

Branching Out: The Role of Selection in Bank Branch Entry and Economic Growth

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Abstract

Banks expand their branching networks in areas that subsequently exhibit economic growth. But in which direction does causality run? I exploit variation in completion across planned bank branches to disentangle selection and treatment effects of branch entry on local growth. Areas where a bank planned to open a branch, but did not, exhibit higher growth than similar areas (reflecting a selection effect). However, locations where a bank opened a branch only slightly outgrow locations where a bank planned to open a branch but did not (treatment effect). Both effects are limited to the immediate geographic vicinity of proposed branches. These findings contrast with previous studies reporting positive treatment effects of branch entry and instead emphasize banks' skill in selecting locations poised for growth.

Keywords: Bank branches, economic growth, satellite data, financial development

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1 Introduction

What is the relationship between financial development and economic growth? Whereas [Schumpeter \(1969\)](#) argues that financial systems promote growth, [Robinson \(1952\)](#) contends that their development merely anticipates growth but does not cause it. Motivated by this theoretical tension, a large literature beginning with the seminal work of [Jayaratne and Strahan \(1996\)](#) uses the staggered implementation of bank branching deregulation across U.S. states to argue that financial market development causes increased economic growth. Although this approach was innovative, state-level shocks may be too coarse to reflect subtleties in the relationship between branch entry and growth. Another body of literature (e.g., [Petersen and Rajan \(2002\)](#), [Degryse and Ongena \(2005\)](#)) shows that the geographic distance between lenders and borrowers is related to the terms and availability of lending, which suggests that branch entry may have stronger effects nearer to entering branches. Thus, state-level shocks may be insufficient to assess both the economic magnitude of these effects and the degree to which they are localized. Investigating the impact of branching expansion therefore calls for more granular data in terms of both treatment (branch openings) and economic outcomes. In this paper, I exploit variation in completion across planned branches and proxies for local growth to show that banks establish branches where they expect greater growth (selection) but do little to contribute to this growth by branching there (treatment).

My novel identification strategy uses granular variation in the completion of planned branches to account for location selection by banks.¹ The branching deregulation literature relies on broader state-level shocks but may still suffer from selection bias due to the endogeneity of a state’s choice to deregulate ([Kroszner and Strahan \(1999\)](#), [Freeman \(2002\)](#)). Moreover, it does not address banks’ decisions of where to establish a branch *within* a particular state, further obfuscating the local treatment and selection effects of branch entry. To disentangle these two effects, I use data from the Corporate Application Search (CAS) of the Office of the Comptroller of the Currency (OCC). The data report where OCC-regulated banks apply to establish a branch, regardless of whether the bank ultimately opens the branch. Roughly 90% of the branch establishment applications are fulfilled (i.e., the bank opens the proposed branch), but a small subset of the applications are unfulfilled “usually...due to delays in construction” ([OCC Licensing Office \(2023\)](#)). This anecdotal evidence suggests that variation in completion of branches across applications is largely idiosyncratic.

¹For anecdotal evidence that banks select branch locations based on their expectations for the local economy see, e.g., <https://www.wsj.com/finance/banking/jpmorgan-chase-bank-branches-expansion-cc7973dc>

Furthermore, the stringent application requirements, high branch completion rates, and short application timeline (see Section 2.1) are all evidence that application credibly signals a bank’s intent to open a branch. Thus, locations where a bank planned to open a branch (but did not) are compelling counterfactuals for locations where a bank did open a branch, allowing me to isolate the treatment effect of branch entry. These locations also serve as benchmarks to quantify the selection effect: how outcomes differ between locations where a bank planned to establish a branch (but did not) and locations where no bank planned a branch. This identification strategy is similar to others that rely on variation in fulfillment of a planned intervention to study otherwise endogenous treatments (e.g., [Baum-Snow \(2007\)](#) studies the effect of highways on suburbanization; [Bernstein \(2015\)](#) studies initial public offerings and innovation).

I apply my branch-level identification strategy to study the effects of branch entry on three local economic outcome variables: nighttime light emissions (“night lights”), new business registrants, and small business lending amounts. Light emissions are an especially suitable proxy due to their granularity and strong support in the development literature ([Henderson et al. \(2012\)](#)): Satellites record differences in light intensity over areas that are smaller than one square kilometer, allowing me to test for effects in the immediate proximity of proposed branches and to assess the extent to which these effects are localized. [Henderson et al. \(2012\)](#) demonstrate the usefulness of night lights as an economic proxy and argue that where GDP (Gross Domestic Product) estimates are unreliable, an optimal estimate of economic growth is a roughly equal-weighted blend of conventional GDP growth and growth predicted by changes in light emissions. I further validate the proxy by demonstrating that U.S. county-level changes in night lights positively correlate with changes in GDP per capita and confirming that my results are qualitatively similar across all three outcome variables. These proxies are better suited to evaluate branch-level effects than conventional measures, like GDP, that are not available at a sub-county level and may suffer from manipulation ([Martínez \(2022\)](#)) or poor data quality ([Deaton and Heston \(2010\)](#)).

I use the branch application data reported in the CAS within a difference-in-difference framework to disentangle the selection and treatment effects of branch entry on local growth. In particular, I modify the [Callaway and Sant’Anna \(2021\)](#) approach to compare, within the same application year, locations where a bank opened a branch (i.e., treated units) with locations where a bank planned to open a branch, but did not (i.e., control units). To estimate selection effects, I compare locations where a bank planned to open a branch, but

did not, to other locations a bank might have plausibly chosen. Existing post offices are suitable reference points around which to measure changes in light emissions for estimating this selection effect; post office presence indicates that there is some degree of economic activity nearby, making their locations more realistic candidates for banks to open a branch than randomly selected locations. In 2021, there was even a postal banking pilot program launched to allow customers to cash checks at certain post office locations ([Anthony \(2024\)](#)), further suggesting the similarity in location criteria for bank branches and post offices.

I first estimate the effects of branch entry on night lights within five kilometers of the proposed branch. I find a strong and statistically significant selection effect; locations where a bank planned to open a branch, but did not, exhibit a 3.1 percent increase in light emissions seven years after application compared to post offices with similar pre-trends in light emissions. The average annual selection effect is 2.4 percent, roughly four times the average annual treatment effect (0.6 percent). Back-of-the-envelope calculations suggest that the selection effect corresponds to an economically substantial increase in seven-year per capita GDP growth of roughly 0.24 percentage points, versus a trivial treatment effect of only 0.07. These results indicate that banks open branches where there will be subsequent economic growth but that the opening of the branch itself does little to increase growth.

As an alternative localized measure of economic activity, I also consider new business registrants in the proposed branch's ZIP code as an outcome variable. The selection effect is again large and statistically significant in each event year. Compared to ZIP codes with comparable pre-trends in new business formation, ZIP codes where a bank planned to open a branch (but did not) see a 5% increase each year in new business registrants, measured as a fraction of the number of registrants in the year before application. On the other hand, I find no evidence of a treatment effect of branch entry on new business formation rates. Banks choose to open branches in ZIP codes that will see an increase in new businesses, but they don't cause that increase by branching there.

These results are internally consistent but seemingly contradict existing empirical evidence (e.g., [Jayaratne and Strahan \(1996\)](#), [Chava et al. \(2013\)](#), [Butler and Cornaggia \(2011\)](#)) documenting positive effects of financial development on economic outcomes. To reconcile this apparent disparity, I consider whether branch entry increases access to capital or simply precedes it by using ZIP code-level data on the dollar value of loans issued to borrowers in the branch's ZIP code through the Small Business Administration's (SBA's) 7(a) program. Using this outcome variable again reveals a large positive selection effect that is about twenty

times the size of the treatment effect. ZIP codes where a bank planned to open a branch (but did not) see an increase in loans issued in each of the seven years after application that is 74% more than the increase in ZIP codes with similar pre-trends in lending. In other words, areas where a bank plans to open a branch go on to see increased lending regardless of whether the branch is completed. This finding contextualizes the other results; rather than suggest there is no effect of financial development on economic growth, it suggests that bank branches may not be a good measure of financial development. To that extent, it seems that the finance-growth literature may place undue emphasis on the role of bank branches.

Having documented consistent effects across outcome variables, I next assess the extent to which the effects are localized using the night lights data. I calculate changes in light emissions over concentric ring shapes of varying inner and outer radii around each proposed branch and repeat my tests for each specified distance (ranging from 5 to 40 km). Both the selection and treatment effects attenuate in the longer distance specifications, though the selection effect remains larger and statistically significant in each specification for at least three years post-treatment. Finally, I assess the plausibility of my results by considering the effect of another business entering an area, namely, Walmart stores. Like banks, Walmart is likely to expand its footprint in areas that are expected to experience growth. The total (selection+treatment) effect of Walmart store entry has a similar magnitude to the selection effect of branches, confirming the plausibility of the magnitudes I report in my main tests.

My paper is most directly related to the lengthy literature using state-level bank branching deregulation shocks to study the effect of financial development. [Jayaratne and Strahan \(1996\)](#) argue that intrastate branching deregulation led to large increases in economic growth, spawning a host of related papers.² [Chava et al. \(2013\)](#) find that intrastate branching deregulation led to an increase in innovation but that interstate deregulation had the opposite effect. [Amore et al. \(2013\)](#) and [Cornaggia et al. \(2015\)](#) argue that interstate deregulation improved innovation for capitally constrained firms and those proximate to entering banks. Rather than adopt a lens of deregulation like these papers, I use more granular branch-level shocks and data to study what happens to local economies when a branch opens. Moreover, I document compelling *selection* effects of branch entry, a novel finding that regulatory shock approaches miss.

²[Zdrojewski and Butler \(2024\)](#) apply the [Goodman-Bacon \(2021\)](#) critique to the intrastate branching deregulation setting, showing that the Two-Way Fixed Effect (TWFE) estimates of [Jayaratne and Strahan \(1996\)](#) are biased. The prominence of a TWFE approach in this literature calls into question the validity of many of its findings, especially for papers using these same shocks.

An important substrand (e.g., [Kroszner and Strahan \(1999\)](#), [Freeman \(2002\)](#)) of the branching deregulation literature documents the determinants of deregulation timing. The endogeneity of deregulation timing suggests a potential state-level selection bias that subsequent papers try to correct via improved counterfactuals. [Huang \(2008\)](#) uses pairs of contiguous counties across state lines to show that the effects of intrastate deregulation attenuate at a county-level. [Berger et al. \(2021\)](#) use synthetic controls to mitigate selection bias concerns, finding positive effects of deregulation only for states with geographically concentrated capital. These papers conclude that the previously reported effects of branching deregulation on growth may be overstated due to selection biases. I expand upon this notion by directly quantifying substantial *branch*-level selection effects and treatment effects that are practically zero. The deregulation experiments may not only suffer from a state-level selection bias but may also be affected by the cumulative biases of where banks choose to branch within a given deregulated state.

Several papers use branch-level variation to study other questions. [Gilje et al. \(2016\)](#) show that branch networks can transmit access to finance across county lines. [Nguyen \(2019\)](#) uses overlap in branch networks following bank mergers as an instrument to show that branch closures cause local business lending to decrease. Although my paper does not directly contradict these studies, it does seem to suggest that branch presence may be less important than this literature implies. Future investigations may use the variation in branch completion I introduce to explore lines of inquiry that directly address these studies.

This paper makes several contributions. First, my novel finding that branch entry is associated with strong selection effects and negligible treatment effects prompts a reevaluation of the lengthy and important literature related to bank branches. From a policy perspective, this finding should also dissuade attempts to artificially encourage branch establishment in underdeveloped locations to cause growth. Second, the granular data that I exploit allow me to show that both treatment and selection effects of branch entry are extremely localized. Third, the paper introduces a novel identification strategy that researchers may adapt to answer other questions about the effects of branch entry. Fourth, the paper further confirms the usefulness of night lights as an economic proxy through my validation exercises and the consistency in effects I find using night lights and other economic variables as outcomes.

2 Data and Background

I collect data on economic variables like population and GDP from the Bureau of Economic Analysis (BEA), and branch-level deposit data from the Federal Deposit Insurance Commission (FDIC)’s summary of deposits, which spans 1994 to 2023. ZIP code-level data on new business registrations come from the Startup Cartography project, which is introduced in [Andrews et al. \(2022\)](#), and cover years 1988 to 2016. SBA-7a loan data come from the Small Business Administration and cover years 1991 to 2024. From the Harvard Dataverse ([Blevins and Helbock \(2021\)](#)), I collect a list of post office locations and their establishment dates to use as reference points.

The bank branch applications that serve as the primary units of analysis in this paper are from the OCC’s Corporate Application Search (CAS) and span years 1990 to 2023. In the next two subsections, respectively, I discuss in detail the branch establishment application process and nighttime light emissions as an economic proxy ([Henderson et al. \(2012\)](#)).

2.1 OCC Branch Establishment Applications

The OCC regulates all nationally chartered banks, federal savings associations, and federal branches and agencies of foreign banks. These institutions control a sizeable portion of the branching footprint within the United States; as shown in Panel B of [Figure 1](#), roughly half of U.S. branches belong to OCC-regulated banks. Moreover, Panel C of the figure also shows that more than half of deposits are with OCC-regulated banks. These institutions are required to submit applications to the OCC to conduct many of their business operations, including branch establishments, closures, and relocations. This requirement results in a paper trail documenting locations where banks intended to establish a branch, even if this branch is not completed. The OCC makes the data on these applications available in its Corporate Applications Search, allowing me to observe variation in completion across a sizeable portion of planned branches in the U.S. ³

To establish a new branch, an OCC-regulated bank must navigate an involved multi-step process that is likely to dissuade the bank from applying to open branches that the bank does not seriously intend to establish. As part of the submission process banks “must publish notice of the proposed branch establishment or branch/main/home office relocation in a newspaper of general circulation in the community or communities in which the applicant

³https://apps.occ.gov/CAAS_CATS/

proposes to establish ... the branch” ([Office of the Comptroller of the Currency \(2023\)](#)). After such a publication, a 30-day public comment period opens, at which point any person can write the Director of Licensing at the nearest OCC office to provide comment on the proposed branch.

