Local Finance, Growth and Autocracy:
The Spanish Banking Expansion Plans

Master's Thesis

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Abstract

This study exploits the staggered implementation of the Banking Expansion Plans during the late Francoist regime as conditionally-exogenous shocks to local economic outcomes. Using an original data set that combines rich municipality-level data with the Bank of Spain's own information during the drafting of the Plans, this quasi-experiment provides compelling reduced-form evidence of a positive and lasting effect on local measures of growth and entrepreneurship associated to the state-led expansion of the banking sector. The effect is stronger for municipalities that lacked alternative financial intermediaries, and results remain significant irrespective of local economic potential and synchronous regional policies. Overall, this paper reinforces the argument that policy-led expansion of credit intermediaries can be an effective tool to promote growth and alleviate financing constraints.

JEL Codes: N0, N24, G21, L26.[1]

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I Introduction

The relationship between finance and economic growth has received considerable attention ever since the cross-country work of King and Levine (1993) empirically tested Schumpeter's argument that financial intermediaries fulfill an important role as agents that monitor, finance and foster entrepreneurship and innovation. In their absence, would-be entrepreneurs may lack sufficient resources to engage in self-employment and alter their production choices (Evans and Jovanovic 1989, Holtz-Eakin et al. 1994), reducing the rate of innovation and economic growth (Aghion and Bolton, 1997).[2] Much of the empirical evidence, however, employs cross-country comparisons to estimate the relationship between financial markets and economic performance (Levine, 2004), which is subject to identification concerns stemming from unobserved differences in monetary, legal and cultural frameworks (Levine et al., 2000). In addition, causally establishing the specific role financial institutions play in promoting entrepreneurship and growth within a country remains challenging due to the non-random allocation of financial services and the hardships associated with isolating the effects in quasi-experimental designs (Carlson et al., 2019).

Understanding within-country heterogeneity between financial development and economic outcomes is critical for both developing and developed nations. Small businesses and entrepreneurs concentrate their borrowing on local banks with which they establish long-term relationships (Strahan and Weston, 1998), affecting both the incentives and ability of these specialized banks to acquire information about the regional markets they operate in (Petersen and Rajan, 1995). Berger and Udell (1996) argue these long-term relationships generate distinct monitoring dynamics for small businesses over large firms, which helps explain credit heterogeneity at the regional or local level. Specialized banks are thus critical for the analysis of local financial development and the uneven effects they may have on the performance of small firms, who are ever reliant on local conditions for financing in both developing (Rajan and Zingales, 2003) and developed (Guiso et al., 2004) countries.

Although most empirical evidence points to a positive relation between financing conditions and innovation and growth (Levine, 2004), there are some noticeable demand-driven exceptions. Theoretical models, in which banks' screening and monitoring functions allow for a more efficient access to external finance by households and firms, crucially assume both demand and supply of loans to be interdependent and mutually reinforcing (Bencivenga and Smith 1991, Benhabib and Spiegel 2000). Against this, work in the empirical microfinance literature stresses that results from random experiments in developing economies can suffer from low take-up by individuals (Banerjee et al. 2015a, Dupas et al. 2012), while policies that aim at increasing banking services in rural areas can be short-lived due to large costs and low returns (Burgess

[2]See Buera et al. (2015) for a review of the macro-development literature on financial frictions and growth.

and Pande, 2005). In line with the fact that regional financial specialization is spatially sensitive and driven by long-term relationships (Berger and Udell, 1995), understanding the geographic relevance of financial institutions requires understanding their interaction with regional practices and the local history of banking services.

More encouraging results are to be found when the country's demand for financial services precedes its supply. Jayaratne and Strahan (1996) are the first to empirically document the effects from the US banking deregulation that took place between 1972 and 1991, and report positive results of the associated increase in financial development on growth. Similarly, Dehejia and Lleras-Muney (2003) study the impact of changes in US banking regulation in the first half of the XXth century, and find that financial development is positive for growth as long as the expansion of credit is not unrestrained. Fulford (2015) and Carlson et al. (2019) shift the focus to the National Banking Act of 1864 and exploit discontinuities in capital requirements to obtain similar effects from improved financing services in the context of a rapidly expanding economy.

This paper examines a similar period of fast financial and economic growth in the context of authoritarian Spain, where the 1962 reform of the banking *status quo* that had ground all branching expansions to a halt two decades earlier allowed for pent-up demand for financial services to be finally met in force by both commercial and savings banks. The 1962 Banking Law dictated that "more freedom and greater ease of access to, and exercise of, the banking profession" would be granted, allowing banks to close the gap between the demand and supply of financial services that had built over years of neglect and *status quo* (Malo de Molina and Martín-Aceña, 2013).[3] Following the long spell of thwarted growth opportunities, financial institutions used this novel form of expansion as their principal competitive instrument in the years that followed, increasing the volume of their deposits by location and advertising (Pons, 1999).

The development of the financial sector in the years that followed the 1962 Banking Law was framed by the so-called Banking Expansion Plans.[4] These Expansion Plans were prepared on an annual basis by the Bank of Spain at the behest of the Ministry of Finance, and determined the number and location of new branches commercial banks could open as well as the growth capacity each commercial bank had to expand.[5] Nine plans were drafted between 1964 and

[3] *Ley 2/1962, de 14 de abril, sobre bases de ordenación del crédito y de la Banca*, Official State Gazette, 16 of April 1962.

[4] Concerned with the capacity of commercial banks to expand rapidly following decades of stagnant branching growth, Article 7 of the 1962 Banking Law stated that "forthcoming changes to the existing regulation shall avoid the excessive proliferation of branches, which shall be objectively determined using criteria established by the Ministry of Finance so that the number of new branches is adjusted to the national needs".

[5] The formula that determined a bank's expansion capacity was complex and somewhat archaic, as it

1974, permitting a total of 2,549 new offices to be opened, which almost doubled the 2,758 that existed at the end of the *status quo*. Importantly, some of the municipalities included in the plans did not respond to strict economic criteria but instead were included due to their absence of banking services and relatively large population.[6]

To analyze the effects of these Banking Expansion Plans, I create a novel data set of Spanish municipalities with data on economic outcomes and proxy measures of entrepreneurship spanning between 1963 and 1975.[7] This data is matched with internal sources used by officials at the Bank of Spain in the drafting of the Expansion Plans, which I obtain from the Historical Archive of the Bank of Spain and which allows me to quantitatively verify the opening of branches, the number of banks that expressed interest for each municipality and their relative capital size. Unique to this dataset is information on informal financial services, which during the *status quo* acted in place of legal branching expansion as a mechanism of competition and which is likely to have moderated the effects of branches associated to the Expansion Plans. I also collect qualitative evidence to test whether the policy-driven guidelines the identification assumption rests on are reliable and time-invariant. Suggestions and comments made to the Bank of Spain by commercial banks on the issue of Expansion Plans are also collected, as well as all correspondence between official institutions involved in the process.

The identification strategy exploits the exogenous variation in bank branching growth caused by the inclusion in the Banking Expansion Plans of municipalities that had no access to financial services during the *status quo*. These municipalities were selected under an objective and publicly available set of rules, published in the First Expansion Plan and ratified as late as in the drafting of the Fourth Expansion Plan. Using the data collected from available municipal sources and the Bank of Spain, the identifying assumption of this paper is that, conditional on controlling for the variables used to gauge a location's fit in an Expansion Plan, the selection of non-banked municipalities is rendered as good as random.[8]

In a first stage, I show that the Expansion Plans strongly predict the likelihood of a municipality receiving a bank branch six months after the approval by the Ministry of Finance, with no anticipatory or free-riding behavior being observed among commercial banks. Importantly,

included the number of rooms in all branches a given bank possessed.

[6]This was acknowledged by the state as an unfortunate consequence of the *status quo*, and authorities justified the inclusion in the plans of these economically unremarkable municipalities on grounds that financing services were necessary to unlock their potential.

[7]The choice of a municipality as the unit of treatment by Spanish authorities reflects the geographical dispersion and poor transport communication that was endemic to Spain. As in Petersen and Rajan (2002) and Guiso et al. (2004), I posit that the municipality is the local market of relevance in this study, as individuals faced severe difficulties when trying to tap financial markets other than the local, immediate one.

[8]These municipalities were added to the Expansion Plans under category A. Inclusion under this criteria was largely determined by the relative size of the non-banked population, and not economic reasoning. The opposite holds true for categories B, C and D. See Section II for details.

the selection of municipalities is driven by the criteria detailed in the First Plan, but does not predict baseline economic outcomes. Conversely, the actual expansion of commercial branches in Spanish municipalities is shown to strongly correlate with baseline economic outcomes, suggesting the identification strategy is robust to endogeneity concerns stemming from the non-random allocation of bank branches.[9]

Having established that the selection of a non-banked municipality in a Banking Expansion Plan predicts a commercial branch expansion, I show that the exogenous improvement in financial services boosted local measures of entrepreneurship and retail activity, similar in size to results from Guiso et al. (2004). I also shed some light on the mechanisms that underpin this effect, exploiting the clear distinction in services provided by savings and commercial banks in the context of authoritarian Spain. The effect seems to be associated with the additional provision of short-term lending, a service that at the time was almost exclusively offered by commercial banks and that is well captured in the theoretical framework of Evans and Jovanovic (1989): commercial banks ease the constraint individuals face in regards to outside financing, which spurs entrepreneurship and local economic growth. A savings mechanism is also at play, with synchronous increases in savings bank branches shifting individuals' consumption patterns towards durable goods, a similar result to that of Banerjee et al. (2015a). Savings institutions limited ability to lend locally, however, does not have a major effect on entrepreneurship and trade activity.[10]

The effects of the Banking Expansion Plans are shown to be dynamic and build up over time as relationships between potential customers and financing institutions are established (Berger and Udell, 1995), information barriers are raised and monitoring procedures are defined. The strongest effects on entrepreneurship are concentrated in municipalities that receive a small-sized specialized bank branch, providing descriptive evidence that small commercial banks are better poised to monitor and lend to retail businesses in locations hitherto devoid of access to formal financial services on the back of a stronger grasp on idiosyncratic local market conditions (Petersen and Rajan, 1995). My findings are agnostic about the role of competition; instead, I exploit the lack of local incumbent competitors to isolate a credit provision channel from that of heightened competition characteristic of the existing literature (Jayaratne and Strahan, 1996). I also show that the results on entrepreneurship and retail activity are not dominated by thriving coastal areas or spatial proximity to state-sponsored industrial development nodes.

[9] Variation between the two variables stems from the fact that banks bid for the listed municipalities, and locations that did not receive any bids were not allocated.

[10] Mandatory investment requirements imposed on savings banks channeled over 80% of total deposits to large public spending programs (Comín, 2007).

Lastly, I highlight some of the risks associated with not accounting for alternative and informal financial intermediaries when empirically testing for the effects of an expansion in the formal financial sector. Using the Bank of Spain's internal dataset on irregular financial agents, I provide compelling evidence that these make-do institutions were effective substitutes for commercial banks. Municipalities where these agents were present experienced a much smaller increase in commercial licenses upon their inclusion in an Expansion Plan, while the effect in locations with no alternative sources of external financing was considerably larger. The sizeable degree of masked heterogeneity in the untapped demand for external financing serves as a cautionary tale of the downfalls of not properly accounting for informality when analyzing developing economies and their growing financial sectors.

This paper adds to the empirical literature of the effects of financial development on local economic activity and the dynamics of creative destruction and entrepreneurship. Black and Strahan (2002) show that the rate of business creation increased following deregulation of branching restrictions in the United States. Against the same backdrop, Cetorelli and Strahan (2006) find that banking market concentration acts in detriment of would-be local entrepreneurs, showing that enhanced branching competition instead leads to increased business entry and more atomic firm creation. Kerr and Nanda (2009) further show that the US branching deregulation caused both growth in entrepreneurship as well as business closures. Elsewhere, Guiso et al. (2004) show that local financial conditions in Italy are important predictors of the propensity of an individual engaging in self-employment. Benfratello et al. (2008) further suggest local financial institutions matter for process innovation, but have little impact on product innovation. Pascali (2016) shows differences in local banking conditions have long lasting effects on economic and social capital measures. The role of credit availability for small enterprises has also been highlighted in work by Banerjee et al. (2015a) in the context of India, while similar linkages between financial intermediaries and small enterprises are showcased in de Mel et al. (2008) for Sri Lanka and in Kaboski and Townsend (2012) for Thailand. Hasan et al. (2015) shows non-linear results from China, noting that while specialized, local financial institutions are instrumental in propelling economic growth and small business development in rural China, participation of large banks in local credit markets negatively impacts entrepreneurial activity.

The rest of this paper is organized as follows. Section II discusses the historical context around the time period studied, and provides a theoretical framework to motivate the empirical strategy. Section III documents the data collection and the sample selection. Section IV describes the empirical strategy and tests the validity of my identification assumption on conditional exogeneity. Section V presents the empirical results on the effect of branching expansion on retail activity and entrepreneurship. Section VI discusses limitations of the

empirical design and provides robustness checks to verify its local validity. Section VII concludes.

II Historical Context and Theoretical Framework

This section describes the details of banking regulation in the late Francoist regime and how its reform can be used to identify the effect of branching expansion on local economic outcomes. A parsimonious theoretical model is also proposed that links the lessening of financial constraints to local retail activity and entrepreneurship outcomes.

II.A Historical Background

In the aftermath of the Spanish Civil War, Francoist authorities set to rebuild the fabric of the financial system. Concerned with its stability (Sáez de Ibarra, 1954), the fascist government adopted the so-called banking *status quo*, establishing a *numerus clausus* for all banks operating in Spain and their network of regional and local branches. The post-war regulatory wave that ensued culminated in the 1946 Banking Law, which set the legislative framework for the foreseeable future and did away with any aspirations the private banking sector had of lifting some of the most punishing restrictions.[11]

The Banking Law of 1946

The 1946 Banking Law ushered in a period of rigid control of banking creation and branch expansion, reflecting a profound mistrust of private initiative and the functioning of the financial market (Malo de Molina and Martín-Aceña, 2013).[12] The interventionist approach adopted by authorities intended both the reduction of banks' monopolistic influence as well as the mobilization of banking resources to the financing of large scale public programs of accelerated industrialization (París Eguilaz, 1947).[13] Interest rates were regulated, and preferential rates were set for state-sponsored industries. Branching expansion was severely limited by stringent requirements relating to the financial development of each region. All banking operations were subject to controls: only discounting and 90-day commercial loans were authorized, with long-term credit severely regulated until the late sixties. Quantitative

[11] In a report on the proposed 1946 legislation, the National Banking Council explicitly asked for the branching expansion controls to be removed, to no avail (Pons, 1999). Problems associated with branching control were also commonplace in commercial banks' annual reports.

[12] This mistrust was ideologically motivated. The nationalization of the banking sector, which Falangists saw as a bulwark of 'international capitalism', was one of the objectives of the party's Twenty-Six Point Program from which early Francoist legislation drew inspiration.

[13] A controversial debate in the Spanish historiography discusses whether the *status quo* reflected the banking sector's submission to the state or, contrarily, the capture of the state by financial institutions who thrived by absorbing smaller entities and providing lucrative credit to protected industries. See Pueyo Sánchez (2006) for a complete discussion.

credit ceilings were imposed on all commercial banks, while ideological pressure led to strict control of dividend distribution and share acquisition.

The punitive regulatory framework would remain in place for two decades.[14] By the late 1950s, however, fiscal and monetary imbalances had built up as a result of the government's interventionist strategy and pursue of self-sufficiency, while protectionism had induced severe industrial inefficiencies and a deficient allocation of financial resources. In a bid to overcome the associated inflationary pressures and a widening gap in the balance of payments, the Francoist government revamped national economic policy and adopted the 1959 Stabilization Plan. A series of reforms followed that aimed at reducing state participation in the economy and enhance the role of the market. In line with this shift towards market liberalization, some of the restrictions imposed on financial institutions were overhauled in the 1962 Banking Law.

The Banking Law of 1962

The approval of the 1962 Banking Law officially put an end to the long period of *status quo* and paved the way for a phase of reforms in the financial sector. The preamble of the Law of 1962 is instructive of the government's rationale for reforming the financial system: inadequacy of tools to conduct monetary policy, shortage of medium and long-term credit hampering economic growth and lack of specialization dragging the sector's ability to meet all borrowing needs.

Nonetheless, the resulting changes were a far cry from a move towards liberalization, as the degree of interventionism by the state was barely reduced in the years that followed. Entry barriers persisted, asset and liability operations remained subject to controls and interest rate floors and ceilings continued to be set by authorities. In addition, new legislation introduced a so-called investment coefficient, whereby banks were compelled to maintain a proportion of their portfolio in government bonds or protected industries, a move perceived by contemporary economists as an attempt by the Francoist regime to tighten its grip on the financial sector (Poveda, 1980).

In the drafting of the law, regulators adopted the view that banks, through a lack of competition during the *status quo*, were inefficient and ill-prepared to fully undertake the necessary geographical expansion in financial services the regime considered vital to the Party's political and economic goals. Instead, a staggered program was devised that would enable a gradual expansion of bank branches across the Spanish territory while maintaining a

[14]The punishing nature of the restrictions should not be overstated. Large banks thrived on the back of deficient competition and privileged credit circuits, as noted by major contemporary bankers (Villalonga, 1961). Pons (1999) and Martín-Aceña et al. (2012) argue that these restrictions resulted from collusive behavior between large influential bankers and the regime, who depended on support from these to guarantee their political survival.

manageable pace of credit lending growth and preventing unbridled competition stemming from "the excessive proliferation of branches".[15] The program also served to meet regional political goals as well as support synchronous state-led initiatives, restricting banks' ability to expand to locations of their own choosing.

