omega^F(\tau) > 0$, i.e., firm R must gain some surplus in the minimum wage the worker is willing to accept in order to move. If the worker and firm R divide the surplus according to a Nash bargaining mechanism, the wage of the worker if hired by firm R will be given by:

$$\omega^R(\tau) = \omega^F(\tau) + \beta^R\left(f^R(\tau) - \omega^F(\tau)\right), \tag{2}$$

where β^R is the workers' negotiating power vis-à-vis firm R.

Similarly, the wage that firm F has to pay to retain its workers if they threaten to leave is given by:

$$\omega^F(\tau) = \omega^R(\tau) + \beta^F\left(f^F(\tau) - \omega^R(\tau)\right), \tag{3}$$

where $f^F(\tau)$ is the value to firm F of retaining the workers, β^F represents the workers' negotiating power with firm F. In this case, the necessary condition is given by $f^F(\tau) - \omega^R(\tau) > 0$.

The worker moves from firm F to firm R if $\omega^R(\tau) > \omega^F(\tau)$, i.e.:

$$\omega^F(\tau) + \beta^R\big(f^R(\tau) - \omega^F(\tau)\big) > \omega^R(\tau) + \beta^F\big(f^F(\tau) - \omega^R(\tau)\big). \tag{4}$$

If we assume a worker will stay in firm F if s/he receives the same wage that firm R would offer, firm F would not pay a wage higher than $\omega^R(\tau)$; therefore, the wage firm F would offer is $\omega^R(\tau)$. Consequently, we can substitute $\omega^F(\tau)$ in Equation (3) with the maximum wage firm F would be willing to pay in order to determine the conditions under which firm R is willing to pay more, i.e.:

$$f^R(\tau) - \omega^R(\tau) > \frac{\beta^F}{\beta^R}\big(f^F(\tau) - \omega^R(\tau)\big). \tag{5}$$

Hence, the worker will move to firm R if the surplus of wage $\omega^R(\tau)$ at that firm is larger than $\frac{\beta^F}{\beta^R}\big(f^F(\tau) - \omega^R(\tau)\big)$. The greater the worker's contribution to the production of firm R, and the higher the worker's negotiating power with that firm, the higher the probability that the worker will move to firm R. However, the higher the contribution of the worker to firm F and the higher his or her negotiating power is with that firm, the more likely it is that the worker will stay with firm F.

Note that firm F will not compete for the worker by paying $\omega^R(\tau)$ only if $f^F(\tau) - \omega^R(\tau) < 0$. If that is the case, the wage firm R would offer is $f^F + \varepsilon$, with $\varepsilon > 0$. The worker will move to firm R if their contribution to that firm is larger than their contribution to firm F. In that case, it is necessary that $f^R > f^F$.

Therefore, workers are most likely to move when the knowledge they have acquired has a greater value to firm R than to firm F. This is likely to happen when workers participate in innovation projects that are "new to the market." In these cases, while before project implementation the value of the knowledge potentially acquired by K workers is the same for both firms R and F, after the project is implemented, the value of this knowledge could be much higher for firm R. Under the simplifying assumption that firm F can codify the knowledge produced during the innovation process and fully embed it into its production function, the cost of losing K workers would be related only to the new skills acquired by these workers during the process.[13] Yet, the benefit to firm R of hiring K workers would be related not only to their increased skills, but also to the value of (at least part of) the knowledge produced during the innovation process.

If the innovation projects supported by the program are new and relevant to the market, and at least partially codifiable by the innovative firms, K workers would have a stronger incentive to move (and firms R would have stronger incentives to hire them) than firms F would have to retain them. If this were the case, we would see a high level of mobility of K workers from firms F to firms R. However, this mobility could be lower when: i) a relevant portion of the knowledge produced is non-codifiable (tacit) or more specific; ii) there are high mobility costs or significant information asymmetries; or iii) the markets are highly concentrated.

4. The effect of innovation support on workers' wages

If the knowledge generated through the innovation projects supported by FONTAR is at least partially embedded in human capital, both innovating firms and other firms will be willing to pay higher wages to workers who were involved in those projects. In our framework, a higher wage reflects a higher value for the firm of $f(\tau)$. Therefore, by estimating the effect on wages, we can infer the program's effect on the value of the marginal product of workers for the firms.

We estimate the effect of the program on workers' wages using the following equation:

$$W_{j,i,p,s,t} = \rho D_{j,i,p,s,t\text{-}1} + \gamma X_{j,i,p,s,t} + \beta X_{i,p,s,t} + \epsilon_j + \epsilon_i + \epsilon_t + \epsilon_{p,s,t} + \epsilon_{o,t} + \varepsilon_{j,i,p,s,t}\,, \qquad (6)$$

where $W_{j,i,p,s,t}$ is the monthly nominal wage of worker j in period t, worker fixed effects ϵ_j fully absorb any permanent heterogeneity at the worker level, and $X_{j,i,p,s,t}$ is a vector of time-varying control variables at the worker level that includes the age and gender of the worker and their tenure at the firm. $D_{j,i,p,s,t}$ is a binary variable that takes a value of 1 after a worker j is exposed to FONTAR – i.e., after the firm participates in the program. Therefore, ρ measures the effect of being exposed to innovation on workers' wages. We also estimate this equation separately for workers who stayed in a FONTAR firm and those who were hired by other firms.

Our dataset has employee-level information about wage, age, gender, and starting and ending dates of their employment (see Annex A for details). This information allows us to track the mobility of workers between firms. Between 1997 and 2013, labor mobility was relatively high, involving around 10% of total employment in Argentina every month. This implies that approximately 5% of employees left their jobs during this time and 5% filled them (Figure 2). One of the main characteristics of this high labor mobility was the short period of time that new workers remained in firms: nearly 40% of new workers left their firm within the first three months of employment, and nearly 60% did so during the first year.

Figure 2. Dynamics of private sector employment, average of monthly rates (1997–2013)

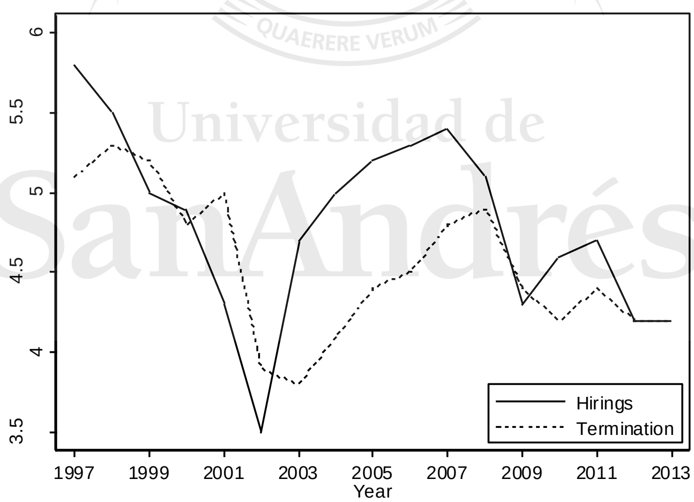


Table 3 shows that the mobility of FONTAR workers was also high: approximately 40% of these workers eventually moved to a different firm. When we restrict the analysis to workers who were exposed to the program for at least two years, mobility drops considerably. This pattern applies to both low- and high-skilled workers.

Table 3. The mobility of workers in firms supported by FONTAR

	Years in a FONTAR firm				Total
	< 2		≥ 2		
	#	%	#	%	#
All					
Stay in the firm	54,111	27%	143,507	73%	197,618
Move to other firms	79,691	60%	52,493	40%	132,184
Total	133,802	41%	196,000	59%	329,802
Low-skilled					
Stay in the firm	48,874	32%	102,195	68%	151,069
Move to other firms	72,542	63%	41,878	37%	114,420
Total	121,416	46%	144,073	54%	265,489
High-skilled					
Stay in the firm	5,237	11%	41,312	89%	46,549
Move to other firms	7,149	40%	10,615	60%	17,764
Total	12,386	19%	51,927	81%	64,313

Notes: High-skilled workers are defined as being in the top quartile of their current firm's wage distribution, while low-skilled workers are the rest of workers in the distribution.

Table 4 presents worker-level descriptive statistics. It confirms that both low- and high-skilled workers from FONTAR-supported firms have, on average, higher wages than those from other firms. However, FONTAR workers who moved to other firms have higher wages than those who stayed at the firm, which may signal a wage premium due to their mobility. Also, consistently with the mobility explanation, workers who stayed at FONTAR firms have longer tenures than those who left or those who work in non-FONTAR firms. Finally, while workers in each skill category have similar ages, those in FONTAR firms are more likely to be men.

4.1 All workers

We first analyze the effect on all workers from firms supported by FONTAR, including those who remained at the firm and those who moved to other firms after the innovation project. We group workers as either low or high skilled and compare each group with similar workers in the rest of firms – i.e., those that did not participate in the program or hire workers from FONTAR firms.

Table 5 reports the regression results for Equation (6). We find that both low- and high-skilled workers benefit from exposure to innovation activities (Columns [1] and [2]). However, the effect on high-skilled workers is greater. The average annual effect on wages for high- and low-skilled workers is almost 3% and 2%, respectively, compared to the control groups for a 15-year period after the intervention. This difference is consistent with the hypothesis that high-skilled workers acquire most of the knowledge related to the design and implementation of the innovation projects.

Table 4. Worker-level descriptive statistics, 1998–2013

Variables	Low-skilled workers Obs.	Mean	SD	High-skilled workers Obs.	Mean	SD
I. FONTAR workers						
Average monthly wage (LCU)	632,834	3,365	6,669	260,713	5,695	11,412
Tenure (months)	632,834	86	64	260,713	111	69
Age (years)	632,834	36	11	260,713	41	12
Gender (= 1 male, = 0 female)	632,834	0.80	0.40	260,713	0.87	0.33
II. FONTAR workers who stayed						
Average monthly wage (LCU)	394,946	3,271	3,365	181,175	5,262	7,601
Tenure (months)	394,946	101	68	181,175	125	70
Age (years)	394,946	36	12	181,175	42	12
Gender (= 1 male, = 0 female)	394,946	0.79	0.41	181,175	0.87	0.34
III. FONTAR workers who moved						
Average monthly wage (LCU)	237,888	3,521	9,973	79,538	6,681	17,142
Tenure (months)	237,888	63	48	79,538	78	54
Age (years)	237,888	35	10	79,538	40	11
Gender (= 1 male, = 0 female)	237,888	0.82	0.38	79,538	0.89	0.31
IV. “Rest of firms” workers						
Average monthly wage (LCU)	17,653,622	2,076	2,508	12,154,862	2,632	4,419
Tenure (months)	17,653,622	86	65	12,154,862	100	67
Age (years)	17,653,622	36	12	12,154,862	41	12
Gender (= 1 male, = 0 female)	17,653,622	0.66	0.48	12,154,862	0.72	0.45

Notes: High-skilled workers are defined as being in the top quartile of their current firm’s wage distribution, while low-skilled workers are the rest of workers in the distribution.

4.2 Workers who stayed at the firm

We then explore the effect on the wages of the sub-sample of workers who remained at the firms after the innovation project was implemented. In the regression, D takes a value of 1 for these workers after the firm participates in FONTAR and 0 before; D is coded 0 for workers in firms that did not participate in FONTAR or did not hire workers from FONTAR firms. Columns [3] and [4] in Table 5 show the results.

The average annual effect on high-skilled workers (2.6%) is more than twice the effect on low-skilled workers (1.2%). Thus high-skilled workers captured most of the knowledge and therefore benefited more from the improvement in firm performance generated by the project.

4.3 Workers who moved to other firms

What happened to the workers who eventually moved? To determine whether they took part of the knowledge created by the program with them, we explore the effect on wages for workers who were hired by other firms after participating in a FONTAR project.

We estimate Equation (6) comparing workers who left FONTAR firms – D takes a value of 1 after these workers were hired by other firms and 0 before – with similar workers from firms that did not participate in FONTAR or did not hire workers from FONTAR firms (in the regression, D always takes a value of 0 for these workers). Given that moving could increase the wages of workers independently of whether they were exposed to a FONTAR program, we include a dummy variable that takes a value of 1 after workers move to other firms. Failing to control for this factor might confound our effect of interest with wage improvements due to simple mobility.

Columns [5] and [6] in Table 5 show that workers who moved to other firms received a higher wage premium, confirming that the knowledge acquired through exposure to innovation has a recognizable market value. The estimates of the wages of high- and low-skilled workers are 6% and 5%,[14] i.e., around two and four times higher than those for similar workers who stayed in the FONTAR firm, respectively.[15] These findings show that knowledge spillovers associated with labor mobility are at least partially internalized in the labor market.

5. Knowledge spillovers through labor mobility

5.1. The effects on receiving firms

To identify the effect of knowledge spillovers on firm performance, information is needed at both the firm and employee levels, which makes the employer–employee structure of our data extremely valuable. It allows us to track the mobility of workers and to identify the receiving firms – those that may have benefited from the program indirectly by hiring workers previously employed in firms that developed an innovation project using FONTAR funds.

We restrict the analysis to workers who have accumulated knowledge (i.e., those who were employed in a FONTAR firm for at least two years after the firm received program support) in order to measure knowledge transfer. We define *receiving firms* as those that: (i) never participated in FONTAR but (ii) hired employees who were employed by a FONTAR firm for at least two years after the firm received program support.

Table 6 displays descriptive statistics for the groups of interest. Receiving firms are, on average, larger, older, pay higher wages, have a higher probability of exporting (and export more), and are more likely to hire workers than other firms in Argentina. Receiving firms of high-skilled workers also tend to have higher outcome indicators than those supported by FONTAR and are more likely to be multinationals. This suggests that high-skilled switchers tend to go to larger firms, which presumably are more productive. By contrast, firms that receive low-skilled workers tend to have lower performance indicators than FONTAR firms. This means that, on average, low-skilled switchers tend to move to firms with similar or lower performance levels than their current firm. Finally, Figure 3 incorporates receiving firms into Figure 1. While the dynamics of firms that receive high-skilled workers are closer to the dynamics of FONTAR firms, the dynamics of firms that receive low-skilled workers are more similar to those of non-FONTAR firms.

Table 5. Effect on workers' wages

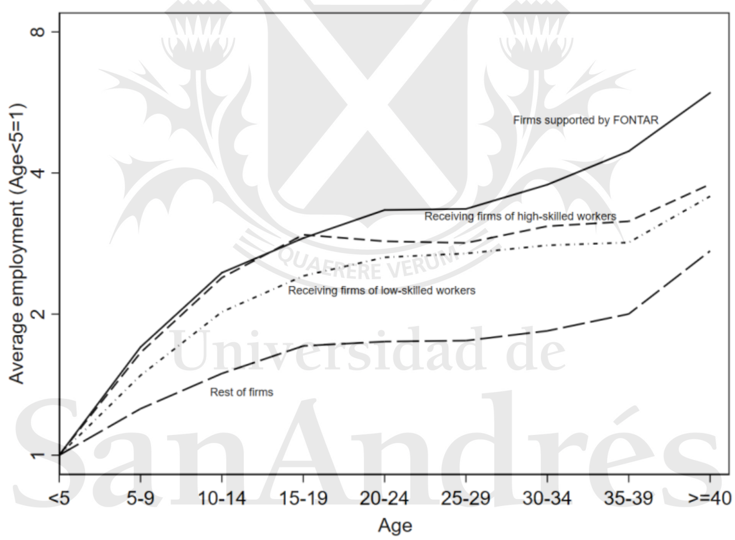
Dependent variable	Wage (LCU)					
	All		Workers who stayed		Workers who moved	
	Low-skilled [1]	High-skilled [2]	Low-skilled [3]	High-skilled [4]	Low-skilled [5]	High-skilled [6]
Average effect	650.9***	1,077***	368.8***	1,046***	1,373***	2,011**
	(139.4)	(146.1)	(63.90)	(134.4)	(428.2)	(873.8)
Number of observations	18,286,456	12,415,575	18,048,568	12,336,037	17,891,510	12,234,400
Number of workers	3,346,585	1,664,974	3,320,872	1,658,051	3,262,477	1,632,578
Number of firms	498,059	715,589	485,596	711,064	498,035	715,461
R-squared	0.827	0.722	0.876	0.737	0.825	0.720
Average of dependent variable in control group (no logs)	2,076	2,632	2,076	2,632	2,076	2,632

Notes: *(a) Estimates of fixed effects model. (b) All regressions include firm, year, province-industry-year, multinational-year and type of corporation-year fixed effects, age and age squared, and worker fixed effects, age, age squared, sex and tenure, and a dummy variable that takes a value of 1 starting the year after the worker moved to another firm and for all subsequent years. (c) Standard errors clustered at the industry level. (d) ***, **, * statistically significant at 1%, 5%, and 10%.*

Table 6. Firm-level descriptive statistics, 1998–2013

Variables	Receiving firms of low-skilled workers			Receiving firms of high-skilled workers			Rest of firms		
	Obs.	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD
= 1 if survives	102,867	0.98	0.13	35,910	0.99	0.11	1,880,016	0.94	0.23
= 1 if exporting	102,867	0.16	0.37	35,910	0.29	0.46	1,880,016	0.06	0.23
Exports, if > 0 ('000 US$)	16,512	3,776	40,893	10,593	5,750	24,000	104,699	1,401	20,778
Exports ('000 US$)	102,867	606	16,400	35,910	1,696	43,947	1,880,016	78	4,914
Number of employees	102,867	68	101	35,910	99	131	1,880,016	21	36
Average monthly wage (LCU)	102,867	2,693	3,220	35,910	3,546	4,503	1,880,016	2,122	2,698
Age	102,867	17	16	35,910	19	18	1,880,016	15	15
= 1 if multinational	102,867	0.03	0.18	35,910	0.09	0.29	1,880,016	0.01	0.07

Figure 3. Firm employment by age



To determine the spillover effect, we estimate Equation (1) comparing receiving firms and non-FONTAR firms (rest of firms).[16] Given that new workers could improve the performance of firms regardless of having been exposed to the FONTAR program, we include a dummy variable that takes a value of 1 after the firm hires workers. Failing to control for this factor might confound spillovers with improvements in firm performance due to better matching between workers' skills and the firm's needs.

The results in Table 7 show that firms that hired workers who were previously exposed to innovation projects (knowledge carriers) improved their performance along several dimensions. Specifically, receiving firms of high- and low-skilled workers increased their probability of survival (1.0 vs. 0.4 percentage points, respectively), probability of exporting (1.8 vs. 1.5 percentage points), the value of exports (for firms that exported) (32% vs. 14.6%), employment (28.5% vs. 20%), and average wages (28.5% vs. 12.7%). These magnitudes are generally lower than those obtained by FONTAR firms, but are still relevant and statistically significant.[17]

The spillover effects are higher when the firm hired high-skilled workers. This finding, together with the worker-level findings, shows that high-skilled workers absorbed and transmitted more knowledge than their low-skilled counterparts, and internalized the benefits of the innovation project to a greater extent. These results also show that knowledge spillovers through labor mobility are only partially internalized in the labor market.[18]

Table 7. Spillover effects through labor mobility

Dependent variable	= 1 if survives	= 1 if exporting	Exports if value > 0 (in logs)	# of employees (in logs)	Average wage
	[1]	[2]	[3]	[4]	[5]
Receiving firms of low-skilled workers					
Average spillover effect	0.004***	0.015***	0.146***	0.201***	269.3***
	(0.002)	(0.004)	(0.039)	(0.013)	(48.34)
Number of observations	1,982,883	1,982,883	121,211	1,982,883	1,982,883
Number of firms	251,620	251,620	22,133	251,620	251,620
R-squared	0.404	0.700	0.793	0.760	0.796
Control mean (no logs)	0.94	0.06	78,020	21	2,122
Receiving firms of high-skilled workers					
Average spillover effect	0.010***	0.018***	0.321***	0.285***	605.1***
	(0.003)	(0.005)	(0.061)	(0.016)	(77.02)
Number of observations	1,915,926	1,915,926	115,292	1,915,926	1,915,926
Number of firms	245,748	245,748	21,175	245,748	245,748
R-squared	0.406	0.702	0.797	0.755	0.794
Average of dependent variable in control group (no logs)	0.94	0.06	78,020	21	2,122

*Notes: (a) Estimates of fixed effects model. (b) All regressions include firm, year, province-industry-year, multinational-year and type of corporation-year fixed effects, age and age squared, a dummy variable that takes a value of 1 starting the year after the firm hired low-skilled workers and for all subsequent years, and a dummy variable that takes a value of 1 starting the year after the firm hired high-skilled workers and for all subsequent years. (c) Standard errors clustered at the industry level. (d) ***, **, * statistically significant at 1%, 5%, and 10%.*

5.2. Understanding the change in wages in receiving firms

Understanding the effect on wages is crucial because, as we noticed in the analytical framework, wages largely reflect the value of the marginal productivity of the workers and the availability of alternative employment. This is the case because a firm will pay higher wages only if the value of the worker's marginal productivity is higher than or equal to his or her wage and the worker has a better outside option. Our estimates of the effect on wages at the worker level suggest that these two conditions occurred after the firm's participation in an innovation project. The marginal productivity of workers increased (i.e., innovating firms are willing to pay more to retain workers), and workers have better outside options (i.e., other firms are willing to pay more for the knowledge generated by the innovation project).

At the firm level, we also found that firms pay a higher average wage after they participate in an innovation project financed by FONTAR or after they hire workers previously employed by a FONTAR firm. The latter increase in average wage can be confounded with a change in the skill composition. If a firm is hiring skilled workers, it is likely to observe an increase in its average wage independently of the effect of these workers on the firm's productivity. Given that receiving firms are hiring knowledge carriers, to see if the effect on the average wage is due to an improvement in marginal productivity or the change in the skill composition, we decompose the change in the average wage paid by firms into these two components and then estimate Equation (6) on each component.

The change in the average wage paid by each firm can be broken down into changes due to (1) shifts in the wages of workers who remained at the firm or (2) hiring/firing workers. These terms allow us to identify two important sources of wage variation at the firm level. While the first is more closely related to changes in marginal productivity, the second is related to changes in the firm's skill composition.

Formally, let the average wage that firm i pays to workers in period t be $W_{it} = \sum_{j=1}^{N_{it}} \frac{1}{N_{it}} \omega_{jt}$, where ω_{jt} is the wage of worker j in period t, and N_{it} the number of workers in firm i in period t. The change in the average wage of each firm i can be broken down using a similar approach to the one used to study the change in aggregate productivity (see, for example, Foster et al., 2008). The average wage of firms' decomposition is given by:

$$\Delta W_{it} = \sum_{j \in C} s_{jt-1}\, \Delta\omega_{jt} + \sum_{j \in C} s_{jt}\, (\omega_{jt-1} - W_{it-1}) + \sum_{j \in C} s_{jt}\, \Delta\omega_{jt} + \sum_{j \in N} s_{jt}\, (\omega_{jt-1} - W_{it-1}) - \sum_{j \in X} s_{jt-1}\, (\omega_{jt-1} - W_{it-1}),$$

where s_{jt} is the weight of worker i in the average wage and is equal for all workers in the firm, i.e., $s_{jt} = \frac{1}{N_{it}}$. The sets $C, N,$ and X represent the set of continuing, entering, and exiting workers, respectively.