In addition to providing proof of public notification of their intent to establish the branch, banks must also verify that the branch establishment will not be in violation of either state or federal laws, and, in many cases, must submit legal opinions to support this argument. Additionally, the bank has to affirm that the branch will not create a prohibited management interlock, have an effect on the human environment (as specified by the National Environmental Policy Act), or affect any historical location protected under the National Historic Preservation Act ([Office of the Comptroller of the Currency \(2023\)](#)). If the proposed branch fails any of these criteria, the bank must identify how and, in some cases, provide a plan of rectification. In short, applying to open a branch requires banks to step through a series of hurdles involving public notification, premises suitability, and legal compliance. These institutional details of the application process suggest that banks are unlikely to apply to establish a branch in a location without first carefully researching it, establishing its suitability, and seriously intending to establish a branch there.

Each application to establish a new branch must provide a precise planned location. [Figure 2](#) shows an example record from the CAS database of a completed branch in Newberry, Florida. The record details the precise location of the branch as well as the beginning and end dates of the associated comment period and the days on which the application was received, approved, and made effective (i.e., the branch was opened). [Figure 3](#) is similar but instead shows an example of an application for which a branch was planned in Ocoee, Florida but was not completed.

[Figure 4](#) provides a flowchart of the application process to help visualize the common outcomes of applications, their frequencies, and the approximate timing between subsequent steps in the application. As indicated in the leftmost node, the CAS database provides data on 42,396 branch establishment applications. Subsequent nodes report the percentage of these applications that reach each step, and the nodes are scaled in proportion to the total number of applications received. For 96.6% of these applications (the next node), the first recorded step in the application process begins with receipt (and the rest seem to be data errors concentrated in the first year of data, 1990). After the OCC receives the application, it typically makes a decision within approximately a month. 92.9% of applications are received

and then approved, whereas only 0.1% (fewer than 100) are denied and another 2.7% are withdrawn before the OCC reaches a decision. Following approval, banks have 18 months to begin operations at the branch before the approval expires. As [Figure 4](#) indicates, 84.4% of the applications are approved and effective. 4.9% of applications are approved but eventually withdrawn, and 2.6% are approved but allowed to expire.

It is worth emphasizing that it is very rare for an application to be denied. Out of all branch establishment applications that have a documented decision from the OCC, 99.8% are approved. This fact suggests that banks diligently ensure compliance with all state and federal branching restrictions prior to application, bolstering the credibility of application as a serious signal of intent to branch. Although denied applications generate variation that is cleanly exogenous from the perspective of the establishing bank, they are of limited use to researchers because of how few of them there are. In this paper I therefore focus on the more common occurrence of an application that is either denied, withdrawn, or allowed to expire. In other words, my identifying variation comes from the applications represented in [Figure 4](#) by the “Denied” node, the “Expired” node, and the two “Withdrawn” nodes. For the sake of simplicity, I refer to these applications, and their corresponding branches, collectively as “unfulfilled”.

As demonstrated in [Figure 4](#), the OCC reaches decisions on most applications within roughly a month (typically a few days after the 30-day comment period has ended). After the OCC has approved an application, the bank has 18 months to begin operations at the specified location. If after 18 months the branch is not conducting business operations, then the bank must either apply for an extension, reapply, or forfeit their plans to establish a branch. This short required turnaround is a useful feature of the setting, as it makes it more likely that banks are already working on the arrangements needed to begin operations while the application is under review. Thus, banks are likely to have invested resources and effort into the branch, further bolstering my claim that application is a strong signal of intent to establish a branch. Moreover, the quick timeline means that local economic conditions around the branch are unlikely to have changed from the time the bank signaled its interest in establishing a branch to the time that the bank decides not to complete the branch.

In [Figure 5](#), I present the number of total applications to establish a branch each year (top panel) and the percentage of that year’s applications that go unfulfilled (bottom panel). The number of applications each year peaks in the late 1990s and generally decreases thereafter, with a substantial drop around the Great Financial Crisis in 2008. The percentage of

applications that are ultimately unfulfilled varies over time, with an average of roughly 10% across years. Although the unfulfilled percentage increased to 20% in 2009, this increase does not seem to be attributable to the recession when compared with the similar rate in 2011 or the even higher rate in 2015. Rather, the drop in applications overall following the financial crisis seems to have increased the volatility of the fraction of applications which are unfulfilled by way of reducing its denominator. Overall, the figure does not provide compelling evidence that the macro-economy affects whether branch establishment applications are fulfilled, which is reassuring for my identification claims. Similarly, [Figure 6](#) plots on a map of the continental U.S. the locations of the unfulfilled branches that are used in my main analysis and shows that these locations exhibit no obvious patterns.

In addition to the institutional details that strongly suggest that locations with unfulfilled branch establishment applications are suitable counterfactuals for locations where a branch is opened, I present corresponding empirical evidence in [Table 2](#). I compile three covariates as observed within five kilometers of each proposed branch and split the sample to compare the location of fulfilled and unfulfilled branches: *lights*, *deposits*, and *branches*. *lights* refers to nighttime light emissions, a variable that is discussed extensively in the next section. *branches* refers to the number of bank branches within five kilometers of the proposed branch, and *deposits* refers to the total value of the deposits held at these branches in 1994 dollars. I also compile county-level figures for the number of people employed in construction (*Constr.Employment*) and their total payroll (*Constr.Payroll*). For some of the variables, I also calculate percentage growth over a specified window relative to the application year. These variables are preceded by a Δ , with the window specified as a subscript. For instance, the mean value for $\Delta lights_{t-2,t-1}$ for fulfilled branches is -0.01, meaning that the night lights within five kilometers of these locations decreased 1 percentage point, on average, between years -2 and -1 relative to the year the branch was opened.

The left columns of [Table 2](#) report the number of observations, mean, and median of each of these variables for locations where a branch establishment application was fulfilled. The middle columns report the same for locations where a branch establishment application was unfulfilled. The rightmost columns report the mean difference (Fulfilled - Unfulfilled) of each variable across the two subsamples and its *p*-value. Overall, the table demonstrates remarkable similarity across the two location types; of the thirteen variables I examine, only the difference in $\Delta lights_{t-4,t-1}$, the percentage change in lights from year -4 to year -1 (relative to application), is statistically significant at a 10% level. In my main analyses, I

control for light level over all four pre-treatment years to mitigate this slight difference in trend.

The OCC cites construction delays as the usual reason for applications not being fulfilled, and I contend that the fulfillment of an application is therefore likely to be unrelated to expectations of future economic growth. One might argue, however, that construction delays occur as a result of increased demand for construction due to expectations of growth. The results of [Table 2](#) help to assuage this concern, as none of the differences in county-level construction employment or payroll growth variables is statistically significant. As the table shows, these differences are insignificant in the pre-treatment windows and in the two-year window ($[t - 1, t + 1]$) around the application. Individual applications being unfulfilled, then, does not seem to be a result of widespread changes in the local construction industry, supporting my argument that variation in the completion of planned branches is credibly idiosyncratic. Overall, locations with fulfilled branches are observably similar to those with unfulfilled branches, supporting my use of the latter as counterfactuals for the former.

2.2 Night Lights

2.2.1 Data Description

In the 1970s, the United States Air Force Defense Meteorological Satellite Program (DMSP) began using satellites to detect moonlit clouds via Operational Linescan System (OLS) sensors. Although the program’s intent was meteorological, the sensors also recorded light emitted by human activity. Each satellite circles the earth 14 times each day, registering a luminosity observation between 8:30 and 10:00 pm local time every day. The digital records of these observations are available dating back to 1992. Prior to releasing the data to the public, the National Oceanic and Atmospheric Administration’s (NOAA) National Geophysical Data Center pre-processes this data to remove daily observations affected by natural variation in light, including moon and sunlight, natural fires, and auroral activity ([Henderson et al. \(2012\)](#)). The data center then averages data from each orbit over time to create satellite-year level datasets, which are publicly available through Google Earth Engine for years 1992 through 2013.

The data for each satellite-year are a grid that records light intensity at a 30 arc-second granularity. [Henderson et al. \(2012\)](#) contextualize this measurement by noting that this grid results in pixels that are approximately 0.86 square kilometers at the equator. Further

north or south, the arc-second grid tightens so that these pixels become even smaller. In particular, [Henderson et al. \(2012\)](#) note that pixel size varies in proportion to the cosine of latitude so that a pixel in London (51.5 degrees north) is roughly .53 square kilometers. A similar calculation with a U.S. reference point shows that a pixel in Washington, D.C. (38.9 degrees north) will be roughly .67 square kilometers. This data therefore have a much finer geographic granularity than traditional economic data, allowing the researcher to examine more localized units of observation. However, limitations in the technology employed to capture these images cause the data to present some unique challenges for an economist.

First, observations taken from the same satellite in different years are not immediately comparable due to the changing position of the satellite in its orbit. Thus, time-series analysis requires an intercalibration across separate satellite-years. I calibrate individual pixels following [Elvidge et al. \(2009\)](#) and take the mean luminosity of these pixels over my area of interest from the given annual composite. For years when there are multiple satellites with available data, I calculate the luminosity in that year as the average value recorded by the satellites.

Another difficulty with DMSP data is that its sensors can only score each pixel from zero (darkest) to sixty-three (brightest). As a result, variation in extremely bright or dark areas may not be fully observable, limiting the number of locations that are useful for making economic inferences. I therefore discard extremely bright areas (with pre-treatment luminosity greater than fifty-three) and dark areas (with pre-treatment luminosity less than ten), from my analyses to examine only areas where variation in light is observable.

In [Figure 7](#), I demonstrate visually how I calculate lights around an individual branch. It depicts an aerial view of the Newberry, Florida branch from [Figure 2](#), indicated by the black dot at the center. In my primary analysis, I calculate the average luminosity of pixels within five kilometers of each branch (the pink area in the figure). In secondary analyses, I then consider outer rings that are farther away from the branch. For example, the blue area in the figure depicts a ring with an outer radius of ten kilometers and an inner radius of five kilometers. Using this approach, I can explore the degree to which the treatment or selection effects are localized. I do so using rings defined by inner and outer radius pairs of 0 and 5 km, 5 and 10 km, 10 and 20 km, and 20 and 40 km.

2.2.2 Economic Interpretation

The main advantage of the night lights data in my setting is its geographic granularity, as discussed in the previous subsection. This advantage comes with the disadvantage of not being readily interpretable in terms of standard economic quantities. [Henderson et al. \(2012\)](#) show that in developing countries, the information contained in this proxy can improve estimates of economic measures like GDP. In this section, I expand upon these results to argue that night lights are a useful proxy for local economic activity within developed countries.

Because my paper primarily deals with the measurement of economic growth across localities, I first analyze the long-run cross-sectional relationship between light growth and economic growth across U.S. counties. I calculate growth rates in GDP and in lights for each county over the period when the data overlap, 2001 to 2013. [Figure 8](#) plots each county in blue with its per capita GDP growth over this time against its growth in lights. There is a clear positive relationship between the two, captured visually by the best-fit line, the slope of which is a statistically significant .21. This coefficient means that on average, a one percentage point increase in luminosity between 2001 to 2013 corresponds to a 21 basis point increase in GDP. I label several counties that are clear visual outliers and note that these are all beneficiaries of the U.S. shale boom in this period. I therefore also run a median regression to reduce the influence of these points and obtain a more modest but still significant coefficient estimate of .08. These coefficients provide a rough way to translate effects on night lights around bank branches in to economic quantities; a one percentage point change in light growth over a long period corresponds to either a 21 basis point or 8 basis point change in per capita GDP growth over the same period, depending on which model is preferred. In my back-of-the-envelope calculations, I employ the 8 basis point estimate as it seems to be less affected by outliers.

In addition to the long-run relationship demonstrated in [Figure 8](#), I also confirm that lights are a useful proxy in higher-frequency regressions. For each year, I run annual cross-sectional regressions of county-level real GDP growth on county-level light growth and an intercept. I then average the estimated coefficients across years. Panel A of [Table A.1](#) reports the results using GDP growth as the dependent variable, and Panel B reports the same but uses GDP growth on a per capita basis. The first column of Panel A indicates that a one percentage point growth in light emissions corresponds to a 3 basis point increase in GDP growth in the same year. This result is not simply attributable to increasing population,

however, as the first column in Panel B reports a statistically significant per capita effect of two basis points as well. Moreover, the statistically significant and economically meaningful coefficients in the second and third columns of Panel A show that increases in light emissions are also predictive of a county’s future GDP growth (although not significant on a per capita basis).

Although unorthodox, the use of night lights as a proxy for economic activity has a strong intuitive basis, as nearly all consumption activities at night require light emissions. [Henderson et al. \(2012\)](#) build on this intuition to show that it is a useful economic proxy in developing nations. In [Figure 8](#) and [Table A.1](#), I further validate the usefulness of the proxy at a subnational (county) level to conclude that light emissions are a promising outcome variable to use in studying the relationship between bank branch entry and local economic growth.

3 Methods

This paper employs an event-study approach to identify treatment and selection effects of branch entry. I focus on the first time an application is submitted to establish a branch at a particular location. For analyses using night lights as an outcome variable, I define a location as a latitude/longitude coordinate pair, and for each of the other variables, I use a ZIP code-level definition. As noted in previous sections, I consider applications and their respective branches to be unfulfilled if the proposed branch is not completed. In the CAS database, unfulfilled applications are those which are marked as withdrawn, denied, or allowed to expire.

Most approaches consider treated units and estimate a counterfactual for them consisting of untreated units. In my case, however, the number of treated units (locations where a bank opens a branch) far exceeds the number of untreated units (locations where a bank planned to open a branch, but did not) each year. [Table 1](#) shows the number of unfulfilled applications that are used in my primary analysis, using night lights as an outcome variable, broken out by the year the application was submitted. There are roughly 32 unfulfilled and 520 fulfilled applications in the average year. As a result, matching locations where a bank opens a branch to locations where a branch was planned but not completed is likely to produce unreliable counterfactuals. I instead match places where a branch is unfulfilled to a counterfactual consisting of places where a branch is opened. The difference in outcome

between this counterfactual and the location of unfulfilled branches is thus my estimate of the treatment effect of branch entry on the untreated locations.