This framework set the basis for the Banking Expansion Plans, a series of annual plans drafted by the Bank of Spain which identified the growth capacity of each financial institution as well as the number and location of branches that ought to be opened. Nine Expansion Plans would be drawn up before the winds of liberalization in 1974 prompted authorities to dismantle their control apparatus. The Expansion Plans authorized the opening of a total of 2,549 new offices, which almost doubled the 2,758 branches that existed at the onset of the program and which significantly transformed the Spanish financial landscape. The increase in bank branches was generalized across all Spanish provinces, while the number of non-banked municipalities declined precipitously as authorities set out to promote financial services in locations most afflicted by the restrictions associated to the *status quo*.

The Banking Expansion Plans

The program that set out to remedy the imbalances that had built up following two decades of stagnant branch network growth required the Bank of Spain to list candidate municipalities for expansion under one of four categories:

(A) A total absence of banking services.

(B) Insufficient banking services given a location's wealth, population and activity.

(C) Insufficient banking services considering regional economic programs.

(D) Sufficient banking services but could benefit from added competition.

The Bank of Spain relied on a series of objective measures to identify candidate municipalities for all four categories listed above. These measures were made public to commercial banks in the draft of the First Plan, and their use was ratified in subsequent Plans. Objective criteria included: the most recent population censuses, the existence of informal financial services, the geographic distribution of authorized and existing banking branches, the location of regional industrial promotion programs associated to the Development Plans, the economic performance of each province, the relevance of the local tourism industry, and the capacity of banks to expand. In addition, municipality-level analyses were conducted by the regional

[15] *Decreto 1312:1963, de 5 de junio, sobre modificación del status quo bancario*, p.1., Official State Gazette, 9 June 1963.

branches of the Bank of Spain on an annual basis, while authorities also took into consideration objective suggestions put forth by commercial banks.

Preliminary plans were drafted using the set of objective measures as well as the studies submitted by the regional branches of the Bank of Spain. These studies were instrumental in the selection of final municipalities, as they included recommendations made by the regional representative of the central bank for whether considered municipalities ought to be included under one of four categories. Municipalities included this way were frequently listed in the final version of the Expansion Plan. Archival evidence shows that the criteria followed by the regional representatives responded to a similar set of measures listed by the parent office in Madrid, and relevant differences in the information provided largely reflect the disparate reality different Spanish regions lived under. Municipalities considered under category A regularly reported the distance to the closest branch and the number of inhabitants. However, some regional branches made emphasis on the high economic potential of the location, whereas others highlighted outgoing migration and the need to provide an economic boost to the region in order to buck the migratory trend.[16] Similar region-specific differences were observed between recommendations made for other categories, but these regularly placed more emphasis on the economic potential and diversity of the listed municipalities.

The preliminary plans were shared with the National Banking Council and commercial banks, who would proceed to submit suggestions and demands to central bank authorities. Although a second version of the Plan required the Bank of Spain to factor in this feedback, in practice the only relevant changes made over the preliminary version addressed the Bank's own *errata*. The plan included both the list of municipalities as well as estimates of each financial institution's expansion capacity. This was defined as the difference between available capacity and consumed capacity, which was estimated as the sum of each bank's equity and borrowing, minus the estimated value of the institution's offices and branches. In a bid to give equal opportunity of expansion to small and large banks, the Bank of Spain granted a privileged status to the former by multiplying their consumed capacity by a factor of less than one.[17]

Following approval by the Ministry of Finance of the final version of the plans, these were circulated to all financial institutions who bid for the listed municipalities. Commercial banks had a month to submit their list of preferences, and each municipality carried a cost in consumed capacity associated to the location's population size. In the first years on the program, unused expansion capacity could be partly carried over to the following year,

[16] The fact that the banking sector was seen as a boon to growth capable of assisting under-performing areas was a core reason for including category A municipalities in the Plans.

[17] This was a consequence of the Banking Law of 1962, which stated that progressive liberalization ought to be accompanied with equality of opportunities for all banking institutions.

which in combination with the fact that municipalities included in category A were generally perceived as less economically viable than their category B or D counterparts, caused many commercial banks to withhold from requesting some of the former locations. Preferential treatment was again given to local and regional banks in the bidding process, as five different 'turns' were created.[18] Upon resolving all requests and following approval of the resulting allocation by the Ministry of Finance, commercial banks had up to six months to establish their allocated local branch.[19] The number of awarded municipalities in the course of the first seven Expansion Plans is shown in Table 1.

Table 1: *The Banking Expansion Plans*

		Requested					Not Requested				
Plan	Appr. Date	A	B	C	D	Total	A	B	C	D	Total
1st	Oct. 64	128	0	0	122	250	28	1	0	5	34
2nd	Oct. 65	158	152	0	64	374	43	19	0	16	78
3rd	Jan. 67	70	140	0	61	271	14	7	0	0	21
4th	Dec. 67	76	84	0	30	190	22	7	0	2	31
5th	Oct. 68	33	64	11	101	209	7	1	0	3	11
6th	Oct. 69	35	108	9	52	204	14	10	0	3	27
7th	Jan. 71	15	96	0	31	142	8	1	0	0	9
Total		515	644	20	461	1640	136	46	0	29	211

Note: The data is collected from the Historical Archives of the Bank of Spain (AHBE) and represents the number of municipalities included in the Banking Expansion Plans classified on whether a commercial bank requested the location. Numbers in this table are slightly different to those from other academic contributions to the study of the banking sector, since I directly use the Bank of Spain's data which shows some additional municipalities were awarded after approval of the Plan by the Ministry of Finance.

In the first two Expansion Plans, the Bank of Spain set out to tackle the deficient provision of financial services in non-banked municipalities. Of the 250 commercial bank branches awarded in the First Plan, 128 corresponded to municipalities classified under category A. This number would increase to 158 out of 374 awarded branches in the Second Plan. The relative success of the first two Expansion Plans, coupled with suggestions made by commercial banks that not enough profitable municipalities were being offered to financial institutions (partly reflected in a significant portion of category A municipalities being left vacant in the second Plan), led authorities to gradually reduce the number of category A municipalities, with 70 municipalities granted in the Third Plan and 76 in the Fourth Plan. This number would continue to decrease as banking authorities perceived the spell of rapid expansion had already run its course and

[18]Local banks operating in a single province were awarded their requested locations first, followed by those operating in a banking district, and in several banking districts. Requested municipalities would then be granted to regional banks if not already granted, and national banks would fill the last round.

[19]In practice some took up to two years, but branching competition was sufficiently fierce during the Expansion Plan years that a majority of banks were expedite in opening their awarded branches.

a prudent moderation in the number of new branches was warranted.[20] Figure 1 shows the geographical evolution of allocated and non-requested branches for category A municipalities over the first seven plans.

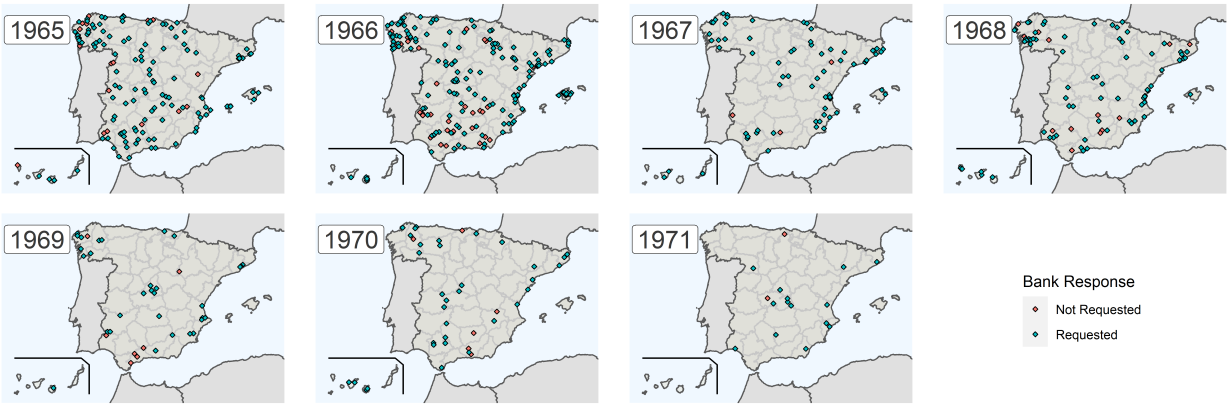

Figure 1: Geographic distribution of municipalities included in a Plan under criteria A (1965-1971)

The figure shows the timing of treatment for municipalities included in the Plans under criteria A. The municipalities are classified on whether at least one private bank requested opening a branch at the location or not.

Commercial banks were of the opinion that expansion into many of the category A municipalities was impractical and economically unfeasible. In their annual suggestions, several banks claimed that a more prominent role should instead be given to branches in locations where a thriving economy made it imperative. Additionally, many argued that the relative large number of non-requested A category municipalities was evidence of their economic intractability, and requested that the Bank of Spain refrain from including vacant locations in subsequent plans. The National Banking Council was also of the opinion that more category B municipalities should be offered instead of their category A counterparts. However, commercial banks made no attempts to push for the removal of these municipalities from the Plans, since expansion into category A locations was voluntary and ultimately motivated by political reasons. Instead, large commercial banks focused their lobbying efforts on dismantling the system of privilege that benefited local and regional banks during the expansion plans, something that was achieved in the last two iterations of the program (Cruz-Roche, 1974).

On 15 November 1971, the Ministry of Finance enforced a partial overhaul of the expansion program, arguing that "against a backdrop of rigidity which prevents banks from growing adequately, further flexibility is warranted to enable a free selection of municipalities".[21] The two Plans that followed included a new type of authorization, whereby commercial banks

[20] Following the first four Plans alone, the number of banking branches had grown by a third over the country's total.

[21] *Orden Ministerial de 15 de noviembre por la que se modifican las normas de expansión bancaria*, Official State Gazette, 18 of November 1971

were allowed to use 80% of their expansion capacity on municipalities of their own choosing, with the remaining 20% constrained to locations selected by the Bank of Spain under the same system as before. Of 172 municipalities suggested by the central bank (many under category A), only 17 were requested by commercial banks, who instead used the unrestricted portion of their capacity to open 326 new offices. In the last Plan before full liberalization in 1973, another 387 branches were awarded under the unrestricted category, while 117 were allocated following suggestions by the Bank of Spain.[22]

II.B A Theoretical framework on the role of financial intermediaries

The underlying premise that partly motivates the inclusion by central bank officials of an A category in the Banking Expansion Plans is that there are important frictions in credit markets that preclude potentially high-quality entrepreneurs with positive net value projects from starting a new business or expanding an existing one due to inadequate capital.[23] If financial constraints are an important limiting factor for growth and entrepreneurship, and if these constraints are to an extent driven by asymmetric information and cost differences in acquiring it given geographical distance, then ceteris paribus business start-ups ought to be sensitive to exogenous shocks to the wealth or borrowing capacity of potential entrepreneurs.

The seminal paper by Evans and Jovanovic (1989) presents the canonical static model that can help pin down the relationship above between a municipality inhabitants' financing capacity and entrepreneurship. Formally, consider individuals $i \in I$ in a municipality $m \in M$ who at the baseline must decide whether self-employ or work for someone else. At the endline, individual i in town m will either receive a payoff y from self-employment or a wage w from being employed by some other party. Foregoing the discussion over the wage formulation, a self-employed payoff can be modelled as:

$$y = \theta k^{\alpha} \varepsilon$$

where θ is considered to be the observed entrepreneurial ability by the individual, k is the amount of capital that was invested in the venture and ε captures random noise assumed normally distributed, i.i.d. and unobserved at baseline. Individuals with more entrepreneurial aptitude have both higher ability θ and larger returns from capital, regulated by α. The net income for a self-employed individual i is the payoff and the return on wealth during the

[22] Since my identification exploits the restrictive nature of branching growth under the Banking Expansion Plans, the last two unconstrained iterations of the program are ignored subsequent sections.

[23] The Decree 1312:1963, published by the Ministry of Finance at the onset of the program, explicitly argued that the expansion of financial services may precisely be needed the most where its precludes economic development.

period, written as:

$$\Pi = y + r(z - k)$$

where r is one plus the interest rate, z is the self-employed individual's baseline wealth and k is the invested capital. The individual, which for the purposes of simplicity is assumed as unable to default, is either a net borrower if the invested capital k is larger than their baseline wealth z, and a net lender otherwise. The amount of additional capital that individual i can borrow is then proportional to the baseline wealth z, which Evans and Jovanovic denote as $(\lambda_m - 1)z$, which sets an upper bound to the amount that can be invested equal to $z + (\lambda_m - 1)z = \lambda_m z$. The self-employed individual then faces the following borrowing constraint:

$$0 \leq k \leq \lambda z$$

where the parameter λ_m is lower bounded at 1 and is municipality specific. In the most severe scenario where $\lambda_m = 1$, individuals can only invest their own baseline wealth and outside financing is unavailable.

The investment decision k made by a risk-neutral individual at baseline can then be retrieved from the following optimization problem

$$\max_{k_i \in [0, \lambda_m z_i]} [\theta_i k_i^{\alpha} + r(z_i - k_i)]$$

where the optimal solution for k reads

$$k_i = \left(\frac{\theta_i \alpha}{r} \right)^{1/(1-\alpha)}$$

The choice of k is thus always dependent on the individual's ability, but may also depend on initial wealth z if the borrowing constraint limit that coerces $k_i \in [0, \lambda_m z_i]$ binds. Further, an individual will decide to self-employ itself if the payoff in expectation of investing the optimal amount k is larger than the wage return from being employed elsewhere, ie.

$$[\theta_i k_i^{\alpha} + r(z_i - k_i)] > w_i + r(z_i)$$

In the inequality above, the left-hand side is increasing on θ, suggesting that more able individuals are also more likely to self-employ. However, if financial constraints λ_m are severe, some individuals may be financially constrained, which will trigger sub-optimal levels of venture investment and turn unprofitable projects that would otherwise have been profitable for an unconstrained entrepreneur. As such, the central prediction of the model by Evans and Jovanovic is that the propensity to self-employment is not only a function of an individual's

innate ability to do so but also of their personal wealth and their outside financing options.

The Spanish Expansion Plans present a unique opportunity to estimate the effects a lift in financing constraints caused by the program had on measures of local entrepreneurship, as the state-led promotion of local banking services is akin to an exogenous increase on λ_m for treated municipalities over those untreated, uncorrelated with wealth or unobserved ability.[24] In this setup, identification not only requires exogeneity in the treatment,[25] but also that some individuals with sufficiently high θ_i within the treated municipalities will switch from employment to self-employed upon the increase in λ_m.

The empirical approach motivated by the Evans and Jovanovic (1989) model is most similar to other designs that overcome endogeneity stemming from reverse causality by using exogenous shocks to wealth or borrowing capacity, such as work by Holtz-Eakin et al. (1994) and Blanchflower and Oswald (1998). Nonetheless, these focus on individual or firm level data, and instead this work bears more semblance to that of Guiso et al. (2004), who examine local variation in the supply of credit across Italian regions. They find that even in a fully developed financial market such as the Italian one, regions with more comprehensive financial markets promote higher entry and increase the propensity of individuals to venture into new businesses. Finally, this analysis is agnostic about the fact that preferences for entrepreneurship may be correlated with wealth levels, so that the effects of the banking expansion on a municipality may be non-monotonic and only significant after a certain wealth threshold (Hurst and Lusardi 2004, Nanda 2011). Nonetheless, the sample of untreated and treated municipalities under category A suggests there was no sorting of wealth in either group (which may have been the case for other categories) and the evolution of outcomes can be assumed as parallel across municipalities.[26]

III Data

In order to study the effects of the Banking Expansion Plans on local retail activity and entrepreneurship outcomes, several historical sources are employed. A description of these

[24] The limitations in the data presented in this article make it impossible to discern whether the effect may have ocurred along an intensive (investment within existing business) or extensive (investment in new ventures) margin, since a single business could possess several commercial licenses if its product list was broad enough. In the model's parlance, I am not able to discern effects from an increase in λ_m that either allow for enhanced k decisions or for some individuals to switch to self-employment.

[25] This is discussed in Section IV.C, where it is shown that municipalities selected under criteria A do not correlate with baseline wealth characteristics. This is unlikely to be the case for other selection criteria, as these were explicitly associated to existing financial conditions and may as such correlate with wealth and confound the effect from λ_m with relatively faster increasing levels of z_i.

[26] Implicitly, this assumption makes it so that wealth imbalances between groups are cancelled out in the difference-in-differences specification presented in section IV. Visual evidence of parallel trends is provided in Appendix Figure A3 and Figure 3.

sources is presented in the section immediately below, while the specific variables employed in the empirical part of the paper are discussed in the next section.

Data Sources

The novel dataset used in this paper employs one of the most significant municipality-level data collection efforts conducted at the time in Spain by Tarrats (1963), an endeavour that was subsequently matched in the Yearbooks of the Spanish Bank of Credit, or *Banesto* for short. These almanacs were published on an annual basis and provide information on a wide range of municipality-level variables collected from provincial and local statistical sources. The data is fairly granular and comes in two publications, both of which I digitize for the purposes of this study. In odd years, the publication included all Spanish municipalities with more than 3,000 inhabitants, while publications in even years incorporated data for municipalities with a population of between 1,000 inhabitants and 3,000 inhabitants. Both series of publications are combined with spatial data and information on synchronous regional policies collected from Pujadas and Font (2001) to build a unique panel of Spanish municipalities during the late Franco regime.[27]

Secondly, an important contribution from this article is the ability to match the municipality-level data presented in the *Banesto* yearbooks with the Bank of Spain's own data used by officials when drafting the Banking Expansion Plans. I am able to match a majority of municipalities with more than 1,000 inhabitants, and I also obtain data for those locations with less than 1,000 inhabitants considered by the Bank of Spain as candidate municipalities.[28] This data is obtained from the Historical Archives of the Bank of Spain and is collected from over 3,000 documents related to the drafting of the first seven Banking Expansion Plans.[29] These include data for the main explanatory variables: information on municipalities that were awarded a branch, whether commercial banks requested the branch, and whether these branches were eventually opened in the selected town. Archival data also provides novel variables that are essential in the causal identification of the Plans' effects on local entrepreneurship. The most important among these is data on informal financing services in

[27] These series are completed using the original sources, where viable. This includes data on commercial licenses, which is produced by the provincial Chambers of Commerce, Industry and Navigation; population data, which is instead sourced from a combination of municipality registers and the 1950, 1960 and 1970 Population Censuses published by the Spanish Statistical institute and commercial and savings bank branches, which are collected from available data at the Bank of Spain archives. Variables for which data collection efforts proved unfeasible are instead imputed for the missing years. The more conservative approach of exclusively using data collected from the *Banesto* Annual Yearbooks reduces the longitudinal size of the panel but poses no threat to the results of the paper. This is shown in the robustness section, and stems from the fact that imputed variables have low variation and imputation is relatively straightforward.