This decomposition has five terms that represent the contributions of various components of the average wage of the firm. The first three terms measure the change in the average wage paid by firm i to workers who remain at the firm. The last two terms measure the change in the average wage paid to new workers and those who left the firm. If new workers are paid more than the firm's average wage level, then the average wage of firm i has increased, which could be the case if the firm hired high-skilled workers. Similarly, if workers who left the firm were paid a below-average wage, then the average wage has increased. This could be the case if the firm fired low-skilled workers. The sum of the last two terms reflects changes in the skill composition of the firm.

Table 8 illustrates that the rise in average wages is mostly due to an increase in the wages of workers who already worked at the receiving firms, rather than an increase in the wages of newly hired workers. This finding reveals that the increase in wages is mostly due to an improvement in marginal productivity rather than to a change in the skill composition. We find that the productivity term increases by nearly 2% and 1% in receiving firms of high- and low-skilled workers, respectively. By contrast, the effect on the firm's skill composition is significantly lower and similar between types of workers.

These results confirm our hypothesis that the knowledge previously acquired through exposure to an innovation project indirectly improves the performance of other firms. That is, the benefit of receiving a knowledge carrier is not only related to the increased skills of the hiring firm, but also (and mainly) to the contribution of the new knowledge to its production process.

Table 8. Productivity hypothesis vs. skill composition change

Dependent variable	Change in average wage	Productivity hypothesis	Skill composition hypothesis
	[1]	[2]	[3]
Receiving firms of low-skilled workers			
Average spillover effect	74.27***	53.89***	20.378**
	(12.03)	(12.37)	(5.837)
Number of observations	1,803,692	1,803,692	1,803,692
Number of firms	229,904	229,904	229,904
R-squared	0.233	0.236	0.195
Average of dependent variable in control group (no logs)	437	384	53
Receiving firms of high-skilled workers			
Average spillover effect	148.5***	122.9***	25.53**
	(20.78)	(16.92)	(10.10)
Number of observations	1,740,698	1,740,698	1,740,698
Number of firms	224,123	224,123	224,123
R-squared	0.231	0.233	0.198
Average of dependent variable in control group (no logs)	437	384	53

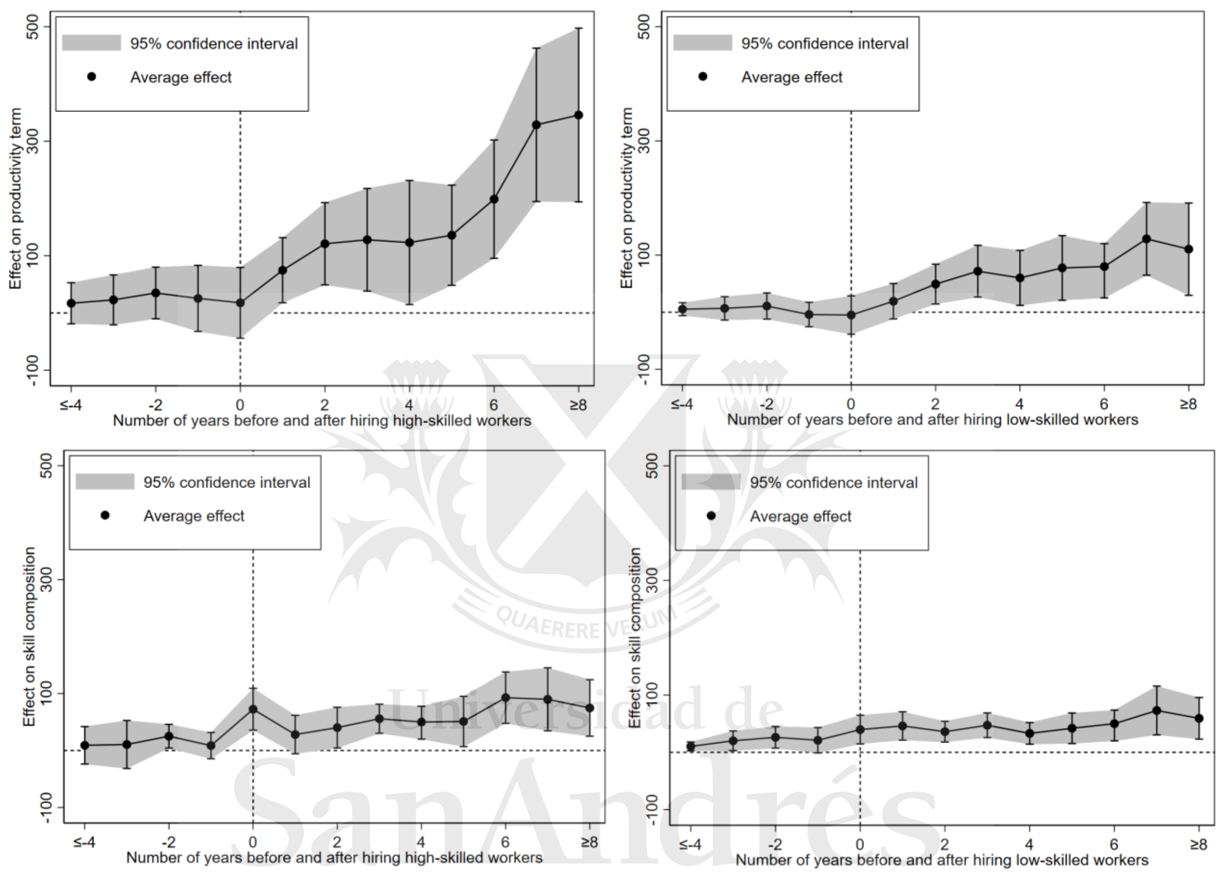
*Notes: (a) Estimates of fixed effects model. (b) All regressions include firm, year, province-industry-year, multinational-year and type of corporation-year fixed effects, age and age squared, a dummy variable that takes a value of 1 starting the year after the firm hired low-skilled workers and for all subsequent years, and a dummy variable that takes a value of 1 starting the year after the firm hired high-skilled workers and for all subsequent years. (c) Standard errors clustered at the industry level. (d) ***, **, * statistically significant at 1%, 5%, and 10%.*

Given that we observe receiving firms each year after they hired the knowledge carrier, we can estimate the dynamics of the effects on these two components from the decomposition of the average wage (see Figure 4). The spillover effect increases productivity at a growing rate for both types of receiving firms, as shown by the increasing coefficients on the dummy variables.

Also, we observe a positive effect on the skill composition of receiving firms that is likely related to the hiring of knowledge carriers. Finally, in line with previous findings, receiving high-skilled workers has a large impact on marginal productivity.[19]

Figure 4. Productivity hypothesis vs. Skill composition change

(a) Receiving firms of high-skilled workers *(b) Receiving firms of low-skilled workers*



6. Conclusions

This paper examines the effects of knowledge spillovers on firm performance and worker wages. We used participation in an R&D support program as a variation in knowledge accumulation and pinpointed the knowledge diffusion by tracking the mobility of workers among firms using a 16-year employer–employee panel dataset.

We organized our empirical analysis into two levels: firm- and worker-level effects. At the firm level, we estimated both the effect of the FONTAR support program and the effect of hiring workers previously exposed to R&D and innovation activities supported by FONTAR on different measures of firm performance. At the worker level, we estimated the effect of the program on the wages of all workers employed by a firm that participated in the program, those who stayed at the firm, and those who moved to other firms.

We find that new relevant productive knowledge is generated and diffused through labor mobility. We first confirm this finding by demonstrating the FONTAR program’s positive effects on the performance of supported firms. We also find that this new productive knowledge is applicable and beneficial to firms that access it through labor mobility, as shown by their improved performance.

Firms that hired workers previously exposed to the innovation program improved their performance after hiring them: they increased their survival probability, the probability of exporting, the value of their exports, the number of employees, and the average wages they pay. These effects are higher when hiring high-skilled workers.

At the worker level, our results are also consistent with the existence of knowledge spillovers. Our findings show that workers exposed to innovation benefit from it and increased their wages. These results confirm that workers acquired valuable productive knowledge that firms were willing to pay for in order to either retain or acquire them. We also find that the effect on wages depends on the workers' skill level and on whether they stay or move to other firms.

First, our results show that high-skilled workers absorb most of the knowledge related to innovation activities and benefit more from it. Second, workers who were exposed to the knowledge generation but then moved to other firms enjoy a higher wage premium compared to those who stayed. That is, receiving firms are willing to pay a wage premium to acquire knowledge carriers that is higher than the wage premium innovative firms want to pay to retain them. This finding further confirms the hypothesis that the innovation process generates valuable productive knowledge.

Our dataset has advantages as well as limitations. While it allows us to use the entire population of firms and workers in Argentina and identify an important source of knowledge diffusion (i.e., labor mobility), it does not contain data about occupations or innovation outcomes, or information we could use to construct a direct measure of firm productivity. Having detailed data on occupations would have allowed us to better group workers and define knowledge carriers, and to determine which workers were more involved in R&D activities. Similarly, data on innovation outcomes would have allowed us to further confirm the effectiveness of the FONTAR program, and productivity information would have enabled us to directly measure the program's impact on firm productivity.

Our findings clearly confirm the hypothesis that the FONTAR program generated valuable productive knowledge, which has spilled over to workers who directly participated in the program and has been diffused through labor mobility to other firms. These findings have two main policy implications. First, our results strongly support the most important justification of innovation policy: firms that invest in innovation do not reap the full benefits of their investment. Therefore, subsidies and matching grants should be used to promote knowledge creation and increase productivity. Second, because externalities in the form of spillover effects are often overlooked in *ex ante* cost–benefit analyses of this kind of instrument, decisions about the size of such interventions could be downward biased and lead to designing programs that are inconsistent with their potential social returns, with the result that they are likely to be undersized and underfunded.

Endnotes

[1] Other mechanisms of knowledge diffusion include geographic proximity (Audretsch & Feldman, 1996; Anselin et al., 1997; Fosfuri & Ronde, 2004) and/or technological proximity (Jaffe, 1986; Aw, 2002), inter-industry linkages (Bernstein & Nadiri, 1989; Paz, 2014), provision of goods and services (Bonte, 2008; Lopez & Yadav, 2010; Isaksson et al., 2016), equity and foreign direct investments (Kokko et al., 1996; Aitken & Harrison, 1999; Javorcik, 2004; Marin & Bell, 2006; Chudnovsky et al., 2008; Newman et al., 2015; Thang et al., 2016), common participation in associations and consortia (Gilbert et al., 2008; Chyi et al., 2012), and patent citations (Henderson et al., 1993; Thompson & Fox-Kean, 2005; Nelson, 2009; Murata et al., 2014).
[2] See Binelli and Maffioli (2007) for an evaluation of FONTAR's effect on innovation activities.
[3] During the period under analysis, Argentinian government resources were enough to fund all approved projects.
[4] The percentage financed varies according to the type of instrument.
[5] The seminal works by Nelson (1959) and Arrow (1962) maintain that, once produced, new knowledge is a *non-rival good*: it can be used simultaneously by many different firms. This characteristic is an extreme form of decreasing marginal costs as the scale of use increases: although the costs of the first use of new knowledge may be large since they include the costs of its generation, further use incurs negligible incremental costs (Aghion, David, and Foray, 2009). Knowledge is said to be *non-excludable* due to the difficulty and cost of trying to retain exclusive possession of it while using it.
[6] For a review, see also Hall and Lerner (2010) and Keller (2010).
[7] We use the STATA command "*reghdfe*" to absorb several high-dimensional fixed effects.
[8] For each estimation in this study, we test whether the trends in the treatment and control group of firms (or workers) were the same during the pre-intervention periods. We find that this was the case for our main outcomes of interest. For some estimations, this can also be seen in Figure 4 which shows how the effects were around zero and statistically non-significant during the pre-treatment period. For the sake of brevity, we do not present the tables of these tests in the paper. Results are available from the authors upon request.
[9] The sample for this estimation only includes FONTAR firms and those that did not participate in the program and did not hire workers from FONTAR firms (rest of firms).
[10] These estimates reflect average cumulative effects for the entire post-intervention period.
[11] Several studies have shown how high-skilled workers' job mobility facilitates the dissemination of embodied tacit knowledge (Almeida & Kogut, 1999; Maskell & Malmberg, 1999; Cooper, 2001; Power & Lundmark, 2004).
[12] While firms may try to prevent this by offering initial salaries that include the market value of the knowledge that workers are expected to acquire during the process, this value is hard to predict.
[13] The model assumes there are no negative effects on firms F resulting from the movement of their workers to firms R.
[14] These estimates reflect average cumulative effects for a maximum post-treatment period of 13 years.
[15] The wage premiums are doubled when the receiving firm is an exporting firm (see Annex Table B3). This result is in line with the findings in Araujo and Paz (2014).
[16] Maliranta et al. (2009) find that hiring workers prior engaging in R&D activities improves productivity and profitability of firms without R&D activity. They interpret this finding as a transmission of knowledge that can be transferred with little additional R&D effort. Similarly, Stoyanov and Zubanov (2012) find that firms that hired skilled workers increased their productivity.
[17] These estimates reflect average cumulative effects for a post-hiring period of 13 years.
[18] Since not every firm hired the same number of knowledge carriers, we also explored how the spillover effects vary according to the number of knowledge carriers as a proportion of total workers in receiving firms. The greater this proportion, the greater the number of possible workplace interactions and potential transfer of knowledge. For this calculation, instead of a dummy variable identifying receiving firms, the main explanatory variable is a continuous variable that measures the ratio of the number of knowledge carriers hired to the total number of workers when the knowledge carriers were hired. Therefore, this variable measures how firm performance changed according to variations in the intensity of the knowledge diffusion. Tables B4 and B5 in Annex B show results consistent with those in Table 7. As expected, the larger the number of knowledge carriers hired as a proportion of total workers, the greater the spillover effects on firm performance, *ceteris paribus*.
[19] Tables B4 and B5 in Annex B show that the cumulative spillover effects are maintained over time for all outcome variables analyzed.

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III. Public Support to R&D, Productivity, and Spillover Effects: Firm-level Evidence from Chile[1]

Abstract

This paper estimates the direct and spillover effects of two matching grants schemes designed to promote firm-level research and development (R&D) investment in Chile on firm productivity. Because the two programs target different kinds of projects—the National Productivity and Technological Development Fund (FONTEC) subsidizes intramural R&D, while the Science and Technology Development Fund (FONDEF) finances extramural R&D carried out in collaboration with research institutes—analyzing their effects can shed light on the process of knowledge creation and diffusion. The paper applies fixed-effects techniques to a novel dataset that merges several waves of Chile's National Manufacturing Surveys collected by the National Institute of Statistics with register data on the beneficiaries of both programs. The results suggest that while both programs have had a positive impact on participants' productivity, only FONDEF-funded projects have generated positive spillovers on firms' productivity. The analysis reveals that the spillover effects on productivity display an inverted-U relationship with the intensity of public support. Spillover effects were found to occur only if firms were both geographically and technologically close.

JEL Codes: D24, D62, H43, L60, O32, O38.
Keywords: R&D Subsidies, Productivity, Spillover Effects, Policy Evaluation, Chile.

[1] The final version of this study was published in *World Development* (2020) and coauthored by Gustavo Crespi (IDB), Lucas Figal Garone (UdeSA-IDB), Alessandro Maffioli (IDB), and Ernesto Stein (IDB). This study was developed as part of the ESW "Science, Technology and Innovation Policy Effectiveness: Moving the Frontier and Expanding External Validity" project coordinated by the Competitiveness and Innovation Division in collaboration with the Strategy and Development Department and the Research Department of the Inter-American Development Bank Group. We wish to thank Arun Agrawal and three anonymous referees for useful comments. Maria Laura Lanzalot, Santiago Llamas, Andrés Sebastián Mena, and Victor Zuluaga provided exceptional research assistance. The opinions expressed in this paper are those of the authors and do not necessarily reflect the views of the IDB Group, its respective Boards of Directors, or the countries they represent. The usual disclaimers apply.

1. Introduction

This paper estimates the impact of matching grant programs to promote firm-level research and development (R&D) investment in Chile on firm productivity. Unlike most of the literature, which has focused on the direct effects of R&D programs on grant recipients, our focus is on spillover effects. The reason is that it is the existence of spillovers—and not the potential impact of grants on grant recipients—that is the main rationale for government intervention in these programs.

The R&D undertaken by one firm can affect the performance of other firms operating in the same or in other industries, either locally or abroad. A discovery in one firm, sector, or country can trigger new avenues of research, inspire new research projects, lead to new applications, or simply be imitated by other firms, sectors, or countries (Hall and Lerner, 2010). It is well established that knowledge is a non-rival, and only partially excludable, good (Nelson, 1959; Arrow, 1962). Because of weak or incomplete intellectual property protection, the difficulty of keeping innovations secret, and the possibility of reverse engineering and imitation, some of the knowledge and benefits from R&D spill over to other firms.[2]

Knowledge spillovers are at the heart of growth and development because they lay the foundation for further knowledge creation and diffusion (Aghion and Howitt, 1990; Romer, 1990; Grossman and Helpman, 1991a, 1991b; Parente and Prescott, 1994; Jones, 2005; Bravo-Ortega and García Marín, 2011). These spillovers create a wedge between private and social returns and generate disincentives for private investment in knowledge production. While granting intellectual property rights (IPR) can help safeguard and thus stimulate such investments, this approach usually offers limited legal coverage, particularly in developing countries, where very few firms are able to produce knowledge that is novel enough to be eligible for IPR protection. Policy interventions are therefore a plausible way to close this gap, for example through targeted subsidies.

Although knowledge spillovers are the main rationale for public subsidies to support business R&D, most previous impact evaluations of innovation programs have focused on their impact on direct beneficiaries (Hall and Maffioli, 2008; Cerulli, 2010; Hall and Lerner, 2010; Crespi et al., 2014; Doraszelski and Jaumandreu, 2013; Zúñiga-Vicente et al., 2014; Cerulli et al., 2016; Figal Garone and Maffioli, 2016). Yet this approach is not informative enough to assess whether such subsidies are justified. For example, a subsidy would not be justified if all the benefits from the R&D investment are concentrated in one firm. While such an investment would be socially desirable, the private firm would be motivated to undertake it without the need for public incentives. In such cases, a traditional impact evaluation focused on direct beneficiaries would indicate that the intervention increased investment and productivity, even if it failed to generate knowledge spillovers.[3] If the justification for such a policy intervention is indeed the potential to trigger knowledge spillovers, it is important to assess whether such spillovers have occurred.

[2] Of course, spillovers may not always be positive, and can depend on the technological, spatial, and other economic distances between firms (Jaffe, 1986). While knowledge spillover effects may increase the productivity of other firms, through business-stealing effects, productivity gains in an innovating firm may decrease the value of competing firms. For example, Bloom et al. (2013) show that R&D conducted by neighbors that are close in the technology space is associated with a higher firm market value, patenting, and total factor productivity (TFP) (i.e., the knowledge spillover effect), while R&D by neighbors that are close in the product market space exacerbates the rivalry effect, lowering the firm's market value without affecting patents or TFP.

[3] Even worse, in the absence of spillovers, the subsidy could lead to an increase in R&D investment in projects that

Moreover, different types of R&D may vary in their potential to generate spillovers. For instance, firms conducting intramural R&D—that is, R&D activities that are developed within the firm—may find it easier to protect the knowledge generated internally, and thus be able to limit diffusion and corresponding knowledge spillovers. By contrast, knowledge generated via extramural R&D—that is, R&D activities that are undertaken in collaboration with (or by) an external partner such as a firm, consortium, university, or another institute—may be more generic and/or easier to codify, and its benefits more difficult to appropriate. Thus, extramural R&D could be expected to produce more knowledge spillovers (Cassiman and Veugelers, 2002).

This paper contributes to the literature in three main ways. First, it evaluates the long-term direct and indirect (spillover) effects of public support to R&D on firm performance in Chile. Rather than focusing only on direct beneficiaries, it assesses the extent to which R&D subsidies have also indirectly affected untreated firms—that is, the occurrence of spillover effects—using an indicator of spatial and technological (sectorial) proximity between treated and untreated firms. Spatial and technological distances have been largely shown to be important mechanisms for transmitting knowledge between firms (Jaffe, 1986; Bernstein and Nadiri, 1989; Audretsch and Feldman, 1996; Anselin et al., 1997; Aw, 2002; Fosfuri and Ronde, 2004; Orlando, 2004; Paz, 2014; Aldieri et al., 2018).[4] In addition, rather than looking at the impact of the subsidies on R&D or behavioral efforts, we focus on the impact on firm performance, that is, productivity.

Second, to further explore the source of the spillover effects and the mechanisms that generate them, we compare the effect of two R&D subsidy schemes that target different kinds of R&D projects: the National Productivity and Technological Development Fund (FONTEC), which subsidizes intramural R&D, and the Science and Technology Development Fund (FONDEF), which finances extramural R&D carried out in collaboration with research institutes. This unique Chilean setting allows us to determine which type of policy design could more effectively address market failures due to the lack of knowledge appropriability.

Finally, we characterize the nature of spillover effects by studying how they change with differences in policy intensity, that is, when more (or fewer) firms are supported. This analysis allows us to understand how two countervailing spillover effects operate: positive effects from knowledge spillovers vs. negative business-stealing effects from product market rivals.

To identify these effects, we compute firm-level (total factor) productivity and apply fixed-effects techniques to a novel dataset. This dataset merges several waves of Chile's National Manufacturing Surveys collected by the National Institute of Statistics (Instituto Nacional de Estadística, or INE) with register data containing information on the beneficiaries of both

are socially undesirable but for which the private returns exceed private (after subsidy) costs, for example, a project with a private return of $5, no spillovers, and a cost of $6, $3 of which is paid by the subsidy. Note that this investment would be undesirable even if there were spillovers, if they are small enough (in this example, smaller than $1).

[4] Other specific mechanisms of knowledge diffusion include labour mobility (Fosfuri et al., 2001; Rao and Drazin, 2002; Kim and Marschke, 2005; Gorg and Strobl, 2005; Moen, 2005; Boschma et al., 2009; Maliranta et al., 2009; Balsvik, 2011; Filatotchev et al., 2011; Stoyanov and Zubanov, 2012; Poole, 2013; Castillo et al., 2019), provision of goods and services (Bonte, 2008; Lopez and Yadav, 2010; Isaksson et al., 2016), equity and foreign direct investments (Aitken and Harrison, 1999; Javorcik, 2004; Irsová and Havránek, 2013; Luosha et al., 2014; Newman et al., 2015; Thang et al., 2016), common participation in associations, consortia and clusters (Gilbert et al., 2008; Kesidou and Romijn, 2008; Chyi et al., 2012), and patent citations (Henderson et al., 1993; Thompson and Fox-Kean, 2005; Guangzhou Hu, 2009; Nelson, 2009; Murata et al., 2014). These mechanisms are also exacerbated in the context of spatial and technological proximity.

programs. Our final dataset is a 17-year panel covering almost 9,000 firms and 600 program beneficiaries.

Our findings show that R&D subsidies in Chile do generate spillover effects. Indeed, when considering both programs together, we find that policy intervention increases the productivity of both treated firms (direct beneficiaries) and untreated firms located in the same region and sector (indirect beneficiaries). Directly participating in an R&D support program (either FONTEC or FONDEF) increases a firm's total factor productivity (TFP) by around 4 percent. In terms of spillover effects, a one-standard-deviation increase in the share of supported firms increases TFP of firms that are close in both the geographic and technology spaces by around 1 percent.