Similarly, I identify the selection effect at each location by constructing a counterfactual for each location where a branch is unfulfilled that uses a pool of comparison units that are not conditioned on branch entry plans. The difference in outcome between the location of the unfulfilled branch and this counterfactual is my estimate of the selection effect: the difference in outcome that is associated with branch location selection by banks. For ZIP code-level variables like new business registrants or SBA-7a loan amounts, I simply use the set of other ZIP codes as comparison units to find the selection effect. Using night lights as an outcome variable is less straightforward, however, because randomly selected geographic coordinates are likely to be different from those in the choice set of locations for entering banks. For instance, a point in a remote and uninhabited area is probably not useful for comparison. So, selecting comparison locations for this analysis merits careful consideration.

Intuitively, good comparison points will be accessible to a nearby population maintaining a reasonable level of economic activity. To identify such locations, I compile a list of post offices that were extant in 1991, prior to the start of my night lights data. With their exact geographic coordinates, I calculate the light emissions around post offices in exactly the same manner as I do for bank branches (see [Figure 7](#)). The first branch entries I consider using the lights data are in 1996, so by using post offices extant in 1991 I am assuaging concerns that these locations may be subject to entry timing by the USPS. The presence of a post office ensures that there is a baseline level of nearby economic activity, making these locations more suitable for comparison than randomly selected geographic coordinates. In fact, during the United States Postal Savings Program (1911-1967) some post offices in the U.S. offered banking services ([Schuster et al. \(2019\)](#)). More recently, in 2021 there was a postal banking pilot program launched that allowed customers to effectively cash business and payroll checks at certain post office locations ([Anthony \(2024\)](#)).⁴ Although customers seem to have had little interest in banking with the USPS, the plausibility of the program and its historical context speak to the suitability of using post offices as comparison locations for bank branches.

I employ a staggered difference-in-differences approach to estimate both the treatment and selection effects of branch entry. A key advantage of such a setting is that the staggered nature of interventions reduces concerns that there might be a single event that

⁴<https://www.cato.org/blog/no-customers-no-success-postal-banking-failure-exposed>

confounds treatment by simultaneously affecting all of the observations. However, a burgeoning literature has pointed out that some commonly used empirical approaches in a staggered difference-in-differences setting may generate biased estimates of treatment effects (Goodman-Bacon (2021), Sun and Abraham (2021), de Chaisemartin and D’Haultfœuille (2020)). A major reason for concern is that some approaches, like two-way fixed-effect regressions, implicitly compare treated units to a control group that includes previously treated units. With dynamic treatment effects, this comparison can result in estimates that may not only be biased but possibly even the wrong sign (Goodman-Bacon (2021)).

To avoid making these inappropriate comparisons to previously treated units, Callaway and Sant’Anna (2021) propose a methodology that constructs counterfactuals for each treatment cohort using units that are never and/or not yet treated. I adapt this approach to my setting by making my comparisons within application cohorts (application years). That is, I estimate the treatment effect of branch entry by comparing the outcomes of places where branches are completed in a given application cohort with those of places where a branch was planned but not completed in the same application cohort. The Callaway and Sant’Anna (2021) method is computationally inexpensive and allows the researcher to make informed choices about how to weight estimated effects across cohorts and event-time. It allows for an intuitive approach to including covariates and ensures that these covariates do not affect estimates by themselves changing as a result of the treatment. The approach also allows for visual confirmation that there are no pre-treatment divergences in outcome across treated and control units, lending crucial support to the parallel trends assumption under which the estimates are unbiased.

The following discussion formalizes my adaptation of the approach employed by Callaway and Sant’Anna (2021). To make the intuition of the approach clear, I discuss what the estimates and assumptions look like in a case without covariates. Then I describe intuitively how inclusion of covariates affects the estimation.

3.1 Adaptation of Callaway and Sant’Anna (2021)

Let G index the year that a bank applies to open a branch at a particular location. U is an indicator variable equal to one if the branch at that location is unfulfilled. I adopt a potential outcomes framework: $Y_t(g, u)$ is what the outcome would be at time t if the location receives an application in year g and has $U = u$. I define the Average Treatment

effect on Untreated units (ATU - analagous to ATT of [Callaway and Sant'Anna \(2021\)](#)):

$$ATU(g, t) = \mathbb{E}[Y_t(g, 0) - Y_t(g, 1)|G = g, U = 1] \quad (1)$$

The above is the expected difference in outcome at time t of a place receiving an application in year g between whether the branch is fulfilled versus unfulfilled. The conditional expression in the above simply requires that the application be unfulfilled and that the application occur in year g .

The most important assumption on which my approach relies is analogous to the parallel trend condition required by [Callaway and Sant'Anna \(2021\)](#). I assume that the following holds for each application year group g and each time $t \geq g$:

$$\begin{aligned} & \mathbb{E}[Y_t(g, 0) - Y_{t-1}(g, 0)|G = g, U = 1] \\ &= \mathbb{E}[Y_t(g, 0) - Y_{t-1}(g, 0)|G = g, U = 0] \end{aligned} \quad (2)$$

The above states that, in expectation, if an unfulfilled branch from application year g had instead been completed, the outcome at its location would have changed the same amount between $t - 1$ and t as it did at the locations where the branch was in fact completed. Under this assumption (and more mundane assumptions analogous to those of [Callaway and Sant'Anna \(2021\)](#) that I omit), $ATU(g, t)$ can be identified by:

$$\begin{aligned} ATU(g, t) = & \mathbb{E}[Y_t(g, 0) - Y_{g-1}(g, 0)|G = g, U = 0] \\ & - \mathbb{E}[Y_t(g, 1) - Y_{g-1}(g, 1)|G = g, U = 1] \end{aligned} \quad (3)$$

Abusing notation slightly, the above can be simply estimated by the appropriate difference in differences of sample means:

$$\widehat{ATU}(g, t) = \bar{Y}_t(g, U = 0) - \bar{Y}_{g-1}(g, U = 0) - (\bar{Y}_t(g, U = 1) - \bar{Y}_{g-1}(g, U = 1)) \quad (4)$$

That is, a simple estimate of the cohort-time treatment effect is the difference in differences in average outcome for locations where a branch is fulfilled versus unfulfilled. Estimation of selection effects is similar but uses locations where a branch was planned but not completed in the first difference and in the second uses other suitable locations regardless of whether a bank applied to open a branch there.

3.2 Inclusion of Covariates

In this section, I build on the framework of the previous section to provide an intuitive explanation of how I modify the approach to account for covariates. Callaway and Sant’Anna (2021) propose an inverse probability weighting (*ipw*) approach that essentially reweights control units so that they are more similar to the treated units in terms of the covariates. Let U_g be an indicator variable equal to one when a location that received an application in year g is unfulfilled and F_g be an indicator variable equal to one when a location that received an application in year g is fulfilled. $p_g(X)$ is the probability that a location that received an application in year g is unfulfilled, conditional on the covariates, X . Then, the $ATU(g, t)$ s can be estimated by:

$$ATU_{ipw}(g, t) = \mathbb{E} \left[\left(\frac{\frac{p_g(X)F_g}{1-p_g(X)}}{\mathbb{E} \left[\frac{p_g(X)F_g}{1-p_g(X)} \right]} - \frac{U_g}{\mathbb{E}[U_g]} \right) (Y_t - Y_{g-1}) \middle| G = g \right] \quad (5)$$

The above is similar to Equation 3 in that the product of differences it contains can be rewritten as a difference-in-differences. However, the first difference is reweighted to emphasize fulfilled units in each application year that are most like the unfulfilled units of that application year. To see this point, consider that as $p_g(X)$ increases for a given unit, $\frac{p_g(X)F_g}{1-p_g(X)}$ also increases, assigning more weight to that unit. The division by the expected value of this term, $\mathbb{E} \left[\frac{p_g(X)F_g}{1-p_g(X)} \right]$, ensures that across fulfilled units these weights sum to one.

To estimate Equation 5, one simply replaces the given expectations with their sample means and replaces $p_g(X)$ with $\widehat{p_g(X)}$, which I estimate using a logistic regression approach following Callaway and Sant’Anna (2021). For statistical inference, I generate standard errors following a within cohort bootstrapping procedure. Specifically, if there are u unfulfilled units in a particular application year, I sample (with replacement) u of the unfulfilled units from that application year, doing so for each application year. This sample is a single bootstrap draw, for which I calculate each of the relevant statistics. I repeat this bootstrap 1000 times, allowing me to calculate the bootstrapped standard errors for each of the statistics I report. Performing other variations of this specific procedure does not generate materially different inferences.

4 Results

4.1 Main Results

In this section, I use variation in the completion of planned bank branches to disentangle the selection and treatment effects of branch entry with respect to three main economic variables: night lights observed within five kilometers of the proposed branch, the number of new business registrants within the branch’s ZIP code, and total SBA-7a loan amounts to borrowers within the branch’s ZIP code. For each variable, I require four years of pre-treatment data and eight years of post-application data, including the application year. I employ my adaptation (described in the previous section) of the approach in [Callaway and Sant’Anna \(2021\)](#), controlling for the outcome variable in each of the four years pre-treatment.

For consistency and computational simplicity, I restrict the pool of comparison units used in estimating the selection effect to have the same size as that used in estimating the treatment effect. To decide which units to keep in the selection pool, I rank them on their similarity to the unfulfilled units’ level of the outcome variable pre-treatment. Then I keep the closest n observations, where n is the number of units with fulfilled applications in that year.

In each of the plots, I calculate event-time effects by weighting the estimated cohort-time treatment effects ($ATU(g, t)$) by the number of unfulfilled units. Thus, my scheme weights my estimates in proportion to how much of my experimental variation occurs in each cohort. A simple equal-weighting scheme across cohorts gives similar results.

4.1.1 Night Lights

To explore the relationship between branch entry and economic growth, my first analysis uses nighttime light emissions within five kilometers of the proposed location as an outcome variable. [Table 1](#) lists the number of unfulfilled branches used in this analysis from each year, along with the number of fulfilled branches in the pool of comparison units. With 349 unfulfilled branches occurring over eleven years, I have substantial variation to test for an effect.

In [Figure 9](#), I show the estimated annual selection effects (blue) and treatment (green) of branch entry on the log-level of light emissions. Neither effect shows evidence of a trend pre-

treatment, partly as a result of controlling for the pre-treatment outcomes. Post-treatment, a strong selection effect immediately manifests and increases over time. By year seven, the selection effect is .031, meaning that locations where a branch was planned but not completed grow their night lights by 3.1 percentage points more after seven years compared to post offices that saw similar growth prior to application. The estimated selection effect in each year is positive and statistically significant at all levels, with an average annual effect of .024.

The treatment effect, by comparison, is much smaller. Only event year one has a statistically significant treatment effect, and the average effect across years is only .006, or .6 percentage points. In year seven, the effect is .0091 or .91 percentage points but statistically insignificant. Across years, the average selection effect is roughly four times that of the treatment effect. Though crude, the coefficients from [Figure 8](#) provide one way to contextualize these effects in terms of more standard economic figures. Using the median-regression estimates, the year-seven selection effect of .031 corresponds to a $3.1 \times .08 = .24$ percentage point increase in the seven-year growth rate in per capita GDP. Similarly, the year-seven treatment effect of .0091 corresponds to a $.91 \times .08 = .07$ percentage point increase in the seven-year growth rate in per capita GDP. In terms of economic magnitudes, the selection effect is large, whereas the treatment effect is negligible.

The estimates presented in [Figure 9](#) are valid under the assumption that banks do not choose to not complete planned branches as a function of expected local economic growth. [Section 2.1](#) makes the argument that this assumption is credible, but it is worth considering what it means if the assumption does not hold. Intuitively, it seems unlikely that failure to complete a branch would be positively correlated with expectations of economic growth; stronger local growth will probably lead to greater demand for capital to finance new ventures, increasing bank profitability and therefore incentivizing the bank to complete its branch. On the other hand, one might imagine that banks choose not to complete planned branches in locations where their expectations of economic growth worsen after the submission of the application. In this sense, if unfulfilled branch applications are related to expectations of economic growth, it is likely in the direction of mechanically overestimating the treatment effect and underestimating the selection effect. Thus, in the presence of bias, my results would be an *understatement* of the true selection effect and an *overstatement* of the true treatment effect. Moreover, the positive and strong selection effect makes this bias unlikely; if branches are unfulfilled due to a substantial downgrade in expectations for the local economy, we would expect to see a negative selection effect.

The finding that branch entry has an economically small and statistically weak effect on light emissions growth seems to run counter to prior literature. It has emerged as a stylized fact of the finance-growth nexus that access to finance improves economic outcomes (Jayaratne and Strahan (1996), Chava et al. (2013), Butler and Cornaggia (2011), many others), so why do I fail to find an economically compelling effect in this setting? One possibility is that the treatment effect of opening a branch only manifests in areas with limited access to capital ex ante. To test this hypothesis, I calculate the dollar value of deposits within 20 km of each proposed branch and repeat the procedure limiting the sample to observations in the bottom quartile of that value for each year. Figure A.1 shows the treatment effect in these areas is again not statistically different from zero. One might also wonder whether my findings are an artifact of my weighting scheme or decision to control for the outcome pre-treatment. For robustness I therefore test two additional specifications. Figure A.2 repeats the exercise using an equal-weighting scheme across cohorts, rather than weighting each cohort by the number of unfulfilled applications in that year, and finds essentially the same results. Figure A.3 repeats the exercise without controlling for pre-trends, obtaining essentially the same estimates. However, pre-trends are clearly not parallel for the selection effect in Figure A.3, and arguably not parallel for the treatment effect. This visual both confirms the plausibility of the main estimates from Figure 9, and emphasizes the need to control for pre-trends by reweighting the control units to emphasize those with a trajectory similar to that of the unfulfilled units.

