

# 21DM008 TERM PAPER: LOCAL GROWTH AND FINANCING: THE CASE OF FRANCOIST SPAIN

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## Abstract

This term paper 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. Importantly, while the spatial distribution of commercial licenses and awarded municipalities in a Banking Expansion Plan is geographically clustered, our results are robust to most specifications of Conley spatial-robust standard errors. We also control for most spatial spillovers by building kernelized spatial measures of banking presence and least cost paths using historical road maps. Overall, our results reinforce the argument that policy-led expansion of credit intermediaries can be an effective tool to promote growth and alleviate financing constraints.

## I Introduction & Research Question

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, reducing the rate of innovation and economic growth ([Aghion and Bolton, 1997](#)). 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. 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,

affecting both the incentives and ability of these specialized banks to acquire information about the regional markets they operate in. 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 (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. 2015, Dugas 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 and Pande, 2005). In line with the fact that regional financial specialization is spatially sensitive and driven by long-term relationships, 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 term paper attempts to examine 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).<sup>1</sup> 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,

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<sup>1</sup> Ley 2/1962, de 14 de abril, sobre bases de ordenación del crédito y de la Banca, Official State Gazette, 16 of April 1962.

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.<sup>2</sup> 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.<sup>3</sup> Nine plans were drafted between 1964 and 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.<sup>4</sup>

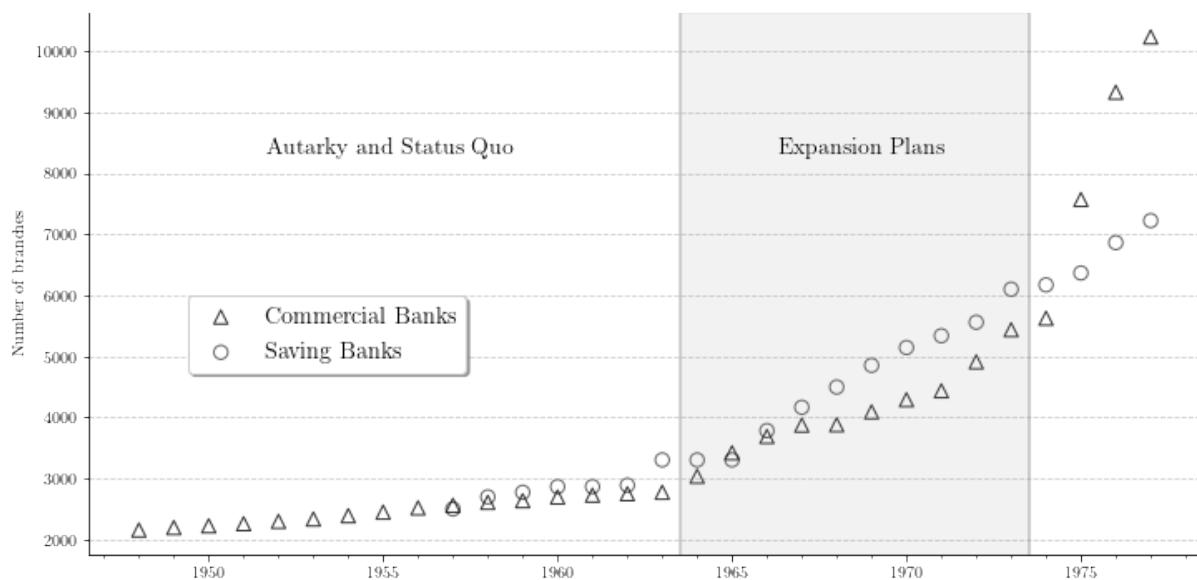


Figure 1: Number of Commercial and Savings Bank branches

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.

<sup>2</sup>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".

<sup>3</sup>The formula that determined a bank's expansion capacity was complex and somewhat archaic, as it included the number of rooms in all branches a given bank possessed.

<sup>4</sup>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.

In a first stage, we 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, the selection of municipalities is driven by the criteria detailed in the First Plan, but preliminary results show that it 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.<sup>5</sup> Having established that the selection of a non-banked municipality in a Banking Expansion Plan predicts a commercial branch expansion, we aim to show that the exogenous improvement in financial services boosted local measures of entrepreneurship and retail activity.

## 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.<sup>6</sup>

#### 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).<sup>7</sup> 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).<sup>8</sup> Interest rates were regulated, and preferential rates were set for

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<sup>5</sup>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.

<sup>6</sup>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.

<sup>7</sup>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.

<sup>8</sup>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

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 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.<sup>9</sup> 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 manageable pace of credit lending growth and preventing unbridled competition stemming from "the excessive proliferation of branches".<sup>10</sup> 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

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absorbing smaller entities and providing lucrative credit to protected industries. See Pueyo Sánchez (2006) for a complete discussion.

<sup>9</sup>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.