When looking at each program separately, the direct effect remains quite similar. However, our results suggest that spillover effects are contingent on program design: while both programs increase productivity for direct beneficiaries, only FONDEF-funded projects (i.e., extramural R&D) generate positive spillover effects.

When we analyze the spillover effects in more depth, the results are striking in two respects. First, we find an inverted-U relationship between the intensity of the support, captured by the share of firms receiving R&D subsidies in the same sector and location, and the spillover effects on productivity. This suggests that two countervailing spillover effects may be in play: positive knowledge spillover effects dominate if the share of treated firms in the sector-location is relatively low; by contrast, if the program supports a larger fraction of a firm's rivals, business-stealing may produce decreasing spillover effects on productivity. The inverted-U shaped curve may be generated, for example, if there are decreasing returns on knowledge spillovers as more firms adopt a technology—that is, firms can learn most of what there is to learn from early adopters—but the negative business-stealing effects may be linear based on the number of adopters.

Second, we find that proximity in both geographic and technology spaces is necessary for spillovers to occur. That is, knowledge flows more easily among geographically proximate firms that belong to the same sector.

The paper is structured as follows. Section 2 summarizes the relevant literature, focusing on the rationale behind R&D subsidies and evidence of the effectiveness of R&D support programs. It also describes the main features of the two Chilean subsidy programs. Section 3 presents the empirical strategy used to measure the programs' direct and spillover effects. Section 4 describes the data and analyzes some descriptive statistics. Section 5 presents the results, and Section 6 concludes.

2. Background

2.1. The Rationale behind R&D Subsidies

The fundamental premise underlying R&D subsidies is that government intervention can be beneficial if profit-driven actors underinvest in R&D from a social welfare perspective due to the presence of spillover effects associated with the 'public good' nature of knowledge (Steinmueller, 2010). If knowledge is a non-rival and non-excludable good,[5] then a firm's rivals

[5] The seminal work by Nelson (1959) and Arrow (1962) maintains that, once produced, new knowledge is a non-rival good: it can be used simultaneously by many different firms. This characteristic represents an extreme form of decreasing marginal costs as the scale of use increases: although the costs of the first use of new knowledge may be large since they include the costs of its generation, further use incurs negligible incremental costs (Aghion et al., 2009). Knowledge is said to be non-excludable due to the difficulty and cost of trying to retain exclusive possession of it while using it.

may be able to free ride on its investments. These spillovers may create a gap between private and social returns, and a disincentive to privately invest in knowledge production.

Spillovers are not automatic, however, and should not be taken for granted, since not all knowledge is considered a public good to the same extent. Certainly, the public good rationale of knowledge applies more strongly to generic or scientific knowledge than to technological knowledge, which tends to be more applicable and specific to the firm. Furthermore, for the public good rationale to be valid, there should be some possibility of free riding. If the originator can protect the results of the knowledge generated (through entry barriers or the use of strategic mechanisms, for example), then the potential for market failure declines. In this regard, knowledge generated through collaboration among different parties might be more difficult to protect, and therefore might be more prone to spillovers than knowledge generated by individual entities based on their internal capabilities.[6]

Other market failures, including asymmetric information and uncertainty, affect the financing of innovation activities. R&D projects are different from other investments in three main ways (Hall and Lerner, 2010): (i) the returns on R&D investments are more uncertain and take longer to materialize; (ii) innovators may be reluctant to disclose information about their projects due to the risk of spillovers; and (iii) R&D investments normally involve intangible assets that have very limited use as collateral. For these reasons, firms without deep pockets may find it difficult to access financing for innovation projects, even when these have positive expected private rates of return. Thus, some potentially profitable projects will never be carried out. However, it is important to establish that, in the absence of spillovers, R&D subsidies are not the solution to these problems. Rather, the best remedies for a lack of financing are financial instruments such as long-term credit lines or guarantees for intangible assets (IDB, 2014).

R&D projects might also be affected by pervasive coordination failures. Knowledge has important tacit components that cannot be embodied in a set of artifacts, such as machines, manuals, or blueprints. Thus, firms can benefit from networking with one another and with other actors, because they need to learn from the knowledge bases of other organizations. However, these knowledge networks are less effective if private and public agents fail to coordinate their knowledge investment plans to create mutual positive externalities (Aghion et al., 2009). For example, coordination failures could occur in the process of accessing technological infrastructure. Firms that cannot afford infrastructure on their own can gain access to it if they collaborate with others.

Solving coordination problems requires paying special attention to institutional settings that can affect the linkages between different actors in the innovation system. In most cases, this requires institutional reforms that provide appropriate incentives for innovation actors to collaborate with each other. R&D subsidies might also help align the parties' incentives, particularly during the preliminary learning phases of a joint venture. By making support contingent on collaboration, these subsidy schemes may help shift collaborating partners to a better equilibrium.

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[6] Under specific circumstances, private R&D investments might even be higher than socially optimal if, for example, firms must invest in R&D to build enough absorptive capacity to benefit from spillovers. Thus, environments with strong spillovers could induce more, rather than less, R&D investment. "Patent race" models, in which a pool of companies invests in R&D to obtain a patent that gives them monopoly control over the knowledge generated, may also inadvertently increase private investment. In such cases, cooperative arrangements for R&D might be better from a welfare perspective than simple R&D subsidies (Cerulli, 2010).

In summary, R&D subsidies are primarily justified by the presence of knowledge spillovers, which are more likely to occur when the knowledge generated is more generic and when it is developed within a collaborative joint venture. By assessing the spillover effects of intramural (FONTEC) and extramural (FONDEF) R&D support programs, this paper directly addresses this important issue, which is key to guide policy design in this domain.

2.2. Empirical Evidence of the Impact of R&D Subsidies

There is no guarantee that R&D subsidies will solve the problem of business R&D underinvestment. Their effectiveness will depend on several complex considerations that policymakers will not have advance knowledge of, including the actual presence of knowledge spillovers, the type of knowledge targeted by the intervention, and the reaction to the intervention by supported and unsupported firms (Toivanen, 2009). The need to learn which policies are most beneficial has motivated a growing empirical literature analyzing the impact of R&D subsidies.[7]

The inherited literature on the impact evaluation of R&D subsidies suggests that a proper assessment of their impacts requires looking at three different sorts of additionalities (and their linkages): input, behavioral and output. While the focus of input additionality is on the effects of subsidies on R&D efforts, the attention of behavioral and output additionalities is respectively on issues such as organizational learning and the results in terms of either innovation outcomes such as patents and new products or processes, or firm performance, namely, productivity. The literature also highlights that the three additionalities are interlinked which suggests that a proper assessment of R&D subsidies might require the specification of a two or even three-step model where the different additionalities are connected. In other words, that in order to study the effect of the subsidy on output additionality (i.e. firm performance), one must account for the "mediating" action operated by behavioral and input additionality (Cerulli et al., 2016). The literature also suggests that there are non-negligible time lags among the three additionalities, with input, behavioral and output additionalities occurring over different time spans. This suggests that a recursive rather a simultaneous system might be needed (Crespi, 2011).

Most of the empirical literature has measured the results of R&D subsidies in terms of input additionality, or the extent to which subsidies crowd in or out private R&D investment. The implicit assumption underlying this approach is that, to the extent that subsidies are rightly targeting the market failure (e.g., knowledge spillovers), they will allow firms to pursue projects that they would not have implemented otherwise.

Zúñiga-Vicente et al. (2014) conducted one of the most comprehensive reviews of the impact of R&D subsidies on private R&D investments around the world. They document the results of 76 studies carried out at the firm level since the early 1960s, most of which were published in the 2000s. Although the studies are not fully comparable a general pattern clearly emerges: in 60 percent of the cases, the crowding-in hypothesis cannot be ruled out. The rest of the studies find either crowding out or non-significant effects (20 percent each). More recently, Dimos and Pugh (2016) provides a Meta-Regresion Analysis (MRA) of micro-level studies published since 2000 on the impact of public subsidy for R&D on either input or output R&D. Their MRA findings reject crowding-out of private investment by public subsidy but reveal no evidence of substantial additionality.

As in other regions, the most common approach of assessing the effectiveness of R&D subsidies in Latin America and the Caribbean has been to evaluate their effects on private R&D investment. Crespi et al. (2014) and Figal Garone and Maffioli (2016) summarize the results of 16 impact evaluations undertaken in the region. Their analysis shows that in most cases, subsidies do stimulate R&D investments, and there is evidence of a crowding-in effect. Interestingly, the effects tend to be larger when subsidies target

[7] For a detailed review of the pros and cons of different approaches to assessing the impact of R&D subsidies, see Cerulli (2010). Regarding science and technology programs, see also Crespi et al. (2011).

projects that involve collaboration between firms and research institutes.

Regarding behavioral additionality, the evidence is scarcer in part because of the problems on the measurement of behavioral change. Despite this, some efforts that use survey collected data have been carried out. For example, Clarysse et al. (2009) focus on the effects of R&D subsidies on behavior defined it as the changes in management practices of innovation process within the company. They find that the subsidies have a positive effect on behavioral additionality but only for the first supported projects; afterwards, learning effects decrease with the number of subsidized projects that are undertaken by the company. Interestingly, they also find that supported projects that are carried-out in collaboration with other companies generate stronger learning effects. Some studies have also focused on R&D collaboration as a special dimension of behavioral additionality. Busom and Fernandez-Ribas (2008) study the effect of public support on firm's propensity to cooperate with other firms or with public research organizations (PRO). They only find positive effects when firms cooperate with PROs. Similarly, Cerulli et al. (2016) also find a positive effect of R&D subsidies on their R&D collaboration index. Finally, regarding whether there are complementary effects between input and behavioral additionalities the results are mixed ranging from complementarity effects (Cerulli et al., 2016) to strong substitution effects (Irwin and Klenow, 1996).

Thus, the empirical evidence tends to confirm that R&D subsidies are an effective way to increase private R&D investment and to induce favorable behavioral organizational effects towards innovation. But what are the actual returns on these investments? To the extent that knowledge is a production input, the right setting in which to assess the impact on outputs is a knowledge-augmented production function model. In other words, properly assessing the effectiveness of R&D subsidies requires at the end also evaluating their impact in terms of their output (i.e., innovation or productivity). The main difficulty associated with this type of study is that a longer time horizon is required to detect the effects. While R&D expenditures or behavioral effects can be detected almost immediately after the receipt of public financing, productivity effects can only be assessed after an innovation has taken place. Rigorous impact evaluation of these effects therefore requires panel data to track firms' progress after receiving the subsidy.[8]

More importantly, are R&D subsidies targeting projects that generate knowledge spillovers and hence are less likely to be implemented without the subsidy? Although a growing empirical literature seeks to identify knowledge spillovers, far fewer studies have sought to link these spillovers to public support for R&D and integrate them into an empirical impact evaluation framework (see, for example, Branstetter and Sakakibara, 1998; Møen, 2004; USP Research Groups's, 2013; and, Castillo et al., 2019).

Although they do not evaluate policy impacts, two empirical papers on knowledge spillovers are highly relevant to our study. First, Bloom (2007) shows that the relationship between a firm's R&D and that of rival companies operating in the same sector depends on the degree of complementarity/substitutability of innovative outputs (patents). Indeed, when products are complements, companies can take advantage of other firms' inventions (and hence, others' R&D efforts), which gives them an incentive to increase their own R&D investment. The opposite occurs when products are substitutes. In a second important paper, Bloom et al. (2013) emphasize that two different types of spillover effects might be present: a positive effect from knowledge spillovers and a negative, business-stealing effect from product market rivals. Using different measures of firm proximity to analyze panel data on U.S. firms, their results suggest that positive knowledge spillovers quantitatively dominate, so R&D gross social returns are likely twice as high as R&D private returns.

The focus of our paper is on the last class of additionality, the output additionality on direct and indirect beneficiaries of public support to R&D. Rather than focus on input or behavioral additionality, we will focus on an output variable, namely, productivity. And rather than exploring just the impact on direct beneficiaries, we will focus on spillovers. We recognize that a more complete assessment of these

[8] For some examples on impact evaluations of R&D subsidies on firms' performance see Crespi et al. (2015), Castillo et al. (2019), and the literature there cited.

programs might require also looking at input (R&D) and behavioral (organizational) additionalities as well as the effects on innovation outputs (new products or processes). However, given that our empirical analysis is mostly based on register (census) data, we lack good information on firm level R&D expenditures, organizational practices and innovation outputs as to pursue the analyses of these types of additionalities and model their linkages.[9] Therefore, our empirical approach should be interpreted as a reduced form model which we think provides the basic foundations to assess whether knowledge spillovers from R&D policies are present and untangle the right policy design in order to maximize them.

2.3. Chilean Business Innovation Policy: 20 Years of Experimentation

Although by early 1990s Chile already had an income per capital of about $7,500 (PPP equivalent), its TFP was only 64% of the USA's one, considered the latter as a good proxy of the technological frontier.[10] R&D expenditures were negligible and mostly funded by the government. In order to increase private sector R&D investment and start closing the gap with the productivity frontier, the Chilean government started the implementation of several programs aimed at supporting innovation and productivity improvements in private firms. This paper focuses on two of such programs, FONTEC and FONDEF.

FONTEC, managed by the Chilean National Development Agency (CORFO), provides financing for innovation projects carried out by private firms. It has supported more than 2,200 business innovation projects since its creation in 1991. FONTEC uses a matching grant approach, subsidizing 40–65 percent of the total costs of private projects with private co-funding in the form of ex post reimbursement of approved eligible expenditures (Benavente et al., 2007). Providing only partial funding helps align the goals of the public agency and the firm and eases the potential moral hazard problem. While FONTEC can allocate resources in different ways, the most important instrument consists of direct business R&D subsidies, which finance innovation projects carried out by individual firms.

FONTEC grants are allocated under an open window system, on a rolling first-in-first-out basis. External peer reviewers technically assess innovation projects submitted by firms, and an adjudicatory committee with representatives from both the public and the private sectors makes the final allocation decision. Although this approach is more flexible in response to firms' demands for support, it may be less competitive than a system based on a call for proposals. FONTEC is designed to help closing the gap between social and private returns to business R&D. In principle, its subsidies should be targeting knowledge spillovers generated by R&D projects implemented by individual firms based on their internal capabilities.

FONDEF, managed by the National Science and Technology Council (CONICYT), provides funding for pre-competitive R&D and technology projects executed jointly by universities, technology institutes, and the private sector. The government subsidy also entails a matching grant covering a portion of the total costs of the project (up to a maximum of 55 percent). Universities and non-profit R&D institutions are the main beneficiaries, but private sector participation is required. The research institution (executor) involved in the project is required to contribute the equivalent of 20 percent of the total cost of the project, while associates and companies must contribute a minimum of 25 percent of the total project cost. The grants are awarded through an annual public bidding process after a review of project proposals.

FONDEF's economic justification seems to involve internalizing R&D spillovers by forming joint ventures and facilitating collaboration among R&D innovation system actors. In other words, the program seeks to align the interests of public research organizations with those of the productive sector. These incentives also give private firms access to a large set of complementary knowledge assets (external capabilities) and technological infrastructure to help implement their R&D projects.

[9] Some partial assessment on both input and behavioral additionalities for Chilean R&D support programs is carried out for some sub-samples in papers summarized in Crespi et al. (2014) and Figal Garone and Maffioli (2016).
[10] PWT 9.0 (2011 dollars).

In sum, although FONDEF and FONTEC are designed to increase private R&D investment and productivity at the firm level, they use very different mechanisms to achieve this goal. While FONTEC focuses on alleviating the lack of appropriability that harms business R&D by providing support to individual firms to implement their projects based on their internal capabilities, FONDEF addresses the same problem by fostering collaboration and interaction between public research organizations and firms. Given the different designs of these programs, it is important from a policy perspective to compare their performance. To the extent that FONDEF is more likely to produce more generic knowledge and that firms executing (intramural) FONTEC projects are more likely to be able to protect their acquired knowledge, we expect FONDEF projects to have a greater potential to produce externalities. The involvement of multiple parties could also increase the likelihood of major knowledge spillovers.

3. Empirical Strategy and Expected Impact

3.1 Direct Impact: Public Support to R&D and Productivity

In our empirical analysis, we first look at the effect of FONTEC and FONDEF support on participant firms' performance. While having an impact on the performance of direct beneficiaries is not sufficient as a justification for government intervention, it is a necessary condition, as it would be hard to argue that there are spillover effects on other firms if subsidies did not enhance performance of direct beneficiaries. As a measure of performance, we use a measure of total factor productivity (TFP).[11] The use of productivity as the outcome variable of interest is consistent with a knowledge-augmented production function model, which also provides the right setting in which to assess the importance of knowledge spillovers (Hall and Lerner, 2010; Keller, 2010).

For this purpose, we build on the R&D capital model laid out, for example, in Griliches (1973, 1979, and 2016). Variations of this framework are widely used in studies of the returns to R&D.[12] We follow Moretti (2004) to specify the following basic model:

$$Y_{irjt} = A_{irjt} H_{irjt}^{\alpha H} L_{itjt}^{\alpha L} M_{irjt}^{\alpha M} K_{irjt}^{\alpha K} \,, \qquad (1)$$

where Y_{irjt} is output, H_{irjt} is skilled labor, L_{irjt} unskilled labor, M_{irjt} is raw materials, K_{irjt} is capital stock, and A_{irst} is TFP, for firm *i*, region *r*, sector *j*, and period *t*.

We assume that an R&D subsidy operates by shifting the TFP parameter. Therefore, before assessing the impact of the R&D support on TFP we need to estimate TFP. For this, we use a parametric method under the following assumptions: (i) technology is Cobb-Douglas; (ii) factor prices equal marginal products; and (iii) there are constant returns on scale to capital, materials and labor. Our measure of productivity ($\hat{A}_{irjt}$) is computed from the residual of the production function in equation (1) estimated using a fixed-effects (differences-in-differences) log-log regression model. The elasticities are the factor shares measured at the 2-digit sector and region levels using the mean of plant-specific ratios of input costs over total costs.[13]

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[11] Although R&D and productivity have an indirect relationship mediated by the success (or lack thereof) of innovation outcomes (such as new products or processes), data availability forces us to focus on firm performance.

[12] See Mairesse and Sassenou (1991) and Wieser (2005) for good reviews on this methodological and empirical literature.

[13] We replicate the results for two alternative measures of productivity. The first is estimated from the residual of a production function estimated at the 2-digit industry level using the Levinshon-Petrin approach (for details, see Alvarez and Crespi, 2007). The second measure is similar to the main one but uses factor shares measured at the 2-digit sector level. The results, available upon request, are identical to those presented in the paper. This outcome is consistent with findings by Van Biesebroeck (2008) who compares the performance of common methods of estimating TFP – index numbers, non-parametric methods, and parametric methods – and concludes that "the choice of estimation method for productivity is immaterial to the conclusions" (Van Biesebroeck, 2008: 326) – that is, more

Since the subsidies are not granted randomly, beneficiaries may differ from non-beneficiaries due to selection bias: beneficiaries are likely to be more productive than non-beneficiaries. Therefore, beneficiaries would show different outcomes than non-beneficiaries even in the absence of program support.

A major advantage of using longitudinal firm-level datasets is that they allow us to account for constant unobservable factors that may affect both the outcome of interest and participation in the program. We estimate the effects of public support to R&D (either FONTEC or FONDEF) on direct beneficiaries using the following fixed-effects (differences-in-differences) log-linear regression model:[14]

$$ln(\hat{A}_{irjt}) = \rho D_{irjt-1} + \beta_k X^k_{irjt} + \epsilon_i + \epsilon_t + \epsilon_{rt} + \epsilon_{jt} + \varepsilon_{irjt} \quad (2)$$

Firm fixed effects ϵ_i fully absorb any permanent heterogeneity at the firm level, and ϵ_t represents yearly shocks that affect all firms. Regarding the interaction terms, ϵ_{rt} denote region-year effects, and ϵ_{jt} fully absorb industry-year effects—time-specific shocks that affect the productivity of all firms in industry j. D_{irst-1} is a binary variable that takes a value of 1 the year after firm i receives the R&D subsidy and thereafter. Therefore, ρ represents the parameter of interest, which captures the causal effect of D_{irjt-1} on productivity.[15] In other words, ρ is the program's average impact on participating firms in the post-treatment period. X^k_{irjt} are time-varying firm characteristics, in our case, the log of the firm's age and age squared, and ε_{irjt} is the usual error term assumed to be uncorrelated with D_{irjt-1}. In this case, the identifying assumption is independence of treatment status and potential outcomes, conditional on time-invariant unobservable and observable factors as well as time-varying observable confounders.

Since we are evaluating two programs, we extend the empirical model in equation (2) to obtain different impact parameters for each program:

$$ln(\hat{A}_{irjt}) = \rho_C D_{C,irjt-1} + \rho_F D_{F,irjt-1} + \beta_k X^k_{irjt} + \epsilon_i + \epsilon_t + \epsilon_{rt} + \epsilon_{jt} + \varepsilon_{irjt}\,, \quad (3)$$

where $D_{C,irjt-1}$ and $D_{F,irjt-1}$ correspond to FONTEC or FONDEF, respectively.

3.2 Indirect Impact: Spillover Measure and Effects

The presence of knowledge spillovers in a production function setting has implications for the estimation of the impact of R&D subsidies on participant firms (direct beneficiaries) and requires a strategy for the identification of the knowledge spillovers. Regarding the former, using non-supported firms to evaluate what would have happened to supported firms if they had not been supported assumes that R&D subsidies have no spillover effects on non-supported firms, which is clearly problematic. The question is whether the performance of non-supported firms can be considered independently of the support given to treated firms (Klette et al., 2000). If knowledge spillovers are present, this might lead us to underestimate the impact of R&D subsidies on treated firms. Therefore, to obtain proper estimates of the impact of R&D subsidies on treated firms' economic performance, it is important to control for spillover effects in the empirical approach.

Regarding the identification of knowledge spillovers within a production function framework, most

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productive firms look more productive under any of the commonly used methods.

[14] See Bertrand et al. (2002) for a formal discussion of DID estimates.

[15] It is worth emphasizing that we are using a two-step approach to measure the impact of R&D subsidies on productivity. We first measure productivity using different methodologies. In the second step we correlate the estimated TFP with the R&D variables. To the extent that the methodology used to measure TFP in the first step does not include R&D variables (in other words, it assumes that productivity is exogenous), any estimated impact in the second step will be underestimated (see Doraszelski and Jamandreu, 2013).

empirical approaches augment the production function with a variable capturing the "pool" of outside knowledge relevant to each firm. This pool is normally constructed using a weighted average of other firms' knowledge, where the weights capture the degree of (technological, geographic, vertical, etc.) proximity among firms. However, one important methodological challenge associated with identifying knowledge spillovers—in addition to measuring them—is avoiding spurious correlation due to correlated unobservables across technologically related firms (Griliches, 1998).

Equation (2) can then be augmented to assess the presence of geographic-technological spillover effects:

$$ln(\hat{A}_{irjt}) = \rho D_{irjt-1} + \rho_S S_{irjt-1} + \beta_k X^k_{irjt} + \epsilon_i + \epsilon_t + \epsilon_{rt} + \epsilon_{jt} + \varepsilon_{irjt} \quad (4)$$

where S_{irjt-1} is a measure of exposure to spillovers within a region-sector. The parameter ρ_S captures the spillover effects of the R&D subsidies. Therefore, a beneficiary firm receives a direct impact of the program (ρ) and an indirect impact (ρ_S).