Given the robustness of these estimates across the specifications in Figures A.1-3, I turn to another plausible explanation for the contradiction between my work and prior research: night lights may simply be a poor proxy for economic growth, making detection of an effect difficult. Considering how thoroughly established night lights is as a proxy in the development literature, this explanation seems unlikely. Nevertheless, in the next subsection I consider a more orthodox economic variable, ZIP code-level new business registrants, and find similar results.

4.1.2 New Business Registrants

My interpretation of the results from Section 4.1.1 is only as useful as my proxy for economic activity, namely nighttime light emissions. Although like prior literature (e.g., Henderson et al. (2012)), I document the usefulness of the proxy, there might persist con-

cerns that the economically null treatment effect I observe is due to my choice of proxy rather than a lack of a true economic effect. Therefore, I extend the results by considering a more conventional economic proxy, the number of new business registrants, which is available at a ZIP code level courtesy of the Startup Cartography Project ([Andrews et al. \(2022\)](#)). In [Table A.2](#), I document the number of ZIP codes where branches are planned but not completed each year, along with the number of ZIP codes that are available to use as comparison units. Once again, I have substantial variation coming from 513 planned but not completed branches dispersed across 18 years.

[Figure 10](#) presents the annual selection and treatment effects associated with a branch entering a ZIP code on the log of its new business registrations and largely tells the same story as [Figure 9](#). I find strong evidence of a selection effect, with all but one of the individual yearly effects' being statistically positive. The effects are large; the average effect across years is .05, meaning that in the seven years following application, ZIP codes where a branch was planned but not completed see an average of 5 percent more new business registrants than other similar ZIP codes, as a portion of the number in the year before application. As was the case in the night lights analysis, however, there is essentially no evidence of a treatment effect. None of the individual yearly effects are statistically significant, and two of the eight are in fact negative. The average treatment effect across years is a statistically insignificant .002, or .2 percent. Across years, the average selection effect is 25 times that of the average treatment effect. In short, using ZIP code-level data on new business registrants, I again find no evidence of a treatment effect of branch entry on economic growth. Taken together, the results of [Figure 10](#) assuage concerns that effects reported in [4.1.1](#) are simply attributable to a poor choice in outcome variable.

Sections [4.1.1](#) and [4.1.2](#) show that banks selectively enter into high-growth areas but are not themselves a significant cause of local growth. If bank branches are a reasonable metric of financial development, then these results seem to conflict with the battery of empirical papers that document positive effects of financial development on growth. This apparent disparity motivates my next analysis, which tests whether bank branch entry increases access to capital (SBA-7a loan amounts) and whether areas selected by banks receive greater capital even when the branch is not realized.

4.1.3 SBA-7a Loan Amounts

In this section, I explore whether branch entry impacts the amount of capital available for small businesses in the entered area by using ZIP code-level data on total SBA-7a loan issuance amounts. For ZIP codes where I have this data, [Table A.3](#) reports the number of ZIP codes where a branch was planned but not opened and the number of comparison ZIP codes within each calendar year. Using this dataset, I have about half of the number of unfulfilled branches as were available in the prior analyses because there were many ZIP codes that had values of zero in this data, making them unsuitable for use as logged values. Nevertheless, I have substantial variation stemming from 271 unfulfilled branches, with roughly ten times as many comparison ZIP codes, staggered over 22 years.

[Figure 11](#) shows the event-year selection and treatment effects of branch entry on the log of the total loan amounts issued within a ZIP code. The results are similar to those derived from night lights or new business registration data; selection effects are strikingly large and significant, with much weaker evidence of a treatment effect. The average selection effect estimate across years is .739, which is roughly 13 times the size of the average treatment effect of .056. In the average post-treatment year, branch entry caused the dollar value of SBA-7a loans to borrowers in the entered ZIP code to increase by 5.6% of the pre-treatment value, although this effect is not statistically significant. On the other hand, ZIP codes where branches were planned but not completed see 74% greater loan amounts, as a fraction of the pre-application value, in the years following application than other ZIP codes with similarly trending loan amounts. Although the overall treatment effect is significant, only one of the yearly treatment effects is significant, and the effect is smaller and less statistically reliable than the selection effect.

The results of [Figure 11](#) help to resolve the seeming contradiction between the previously reported results and the extant literature and so deserve special discussion. Not only is the selection effect dominant in terms of economic growth, but it even dominates the main channel through which one might expect an effect, increased access to capital. This result suggests that areas with promising economic futures are likely to receive substantial loans *regardless of branch entry*. Therefore, my previous results do not declare a null effect of access to finance on economic growth but rather suggest that bank branches are not actually a good measure for local access to capital. As pertains to the effect of access to finance on economic growth, the literature's emphasis (e.g., [Jayaratne and Strahan \(1996\)](#), [Chava et al.](#)

(2013), Amore et al. (2013), etc.) on bank branches may be misplaced.

4.2 Effect Localization

My paper is most closely related to the seminal work of Jayaratne and Strahan (1996) and the family of papers it has spawned (Jayaratne and Strahan (1998), Amore et al. (2013), Chava et al. (2013), Cornaggia et al. (2015), etc.), which use state-level regulation changes as plausibly exogenous shocks to branch presence. These papers all report positive effects of loosening branching restrictions, whereas my paper finds only a minimal effect for branch entry. The disparity between these results highlights an important value-add of both my identification strategy and my data: the ability to study *local* effects of branch entry rather than aggregate state-level changes in response to deregulation. I uncover two novel results: that banks select to branch in locations that are prepared for growth and that the branches themselves contribute little to this growth. In this section, I further exploit the geographic granularity of the night lights data to explore the extent to which these results are localized.

Around each branch, I calculate the mean luminosity observed within a radius of 5 km and then over rings with inner and outer diameter pairs of 5 and 10 km, 10 and 20 km, and 20 and 40 km. Figure 7 shows an aerial view of an example branch, and illustrates how these areas are drawn. The black dot at the center of the figure represents the branch, the pink-shaded area represents the innermost circle of 5 km, and the blue-shaded area represents the ring with an inner radius of 5 km and an outer radius of 10 km. For each of these areas, I calculate the mean luminosity observed across pixels, doing so for both fulfilled and unfulfilled branches as well as post office locations (for comparison, as in Section 4.1.1). Using this approach, I can explore the degree to which the treatment and selection effects are localized.

Figure 12 shows the results. The top left panel recreates Figure 9 using a simple circle of radius 5 km, and then from left to right, the remaining panels repeat the exercise using increasingly expansive rings, as described in the previous paragraph. Several facts emerge from this exercise. First, the selection effect attenuates substantially as the distance increases, although it remains statistically significant and positive for at least three event-years in each specification. This result indicates that banks are selective in choosing precise locations, rather than general regions, that are likely to experience growth. Although the treatment effect is much smaller by comparison, it too attenuates in the longer distance

specifications. In addition to being economically small, the effect of branch entry seems to be limited to the area in the immediate vicinity of entering branches. Taken together, the spatial distribution of the two effects emphasizes both banks' skill in location selection and the importance of employing a granular level of analysis to uncover subtleties of the finance-growth relationship.

Overall, the results of [Figure 12](#) provide a novel insight within the literature on the bank branching and growth nexus: studies using aggregate (i.e., state-level) shocks may suffer from built-in biases because they are affected by local selection issues. By focusing on localized effects, my findings reveal that banks tend to establish branches in areas already prepared for growth and that the branches themselves do little to spur further development. This observation suggests that aggregate-level analyses could be misleading, as they may conflate the inherent growth potential of certain areas with the actual impact of policy changes. For instance, in a setting with state-level branching deregulation (as in, e.g., [Jayaratne and Strahan \(1996\)](#)), one may attribute positive economic outcomes to the regulation change itself rather than local factors potentially correlated with state regulation changes. My observation that banks strategically enter regions that are already on a growth trajectory, paired with banks' documented influence on deregulation ([Kroszner and Strahan \(1999\)](#)) hints at the plausibility of such a conflation-inducing correlation. My unique approach therefore highlights the importance of considering localized selection biases, providing a clearer picture of the true effects of branch expansion on local economic growth.

4.3 Selection Effects: Bank Branches vs. Walmarts

In previous sections, I establish that banks select high-growth areas for entry but do not contribute much to growth themselves. This observation raises the question of just how skilled banks are at location selection compared to other types of businesses. In this section, I examine for comparison the total effect (selection + treatment effects) of other businesses that are likely to exert substantial effort in location selection: Walmarts. I calculate the luminosity around newly entering Walmart locations and compare the subsequent changes with those at extant post offices, as in previous sections. Because I do not observe planned Walmart locations but only those that are completed, the differences I calculate will combine both selection and treatment effects associated with new Walmarts.

[Figure 13](#) shows the estimated total effects of Walmart entry (green) along with the se-

lection effects associated with branch entry (blue) at various distances. As in [Figure 12](#), the top-left panel uses a simple circle of radius 5 km, and then from left to right, the remaining panels extend the exercise using increasingly expansive rings, as described previously. Across each panel, the bank selection effect is of a similar magnitude as the total Walmart effect for the first several event-years. As the rings become more expansive in the longer-distance specifications, the total effect associated with Walmart entry remains relatively stable, whereas the selection effect of branch entry decreases.

This exercise yields several insights. First, the comparable magnitudes of the bank branches' selection effect and Walmart's total effect, especially at the shortest distance of 5 km, serves to bolster the plausibility of the estimates in previous sections. Second, the fact that the banks' selection effect dissipates at longer distances and Walmart total effects do not highlights their contrasting strategic focuses. For banks, the substantial local effect at short distances suggests a location selection criteria that heavily emphasizes the immediate vicinity of each branch. The diminishing effect at longer distances reflects the importance of proximity to growth in making their branch location selection decisions. Conversely, the sustained Walmart effect at greater distances indicates that Walmart may choose to establish new locations based on broader regional expectations rather than more localized expectations. In other words, compared to Walmarts, banks seem to be more concerned with local growth rather than regional growth. As in [Section 4.2](#), this fact further emphasizes that studies using broad geographic shocks miss an important aspect of the relationship between branch entry and economic growth by failing to account for local selection effects.

5 Conclusions

This paper offers a new angle to address a classic finance-growth topic: the impact of bank branching on economic growth. Using variation in completion across planned branches and granular proxies for economic growth, I disentangle selection and treatment effects of branch entry at a local level. Using a series of outcome variables (nighttime light emissions, new business registrations, and SBA-7a loan amounts), I reveal a striking pattern: banks are highly selective in their branch placements, favoring areas with pre-existing growth potential, but their actual contribution to economic development is small.

The divergence between selection and treatment effects sheds light on economic aspects of branching that are missed by aggregate-level analyses. The extensive literature studying

state-level shocks to branching regulations (e.g., [Jayaratne and Strahan \(1996\)](#), [Jayaratne and Strahan \(1998\)](#), [Chava et al. \(2013\)](#), [Amore et al. \(2013\)](#)) may inadvertently conflate selection effects with the causal impacts of branch entry. By focusing on state-level shocks, these studies might overestimate the true economic benefits of deregulation, as the observed positive effects could be more reflective of the inherent growth potential of the selected areas rather than the impact of the bank branches themselves.

I also explore the spatial distribution of the effects that I document; both selection and treatment effects are very localized, further highlighting the importance of distance in the finance-growth relationship. Ultimately, this research emphasizes the need to critically evaluate the assumptions underlying aggregate-level analyses and consider the relationship between finance and growth at a more granular level.

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**Table 1 : Number of Unfulfilled and Fulfilled Branches with Night Lights
Data: 1996-2006**

The table reports the number of units from each year that are included in the difference-in-differences analyses using log-lights as the outcome variable (see [Figure 9](#)). For each of the 349 unfulfilled branches in this data, I estimate what the average treatment effect across these locations would have been had the branch been completed. The size of the pool of comparison units for this counterfactual is given in the column “Fulfilled Branches.” For consistency, I restrict the pool of comparison units used to estimate the selection effect of each cohort to be the same size as that used to estimate the treatment effect.

Table 1: Number of Unfulfilled and Fulfilled Branches with Nightlights Data: 1996-2006

Year	Unfulfilled Branches	Fulfilled Branches
1996	28	634
1997	29	719
1998	46	572
1999	34	595
2000	35	409
2001	27	451
2002	21	411
2003	29	428
2004	34	575
2005	30	495
2006	36	444
Total	349	5733

Table 2 : Covariate Balance: Fulfilled vs. Unfulfilled Branches

The table reports summary statistics of relevant covariates for each location included in my main analysis (see Figure 9). The sample is split based on whether the bank branch application corresponding to that location was fulfilled (i.e., the branch opened) or unfulfilled (i.e., the branch was not opened). The two subsamples (Fulfilled/Unfulfilled) are indicated in the column headers. The far right columns report the mean difference of each variable (Fulfilled - Unfulfilled) and test for its statistical significance. The construction employment and payroll variables are calculated from county-level data, whereas each of the others variables are calculated within five kilometers of the proposed branch location. For instance, $lights_{t-1}$ refers to the mean night lights value within a five kilometer radius of the proposed branch in the year prior to application. Variables preceded by Δ are growth rates in the variable over the corresponding window. For instance, the mean value for $\Delta lights_{t-2,t-1}$ for fulfilled branches is -0.01, meaning that the night lights within five kilometers of these locations decreased 1 percentage point, on average, between years -2 and -1 relative to the year the branch was opened. $branches$ refers to the number of branches within five kilometers of the proposed branch. $deposits$ corresponds to the total value of deposits held by these branches in 1994 dollars. Not every variable is available for all observations because of gaps in coverage in the FDIC summary of deposits data or county business patterns data.