<sup>10</sup>Decreto 1312:1963, de 5 de junio, sobre modificación del *status quo bancario*, p.1., Official State Gazette, 9 June 1963.

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 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.<sup>11</sup> 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.<sup>12</sup>

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, 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.<sup>13</sup> 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.<sup>14</sup> The number of awarded municipalities in the course of the first seven Expansion Plans is shown in Table 1.

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<sup>11</sup>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.

<sup>12</sup>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.

<sup>13</sup>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.

<sup>14</sup>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.

Table 1: The Banking Expansion Plans

Plan	Appr. Date	Requested				Not Requested				Total	
		A	B	C	D	Total	A	B	C	D	
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 a prudent moderation in the number of new branches was warranted.<sup>15</sup> Figure 2 shows the geographical evolution of allocated and non-requested branches for category *A* municipalities over the first seven plans.

<sup>15</sup>Following the first four Plans alone, the number of banking branches had grown by a third over the country's total.

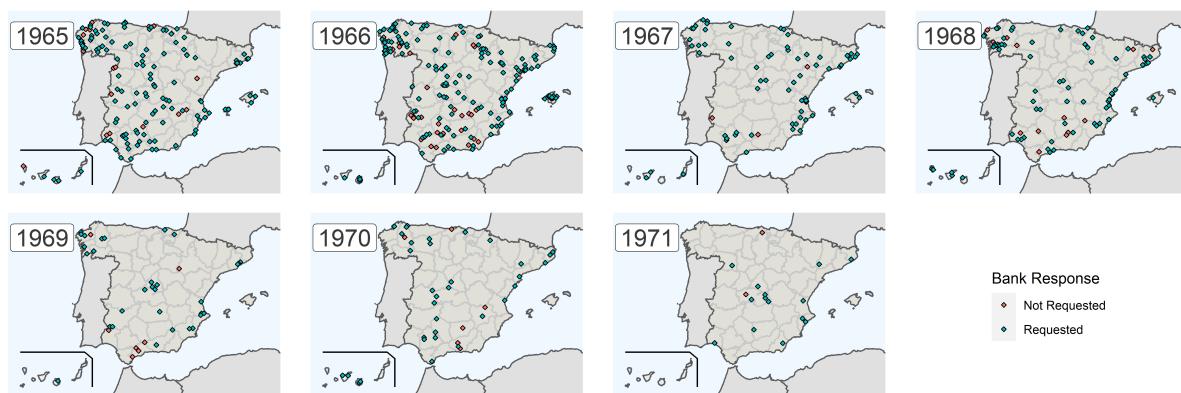


Figure 2: Geographic distribution of municipalities included in a Plan under criteria A (1965-1971)

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".<sup>16</sup> The two Plans that followed included a new type of authorization, whereby commercial banks 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.<sup>17</sup>

<sup>16</sup> Orden Ministerial de 15 de noviembre por la que se modifican las normas de expansión bancaria, Official State Gazette, 18 of November 1971

<sup>17</sup> 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.

## II.B A theory of the role of financial intermediaries

The underlying premise that partly motivates the inclusion by central bank officials of unbanked municipalities 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.<sup>18</sup> 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  $\theta$  is considered to be the observed entrepreneurial ability by the individual,  $k$  is the amount of capital that was invested in the venture and  $\varepsilon$  captures random noise assumed normally distributed, i.i.d. and unobserved at baseline. Individuals with more entrepreneurial aptitude have both higher ability  $\theta$  and larger returns from capital, regulated by  $\alpha$ . The net income for a self-employed individual  $i$  is the payoff and the return on wealth during the 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  $\lambda_m$  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

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<sup>18</sup>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.

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  $\theta$ , suggesting that more able individuals are also more likely to self-employ. However, if financial constraints  $\lambda_m$  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  $\lambda_m$  for treated municipalities over those untreated, uncorrelated with wealth or unobserved ability. In this setup, identification not only requires exogeneity in the treatment, but also that some individuals with sufficiently high  $\theta_i$  within the treated municipalities will switch from employment to self-employed upon the increase in  $\lambda_m$ .

### III Data

To analyze the effects of these Banking Expansion Plans, we create a novel data set of Spanish municipalities with data on economic outcomes and proxy measures of entrepreneurship spanning between 1963 and 1975. This data is matched with internal sources used by officials at the Bank of Spain in the drafting of the Expansion Plans, which we obtained from the Historical Archive of the Bank of Spain in March 2021 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. We 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. Table 2 presents summary statistics of some of the variables.

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.

### III.A Spatial Methodology

We take particular care about the spatial modelling of individuals' access to financial intermediaries. In particular, we attempt to dispel concerns about spatial spillovers and masking geographical heterogeneity by presenting a battery of spatial measures and robustness checks. We present the methods used to attain the necessary measures and the measures themselves below.