[28] The fact that almost none of these smaller towns was eventually included in the Expansion Plans supports the contention that no spurious bias is introduced by truncating the sample of municipalities to those with more than 1,000 inhabitants.

[29] *Archivo Histórico del Banco de España*, AHBE.

each municipality, which in the empirical section is shown to play a critical role in moderating the effect of branch openings on entrepreneurship.

In addition, ample qualitative information is obtained from the documents of the Historical Archive of the Bank of Spain. Such evidence is used to test whether the category A selection guidelines were followed, as well as to which extent deviations from these occurred and the reasons for it.[30] Lobbying efforts by commercial banks attempting to influence the Bank of Spain's decisions are also documented, as well as the opinion, views and suggestions regularly put forth by many of the members involved in the drafting process. Qualitative data is also obtained from the minutes and meetings of the Bank of Spain and the National Bank Council (*Actas del Consejo de Gobierno del Banco de España* and *Actas del Consejo Superior Bancario*, respectively), as well as private correspondence between the Ministry of Finance and the Governor of the Bank of Spain on the subject of the Expansion Plans.

Sample Selection & Data Description

Identification of the causal effect of financial development on local economic outcomes requires a strategy robust to potential bias stemming from reverse causality or omitted variable bias. Since the selection of municipalities under category B, C and D was driven by economic motives, the task of disentangling a potential unidirectional effect from this data is impractical. Instead, category A municipalities were strictly chosen on the fact that they represented a large body of Spanish nationals who lacked access to formal commercial banking services, with other economic reasoning being captured by the objective guidelines of the Bank of Spain.[31] Under the credible assumption that all determinants of Plan treatment are observable and accounted for, the use of category A municipalities provides an exogenous source of variation in financial services which is able to identify effects on local outcomes.

Accordingly, the sample of municipalities is built using all towns included in the Banking Expansion Plans under category A as well as municipalities that were excluded from the Plans altogether.[32] The resulting sample includes 2,950 municipalities for which I have complete

[30] Some of the findings suggest the Bank of Spain did alter the guidelines and would temporarily include additional measures to meet short-term concerns, such as prioritizing locations with informal financing services, municipalities whose population was dispersed, and towns that had a difficult road access.

[31] As discussed in Section II, this is further shown by the studies produced by the Bank of Spain's regional branches and annually submitted to the parent institution. In these, economic information on non-banked municipalities was scant and varied between regions. Instead, the population estimate and distance to closest branch were the most frequently reported facts. Some noted their recommendation was driven by the fact that there was important outgoing migration and scarce local economic activity (arguments used by the Ministry of Finance to motivate the program), whereas others made their recommendation on the fact that economic potential was significant. No distinctions seem to have been made by the Bank of Spain when aggregating these sources and drafting a list, suggesting the final choice was not driven by economic outcomes. Section IV.C provides empirical evidence of this.

[32] Alternative samples used in Section V.A restrict the sample of control observations to municipalities also

outcome, explanatory and control variable data. This sample represents over a third of total Spanish municipalities and over 80% of Spanish municipalities with at least 1,000 inhabitants. Summary statistics for these municipalities as well as those towns included in categories other than *A* are presented in Table 2.

Table 2: *Descriptive Statistics*

Variable	Full Sample		Cat. A		Cat. B		Cat. D		Untreat	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Surface (km2)	112	12.0	135	40.5	200	15.7	158	14.2	93	14.2
Population	8,010	875.0	5,574	237.6	22,428	1415.3	43,649	10881.6	3,120	57.9
Telephones, 1000 inhab.	35.4	1.0	43.5	4.1	60.0	3.9	85.8	4.0	24.9	0.6
Trucks, 1000 inhab.	5.5	0.1	6.0	0.2	6.7	0.3	9.8	0.4	4.7	0.1
Cultural Tax Revenue, 1000 inhab.	4,602	123.6	5,625	436.1	7,909	596.9	11,663	628.5	3,216	92.6
Municipality Budget, 1000 inhab.	342.8	4.5	340.9	14.5	435.8	11.4	533.2	18.0	312.3	4.8
Commercial Licenses, 1000 inhab.	15.1	0.2	15.4	0.4	19.0	0.7	23.9	0.7	13.7	0.2
Tourism Index	26.0	5.7	17.7	5.8	38.0	9.2	247.8	70.3	1.6	0.4
Altitude	448	5.3	352	12.6	282	17.8	379	19.7	496	6.3
Distance to the sea	69.9	1.2	60.4	2.9	50.9	4.2	68.2	4.3	74.4	1.4
Initial distance to a bank	8.1	0.2	10.3	0.4	0.4	0.1	0.7	0.4	9.2	0.2
Distance to Develop. Pole	187.5	3.5	198.7	10.3	229.1	16.8	183.4	12.2	181.1	3.8
Number of municipalities	3,455		585		236		269		2,365	

Note: The table shows the means and standard errors of a subset of covariates and outcomes for five different subsamples of data. Category A data corresponds to municipalities included in the Plans under criteria A, which applied to municipalities with absolute absence of banking services. Criteria B and D include municipalities which had banking presence but either was insufficient given its wealth (B) or could benefit from additional competition (D). Untreated municipalities are those not included in the plans for which the author has complete data.

The outcome variable of interest is defined as the number of commercial licenses for every 1,000 inhabitants in a municipality, which provides the best available measure of entrepreneurship in the retail and wholesale trade industries. These licenses were levied items required by establishments to engage in sales of different types of products, making them one of the few proxies collected annually.[33] The downside of this measure is that business owners could possess multiple commercial licenses at once, making it impractical to break down the effect in extensive and intensive margins. Nonetheless, contemporary economists estimated that the average Spanish establishment used around 1.2 commercial licenses per establishment (Tarrats, 1965), indicating the measure is also a relatively good proxy for the number of commercial establishments.[34] Other outcome measures employed below mainly relate to proxies of local economic activity, namely the number of trucks and telephone lines per 1,000 inhabitants. These variables are primarily collected using data from the *Banesto* volumes.

Demographic controls include log-population and population growth, both measures estimated

lacking banking services. The results are shown not to be driven by the inclusion among control observations of municipalities with a commercial bank branch in 1963.

[33] A breakdown by type of product license is analyzed in Section V.B.

[34] In spite of this, my interpretation of the results does not assume this. Instead, it is underpinned by the theoretical predictions in Section II, whereby both new entry of entrepreneurs and optimized investment levels from incumbent businesses are possible.

from Census data and municipal records. Economic controls sourced from the Yearbooks include a municipality's budget, local tax revenue from cultural shows as well as a measure of tourist intensity, which is computed as an index that weights the number of hotel rooms and camping sites over the total in the country. In addition, geographical proximity to the Industrial Development Poles, the main regional policy that may generate spurious relations with the Banking Expansion Plans, is included using data of Pujadas and Font (2001). From the Historical Archives of the Bank of Spain I build indicator variables for whether a municipality is included in a Plan, whether this municipality is requested by a commercial bank, the capital size of each solicitor, whether a branch opening actually occurs in a municipality or whether informal financing services by means of banking correspondents are provided locally. Data for savings bank branches distributed across the Spanish mainland is also obtained from archival data. Spatial controls include a municipality's altitude, latitude, longitude as well as distance to the sea, with information collected from the National Geographic Institute.

IV Empirical Design

The research strategy looks at changes in local economic activity in the awarded municipalities relative to the non-awarded locations before and after the implementation of the seven Expansion Plans put forth by the Bank of Spain between 1964 and 1971.[35] The sample is restricted to include non-banked municipalities awarded under category A, which alleviates reverse causality bias and allows for the identification of a potential credit lending channel orthogonal to that of increased competition. In what follows, a first approach uses a staggered difference-in-differences design, which may however mask heterogeneous and dynamic effects stemming from the changing nature of the Plans, dynamic patterns of behavior by borrowers or the gradual resolution of information-constrained moral hazard problems by banks (Aghion and Bolton, 1997). As such, a second design explores the evolution of the outcomes of interest using leads and lags for the main treatment indicator.[36]

IV.A Difference-in-Differences Design

In order to determine whether the program had a positive effect on municipalities that lacked financial services, the following regression for municipality i in province p during year t is employed:

$$Y_{i,t} = \delta CB_{i,t} + \gamma_t X_{i,t} + \phi Z_{i,t} + \theta_{p,t} + \tau_i + \varepsilon_{i,t} \tag{1}$$

[35] As noted in Section II, the last two annual Plans gave private banks more leeway in the selection of municipalities, and are thus a poor fit to this empirical specification.

[36] Econometric theory has in recent years questioned the validity of such event study designs (Goodman-Bacon, 2018), as differential timing and heterogeneous effects can bias estimates and difficult inference. Modern literature has not yet settled on a standard alternative approach; instead I implement all prime candidate solutions to account for these sources of bias.

where $Y_{i,t}$ denotes an outcome variable (generally the number of commercial licenses per capita as a proxy measure for entrepreneurship) and $CB_{i,t}$ represents an indicator variable for whether a municipality is in the treatment group, ie. becomes a recipient of commercial banking services. The assignment to treatment status depends on the variable of interest, either the opening of a branch within the municipality or its inclusion in one of the Bank Expansion Plans. A basic issue with the former variable is that bank entry is endogenous to profit-making expectations, with commercial banks potentially selecting municipalities on unobservable characteristics. Instead, the assignment to one of seven annual Banking Expansion Plans can be thought of as random once the variables used to draft the list of selected municipalities are accounted for. Section IV.C further develops this argument.

Specifically, defining $Award_{i,t}^{BEP}$ as an indicator for a municipality being included in one of the Plans, I instrument $CB_{i,t}$ in Equation (1) with $Award_{i,t}^{BEP}$ to obtain estimates of local treatment effects on the treated (LTOT). In addition, reduced-form regressions directly employ the instrument $Award_{i,t}^{BEP}$ as the main variable of interest, which provides an estimate of the intent-to-treat (ITT) effect. The discrepancy between these effects depends on both the persistence of treatment status as well as take-up rates by private banks on the selected municipalities, which attenuates the ITT effects relative to the LTOT effects.

Equation (1) also includes a set of municipality-specific covariates assigned to $X_{i,t}$ or $Z_{i,t}$. These variables are *ex ante* defined by both the draft guidelines made available to banks by the Bank of Spain at the time as well as additional variables used internally by central bank officials. Variables in $X_{i,t}$ are interacted with linear time trends, either because their use by the Bank was limited to a specific period (their effect on treatment is temporary) or because they are time-invariant but their effects on both outcomes and selection to treatment may vary over time (Zeldow and Hatfield, 2021). Variables that are time-varying and whose effects are arguably time-invariant are instead included in $Z_{i,t}$, with no time interaction.[37]

In addition, equation (1) includes province-by-year ($\theta_{p,t}$) and municipality τ_i fixed effects. In baseline specifications, province-by-year fixed effects are included as two different sets of fixed effects, but narrative evidence as well as the selection criteria used in the drafts warrant the interaction of the two in the saturated specification of the model.[38] The interacted fixed effects control for any systematic trend differences between provinces over time, which are likely to be correlated with both the economic outcomes and the assignment to treatment

[37] In the most demanding scenario where all variables are included in $X_{i,t}$ so that the full set of coefficients are year-specific, the main results remain significant, albeit with point estimates roughly halved from a baseline scenario with no covariates due to the most variation being captured by the full broadside of time-trended controls.

[38] In particular, time-trended provincial fixed effects captures the fact that central bank officials took the provincial economic outlook of each province into consideration when drafting the Plans.

group. The municipality fixed effects instead control for unobservable and time-invariant variation in outcomes across years within a municipality.

IV.B Event Study DiD Design

A major limitation of the specification above is that a single treatment variable foregoes the debate over dynamic treatment effects and ignores the possibility for differential treatment duration between groups in a sufficiently short panel such as mine. This motivates the estimation of the effects of branching expansion using the following alternative reduced-form expression:

$$Y_{i,t} = Award_i^{BEP} \sum_{\substack{k=-5 \\ k\neq -1}}^{8} \delta_k \mathbb{1}\left[t - t_i^g = k\right] + \gamma_t X_{i,t} + \phi Z_{i,t} + \theta_{p,t} + \tau_i + \varepsilon_{i,t} \tag{2}$$

where indicator variables $\mathbb{1}\left[t - t_i^g = k\right]$ measure the time relative to treatment for each group of treated municipalities g, and $Award_i^{BEP}$ captures whether a municipality is in any such treatment group. The treatment year for each group is considered to be six months following the approval by the central bank of the municipalities requested by private banks. This is meant to match the six months banks had to open a branch in the awarded municipality upon approval of the Plan results by the Ministry of Finance.[39] The omitted relative year is $k = -1$, the period prior to treatment. Therefore, each other estimate δ captures the change in outcomes in treatment municipalities relative to non-treatment ones as measured from the year immediately prior to treatment. The parameters of interest in (2) are δ_0 to δ_8, which show the short and medium-run effects of exposure to state-led banking branch expansion between relative years 0 and 8.

Conditional on the set of covariates described in Section III, the variation in exposure to the Plans ought to come from two sources. The first are differences in exposure duration that result from a municipality's specific year of treatment, and the second is variation in the timing of these Plans and whether a municipality was treated or not. The assumptions that underpin the ability to appropriately identify causal estimates are similar to those traditionally presented in difference-in-differences studies. First, the inclusion of a municipality in a given Plan is uncorrelated with prior trends in the outcomes across observations, so that absent treatment, all groups would have followed similar trajectories. Second, the timing of the Plans does not coincide with other policy shocks that affect the outcomes of treated units disproportionately.

The validity of the first assumption can be tentatively assessed by using the δ_{-5} to δ_{-2}

[39] Documents shared between central bank officials suggest that anticipatory behavior by private banks was not unusual; in practice, some municipalities may have been effectively treated in $t - 1$.

coefficients to test whether selection into the Plans was associated with differential trends prior to treatment.[40] If outcomes in the sample of municipalities were trending similarly prior to the Expansion Plans, one would expect the estimated coefficient to be statistically insignificant and centered around zero. Nonetheless, pre-treatment similarities, while widely used as a test in the event study literature, are neither necessary nor sufficient to guarantee parallel counterfactual trends (Kahn-Lang and Lang, 2020). In the robustness section, additional tests for non-linear deviations of counterfactual trends are presented, as well as checks on whether the model has enough statistical power to actually detect differential pre-trends. Further robustness checks are provided in the form of propensity score matching models, which are included to coerce pre-treatment balance in relevant covariates and alleviate concerns regarding control sample selection.

The second assumption is more concerning for the purposes of this study. The late Franco regime was characterized for ambitious regional policies that assisted a spell of strong economic growth in the country following two decades of failed attempts at self-reliance. If any of these policies correlate with the timing of the Banking Expansion Plans and asymmetrically target municipalities included in the program, treatment effects may capture these and wrongfully attribute them to the Plans. Permutation tests in the robustness section support the contention that any non-controlled-for policies correlated with the timing of the Plans and treated municipalities are not biasing the treatment estimates. In addition, among the covariates are included proxies for those policies specifically accounted for by central bank officials, namely the so-called Development Plans and the existence of local tourist hubs. The former targeted a handful of large cities which are not in the main sample of analysis, and instead a distance measure to any of these cities is included in $X_{i,t}$ to formally control for possible spillover effects. To account for growth in the tourism sector, time-varying data on hotel rooms and camping sites is included as a synthetic index in the set of covariates.[41]

IV.C Instrument Validity

The empirical design outlined above relies on variation in the timing and location of banking branch expansions that stems from the drafting of the Banking Expansion Plans, and not in variation caused by the banks' own rent-seeking motives. In turn, the program's main objective needs not be to maximize local economic outcomes or award branches to municipalities conditional on the latter's economic performance, but instead abide to the observable selection criteria that the Bank of Spain listed in the preface of the first Expansion Plan and that was internally ratified as late as in the drafting of the fourth Plan.

[40] Appendix Figure A3 also provides some visual evidence of the parallel evolution in the treatment groups.

[41] The assumption is that these variables fulfill the backdoor criteria, by which they dispel noise associated to both treatment and outcome without adding any additional spurious relations.

In Section II it was noted that the first two Plans mainly focused on category A municipalities, which were usually perceived by commercial banks as less desirable over other listed municipalities. In an answer to requests made by the National Banking Council to prioritize more economically developed but insufficiently banked municipalities (category B) in the drafting of the first Plan, the Bank of Spain flatly refused, arguing that "selecting municipalities that already have access to financial services over those that lack it would tarnish the raison d'être that inspires the drafting of this Plan".[42] Similarly, the National Banking Council requested that only municipalities that would ensure a short-term profit for commercial banks ought to be included, to which central bank officials responded by arguing that it was precisely the existence of banking services what would enable some laggard regions to catch up with the more economically developed ones.[43]

Other archival evidence for the Plans further supports the claim that, conditional on observable controls, selection of a municipality in category A did not correlate with *ex-ante* economic expectations. In comments made to the Bank of Spain during the drafting of the Third Plan, the Banco de Málaga argued that the branches it had opened following the First Plan under category A were still unable to turn a profit, and was requesting such municipalities not be included in subsequent Plans.[44] On a similar note, the Banco de Vizcaya pointed out that many A category municipalities that had not been requested by commercial banks in the first two Plans were again included in the Third one, which in the opinion of the bank highlighted the locations' lack of economic viability and the need for these to be discarded in any final version of the Plan.[45] The Banco Español de Credito further pointed that many of these A criteria municipalities were not requested by commercial banks due to optimizing behavior in hopes of bidding for more profitable municipalities in a subsequent Plan.[46] In discussions ahead of the Fifth Plan, the same bank criticized the selection criteria central bank officials followed for category C and D municipalities, arguing that unlike those that guided the A and B criteria, the former were perceived to be subjective and were inconsistent across years and between regions.[47]

In order to further test the validity of the relevance and exogeneity assumptions that buttress the use of municipalities included in the Banking Expansion Plans as the main explanatory

[42] *Carta del Gobernador del Banco de España al Ministro de Hacienda*, 7 October 1964, AHBE.