Table 2: Covariate Balance: Fulfilled vs. Unfulfilled Branches

	Fulfilled			Unfulfilled			Difference	
	Count	Mean	Median	Count	Mean	Median	Mean	<i>p</i> -value
$lights_{t-1}$	5733	38.54	41.14	349	38.27	41.45	0.27	0.69
$\Delta lights_{t-2,t-1}\%$	5733	-0.01	-0.01	349	-0.00	-0.00	-0.00	0.26
$\Delta lights_{t-4,t-1}\%$	5733	-0.01	-0.00	349	-0.00	0.00	-0.01	0.08
$branches_{t-1}$	5733	7.43	7.00	349	7.64	7.00	-0.21	0.52
$log(deposits)_{t-1}$	5544	12.22	12.41	340	12.21	12.45	0.01	0.85
$\Delta deposits_{t-2,t-1}\%$	5551	0.05	0.03	340	0.07	0.03	-0.01	0.16
$\Delta deposits_{t-4,t-1}\%$	4240	0.28	0.08	284	0.29	0.08	-0.02	0.74
$\Delta Constr.Employment_{t-1,t+1}\%$	5705	0.09	0.08	344	0.08	0.08	0.01	0.16
$\Delta Constr.Employment_{t-2,t-1}\%$	5694	0.05	0.04	346	0.05	0.05	-0.00	0.71
$\Delta Constr.Employment_{t-4,t-1}\%$	5699	0.15	0.13	347	0.15	0.12	-0.00	0.70
$\Delta Constr.Payroll_{t-1,t+1}\%$	5705	0.18	0.17	344	0.17	0.17	0.01	0.31
$\Delta Constr.Payroll_{t-2,t-1}\%$	5694	0.09	0.08	346	0.09	0.08	0.00	0.99
$\Delta Constr.Payroll_{t-4,t-1}\%$	5699	0.28	0.26	347	0.28	0.24	0.00	0.88

Figure 1 : Banks, Branches, and Deposits by Regulator

The figure displays the number of banks (Panel A), branches (Panel B), and deposits (Panel C) that are under regulation by each of the major regulators each year from 1994 to 2023. In each plot, the bottom bar (blue) represents the OCC.

Banks, Branches, and Deposits by Regulator

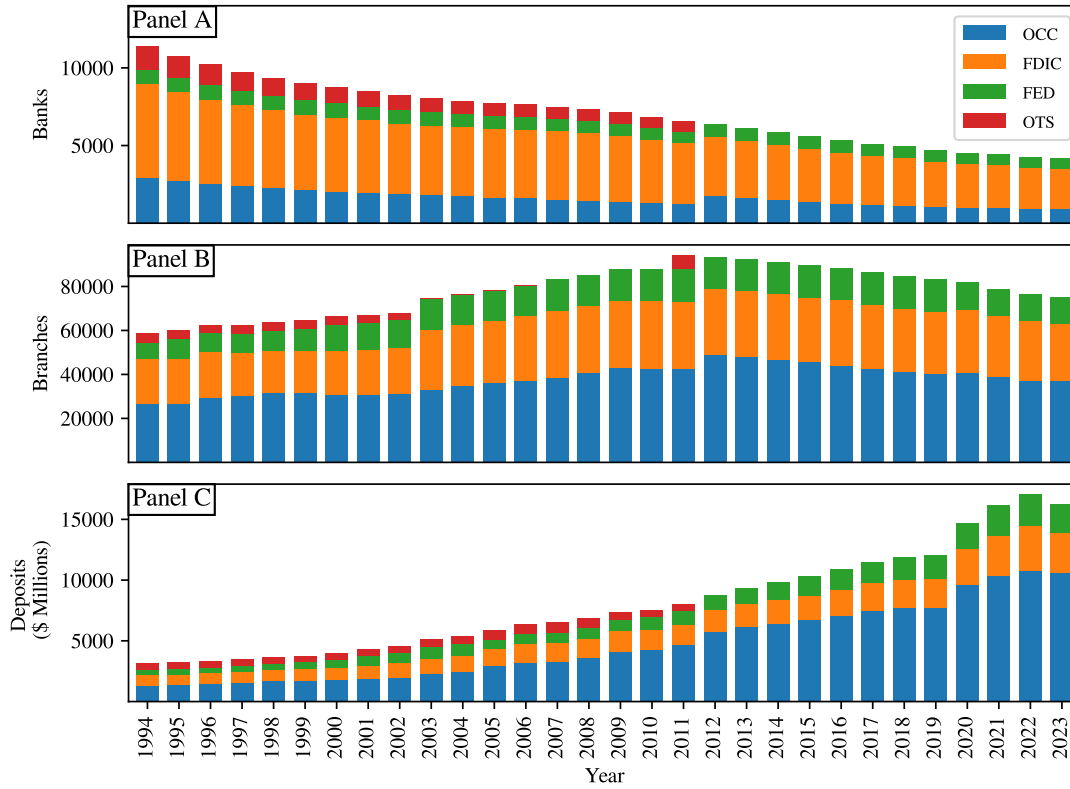


Figure 2 : Example of a Fulfilled Application - Newberry, Florida

The figure displays an example entry from the CAS database detailing information on a completed branch in Newberry, Florida. The top section, “Details For OCC Control Number: 2000-SE-05-0027,” contains basic information about the applying bank, including its name, charter number, and headquarters location. Under “Proposed Branches” the system provides the proposed location of the new branch. “Public Comment Information” details the start and end dates between which the public could comment on the proposed branch. “Filing Status” documents the receipt, approval, and effective dates of the branch’s application. [Figure 7](#) portrays an aerial view of the branch and a demonstration of the area over which light emissions are calculated.

Corporate Applications Search Result Details

Details For OCC Control Number: 2000-SE-05-0027 Return to List

Application Type: Branch Establishment

Transaction Form: Branch Establishment - Staffed Branch

Bank: The First National Bank of Alachua

Charter/License #: 8980

Bank Headquarters Location: 15000 NW 140TH STREET
ALACHUA, FL 32616
County: Alachua

Proposed Branches:

Branch Name	Street Address	Suite	City	State	Zip	County	Certification #
NEWBERRY BRANCH	24202 WEST NEWBERRY ROAD, SUITE F		NEWBERRY	FL	32669	Alachua	117659A

Public Comment Information:

Comment Period Start Date	Comment Period End Date	Adjusted Period Start Date	Adjusted Period End Date	OCC Contact
2000-02-17	2000-03-17			Southeast District Office Contact Info

Filing Status:

Action	Date
Receipt	2000-02-15
Approved	2000-03-23
Consummated/Effective	2000-05-08

Figure 3 : Example of a Withdrawn Application - Ocoee, Florida

The figure displays an example entry from the CAS database detailing information on a proposed branch in Ocoee, Florida whose application was withdrawn. The top section, “Details For OCC Control Number: 1999-ML-05-0220,” contains basic information about the applying bank, including its name, charter number, and headquarters location. Under “Proposed Branches” the system provides the proposed location of the new branch. “Public Comment Information” details the start and end dates between which the public could comment on the proposed branch. “Filing Status” documents the receipt, approval, and withdrawal dates of the branch’s application.

Corporate Applications Search Result Details

Details For OCC Control Number: 1999-ML-05-0220 Return to List

Application Type: Branch Establishment
Transaction Form: Branch Establishment - Staffed Branch
Bank: FIRST UNION NATIONAL BANK
Charter/License #: 1
Bank Headquarters Location: FIRST UNION PLAZA
 CHARLOTTE, NC 28288
 County: Mecklenburg

Proposed Branches:

Branch Name	Street Address	Suite	City	State	Zip	County	Certification #
CLARKE ROAD BRANCH	VICINITY OF SE SILVER STAR AND CLARKE ROAD		OCOEE	FL	32701	Orange	116857A

Public Comment Information:

Comment Period Start Date	Comment Period End Date	Adjusted Period Start Date	Adjusted Period End Date	OCC Contact
1999-09-28	1999-10-27			ML Contact Info

Filing Status:

Action	Date
Receipt	1999-09-28
Approved	1999-10-29
Withdrawn	2000-10-27

Figure 4 : Branch Establishment Application Process

The figure portrays the portion of branch establishment applications that reach each stage as well as the amount of time that typically passes between each stage. The process flows from left to right starting with the “Applications” node, which indicates that there are 42,396 applications in the OCC’s CAS data. Subsequent nodes report the percentage of these applications that reach the corresponding step (e.g., 96.6% of applications’ documentation begins with the OCC receiving the application, whereas the remaining 3.4% of applications are data errors). Each node is scaled in proportion to the number of applications that it represents, and where appropriate, the passage of time between steps is indicated at the bottom of the figure.

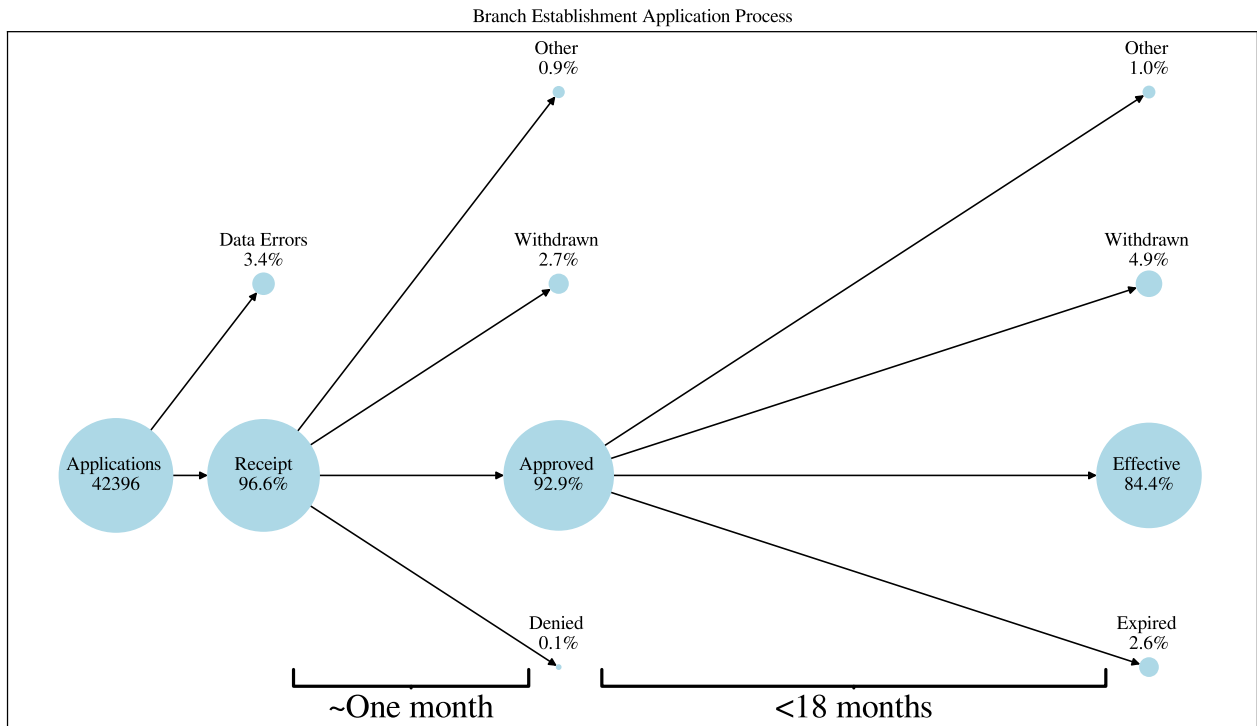


Figure 5 : Number of Bank Branch Applications and Percentage Unfulfilled Over Time

The figure displays the number of branch applications received each year (green, solid line) as well as the percentage of these that are not fulfilled (blue, dashed line).

Number of Bank Branch Applications and Percentage Unfulfilled Over Time

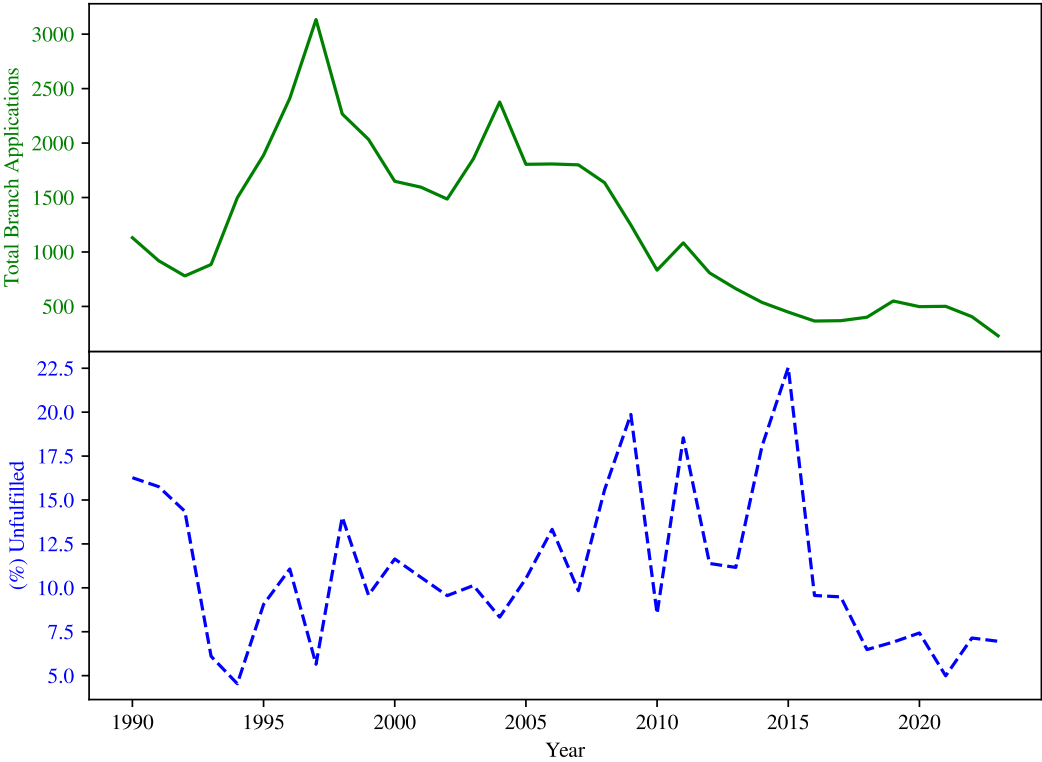


Figure 6 : Locations of Unfulfilled Branches

The figure displays the location of each unfulfilled branch that is used in the main analysis (which uses log-level of light emissions as the outcome variable).