#### Map Digitization

In order to analyze the road network's impact we needed to recover the data from a set of historical maps. Since the data is only a jpeg image, we first georeference the map to convert it into a raster file that can be worked with. Figure 3a is an example of the 1970 map that has been georeferenced; this same process was employed for the three other maps of 1960, 1965, and 1975. Since we are concerned with distances, we reproject the image into the UTM time zone that is consistent with spain. This happens to be the coordinate reference system EPSG:62630. Figure 3b shows the reprojected raster overlayed onto a base map to verify that our georeferencing and projection is accurate. Next we digitize the map. We take advantage of the fact that the roads have high contrast relative to the background values and manually digitize the map by setting thresholds for RGB values. In general the background RGB values are above 200 for multiple channels, and by using a simple additive algorithmn we were able to digitize the map fairly accurately to produce figure 4a. Finally we use a shapefile of Spain to clip the raster leaving us with figure 4b. We then use these processed rasters to compute least cost paths to inform our

economic model.



Figure 3: Processing of 1970 Spanish Map

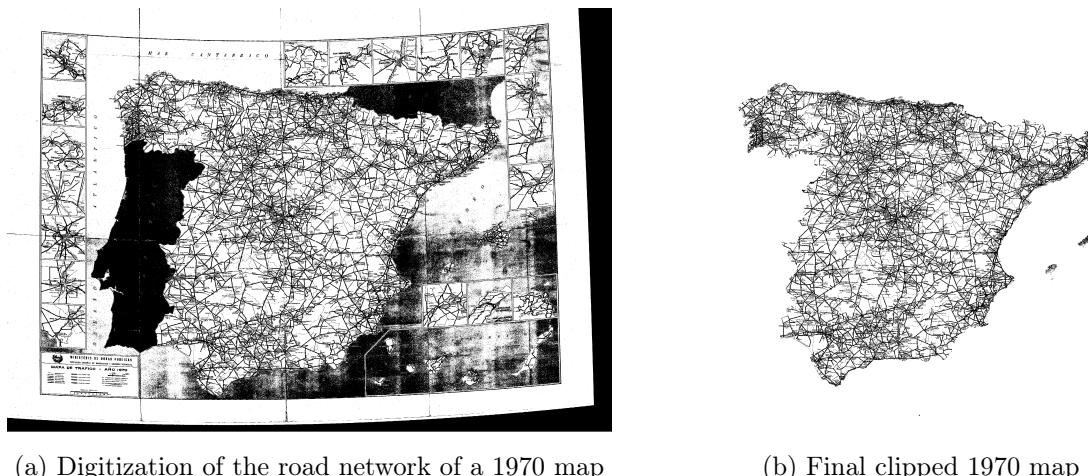


Figure 4: Rasterizing 1970 Spanish Map

### Least Cost Paths

The distance to banking services matters, as was repeatedly observed by officials at the Bank of Spain when drafting the plans. This is well in line with the oft-cited statement made by the president of the Italian Association of Bankers at the time: “the banker’s rule-of-thumb is to never lend to a client located more than five kilometers from his office”. In order to account for this, we build a dataset with average least cost paths for each municipality over each year. We employ historical maps of the traffic roads of mainland Spain during the sixties and seventies to accurately capture municipalities’ relative ability to access such services when absent locally. An example of these digitized maps is shown in Appendix Figure A1.<sup>19</sup>

The approach above provides us with a time-varying dataset of least cost paths to financial services, which evolves in time both due to the opening of local branches as well as the building

<sup>19</sup>We approximately georeference the maps by matching locations between the maps and OpenStreetMaps.

of new roads. Figures 5 and 6 highlight this evolution over time in the dataset. We employ a parsimonious cost function: a cost of 2 is incurred whenever on a road, while a cost of 5 in addition to the slope is incurred when travelling off-road. The results are not sensitive to the choice of cost function or its standardization, which provides reassuring evidence that our underlying identification assumptions hold.