[43] This opinion was formally echoed by the Ministry of Finance, suggesting the regime's policymakers saw the Expansion Plans, at least initially, as an opportunity to broaden access to banking services across the Spanish mainland irrespective of economic characteristics.

[44] *Carta al Gobernador del Banco de España*, 31 October 1967, AHBE.

[45] *Sugerencias del Banco de Vizcaya al Gobernador del Banco de España*, 8 October 1967, AHBE.

[46] *Resumen de las Sugerencias de los Bancos Nacionales*, 18 May 1966, AHBE. Note that commercial banks were allowed to carry over 'expansion capacity' to the next Plan if left unused, which triggered this behavior.

[47] *Observaciones del Banco Español de Crédito al proyecto del Quinto Plan*, 30 October 1968, AHBE.

variable of interest, Table 3 presents saturated regressions of baseline economic characteristics on the dummy for treatment and actual branch openings. Additionally, Appendix Figure A2 shows a coefficient plot for the regression of the treatment variable on the controls that central bank officials used to guide their decision process, which highlights the relative relevance of the different controls in the first stage.

Table 3: *First stage instrument relevance and selection into treatment*

Dependent Variable:	Commercial Bank (1)	1963 Comm. Lic., pc. (2)	1963 Telephones, pc. (3)	1963 Trucks, pc. (4)	1963 Budget, pc. (5)
Panel A: Treatment					
Awarded in Plan	0.77***	-0.25	2.75	0.10	9.57
	(0.02)	(0.32)	(2.35)	(0.17)	(10.05)
Panel A: Branch opening					
Commercial Bank		3.36***	9.56***	1.09***	57.32***
		(0.32)	(1.64)	(0.19)	(13.18)
Controls	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	No	No	No	No
Year x Province FE	Yes	Yes	Yes	Yes	Yes
Mean dependent variable	0.3	13.7	26.9	4.7	300.9
Number of observations	35,400	35,400	33,996	34,369	35,400
Number of municipalities	2,950	2,833	2,846	2,950	2,950

Note: Panel A shows the instrument's relevance for commercial bank entry as well as its ability to predict baseline observations of proxies for local economic activity. Panel B shows, absent an instrument, the non-random selection of municipalities that receive a bank branch expansion by repeating the same regressions on baseline measures of economic activity. The coefficients are obtained from the regression $Y_{i,t} = \delta T_{i,t} + \gamma_t X_{i,t} + t\theta_p + \tau_i + \varepsilon_{i,p,t}$ for municipality i in province p during year t. In Panel A, T represents the treatment indicator, whereas in Panel B it represents an indicator variable for whether a bank branch operates in the location. Time-invariant, but with plausibly time-varying effects on both treatment and outcome, are time trended and included in the set of covariates X.
***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests.

The first column of Panel A shows the relevance results of the instrument, by which the opening of a commercial bank branch on a given municipality is regressed on the treatment variable. The strong and positive relationship is consistent with the limited scope commercial banks had in opening branches beyond the ones dictated by official institutions, but also indicates that banks enforced their ability to either not request a treated municipality or not open a branch even when assigned the municipality, which pushes the coefficient away from unity.

The remaining four columns of table 3 use 1963 observable economic characteristics as dependent variables in a bid to test whether treatment groups were sorted by baseline municipality outcomes. The results in Panel A are revealing, with the Plan treatment not being associated to any significant differences in initial economic measures, conditional on the above-defined set of controls. Telephones and trucks, which the Spanish historiography uses to proxy for services and industrial activity (Tarrats, 1965), respectively, are not correlated with treatment. The same goes for our main variable of interest, commercial licenses, which captures retail activity and entrepreneurship. Treatment assignment is also uncorrelated with a municipality's 1963 budget. Conversely, the use of actual bank branch openings as the main explanatory variable yields positive and statistically significant coefficients when regressed against baseline economic outcomes. These results provide reassuring evidence that

the instrumental variable does not correlate with economic outcomes prior to the Plans and is a valid instrument for the endogenous variable measuring actual branch openings.

V Results

This section presents the results of an increase in the provision of financial services on proxy measures of local entrepreneurship and economic activity. As with other attempts at identifying such effects, the counterfactual is not financial autarchy such that $\lambda_m = 1$: individual and trade credit, savings banks and commercial bank correspondents all provided some access to financial services. Instead, this paper focuses on measuring the effects of gaining access to additional working capital and liquidity provision services, which at the time was provided to individuals and small businesses almost exclusively by commercial banks.[48] The first section studies difference-in-differences estimates using reduced-form and instrumental variable approaches, whereas the second part focuses on the reduced-form event-study case to shed light on the dynamics of the main empirical results.

V.A Difference-in-Differences Analysis

Baseline Estimates

Table 4 reports results from equation (1), where the number of commercial licenses per 1,000 inhabitants is regressed on different explanatory variables and on increasingly saturated models.

Column 1 of Panel A shows that in a model with no covariates or time-trended provincial fixed effects, including a municipality in a Banking Expansion Plan under criteria A is associated to an increase in per capita commercial licenses of around 5.5% over baseline values, suggesting the state-induced increase in credit provision services had a positive and significant impact on local entrepreneurship. Column 2 includes demographic controls, namely log population and changes in population, which capture agglomeration effects and increase the reduced-form coefficient on per capita licenses to just above unity.[49] The inclusion in columns 3 and 4 of spatial and economic controls, respectively, removes much of this effect, but per capita commercial licenses still increase by half a unit upon treatment. The most demanding specification is shown in column 5, which includes linear trends for all Spanish provinces and shows the effects remain positive and robust. A further break down of some of the main covariates is provided in Panel A of Appendix Table A1. In line with the contention

[48] As mentioned before, commercial banks were not burdened with the regulatory constraints savings banks faced, which imposed compulsory investment coefficients of up to 80% of deposits (Comín, 2007) and were oft used by the Francoist state to channel domestic savings into large development plans or the investment programs of the Ministry of Labor.

[49] All results are robust to either a binary or continuous measurement of population growth.

Table 4: *The effect of bank branch deregulation on local commercial activity*

Dependent Variable:	Commercial Licenses, per 1000 inhab.				
	(1)	(2)	(3)	(4)	(5)
Panel A: Intent to Treat					
Awarded in Plan	0.86***	1.01***	0.82***	0.53***	0.64***
	(0.18)	(0.17)	(0.16)	(0.15)	(0.15)
Panel B: Treatment on Treated					
Commercial Bank	0.87***	1.11***	0.93***	0.69***	0.72***
	(0.18)	(0.17)	(0.16)	(0.15)	(0.15)
Panel C: Local Treatment on Treated					
Commercial Bank	1.07***	1.29***	1.05***	0.68***	0.83***
	(0.22)	(0.21)	(0.21)	(0.20)	(0.19)
Demographic Controls	No	Yes	Yes	Yes	Yes
Spatial Controls	No	No	Yes	Yes	Yes
Economic Controls	No	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year x Province FE	No	No	No	No	Yes
Mean dependent variable	15.5	15.5	15.5	15.5	15.5
Number of observations	35,400	35,400	35,400	35,400	35,400
Number of municipalities	2,950	2,950	2,950	2,950	2,950

Note: The table shows the intent-to-treat (ITT), treatment-on-the-treated (TOT) and instrumented local treatment-on-the-treated (LTOT) effects of the Banking Expansion Plans on the number of commercial licenses per capita, used by the author as a proxy measure for entrepreneurship and commercial engagement. The table displays the difference-in-differences estimates for the post-expansion period, and columns reflect different specifications of the main DiD regression. See text for more details on the covariate groups.
***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests. Robust standard errors are clustered at the municipality level.

that savings and commercial banks were hardly perfect substitutes of each other, the inclusion of an indicator variable for the presence of a local savings bank branch does little to erode the identified effect on commercial bank branch expansions.

Panel B reports estimates from the OLS regression of actual branch openings on commercial licenses per capita. This coefficient is consistently above that of the reduced-form estimates, which substantiates the claim that the variable may be endogenous to potential outcomes due to commercial banks' ability to withhold from opening a branch to the extent that the context of coerced expansion allowed them to. The coefficients follow a similar pattern to those in Panel A as controls are added, suggesting uncontrolled agglomeration effects originally weight negatively on the estimate and that spatial and economic controls capture otherwise spurious

relations with the main variable of interest. It is important to remark that the endogeneity concern is not as pronounced as in similar studies in the finance-growth literature, as the candidate category *A* municipalities commercial banks could bid for during the Expansion Plans were not often regarded as highly profitable.

The instrumental variable estimations are reported in Panel C of Table 4. Absent the necessary controls that define the non-randomness of the choices made during the Expansion Plans by policymakers, instrumented estimates of the presence of a local branch are upwardly biased, as shown in column 1. The results with the necessary controls remain nonetheless above those of the endogenous variable, which defies the assumption that the instrument is able to clear out some of the endogeneity present in the choice of branching expansions.

There are several possible explanations as to why IV estimates would be higher than their OLS counterparts. Differences between actual branch openings and selection in a Plan are not only limited to those selected municipalities banks did not find profitable to expand to during the Plans, but also branch expansions that may have taken place beyond the scope of the first seven Plans. The latter can either represent the handful of branches that were authorized to open prior to the reform of the *status quo* which led to the Expansion Plans, or branches that were opened after the partial overhaul of the program in 1972 and that gave commercial banks ample leeway in the selection of which municipalities to expand to. If the characteristics of these branches are fundamentally different to those under the Plans, the variation that the instrument uses to fit the endogenous variable may by construction be of a different nature from that the OLS estimate uses.

Archival and empirical evidence suggest that this was the case in Spain during the period of interest. A major point of contention during the years of *status quo* was that the Spanish banking sector had drifted towards an oligopoly, with national banks incorporating lesser entities into their ranks and increasingly capturing a larger share of the market. This trend was significantly, albeit only temporarily, halted as a result of the Expansion Plans, which as discussed in Section II favored local and regional banks over their national counterparts by giving the former priority in the selection of shortlisted municipalities and awarding these at a lower cost in terms of their 'expansion capacity'. These privileges were gradually eroded as a result of intense lobbying by national banks, and by the time the overhauled Eighth Plan was drafted, local and regional institutions had lost much of their edge.[50] If such differences correlate with the type or quality of service they were able to provide in category *A* municipalities, then the IV estimates will be biased towards the effects associated with the banks whose participation was more acute during the seven Plans, namely local banks.

[50]This is reflected by their share of awarded municipalities declining from an average of 45% over the total in the first seven Plans to less than 35% in the last two Plans (Cruz-Roche, 1974).

Panel B in Table A1 shows for a fully saturated model the effects actual branch openings had on commercial licenses moderated by the size of the bank who bid for the municipality. Branches that were opened by local banks witnessed a stronger increase in the number of commercial licenses per 1,000 inhabitants, while in locations where a regional or national bank opened a branch, the effect was noticeably smaller. Commercial licenses in municipalities that experienced a branch opening not included in the Plan were on average left unchanged.

The interaction with local banks arguably captures not only differences in potential service quality, but also the fact that these municipalities were selected before national banks could bid for them and may have been ex-ante more likely to see more entrepreneurial behavior. Nonetheless, the fact that most of these municipalities were uniformly perceived as not particularly profitable by major banks, and that local banks were for the most part constrained to their own province and could as such not engage in a nation-wide strategic selection of municipalities suggests the latter effect may not play a particularly important role. This is reinforced by the fact that regional banks, whose turn came before that of national banks, have the smallest impact on local retail outcomes. Instead, local banks appear to have been more successful at lifting financing barriers in these non-banked municipalities given their accumulated knowledge about local market conditions, which in turn reduce costs of producing information about potential borrowers (Petersen and Rajan, 1995).[51] The results here seem to tentatively validate other empirical papers that suggest local agents are best poised to overcome costs of screening and monitoring when providing local capital to entrepreneurs (Guiso et al., 2004), while the fact that their relevance diminished after the Seventh Plan indicates the IV approach estimates a local treatment effect from these small-sized banks that differs from the broader OLS estimate on branch opening.[52]

The insignificant coefficient from branch openings not included in the first seven Plans points to a second reason why OLS estimates may be biased downwards. In what is a traditional weakness of difference-in-differences designs that study policies with staggered timing and dynamic effects (Goodman-Bacon, 2018), observations that are treated towards the end of the sample lack sufficient periods for dynamic effects to fully materialize. In the setting under

[51] Archival evidence indicates that this was precisely the main argument put forth by local bankers when defending their privileges in debates at the National Banking Council. Other contemporaneous sources further supports this: in a survey made in 1959 among entrepreneurs and executives of non-financial firms about their relations with banks (Linz and de Miguel Rodríguez, 1963), a majority of respondents pointed out that there existed at least some differences between banks, mostly stemming from divergences in product characteristics. Against this backdrop, these interviewees most frequently stated that they preferred to work with regional or local banks rather than larger institutions since it was easier to establish bonds of friendship with the former's branch officials.

[52] This does not mean local banks were necessarily better for local development. An alternative interpretation of the results is that the differential effect merely captures differences in the time it takes banks to build a portfolio of borrowers.

study here, this is consistent with the relationship lending literature (Berger and Udell, 1995) by which commercial banks need time to clear asymmetric information hurdles and set up effective monitoring systems. If the effects of lifting financial constraints are only gradual and muted at first, treatment at the end of the sample may be misleadingly insignificant. Branch openings that occurred in 1972 and 1973 in a sample that ends in 1974 are thus likely to cause bias in the average treatment on the treated OLS estimate. Conversely, an IV regression that exploits variation at the middle of the sample is less likely to suffer from such downward bias. Taken together, the differences between instrument and OLS regressions seem to be warranted by the structure and underlying differences in who opens a bank branch and when it opens it.

Alternative Estimates and Samples

A possible caveat of the empirical design outlined above is that the treatment coefficients on commercial licenses may not capture the direct lending link between entrepreneurship and access to financial services but rather an indirect effect on local economic conditions which then lifts individual wealth and the propensity of becoming an entrepreneur. Table 5 shows results from regressing the main variables of interest on different measures of economic activity to study if such phenomenon is present in the data. These results also serve to shed some light on alternative mechanisms that may assist a municipality's observed economic performance, namely the expansion of savings bank branches and its effect on local outcomes prompted by changes in an individual's ability to manage consumption.

The first three columns of Panel A in Table 5 again shows the effects of banking branch expansion on commercial licenses. The indicator variable measuring the presence of a savings bank branch in a municipality is positive and significant at the 10% level, suggesting that services provided by savings banks may have marginally benefited individuals and allowed them to save enough to invest in their own ventures. Additionally, the coefficient may also capture the small percentage of deposits savings banks were able to put to use for direct lending. Taken together, however, these effects are relatively muted when compared with the impact a commercial bank branch had on local entrepreneurship.

If commercial bank branches affected entrepreneurship through local growth and not through a decrease in financing barriers, the main explanatory variables ought to positively and significantly correlate with proxies of economic activity. Using the number of telephone lines per 1,000 inhabitants in a municipality as a proxy for local economic activity (Tarrats, 1963), Panel B in Table 5 shows results for such a regression. Estimates are positive but insignificant, suggesting the effect commercial banks had on the measure of entrepreneurship was direct and not a second-order effect. Conversely, the savings mechanism is shown to be at full swing here, with an opening of a savings bank branch being associated with a strong pickup in telephone

lines per capita. The latter result is in itself an interesting finding and aligns with work in the development literature on micro-finance which shows savings and small-credit facilities can help households substitute immediate consumption for durable goods (Banerjee et al., 2015b). Finally, Panel C employs the number of trucks per capita as the main dependent variable, which proxies for local industrial activity. The results in this last panel again verify that the lending effects identified previously are not due to general economic dynamics but the direct effect of lower financing barriers on local entrepreneurship.[53]

Columns 4-6 in Table 5 shift the discussion to the sample selection. Section IV.C extensively discussed the use of category *A* municipalities as the main treatment group due to their selection being as if random once controlling for the criteria used by policymakers. So far, control observations have included all municipalities not listed in the Plans, irrespective of whether had access to formal banking services. Columns 4-6 instead focus on regressions that use as control municipalities those locations that lacked a commercial bank branch by 1963. This has the advantage of comparing municipality groups that had a similar lack of access to financial services in 1963, before the Banking Expansion Plans were enacted. The results are similar to those obtained from regressions using the full set of control municipalities, although magnitudes change somewhat. The effect of treatment on commercial licenses is milder, while the point estimates on economic variables are larger and on the verge of 10% significance.

Nonetheless, the use of this alternative sample of municipalities is subject to common support concerns in the data. The fact that the Bank of Spain targeted the largest non-banked municipalities makes it so that the never-treated, non-banked locations are disproportionately small sized. Structural, unobserved differences between large and small municipalities, such as migration to larger towns that may correlate with entrepreneurs, may make this conservative sample of control observations an inconvenient one. The choice of the full set of control observations instead requires that those municipalities that were untreated in the Plans but that had banking services by 1963 do not present different trends over non-banked municipalities. In other words, if the trend followed by the control observations with a bank is different from that followed by towns that had no access to a bank branch by 1963 as a result of the associated slope change being permanent, two-way fixed effect difference-in-differences estimates will be biased.