Locations of Unfulfilled Branches

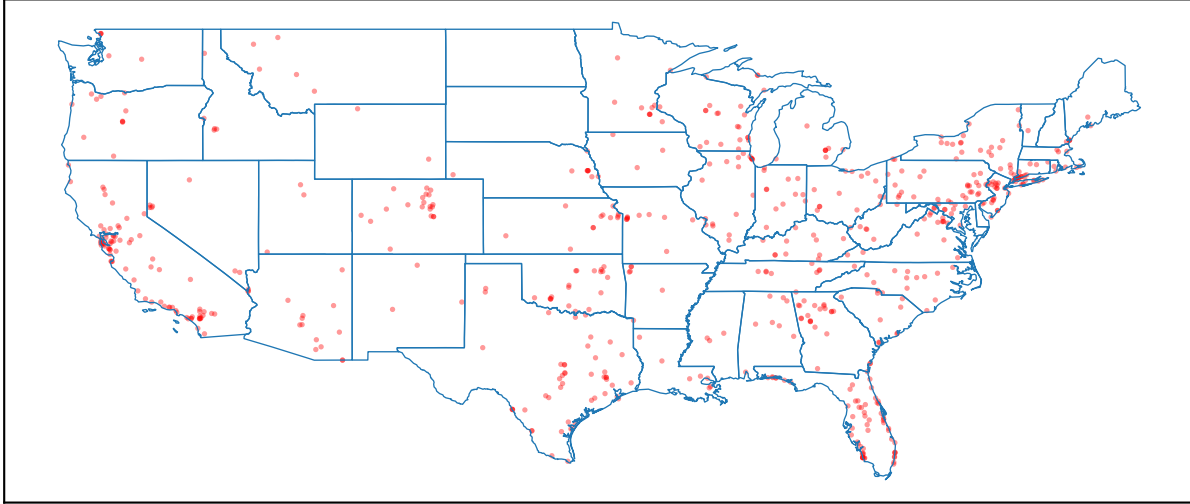


Figure 7 : Newberry, Florida, Aerial View

The figure uses an aerial view of the Newberry, Florida branch from [Figure 2](#) to portray the luminosity calculation. The (completed) branch is indicated in the center of the map by a black dot. The two concentric circles have radii of 5 and 10 kilometers. I calculate the average light value for both the pink-shaded inner circle as well as the blue-shaded ring shape. I do the same for additional rings (not shown in the picture) with inner/outer radii pairs of 10/20 km and 20/40 km. I employ the same approach to calculate lights around branches that are fulfilled or unfulfilled and around post offices as a reference point.

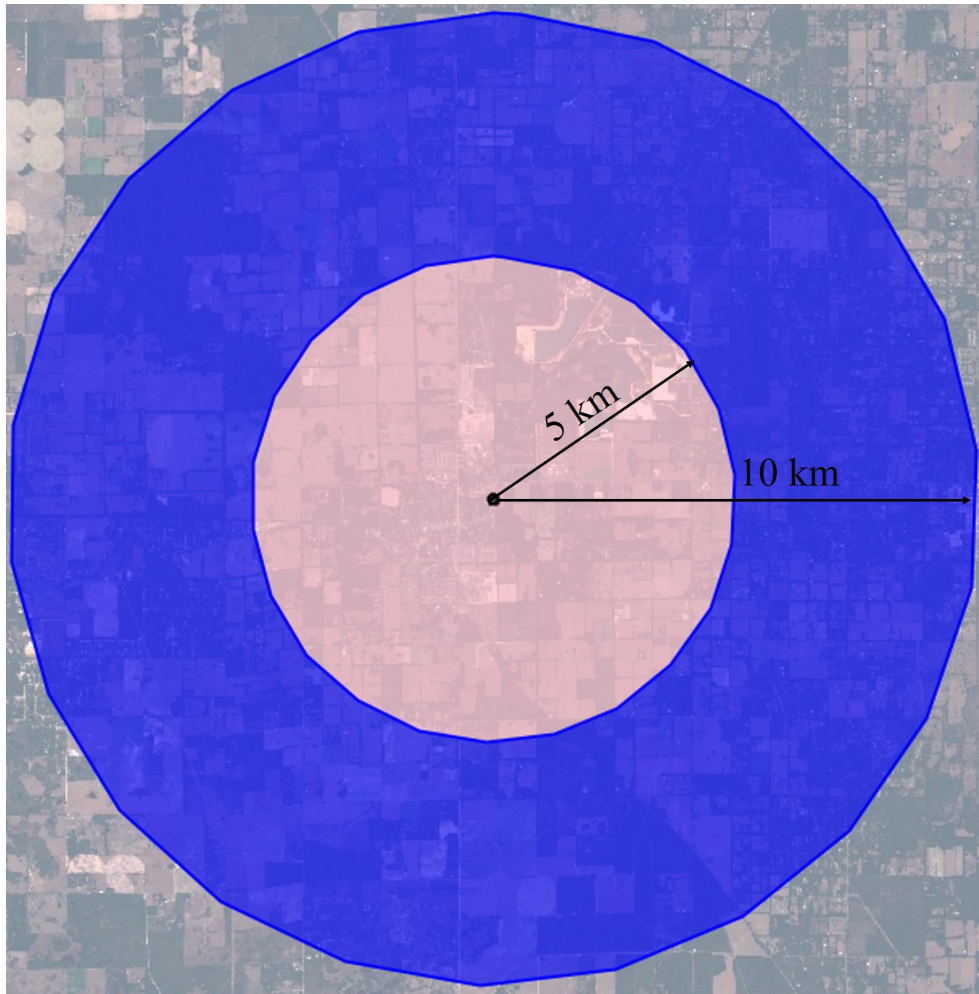
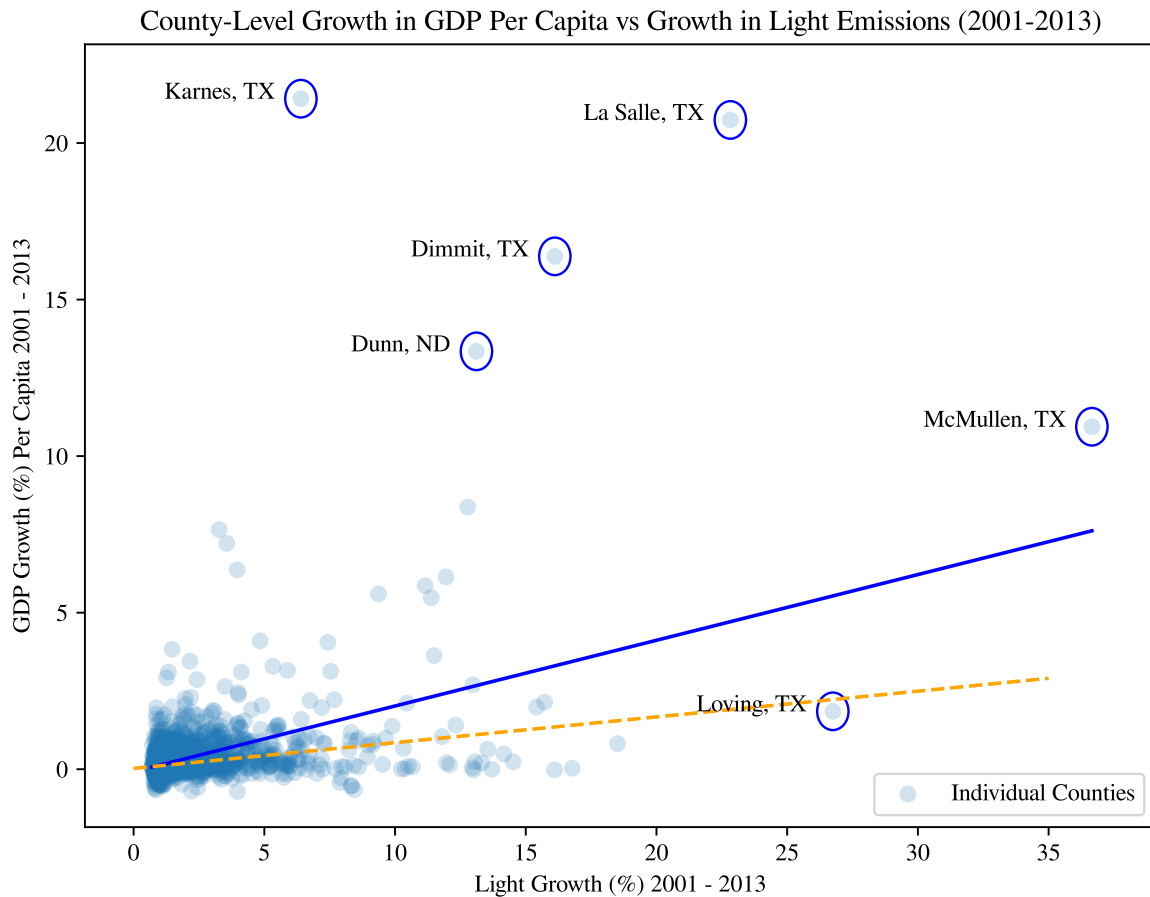


Figure 8 : County-Level Growth in GDP Per Capita vs Growth in Light Emissions (2001-2013)

The figure displays the cross-sectional relationship between county growth in per capita GDP over 2001-2013 and county growth in light emissions over the same period. Real GDP growth per capita is reported on the vertical axis, with growth in light emissions reported on the horizontal axis. Each blue dot represents an individual county. Best-fit lines are estimated using both OLS (blue line) and median regression (orange-line) approaches, with these regressions reported below the plot. Extreme outlier counties are labeled for context.



OLS Regression (Blue):
 $y = -0.08 + 0.21 * x$
 T-statistics: (-4) (29)
 R-squared: 0.21

Median Regression (Orange):
 $y = 0.03 + 0.08 * x$
 T-statistics: (4) (35)
 R-squared: -----

Figure 9 : Selection and Treatment Effects of Opening a Branch on Log-Level of Light Emissions Within 5 km

The figure displays estimated selection and treatment effects of branch entry on the log-lights around the branch's location. Effects are estimated using a modification of the [Callaway and Sant'Anna \(2021\)](#) approach. Selection effects compare changes around planned but not complete branches to those around post offices, whereas treatment effects compare locations where a bank opens a branch to planned but not complete branches of the same cohort. Lights are calculated as the mean across pixels within five kilometers of the proposed branch. The values of yearly effects are reported on the left-hand axis. The outcome variable over each of the four years pre-treatment is included as a covariate/control. 95% and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling unfulfilled units with replacement within each cohort and calculating the relevant statistic for each such random sample.

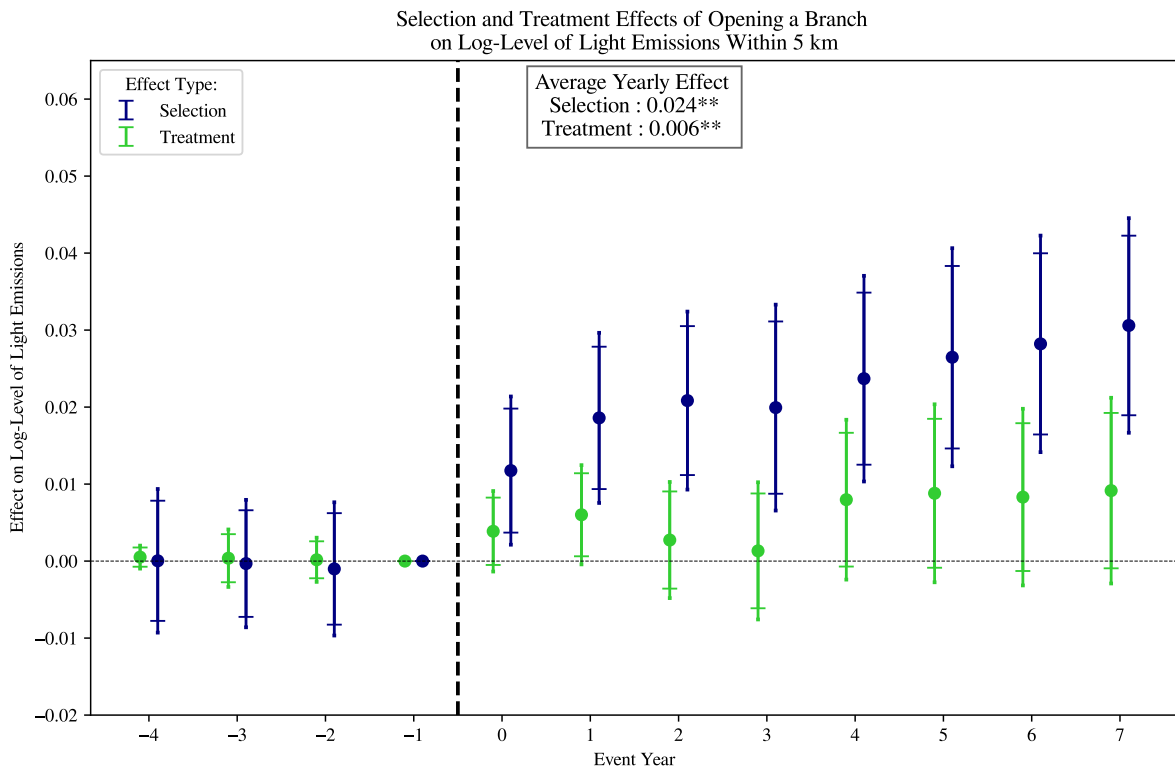


Figure 10 : Selection and Treatment Effects of Opening a Branch on Log of New Business Registrants in That ZIP Code

The figure displays estimated selection and treatment effects of branch entry on the log-number of new business registrants in the entered ZIP code. Effects are estimated using a modification of the [Callaway and Sant'Anna \(2021\)](#) approach. Selection effects compare changes ZIP codes where a branch was planned but not completed to all other ZIP codes, whereas treatment effects compare opening ZIP codes to ZIP codes where a branch was planned but not completed of the same cohort. The values of yearly effects are reported on the left-hand axis. The outcome variable over each of the four years pre-treatment is included as a covariate/control. 95% and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling ZIP codes where a branch was planned but not completed with replacement within each cohort and calculating the relevant statistic for each such random sample.

