Firm Responses to Hiring and Investment Subsidies: Regression Discontinuity Evidence from the California Competes Tax Credit*

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Abstract

We examine firm responses to state hiring and investment subsidies. We leverage institutional features of the California Competes Tax Credit (CCTC), a large-scale business incentive program that incorporates best practices from prior job creation policies. The CCTC award selection procedure combines formula-based and discretionary components. Leveraging applicant score eligibility cutoffs in a regression discontinuity design and taking advantage of rich longitudinal microdata on establishments and their parent firms, we find that firms expand activity in California in response to CCTC awards. We also find little evidence that these expansions come at the expense of firms' operations in other states. Our results suggest that targeted and audited hiring and investment subsidies can be effective in promoting local business expansions without inducing significant cross-state displacement effects.

JEL Codes: H25, H71, H73, J23, J38, R12, R38, R58

Keywords: business tax incentives, hiring subsidies, interjurisdictional tax competition, industrial policy, regression discontinuity

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1 Introduction

State and local governments in the United States spend billions of dollars each year to attract and retain businesses (Moretti, 2011; Bartik, 2019). By some estimates, these expenditures exceed total national outlays for programs such as the Earned Income Tax Credit and unemployment insurance.[1] However, the evidence on the effectiveness of business incentives is mixed, and state-provided subsidies to firms remain highly controversial (Slattery and Zidar, 2020). Concerns about the welfare implications of state business incentives are even more pronounced at the national level given their potential to merely shift economic activity in space. In that case, states' competition over firms may come at a high cost for taxpayers and have little aggregate benefit.

This paper examines firm responses to a state hiring and investment subsidy program that attempts to incorporate "best practices" from prior government efforts to attract and retain businesses. We leverage institutional features of the California Competes Tax Credit (CCTC), a state program that allocated over $1.5 billion in tax credits to businesses between 2014 and 2021. The CCTC represents one of a new generation of business incentive policies that features discretionary tools, audited job creation benchmarks, and enforceable recaptures of benefits when hiring and investment goals are not met. The structure of the CCTC also lends itself to rigorous evaluation, whereas many evaluations of economic incentive programs face challenges in identifying valid counterfactuals (Bartik, 1991; Rodrik, 2007; Neumark and Simpson, 2015). In particular, the CCTC uses a cost-benefit formula to score applicants based on the projected net employment and investment impacts of tax credits in the state, making discretionary decisions to award credits if firms' scores meet an initial cutoff for eligibility. Firms are unable to manipulate these scores because the cutoff is unknown ex ante and depends on the applications submitted by all firms in a given allocation period as well as the available budget. This setting thus provides a rare opportunity to use a regression discontinuity (RD) design to estimate the causal effects of a large-scale business incentive program.

Taking advantage of administrative data from the CCTC program combined with confidential microdata from the Longitudinal Business Database (LBD) from 2009 to 2019, we examine the CCTC program's effects on establishment location, employment, and payroll growth within California, as well as on revenue and reallocation patterns within firms on a national scale. Our

[1] Kline and Moretti (2014) estimate that $95 billion was spent on business location incentives in 2012, exceeding outlays on both the Earned Income Tax Credit (EITC), which have ranged between $65 and $70 billion since 2010 (`https://taxfoundation.org/earned-income-tax-credit-eitc/`), and benefits paid out under unemployment insurance (UI), which amounted to about $43 billion in 2012 (`https://oui.doleta.gov/unemploy/claimssum.asp`).

context and data allow us not only to study the impacts of a business incentive program within a state, including the extent to which it disproportionately affects lower-income areas, but also to explore its implications for firms' allocation of activity across states. Thus, we evaluate the effectiveness of the CCTC in achieving its goals of increasing business activity and employment in California as well as provide new visibility into the broader implications of interjurisdictional tax competition over businesses.