To estimate Equation (4), the measurement of S_{irjt-1} is critical.[16] For this, let T be the universe of firms directly supported by FONTEC or FONDEF and N the universe of firms. We then construct the share of treated firms other than firm i in the region and 2-digit sector of firm i, that is:[17]

$$S_{irjt-1} = \begin{cases} \dfrac{T_{rjt-1} - 1}{N_{rjt-1} - 1} \; if \; D_{irjt-1} = 1 \\ \dfrac{T_{rjt-1}}{N_{rjt-1} - 1} \; if \; D_{irjt-1} = 0 \end{cases} \quad (5)$$

The numerators of S_{irjt-1} are the number of treated firms other than firm i in the region-sector, and the denominators are the number of all firms other than firm i in the region-sector (or "*rj*" cluster). Thus, we assume that the size of the spillover is proportional to the share of other firms in the region-sector that receive treatment.

Assigning a constant S_{irjt-1} to each *rj* cluster implies that we assume that each treated firm equally affects all its neighbors' TFP. Assigning a linear growth on S_{irjt-1} implies that we assume there is no complementarity between individual firms' spillovers (we will relax this later). We also assume there are no spillovers between clusters (i.e., we assume that there are only within-cluster spillovers and we will test for that). All these assumptions are included in the broader assumption that there is independence of spillovers reception status and potential outcomes, conditional on time-invariant unobservable and observable factors as well as time-varying observable confounders. We can then define geographic-technological spillover as the change in $\hat{A}_{irjt}$ caused by the change in the share of treated neighbors S_{irjt-1}.

In a similar fashion, we can estimate the presence of spillovers for the different treatments as follows:

$$\ln(\hat{A}_{irjt}) = \rho_C D_{C,irjt-1} + \rho_F D_{F,irjt-1} + \rho_{S,C} S_{C,irjt-1} + \rho_{S,F} S_{F,irjt-1} + \beta_k X^k_{irjt} + \epsilon_i + \epsilon_t + \epsilon_{rt} + \epsilon_{jt} + \varepsilon_{irjt} \quad (6)$$

where $\rho_{S,C}$ and $\rho_{S,F}$ capture the FONTEC and FONDEF spillover effects, respectively.

[16] Bloom et al. (2013) made the first attempt to provide an "axiomatic" basis for evaluating different measures of technology proximity and spillovers by proposing seven desirable properties. Since none of their measures dominates all the others, they conclude that the relative weight of these properties should be the choice of the empirical researcher depending on the research question.

[17] We construct the spillovers at the 2-digit sector level to ensure we have a reasonable number of treated firms within the region-sector. This improves the statistical power of the estimations.

As mentioned in Section 2.3, FONTEC and FONDEF are expected to have different spillover effects on productivity. Given that FONTEC finances intramural R&D, a project submitted by a firm to this program is likely to be more closely related to the firm's internal capabilities. Since this type of project is more appropriable, the firm may have carried it out anyway. By contrast, FONDEF finances extramural R&D, which almost by definition involves knowledge that is more generic or further away from firms' internal capabilities. The knowledge generated in such projects is less likely to be appropriable. Thus, we expect spillover effects to be higher for FONDEF than for FONTEC projects. That is, we expect $\rho_{S,C} < \rho_{S,F}$.

4. Data and Preliminary Statistics

We use two datasets for our analysis. The first is the National Annual Manufacturing Survey (Encuesta Nacional Industrial Annual, or ENIA) of all manufacturing firms with 10 or more employees (n = 5,000) collected every year from 1990 to 2006. Second, we use administrative data provided by CORFO and CONICYT with the collaboration of INE to identify which firms in the ENIA data received FONTEC or FONDEF funding during this period.

Table 1 presents the number of firms by cohort of entry to FONTEC or FONDEF and the breakdown by program. The number of treated firms started to grow gradually in 1995 until reaching a typical flow of about 50 per year.[18]

Table 2a describes the universe of manufacturing firms between 1990 and 2006. It shows separate statistics for firms participating in either program and non-participant firms (control group).[19] Table 2a highlights that treated firms generally score 28 percent higher than the control group on our main performance variable, TFP. Treated firms also outperform control firms across various firm characteristics. Treated firms are slightly older and considerably larger than control firms (treated firms have an average of 192 employees, while the typical control firm has only 68 employees). Treated firms also have a greater share of white-collar workers, amounting to 39 percent, compared to 35 percent for control firms. Finally, treated firms are more outward oriented, with a higher participation of foreign direct investment ownership and export intensity. While significant, the differences are smaller with regards to the firms' sector distribution. Treated firms have a higher participation in food, chemicals, basic metals, and machinery, while controls have greater participation in sectors such as textiles and wood processing. Finally, treated firms are dispersed throughout Chile, while control firms are more densely concentrated in Santiago.

Table 2b summarizes the descriptive statistics for the beneficiaries of each program. In general, the differences across the programs are relatively small, particularly when compared with the control group. Perhaps the most striking difference is that firms that received FONTEC benefits tend to be smaller and less highly skilled than those that received FONDEF subsidies. Firms participating in the FONTEC program seem to be over-represented in the chemicals and machinery sectors, while FONDEF participants are biased toward the foodstuff sector. Finally, FONTEC firms are more often located in Santiago, while FONDEF tends to mostly support firms in other regions of the country. Despite these differences across firm characteristics, there are no striking differences in performance or productivity.

[18] It is important to note that both programs are demand driven in the sense that the firm must submit a proposal to the funding agency. Volatility due to business cycles is also expected. Both programs are horizontal, since they support innovation activity regardless of the firm's sector or region. However, given that the ENIA is a manufacturing survey, the figures for treated firms are for manufacturing firms.

[19] We exclude the small number of firms that participated in both programs from our estimations in order to better estimate the effects of both programs individually and avoid drawing wrong conclusions on the combined effect due to statistical power problems. These firms are on average more productive and larger than those that participated only in FONTEC or FONDEF. Almost all these firms are from the food and chemical sectors but are dispersed throughout Chile.

As discussed above, the differences in performance between the control and treatment groups cannot be automatically attributed to the programs, since the participating firms were not chosen at random. To identify the direct and spillover effects of the R&D support programs, we follow the empirical strategy described in Section 3 to properly account for potential selection bias.

Table 1: Number of Firms by Cohort of Entry to the Program, 1990–2006

Year	Any treatment	FONTEC	FONDEF
1990	0	0	0
1991	0	0	0
1992	0	0	0
1993	0	0	0
1994	0	0	0
1995	11	11	0
1996	29	29	0
1997	105	31	74
1998	61	30	31
1999	68	42	26
2000	44	29	15
2001	39	24	15
2002	58	27	31
2003	55	29	26
2004	31	22	9
2005	20	8	12
2006	46	23	23
Total	**567**	**305**	**262**

Source: *Authors' calculations based on CORFO and CONICYT administrative register data.*

Table 2a: Descriptive Statistics, 1990–2006

	Control group			Treatment group (FONTEC+FONDEF)		
	Obs.	*Firms*	*Mean*	*Obs.*	*Firms*	*Mean*
TFP	66,484	8,036	-0.02	5,930	540	0.25
Age	77,529	8,436	11.44	6,952	567	13.07
Sales (log)	74,618	8,436	12.66	6,794	567	14.57
Employment	74,617	8,436	67.66	6,794	567	191.73
Skilled	74,618	8,436	20.84	6,794	567	69.88
Skill Intensity	74,616	8,436	0.35	6,794	567	0.39
Export	77,529	8,436	0.15	6,953	567	0.48
FDI	77,529	8,436	0.07	6,953	567	0.22
Size						
Small	77,529	8,436	0.67	6,953	567	0.34
Medium	77,529	8,436	0.23	6,953	567	0.36
Large	77,529	8,436	0.11	6,953	567	0.29
Sector						
Food	77,529	8,436	0.30	6,953	567	0.33
Textile	77,529	8,436	0.16	6,953	567	0.03
Wood	77,529	8,436	0.10	6,953	567	0.05
Paper	77,529	8,436	0.07	6,953	567	0.04
Chemicals	77,529	8,436	0.11	6,953	567	0.21
Non-Metallic	77,529	8,436	0.04	6,953	567	0.04
Basic Metal	77,529	8,436	0.01	6,953	567	0.08
Machinery	77,529	8,436	0.18	6,953	567	0.22
Other	77,529	8,436	0.01	6,953	567	0.00
Region						
Tarapacá	77,529	8,436	0.03	6,953	567	0.03
Antofagasta	77,529	8,436	0.02	6,953	567	0.07
Atacama	77,529	8,436	0.01	6,953	567	0.05
Coquimbo	77,529	8,436	0.02	6,953	567	0.01
Valparaiso	77,529	8,436	0.08	6,953	567	0.06
O´Higgins	77,529	8,436	0.03	6,953	567	0.05
Maule	77,529	8,436	0.04	6,953	567	0.03
Biobio	77,529	8,436	0.11	6,953	567	0.12
La Araucanía	77,529	8,436	0.03	6,953	567	0.02
Los Lagos	77,529	8,436	0.04	6,953	567	0.07
Aisén	77,529	8,436	0.00	6,953	567	0.01
Antártica	77,529	8,436	0.01	6,953	567	0.01
Santiago	77,529	8,436	0.58	6,953	567	0.45

Source: Authors' calculations based on CORFO and CONICYT administrative register data and INE.

Table 2b: Descriptive Statistics, 1990–2006

	FONDEF			FONTEC		
	Obs.	*Firms*	*Mean*	*Obs.*	*Firms*	*Mean*
TFP	2,608	247	0.26	3,322	293	0.25
Age	3,211	262	13.39	3,741	305	12.80
Sales (log)	3,157	262	15.53	3,637	305	13.74
Employment	3,157	262	280.15	3,637	305	114.99
Skilled	3,157	262	111.82	3,637	305	33.48
Skill Intensity	3,157	262	0.43	3,637	305	0.35
Export	3,212	262	0.56	3,741	305	0.42
FDI	3,212	262	0.28	3,741	305	0.17
Size						
Small	3,212	262	0.23	3,741	305	0.44
Medium	3,212	262	0.34	3,741	305	0.38
Large	3,212	262	0.43	3,741	305	0.18
Sector						
Food	3,212	262	0.38	3,741	305	0.29
Textile	3,212	262	0.01	3,741	305	0.05
Wood	3,212	262	0.08	3,741	305	0.03
Paper	3,212	262	0.05	3,741	305	0.03
Chemicals	3,212	262	0.16	3,741	305	0.24
Non-Metallic	3,212	262	0.05	3,741	305	0.03
Basic Metal	3,212	262	0.15	3,741	305	0.02
Machinery	3,212	262	0.11	3,741	305	0.31
Other	3,212	262	0.01	3,741	305	0.00
Region						
Tarapacá	3,212	262	0.02	3,741	305	0.04
Antofagasta	3,212	262	0.11	3,741	305	0.03
Atacama	3,212	262	0.08	3,741	305	0.02
Coquimbo	3,212	262	0.01	3,741	305	0.02
Valparaiso	3,212	262	0.08	3,741	305	0.04
O´Higgins	3,212	262	0.08	3,741	305	0.03
Maule	3,212	262	0.03	3,741	305	0.03
Biobio	3,212	262	0.19	3,741	305	0.06
La Araucanía	3,212	262	0.01	3,741	305	0.03
Los Lagos	3,212	262	0.12	3,741	305	0.04
Aisén	3,212	262	0.02	3,741	305	0.01
Antártica	3,212	262	0.03	3,741	305	0.00
Santiago	3,212	262	0.22	3,741	305	0.65

Source: *Authors' calculations based on CORFO and CONICYT administrative register data and INE.*

5. Results

5.1. Direct and Spillover Effects

We first estimate the direct effect on total factor productivity (TFP) of an overall measure of the R&D subsidies program (i.e., having participated in any treatment). Table 3, Column 1 presents fixed effects estimates of Equation (2). The estimated coefficient of interest (ρ) is positive and statistically significant, indicating that R&D subsidy programs have a positive direct effect on beneficiary firms' productivity. In general, participating in an R&D support program in Chile increases a firm's TFP by an average of 4.3 percent in the post-treatment period (Table 3, Column 1). Looking at each program separately, the direct effects remain statistically significant and are similar in magnitude (Column 2).

A key identifying assumption of our model is that outcome trends between treated and control groups would be the same in the absence of the R&D support programs. Although it is not possible to test for this during the treatment period, we can explore whether both groups exhibit similar productivity trends during the pre-treatment period. Following Castillo et al. (2019) and Cerulli and Ventura (2019), we include in Equation (3) and (4) lags and leads of the treatment variable. Results of this exercise are plotted in Figures 1-3. As shown in these figures, we find that the coefficients for the pre-treatment dummy variables are not statistically different from zero. That is, the null hypothesis that the pre-treatment trends are the same for the treated and control firms cannot be rejected. We also find that the effects on TFP occurs mainly in medium and long term, which is consistent with previous evidence described in Section 2.

In Table 3, Column 3 we augment the model by introducing an overall (for both programs) spillover variable, as defined in Equation (5). The results show that the estimated coefficient for S_{irst-1} is positive and statistically significant, while the direct treatment effect remains the same. These results imply that the subsidy programs, taken together, have positive spillover effects on non-treated firms. The existence of these positive spillovers suggests that the social returns from R&D are greater than the private returns, thus justifying the provision of R&D subsidies. Given the paucity of evidence on spillovers from R&D subsidies onto productivity, we think this is an important result. It means that a one-standard-deviation increase in the share of supported (innovative) firms increases TFP of firms that are close in both the geographic and technology spaces by around 1 percent.

To examine whether the programs differ in the likelihood of generating spillovers, our next estimate untangles the spillover effect based on the type of innovation support program. For this, in Equation (6) we extend the basic model to include different spillover parameters, as well as different direct effect parameters, for each program.

Table 3, Column 4 summarizes the results of these estimates. Our findings show that only FONDEF, which finances collaborative R&D that is expected to be less appropriable than that financed by FONTEC, has positive spillover effects on the productivity of other firms in the same region-sector. The spillover effects generated by FONDEF are also economically relevant: a one-standard-deviation increase in the spillover's variable increases TFP by 1.1 percent.

These results could be criticized on the grounds that FONDEF simply generates more spillovers since it includes more cooperating partners. We believe this is not the case, because the nature of the cooperating partners is different. FONDEF promotes firm-level innovation through encouraging collaboration between firms and universities or research institutes. Knowledge generated by these organizations is normally more generic and thus more likely to leak to other actors through publications, presentations, or the movement of researchers. This knowledge may also require, and help to generate, capabilities that firms do not have and which are likely to become a public good.

Table 3: Direct and Spillover Effects of Public Support to R&D on Productivity

	Total factor productivity			
	(1)	(2)	(3)	(4)
Treatment	0.0435**		0.0423**	
	(0.017)		(0.017)	
FONTEC		0.0412*		0.0416*
		(0.023)		(0.023)
FONDEF		0.0467*		0.0429*
		(0.029)		(0.029)
Spillover share			0.1733***	
			(0.060)	
Spillover share FONTEC				-0.0192
				(0.181)
Spillover share FONDEF				0.2230***
				(0.062)
Age & age^2	Yes	Yes	Yes	Yes
Firm effect	Yes	Yes	Yes	Yes
Time effect	Yes	Yes	Yes	Yes
Sector-year effect	Yes	Yes	Yes	Yes
Region-year effect	Yes	Yes	Yes	Yes
Number of firms	8,576	8,576	8,576	8,576
Observations	63,863	63,863	63,863	63,863
R-squared	0.937	0.937	0.937	0.937

Notes: *(a) Estimates of fixed-effects model. (b) Clustered standard errors at 2-digit sector-region in parentheses. (c) ***, **, * statistically significant at 1%, 5%, and 10%.*

Figure 1. Direct Effect of Public Support to R&D on TFP

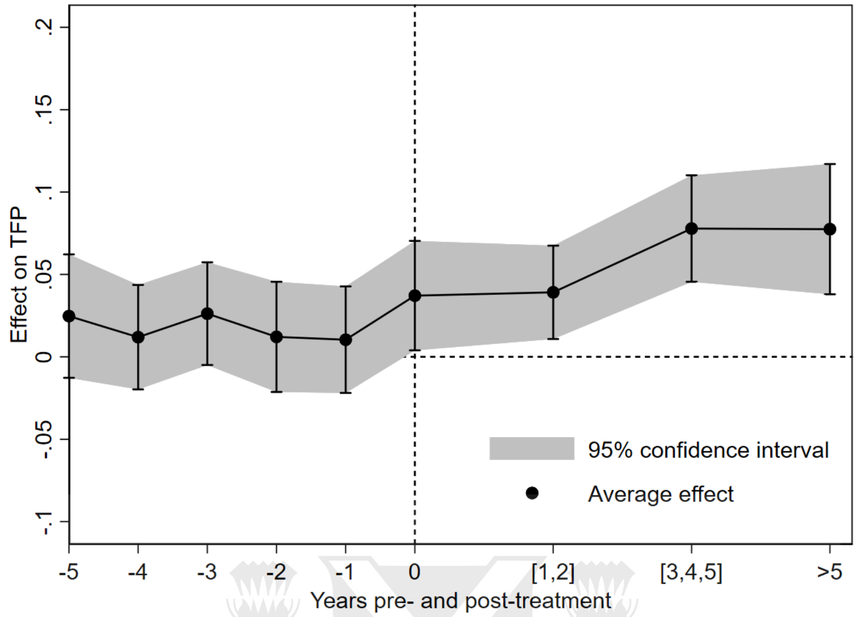


Figure 2. Direct Effect of FONTEC on TFP

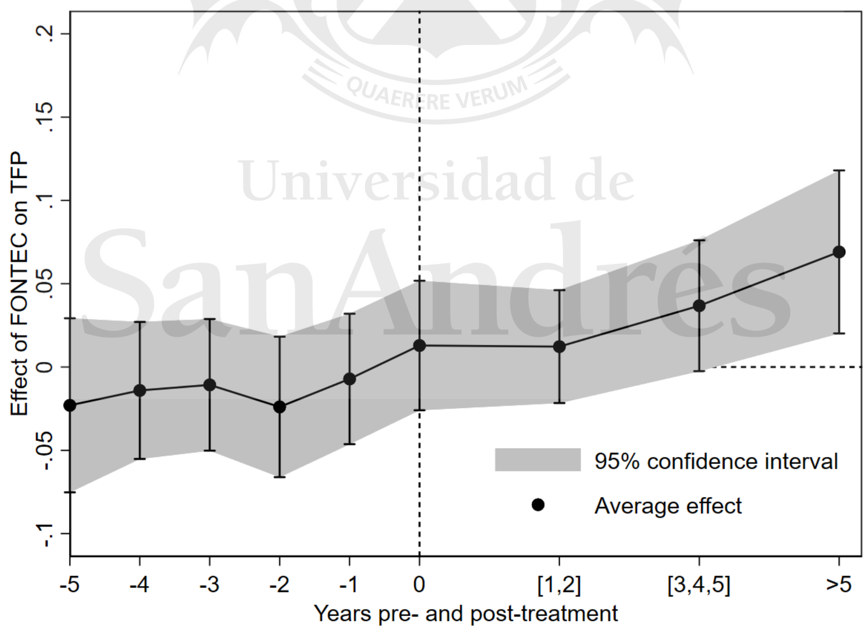
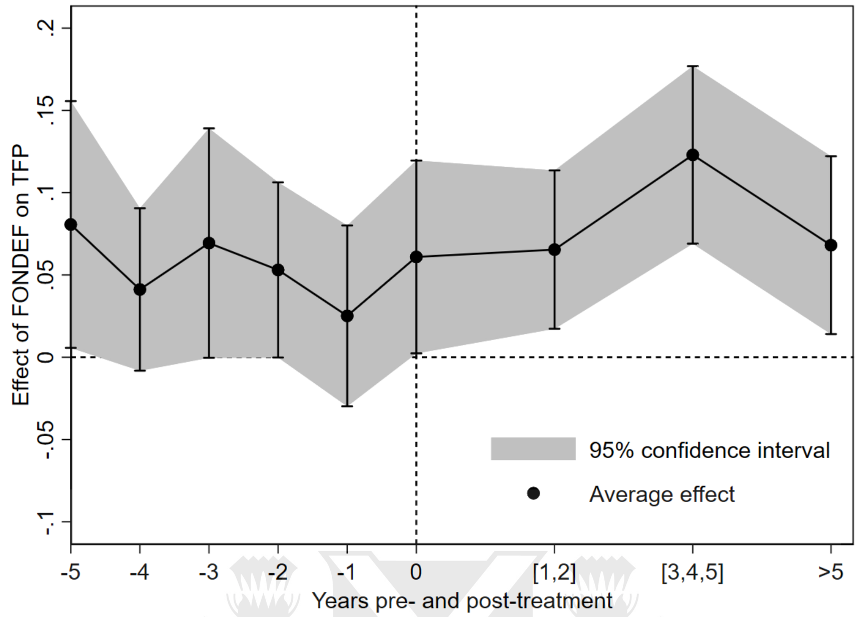


Figure 3. Direct Effect of FONDEF on TFP



5.2. Intensity of Spillovers and Countervailing Effects

The specifications used above implicitly assume that spillover effects depend linearly on the number of treated firms in the same region and sector. Yet the impact of the number of treated firms on the spillover may be nonlinear. For instance, firms may earn more from the early adopters of a given technology but learn very little from additional adopters beyond a critical mass of firms. Moreover, as mentioned before, two countervailing spillovers may be affecting firm performance: a positive effect from knowledge spillovers and a negative, business-stealing effect from product market rivals. These two effects may be interacting in non-linear ways.

To capture potential non-linear effects in the spillover term, we specify a polynomial function for the spillover term in Equation (4). This allows us to directly search for the right functional form of the relationship between the intensity of the treatment and the spillover effect.

$$\ln(\hat{A}_{irjt}) = \rho D_{irjt-1} + \rho_S S_{irjt-1} + \rho_{S,sq} S^2_{irjt-1} + \rho_{S,cu} S^3_{irjt-1} + \beta_k X^k_{irjt} + \epsilon_i + \epsilon_t + \epsilon_{rt} + \epsilon_{jt} + \varepsilon_{irjt} \quad (7)$$

The results (presented in Table 4, Columns 2 and 4) show that the squared terms are strongly positive and significant, while the cubic terms are strongly negative and significant for both the overall measure of spillovers and the FONDEF spillover measure. To understand what this means, we predict spillover effects on TFP by the intensity of the treatment and plot the results. In the case of FONTEC, the coefficients have similar signs, but are not significant.