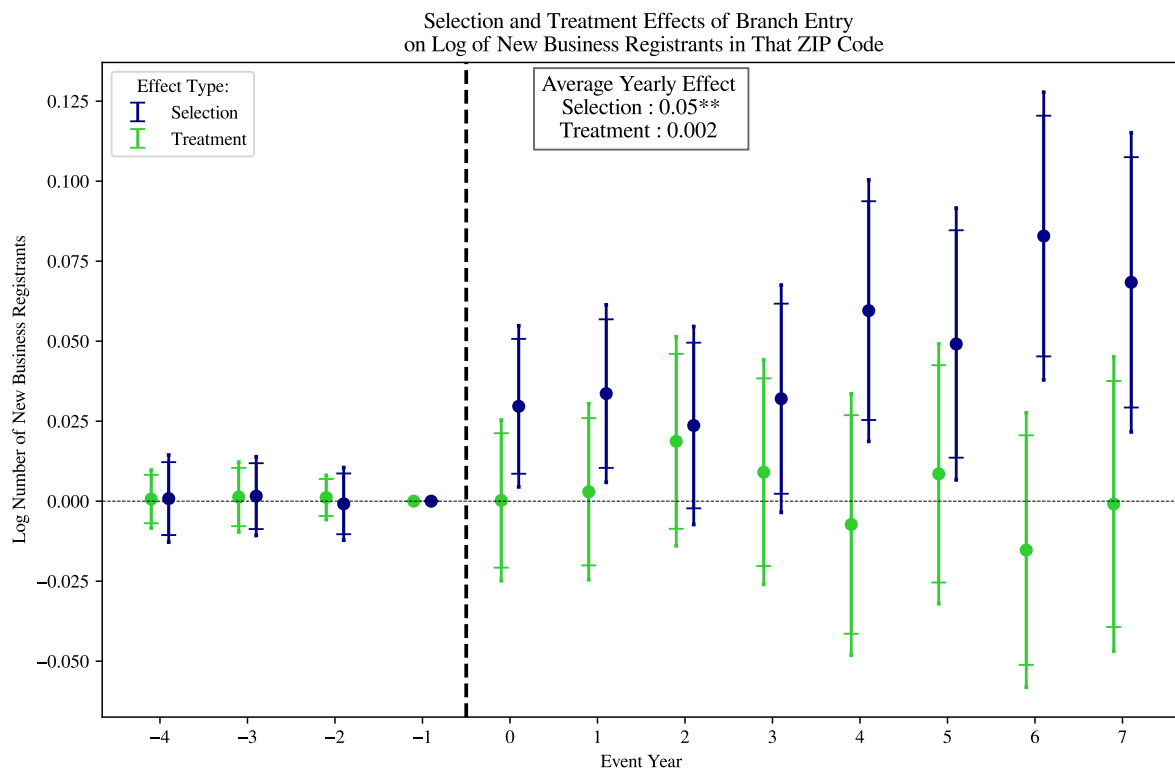


Figure 11 : Selection and Treatment Effects of Opening a Branch on Log of SBA-7a Loan Amounts Issued to Borrowers in That ZIP Code

The figure displays estimated selection and treatment effects of branch entry on the log of SBA-7a loan amounts issued to borrowers in the entered ZIP code. Effects are estimated using a modification of the Callaway and Sant'Anna (2021) approach. Selection effects compare changes in ZIP codes where a branch was planned but not completed to all other ZIP codes, whereas treatment effects compare opening ZIP codes to ZIP codes where a branch was planned but not completed of the same cohort. The values of yearly effects are reported on the left-hand axis. The outcome variable over each of the four years pre-treatment is included as a covariate/control. 95% and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling ZIP codes where a branch was planned but not completed with replacement within each cohort and calculating the relevant statistic for each such random sample.

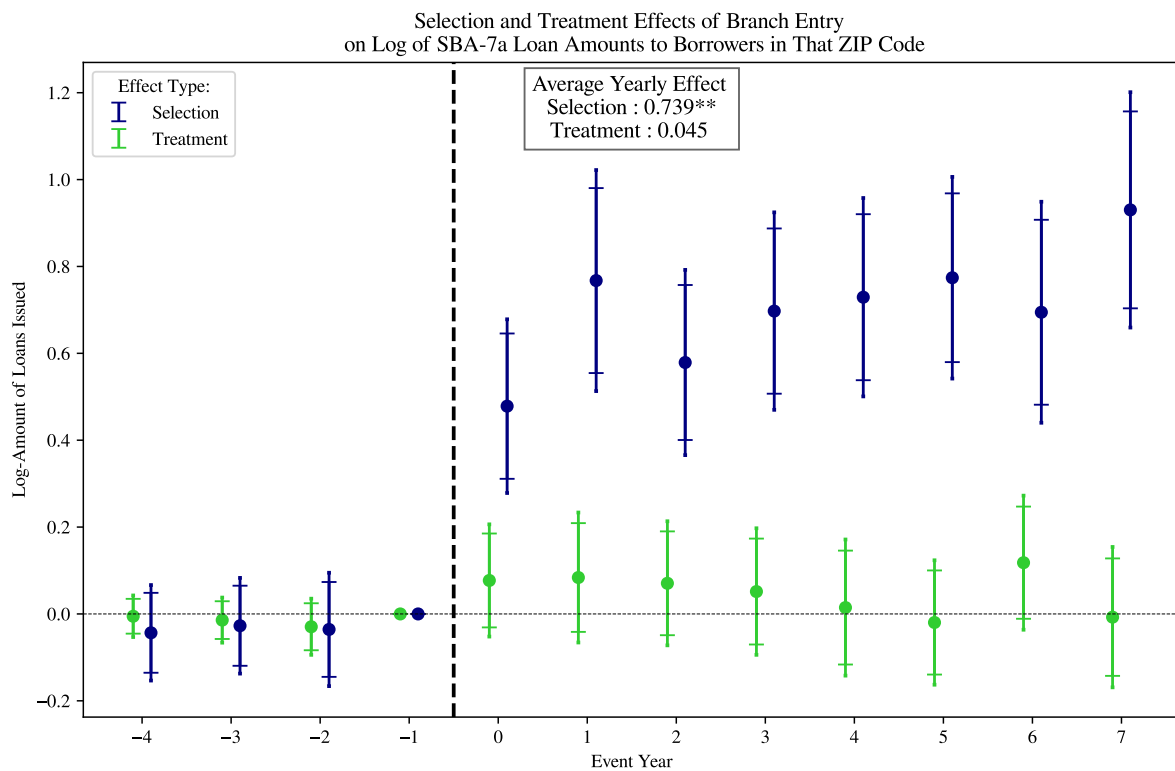


Figure 12 : Selection and Treatment Effects of Opening a Branch on Log-Level of Light Emissions at Assorted Distances

The figure displays estimated selection and treatment effects of branch entry on the log-level of light emissions at assorted distances from planned bank branches. Effects are estimated using a modification of the [Callaway and Sant'Anna \(2021\)](#) approach. Selection effects compare changes around unfulfilled branches to those around post offices, whereas treatment effects compare locations where a bank opens a branch to unfulfilled branches of the same cohort. Lights are calculated as the mean across pixels over ring-shaped areas around the proposed branch or post office (see [Figure 7](#)). The top of each panel states the range of distances from the branch over which luminosity is calculated. The values of yearly effects are reported on the left-hand axis. The outcome variable over each of the four years pre-treatment is included as a covariate/control. 95% and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling unfulfilled units with replacement within each cohort and calculating the relevant statistic for each such random sample.

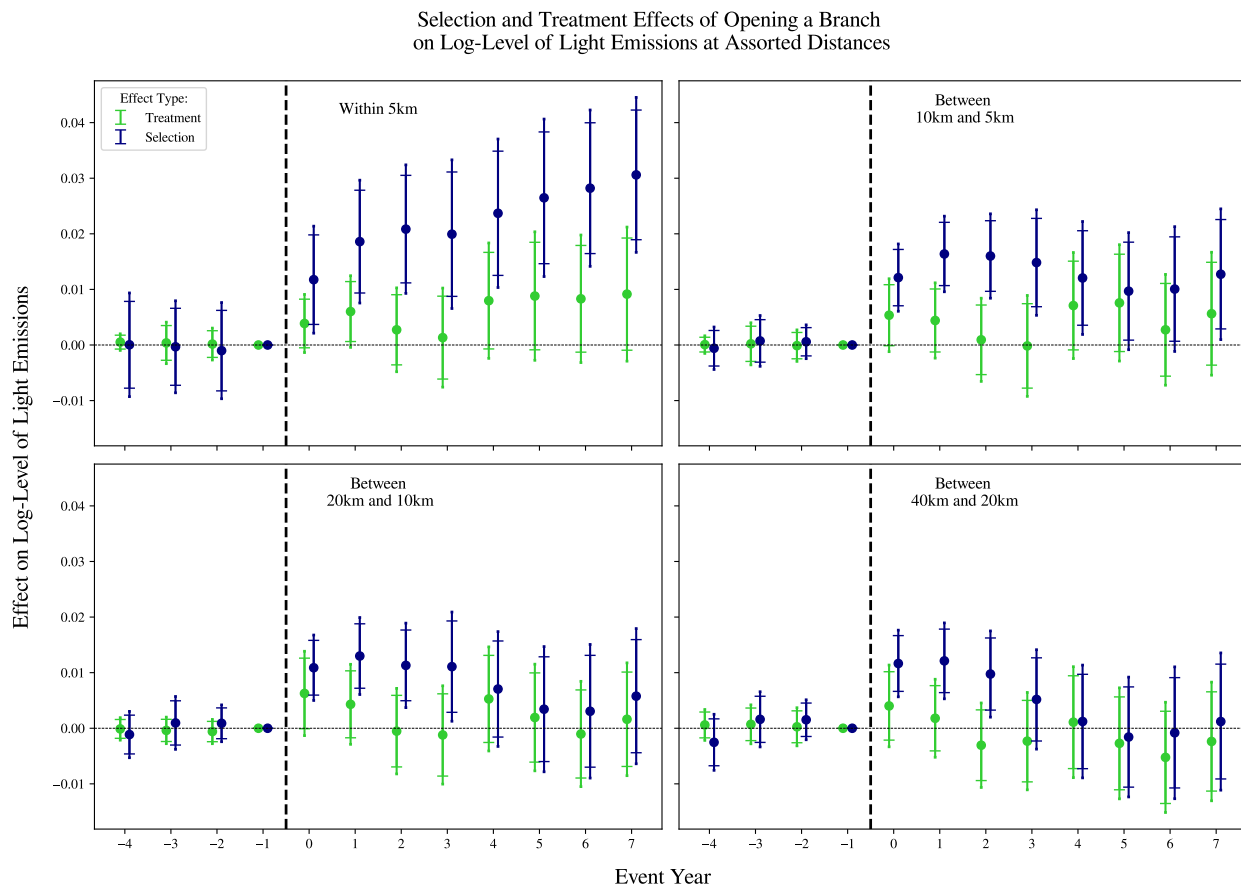
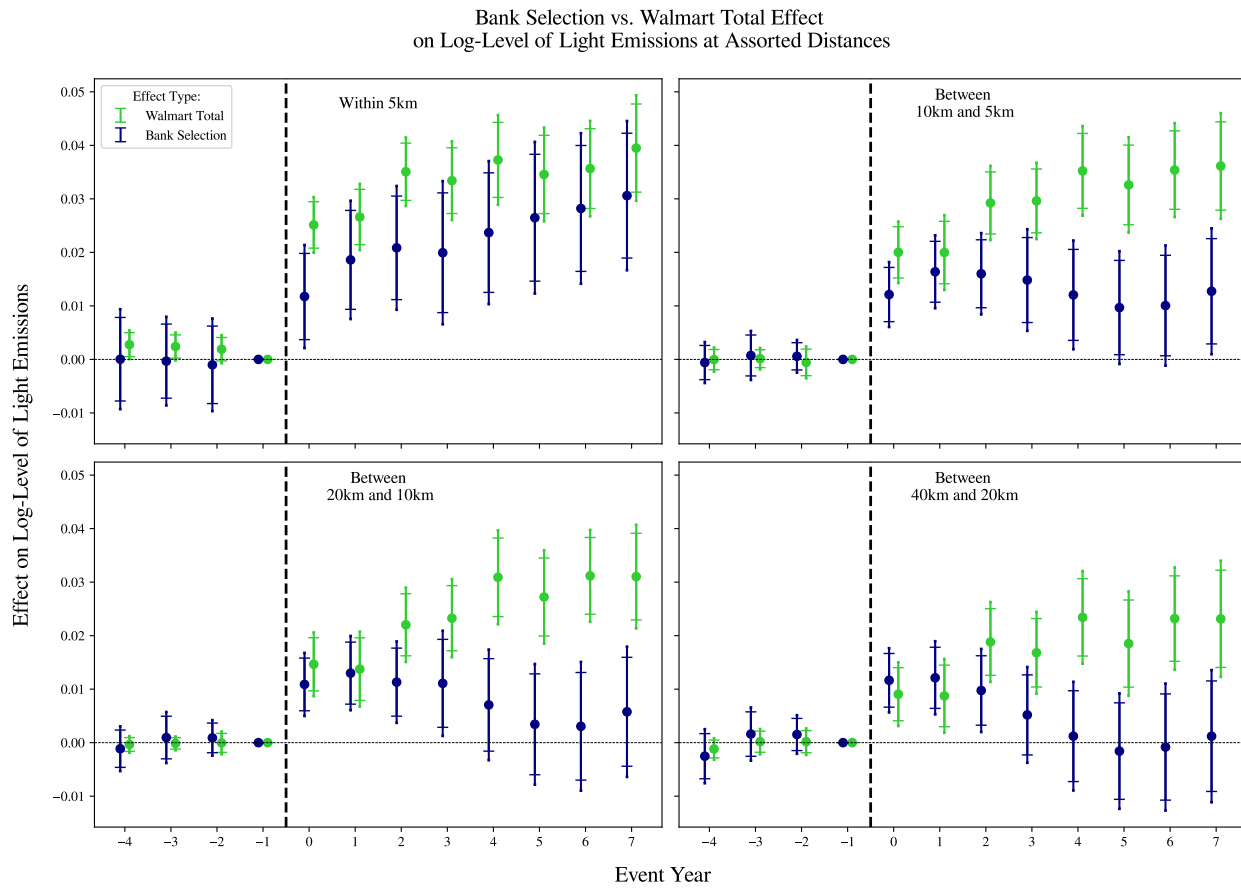


Figure 13 : Bank Selection vs. Walmart Total Effect on Log-Level of Light Emissions at Assorted Distances

The figure displays estimated selection effects of branch entry and total effect of Walmart entry on the log-level of light emissions at assorted distances from the relevant location. Effects are estimated using a modification of the [Callaway and Sant'Anna \(2021\)](#) approach. Bank selection effects compare changes around unfulfilled branches to those around post offices, whereas Walmart total effects compare locations that open a Walmart to extant post offices. Lights are calculated as the mean across pixels over ring-shaped areas around the relevant geographic point (see [Figure 7](#)). The top of each panel states the range of distances from the point over which luminosity is calculated. The values of yearly effects are reported on the left-hand axis. The outcome variable over each of the four years pre-treatment is included as a covariate/control. 95% and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling unfulfilled branches or completed Walmarts with replacement within each cohort and calculating the relevant statistic for each such random sample.