The historical context provides some support against this claim, as most such control observations received their branches over a decade prior to the Plans, and the event study results in Section IV.B suggest the change in entrepreneurship trends elicited by the treatment is

[53]Industrial activities were structurally less reactive to local financing conditions, as banks were used during the Franco regime as an instrument to promote forced industrialization (Pons, 1999). As such, access to financing for many industrial activities may not have been constrained locally.

Table 5: *The effect of bank branch deregulation on local economic outcomes*

	Criteria: A			Criteria: A (alt)		
	ITT	ToT	Local ToT	ITT	ToT	Local ToT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Commercial Licenses						
Awarded in Plan	0.64***			0.52***		
	(0.15)			(0.15)		
Commercial Bank		0.72***			0.64***	
		(0.15)			(0.15)	
Commercial Bank (IV)			0.83***			0.64***
			(0.19)			(0.19)
Savings Bank	0.16*	0.15*	0.15*	0.17*	0.17*	0.17*
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Mean dependent variable	15.5	15.5	15.5	14.4	14.4	14.4
Number of municipalities	2,950	2,950	2,950	2,465	2,465	2,465
Panel B: Telephones						
Awarded in Plan	2.49			3.47*		
	(1.88)			(1.97)		
Commercial Bank		2.62			4.28	
		(1.97)			(2.72)	
Commercial Bank (IV)			3.18			4.24
			(2.43)			(2.85)
Savings Bank	3.81***	3.80***	3.80***	5.73***	5.74***	5.73***
	(1.25)	(1.25)	(1.25)	(1.16)	(1.16)	(1.16)
Mean dependent variable	51.8	51.8	51.8	46.1	46.1	46.1
Number of municipalities	2,873	2,873	2,873	2,398	2,398	2,398
Panel C: Trucks						
Awarded in Plan	0.30			0.45*		
	(0.22)			(0.24)		
Commercial Bank		0.47**			0.54**	
		(0.22)			(0.24)	
Commercial Bank (IV)			0.39			0.55
			(0.28)			(0.36)
Savings Bank	-0.01	-0.01	-0.01	0.11	0.12	0.12
	(0.14)	(0.14)	(0.14)	(0.15)	(0.15)	(0.15)
Mean dependent variable	11.4	11.4	11.4	10.7	10.7	10.7
Number of municipalities	2,950	2,950	2,950	2,465	2,465	2,465
Controls & Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows the intent-to-treat (ITT), treatment-on-the-treated (TOT) and instrumented local treatment-on-the-treated (LTOT) effects of the Banking Expansion Plans on different economic outcomes and for different sample specifications. Criteria A represents unbanked municipalities treated in the Expansion Plans and all untreated municipalities. Criteria A (alt) only includes as controls municipalities that had no local commercial bank branch in 1963. Explanatory variables are either the treatment or an indicator variable for whether a local commercial bank branch exists in a given municipality. Panel A shows the effect of banking expansion on per capita commercial licenses. Panel B and C perform similar analyses on either the number of local telephone lines or the number of registered industrial trucks within a municipality.

***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests. Robust standard errors are clustered at the municipality level.

not permanent, and largely stabilizes five years into treatment. Assuming similar dynamics dictate the evolution for these control observations, treatment a decade prior to the Plans should not invalidate the use of these as controls.[54]

Overall, the results from the two-way fixed effect difference-in-differences estimates suggest the Plans were effective tools at providing access to financial resources and encouraging entrepreneurship. The effect was particularly positive for municipalities that received a local bank branch, which indicates that short-term capital was best provided, at least within the limited scope of this study, by smaller and local institutions that had a lower cost of acquiring information and monitoring their newfound borrowers. This is well in line with survey evidence that pointed at the time business owners oft preferred dealing with local bank branch officials. Moreover, the credit lending mechanism is shown to have a first order effect on financing conditions and entrepreneurship, and not an indirect impact through general economic effects. Synchronous expansions in savings bank branches point at a savings channel being instrumental for local development and shifting individuals' consumption patterns towards durable goods, but having no major effect on entrepreneurship.

V.B Event Study Analysis

The results above, while compelling, are limited by the use of two-way fixed effect estimates, which the econometric literature has in recent years stressed may yield biased estimates in the context of staggered treatment timing and heterogeneous effects across treatment groups.[55] In particular, Goodman-Bacon (2018) shows that in a setting of multiple treatment groups across different periods, the δ estimate in equation (1) is a variance and group-weighted average of all potential two-group, two-period difference-in-differences estimates. These possible combinations include those that compare a treatment group with the never-treated observations, but more concerning for identification are the comparisons between groups that are both treated and where the only difference is the timing of treatment. If the treatment effects are time-varying, these comparisons will generate bias. This section addresses the dynamics of the relation between branching expansion and entrepreneurship, and tests on group selection are addressed in the robustness section.

Figure 2 describes the chronological and geographic timing of treatments across provinces. The coefficient estimated from equation (1) is an aggregate of its constituent components:

[54]Because two-way fixed effect designs use changes in treated outcomes over changes in untreated outcomes, differences on levels pose no threat to identification, but differences in trends do. This relates to the discussion on using 'clean controls' in a context of staggered treatments. For a published paper that discusses this, see Cengiz et al. (2019). For visual evidence of similar trends prior to and early on during the Plans, see Appendix Figure A3.

[55]Some of the more prominent papers studying this include Callaway and SantAnna (2020), Sun and Abraham (2020), de Chaisemartin and DḦaultfoeuille (2020) and Borusyak et al. (2021).

comparisons between treated and untreated municipalities, comparisons between earlier treated and later treated municipalities and comparisons between later treated and earlier treated municipalities. The plot in Figure 2 reports the Goodman-Bacon (2018) diagnostic that decomposes this aggregate estimate into the sum of its components.[56]

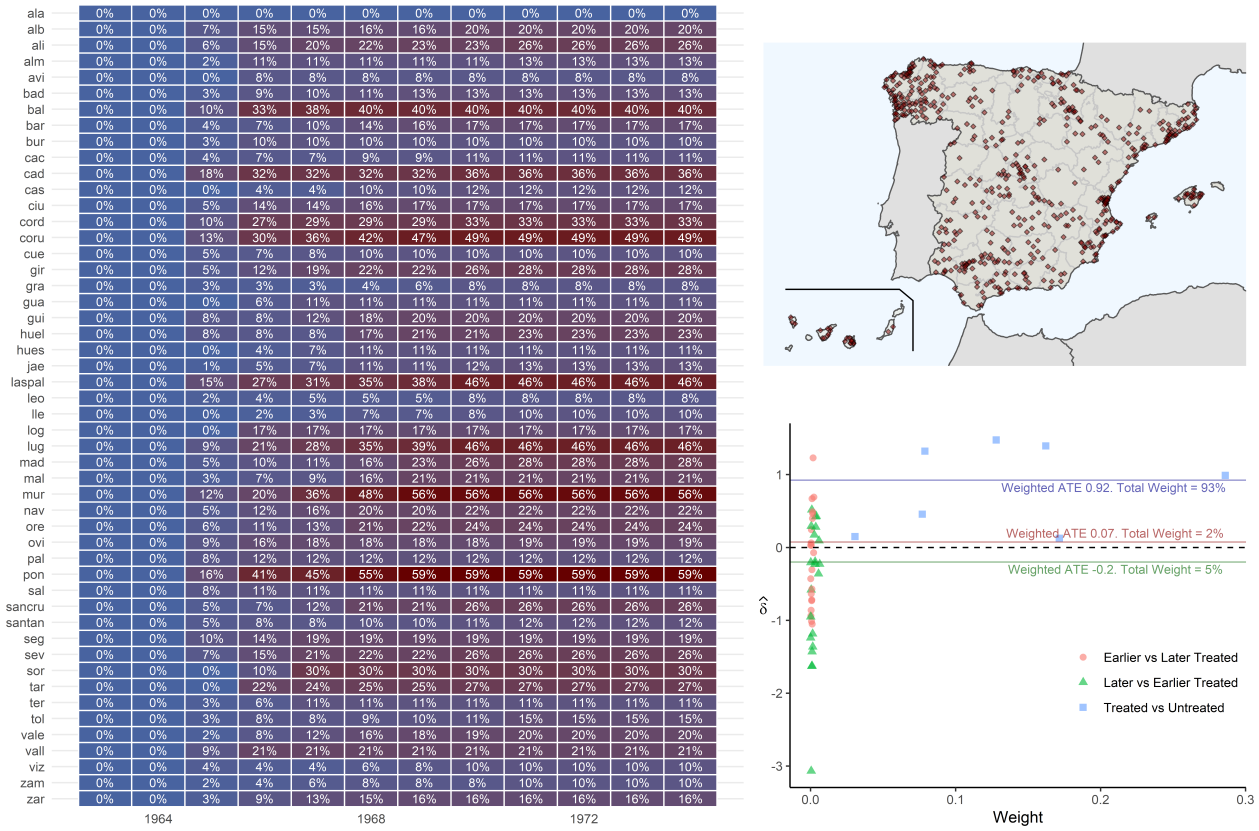

Figure 2: Goodman-Bacon Decomposition Diagnostic

The figure displays the different timings in treatment by province as well as the decomposition of the overall Two-Way Fixed Effects Difference-in-Difference estimate into its constituents. The figure on the bottom right shows estimates and weights for the different groups that conform a traditional TWFE DiD estimate, as in Goodman-Bacon (2018). The figure on the upper right shows all municipalities included in the plan under criteria A. The figure on the left shows the percentage of available municipalities included in any Plan over the years.

The results are reassuring, as over 90% of the weighted estimate uses the comparison of treated with never-treated observations, and the overall effect is not driven by any single individual two-group difference-in-differences comparison. Conversely, echoing the literature above that warns against using treatment observations as controls in staggered and heterogeneous designs, comparing later treated observations, where the effect from branch expansion may be slow to materialize, with earlier treated municipalities, where the effect may be well underway, leads to a bias that is severe enough to yield a negative estimated effect from a bank branch opening on commercial licenses.

[56]The decomposition is performed using the `bacondecomp` package in R, and the full diagnosis presented in Figure 2 is only available without covariates.

Baseline Estimates

Figure 3 presents the main results of estimating the effects of state-led baking branch expansion on commercial licenses using equation (2). The first plot illustrates visually the linear probability of a banking branch opening in a municipality following the inclusion of the location in one of the Banking Expansion Plans. Insignificant pre-treatment estimates are suggestive of a lack of anticipatory or unauthorized branch openings, while the response in bank branch openings is immediate after treatment and with a probability similar to that measured using the two-way fixed effects design.[57]

The second plot displays event study estimates from equation (2) on the number of commercial licenses per 1,000 inhabitants after controlling for municipality and year fixed effects. Prior to the selection of a municipality in a Plan, commercial licenses trended similarly across treatment and control groups. This argues in favor of the causal interpretation put forth in this paper, as the insignificant lag estimates indicate no confounding differential trends and that treatment groups were not assigned by economic outcomes prior to the program. Upon the listing of a municipality in one of the Banking Expansion Plans, commercial licenses tick up, with the effect five years into treatment reaching unity for a 6.4% percentage point increase over a counterfactual scenario of limited credit provision capacity. This is similar to findings in Guiso et al. (2004), where moving from the least to most financially developed region in Italy is associated to a 5.6% increase in an individual's ability to start their own business.[58] Additionally, and in line with the relationship lending literature (Berlin and Mester 2015, Berger and Udell 1995), this effect is only gradual as hurdles relating to monitoring borrowers and solving asymmetric information problems are worked out over time.[59]

[57]Note the immediate effect is partly driven by the way the treatment timing is defined, which reflects the deadline for a bank to open a branch in an awarded location as discussed in Section IV.B.

[58]The result for Spain may overstate this likelihood, as available data does not allow to differentiate individuals that start a business from those that expand it.

[59]Delayed full effects also support claims in Section IV.A that the OLS estimates may be biased downwards due to non-program end-of-sample bank branch openings.

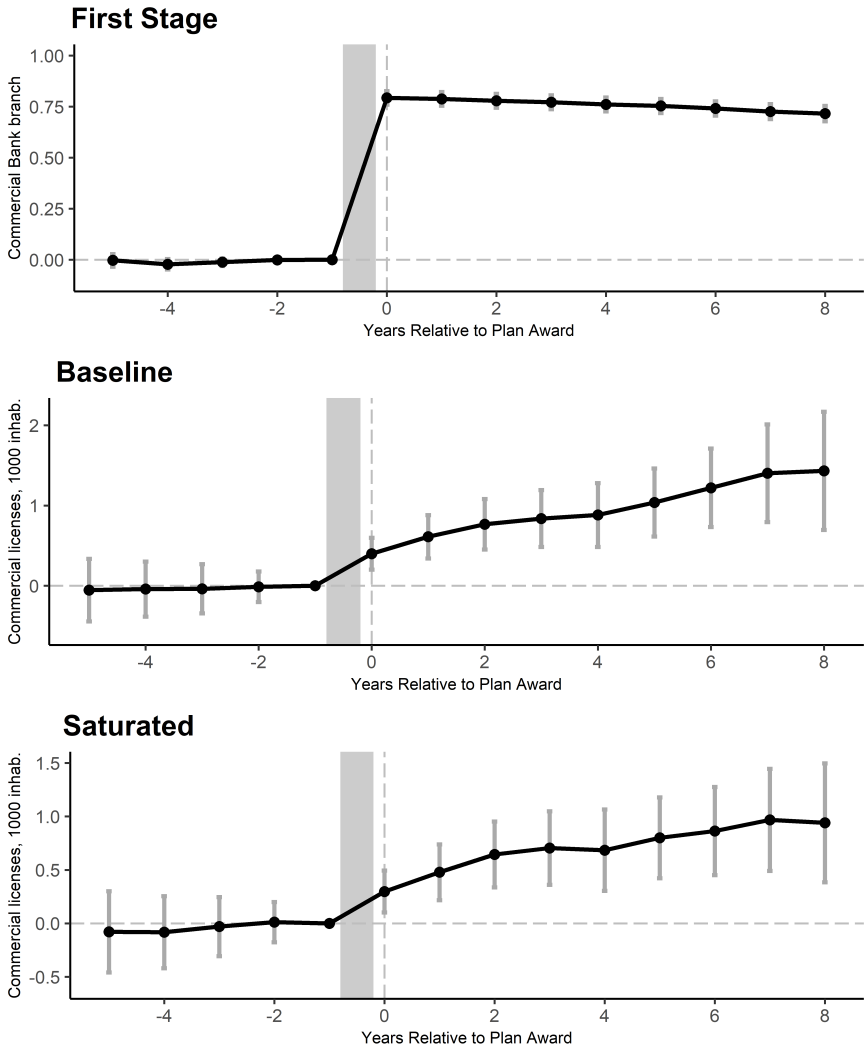

Figure 3: The effect of banking branch deregulation on non-banked municipalities

The figures report coefficients from different estimations of equation (2). The first figure displays the first stage results on the effect of a municipality being included in a Plan on the local presence of a bank branch. The second figure reports the effects of a municipality being included in the plan on total commercial licenses, absent controls and with municipality and time-trended provincial fixed effects. The last figure saturates the model with potential confounders that fulfill the backdoor criteria. Point estimates are in relative time to their group-specific treatment period, and bars depict the 95 percent confidence intervals calculated from standard errors clustered at the municipality level.

The third plot in Figure 3 further provides evidence of the hypothesized causal effect on a fully saturated model, which includes the full broadside of control covariates and time-trended province fixed effects. The results are largely unchanged from the baseline specification: no differential behavior prior to treatment but a clear uptick in entrepreneurial activity following treatment. The inclusion of controls does however reduce the point estimate of the lead variables, and the effect appears to stabilize half a decade after treatment. These results are also presented in Appendix Table A3.

Heterogeneity

The results above reinforce the claim that gaining a commercial bank branch had positive yet staggered effects in the number of commercial licenses in a given municipality. If taken for a proxy measure of local entrepreneurship, this is empirical evidence the Plans were effective at promoting economic activity and growth.

Nonetheless, a potential caveat to these findings relates to banking correspondents, an informal method used by commercial banks to expand their network of services across the Spanish territory while circumventing *status quo* restrictions on branching expansion. These correspondents were legal intermediaries of regional and national banks that often acted as local branches themselves. Even though the Ministry of Finance and the Bank of Spain came to regulate this unlawful competition following orders by the former in 1965,[60] many exceptions were made on non-requested municipalities, while many correspondents eluded restrictions and continued operations under different names or brands.[61] The fact that these quasi-branches remained in business until the liberalization in 1974 suggests some level of heterogeneity in the counterfactual estimates of branching expansion. The contention is that awarded municipalities that had correspondents operating locally should have seen less of an effect from gaining access to new financial services as long as these services were to an extent successfully provided by correspondents.

The first two columns of Table 6 report results for event study regressions on municipalities classified on whether there was at least one local correspondent providing some form of financial service. As mentioned in Section III, the set of correspondents operating in local municipalities is sourced from internal information policymakers used in the drafting of the Plans. This list combines information provided by regional branches of the Bank of Spain, commercial banks' own reporting and data obtained by the Bank of Spain's supervision unit. Point estimates suggest that to a large extent correspondents were effective in their role as make-do providers of financial services, with the effect from assignment to a Plan still being positive but somewhat muted and barely significant. Conversely, the effect is much more noticeable for branch openings in municipalities that had no local correspondent and thus limited access to other sources of financing.[62] Taken together, these results unmask hidden heterogeneity in the effects of the Banking Expansion Plans and suggest correspondents were instrumental in providing individuals with financing services in the context of *status quo* and

[60] *Orden del 5 de mayo de 1965 sobre actuación de oficinas y corresponsales de Bancos y Banqueros*, Official State Bulletin, 12 May 1965.

[61] Evidence of this is that in the years that followed the Order the Bank of Spain routinely requested commercial banks to submit lists of the correspondents that still operated for them as well as directing the institution's supervision services to surveil suspected correspondents (Moreno Fernández, 2008).

[62] Since I do not observe closures of correspondents, using this time-invariant variable as moderator is unfeasible under a difference-in-differences design with municipality fixed effects.

limited legal branching expansion.