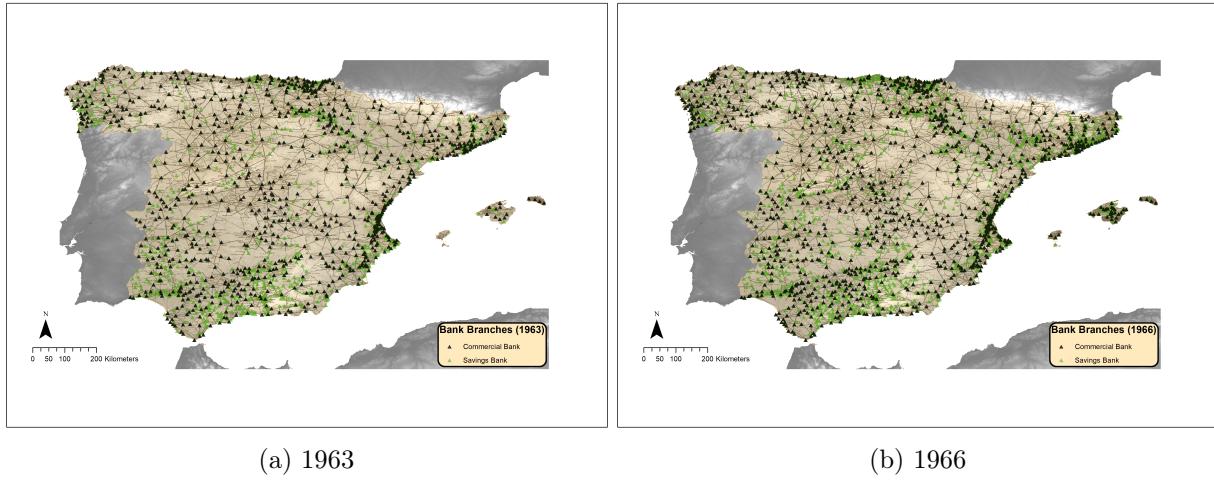


Figure 5: Banking branches and Spanish Road Network

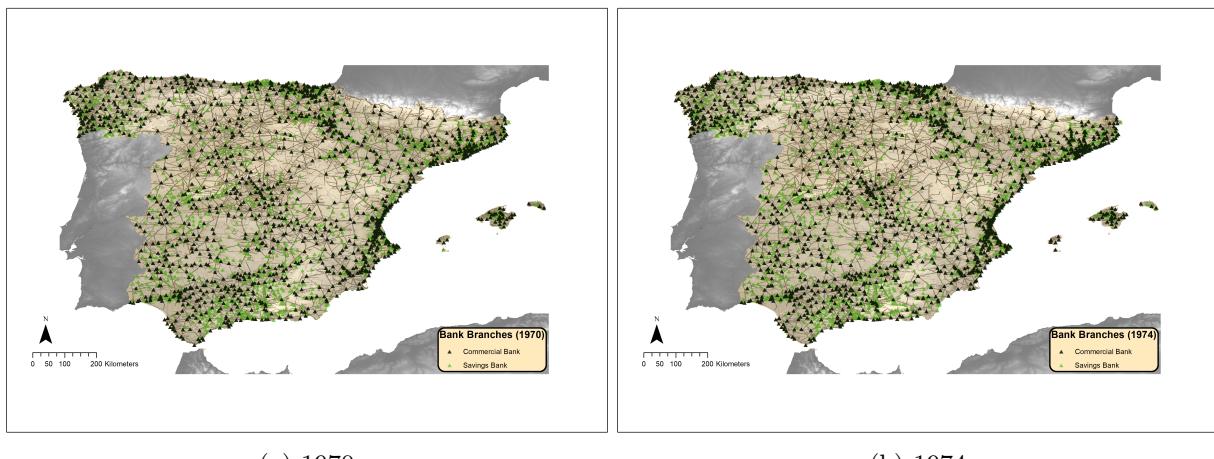


Figure 6: Banking branches and Spanish Road Network

## General Distance Measures

In addition to the least-cost path variables defined above (for both savings and commercial banks), we also consider additional variables specific to each municipality. On the one hand, we create buffer areas and estimate the presence of roads as a measure of a municipality's interconnectedness. Additionally, we compute the geographical distance of a municipality to the coastline, which as expected is a strong moderator of our outcome of interest as the Spanish tourism miracle unfolds throughout the sixties. We also consider simpler measures of elevation and latitude and longitude, which nonetheless have been shown in the literature to be efficient at capturing spatial correlation (see [Voth \(2020a\)](#) for a recent review of the subject).

## Kernel Weights

Finally, we want to control for the spillover effects of nearby regions. We do this by controlling for the change in commercial licenses of nearby regions. To do this we construct a kernel of spatial weights and create spatial lags of the number of commercial licenses per capita. We construct this spatial kernel by using either a Gaussian kernel or a triangular kernel. We tested both kernels to ensure our results are robust to the choice of kernel. The Gaussian and triangular kernels are given by:

$$K_{Gaussian}(x, y) = \frac{1}{\sqrt{2\pi}} e^{d(x,y)^2/2h^2}$$
$$K_{Triangular}(x, y) = (1 - |\frac{d(x, y)}{h}|) \mathbb{1}_{|\frac{d(x, y)}{h}| \leq 1}$$

Where  $d(x, y)$  is the distance between  $x$  and  $y$ , and  $h$  is a fixed bandwidth. For the Gaussian Kernel we can see that the bandwidth is equivalent to the standard deviation of a Normal distribution and can be interpreted as such. For example, we consider a fixed bandwidth of 20km, which means the spatial weights falls quickly as we move further away. In particular, within two or three standard deviations there is little to no effect ( $>40$ -60km). The triangular kernel has a harder boundary where, due to the indicator function, if we are outside of 20km, the spatial weights are set to zero. Otherwise, the spatial weights decrease proportionately. The fixed bandwidth can be interpreted as the reasonable distance at which we might expect spillover effects to actually occur. We construct this spatial weights matrix in this way using the fifty nearest neighbours to encapsulate the economic activity of the neighbouring regions. Reassuringly, we find that when we change the bandwidth size, the neighbours or the kernel choice our results remain robust and do not change much. Using this methodology we are able to create variables that parametrize the spatial lag of commercial licenses per capita in the neighbourhood of our region of interest.