Appendix

Table A.1: County-Level GDP Growth on Growth in Light Emissions: 2001-2013

For each year, I run annual cross-sectional regressions of county-level real GDP growth on county-level light growth and an intercept. I then average the estimated coefficients across years. I repeat this regression using GDP growth as calculated contemporaneously (t), over the next year ($t + 1$), and over the next five years ($t + 5$). Panel A reports the average coefficient on lights growth and the average cross-sectional R-squared. Panel B reports the same, but GDP growth is now reported on a per capita basis. Data are 2001-2013, and the 50 brightest counties are excluded. Standard errors are calculated using the [Newey and West \(1987\)](#) adjustment allowing for serial correlation up to ten lags.

Table A.1: County-Level GDP Growth on Growth in Light Emissions: 2001-2013

Panel A: County GDP Growth on Growth in Light Emissions			
	GDP growth (t)	GDP growth (t+1)	GDP growth (t+5)
Lights Growth	0.03*** (6.52)	0.02*** (2.94)	0.06*** (5.2)
Average R-squared	0.01	0.01	0.01

Panel B: County GDP Per Capita Growth on Growth in Light Emissions			
	GDP growth (t)	GDP growth (t+1)	GDP growth (t+5)
Lights Growth	0.02*** (4.02)	0.01 (0.74)	0.02 (1.09)
Average R-squared	0.01	0.01	0.01

Table A.2: Number of ZIP Codes with Unfulfilled and Fulfilled Branches with New Business Registrant Data: 1992-2009

The table reports the number of units from each year that are included in the difference-in-differences analyses using the log of new business registrants as the outcome variable (see [Figure 10](#)). For each of the 513 unfulfilled branches included in the data, I estimate what the average treatment effect across these locations would have been had the branch been completed. The size of the pool of comparison units for this counterfactual is given in the column “ZIP Codes with Fulfilled Branches”. For consistency, I restrict the pool of comparison units used to estimate the selection effect of each cohort to be the same size as that used to estimate the treatment effect.

Table A.2: Number of ZIP Codes with Unfulfilled and Fulfilled Branches with New Business Registrant Data: 1992-2009

Year	ZIP Codes with Unfulfilled Branches	ZIP Codes with Fulfilled Branches
1992	11	157
1993	4	197
1994	14	290
1995	15	424
1996	42	494
1997	22	598
1998	61	450
1999	37	382
2000	38	318
2001	27	317
2002	20	312
2003	26	350
2004	28	433
2005	31	363
2006	38	350
2007	32	363
2008	36	299
2009	31	200
Total	513	6297

Table A.3: Number of ZIP Codes with Unfulfilled and Fulfilled Branches with SBA-7a Data: 1995-2016

The table reports the number of units from each year that are included in the difference-in-differences analyses using SBA-7a lending to borrowers in the entered ZIP code as the outcome variable (see [Figure 11](#)). For each of the 271 unfulfilled branches included in the data, I estimate what the average treatment effect across these locations would have been had the branch been completed. The size of the pool of comparison units for this counterfactual is given in the column “ZIP Codes with Fulfilled Branches.” For consistency, I restrict the pool of comparison units used to estimate the selection effect of each cohort to be the same size as that used to estimate the treatment effect.

Table A.3: Number of ZIP Codes with Unfulfilled and Fulfilled Branches with SBA-7a Data: 1995-2016

Year	ZIP Codes with Unfulfilled Branches	ZIP Codes with Fulfilled Branches
1995	7	135
1996	21	172
1997	11	239
1998	34	176
1999	15	148
2000	18	150
2001	7	149
2002	9	121
2003	13	175
2004	14	225
2005	14	171
2006	19	152
2007	11	197
2008	22	157
2009	12	115
2010	5	89
2011	4	108
2012	6	69
2013	7	61
2014	10	52
2015	9	30
2016	3	34
Total	271	2925

Figure A.1 : Selection and Treatment Effects of Opening a Branch on Log-Lights Within 5 km (Low-Deposit Areas)

The figure displays estimated selection and treatment effects of branch entry on the log-lights around the branch's location. Effects are estimated using a modification of the [Callaway and Sant'Anna \(2021\)](#) approach. Selection effects compare changes around unfulfilled branches to those around post offices, whereas treatment effects compare locations where a bank opens a branch to unfulfilled branches of the same cohort. Lights are calculated as the mean across pixels within five kilometers of the unfulfilled branch. The values of yearly effects are reported on the left-hand axis. 95 and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling unfulfilled units with replacement within each cohort and calculating the relevant statistic for each such random sample. This figure is similar to [Figure 9](#) except this version only includes locations in the bottom quartile of deposits located within 20 km and only displays treatment effects of branch entry.

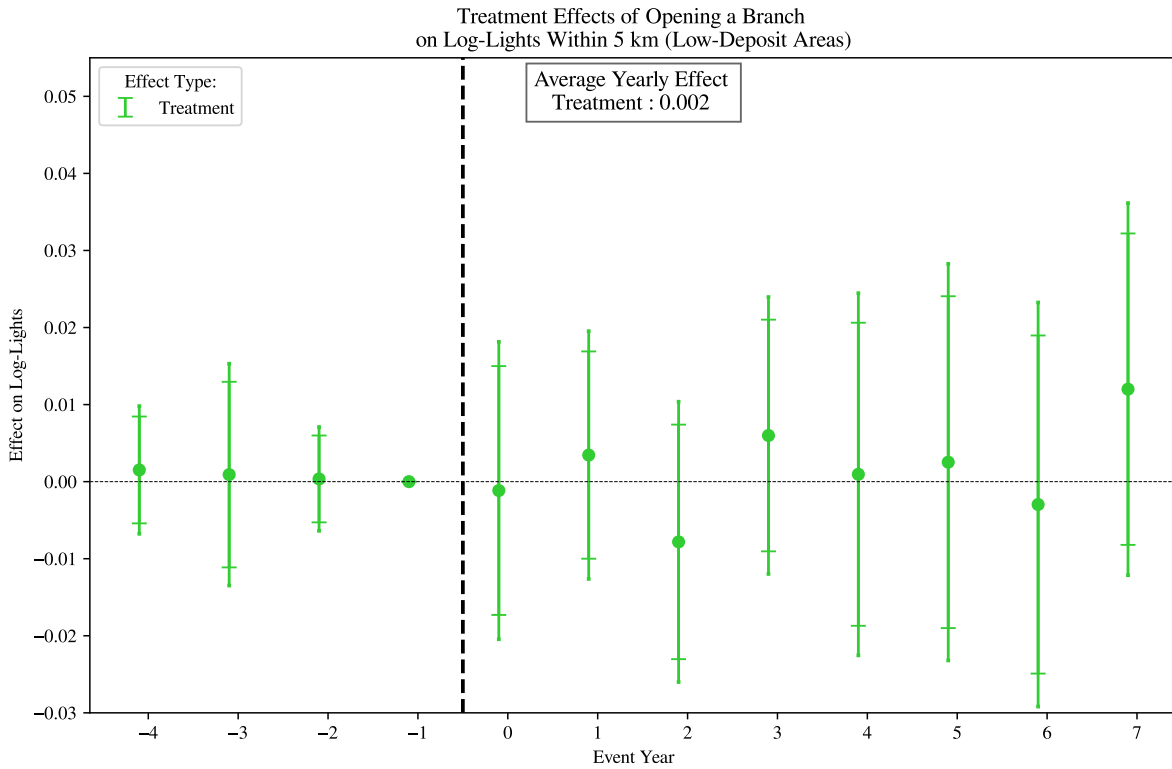


Figure A.2 : Selection and Treatment Effects of Opening a Branch on Log-Lights Within 5 km (Equal Weighting Across Cohorts)

The figure displays estimated selection and treatment effects of branch entry on the log-lights around the branch's location. Effects are estimated using a modification of the [Callaway and Sant'Anna \(2021\)](#) approach. Selection effects compare changes around unfulfilled branches to those around post offices, whereas treatment effects compare locations where a bank opens a branch to unfulfilled branches of the same cohort. Lights are calculated as the mean across pixels within five kilometers of the unfulfilled branch. The values of yearly effects are reported on the left-hand axis. 95 and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling unfulfilled units with replacement within each cohort and calculating the relevant statistic for each such random sample. This figure is similar to [Figure 9](#) except this version weights cohort-year estimates equally across cohorts, rather than by the number of unfulfilled branches in that application year.

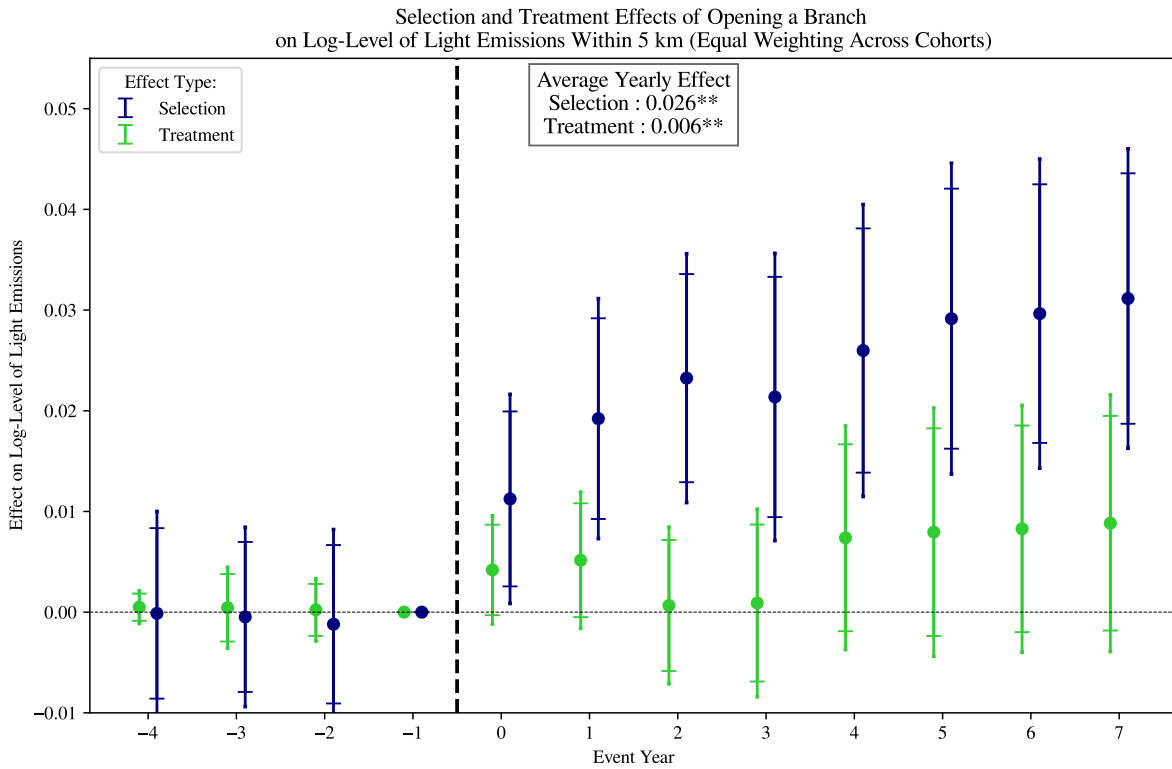


Figure A.3 : Selection and Treatment Effects of Opening a Branch on Log-Lights Within 5 km (No Covariates)

The figure displays estimated selection and treatment effects of branch entry on the log-lights around the branch's location. Effects are estimated using a modification of the [Callaway and Sant'Anna \(2021\)](#) approach. Selection effects compare changes around unfulfilled branches to those around post offices, whereas treatment effects compare locations where a bank opens a branch to unfulfilled branches of the same cohort. Lights are calculated as the mean across pixels within five kilometers of the unfulfilled branch. The values of yearly effects are reported on the left-hand axis. 95 and 90% confidence intervals are estimated via bootstrapped standard errors. The bootstrap is performed by repeatedly sampling unfulfilled units with replacement within each cohort and calculating the relevant statistic for each such random sample. This figure is similar to [Figure 9](#) except this version does not include any control variables.