Table 4: The Intensity of Spillover Effects

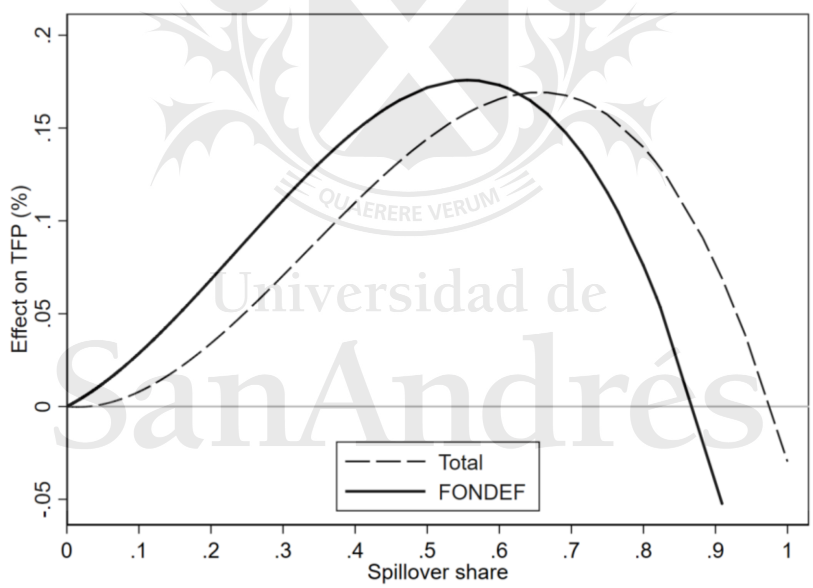
	Total factor productivity			
	(1)	(2)	(3)	(4)
Treatment	0.0423**	0.0421**		
	(0.017)	(0.017)		
Spillover share	0.1733***	-0.0370		
	(0.060)	(0.179)		
Spillover share^2		1.2934**		
		(0.579)		
Spillover share^3		-1.2861***		
		(0.472)		
FONTEC			0.0416*	0.0417*
			(0.023)	(0.023)
Spillover share FONTEC			-0.0192	-0.2891
			(0.181)	(0.304)
Spillover share FONTEC^2				1.7462
				(1.391)
Spillover share FONTEC^3				-1.3835
				(1.215)
FONDEF			0.0429*	0.0408*
			(0.025)	(0.029)
Spillover share FONDEF			0.2230***	0.2020
			(0.062)	(0.177)
Spillover share FONDEF^2				0.9782*
				(0.626)
Spillover share FONDEF^3				-1.3900**
				(0.542)
Age & age^2	Yes	Yes	Yes	Yes
Firm effect	Yes	Yes	Yes	Yes
Time effect	Yes	Yes	Yes	Yes
Sector-year effect	Yes	Yes	Yes	Yes
Region-year effect	Yes	Yes	Yes	Yes
Number of firms	8,576	8,576	8,576	8,576
Observations	63,863	63,863	63,863	63,863
R-squared	0.937	0.937	0.937	0.937

*Notes: (a) Estimates of fixed-effects model. (b) Clustered standard errors at 2-digit sector-region in parentheses. (c) ***, **, * statistically significant at 1%, 5%, and 10%.*

Figure 4 illustrates the spillover effects by the intensity of support (i.e., the share of treated firms other than firm *i* in the region and sector of firm *i*). There is an inverted-U relationship between spillover effects on productivity and the intensity of public support (innovation). Thus, it appears that the positive knowledge spillover effects generated by FONDEF dominate when the share of treated firms in the region-sector is relatively low. If the program supports a larger fraction of a firm's rivals, however, business-stealing effects may be producing decreasing total spillover effects on TFP.

In order to provide a rationale for this non-linear result, let's assume that the program facilitates one obvious innovation in a particular region-sector (e.g., adoption of numerical control machinery). Most of what a firm has to learn from observing others can be learned from the early adopters. When the proportion of supported firms is low, knowledge spillovers might be important and dominate market-stealing effects. However, knowledge spillovers might have decreasing returns as more supported firms adopt the technology (not much is left to be learned after the few early adopters adopted the innovation). In contrast, when the program supports a greater share of a firm's rivals (which in turn incorporate the technology and hence become more efficient), the negative business-stealing effect on laggards may be assumed to be linear. Under these assumptions, as more firms adopt the new technology, the negative impact on the remaining firms will dominate. The combination of these two effects would be consistent with an inverted-U curve as the one simulated in Figure 5.[20]

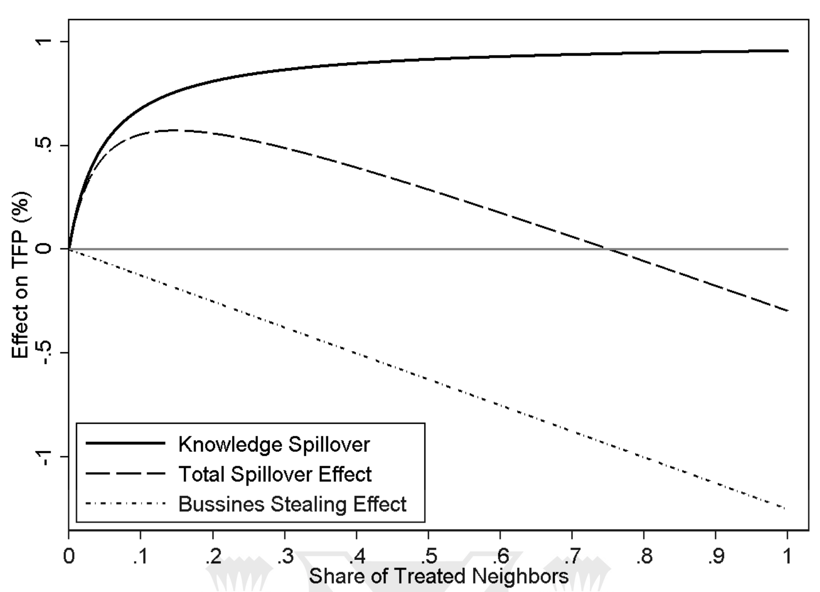
Figure 4. Spillover Share and Effect on TFP (Total + FONDEF)



[20] Let T be the number of supported firms and N the universe of firms. For our simulation, we define:

- Knowledge spillovers $KS = \frac{T/N}{0.05 + T/N}$;
- Business-stealing effect $BS = -T/N$; and
- Total spillover effect $TS = KS + BS$.

Figure 5. Simulation: Knowledge Spillover Effects vs. Business-stealing Effects



6. Robustness Checks

6.1. Falsification Tests: Changes in Geographic and Technological Distances

The key assumption of our spillover measure is that knowledge flows more easily among geographically proximate firms that belong to the same sector. That is, proximity in both geographic and technology spaces is necessary for spillovers to occur.

To show that the results obtained in Section 5 are not spurious correlations, and to validate our assumption, we explore how the spillover effect varies with changes in geographic and technological distances. The idea behind these falsification tests is that the inherent validity of the results would be limited if we obtain similar or larger spillover effects with more distance in the geographic and/or technological spaces.

First, we compared within-region/within-sector spillover estimates from Table 3, Columns 3 and 4 (and reproduced here in Table 5, Columns 1 and 2) with the effect of the share of treated firms from other sectors and other regions (across-region/across-sector spillover effect, shown in Table 5, Columns 3 and 4). Finding similar effects would cast doubt on the validity of our hypothesis and main results, as they would imply that geographic and technological proximity do not affect spillovers. As shown in Table 4, the spillover effects disappear when looking across regions and sectors.

Second, we construct the share of treated firms within the sector but outside the region, as well as the share of treated firms outside the sector but within the region. The lack of significant results in Columns 5–8 suggests that both geographic and technological proximity are needed for spillovers to occur. Spillovers do not seem to travel well on either the geographic or technological dimensions alone.

Table 5: Falsification Tests: Changes in Geographic and Technological Distances

	Total factor productivity							
	Within region - Within sector		Across region - Across sector		Across region - Within sector		Within region - Across sector	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.0423**		0.0433***		0.0425***		0.0415***	
	(0.017)		(0.016)		(0.017)		(0.016)	
FONTEC		0.0416*		0.0402**		0.0401*		0.0401*
		(0.023)		(0.024)		(0.023)		(0.022)
FONDEF		0.0429*		0.0465*		0.0459*		0.0439*
		(0.029)		(0.027)		(0.029)		(0.027)
Spillover share	0.1733***		-0.0252		-0.0065		-0.1234	
	(0.060)		(0.105)		(0.040)		(0.145)	
Spillover share FONTEC		-0.0192		0.2180		-0.0056		0.0059
		(0.181)		(0.503)		(0.095)		(0.313)
Spillover share FONDEF		0.2230***		-0.1241		-0.0074		-0.1443
		(0.062)		(0.208)		(0.044)		(0.138)
Age & Age^2	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	-	-	Yes	Yes	Yes	Yes
Sector-Year effect	Yes	Yes	-	-	Yes	Yes	-	-
Region-Year effect	Yes	Yes	-	-	-	-	Yes	Yes
Number of firms	8,576	8,576	8,576	8,576	8,576	8,576	8,576	8,576
Observations	63,863	63,863	63,863	63,863	63,863	63,863	63,863	63,863
R-squared	0.937	0.937	0.937	0.937	0.937	0.937	0.958	0.958

*Notes: (a) Estimates of fixed-effects model. (b) Clustered standard errors at 2-digit sector-region in parentheses. (c) ***, **, * statistically significant at 1%, 5%, and 10%. (d) "-" means omitted because of collinearity with the spillover variable*

6.2. Random Treatment: Disregarding Agglomeration Effects

Our spillover measure, by construction, could be correlated with the size and productivity of a sector in a region, and therefore, capture agglomeration effects. To discard this hypothesis, we create random treatment variables with the same mean by year as the original ones (overall treatment, FONTEC and FONDEF). For this, we use uniformly distributed random variates on the interval [0,1) and replicate the share of supported firms by entry year for each treatment.

Table 6 shows the results of estimating Equation (5) and (6) using the random variables of the overall treatment, FONTEC and FONDEF, and the resulting spillover variables. As shown in this table, neither the direct effects nor the spillover effects are statistically different from zero.

Table 6: Random Treatment and Agglomeration Effects

	TFP	
	(1)	(2)
Random Treatment	-0.0081	
	(0.011)	
Random FONTEC		0.0127
		(0.028)
Random FONDEF		-0.0413
		(0.030)
Spillover share (Random Treatment)	0.0876	
	(0.073)	
Spillover share Random FONTEC		0.0081
		(0.121)
Spillover share Random FONDEF		-0.0514
		(0.109)
Age & age^2	Yes	Yes
Firm effect	Yes	Yes
Time effect	Yes	Yes
Sector-year effect	Yes	Yes
Region-year effect	Yes	Yes
Number of firms	8,576	8,576
Observations	63,863	63,863
R-squared	0.937	0.937

*Notes: (a) Estimates of fixed-effects model. (b) Clustered standard errors at 2-digit sector-region in parentheses. (c) ***, **, * statistically significant at 1%, 5%, and 10%.*

6.3. Common Support of Firms

To strengthen the validity of our results, we run regressions (5) and (6) on a common support sample created by selecting from the universe of firms those firms that are similar to beneficiaries in terms of pretreatment observed characteristics, including the trends of relevant variables. This strategy involves three steps: (i) estimate the probability of participating in the programs (i.e., the propensity score) with a probit model using two-year lagged information –excluding from the pool all post-treatment observations of beneficiary firms;[1] (ii) restrict the sample to a common support area based on the propensity score;[2] and, (iii) estimate using FE. Table 7 shows that the effects of our variables of interest are robust. The coefficients show very similar values compared to the main results. While both programs have a direct impact on productivity, only FONDEF positively affects the productivity of other firms in the same region-sector.

6.4. Similarity Control Variable

Another problem with the estimation of the spillover effects is that firms in the region-sector with more treated firms can increase their productivity not because there are more treated firms but because there are more firms with similar characteristics to the treated ones. To test this hypothesis, we use the propensity score obtained in the previous exercise and add to Equations (5) and (6) a variable that measure the share of non-treated firms in the sector-region that are very similar to the treated firms.[3]

Columns 5 and 6 in Table 7 shows the results of this robustness test. Our results remain equal after controlling for the degree of similarity between treated and non-treated firms in a region-sector.

7. Conclusion

There is increasing interest in Latin American and Caribbean countries in granting fiscal incentives to encourage private investment in R&D. This interest has inspired a diverse set of policy experiments, ranging from the provision of matching grants to tax incentives. There is a need to assess the extent to which these interventions have corrected the various market failures that hinder private sector investment in R&D. However, many of the impact evaluations that have been carried out so far focus on the subsidies' impacts on direct beneficiaries (treated firms). Although much of this evidence suggests that these interventions have succeeded in increasing firm-level innovation investment (and sometimes productive performance), these findings are not sufficient to claim that the policy interventions have been effective. To the extent that R&D subsidies are justified by the presence of knowledge leakages and spillovers, an informative impact evaluation should also look at the programs' impact on the performance of indirect beneficiaries.

This paper aims to narrow this knowledge gap by focusing on the effects of two matching grant schemes to promote firm-level R&D investment in Chile. The analysis applies fixed-effects techniques to a novel dataset that merges several waves of Chile's National Manufacturing Surveys with register data on the beneficiaries of both programs. The differences in the structure of the two programs enable a more nuanced analysis. While one program subsidizes intramural R&D projects (FONTEC), the other (FONDEF) finances extramural R&D projects conducted by firms in collaboration with research institutes. This difference is important since, due to their collaborative nature, FONDEF projects may be more generic and more prone to knowledge leakages than the intramural R&D promoted by FONTEC.

The results suggest that only FONDEF-funded projects generate positive spillover effects on non-beneficiary firms. We find that while FONTEC-supported projects have a positive, significant impact on the direct beneficiaries, they have no effect on indirect beneficiaries. In other words, FONTEC subsidies would not be justified based on our analysis.

Are there potential alternative explanations? After all, FONDEF and FONTEC also differ in other

[1] We include in the probit model: TFP, sales, employment, skilled labor, export status, age and age squared, dummies for years, whether the firm has foreign direct investment, size, sector and region. We also include two-year lagged information on TFP, sales, employment, skilled labor and export status.

[2] We adopt a min-max criterion and eliminate control group firms that present a higher or lower average propensity score than the maximum or minimum propensity score of the treatment group, respectively.

[3] Similar non-treated firms are defined as firms with a propensity score that satisfied this condition:

$$\overline{pscore}_{treated} - SD\left(pscore_{i,treated}\right) \leq pscore_i \leq \overline{pscore}_{treated} + SD(pscore_{i,treated})$$

dimensions. First, FONDEF allocates resources based on a call-for-proposals system, which promotes direct competition for resources across projects, and thus may fund higher-quality projects. Second, FONDEF might address a coordination failure by giving firms access to sophisticated technological infrastructure that is available only in universities or research centers, thus allowing them to implement more complex R&D projects. Both alternative explanations would be expected to result in larger treatment effects for direct beneficiaries of FONDEF. This is not the case in our empirical results, which suggests that the direct effects of both treatments are broadly the same.

Our findings also generate complementary evidence on two important underlying mechanisms that might trigger these spillovers. First, spillovers have non-linear effects on productivity, which may be due to a combination of pure knowledge spillover effects and business-stealing mechanisms. These non-linear effects have two important implications for policy design: (1) there may be a critical mass in the number of treated firms that must be reached in order to generate these spillovers (i.e., pilot programs or small programs might not induce any spillovers at all) and (2) there are saturation points (i.e., programs that are too large will dilute the true knowledge spillovers through business-stealing effects).

Second, we implement several falsification tests, changing the location and technology distances in the measurement of spillovers. The results show that both geographic and technological proximity are required for the occurrence of spillovers. However, we also show that these spillovers are not the results of simply agglomeration effects or due to the presence in the cluster of untreated firms that are similar in terms of observable characteristics as treated firms. In other words, a treatment (a subsidy) must be present in order to generate spillover effects.

An important policy implication of our results is that innovation policy designs that encourage research collaboration among different actors, particularly firms and universities or technological institutes, should be preferred over those that simply subsidize intramural R&D. As for Chile, the country is one of the few cases of growth success in Latin America as it currently shows a per capita income of nearly $21,000 and it has been an OECD member since 2008. However, its TFP is about 68% of the USA one—just slightly higher than the 1990 ratio.[4] Its total R&D expenditure is around 0.35% of the GDP, which is the lowest value among the OECD countries, with just one third of this being financed by firms.[5] So, it is clear that Chile suffers from an innovate shortfall and that more should be done in order to increase private sector R&D investment and accelerate productivity growth. However, this requires not only increasing the coverage of R&D policies, as they are able to increase firm productivity, but also putting special attention regarding how these policies are designed. In other words, and based on our results, Chile should expand FONDEF's coverage by re-allocating FONTEC resources to it. However, we acknowledge that collaborative schemes such as those encouraged by FONDEF require collaborative partners with enough human capital and technological infrastructure to address the technological challenges faced by the firms, as well as firms with enough absorptive capacity to adopt the solutions developed. Thus, although collaborative schemes might work for a middle-income country such as Chile, they might not be the best solution for less developed countries.

[4] PWT 9.0.
[5] RICYT (2019).

Table 7: Direct and Spillover Effects on Productivity – Common Support

	Total factor productivity					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0571***		0.0567***		0.0565***	
	(0.013)		(0.013)		(0.013)	
FONTEC		0.0596***		0.0599***		0.0603***
		(0.018)		(0.017)		(0.017)
FONDEF		0.0535*		0.0508*		0.0500*
		(0.030)		(0.030)		(0.030)
Spillover share			0.1601**		0.1590**	
			(0.072)		(0.072)	
Spillover share FONTEC				-0.1207		-0.1021
				(0.207)		(0.208)
Spillover share FONDEF				0.2263***		0.2184***
				(0.076)		(0.074)
Share of similar FDT firms					0.0091	
					(0.015)	
Share of similar FONTEC firms						-0.0118
						(0.012)
Share of similar FONDEF firms						0.0202
						(0.016)
Age & age^2	Yes	Yes	Yes	Yes	Yes	Yes
Pscore & pscore^2	Yes	Yes	Yes	Yes	Yes	Yes
Firm effect	Yes	Yes	Yes	Yes	Yes	Yes
Time effect	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year effect	Yes	Yes	Yes	Yes	Yes	Yes
Region-year effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	6,959	6,959	6,959	6,959	6,959	6,959
Observations	40,880	40,880	40,880	40,880	40,880	40,880
R-squared	0.947	0.947	0.947	0.947	0.947	0.947

*Notes: (a) Estimates of fixed-effects model. (b) Clustered standard errors at 2-digit sector-region in parentheses. (c) ***, **, * statistically significant at 1%, 5%, and 10%.*

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IV. Credit cards issued by non-financial companies: An alternative tool for financial inclusion and economic development?*

Abstract

This study assesses the impacts of acquiring a credit card offered by a non-financial company in Colombia. The card, which is mainly targeted at low-income and unbanked individuals, can be used to fund home improvements and purchase home and personal goods in selected stores. We find that access to the credit card fostered financial inclusion and improved households' standard of living and well-being. Beneficiaries were more likely to obtain financing through credit cards, and increased their total debt and expenses in credit repayments while reducing the likelihood of borrowing from informal credit sources. However, we find no effect on accessing credit from the traditional financial sector. Acquiring the card also increased the likelihood of making key home improvements and purchasing certain expensive time-saving durables. Finally, the household's saving capacity increased, which signals an improvement in economic well-being and shows that the debt repayment is manageable.

Keywords: Alternative credit, credit cards, financial inclusion, economic development, impact evaluation, developing countries.

JEL Classification: D14, E51, G23, I30, O12, O54.

*This version of the study is under-review in The Journal of Development Effectiveness and coauthored by Viviane Azevedo (IDB), Lucas Figal Garone (UdeSA-IDB), Alessandro Maffioli (IDB), and Liliana Olarte Rodriguez (Y Analytics). The authors would like to thank Maria Laura Lanzalot and Patricia Yanez Pagans for their useful comments, the Colombian survey firm Sistemas Especializados de Información for its support with data gathering, and Cynthia Boruchowicz, Andrés Sebastián Mena, Johann Salgado, and Germán Pulido for their comments and excellent research assistance. The 2015 survey was funded by the Inter-American Development Bank (IDB). The opinions expressed in this publication reflect the views of its authors and not necessarily those of the IDB Group, its respective board of directors, or the countries they represent. The usual disclaimers apply.

1 Introduction

Many interventions have been proposed to solve the world's most entrenched development problems, particularly related to poverty reduction. These interventions range from child nutrition programs designed to reduce disparities in future productivity and incomes to programs to improve property rights or market functioning. In recent decades, efforts to reduce poverty and foster economic development have also focused on the potential transformative power of access to the financial system (Karlan & Morduch, 2010; Bruhn & Love, 2014; Dupas et al., 2018). As a key enabler for development, financial inclusion is firmly placed on the agenda of most national and sub-national governments as a key policy priority. Indeed, financial inclusion has been identified as an enabler for 11 of the United Nations' 17 Sustainable Development Goals (Klapper et al., 2016).[1]

Demirgüç-Kunt et al. (2008) describes the rationale for placing financial systems at the center of the development process. Inclusive and well-functioning financial systems are crucial for channeling resources more productively and efficiently and ensuring that risk is assumed by those with the greatest capacity to manage it. This, in turn, generates higher levels of growth (Aghion et al., 2005) and more equitable income distribution (Beck et al., 2007), and therefore reduces poverty (Zhang & Naceur, 2019). Indeed, in the absence of inclusive financial systems, poverty traps can hamper economic development since access to financing allows people to invest in their education and dwellings, save, finance projects, become entrepreneurs and improve their standard of living (Demirgüç-Kunt & Klapper, 2012; Demirgüç-Kunt, Klapper, & Singer, 2017).

The most obvious path to promoting greater access to financial services is to strengthen the traditional financial sector (Arbeláez et al., 2007). However, developing regions usually face several macro- and micro-level barriers to access. At the macro level, these challenges include vulnerability to volatile capital flows; a low capacity to devise and implement smart macro-prudential policies and regulations; institutional weaknesses; and a lack of strong legal, informational, technological, and physical infrastructure (such as an inclusive banking infrastructure and efficient retail payment systems). Developing countries also typically have inadequate national personal identification systems, fewer consumer protection regulations, and more informal labor structures (Shimada & Yang, 2011; Grandolini, 2015; Rojas-Suarez, 2016). At the micro level, asymmetric information and economies of scale generate bottlenecks in access to finance. These traditional market failures are compounded by the population's generally low level of financial literacy and a limited supply of useful and adequate financial products and services that cater to low-income people's needs (Grandolini, 2015; World Bank Group, 2016a).