## IV Empirical approach

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. A potential 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. As such, our main design explores the evolution of the outcomes of interest using leads and lags for the main treatment indicator.

#### 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:

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

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.

Specifically, defining  $Award_{i,t}^{BEP}$  as an indicator for a municipality being included in one of the Plans, we 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.<sup>20</sup>

In addition, equation (1) includes province-by-year ( $\theta_{p,t}$ ) and municipality  $\tau_i$  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.<sup>21</sup> 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 group. The mu-

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<sup>20</sup>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.

<sup>21</sup>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.

nicipality 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:

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

where indicator variables  $\mathbb{1}[t - t_i^g = k]$  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.<sup>22</sup> The omitted relative year is  $k = -1$ , the period prior to treatment. Therefore, each other estimate  $\delta$  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  $\delta_0$  to  $\delta_8$ , 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 included by Bank of Spain authorities, 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  $\delta_{-5}$  to  $\delta_{-2}$  coefficients to test whether selection into the Plans was associated with differential trends prior to treatment. 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.

The second assumption is more concerning for the purposes of this study. The late Franco regime

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<sup>22</sup>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  $t - 1$ .

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.

#### 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.

In earlier sections 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".<sup>23</sup> 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. 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 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 variable of interest, Table 3 presents saturated regressions of baseline economic characteristics on the dummy for treatment and actual branch openings.

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<sup>23</sup> *Carta del Gobernador del Banco de España al Ministro de Hacienda*, 7 October 1964, AHBE.

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)
<i>Panel A: Treatment</i>					
Awarded in Plan	0.77*** (0.02)	-0.25 (0.32)	2.75 (2.35)	0.10 (0.17)	9.57 (10.05)
<i>Panel A: Branch opening</i>					
Commercial Bank		3.36*** (0.32)	9.56*** (1.64)	1.09*** (0.19)	57.32*** (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 Empirical 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. The first section briefly studies difference-in-differences estimates using reduced-form 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 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.

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

Dependent Variable:	Commercial Licenses, per 1000 inhab.				
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Intent to Treat</i>					
Awarded in Plan	0.86*** (0.18)	1.01*** (0.17)	0.82*** (0.16)	0.53*** (0.15)	0.64*** (0.15)
<i>Panel B: Treatment on Treated</i>					
Commercial Bank	0.87*** (0.18)	1.11*** (0.17)	0.93*** (0.16)	0.69*** (0.15)	0.72*** (0.15)
<i>Panel C: Local Treatment on Treated</i>					
Commercial Bank	1.07*** (0.22)	1.29*** (0.21)	1.05*** (0.21)	0.68*** (0.20)	0.83*** (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.

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.<sup>24</sup> 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, our least cost path estimates as well as different kernel weights of local financial intensity.

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.

## V.B Event Study Analysis

The results above 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

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<sup>24</sup>All results are robust to either a binary or continuous measurement of population growth.

treatment timing and heterogeneous effects across treatment groups.<sup>25</sup> In particular, [Goodman-Bacon \(2018\)](#) shows that in a setting of multiple treatment groups across different periods, the  $\delta$  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.

Figure 7 presents the main results of estimating the effects of state-led banking 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. Note that these results are also described in Table 5.

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<sup>25</sup>Some of the more prominent papers studying this include [Callaway and SantAnna \(2020\)](#), [Sun and Abraham \(2020\)](#), [de Chaisemartin and D'Haultfoeuille \(2020\)](#) and [Borusyak et al. \(2021\)](#).