We find that the CCTC program induces business expansions in California. Our reduced-form estimates suggest that, relative to applicant firms with scores that just fail to meet the cutoff, applicant firms with scores that meet the cutoff experience employment growth within California of between 11% and 32% within three years. The range of estimates reflects several alternative empirical approaches that trade off sharper identification around programmatic eligibility cutoffs against further exploiting the panel structure of the data and accounting for repeat applicants. Our preferred estimate of a 30% increase in employment translates into an elasticity of local labor demand with respect to state corporate income taxes of -7.6. The estimated effects are largely driven by expansions among existing businesses, but also to a lesser extent new sitings in the state. Consistent with the program's structure, which incentivizes but does not require businesses to locate in higher-poverty areas in California, we also find evidence that the CCTC induces growth among firms in both lower- and higher-income parts of the state, though our estimates of the program's impacts in more distressed areas are less precise and admit both null and large positive effects.

Meanwhile, we do not find strong evidence of spillovers to other states. That is, employment gains in California among eligible firms do not appear to be offset by appreciable employment reductions in other parts of the country. This finding is robust across a range of specifications. It also holds not only for the full sample, but also for the subset of firms whose operations span multiple states at baseline, which arguably have more scope to reallocate resources across space. We similarly find no indication of firm-wide revenue costs of reallocation; if anything, revenue and labor productivity rise in response to CCTC award eligibility. Thus, our combined evidence suggests that the program is inducing business growth that would not have otherwise happened elsewhere, with no clear indication of allocative inefficiency.

Our findings are consistent with the CCTC operating as a *de facto* industrial policy, effectively lowering the costs of capital and labor for specific firm expansions without prompting reallocation of existing activity from other locations. The coupling of investment credits with payroll subsidies potentially makes the CCTC particularly effective relative to other programs offering incentives to firms locating in particular places. The CCTC explicitly targets firms with pre-existing capital growth plans – firms that may be amenable to growing even larger. Furthermore, the design features of CCTC include better targeting and enforcement of large and new capital investments for which the tax advantage is material, and labor requirements sizable. This tax advantage appears to be salient for firm expansions, but not large enough to offset the potential costs of reallocating existing activity from other states to California. The program's tilt toward new projects may also help explain why we do not find firms to be as footloose as in previous work (e.g., Fajgelbaum et al. (2019), Giroud and Rauh (2019)).

We contribute to a growing literature on government programs aimed at encouraging business locations and expansions in particular areas. Bartik (2019) documents the rapid growth in state and local tax incentives for businesses in recent decades, estimating that total expenditures on such incentives have roughly tripled since the 1990s. Many have questioned whether these expenditures can be justified. A major concern is that some recipients of tax breaks or subsidies might have engaged in some hiring or investment even in the absence of incentives. Prior research suggests that local job creation and investment subsidies offered through place-based policies are frequently associated with substantial windfalls (Neumark and Simpson, 2015). A smaller and more nascent empirical literature on the impacts of discretionary subsidies tends to point to more positive effects on job creation and economic activity more generally (Greenstone and Moretti, 2003; Greenstone et al., 2010; Patrick, 2016; Bloom et al., 2019; Criscuolo et al., 2019).[2] Many of these studies focus on local economic outcomes as opposed to individual firm behavior. Among such studies, Freedman et al. (2023) use a difference-in-differences approach to document the effects of CCTC awards on employment at the census tract level. They provide evidence of a significant local multiplier, but are unable to document individual firm responses or examine broader reallocation associated with the CCTC. Using an RD design, we contribute new estimates of the impacts of discretionary subsidies on firm behavior, providing rigorous evidence on what state job creation policies may be able to achieve when well designed.

We additionally contribute to the literature on local policy choice and interjurisdictional subsidy competition (Agrawal et al., 2022). While "bidding" for firms need not necessarily be a

[2]This is particularly true for research focused on discretionary incentives for R&D (Bronzini and Iachini, 2014; Cerqua and Pellegrini, 2014; Howell, 2017; Chen et al., 2021; Santoleri et al., 2022; Dechezleprêtre et al., 2023). Our results also relate to a larger literature on firm responses to different state tax environments (Bartik, 1985; Helms, 1985; Papke, 1991; Bloom et al., 2013; Gale et al., 2015; Suárez Serrato and Zidar, 2016; Fajgelbaum et al., 2019; Giroud and Rauh, 2019).