For example, access to (and the use of) credit from financial institutions is dismally low in Latin America and the Caribbean (LAC), despite recent improvements

[1] Financial inclusion is mentioned in seven of the 17 Sustainable Development Goals (SDGs): no poverty (SDG 1); zero hunger (SDG 2); good health (SDG 3); gender equality (SDG 5); decent work and economic growth (SDG 8); industry, innovation and infrastructure (SDG 9); and reduced inequalities (SDG 10). Financial inclusion can also contributes to: quality education (SDG 4); clean water and sanitation (SDG 6); affordable and clean energy (SDG 7); and, peace, justice and strong institutions (SDG 16).

in other financial inclusion indicators such as account ownership. In 2018, while around 50% of adults in LAC had a bank account, which includes 40% of the poorest, only 20% of adults owned a credit card, and 10% of the poorest 40% did so.[2]

In this context, alternative means of promoting access to financial services – such as credit cards from retail stores, public utility companies, and other non-financial institutions – have flourished in LAC. Non-financial companies are sometimes very well positioned to ease traditional barriers and open new opportunities for specific population niches – e.g. low-income people and/or those who are unbanked or underbanked – to access formal financial products. These opportunities usually present themselves to companies that already track a constant flow of costumer data that can be used to assess risks and therefore to reduce information asymmetries. Most of these companies also rely on digital (commercial and financial) platforms that allow them to effectively manage their pool of clients, reduce the cost of lending, and gain significant scale. It is therefore not surprising that such companies have issued substantially higher numbers of credit cards in recent decades in LAC, and that the volume and total amount of transactions made using them has risen dramatically. For instance, retail stores managed around 210 million credit cards in 2018, which accounted for over 1.5 billion transactions totaling US$30 billion.[3]

However, and despite the increasing popularity of these alternative sources of credit, there is limited evidence of their effectiveness. Important questions remain: Do these credit cards effectively increase and improve financial inclusion? Do they help consumers access traditional loans or other bank products in the future? Do they promote the purchase of specific types of goods? Do they facilitate savings? Is debt repayment manageable?

To explore these questions, this study evaluates the impacts of having access to the credit card "Tarjeta EPM-Somos", offered by the Public Services Company of Medellín (Empresa de Servicios Publicos de Medellín, or EPM). The EPM card was designed to enhance financial inclusion, improve customers' quality of life by increasing their ability to make home improvements and acquire home durables, and foster the efficient use of public services. Although the card is offered to all EPM customers, it is mainly targeted at low-income customers and/or those with no or little previous experience with banks or credit institutions (the "unbanked"or underbanked population). The card can only be used to fund home improvements and purchase home and personal goods from selected stores.

We study a sample of approved applicants who either opted to take the credit card (treatment group) or declined the card (control group). We estimate the impacts using entropy balancing (EB) and ordinary least squares (OLS) methods on cross-sectional data, controlling for a very rich set of pre-treatment observable individual characteristics that might influence consumers' decisions about whether to accept the card. We then check the robustness of the results combining EB with a fixed-effects (FE) approach using retrospective data – which enables us to also control for unobservable characteristics that remain constant over time – and correcting for Multiple Hypothesis Testing (MHT).

We find that access to an EPM credit card fosters financial inclusion and improves

[2]Euromonitor Passport Database from Euromonitor International (Feb 2019).
[3]Euromonitor Passport Database from Euromonitor International (Feb 2019).

households' standards of living and well-being. Three main results emerge from our analysis. First, having an EPM card increased the likelihood of obtaining financing through credit cards (whether issued by EPM or banks or other non-financial institutions) as well as the amount of total debt and expenses in credit repayments, but decreased the probability of borrowing money from family members. However, we find no effect on the probability of obtaining traditional financial products (i.e. savings account, loans, or credit cards) from banks. Second, acquiring an EPM card is associated with making key home improvements, including renewing floors, kitchens, and bathrooms, and acquiring time-saving durable goods such as washing machines, which positively affects the household's quality of life. Finally, we find positive impacts on subjective well-being, namely households' saving capacity.

To the best of our knowledge, this is the first study to evaluate the effects of a credit card designed and provided by a non-financial company, similar in nature to retail store cards which are very popular in the region. Yet despite their popularity, it is not known whether this type of instrument contributes to financial inclusion and economic development. The paper contributes to the growing literature on the effects of access to credit for low-income and unbanked (or underbanked) people in developing countries. Although several prior studies have explored the macro-level effects of financial development on economic growth (Hassan et al., 2011; Arcand et al., 2015; Cecchetti & Kharroubi, 2012) and the impact of access to microcredit on business profits, consumption, and poverty reduction (Augsburg et al., 2014; Angelucci et al., 2013; Tarozzi et al., 2013; Attanasio et al., 2014; Banerjee et al., 2015), there is little evidence on the micro effects of other types of credit.

The rest of the paper is organized as follows. Section 2 discusses and reviews the literature on financial access and economic development and provides an overview of the EPM credit program. Section 3 defines the identification strategy, describes the sample, and offers descriptive statistics. Section 4 presents the results. Section 5 explains the robustness tests, and Section 6 concludes.

2 Background

2.1 Financial Access and Economic Development

Although financial access is a broad concept that encompasses a variety of services such as savings accounts, insurance, and credit lines, the international literature has focused mainly on microcredit provided to start or expand a business, and its impact on poverty reduction. According to Banerjee et al. (2015), throughout the 1990s and the beginning of the 2000s, microcredit generated considerable enthusiasm and raised hopes that it could rapidly and effectively help reduce poverty.[4] The height of publicity for microcredit came in 2006, when the Nobel Peace Prize was awarded to the microfinance company Grameen Bank and its founder, Muhammad Yunus.

[4]For instance, Burgess & Pande (2005) and Bruhn & Love (2014) report on non-experimental studies in India and Mexico, respectively, which found that an increase in the supply of financial services to poor and vulnerable populations reduced poverty and created employment for the poorest people, increased the number of new businesses they started, and boosted their incomes, among other effects.

However, impact evaluations on the area of microfinance that directly addresses the problem of causality have only begun to proliferate in the last decade; these studies have analysed interventions in several countries such us Bosnia-Herzegovina (Augsburg et al., 2014), Ethiopia (Tarozzi et al., 2013), India (Banerjee et al., 2015), Mexico (Angelucci et al., 2013), Mongolia (Attanasio et al., 2014), Morocco (Crépon et al., 2011), and the Philippines (Karlan & Morduch, 2010). A recent study by Meager (2018), which jointly estimates the average effect and the heterogeneity of effects across the aforementioned studies, finds that the impact on household business and consumption variables is unlikely to be transformative and may be negligible.

The empirical evidence on the impacts of microcredit has called into question the excessive attention given to it at the expense of other financial products, and the great expectations of poverty reduction associated with it. According to Karlan & Morduch (2010), the financial needs of the poor go beyond microcredit provided to start or expand a business, many of which are similar to those of higher-income households, such as mechanisms to manage their cash flow, accumulate assets over the short and long term, and manage risk. As Collins et al. (2009) explain in an appraisal of the financial lives of the poor and quasi-poor in Bangladesh, India, and South Africa, the financial activities of these populations are influenced by a basic combination of needs – i.e. guaranteeing daily meals, managing illnesses, paying for school expenses, improving their dwellings, and taking advantage of investment opportunities – that far exceeds creating, managing, or growing a small business.

Traditional microcredit is therefore just one of many possible financial mechanisms for poverty reduction, and is not necessarily the most effective Karlan & Morduch (2010). Financial inclusion mechanisms should also consider the needs of the poor and vulnerable beyond business creation and expansion.

Some basic needs are related, for instance, to the dwelling conditions and the possession of durable goods for the home. Due to their limited access to credit, low-income people often find it difficult to pay for such goods and home improvements. Rojas (2015) present evidence from 17 LAC countries indicating that 12% of homes have at least one of three types of qualitative shortages due to the use of poor construction materials: poor roofs, poor walls, and dirt floors. These shortages present significant heterogeneity between and within countries, and affect mostly countries with lower per capita GDP and households in the first deciles of the income distribution.[5] Possession of home durable goods follows a similar pattern. In LAC, 63% of households own a washing machine, compared to more than 85% in the United States (US), France, and the UK. These goods are heavily skewed toward the upper income brackets in LAC. In Ecuador, for example, 100% of households in the highest income decile have a washing machine, compared to only 6% of those in the lowest decile.[6]

Non-financial companies have responded to low-income people's inability to ac-

[5]For example, in Bolivia, Guatemala and Nicaragua, qualitative shortages affect more than 30% of households, while in Chile and Uruguay such shortages are close to 0%. In addition, around 20% of houses in the first income quintile in LAC present at least one type of qualitative shortage, while for the 5th quintile only 1% of households have shortages.

[6]Euromonitor Passport Database from Euromonitor International (Feb 2019).

cess traditional forms of financing for these types of investments by granting access to loans or credit, usually by issuing credit cards (Figal Garone et al., 2019).[7] While formal financial entities require applicants to have a credit history and collateral in case they default, these alternative credit cards often only require a valid ID and a work/income certificate (or sometimes a recommendation from a current customer), and customers may be instantly approved. By reducing transaction costs and information asymmetries, these non-financial companies provide financing with better terms and conditions, especially for the low-income and/or unbanked or underbanked population.

These alternative sources of credit often allow households to increase their investments in home improvement and acquire key durable goods. Such home improvements produce significant positive impacts on their standard of living and well-being (Bouillon, 2012). Previous studies have found that improving the quality of materials used to construct houses has positive effects on health (Cattaneo et al., 2009; Thomson et al., 2013; Galiani et al., 2017), children's education (Katzman, 2011; Moreno, 2011; Rosero, 2012; Rojas, 2015), and adults' well-being due to increased satisfaction with their dwelling and quality of life (Cattaneo et al., 2009; Mitchell et al., 2016; Galiani et al., 2017).[8]

Multiple empirical studies have demonstrated the importance of acquiring durables such as electrical appliances. For instance, labor-saving housing technologies have the potential to increase female participation in the formal labor market (Coen-Pirani et al., 2010; Ishani & Yabin, 2014; Chen et al., 2015).[9] Improved domestic appliances , such as cooking stoves, may also have positive effects on health (Smith-Sivertsen et al., 2009; Bensch & Peters, 2012; Hanna et al., 2016). Furthermore, the time saved by the use of home durables has positive effects on family relationships, including childcare, which improves children's education and reduces child labor (Chen et al., 2015; García-Jimeno & Peña, 2017; Kerr, 2019).

Credit cards issued by non-financial companies can also serve as a pathway to the traditional financial system. A possible channel for financial inclusion is through a reduction in information asymmetries caused by the generation and sharing of new credit records (Padilla & Pagano, 1997; Jappelli & Pagano, 1999). For instance, access to these credit lines allows unbanked users to access a different type of credit (Arbeláez et al., 2007), the probability of being approved for a formal bank loan (Agarwal et al., 2018), raise credit limits, and foster competition between lenders (Foley et al., 2018).

Finally, there is evidence that the use of new forms of consumer credit (or better

[7]Several non-financial companies in the region have been expanding their credit programs to low-income customers, including El Grupo Monge (Nicaragua, Peru, Honduras, Guatemala, El Salvador, and Costa Rica), Regal Forest Holdings (Trinidad, Guyana, Costa Rica, Barbados, and Paraguay), Garbarino (Argentina), Supermercados Peruanos (Peru), Distribuidora Liverpool, Grupo Famsa and Chedraui (Mexico), La Ganga (Ecuador), Exito, Alkosto, La 14 and Olimpica (Colombia), Falabella (Peru, Argentina, Colombia, Chile), and Ripley, Cencosud, Walmart, and Elecktra (across LAC).

[8]However, the positive effects on subjective well-being may be not lasting due to people's hedonic adaptation. Galiani et al. (2018) find that most of the positive effects on subjective well-being reported by Galiani et al. (2017) disappear after 24 months.

[9]Coen-Pirani et al. (2010) show that the acquisition of washing machines, dryers, and refrigerators explains 40% of the increase in US female labor participation between 1960 and 1970.

and increased access to it) can affect individuals' financial behavior and economic performance. Previous studies have shown that more experienced credit card users display better financial behavior and pay fewer financial fees (Agarwal et al., 2008). Furthermore, access to consumer credit positively impacts job flows, earnings, and entrepreneurship (Herkenhoff et al., 2016b); allows unemployed workers to increase the time they can dedicate to job searching and choose better-paid positions (Herkenhoff et al., 2016a); and improves credit scores (Brown et al., 2019). Finally, there is evidence that consumer credit enhances job retention, food consumption and subjective well-being (Karlan & Zinman, 2010), mortgage repayment rates (Morse, 2011), and job performance (Carrell & Zinman, 2014).[10]

2.2 The EPM Social Financing Program

Colombia is a typical LAC country with a low level of financial development. Its financial depth, approximated by the ratio of private credit to GDP, is far below that of high-income countries – 47% vs. 145% (World Bank Group, 2016b). However, the indicator for financial inclusion[11] increased from 55% in 2008 to 79% in 2017.[12] Additionally, 27% of the adult population has a credit card and 23% has a consumer credit product (Banca de las Oportunidades, 2017).[13] Yet financial access in Colombia remains very unequal: only 5% of the poorest 40% of the adult population reports having a credit card (Demirgüç-Kunt, Klapper, Singer, Ansar, & Hess, 2017). Thus, the proliferation of alternative credit is not surprising: non-financial companies provide financing to an estimated 18% of the population (Banca de las Oportunidades, 2014). Indeed, the number of retailer store credit cards issued nearly doubled between 2011 and 2017, from 3.8 million to 9.3 million.[14]

EPM is a 100% state-owned enterprise founded in 1955 in Colombia that provides household utilities such as electricity, natural gas, water, sewerage, and sanitation. In 1998, it was renamed the State Industrial and Commercial Company (Empresa Industrial y Comercial del Estado) under the ownership of the Municipality of Medellín. The company has a presence in seven countries, with 48 enterprises. It has become the second-most important business group in Colombia and the largest

[10]This evidence is also related to a body of literature on the impact of access to high-cost consumer credit and *payday* loans, which have been found to have negative effects such as increased stress, depression, and personal bankruptcy (Morgan & Strain, 2007; Skiba & Tobacman, 2007; Melzer, 2011; Campbell et al., 2012).

[11]Financial inclusion is defined as the percentage of adults with at least one financial product in a formal financial institution. In Colombia, this indicator mostly includes institutions overseen by the Superintendencia Financiera de Colombia (Colombian government agency responsible for overseeing all banking institutions and preserving the stability of the securities market), and excludes those overseen by Superintendencia de la Economía Solidaria (known as Supersolidaria, the Colombian government agency in charge of overseeing institutions such as cooperatives, employee funds, etc.).

[12]The percentage for 2017 increases to 80% when all financial entities are considered (credit establishments, cooperatives overseen by Supersolidaria, and non-governmental organizations).

[13]These numbers were obtained by dividing the total number of adults with a credit card (9.2 million) or a consumer credit product (8 million) by the adult population in the year (33.83 million) reported in Banca de las Oportunidades (2017).

[14]Euromonitor Passport Database from Euromonitor International (2017).

public household utilities supplier. It provides services to more than 13 million Colombians and nearly 7 million customers in other Central American countries.[15]

With the support of the Inter-American Development Bank Group, EPM created the Social Financing Program in 2008, which aims to provide accessible credit to those at the base of the pyramid.[16] The program provides a card with revolving credit to allow EPM customers to purchase more than 229 products and services, including mainly home and personal goods (electrical and gas appliances, audio and video equipment, entertainment, technology, etc.), home improvement materials, transport, utilities, and water supply (Appendix A.1). The card can be used in 130 affiliated establishments, including seven chain stores that operate nationally (Appendix A.2).

The program differs from traditional forms of credit in three main ways. The first difference is that EPM is a non-financial company: its main activity is to provide public utilities (i.e. non-financial services). The second is how the EPM screens and approves customers and issues the card. EPM uses the billing information and utility payment records of millions of its customers to evaluate the credit card applications. All customers with a record of paying their utility bills on time are eligible to apply. Applicants are then assessed using a scoring model that employs various socio-demographic variables. This approach lessens the information requirements requested by traditional banks, and thus attracts low-income applicants as well as individuals with no (or poor) credit history. The third difference is the card's potential use: customers can only use the card to purchase the goods described above from participating stores.

Although this program may share some commonalities with traditional approaches to microcredit, such as the size of the loans or the use of proceeds in some cases, the products differ in structural ways: while microcredit is granted to entrepreneurs to promote entrepreneurship as a route out of poverty, the EPM program is designed to help supply people's more basic needs, such as improving the quality of their homes or owning electrical appliances, while also functioning as a gateway to access the financial system. Also, unlike some forms of microcredit it does not require social collateral (e.g. group lending with joint liability).

The EPM program seeks to produce three main impacts. First, it aims to increase and improve low-income and unbanked people's access to credit services at competitive market interest rates – 21%, vs. the 100–150% paid by the non-bankarized sector of the population to purchase electrical appliances in Medellín at the time of the program's inception. This would also help customers build up a credit history that can in turn pave the way to accessing other traditional financial services. Second, the program is expected to enhance beneficiaries' quality of life by providing access to financing to implement home improvements and purchase durable goods, along with other goods and services. Finally, the program aims to boost the efficient consumption of public services (electricity, gas, and water) by giving beneficiaries the chance to replace outdated appliances with more efficient ones.

To achieve these objectives, a beneficiary profile was created in 2009, targeting

[15] EPM Group. Estamos ahí, con toda la energía. Retrieved from https://www.epm.com.co/

[16] In October 2015, the program was renamed the SOMOS Recognition Program, and the EPM card was renamed the SOMOS card.

the lower-income segments of the population (strata 1, 2, and 3). These segments have the lowest levels of access to financial services, and are therefore the most likely to resort to informal credit markets, which have much higher interest rates and often engage in predatory lending practices. Starting in 2009, a differential interest rate[17] was established based on each borrower's income stratum.[18] This system was abandoned in late 2015 because the variable nature of the rate resulted in variable repayment stipends, which often caused administrative problems. The maximum interest rate allowed by law (29.45% as of October 2018) is now charged for all strata.[19]

2.3 Approval, Take-up and Use Rate of the EPM Card

Customers apply for a card either electronically via the EPM webpage or through a commercial advisor at one of the customer service points located in selected chain stores in Antioquia (the department in which Medellín is located). To be eligible for the card, a series of preliminary conditions must be met (see Table 1).[20]

Table 1: Conditions of Access

1	Be a customer of EPM (user of at least one of the company's public household utilities).
2	The customer must be between 18 and 74 years old.
3	The customer's supply of any of the services provided by EPM must not have been cut off on more than two occasions over the last 12 months.
4	The service must not be cut off at the time of the credit request.

Source: Official website of the SOMOS recognition program. Retrieved from https://www.somosgrupoepm.com/.

Applicants who fulfill these conditions must fill out a credit application form. The information requested on this form is flexible enough to allow housewives and self-employed and retired individuals to apply (see Appendix A.3). EPM then uses a logistic probability model to classify applicants according to their non-payment risk. This credit rating methodology is more appropriate for the program's pool of

[17]Individuals classified as income strata 1–4 were charged an interest rate of FTD (fixed-term deposits) +11 basis points, whereas those in strata 5 and 6 were charged a rate of FTD+15 basis points. The FTD is the average interest rate that banks, savings and housing corporations, financial corporations, and commercial financing companies commit to paying savers for 90-day fixed-term deposit certificates.

[18]In Colombia, residential buildings that receive public services are classified into six groups according to their geographic location. Residents of areas classified as stratum 1 pay the lowest utility bills, and those in areas classified as stratum 6 pay the highest rates. Stratification does not take into account personal or household income, although strata and income are highly positively correlated.

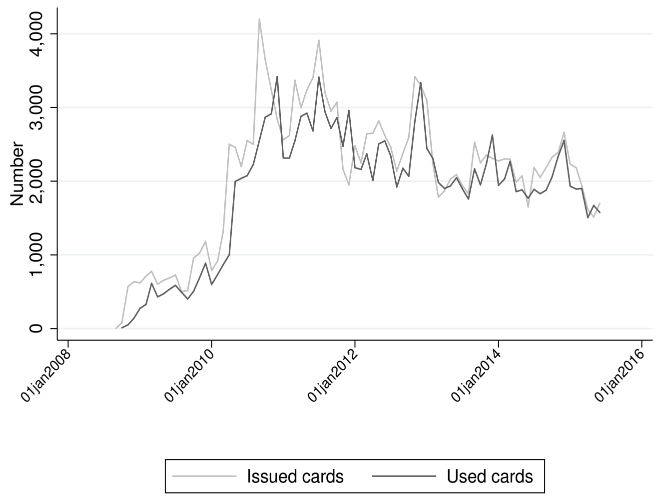
[19]Grupo EPM. Términos y Condiciones. Retrieved from https://www.somosgrupoepm.com/descubre/terminos.

[20]According to the information provided on EPM's website, a clean credit report is not required, but the applicant's estimated risk level must be above the threshold defined by EPM.

applicants, since it can be used to evaluate the creditworthiness of people with scant resources whose income cannot be easily verified. The score is tabulated based on 15 variables, which are weighted according to their relative importance.[21]

According to information provided by the EPM group, by December 2016 around 204,000 cards had been issued, 88% of which had been used at least once (Figure 1). The total value of the transactions has been growing since the program's inception. More people are choosing to use a higher percentage of their credit limit, increasing from an average of 25% of the limit in 2009 to an average of around 100% by 2014.

Figure 1: Cards Issued and Cards Used



Note: Author's own calculations using data provided by EPM.

To shed some more light on these preliminary figures, we also explored a more comprehensive dataset provided by EPM with administrative information on 9,478 individuals (5,293 men and 4,185 women) who applied for a card from September to December 2013. The credit rating scores ranged between 642 and 974. Applications that scored over 732 (n = 9,121) were approved, while those scoring less were denied (n = 357) (Figure 2). Program take-up was high: 76.3% of those who were approved decided to accept the card. An additional 5.3% of those who were initially rejected received a card.[22] Of those who accepted the card, 95% used it at least once, and used the card's credit lines up to 137% of its value.

3 Empirical Strategy

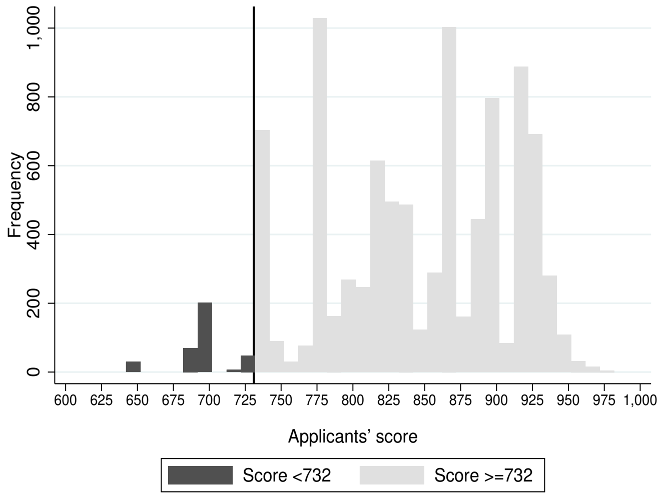
3.1 Identification Strategy

It is difficult to measure the impact (causal effects) of the program (i.e. how many durable goods a customer purchased because they obtained an EPM card) because

[21]EPM does not make the details of these variables public.

[22]For information about rejected applicants, see (Appendix B).

Figure 2: Applicants by Score (Sep-Dec 2013)



Source: Administrative data provided by EPM.
Note: The black vertical line represents the minimum score for approval.

it is impossible to know how many he or she would have bought without the card. Therefore, we built an appropriate comparison group to estimate this counterfactual.

Individuals who scored just below the approval threshold are likely to be similar in observable and unobservable characteristics to those scoring just above the threshold. Thus, the barely rejected applicants represent a counterfactual group to help us estimate the actions of the applicants just above the threshold (the barely accepted applicants) if they had *not* obtained an EPM card. Although this scenario seems ideal for applying a regression discontinuity design (RDD) to estimate impacts, given the assignment to treatment mechanism and its outputs, in this case its implementation is unsuitable. Very few individuals scored below the threshold, and they appear to be outliers with extremely negative credit histories (see Appendix B). In other words, the assumptions to apply RDD are not met in this scenario. This pattern is found both in the September to December 2013 universe of applicants and in the sample we employ to estimate impacts.