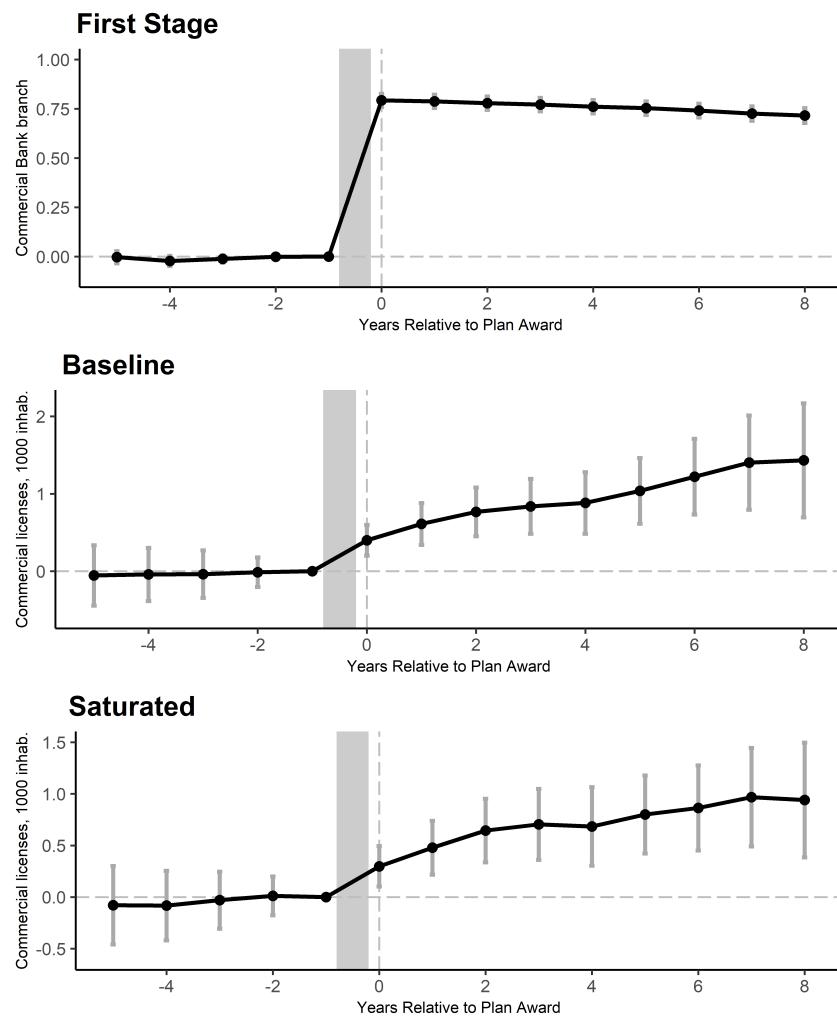


Figure 7: The effect of banking branch deregulation on non-banked municipalities

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.<sup>26</sup> Additionally, this effect is only gradual as hurdles relating to monitoring borrowers and solving asymmetric information problems are worked out over time.

Table 5: First stage instrument relevance and selection into treatment

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<sup>26</sup>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.

The third plot in Figure 7 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 as well as the spatial variables described in the Methodology section. 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.

## VI Robustness

### 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.<sup>27</sup>

Figure 8 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 8 instead uses the full set of control variables to create synthetic group-specific balanced samples to estimate each group-time average treatment effect, including our main body of spatial controls. 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 an eventual increase in commercial licenses following treatment.<sup>28</sup>

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<sup>27</sup>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 period  $g$ ,  $C$  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.

<sup>28</sup>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.

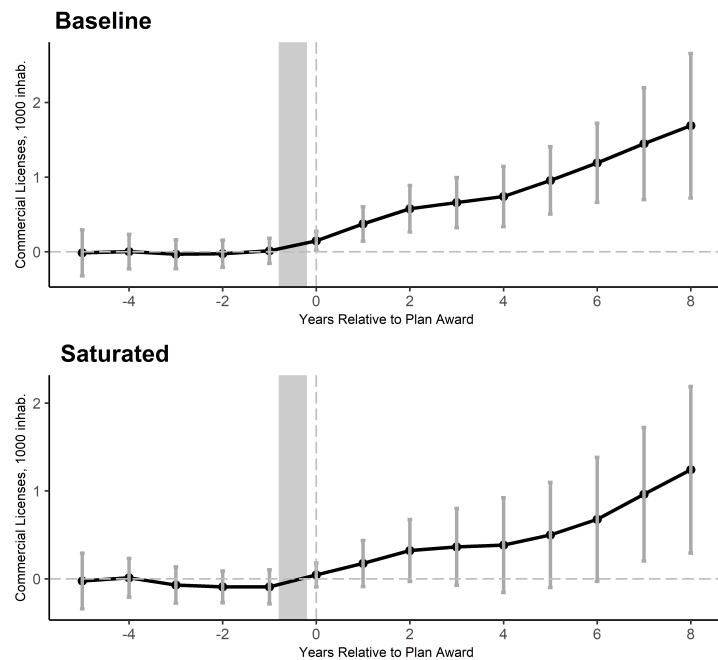


Figure 8: Callaway-Sant'Anna estimates for the effect of banking branch deregulation

The figure reports the resulting coefficients from using (Callaway and Sant'Anna, 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.

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 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 9. 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.