zero-sum game (Black and Hoyt, 1989; Bartik, 1991; King et al., 1993; Glaeser, 2001), concerns that firm-specific subsidies are predominantly beggar-thy-neighbor policies have prompted calls for outright bans on business incentives in the U.S. (Meyer, 2011; Story, 2012; Badger, 2014; Markell, 2017; Farren and Mitchell, 2020) or at least greater coordination across state governments (Randall, 2019). Recent theoretical and empirical work on the extent to which subsidy competition enhances allocative efficiency has reached mixed conclusions (Kim, 2020; Mast, 2020; Slattery, 2022; Ferrari and Ossa, 2023).[3] Our work speaks to this literature by examining intra-firm reallocation of activity in response to subsidies affecting some business locations and not others. Our results are also of particular relevance to modern industrial policy, as the firms that contribute most to identification in our analysis – those considering expansions – represent the primary target of many governmental job creation and economic development efforts.

2 The California Competes Tax Credit

The California Competes Tax Credit (CCTC) represents one of a new generation of business incentive programs that attempt to overcome limitations identified in previous efforts to encourage local business hiring and investment. The CCTC program replaced California's Enterprise Zone (EZ) program as the state's principal economic development tool in 2013. The CCTC is available to businesses in all industries that would like to locate in California, or to stay and grow in the state. Under the CCTC, businesses apply to the California Governor's Office of Business and Economic Development (GO-Biz), specifying the amount of state corporate income tax credits they need in order to achieve specific commitments for net new hiring and investments in California. During each CCTC allocation period, tax credits are awarded based on a two-phase competitive process. As we describe below, this process lends itself to an RD approach to estimating the program's impacts.

Phase I Review. The first phase relies on a quantitative evaluation of the projected costs and benefits of the tax credits requested by an applicant. For each application, a cost-benefit ratio "score" is calculated by dividing the amount of tax credit requested by the five-year cumulative sum of proposed new employee compensation and capital investment by the applicant in the state.[4]

[3] A larger literature dating back at least half a century considers the nature and implications of interjurisdictional tax competition, in which governments in a federation strategically choose tax rates on a potentially mobile tax base (Oates, 1972; Zodrow and Mieszkowski, 1986; Wilson, 1986; Chirinko and Wilson, 2008; Egger et al., 2010; Chirinko and Wilson, 2017).

[4] Qualified capital investment includes depreciable assets "related to the project," such as structures and equipment.

Formally, an applicant i's score in a given allocation period (of which there are three each fiscal year) is calculated as

$$Score_i = \frac{Credits\ Requested_i}{Payroll_i + Investment_i} \tag{1}$$

Within each allocation period, program administrators rank the applicants by scores, from lowest to highest. They then impose a cutoff for the first phase of the review process by moving up the cost-benefit distribution (starting at the lowest score) until the cumulative credits requested among all included applications is two times the budgeted amount for that allocation period.[5]

Thus, two factors determine each allocation period's first phase score cutoff, and consequently each applicant's distance to the score cutoff: (1) the composition of applications in the current allocation period (for example, an application with a large credit request but low score could exhaust most of the budget, resulting in a lower cutoff) and (2) variation in the budget for each allocation period, which is partly a function of unallocated carry-overs from previous period budgets. The score cutoffs and applicant pool sizes in each allocation period as well as fiscal year budgets are illustrated in Figure 1. Importantly, applicants have no way to precisely manipulate their cost-benefit ratio with respect to this cutoff because the cutoff depends on the distribution of all applications received as well as the content of those applications, which are unknown to any given applicant.

Phase II Review. The second phase of review involves a more comprehensive evaluation of each application whose score falls below the first-phase cutoff, with program administrators selecting among score-eligible applications (as well as some exceptions discussed below) those that are qualitatively most consistent with program goals. In particular, during this phase program officials prioritize applicants that they identify as likely to be marginal with respect to growth in the state, and thus most responsive to tax credit awards. They also consider the strategic importance of the firm to innovation in the state and other qualitative information regarding the proposed project.[6]

Certain types of applicants are automatically advanced to this second phase irrespective of their first phase score. These include businesses whose CEO/CFO certifies that the project would

It does not include inventory, prepaid expenses, or raw materials (California Code of Regulations, Title 10, §8000).