Table 6: *Heterogeneous effects of banking branch deregulation*

Dependent Variable:	Commercial Licenses, per 1000 inhab.			
	Correspondents		Distance to Sea	
	Presence	Absence	Coastal	Inland
Year -5	0.10	-0.24	0.07	-0.20
	(0.34)	(0.23)	(0.28)	(0.27)
Year -4	0.05	-0.22	-0.04	-0.14
	(0.28)	(0.22)	(0.26)	(0.23)
Year -3	-0.16	0.05	-0.44*	0.31*
	(0.21)	(0.17)	(0.25)	(0.16)
Year -2	0.11	-0.07	-0.19	0.17
	(0.15)	(0.13)	(0.15)	(0.12)
Year -1 (Omitted)				
Year 0	0.21	0.39***	0.37**	0.26**
	(0.16)	(0.13)	(0.17)	(0.11)
Year 1	0.29	0.69***	0.56**	0.44***
	(0.22)	(0.16)	(0.22)	(0.16)
Year 2	0.51*	0.83***	0.84***	0.53***
	(0.26)	(0.19)	(0.25)	(0.19)
Year 3	0.57**	0.92***	1.02***	0.53**
	(0.28)	(0.22)	(0.30)	(0.21)
Year 4	0.54*	0.93***	1.06***	0.46**
	(0.31)	(0.25)	(0.33)	(0.23)
Year 5	0.55*	1.15***	1.08***	0.67***
	(0.30)	(0.26)	(0.31)	(0.24)
Year 6	0.50*	1.33***	1.11***	0.76***
	(0.30)	(0.29)	(0.35)	(0.26)
Year 7	0.54	1.51***	1.30***	0.82***
	(0.35)	(0.33)	(0.41)	(0.29)
Year 8	0.49	1.50***	1.33***	0.73**
	(0.42)	(0.37)	(0.49)	(0.33)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes
Mean dependent variable	15.30	15.33	15.40	15.23
Number of observations	31,812	31,968	31,596	32,184
Number of municipalities	2,651	2,664	2,633	2,682

Note: The table shows the author's estimation of equation (2) using different samples of municipalities. The first two columns show heterogeneous effects based on the presence of banking alternatives prior to a municipality being included in a Plan. These alternatives, labelled correspondents, represent agents that acted as intermediary for neighboring branches. The last two columns show heterogeneous effects based on distance to the sea, with a cutoff of 25 kilometers. Saturated controls and fixed effects are used in all regressions.

***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests. Robust standard errors are clustered at the municipality level.

An alternative objection is that the results reported above may coincide with a Spanish economic upswing disproportionately linked to tourism and coastal areas in the country. If enough coastal towns are included in the treatment groups, the effects of this economic confounder may not be fully captured by the relevant controls or the time-trended provincial fixed effects, muddling the treatment estimates. Columns 3 and 4 in Table 6 report estimates from equation (2) on the main sample with treatment observations split on whether municipalities are coastal or upcountry. An arbitrary cut of 25 km is used to classify both groups, but results are robust to other cut-offs. Encouragingly, the effect is not solely concentrated on the potentially better-performing coastal towns, as results for inland locations remain positive and robust, albeit roughly halved.[63]

Lastly, outcomes may be driven by the dynamics of a subset of product categories and not by a generalized increase in commercial licenses. Employing the nine different product categories that make up overall commercial licenses, regressions on each of these provides some descriptive evidence as to which type of activities were the most affected following the Banking Expansion Plans, or whether the effect was generalized. Figure 4 reports these results for the first eight categories.[64] Foodstuff commercial licenses, which accounted for over half the number of total licenses, are largely unaffected by the increase in local financing services, which is in line with grocery stores being more inelastic than other services to improved financing conditions for entrepreneurs due to them fulfilling primary basic needs. Instead, construction and woodworking-related activities seem to be the most affected by the Banking Expansion Plans, followed by clothing and textiles licenses.[65]

[63]These results may also be reflective of a concentration of entrepreneurial individuals in coastal towns due to migration in search of better opportunities. The data available makes it unfeasible to disentangle these effects from each other.

[64]The ninth is a catch-all category of difficult interpretation.

[65]The former may partly capture the synchronous economic upswing in the industrial and construction sector that resulted from the years of Spanish 'Developmentalism'.

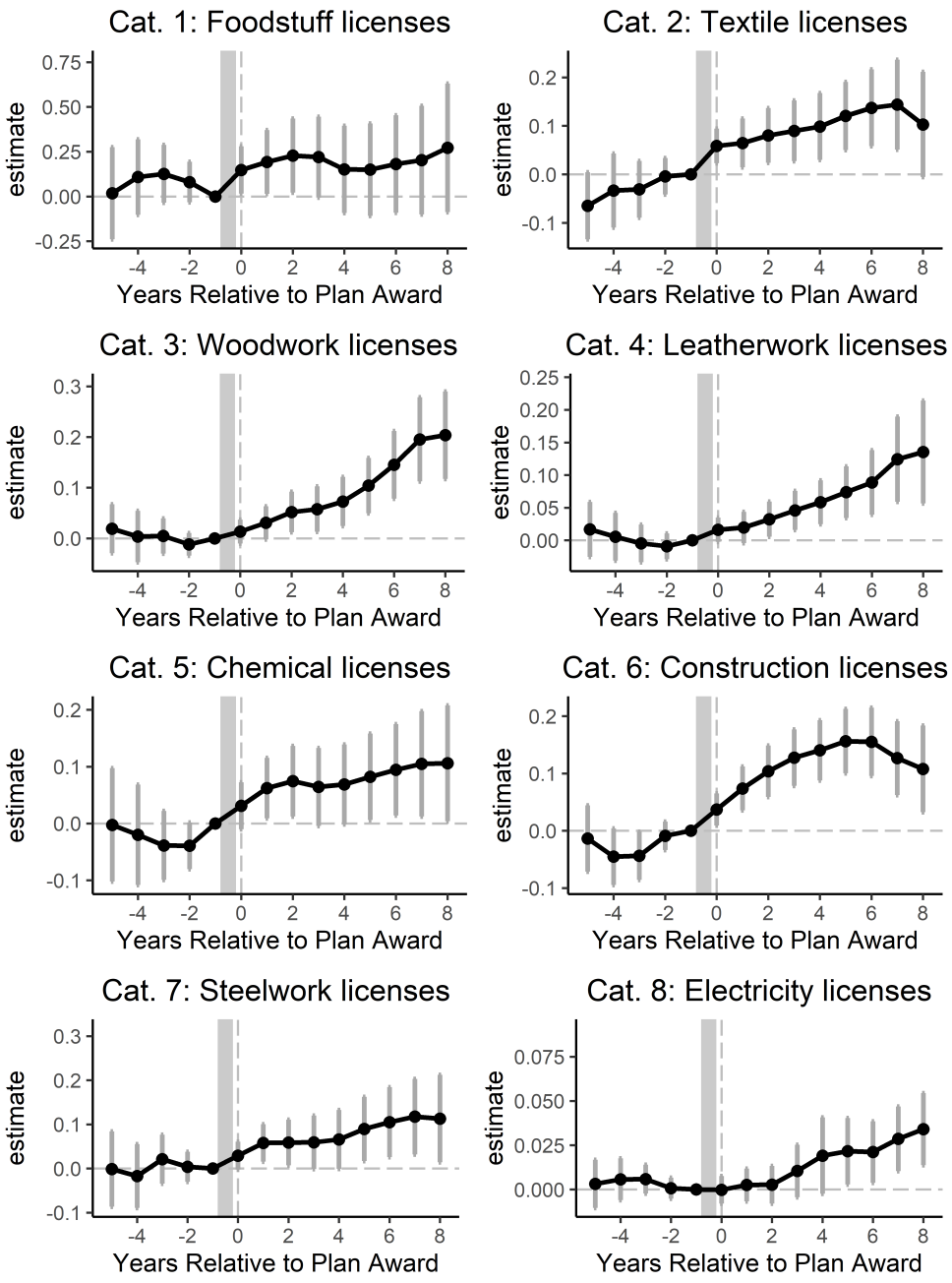

Figure 4: The effects of banking branch deregulation on different types of commercial licenses

The figures report event study coefficients for different categories of commercial licenses. Point estimates are in relative time to their group-specific treatment period, adjusted for municipality and year fixed effects. Bars depict the 95 percent confidence intervals calculated from standard errors clustered at the municipality level.

VI Robustness

The Goodman-Bacon (2018) decomposition suggests that the main effects identified are muddled by earlier treated municipalities being used as control observations to estimate the effect of other treatments at a later date. Since the econometric literature has not yet settled on which alternative estimator best circumvents this problem, below I present results using

different proposed techniques.

Alternative Estimators: Callaway & Sant'Anna

Callaway and SantAnna (2020) generalize earlier work from Abadie (2005) on the subject of assumptions over parallel trends only holding after conditioning for covariates. Against a backdrop of heterogeneous treatment effects and staggered timing in the policy, Goodman-Bacon (2018) shows there are numerous causal parameters of interest, equivalent to as many group pairs there are. Callaway and SantAnna (2020) denote these as group-time average treatment effects, and propose a two-step estimation strategy with a bootstrap procedure to conduct asymptotic inference that adjusts for autocorrelation and clustering. In a first stage, group-specific propensity score weighting is used on baseline values to synthetically build balanced groups of treatment and control observations, and to do so the algorithm ignores already-treated observations as potential controls for later-treated units.[66]

Figure 5 reports aggregated group-time estimates for all seven Banking Expansion Plans on the data sample used in the previous sections. The first plot employs population and population growth as the only control variables used in estimating propensity scores for each treated group and all untreated municipalities, which archival evidence shows were the variables most frequently used and included when drafting the plans as well as the main reason put forward by authorities to justify focus on category A locations. The plot results reinforce the idea that the parallel trends assumption holds and that the Plans effectively increased the number of per capita commercial licenses in those municipalities that were included in the drafts.

The second plot in figure 5 instead uses the full set of control variables to create synthetic group-specific balanced samples to estimate each group-time average treatment effect. The larger bandwidths reflect concerns over common support as an increasing number of variables are used to match treatment propensity scores for a finite subset of control observations. Nonetheless, the results still show insignificant effects prior to treat and a noticeable increase in commercial licenses following treatment. Columns 1 and 2 in Appendix Table A4 displays the same results.[67]

The group-time average treatment effects can also be aggregated at the group level only, which sheds some light on intrinsic differences between the efficacy of each Banking Expansion Plan

[66] The equation for the group-time treatment estimator writes: $ATT(g,t) = E\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{\hat{p}(X)C}{1-\hat{p}(X)}}{E\left[\frac{\hat{p}(X)C}{1-\hat{p}(X)}\right]}\right)(Y_t - Y_{g-1})\right]$, where G is a binary variable equal to 1 when a unit is first treated in priod g, C is a binary variable for never-treated units, X are the set of covariates used to create balanced groups and Y is the outcome of interest.

[67] This estimator does not require the omission of year $t-1$ to avoid multicollinearity since the baseline 1963 values used to match samples are instead discarded.

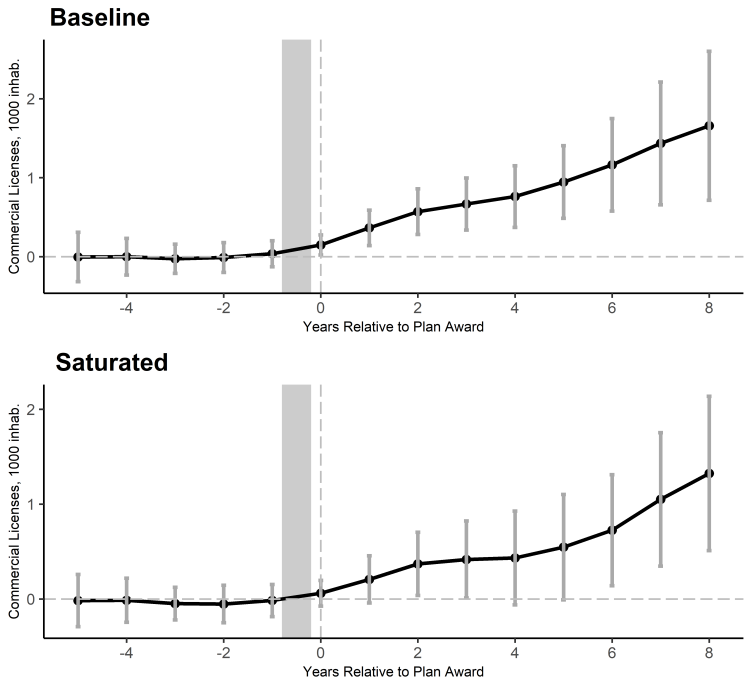

Figure 5: Callaway-Sant'Anna estimates for the effect of banking branch deregulation

The figure reports the resulting coefficients from using (Callaway and SantAnna, 2020), a method robust to staggered treatment designs and heterogeneous effects which generalizes (Abadie, 2005). This approach requires the assumption of parallel trends to hold only after conditioning on observables and builds treatment estimates for each treatment group in the sample by weighting observations on a treatment propensity score. Group-time treatment effect estimates are computed using 'never-treated' municipalities, and a bootstrap procedure is used to conduct asymptotically valid inference adjusting for autocorrelation and clustering.

and further calls to skepticism when dealing with unique difference-in-differences estimates for the effect of any policy. These treatment effects are plotted in Figure 6. Reassuringly, parallel trends hold for municipalities included in late Plans, signalling spatial and other spillover dynamics from earlier branch openings did not have an effect in those municipalities treated later on. Similarly, upon treatment the effect is positive for municipalities in all Plans, suggesting the effect was not driven by a subset of Plans, which in turn may contest claims of treatment exogeneity.

Alternative Estimators: Sun & Abraham and Stacked Regressions

An alternative estimator is presented in Sun and Abraham (2020), where the authors derive similar results on treatment estimate bias to Goodman-Bacon (2018) in the context of event study designs. Their estimator computes treatment effects specific to each group and then uses each group's sample share to build a weighted average of the group-specific estimates. Importantly, never-treated units are the only valid control observations in this estimator. The results are provided in column 3 of Appendix Table A4, and while largely in line with other event study results, the Sun and Abraham (2020) estimator produces much smaller point estimates, with commercial licenses per 1,000 inhabitants only increasing as much as 5% over the counterfactual scenario seven years into treatment.

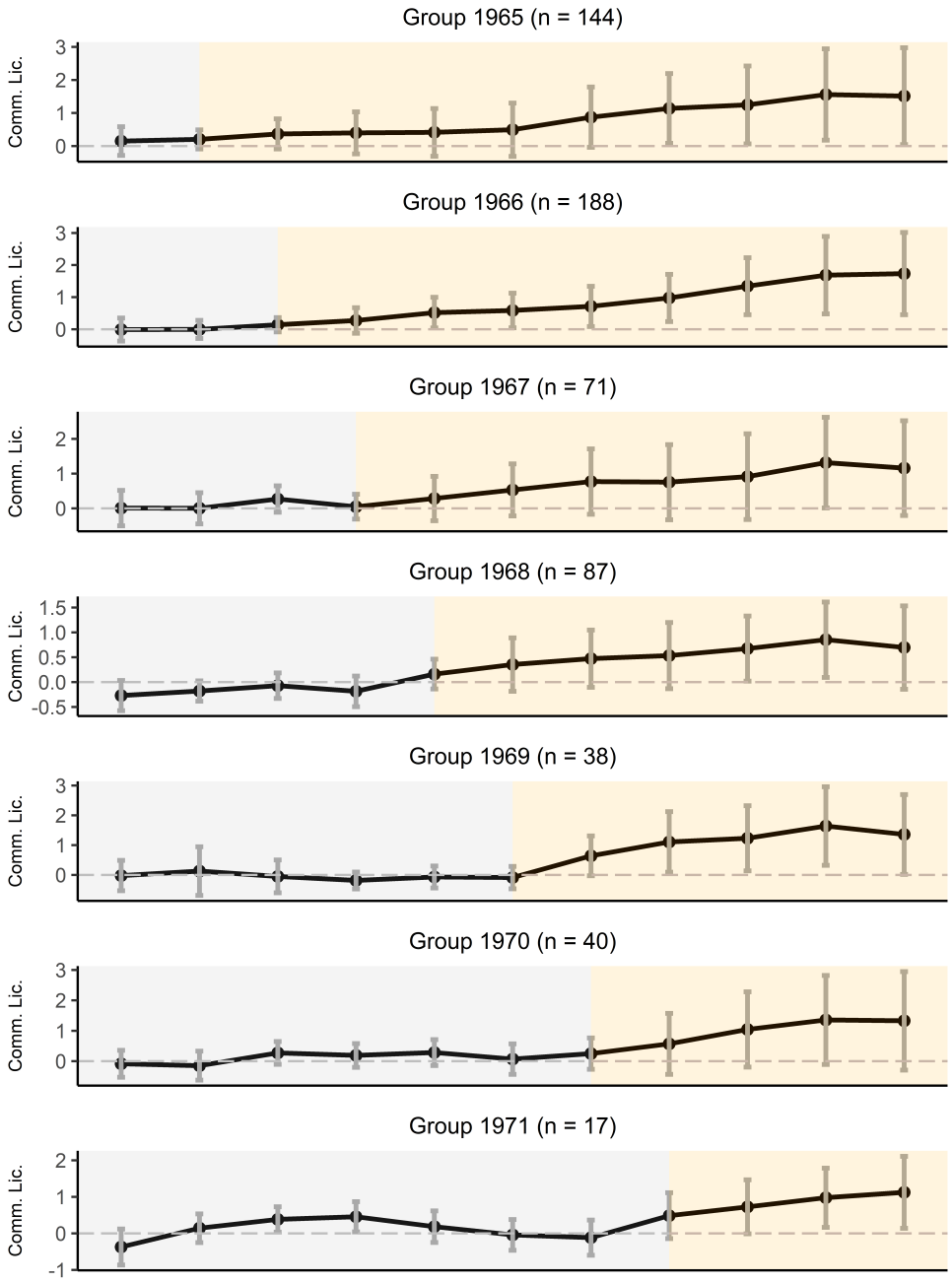

Figure 6: The effects of each Plan on commercial licenses per 1000 inhabitants

The figures report event study coefficients for each Plan released by the Bank of Spain. Point estimates are in relative time to their group-specific treatment period, adjusted for the full set of covariates. Bars depict the 95 percent confidence intervals calculated from standard errors clustered at the municipality level.