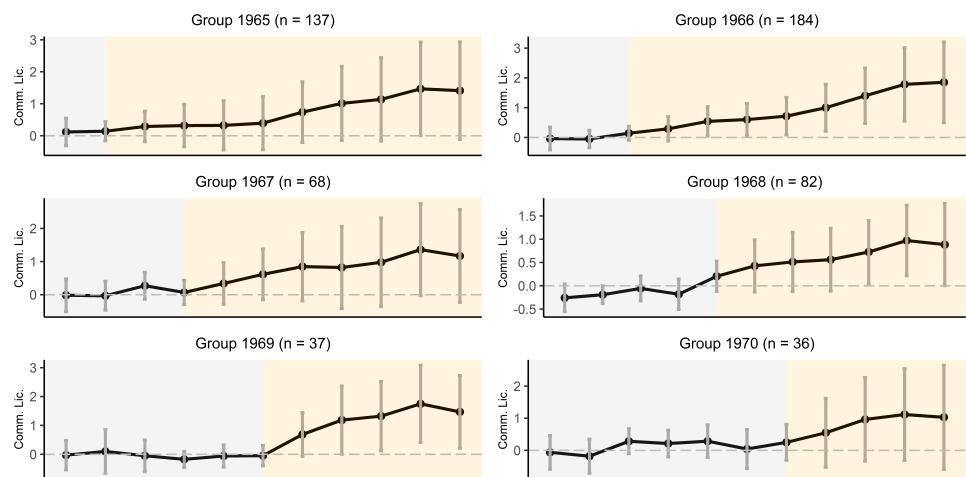


Figure 9: The effects of each Plan on commercial licenses per 1000 inhabitants

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.

### 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, we 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 10.

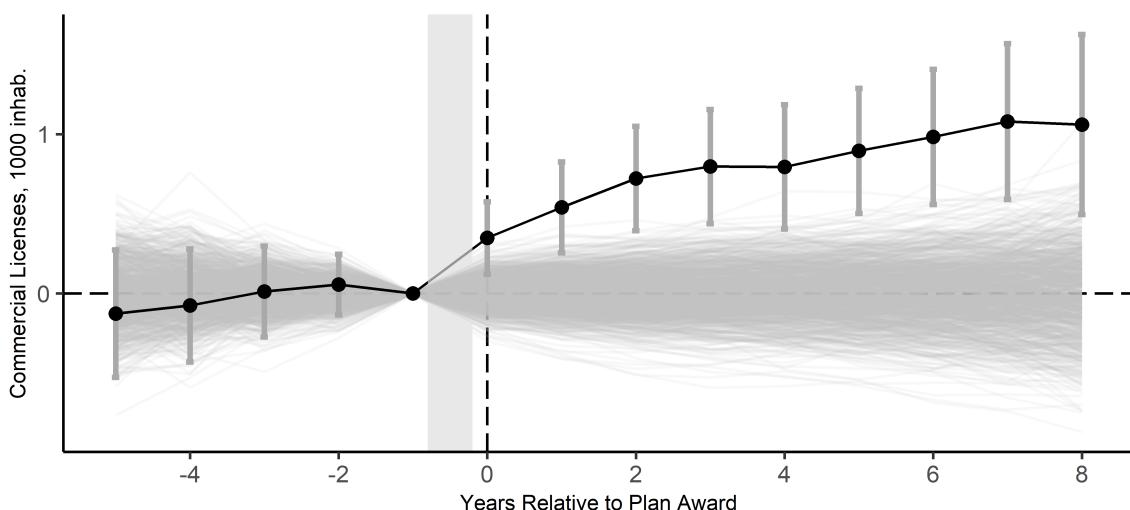


Figure 10: Saturated Permutation test results

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.

Figure 10 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 A1 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.

## 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, Table 6 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.<sup>29</sup> 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, we 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 we 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.

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<sup>29</sup>The diagnostics provided by Kelly have been heatedly contested by other economists due to their cherry-picking nature, see for example Voth (2020b).

Table 6: The effects of banking deregulation account 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	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.20)	-0.06 (0.18)	-0.06 (0.22)	-0.06 (0.21)
Year -4	-0.01 (0.20)	-0.01 (0.20)	-0.01 (0.20)	-0.01 (0.13)	-0.01 (0.17)	-0.01 (0.17)
Year -3	0.03 (0.16)	0.03 (0.17)	0.03 (0.18)	0.03 (0.15)	0.03 (0.20)	0.03 (0.20)
Year -2	0.03 (0.10)	0.03 (0.10)	0.03 (0.10)	0.03 (0.10)	0.03 (0.11)	0.03 (0.11)
Year -1 (Omitted)						
Year 0	0.41*** (0.12)	0.41*** (0.12)	0.41*** (0.11)	0.41*** (0.11)	0.41*** (0.14)	0.41*** (0.15)
Year 1	0.63*** (0.15)	0.63*** (0.15)	0.63*** (0.15)	0.63*** (0.15)	0.63*** (0.17)	0.63*** (0.18)
Year 2	0.81*** (0.17)	0.81*** (0.17)	0.81*** (0.16)	0.81*** (0.18)	0.81*** (0.21)	0.81*** (0.19)
Year 3	0.88*** (0.19)	0.88*** (0.18)	0.88*** (0.18)	0.88*** (0.20)	0.88*** (0.22)	0.88*** (0.20)
Year 4	0.88*** (0.21)	0.88*** (0.19)	0.88*** (0.19)	0.88*** (0.22)	0.88*** (0.23)	0.88*** (0.23)
Year 5	0.98*** (0.21)	0.98*** (0.20)	0.98*** (0.20)	0.98*** (0.22)	0.98*** (0.24)	0.98*** (0.19)
Year 6	1.09*** (0.22)	1.09*** (0.22)	1.09*** (0.21)	1.09*** (0.24)	1.09*** (0.28)	1.09*** (0.22)
Year 7	1.22*** (0.26)	1.22*** (0.26)	1.22*** (0.25)	1.22*** (0.28)	1.22*** (0.32)	1.22*** (0.29)
Year 8	1.27*** (0.30)	1.27*** (0.30)	1.27*** (0.30)	1.27*** (0.31)	1.27*** (0.33)	1.27*** (0.28)