[5] In early years of the program, there was a set-aside for small businesses (those with annual revenues less than $2 million) such that there were separate cutoffs for large and small firms. As we discuss further in Section 3.1, we restrict attention to large firms in our analysis since there was little to no mass to the right of the small business score cutoff in many allocation periods.

[6] This discussion of selection criteria is based in part on conversations with GO-Biz staff, but is also reflected in the public minutes of CCTC Committee deliberations.

occur in a state other than California absent the credit or that the business would terminate employees (shutting down or downsizing operations) and/or relocate employees to another state absent the credit.[7] Starting in calendar year 2017, applicants that attest that at least 75% of their full-time net employment expansion will occur in a program-designated high-poverty or high-unemployment California city or county also automatically advance; prior to 2017, applicants promising job growth in such areas were only given preference in the discretionary review phase. Program-designated high-poverty or high-unemployment areas encompass roughly 33% of ZIP codes in the state, many of which are in California's Central Valley (its interior). These designated areas largely exclude major urban centers and thus cover only about 30% of the state's population.[8] While most CCTC applicants list a candidate site for hiring and investment that would be supported by the CCTC, they are not contractually bound to invest in that specific location (as opposed to other locations in California) in order to receive the credits. However, if an applicant commits to investing in a high-poverty or high-unemployment area, they must ultimately do so in order to claim their credits, although any location in a high-poverty or high-unemployment area will generally be deemed acceptable.

The second, discretionary phase of application review can include clarifying discussions with applicants. Agreements are then negotiated to finalize specific requirements or milestones that must be met to claim the tax credits. These agreements are subsequently either approved or rejected by the CCTC Committee in a public meeting.[9]

Businesses can apply for the CCTC multiple times. During our sample period, 34% of applications were from firms that applied more than once (although not necessarily for the same establishment). As we discuss further in Section 4, we implement methods to account for firms' application histories in estimating the impacts of the tax credits.

Tax Credit Contracts. Tax credits under the CCTC are non-refundable (precluding negative

[7] In this case, the applicant is required to submit declarations that affirm their claims and provide additional details during the second stage of the review process. Misrepresenting the business's situation or intentions in the first phase of review could be grounds for rejection in the second phase.

[8] GO-Biz was able to provide these lists for allocation periods starting in 2017 (after which locating in a high-poverty or high-unemployment area led to automatic advancement). A sample list can be found in Appendix Figure A.1. The composition of these areas (for counties) relative to others in 2010 (pre-CCTC) is documented in Appendix Table A.1. Appendix Figure A.2 illustrates all ZIP codes in California that were ever in a designated high-poverty or high-unemployment city or county under the program.

[9] As discussed further in Appendix B, CCTC Committee deliberations provide further insight into officials' objectives and concerns with the awards. However, nearly all of the contracts put forward by GO-Biz are ultimately approved by the Committee; during our sample period, only one contract advanced by GO-Biz was rejected by the CCTC Committee.

liabilities) and non-tradable, but have a six-year carryforward.[10] Contracts with the state are for five years, but companies commit to meeting yearly milestones in terms of job creation and capital investment relative to an initial benchmark. The contracts also stipulate minimum and average annual salaries for full-time employees hired. If awardees do not meet their annual milestones, they are not permitted to claim tax credits for that year, although they can claim them in future years if they catch up and meet subsequent milestones.[11]

As one example, Tesla Motors Inc. was the largest tax credit recipient during our sample period. On a baseline of about 6,500 employees in 2014, Tesla committed to a roughly 70% increase in employees to about 11,000 workers after five years. In the case of Tesla, this was accompanied by substantial planned investments – about $2.4 billion over the same period.[12] Many of the largest credit recipients in our sample, including Tesla, were courted by other states as they considered expansions. However, applicants without credible options outside California could receive credits if they met the relevant first-phase cutoff and convinced program officials that subsidies were essential to meeting expansion targets within California.