A third approach that also ratifies the event study results uses Plan-specific datasets that bundle together municipalities included in the Plan and control units that are considered to be non-confounding in the context of staggered treatment timing. These datasets, or stacks, are used in a single model similar to equation (2) but with municipality and province-time

fixed effects saturated for each of the data subsets.[68] Column 4 of Appendix Table A4 reports results for a stacked regression that for each stack utilizes as controls untreated municipalities as well as municipalities that are only treated at least three years into the future, and confirms results are not driven by pernicious earlier-to-later treated comparisons. [69]

Propensity Score Matching

Difference-in-differences designs are robust to treatment selection by time-invariant criteria, but identification may be threatened if the probability of a municipality being listed varies over time. Although previous sections have at length shown that time-varying criteria was objective and observable for category A municipalities, controlling for the observable variables that constitute the criteria may be insufficient to purge the treatment from selection bias. Instead, a weaker assumption requires that treatment status is independent of outcome conditional on balancing for these variables between control and treatment observations (Rosenbaum and Rubin, 1983). In order to recover build balanced samples, I implement a propensity score matching algorithm that uses the observable criteria by the Bank of Spain to create two samples of observations with similar predicted probability of being treated given covariates but with actual treatment differing between the two groups. The sample results from this balancing are presented in Table 7.

Balancing for covariates allows to compare control municipalities that have similar number of inhabitants, savings bank branches and commercial licenses to their treatment counterparts, although the Bank of Spain's focus on some observable characteristics when selecting A category municipalities seems to have been exhaustive enough to raise concerns on the common support in the data. This is best exemplified by the fact that the tourism index is much larger on average for treated municipalities even when using propensity score matching to create a more balanced group. That withstanding, column 1 in Appendix Table A5 reports results using the matched sample of control observations which suggest that the empirical strategy followed throughout this paper is valid and controls are sufficient to capture selection based on time-varying observables. Column 2 in Appendix Table A5 uses matching on $t-1$ observable covariates for each treatment group employing the same stacking strategy as in the previous section. This demanding specification does away with some of the significance in the previous results, but positive and significant estimates are still obtained from year five onward.

[68] This is equivalent to adding subscript j for all variables in the equation, where j is one of seven stacks of observations.

[69] This cut-off is arbitrary, but most estimates show two thirds of overall effects materialize following this period of time and is thus a convenient cut-off.

Table 7: *Category means and mean independence tests, pre and post-matching*

Variable	Baseline sample			Matched sample		
	Cat. A	Untreated	P-value	Cat. A	Untreated	P-value
Altitude	351.8	496.3	0.00	351.8	425.2	0.00
Latitude	40.0	40.2	0.31	40.0	39.8	0.27
Longitude	-4.1	-3.6	0.00	-4.1	-4.5	0.16
Distance to sea	60.4	74.4	0.00	60.4	65.8	0.18
Surface (km2)	134.8	92.8	0.33	134.8	110.1	0.54
Population	5,574.1	3,120.0	0.00	5,574.1	5,326.2	0.35
Number Savings Banks	0.8	0.4	0.00	0.8	0.8	0.41
Commercial Licenses, pc	15.4	13.7	0.00	15.4	15.0	0.46
Tourism Index	17.7	1.6	0.01	17.7	3.5	0.02
Trucks, pc	6.0	4.7	0.00	6.0	5.4	0.03
Telephones, pc	43.5	24.9	0.00	43.5	29.8	0.00
Cultural Tax Revenue, pc	5,625.1	3,216.3	0.00	5,625.1	3,802.3	0.00
Municipality budget, pc	340.9	312.3	0.06	340.9	287.5	0.00
Number of municipalities	585	2365		585	585	

Note: The set of covariates presented in this table includes the ones used by the Bank of Spain to determine municipalities that would be included in a Banking Expansion Plan under criteria A, and are the same variables used in the propensity score matching procedure. Variables used for matching include population, altitude, latitude and longitude, the tourist index, the presence of a savings bank branch, and the number of per capita commercial licenses. In addition to the group mean, the table reports p-values from unpaired samples t-test for each control variable.

Permutation tests on the timing of treatment

The different timing of the Banking Expansion Plans makes it improbable that unobserved shocks are confounding the results and driving the change in municipal commercial licenses. In order to test for a possible violation of this assumption, I perform permutation tests whereby the number of assigned municipalities is randomly shuffled across observations in a way that the treatment timing distribution is preserved. Simulating 1,000 scenarios, the results of this random assignment of treatment across time to all municipalities are visually presented in Figure 7.

Figure 7 supports the contention that any unobserved shocks correlated with the timing of the Banking Expansion Plans and the selected municipalities do not bias the estimates of the main event study regression. Table A6 presents p-values of the null hypothesis that random combinations of treatment timing and location would generate similar or larger event study results as those actually reported in the data. These p-values are always equal to or less than 0.01, providing conclusive evidence that the causal effects are strongly linked to the Banking Expansion Plans.

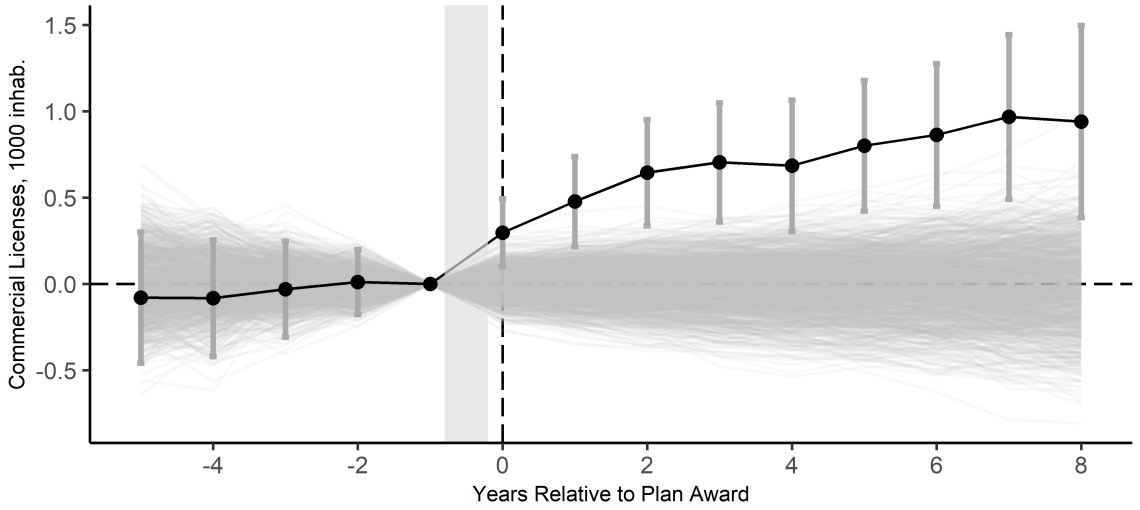

Figure 7: Saturated Permutation test results

The figure displays the results from 1,000 simulations in which the author randomly assigns Plan Award years to municipalities in a way that matches the actual date-of-award distribution shown in Figure 1. The author's own estimates are drawn in black, each simulation in gray. Results from testing whether the simulation produces more significant results on average are reported in Table A6

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Parallel Trend detection

In the event study results section, statistically insignificant lag event study estimates suggest treated municipalities were not on a better outcome trajectory than the non-treated units prior to the Banking Expansion Plan. However, recent econometric literature has pointed out that such tests often fail to reject the null of differential trends due to a lack of sufficient statistical power.[70] Following the approach outline in Roth (2021), I estimate that the event study design is able to detect with 80 percent power a positive linear pre-trend in the number of commercial licenses per capita of 0.11 or larger.[71] This hypothetical trend is plotted in contrast to the estimated event study coefficients in Figure 8. Encouragingly, even in a worst case scenario where the largest possible undetectable trend exists, the biases from the differential trend would not reach the size of the post-treatment coefficient estimates until the eighth year.

The results from Figure 8 assume a linear differential trend between treatment and control groups, an assumption which may not hold in a context of synchronous policy change and economic growth. Building upon Manski and Pepper (2018), Rambachan and Roth (2021) instead propose a methodology to assess the sensitivity of results under different sets of non-parametric assumptions that impose that the difference in trends before treatment does only change gradually in post-treatment periods. In Figure 9, deviations in post-treatment

[70] Some prominent papers include Roth (2021) and Freyaldenhoven et al. (2019).

[71] The design can also detect a linear trend of 0.07 with 50 percent power. Note that the test corresponds here to whether at least one pre-trend coefficient is significantly different from zero.

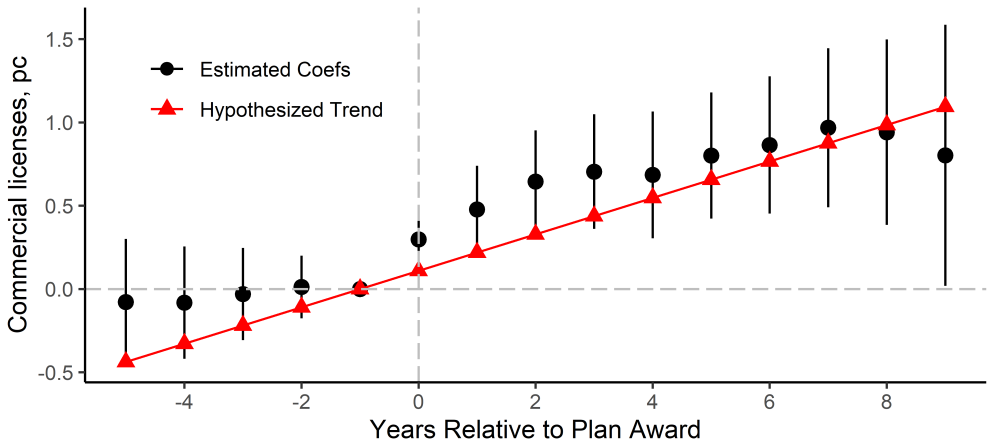

Figure 8: Detectable different linear trend with 80 percent power

The figure shows the linear trend that our event study estimates would be able to detect with 80 percent power. Such a trend would yield at least one statistically significant pre-period event study coefficient (Roth, 2021).

from the largest identified pre-treatment trend are plotted for $t = 3$ and $t = 6$ estimates implementing the authors' method. Using fixed length confidence intervals, the $t = 3$ plot shows that under non-linear violations of the common trends assumption, the effect remains significant as long as the change in slope of the differential trend is 0.02 or less. Predictably, the $t = 6$ point estimate is less robust to non-linear differences in trends, and a change in slopes of at least 0.01 will render the coefficient insignificant. Taken together, this suggests results are fairly robust to non-linear differential trends but highlight the limitations of only considering linear differential trends.

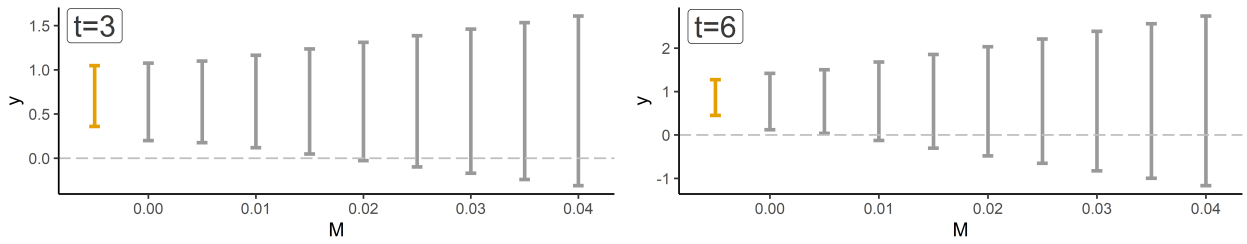

Figure 9: Sensitivity analysis on differently-sloped trends

The figure plots our t=3 and t=6 event study estimates in orange. In gray, fixed-length confidence intervals (FLCI) are plotted (Armstrong and Kolesár, 2018) for the same coefficient on some set Γ (Rambachan and Roth, 2021), which allows to test for violations of the parallel trends assumptions. The Γ set places restrictions of possible deviations in post-treatment trends (which are not identified) given the value of the pre-treatment difference in trends. If closed and convex, effect estimates can be set-identified, even when point-identification is unwarranted.

VI.A Spatial dependence

The spatial distribution of commercial licenses and awarded municipalities in a Banking Expansion Plan is geographically clustered. The latter have been shown to only sporadically use geographic concerns in the selection of municipalities, but the former's spatial clustering is undeniable. In order to control for the likely spatial dependence in the data, Appendix Table

A7 reports event study estimates clustering for different ranges of likely spatial correlation in the data. Multiple ranges are reported following guidelines in Kelly (2020), who points out Conley errors may often prove too small because of low cutoff values, the distance beyond which spatial noise is assumed to vanish.[72] Reassuringly, Conley spatial-robust standard errors do not invalidate any of the results in the previous sections, and the errors are smaller over the ones that cluster for municipality. Additionally, I test the spatial dependence of the residuals in the data using Moran I statistics for each year in the panel. Moran's statistic is significant at the 5% level with the inclusion of municipality fixed effects, suggesting these are insufficient to properly control for spatial correlation. Fortunately, spatial noise significantly declines once time-trended provincial effects are included in the empirical specification, and I fail to reject the null that errors are randomly distributed across the Spanish mainland. These results suggest that the effect of the Banking Expansion Plans on entrepreneurship is not spurious and cannot be explained by spatial noise.

VII Concluding Remarks

Although much has been written about the effects of financial conditions on growth, it is often challenging to identify the causal link between financial intermediaries and local economic outcomes. By focusing on a banking regime characterized by policy-led growth, the analysis of the Spanish Banking Expansion Plans sheds some light on the importance of regional lending on growth and local entrepreneurship. This paper exploits the exogenous variation in the selection of municipalities caused by authorities' attempts at amending two decades of supply and demand mismatch in the provision of financial services linked to the adoption of the banking *status quo* in 1939.

The results reveal the Banking Expansion Plans were an effective tool in promoting branching growth to non-banked municipalities irrespective of the latter's economic characteristics. Municipalities that received financial services in the form of a commercial bank branch are observed to experience faster growth in retail activity and entrepreneurship measures. This effect is gradual and seems to be associated with the additional provision of short-term lending, a service mostly offered by commercial banks. On the other hand, savings banks are shown to positively affect local economic conditions, but their inability to lend locally results in no major effects on entrepreneurship. These effects are demonstrated not to be driven by thriving coastal areas or spatial proximity to state-sponsored industrial development cities. Using a sample of non-banked municipalities, I am also able to disentangle the effect of alleviating binding credit constraints from that of enhanced competition typical to the finance-growth literature.

[72] The diagnostics provided by Kelly have been heatedly contested by other economists due to their cherry-picking nature, see for example Voth (2020).

More generally, I find a significant degree of heterogeneity in the effects different types of financing services have on local measures of economic activity. Small and specialized commercial banks, which the empirical and theoretical literature both predict are more effective at supplying regional credit, are shown to assist small businesses better than their regional and national counterparts. The effect on retail activity is also largely conditional on the type of informal financial services that operated locally. Using novel data on informality, I show that municipalities where local banking correspondents operated experienced a much smaller increase in commercial licenses upon their inclusion in an Expansion Plan, while the effect in locations with no alternative sources of external financing is considerably larger.

Taken together, the results above suggest that financial intermediaries are instrumental in the provision of external credit to local businesses and entrepreneurs in a context of robust demand typical of emerging economies. The banking *status quo* was a major liability for those Spanish municipalities that had no access to formal financial services, forcing them to postpone or reduce business investments and resort to less efficient sources of informal financing. The partial liberalization that followed the Banking Law of 1962 proved a boon to these locations, particularly those that came to be serviced by local banks with better regional idiosyncratic knowledge. Whether these specialized banks were able to capitalize on these new investments remains an important question for future research.

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VIII Appendix

VIII.A Tables

Table A1: *The effect of bank branch deregulation on local commercial activity*

Dependent variable:	Commercial Licenses, pc				
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline model					
Awarded in Plan	0.856***	1.01***	0.819***	0.528***	0.640***
	(0.177)	(0.166)	(0.165)	(0.152)	(0.149)
log(Population)		-1.83***	-3.13***	-3.66***	-3.95***
		(0.547)	(0.606)	(0.597)	(0.632)
Population Growth		-0.095	0.013	-0.008	0.032
		(0.109)	(0.108)	(0.106)	(0.103)
Savings Bank				0.134	0.161*
				(0.089)	(0.090)
log(Municipal Budget)				0.886***	0.843***
				(0.159)	(0.156)
log(Cultural Tax Revenue)				0.041***	0.035**
				(0.015)	(0.014)
Tourism Index				0.011***	0.012***
				(0.002)	(0.002)
Distance to Industrial Plan				0.003***	0.002
				(0.001)	(0.002)
Panel B: Bank type heterogeneity					
Branch awarded to Local Bank					1.202***
					(0.431)
Branch awarded to Regional Bank					0.546**
					(0.269)
Branch awarded to National Bank					0.810***
					(0.200)
Branch not listed in Plan					0.458
					(0.374)
Spatial Controls	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year x Province FE	No	No	No	No	Yes
Observations	35,400	35,400	35,400	35,400	35,400
Number of municipalities	2,950	2,950	2,950	2,950	2,950
Adjusted R^2	0.91	0.91	0.91	0.92	0.93
Within Adjusted R^2	0.004	0.010	0.04	0.13	0.12
Mean dependent variable	15.5	15.5	15.5	15.5	15.5

Note: Panel A of the table shows the intent-to-treat (ITT) effects of the Banking Expansion Plans on the number of commercial licenses per capita, used by the author as a proxy measure for entrepreneurship and commercial engagement. The table displays the difference-in-differences estimates for the post-expansion period, and columns reflect different specifications of the main DiD regression. See text for more details on the covariate groups. Panel B shows actual commercial bank branch openings, measured by an indicator variable that takes value one when a bank is recorded to be established within a municipality in one of the Yearbooks. Explanatory variables measure the interaction of branch openings with the bank category to which a municipality was granted, if included in the Plan.
***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests. Robust standard errors are clustered at the municipality level.