To analyze the impact of acquiring the EPM card on the outcomes of interest, we therefore compare the group of approved applicants (score $\geqslant 732$) who accepted the card (participants or treatment group) with the group of approved applicants who declined the card (non-participants or control group). Since both groups are comprised of approved applicants, they might have similar observable and unobservable characteristics before the intervention. Nevertheless, some differences between the two groups may remain. We thus employ EB techniques to correct for potential biases and identify effects.

EB is a multivariate reweighting method proposed by Hainmueller (2012). This reweighting scheme assigns a scalar weight to each sample unit such that reweighted groups satisfy a set of balance constraints that are imposed in the sample moments (for example, the median) of the covariates' distribution. This approach allows us to obtain a high degree of covariate balance by construction, while keeping the weights as close as possible to the base (unit) weights to prevent a loss of information. As described by Hainmueller (2012), the weights ω_i are chosen as follows:

$$\min_{\omega_i} H(\omega) = \sum_{\{i/T_s=0\}} h(\omega_i)$$

subject to balance and normalizing constraints

$$\Sigma_{\{i/T_s=0\}}\omega_i k_{ri}(X_i) = m_r \quad \text{with} \quad r \in 1, ..., R \text{ , and}$$
$$\Sigma_{\{i/T_s=0\}}\omega_i = 1 \quad \text{and} \quad \omega_i \geq 0 \quad \forall i \quad \text{such that} \quad T_s = 0,$$

where T_s is the treatment status, $h(.)$ is a Kullback (1959) entropy metric, and $k_{ri}(X_i) = m_r$ describes a set of R balance constraints imposed, in our case, on the covariate mean of the reweighted control group in order to equal the covariate mean of the treatment group.[23] In other words, EB allows to construct a 'synthetic' control group based on pre-treatment characteristics. By doing this, EB helps to eliminate a potential source of bias since weighted non-beneficiaries are expected to be more similar to beneficiaries.[24]

Thereafter, we use the weightings that emerge from EB to estimate the following equation using the OLS method:

$$Y_i = \beta T_i + \gamma X_i + \epsilon_i$$

where T_i is the binary variable that indicates whether a person received the card or not (the treatment variable), X_i is a vector of control variables, and ϵ_i is the error term *iid* and estimated robustly. Our parameter of interest is β, which will capture the effect of the program on the outcome of interest Y_i or, in other words, the program's impact on i) access to credit, ii) characteristics of the dwelling and possession of durable goods, and iii) efficiency in the use of public services.

3.2 Sample and Descriptive Statistics

A unique survey designed to measure the EPM program's impacts on relevant outcomes was conducted from July to September 2015 in Medellín and its surrounding municipalities. The survey contained 11 modules that asked about the following aspects of applicants' households: housing (type of dwelling, homeownership, basic services, etc.), household goods (electrical appliances, audio and video equipment, etc.), household characteristics (size, ages, health, educational level, etc.), work (main occupation, business owner, etc.), income, expenses, access to financial services, use of time, subjective well-being, perception of EPM, and savings.

Figure 3 displays the 1,400 individuals who were surveyed from a pool of 2,286 applicants who applied for the credit card between September and December 2013 and whose credit score was near the approval threshold of 732 (range = 640–781). Initially the target was to survey all 357 individuals who scored below the threshold as well as a random sample of 1,528 of the 1,929 individuals who scored above the threshold, for a total of 1,855 individuals. This approach was designed to provide a

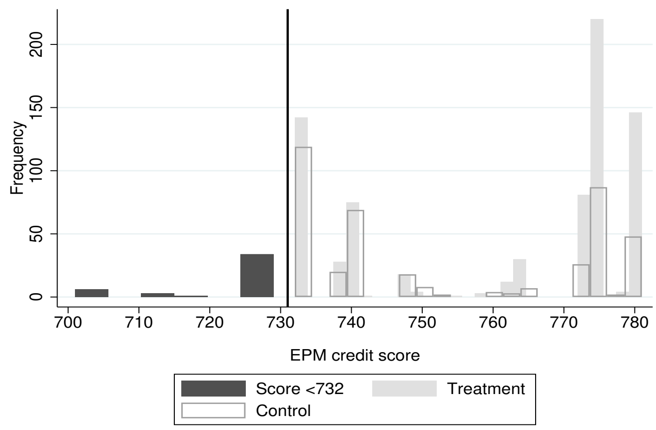
[23]We use the STATA package called ebalance, introduced by Hainmueller & Xu (2013). For implementation issues, see Hainmueller (2012).

[24]Heckman et al. (1997) and Heckman et al. (1998) describe these sources of biases.

better understanding of the characteristics of the individuals who were *not* approved, and to evaluate whether estimating the impact using an RDD would be feasible. However, due to challenges associated with conducting the fieldwork, a total of 221 individuals were surveyed below the threshold and 1,179 above the threshold. Of the approved applicants surveyed, 65% accepted the card, and of the rejected applicants surveyed, 4% managed to acquire the card anyway.

The data confirmed that individuals just above and just below the threshold are not comparable (see Appendix B). The treatment group was defined as approved applicants (score above 732) who accepted the card (766 individuals – solid gray bars to the right of the approval score in Figure 3), and the control group as approved applicants who declined the card (413 individuals – unshaded bars to the right of the approval score in Figure 3).

Figure 3: Histogram of Surveyed Individuals (1,400 applicants)



Note: The black line represents the approval score. Individuals who scored below 732 (solid dark bars to the left of the black line) were not eligible for the EPM card. Individuals who scored 732 or higher (solid and unshaded bars to the right of the black line), were eligible for the card, but not all of them accepted it.

Table 2, Column 1 displays applicants' characteristics and information from the baseline year of 2013, when the card applications were submitted, using retrospective questions from the survey. The approved applicants who accepted vs. declined the card are relatively homogeneous except for homeownership, consumption of public utilities, and ownership of certain durable goods. The approved applicants who accepted the card are more likely to be homeowners and to have Internet access; they also report higher levels of consumption of water and sanitation services. Additionally, these applicants less frequently report having been denied a loan, and are more likely to have opened a credit line with a store. They were also more likely to own washing machines, bicycles, cameras, and PCs. However, these differences disappear once the observations are reweighted using the weights that emerge from the EB method, which shows that the treatment and control groups are balanced in all baseline characteristics, and are therefore comparable (Table 2, Column 2).

Table 2: Descriptive Statistics for EPM Card Applicants at Baseline (2013)
Approved Applicants Who Accepted vs. Rejected the Card

	1. Sample				2. Weighted sample (EB)			
	Mean EPM card	Mean No EPM card	Difference	P-value	Mean EPM card	Mean No EPM card	Difference	P-value
Socio-demographic								
EPM score	761.61	753.44	8.17	0.00	761.61	761.64	-0.02	0.98
Age	33.34	32.51	0.83	0.10	33.34	33.34	0.00	0.99
Age^2	1,181.34	1,123.63	57.71	0.10	1,181.34	1,181.48	-0.13	1.00
Gender	0.53	0.56	-0.04	0.18	0.53	0.53	0.00	0.99
Married/common law relationship	0.57	0.56	0.02	0.59	0.57	0.57	0.00	0.98
Number of dependents	1.86	1.91	-0.05	0.41	1.86	1.86	0.00	0.98
Homeowner	0.16	0.12	0.04	0.09	0.16	0.16	0.00	0.96
Owner of motorcycle	0.14	0.12	0.02	0.43	0.14	0.14	0.00	0.99
Socioeconomic stratum 1	0.14	0.16	-0.02	0.28	0.14	0.14	0.00	0.97
Socioeconomic stratum 2	0.58	0.57	0.01	0.63	0.58	0.58	0.00	0.98
Education								
Completed primary education	0.07	0.08	-0.01	0.58	0.07	0.07	0.00	0.96
Completed secondary education	0.55	0.56	-0.01	0.82	0.55	0.55	0.00	0.99
Technical/Technological	0.37	0.35	0.02	0.59	0.37	0.37	0.00	0.99
Employment								
Has some kind of work contract	0.98	0.97	0.01	0.43	0.98	0.98	0.00	1.00
Incomes and expenses								
Log value of income from economic activity and other incomes	13.84	13.84	0.00	0.87	13.84	13.84	0.00	0.99
Log value of total income of applicant and partner	14.09	14.07	0.01	0.72	14.09	14.09	0.00	0.99
Log value of monthly personal expenses	12.44	12.44	0.00	0.97	12.44	12.44	0.00	0.99
Log value of total expenses	12.61	12.61	0.00	0.93	12.61	12.61	0.00	1.00
Public services								
Log value of energy consumption	9.26	8.97	0.29	0.23	9.26	9.24	0.01	0.96
Log value of water consumption	7.33	6.68	0.65	0.01	7.33	7.34	0.00	0.99
Log value of sanitation services consumption	7.34	6.87	0.47	0.08	7.34	7.34	0.00	0.99
Log value of natural gas consumption	4.27	4.33	-0.06	0.83	4.27	4.27	0.01	0.98
Log value of energy consumption in Kwh	4.16	4.03	0.12	0.27	4.16	4.15	0.01	0.96
Log value water consumption in m3	1.99	1.81	0.18	0.02	1.99	1.99	0.00	0.99
Log value of sanitation services consumption in m3	1.91	1.79	0.12	0.11	1.91	1.91	0.00	0.99
Log value of natural gas consumption in m3	1.19	1.18	0.00	0.96	1.19	1.18	0.00	0.98
Energy source for cooking is natural gas/electricity	0.70	0.70	0.00	0.97	0.70	0.70	0.00	0.99
Internet service	0.53	0.45	0.09	0.00	0.53	0.53	0.00	0.98
Characteristics of the dwelling								
Number of floors	1.14	1.14	0.00	0.95	1.14	1.14	0.00	0.99
Number of rooms	2.64	2.54	0.1	0.13	2.64	2.64	0.00	0.97
Number of rooms with exclusive use	2.54	2.46	0.07	0.24	2.54	2.54	0.00	0.97
Number of kitchens	1.00	0.99	0.01	0.11	1.00	1.00	0.00	0.74
Number of kitchens with exclusive use	1.00	0.99	0.01	0.09	1.00	1.00	0.00	0.74

Table 2: (Cont) Descriptive Statistics for EPM Card Applicants at Baseline (2013)

	1. Sample				2. Weighted sample (EB)			
	Mean EPM card	Mean No EPM card	Difference	P-value	Mean EPM card	Mean No EPM card	Difference	P-value
Number of bathrooms	1.15	1.14	0.02	0.50	1.15	1.15	0.00	0.99
Number of bathrooms with exclusive use	1.14	1.13	0.01	0.59	1.14	1.14	0.00	0.96
Presence of flush toilet and mains sewerage	0.98	0.99	-0.01	0.39	0.98	0.98	0.00	0.97
Roof finished	0.93	0.93	0.00	0.93	0.93	0.93	0.00	0.99
Living room/bedroom floors finished	0.60	0.64	-0.05	0.10	0.60	0.59	0.00	0.98
Living room/bedroom walls finished	0.97	0.97	0.00	0.89	0.97	0.97	0.00	0.97
Kitchen floors finished	0.62	0.65	-0.03	0.27	0.62	0.62	0.00	0.98
Kitchen walls finished	0.96	0.95	0.01	0.44	0.96	0.96	0.00	0.98
Bathroom floors finished	0.75	0.75	0.00	0.88	0.75	0.75	0.00	0.97
Bathroom walls finished	0.91	0.91	0.00	0.81	0.91	0.91	0.00	0.99
Ownership of electrical appliances and other durable goods								
Washing machine	0.66	0.60	0.06	0.05	0.66	0.66	0.00	0.98
Refrigerator	0.92	0.91	0.02	0.35	0.92	0.92	0.00	0.96
Stove	0.95	0.96	-0.01	0.29	0.95	0.95	0.00	1.00
Oven	0.17	0.14	0.03	0.12	0.17	0.17	0.00	0.99
Microwave oven	0.40	0.32	0.08	0.01	0.40	0.4	0.00	0.99
Water heater	0.24	0.26	-0.02	0.4	0.24	0.24	0.00	0.99
TV	0.93	0.93	-0.01	0.67	0.93	0.92	0.00	0.96
DVD	0.56	0.55	0.01	0.77	0.56	0.56	0.00	0.98
Sound system	0.59	0.57	0.02	0.45	0.59	0.59	0.00	0.98
Digital player	0.13	0.14	-0.01	0.72	0.13	0.13	0.00	0.99
Video game console	0.19	0.17	0.02	0.49	0.19	0.19	0.00	1.00
Electric motorcycle	0.01	0.01	0.00	0.73	0.01	0.01	0.00	1.00
Bicycle	0.38	0.33	0.05	0.09	0.38	0.38	0.00	0.99
Pay TV	0.79	0.76	0.03	0.22	0.79	0.79	0.00	0.97
Camera	0.33	0.25	0.08	0.00	0.33	0.33	0.00	0.99
PC	0.46	0.39	0.07	0.01	0.46	0.46	0.00	0.98
Laptop	0.21	0.20	0.01	0.60	0.21	0.21	0.00	0.99
Tablet	0.10	0.09	0.01	0.54	0.10	0.10	0.00	0.99
Financial inclusion								
Has a savings account	0.74	0.70	0.04	0.20	0.74	0.74	0.00	0.97
Refused credit by banks/financial entities	0.09	0.16	-0.06	0.00	0.09	0.09	0.00	0.99
Has bank credit card	0.13	0.12	0.02	0.40	0.13	0.13	0.00	0.99
Has a credit card (with a non-financial institution other than EPM)	0.10	0.08	0.03	0.14	0.10	0.10	0.00	0.99
Has credit with banks	0.16	0.15	0.02	0.38	0.16	0.16	0.00	0.99
Has credit with credit card	0.08	0.07	0.020	0.29	0.08	0.08	0.00	0.99
Has credit with cooperatives	0.08	0.08	0.010	0.55	0.08	0.08	0.00	0.99
Has credit with stores	0.22	0.16	0.06	0.020	0.22	0.22	0.00	0.99
Has credit with compensation funds	0.05	0.03	0.01	0.22	0.05	0.05	0.00	1.00
Has credit with family members	0.04	0.06	-0.02	0.16	0.04	0.04	0.00	1.00
Has trickle credit	0.02	0.02	0.00	0.87	0.02	0.02	0.00	1.00
Has credit from employer	0.01	0.02	-0.01	0.49	0.01	0.01	0.00	1.00
Observations	766	413			766	413		

Note: statistics were constructed using administrative data provided by EPM.
This data reflects information submitted and/or collected by EPM at the time individuals applied for the credit card from September to December 2013.

4 Results

Table 3 (Column 1) displays the impacts of the program from the EB+OLS estimations. The results are divided into five groups: financial inclusion (Panel A), home characteristics (Panel B), ownership of electrical appliances and other durable goods (Panel C), spending on public utilities (Panel D), and time spent on household chores and subjective well-being (Panel E).

Financial inclusion (Panel A). The results show that the program increased beneficiaries' access to finance. Having an EPM card increased the likelihood of obtaining financing through credit cards (whether issued by EPM or banks or other non-financial institutions) by almost 7 percentage points. In line with this result, the program increased the amount of total debt by 143% and expenses in credit repayments by 120%, likely due to an increase in the number of purchases and payments made with the EPM card.[25] These findings reinforce the statistics presented in Section 3 that most of the applicants who obtained an EPM card in fact used it.

In addition, card users were 4 percentage points less likely to borrow from family members. Thus, the program fostered the substitution of informal credit for formal credit sources. However, no statistically significant effects were found regarding cardholders' access to traditional financial products from banks.

Table 3: Impacts of the EPM Card
Panel A. Financial Inclusion

Outcomes	EB + OLS (1)	EB + FE (2)
Has credit with credit cards	0.066***	0.066**
	(0.025)	(0.030)
Log value of total amount of debts	1.431***	1.516***
	(0.423)	(0.548)
Log value of expenses in credit repayments	1.197***	-
	(0.369)	-
Has savings account, credit card, or loan from banks	0.014	0.023
	(0.021)	(0.029)
Has credit with cooperatives, stores, or compensation funds	0.017	0.012
	(0.032)	(0.045)
Has credit from family members	-0.039**	-0.039**
	(0.015)	(0.017)
Observations	**1,179**	**1,179**

Notes: (1) Column 1: OLS regression using EB weights, robust standard errors in parentheses. The set of control variables includes 2015 survey data on demography, education, employment, income and expenditures, and access to public services. The control variables also include EPM credit scores and 2013 administrative data on financial inclusion, characteristics of dwelling, durable goods, and access to public services. (2) Column 2: FE regression using EB weights, clustered standard errors at the individual level in parentheses. (3) ***,**, * statistically significant at 1%, 5%, and 10%.

[25] For all outcomes in logs, we apply the inverse hyperbolic sine transformation (IHST). Unlike traditional log transformation, IHST is defined at zero and can be interpreted in the same way as a log-transformed dependent variable. For a recent application, see Alix-Garcia et al. (2015).

Home characteristics and durable goods (Panel B and Panel C). In line with the program's aims, the results show that having the card is associated with an increase in the number of floors, kitchens, and bathrooms in the beneficiaries' dwellings and in the likelihood of purchasing a washing machine. These findings are not trivial, given that beneficiaries can use the EPM card for a variety of products including personal goods and time-spending technologies. However, they choose to use it to buy materials for key home improvements and a key, time-saving, durable good.

Panel B. Characteristics of the Dwelling

Outcomes	EB + OLS (1)	EB + FE (2)
Number of floors	0.049**	0.049**
	(0.020)	(0.025)
Number of rooms	0.067	0.067
	(0.042)	(0.052)
Number of kitchens	0.007**	0.007*
	(0.004)	(0.004)
Number of bathrooms	0.045**	0.045*
	(0.018)	(0.023)
Roof finished	-0.002	-0.003
	(0.010)	(0.014)
Observations	**1,179**	**1,179**

Notes: (1) Column 1: OLS regression using EB weights, robust standard errors in parentheses. The set of control variables includes 2015 survey data on demography, education, employment, income and expenditures, and access to public services. The control variables also include EPM credit scores and 2013 administrative data on financial inclusion, characteristics of dwelling, durable goods, and access to public services. (2) Column 2: FE regression using EB weights, clustered standard errors at the individual level in parentheses. (3) ***,**, * statistically significant at 1%, 5%, and 10%.

Dwellings represent perhaps the main asset of lower-income individuals. For instance, in Colombia, a 1% increase in the home quality index (e.g. after implementing home improvements) produces an estimated 1.6% increase in the value of the home and a correlated increase in possible rentals. Furthermore, households with a covered floor or remodeled bathrooms and kitchens experience a 15-20% increase in asset value.[26]

EPM advertises laptops and TVs more than washing machines, as the former are considered more attractive purchases. However, according to the National Quality

[26] Authors' own calculations based on the Inter-American Development Bank "Sociometro" database.

Panel C. Purchase of Durable Goods

Outcomes	EB + OLS (1)	EB + FE (2)
Washing machine	0.059***	0.058*
	(0.022)	(0.036)
Refrigerator	0.001	0.001
	(0.008)	(0.017)
Stove	0.001	0.001
	(0.009)	(0.015)
Oven	0.000	-0.000
	(0.019)	(0.021)
Microwave oven	-0.037	-0.037
	(0.029)	(0.037)
Water heater	0.009	0.009
	(0.025)	(0.028)
TV	-0.006	-0.007
	(0.009)	(0.018)
DVD, sound system, or digital player	0.015	0.033
	(0.021)	(0.028)
PC, laptop, or tablet	0.037	0.001
	(0.025)	(0.034)
Observations	**1,179**	**1,179**

Notes: (1) Column 1: OLS regression using EB weights, robust standard errors in parentheses. The set of control variables includes 2015 survey data on demography, education, employment, income and expenditures, and access to public services. The control variables also include EPM credit scores and 2013 administrative data on financial inclusion, characteristics of dwelling, durable goods, and access to public services. (2) Column 2: FE regression using EB weights, clustered standard errors at the individual level in parentheses. (3) ***,**, * statistically significant at 1%, 5%, and 10%.

of Life Survey (DANE, 2015), only 59% of households in Colombia report having a washing machine, compared with 63% in the region as a whole and 85% in the United States.[27] Furthermore, while 100% of individuals in the 10th income decile in Colombia have a washing machine, only 19% in the 1st decile have one; this may be due in part to their price and the fact that they are harder to buy secondhand than other appliances.[28] Our results suggest that the EPM credit card has helped

[27]Authors' own calculations of occupied dwellings, based on the 2013 U.S. Census Bureau Household Survey.

[28]Data from Euromonitor International (2016) shows that the average retail price for a new washing machine is USD $332 – significantly more than the national minimum wage that year (approximately USD $230). Although other home goods appear to be just as expensive (for example, the average retail price for a new TV is USD $559), the replacement cycles for major appliances, like washers, and consumer electronics (i.e. TVs) are different. For instance, the replacement cycle for TVs in 2016 was approximately 6 years, while the expected lifespan of a washing machine was about 10 years according to the National Association of Home Builders. Since shorter life cycles are associated with faster price drops, it is plausible to assume that data on price averages of appliances sold last year may not necessarily reflect the prices paid by low-income consumers for TVs, as they may access these goods (including relatively newer models) at

close this gap in Colombia.

Public services (Panel D). We find no statistically significant effects regarding the use or expense of public services. Although the program aimed to foster a more efficient use of public services through the acquisition of more efficient durable goods, this potential reduction could have been cancelled out by improvements in the quality of the dwelling – such as the creation of more rooms – or the possession of additional home goods, which increase the use of electricity. The absence of such an effect is also a relevant result. It implies that individuals can access credit through the EPM card without a corresponding increase in expenditures on EPMs' services.

Panel D. Public Services

Outcomes	EB + OLS (1)	EB + FE (2)
Log value of EPM utility bill expenses	0.033	-
	(0.040)	-
Energy for cooking is natural gas/electricity	-0.020	-0.020
	(0.022)	(0.031)
Log value of propane gas expenses	0.325	-
	(0.230)	-
Observations	**1,179**	**1,179**

Notes: (1) Column 1: OLS regression using EB weights, robust standard errors in parentheses. The set of control variables includes 2015 survey data on demography, education, employment, income and expenditures, and access to public services. The control variables also include EPM credit scores and 2013 administrative data on financial inclusion, characteristics of dwelling, durable goods, and access to public services. (2) Column 2: FE regression using EB weights, clustered standard errors at the individual level in parentheses. (3) ***,**, * statistically significant at 1%, 5%, and 10%.

Use of time and subjective well-being (Panel D). We find no effects on cardholders' use of time. However, the results suggest that the program improves users' savings capacity and thus their subjective well-being. These findings indicate not only that the EPM card helps beneficiaries manage, control, and plan their family economy better, but also that the new debt they acquire is sustainable over time.