Compliance is monitored by the California Competes office, which requires annual reports from participating businesses, and by the Franchise Tax Board (FTB), which conducts audits and can recover tax credits if companies fail to meet reporting requirements. Some companies have voluntarily surrendered their tax credit allocations because they did not anticipate being able to meet their goals, and the FTB has also required some noncompliant firms to return previously claimed credits (by amending prior years' tax returns and payments). Credits can also be clawed back by the state if businesses do not maintain the jobs created for an additional three years after the five-year contract concludes.[13]

[10] C-corporations can apply credits in full to outstanding state corporate tax liabilities, while S-corporations (including pass-through entities) can only apply one third of credits to liabilities. California's corporate tax rate for most of the sample period was a flat 8.84%, at about the 85th percentile of top marginal corporate tax rates across states during the time frame of our analysis (Scarboro, 2017).

[11] Given that falling even marginally short of a contractual milestone precludes an awardee from claiming tax credits, firms may also be conservative in setting milestones in order to ensure they can meet them. This can lead to a high ratio of created jobs vs. promised jobs.

[12] See Appendix C for additional details on Tesla's contractual milestones. Some applicants plan new sitings at the extensive margin. For example, the health insurance company Centene Corporation (whose contractual milestones are also shown in Appendix C) detailed in their application plans to construct a new 70-acre, 5,000-employee headquarters near Sacramento in exchange for approximately $7 million in tax credits. Other awards are aimed at inducing firms to stay in California instead of moving to another state, including for example one to the audiovisual equipment maker Yamaha Corporation in 2016 for $3 million in tax credits.

[13] In Appendix Table D.1, we estimate an aggregate recapture rate of 37% for our sample period, on a denominator of "offered credits." However, the density of recapture rates shows that the most common outcome is no recapture at all (see Appendix Figure D.1).

3 Data and Policy Timing

3.1 CCTC and LBD Data

We obtained confidential data on CCTC applicants and awardees from the California Governor's Office of Business and Economic Development (GO-Biz). We have complete application information, including the ingredients necessary to construct applicant scores. These are recorded at the time of application. There were approximately 3,800 total CCTC applicants during the allocation periods we consider in our analysis, which include allocation periods between fiscal years 2014-15 and 2017-18.[14] Additionally, the CCTC data contain each awarded applicant's five-year annual milestones for employment, wages, and investments. Table 1 shows the top 20 recipients of tax credits by size of award for the allocation periods we consider in the analysis, and highlights how planned employment growth is often correlated with large expansions in capital investment.[15]

We integrate the CCTC applicant and awardee data with confidential establishment-level microdata from the Longitudinal Business Database (LBD), matching on Employer Identification Numbers (EINs).[16] EINs are federal tax identification numbers used by business entities (firms) to file taxes. We also used legal business names, applicant addresses, and proposed locations of activity (especially useful in the case of new companies) to match applicants. Importantly, we first match each applicant to a single "LBDNUM," the physical establishment identifier in the LBD. The LBDNUM remains constant even if EINs change before or after the establishment is matched (e.g., due to mergers or acquisitions). We then aggregate LBD-measured activity across establishments within matched firms to different geographies, including sub-state, state, and national levels. Our main outcomes of interest thus reflect applicant EIN-year activity measured across various geographies. Measuring outcomes at the firm level ensures that the applicant's response is broadly captured at one of the firm's establishments, whereas assigning each applicant to a particular establishment risks missing the relevant activity (such as in the case of new establishments).

Our LBD data end in 2019. We consequently limit our study to CCTC allocations through calendar year 2017 in order to have at least three years of post-allocation data for each applicant.

[14] The next subsection describes allocation period timing in more detail. We drop the single allocation period that occurred in fiscal year 2013-2014 because of missing information on some applicants.

[15] Appendix Table D.2 shows the top 20 award recipients by size of award between 2014 and 2021.

[16] Per our data use agreement, GO-Biz shared EINs with the research team via a secure encrypted transfer, and the integration with LBD was conducted on a secure server in a Federal Statistical Research Data Center. We describe the matching process in detail in Appendix E.