Table A2: *The effect of bank branch expansion under different official criteria*

Dependent Variable:	Commercial Licenses, per 1000 inhab.			
	Crit. A	Crit. B	Crit. D	Crit. B-D
Number Commercial Banks	0.69***	-0.06	0.06**	0.04*
	(0.14)	(0.08)	(0.02)	(0.02)
Demographic Controls	Yes	Yes	Yes	Yes
Spatial Controls	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Year x Province FE	Yes	Yes	Yes	Yes
Mean dependent variable	15.5	15.6	16.1	16.5
Number of observations	35,400	31,212	31,608	34,440
Number of municipalities	2,950	2,601	2,634	2,870

Note: The table shows the treatment-on-the-treated (TOT) effects of the Banking Expansion Plans on the number of commercial licenses per capita, used by the author as a proxy measure for entrepreneurship and commercial engagement. Columns correspond to different criteria used by the Bank of Spain to classify municipalities under. For details, see the description in the text.
***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests. Robust standard errors are clustered at the municipality level.

Table A3: *The effect of bank branch deregulation on local commercial activity*

Dependent Variable:	Bank Branch	Commercial Licenses, pc	
	(1)	(2)	(3)
Panel A: TWFE DiD Model			
Awarded in Plan	0.77***	0.85***	0.64***
	(0.02)	(0.17)	(0.15)
Panel B: Event Study Model			
Year -5	0.00	-0.06	-0.08
	(0.02)	(0.20)	(0.19)
Year -4	-0.02	-0.04	-0.08
	(0.01)	(0.17)	(0.17)
Year -3	-0.01	-0.04	-0.03
	(0.01)	(0.16)	(0.14)
Year -2	0.00	-0.01	0.01
	(0.00)	(0.10)	(0.10)
Year -1 (Omitted)			
Year 0	0.79***	0.40***	0.30***
	(0.02)	(0.10)	(0.10)
Year 1	0.79***	0.61***	0.48***
	(0.02)	(0.14)	(0.13)
Year 2	0.78***	0.77***	0.64***
	(0.02)	(0.16)	(0.16)
Year 3	0.77***	0.84***	0.70***
	(0.02)	(0.18)	(0.18)
Year 4	0.76***	0.88***	0.68***
	(0.02)	(0.20)	(0.19)
Year 5	0.75***	1.04***	0.80***
	(0.02)	(0.22)	(0.19)
Year 6	0.74***	1.22***	0.86***
	(0.02)	(0.25)	(0.21)
Year 7	0.73***	1.40***	0.97***
	(0.02)	(0.31)	(0.24)
Year 8	0.72***	1.43***	0.94***
	(0.02)	(0.38)	(0.28)
Demographic Controls	Yes	No	Yes
Spatial Controls	Yes	No	Yes
Economic Controls	Yes	No	Yes
Municipality FE	Yes	Yes	Yes
Year x Province FE	Yes	Yes	Yes
Mean dependent variable	0.28	15.51	15.51
Number of observations	35,400	35,400	35,400
Number of municipalities	2,950	2,950	2,950

Note: The table shows the author's estimation of equation (2). Panel A displays the original difference-in-differences estimates for the post-expansion period. Panel B shows the event study coefficients, which report the lead and lag effects of the Banking Banking Expansion Plans on the number of commercial licenses per capita, used by the author as a proxy measure for entrepreneurship and commercial engagement. See the text for a detailed description of the control variables.
***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests. Robust standard errors are clustered at the municipality level.

Table A4: *Alternative event study estimators*

Dependent Variable:	Commercial Licenses, per 1000 inhab.			
	Callaway & Sant'Anna		Sun & Abraham	Cengiz
	(1)	(2)	(3)	(4)
Panel A: Overall ToT				
Awarded in Plan	0.86***	0.57***		
	(0.19)	(0.19)		
Panel B: Event Study				
Year -5	0.00	-0.02	-0.18	-0.19
	(0.14)	(0.15)	(0.22)	(0.25)
Year -4	0.00	-0.01	-0.21	-0.21
	(0.11)	(0.11)	(0.18)	(0.20)
Year -3	-0.03	-0.05	-0.21	-0.16
	(0.09)	(0.10)	(0.13)	(0.15)
Year -2	-0.01	-0.05	-0.18**	-0.16**
	(0.09)	(0.09)	(0.07)	(0.07)
Year -1	0.04	-0.02		
	(0.08)	(0.09)		
Year 0	0.15**	0.06	0.13**	0.11*
	(0.06)	(0.07)	(0.06)	(0.06)
Year 1	0.37***	0.21	0.30***	0.27***
	(0.12)	(0.13)	(0.10)	(0.10)
Year 2	0.57***	0.37**	0.46***	0.41***
	(0.15)	(0.18)	(0.13)	(0.13)
Year 3	0.67***	0.42*	0.52***	0.46***
	(0.17)	(0.22)	(0.15)	(0.15)
Year 4	0.76***	0.43*	0.50***	0.47***
	(0.20)	(0.26)	(0.17)	(0.17)
Year 5	0.95***	0.55**	0.65***	0.62***
	(0.23)	(0.27)	(0.18)	(0.18)
Year 6	1.16***	0.73**	0.68***	0.74***
	(0.28)	(0.30)	(0.21)	(0.19)
Year 7	1.44***	1.05***	0.76***	0.92***
	(0.37)	(0.34)	(0.25)	(0.22)
Year 8	1.66***	1.32***	0.70**	0.96***
	(0.45)	(0.44)	(0.30)	(0.26)
Controls	No	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	No
Year x Province FE	Yes	Yes	Yes	No
Stack x Municipality FE	No	No	No	Yes
Stack x Year FE	No	No	No	Yes
Stack x Province FE	No	No	No	Yes

Note: The table shows the author's estimation of equation (2) using a series of alternative methods proposed in recent years to improve identification and inference of treatment effects in the context of heterogeneous outcomes and staggered designs. Callaway and SantAnna (2020) use a series of group-time ATT estimates weighted by group-specific propensity scores, a generalization of Abadie (2005). Sun and Abraham (2020) instead estimate the dynamic effect for each treatment cohort, which are then weighted by their sample share and aggregated. Cengiz et al. (2019) create group-specific datasets and discard soon-to-be-treated observations. The specification is identical to (3), but now fixed effects are saturated with indicators for the specific stacked datasets.
***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests.

Table A5: *Matching estimators*

Dependent Variable:	Commercial Licenses, per 1000 inhab.	
	Baseline Match	Stack & Match
Year -5	0.03	0.11
	(0.20)	(0.34)
Year -4	-0.07	0.00
	(0.17)	(0.28)
Year -3	-0.02	-0.02
	(0.14)	(0.19)
Year -2	-0.01	-0.02
	(0.09)	(0.10)
Year -1 (Omitted)		
Year 0	0.18	-0.03
	(0.11)	(0.09)
Year 1	0.35**	0.03
	(0.15)	(0.14)
Year 2	0.50***	0.11
	(0.17)	(0.18)
Year 3	0.57***	0.16
	(0.19)	(0.20)
Year 4	0.59***	0.24
	(0.21)	(0.22)
Year 5	0.72***	0.46**
	(0.21)	(0.23)
Year 6	0.85***	0.58**
	(0.23)	(0.26)
Year 7	0.97***	0.60**
	(0.27)	(0.29)
Year 8	1.01***	0.54*
	(0.31)	(0.21)
Demographic Controls	Yes	Yes
Spatial Controls	Yes	Yes
Economic Controls	Yes	Yes
Municipality FE	Yes	No
Year x Province FE	Yes	No
Stack x Municipality FE	No	Yes
Stack x Year FE	No	Yes
Stack x Province FE	No	Yes

Note: The table shows the author's estimation of equation (2) using subsets of matched control municipalities. The first column uses propensity score matching to draw a sample of untreated municipalities with demographic, spatial and economic characteristics that are closest to the treated municipalities. The second column repeats this process for each treatment group on the set of municipalities included in the group. These are then stacked into a model with saturated fixed effects to account for the different datasets.
***, **, and * indicate significance at the 0.01, 0.05 and 0.10 levels, respectively, using two-tailed tests.

Table A6: *p-values of permutation tests on the baseline event study model*

	t=0	t=1	t=2	t=3	t=4	t=5	t=6	t=7	t=8
Percent more than baseline	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Note: Estimates include demographic, spatial and economic covariates as well as time-trended province fixed effects and municipality fixed effects. The table shows the proportion of times the estimates from the permutation tests are more significant than the baseline estimate, over 1,000 simulations.

Table A7: *The effects of banking deregulation accounting for spatial dependence*

Dependent Variable	Commercial Licenses, 1000 inhab.					
Cut-off Value	5 km.	10 km.	25 km.	50 km.	100 km.	200 km.
Year -5	0.02	0.02	0.02	0.02	0.02	0.02
	(0.20)	(0.20)	(0.19)	(0.17)	(0.20)	(0.20)
Year -4	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.19)	(0.19)	(0.19)	(0.13)	(0.15)	(0.16)
Year -3	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.16)	(0.16)	(0.17)	(0.15)	(0.19)	(0.19)
Year -2	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.10)	(0.10)	(0.09)	(0.10)	(0.11)	(0.11)
Year -1 (Omitted)						
Year 0	0.32***	0.32***	0.32***	0.32***	0.32***	0.32***
	(0.10)	(0.10)	(0.09)	(0.09)	(0.10)	(0.10)
Year 1	0.53***	0.53***	0.53***	0.53***	0.53***	0.53***
	(0.14)	(0.13)	(0.13)	(0.12)	(0.13)	(0.13)
Year 2	0.71***	0.71***	0.71***	0.71***	0.71***	0.71***
	(0.16)	(0.15)	(0.14)	(0.16)	(0.17)	(0.17)
Year 3	0.78***	0.78***	0.78***	0.78***	0.78***	0.78***
	(0.18)	(0.17)	(0.16)	(0.18)	(0.19)	(0.18)
Year 4	0.77***	0.77***	0.77***	0.77***	0.77***	0.77***
	(0.20)	(0.19)	(0.18)	(0.20)	(0.20)	(0.19)
Year 5	0.90***	0.90***	0.90***	0.90***	0.90***	0.90***
	(0.20)	(0.20)	(0.19)	(0.21)	(0.21)	(0.18)
Year 6	1.01***	1.01***	1.01***	1.01***	1.01***	1.01***
	(0.21)	(0.21)	(0.21)	(0.23)	(0.26)	(0.21)
Year 7	1.16***	1.16***	1.16***	1.16***	1.16***	1.16***
	(0.25)	(0.25)	(0.24)	(0.26)	(0.30)	(0.27)
Year 8	1.19***	1.19***	1.19***	1.19***	1.19***	1.19***
	(0.30)	(0.29)	(0.29)	(0.29)	(0.31)	(0.24)

VIII.B Figures

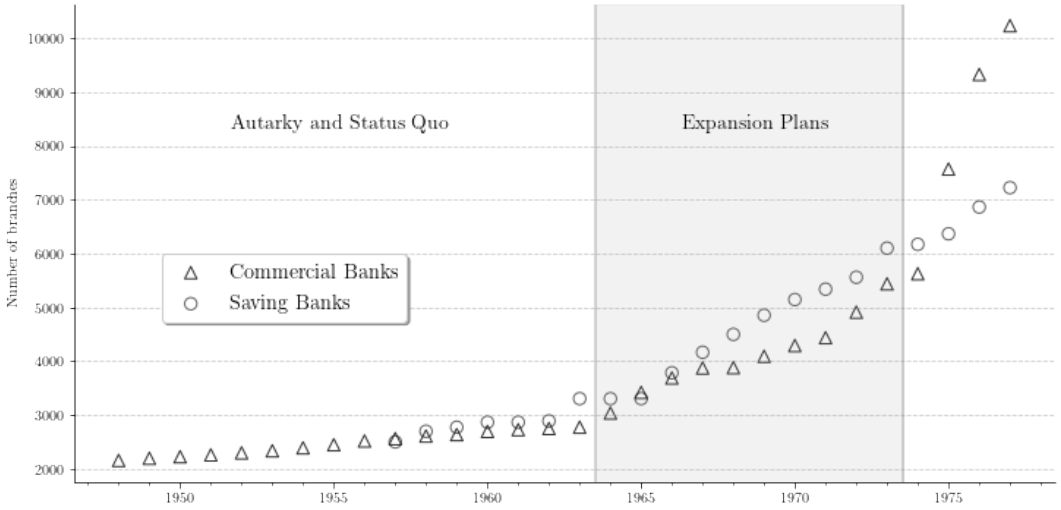

Figure A1: Number of commercial and savings banks (1948-1978)

The graph displays the number of savings and commercial bank branches over time in Spain. Our sample of available municipalities yield slightly lower values for both variables, on account of some municipalities with under 1,000 inhabitants having a branch established within their territorial boundaries. Importantly, the growth in levels observed during the Expansion Plan years is nearly identical to the increase observed in our sample, due to the nature of the Expansion Plans rarely including municipalities with a population of under 1,000 inhabitants.

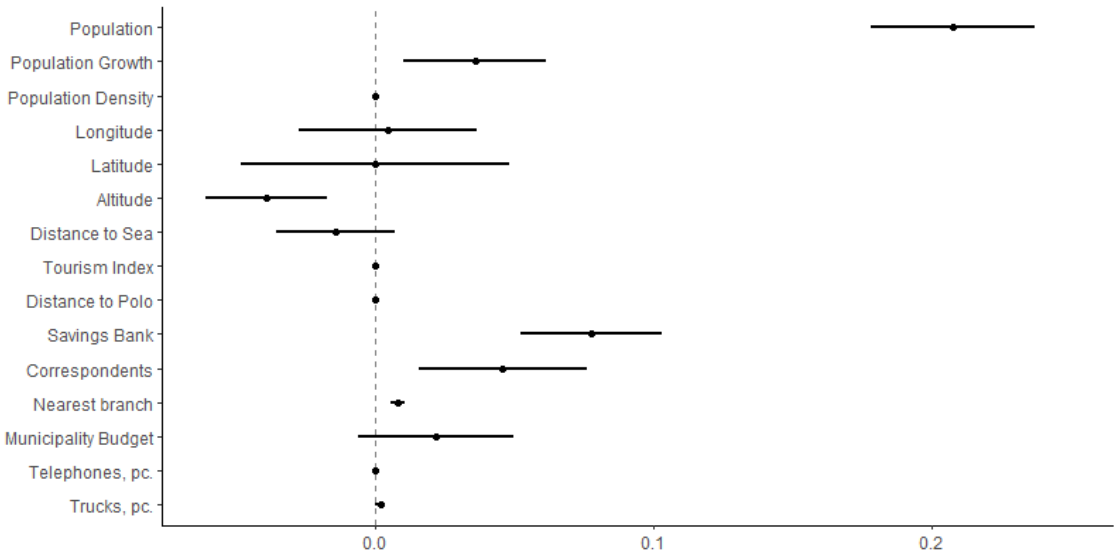

Figure A2: Determinants of Plan Award (1965-1971)

The figure displays coefficient estimates and 95% significance confidence intervals for a battery of covariates that may have influenced policymakers' decision on whether to include a municipality in a Plan under criteria A. The coefficients are estimated from a regression of the form $Award_{i,t} = \gamma X_{i,t} + t\theta_p + \varepsilon_{i,p,t}$ for municipality i in province p during year t.

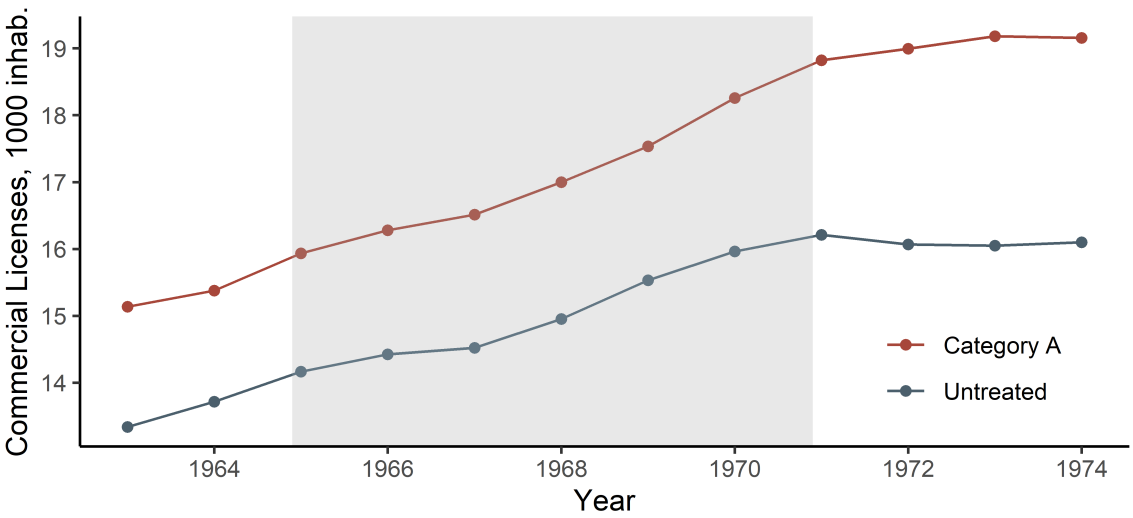

Figure A3: Mean number of commercial licenses per 1000 inhabitants (1963-1974)

The figure displays the average number of commercial licenses per capita for untreated municipalities and treated municipalities under category *A*.