## VII Conclusion

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 do not appear to be driven by thriving coastal areas or spatial spillovers. 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.

## VIII Appendix

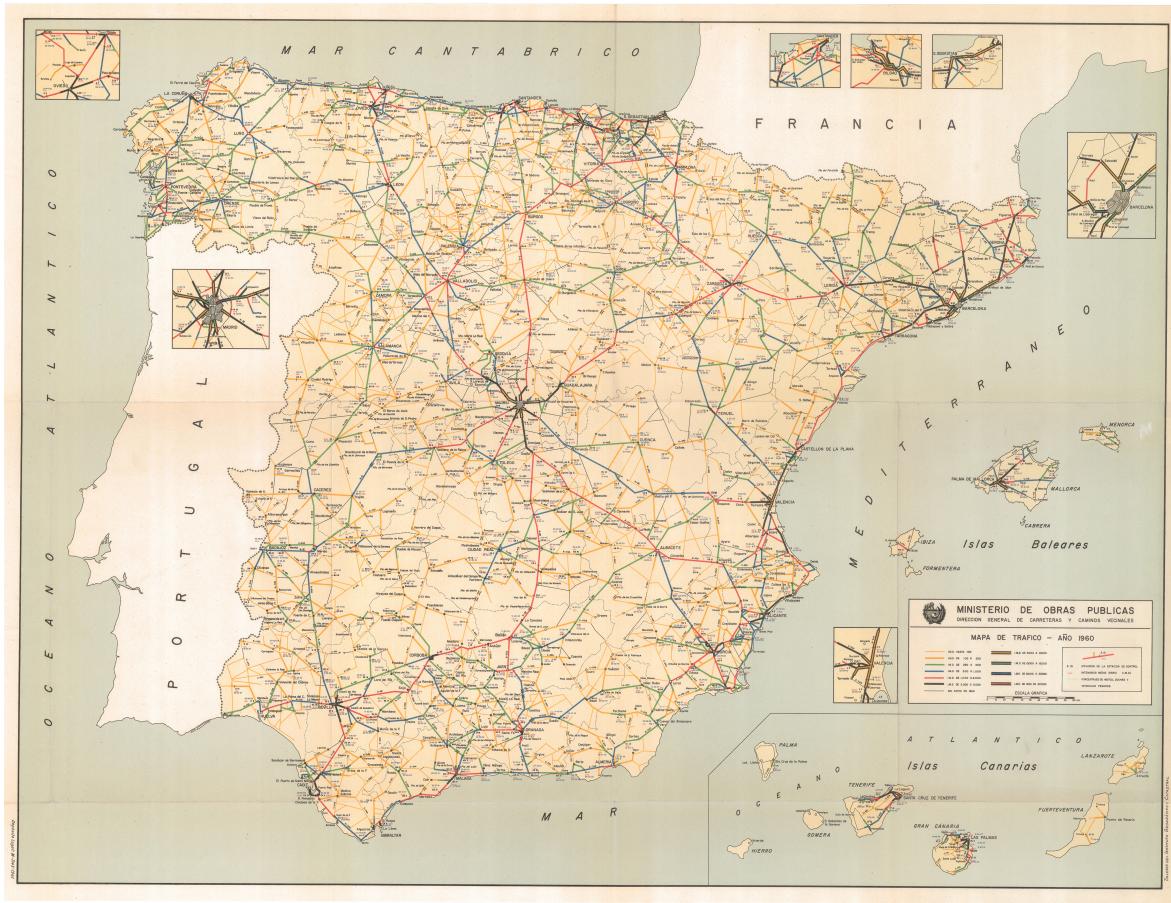


Figure A1: Traffic roads in mainland Spain, 1960

Table A1: 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	0.00	0.00	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.

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