While the LBD data begin in 1976, we keep only data five years before each applicant's allocation year, such that the earliest year of LBD data we use is 2009. We additionally exclude from our sample small firms (those with revenues less than $2 million annually) because, due to set-asides for small businesses in earlier years, small businesses were subject to different cutoffs that frequently did not bind. We match nearly all large CCTC applicants across the ten allocation periods we consider. Our final sample consists of approximately 1,600 to 1,800 applicants depending on the time period analyzed.[17]

The LBD provides rich information on annual establishment employment (the stock as of March 12) and payroll as an annual flow (Jarmin and Miranda, 2002; Chow et al., 2021).[18] By allowing us to identify all other establishments operating under the same EIN as a CCTC applicant, the LBD also permits us to trace potential substitution of activity across establishments in the state, and nationwide, in response to receiving an award. The LBD also provides information on total firm-level revenue, allowing us to study whether tax credit awards in California translate into a broader firm-level expansion, or whether any CCTC-induced relocation has allocative costs.[19] However, revenue is a national firm-level measure, as the LBD does not apportion revenue across individual establishments.

Given the latitude applicants have in choosing locations in which to invest, and the CCTC's primary objective of bringing employment and investment to California as a whole, our empirical analysis begins with a focus on statewide employment, payroll, and establishment outcomes for firms. We supplement this with sub-state analyses (namely high-poverty and high-unemployment California ZIP codes), which speak to distributional implications of the program within California. We then study national employment, payroll, and establishment changes, which shed light on the program's reallocation implications at a national scale. Finally, we consider the firm-wide revenue effects of the CCTC.

3.2 Policy Timing

There are three periods of CCTC applications (and awards) per 12-month period, which are dated by fiscal years. Each fiscal year begins in July of the previous calendar year. We refer to each period

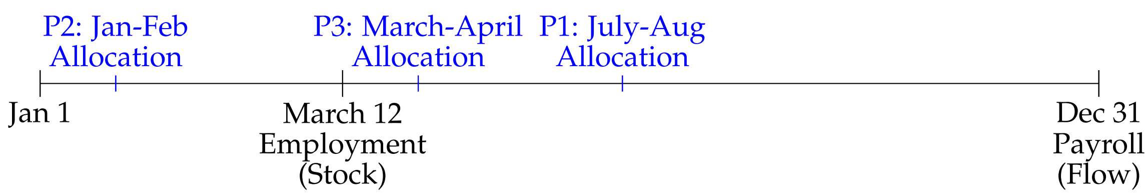
[17] See Appendix E for further details. The reported numbers of applicants are rounded for confidentiality reasons, and vary due to new entrants later in our sample window.

[18] We deflate payroll by the Consumer Price Index using 2010 as a base year throughout the analysis. Unfortunately, the LBD does not contain information on specific employees, nor are we able to link the LBD to other records that would provide such information (e.g., the LEHD, as in Hyman et al. (2022)). This precludes a deeper examination of the CCTC's incidence and distributional implications for workers.

[19] Revenue is provided in both nominal and real terms, where the latter is PPI-deflated using 2009 as the base year.

of application (and awards) within a fiscal year as an "allocation period." We restrict attention to allocation periods that allow us to observe applicants for at least five years before and three years after applying (with the latter including the application year). These specifically include the ten allocation periods in the shaded region of Figure 1.

The first allocation period of each fiscal year (P1) is in the calendar year before the second and third allocation periods (P2 and P3). The timing of the allocation periods is relevant because we have to align the CCTC application and award data with the LBD data. As illustrated in the following figure, the LBD payroll data are year-end calendar measures of total payroll expended, whereas the employment data are as of March 12 of each calendar year.



Since the LBD reports payroll as an annual flow, the effects of CCTC incentives on payroll could be reflected in the same calendar year as an award. For example, if credits are allocated in August 2015, payroll effects could be evident in the end of calendar-year 2015 LBD data. However, since employment is captured only as the stock as of March 12 each year, the effects of this allocation on employment would not appear until the 2016 data.