Overall, our findings bolster the arguments put forward by Karlan & Morduch (2010), who find that specific financial products for vulnerable people can be an effective way to satisfy their needs, such as consumption smoothing, facilitating access to durable goods, improving saving capacity and dwelling conditions, and obtaining loans for sporadic needs. The fact that more far-reaching effects were not found, such as access to the traditional financial sector, is also in line with the empirical evidence and the discussion presented in Section 2. According to the cited evidence, financial products targeted at poor and vulnerable segments of the population can be important for satisfying specific needs, but are often insufficient to achieve other development goals such as entrepreneurship growth and bankarization.

cheaper prices from secondhand markets.

Panel E. Use of Time and Subjective Well-being

Outcomes	EB + OLS (1)
Use of Time	
Time spent on household chores (hours)	-0.010
	(0.110)
Fraction of waking hours spent on household chores	0.000
	(0.007)
Subjective well-being	
Saving capacity in 2015 is better than in 2012	0.066**
	(0.033)
The economic situation in 2015 is better than in 2012	-0.006
	(0.032)
Moderately/entirely satisfied with the household financial situation in 2015	-0.023 (0.031)
Observations	**1,179**

Notes: (1) Column 1: OLS regression using EB weights, robust standard errors in parentheses. The set of control variables includes 2015 survey data on demography, education, employment, income and expenditures, and access to public services. The control variables also include EPM credit scores and 2013 administrative data on financial inclusion, characteristics of dwelling, durable goods, and access to public services. (2) Column 2: FE regression using EB weights, clustered standard errors at the individual level in parentheses. (3) ***,**, * statistically significant at 1%, 5%, and 10%.

5 Robustness Checks

5.1 Entropy Balancing and Fixed Effects

The main advantage of the econometric method implemented (EB+OLS) is that it can be applied to a cross-sectional sample of individuals. However, the main disadvantage is that its underlying assumption of conditional independence could be too strong. It implies that the evaluator observes all the information that determines (influences) participation in the program.

Yet it is likely that only more motivated and entrepreneurial individuals accept the card once they are approved. Therefore, selection into the program (i.e., the decision to accept the card and use it) may also depend on characteristics that are unobservable to the evaluator. If an individual's capacity or motivation (or other factors) is among the drivers of participation, we cannot control for self-selection using EB+OLS.

Therefore, to test the robustness of our results, we combine EB with the FE methodology using retrospective data from 2013.[29] The FE methodology allows us to control for unobservable heterogeneities that are constant over time. For this purpose, we estimate the following equation:

[29] Figal Garone et al. (2015) provides a recent application of EB in combination with FE.

$$Y_{i,t} = \alpha_i + \beta T_{i,t} + \gamma X_{i,t} + \epsilon_{i,t}$$

where α_i captures fixed effects at the individual level, and $\epsilon_{i,t}$ are errors clustered at the individual level.

Table 3 (Column 2) confirms the previous results. Having an EPM card is associated with more and better access to credit, home improvements, and the acquisition of washing machines. It was not possible to estimate the effects on spending on public utilities, use of time, or subjective well-being using EB+FE as there is no retrospective data for these outcome variables.

5.2 Multiple Hyphotesis Testing

Given that more than one null hypothesis is tested simultaneously for each area of impact, we need to adjust p-values for the number of hypotheses tested. In other words, it is necessary to control for the "type I error" rate. Thus, we test the robustness of our results by correcting for MHT using Family-wise Error Rate and False Discovery Rate corrections, which are common practice in the literature.

Section 4 displays the p-values adjusted for MHT for all our outcomes of interest and for both the EB+OLS and EB+FE estimations. Our main results remain statistically significant across several corrections.

Table 4: Family-Wise Error Ratio & False Discovery Rate
Panel A. Financial Inclusion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Outcomes	Coeficient		p-value								q-value			
			Original		Bonferroni		Sidak		Westfall-Young		FDR		FDR Sharp	
	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE
Has credit with credit card	0.066	0.066	0.01	0.03	0.03	0.10	0.03	0.10	0.06	0.12	0.02	0.04	0.01	0.04
Log value of expenses in credit repayment	1.197	-	0.00	-	0.01	-	0.01	-	0.01	-	0.00	-	0.00	-
Log value of total amount of debts	1.431	1.516	0.00	0.01	0.00	0.03	0.00	0.03	0.01	0.03	0.00	0.03	0.00	0.03
Has savings account, credit card, or loan from banks	0.014	0.023	0.49	0.43	0.97	0.86	0.73	0.67	0.75	0.71	0.58	0.54	0.24	0.27
Has credit with cooperatives, stores or funds	0.017	0.012	0.60	0.80	0.97	0.86	0.73	0.80	0.75	0.79	0.60	0.80	0.25	0.47
Has credit from family members	-0.039	-0.039	0.01	0.03	0.04	0.10	0.03	0.10	0.06	0.12	0.02	0.04	0.01	0.04

Panel B. Characteristics of the Dwelling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Outcomes	Coeficient		p-value								q-value			
			Original		Bonferroni		Sidak		Westfall-Young		FDR		FDR Sharp	
	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE
Number of floors	0.049	0.049	0.01	0.04	0.06	0.22	0.06	0.20	0.14	0.25	0.04	0.09	0.04	0.10
Number of rooms	0.067	0.067	0.11	0.20	0.22	0.40	0.21	0.36	0.24	0.38	0.14	0.25	0.07	0.11
Number of kitchens	0.007	0.007	0.04	0.06	0.12	0.22	0.12	0.20	0.17	0.25	0.07	0.09	0.04	0.10
Number of bathrooms	0.045	0.045	0.01	0.05	0.06	0.22	0.06	0.20	0.14	0.25	0.04	0.09	0.04	0.10
Roof finished	-0.002	-0.003	0.82	0.85	0.82	0.85	0.82	0.85	0.84	0.87	0.82	0.85	0.20	0.33

Panel C. Purchase of Durable Goods

Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Coeficient		p-value								q-value			
			Original		Bonferroni		Sidak		Westfall-Young		FDR		FDR Sharp	
	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE	EB+OLS	EB+FE
Washing machine	0.059	0.058	0.01	0.10	0.08	0.93	0.07	0.62	0.13	0.60	0.08	0.93	0.08	1.00
Refrigerator	0.001	0.001	0.88	0.97	1.00	1.00	1.00	1.00	1.00	1.00	0.99	0.99	1.00	1.00
Stove	0.001	0.001	0.95	0.97	1.00	1.00	1.00	1.00	1.00	1.00	0.99	0.99	1.00	1.00
Oven	0.000	0.000	0.99	0.99	1.00	1.00	1.00	1.00	1.00	1.00	0.99	0.99	1.00	1.00
Microwave oven	-0.037	-0.037	0.19	0.32	1.00	1.00	0.78	0.93	0.82	0.93	0.58	0.96	1.00	1.00
Water heater	0.009	0.009	0.72	0.76	1.00	1.00	0.99	1.00	0.99	1.00	0.99	0.99	1.00	1.00
TV	-0.006	-0.007	0.50	0.72	1.00	1.00	0.98	1.00	0.99	1.00	0.91	0.99	1.00	1.00
DVD, sound system, or digital camera	0.015	0.033	0.47	0.24	1.00	1.00	0.98	0.89	0.99	0.88	0.91	0.96	1.00	1.00
PC, laptop, or tablet	0.037	0.001	0.14	0.98	1.00	1.00	0.70	1.00	0.77	1.00	0.58	0.99	1.00	1.00

Panel D. Public Services

Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Coeficient	p-value				q-value	
		Original	Bonferroni	Sidak	Westfall-Young	FDR	FDR Sharp
	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS
Log value of EPM utility bill expense	0.033	0.42	0.75	0.61	0.64	0.42	0.72
Energy for cooking is natural gas/electricity	-0.020	0.38	0.75	0.61	0.64	0.42	0.72
Log value of propane gas expenses	0.325	0.16	0.47	0.40	0.38	0.42	0.72

Panel E1. Use of Time

Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Coeficient	p-value				q-value	
		Original	Bonferroni	Sidak	Westfall-Young	FDR	FDR Sharp
	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS
Time spent on household chores (hours)	-0.010	0.92	1.00	0.99	0.97	0.97	1.00
Fraction of waking hours spent on household chores	0.000	0.97	1.00	0.99	0.97	0.97	1.00

Panel E2. Subjective Well-being

Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Coeficient	p-value				q-value	
		Original	Bonferroni	Sidak	Westfall-Young	FDR	FDR Sharp
	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS	EB+OLS
Saving capacity in 2015 is better than in 2012	0.066	0.04	0.13	0.12	0.16	0.13	0.15
The economic situation in 2015 is better than in 2012	-0.006	0.86	0.89	0.86	0.85	0.86	1.00
Moderately/entirely satisfied with the household financial situation in 2015	-0.023	0.44	0.89	0.69	0.72	0.67	0.80

6 Conclusion

To the best of our knowledge, this is the first study to evaluate the effects of credit products offered by non-financial companies. It evaluates the impacts of acquiring the EPM-SOMOS card on financial inclusion, the probability of making home improvements and purchasing durable goods, and efficiency in the use of public services.

This card represents a non-bank option for accessing credit, especially for vulnerable or informally employed people who have no (or a poor) credit history. Any adult customer of EPM's public utilities with a proven history of paying their bills is eligible for the card.

Three major results emerge from our study. First, EPM beneficiaries were able to access credit on better terms and conditions than via informal channels. They were more likely to use credit cards, which increased their level of debt and expenses in credit repayments. Although there was no noticeable effect on the probability of accessing traditional bank products (e.g. savings account, loan, or credit card), having an EPM card reduced the likelihood of borrowing from family members. Second, obtaining the EPM card is associated with making home improvements, such as increasing the number of floors, kitchens, and bathrooms. It also increases the likelihood of purchasing certain expensive durable goods, such as washing machines. Third, with regard to subjective well-being, an improvement in saving capacity was found. This finding is important, as it indicates that cardholders are better able to plan their family economy, and that the new debt acquired may be manageable over time. This is also relevant since bankarization programs from both microfinance institutions and non-banking institutions have been criticized for charging excessive interest rates, and thus causing over-indebtedness among their customers.

Although the program does not seem to have an impact on access to credit from the traditional financial sector, it does fulfill a significant need in Colombia and LAC more broadly to increase access to home improvements and technologies. The credit card is a viable product from both the supply side – enterprises from the real sector – and the demand side – informal and/or vulnerable people unable to access financing for home improvements and durable goods. On the supply side, the card assignment scheme (scoring) and the low default rates show that these types of products are viable for businesses in the real sector that already have a relationship with these segments of the population and are able to use the information generated during previous interactions with them. On the demand side, the card represents a viable – and perhaps the only – option for families with no credit history that need to finance home improvements or purchase expensive electrical appliances.

Policy makers and other interested stakeholders can work with non-financial companies such as public utilities companies, retail stores, and other types of firms to replicate such projects in other regions and countries. This type of program is expected to work particularly well when two conditions are met: there is a qualitative housing deficit and/or the adoption rate of household technologies is low, and there are high transaction costs and information asymmetries in access to credit, as is often the case among low-income and unbanked populations.

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Appendix

A Annex 1

A.1 List of Stores and Products Available with the EPM-SOMOS Card

Table 4: List of Products that can be Purchased with the SOMOS Card

ELECTRICAL and GAS APPLIANCES	
Large Electrical appliances	***Food preparation***
Electric and/or gas refrigerator	Sandwich maker
Electric and/or gas stove	Electric and/or gas rice cooker
Electric and/or gas cooker	Electric squeezer
Extractor hood parts	Toaster
Electric and/or gas heater	Electric and/or gas coffee maker
Electric and/or gas oven	Kitchen pots and pans
Washing machine and/or tumble dryer	Whisk
Sunken electric and/or gas stove	Microwave oven
Sewing machines	Toaster oven
Freezers	Electric can opener
Dishwashers	Electric juicer
Electric water dispenser	Electric carving knife
Electric and/or gas fireplace	Electric food processor
Electric and/or gas cooler	Blender and parts
Electric and/or gas revolving display case	Juice squeezers
Burners	Frying pan
Electric and/or gas barbecue	Meat-cutting machine
Spares and parts for large electrical appliances	Bread maker
Large electrical appliance combos	Stand mixers
Personal care	Electric and/or gas fryers
Hair curling or straightening iron	Hot dog machine
Hair dryer	Cupcake machine
Electric shaver and depilation machine	Fondue maker
Electric body and face massage machine	Chocolate fountain
Hair clippers	Electric kettle
Electric exercise treadmill	Popcorn maker
Electric stationary bicycle	Raclette maker
Electric elliptical trainer	Grill
Electric stair climber	Waffle or panini maker
Vibration platform machine	Pressure cookers
Home vaporizer	Small electrical appliance combos
Personal care electrical appliance combos	***Home ventilation***
Household cleaning	Air conditioning or heating
Electric polisher	Fan
Electric vacuum cleaner	Air filter
Dehumidifiers	Air purifier
Electric irons	Home ventilation electrical appliances combos
Household cleaning electrical appliance combos	

AUDIO and VIDEO	
Audio and video	***Portable audio***
Televisions	Audio players
Speakers	Recorders
Sound systems' mini and micro components	Radios: electric or with rechargeable batteries
Video player	Portable audio goods combos
Home theater	
Chargers and battery chargers	
TV mounts	
Universal remote control	
TV antennas: over the air and peripheral	
Audio and video goods combos	
ENTERTAINMENT	
Video and digital cameras	***Video games***
Video cameras	Video consoles
Digital cameras	Remote control
Digital picture frames	Video games
Electric musical instruments	Batteries and rechargeable batteries
Electric musical instruments	
Accessories for electric instruments	
TECHNOLOGY	
Computers	***Telephone***
Desktop computers	Fixed telephones (landline)
Laptop computers – tablets for children	Fax
Tablets	Fixed telephones (cordless)
Voltage regulator	Call identifier
Cameras for PCs	Cell phones (all makes)
Hard disks	Extension telephone wiring
CD/DVD unit	Batteries for cell phones and telephones
Video projector	Radiotelephones
Projectors and back projectors	SIM card
Screens	***Network equipment***
Computer workstation	Switch
USB devices (cool pad - lights' memory sticks)	Access point or router
Internet modem	Network cards
GPS	Video or sound cards
Peripheral computing devices	Security video recording equipment
Printers, scanners, and multifunctionals	***Software***
Printers	Licenses and home software
Multifunctionals	
Cash register	
Scanner	
Toner cartridges	

HOME IMPROVEMENTS	
Bathrooms	***Floors and tiling***
Sinks	Floors
Sinks with cabinets	Skirting boards
Toilet paper holders	Decorative borders
Towel rings	Ceramic tiles
Soap dish holders	Adhesives and screeds for ceramic, porcelain and wooden floors
Showers	Grouts
Taps and mixers	Drains
Baths	Painting or building tools
Sanitary ware	Architraves
Porcelain sanitary ware combo	Cement, lime and plaster
Tubes and fittings	Sand
Drainage grates	Bricks
U-bends	Paint, additives, 'matagén' - aniline colors
WC elbow joints	Chippings
Flexible couplings for sanitary ware	Doors and rails
Flexible sink couplings	Rebars, 'piragua'
Faucet and mixer combinations	Silicone coating
Shower cabins	CARPEFIT roofing felt - waterproofing
Specialty recessed bathroom furniture	Polyester fabric
Glue / PVC adhesive/cement removers	Ceilings, wood boarding, tiles
Dry wall false ceilings	Windows and rails
Filters and accessories	Bathroom plumbing
Tools for the home	Floor sealants
Low-energy bulbs	***Laundry rooms***
Electric jigsaws	Laundry tubs
Electric polishers	Clothes washing sink
Electric blowtorches	Washing machines
Electric drill	***Kitchens***
Electric sanders	Kitchen worktops with cabinet
Electric grinders	Kitchen worktops
Electric tools and parts	Cooker - drawer unit combo
Home security alarms	Water and gas regulating valves
Lighting, light-diffusing sheets	Dishwater baskets
Dimmers	Stainless steel bucket
Electronic ballasts	Stainless steel dishwater
Doorbells, switches, circuit breakers, plugs	Gas ring burner
Junction boxes 2x4 and 4x4	Kitchen hood grease traps
Ports for television and cable	Iron gas burner top
Gas and water pipes	Gas diffusers
Christmas lights	Kitchen furniture - premium tower cooker
Electrical cables and wires	Kitchen taps and mixers
Etc.	Kitchen plumbing
TRANSPORT	
Electric transport	***NGV***
Electric vehicles	NGV conversion
Electric motorcycles	
Electric bicycles	
SERVICES	
Electrical appliances	***Audio, video, and ICT***
Extended warranties	Audio, video, and ICT installation
Electrical and/or gas appliance installation	
Home improvements	
Home improvement installations	
WATER TREATMENT	
Equipment	
Pumps	

Note: Based on information from the official website of the SOMOS recognition program (EPM GROUP, 2016).

Table 5: Stores Affiliated with the EPM-SOMOS Program

HYPERMARKETS	**SEWING MACHINES**
Almacenes Exito	Antioqueña de Máquinas
Easy Colombia	Casasinger
Home Center	Macoser Familiar E Industrial
Makro	Máquinas De Coser Janome
Panamericana	Para Coser
Tiendas Jumbo	Servitejer Y Coser
Tiendas Metro	**GAS APPLIANCES**
GENERAL ELECTRICAL APPLIANCES	Mundial De Gas Y Agua
Navarro Ospina	Cobretec
Cacharreria Mundial	Comercializadora Sumeco
Casamagna	Dimargas
Centro Oriental	Famigas
Vima	Gas Y Hogar
Credihogar	Idegas
Dispufil	J&s Distrihogares
Spe	Maxiservicios
Electrobello	Mercantil Supernova
Haceb	Super Gas 21
Hogar Y Moda	**NATURAL GAS VEHICLES**
Inversiones Bermejal	Auto Francia
Almacen Nápoles 3	Euro G.n.v
Luma	Gas Inyección
Multi San Pedro	Gasexpress Vehicular
Multigangas	Suragas Medellín
Multihogar	**ELECTRIC MUSICAL INSTRUMENTS**
COMPUTERS, AUDIO, and VIDEO	Yamaha Musical
Celcomp	**HOME IMPROVEMENTS and DEPOSITS**
Celular	Aeroprofiles
Circulo Digital	Agencia Central
Comercializadora Tecnisumer	Alfagres
Cyberia.com	Alhelí Kitchens Y Bathrooms
Nexcom	Almacences Corona
Sistemas God	Arte Y Design
Etc.	Artefino
MOTORBIKES and ELECTRIC BICYCLES	Bazar Americano
Energy Motion	*Etc.*

Note: Based on information from the official website of the SOMOS recognition program (EPM GROUP, 2016).

A.2 Stores Affiliated to the EPM-SOMOS Program

A.3 Information Required for the Credit Card Application Form

Table 6: Information Required for the Credit Card Application Form

Employee	• Copy of the national ID • Proof of payment of the most recent utility bill
Self-employed	• Copy of the national ID • Proof of payment of the most recent utility bill • One of the following documents: – Income certificate – Bank statements from previous three months – Certificate from an official accountant – Certificate from a provider – Certificate from the Chamber of Commerce or firm's legal ID
Retiree	• Copy of the national ID • Proof of payment of the most recent bill • One of the following documents: – Copy of the last pension payment received – Bank statement from previous three months that reflects the periodic payment of the pension – Pension's legal documents (Resolución de la pensión)
Housewife	• Copy of the national ID • Proof of payment of the most recent utility bill • One of the following documents: – Proof of real property tax – Vehicle ownership – Bank statements from previous three months or proof of remittances' receipt

Note: Based on information from the official website of the SOMOS recognition program (EPM GROUP, 2016).

B Descriptive Statistics

We find some statistically significant differences between the characteristics of the approved vs. rejected applicants. The approved applicants were, on average, older, better educated, and had higher incomes, and were more likely to be married, self-employed, to own their own business, to be homeowners, and to have their own vehicle, among other characteristics.

Table 7: Descriptive Statistics, EPM Administrative Data. All Applicants from September–December 2013

	> Approval score		**< Approval score**		**p-value (Mean diff=0)**
	Median	Sd	Median	Sd	
Demographic					
Treated: has EPM card	0.76	0.43	0.05	0.22	**0.00**
Age	43.96	13.48	25.58	5.25	**0.00**
Gender	0.44	0.5	0.42	0.49	0.41
Married/common law	0.56	0.5	0.62	0.48	**0.01**
Education					
Less than primary education	0.01	0.09	0	0.05	0.31
Completed primary education	0.19	0.39	0.02	0.14	**0.00**
Completed secondary education	0.46	0.5	0.54	0.5	**0.00**
Completed technical/technological	0.23	0.42	0.44	0.5	**0.00**
Completed university or higher	0.12	0.32	0	0.05	**0.00**
Employment					
Employee	0.55	0.5	0.98	0.13	**0.00**
Self-employed	0.2	0.4	0.02	0.13	**0.00**
Housewife	0.12	0.32	0	0	**0.00**
Pensioner	0.13	0.33	0	0	**0.00**
Has some kind of work contract	0.55	0.5	0.98	0.13	**0.00**
Business owner					
Has own business	0.05	0.22	0.01	0.09	**0.00**
Business is affiliated with the Chamber of Commerce	0.12	0.33	0.17	0.41	0.73
Applicant salaries, incomes, and expenses					
Log value of total income	14.3	0.68	14.05	0.49	**0.00**
Log value of income from main economic activity	13.92	0.65	13.54	0.29	**0.00**
Log value amount from other incomes received	13.18	0.82	12.63	0.79	**0.00**
Log value incomes received by spouse	13.75	0.63	13.64	0.53	**0.02**
Log value total expenses	12.95	0.85	12.41	0.55	**0.00**
Log value of monthly personal expenses	12.6	0.68	12.28	0.52	**0.00**
Log value of monthly expenses from financial expenses	12.29	0.85	11.75	0.68	**0.00**
Log value monthly expenses arising from economic activity	12.46	1.43	11.7	1	0.19
Socioeconomic characteristics of the household					
Homeowner	0.5	0.5	0.01	0.07	**0.00**
Log value commercial value of dwelling	18.03	0.75	18.07	0.67	0.89
Socioeconomic stratum	2.31	0.66	2.18	0.65	**0.00**
Household structure					
Number of dependents	1.68	0.88	1.61	0.73	**0.10**
Vehicle ownership					
Ownership of own vehicle	0.06	0.24	0	0	**0.00**
Ownership of motorcycle	0.08	0.27	0.15	0.36	**0.00**
Ownership of vehicle for public use	0.02	0.13	0	0	**0.01**
Public utilities					
Log value of energy consumption in Kwh	4.28	1.74	3.96	1.9	**0.00**
Log value of energy consumption	9.44	3.64	8.89	4.05	**0.01**
Log value of water consumption in m3	2.04	1.16	2.05	1.14	0.84
Log value of value of water consumption	7.5	3.89	7.6	3.83	0.63
Log value of sanitation services consumption in m3	1.98	1.19	1.98	1.17	0.99
Log value of value of sanitation services consumption	7.6	4.19	7.67	4.16	0.76
Log value of natural gas consumption in m3	1.25	1.37	1.08	1.33	**0.02**
Log value of value of natural gas consumption	4.54	4.72	3.95	4.67	**0.02**
Observations	**9,121**		**357**		

Note: statistics were constructed using administrative data provided by EPM. This data reflects information submitted and/or collected by EPM at the time individuals applied for the credit card from September–December 2013.