We thus define τ as the event year, measured relative to the calendar year of the allocation period (which is defined as $\tau = 0$). For employment, an effect of a period 1 (generally July-August) allocation will show up (if there is an effect) at $\tau = +1$ (the following March). An effect of a period 2 (January-February) allocation may very partially show up at $\tau = 0$ (March of the same year), but mainly at $\tau = +1$ (the following March). An effect of a period 3 (March-April) allocation will not show up until $\tau = +1$ (the following March). Meanwhile, for payroll, we would expect to see any effect, if present, at $\tau = 0$. About half of a period 1 allocation will show up (if there is an effect) at $\tau = 0$ (the end of the same calendar year). And period 2 and 3 allocations will nearly fully show up at the end of the same calendar year.[20]

In the main analysis, we focus on the cross-section in $\tau = +2$, which effectively captures employment and payroll changes that occur 2 to 3 years after the relevant allocation period. As a placebo test, we probe for effects at $\tau = -2$, or about two years before the relevant allocation year.

[20] We further discuss the policy timing and exposure information in Appendix F.

We also show the complete dynamic path of estimates over event time, ranging from $\tau = -5$ to $\tau = +2$.

4 Empirical Framework

In this section, we describe our methods for estimating the effects of the CCTC on applicant outcomes. We adopt two approaches, each discussed in more detail below. The first, "pooled" RD approach identifies effects exclusively off of applicants close to allocation round-specific score cutoffs. The pooled RD approach is closest to a classic RD design, and thus has transparent identifying assumptions and a well-understood interpretation. However, it is a data-hungry local method that, in our relatively small sample, requires additional composition controls to achieve as-good-as random covariate balance and produces results that are to some degree sensitive to bandwidth selection approaches. It also ignores possible bias introduced by the presence of repeat applicants; an applicant in a given period may have previously had or will in the future have experience with the program, which in turn could affect estimates at different event times.[21]

Therefore, we additionally consider an alternative "dynamic" RD approach, following a methodology proposed by Cellini et al. (2010).[22] The dynamic approach is similar in spirit to a multiple treatment exposure difference-in-differences design, which adjusts for potential confounding effects of including score-eligible firms that in a later period are ineligible, or conversely, score-ineligible firms that in a later period are eligible. This approach also includes EIN fixed effects, which subsume time invariant characteristics of firms in the sample. However, in contrast to the pooled RD approach, this methodology puts non-trivial weight on applications at the tail ends of the score distribution away from the cutoffs. In practice, the two methods produce qualitatively similar results.[23] We use our empirical estimates from both our pooled and dynamic

[21] For example, program effects might be attenuated in the presence of repeat applicants if an initially rejected applicant is subsequently awarded a tax credit (biasing the control group toward treatment outcomes), or if an awardee is subsequently rejected (biasing the treatment group toward control outcomes). Furthermore, endogenous learning close to the RD cutoff may threaten RD validity; we explicitly test for such learning and find little evidence of it in Appendix Figure D.5 and Appendix Figure D.6.

[22] Pérez Pérez and Suher (2022) also use an approach similar to that of Cellini et al. (2010) to study a North Carolina hiring incentive program, although in their case outcomes are measured at the county level and what potentially repeats is county eligibility for the program.

[23] One feature of our context is that the score cutoff varies substantially across allocation periods (see Figure 1). Therefore, by virtue of recentering applicants' scores and pooling allocation periods, we estimate an applicant-weighted average of intent-to-treat effects over a range of score values with our RD approaches. While the sparsity of our data and disclosure limitations preclude estimating the extent of heterogeneity in intent-to-treat effects at different points in the applicant score distribution, our estimates are not "local" to a specific score but rather capture effects for an array of scores.

approaches as inputs into tax elasticity calculations, which we discuss in Section 7.

4.1 Pooled RD Estimator

We define event year τ as calendar year of the data minus the applicant's allocation calendar year (as discussed in the previous section). We allow i to index applicants and a allocation periods. Let $score_i$ be applicant i's score, $c_{a(i)}$ be the cutoff for advancement to the second-phase of application review in applicant i's allocation period a, and s_i be the difference between an applicant's score and the relevant cutoff score (i.e., $s_i = score_i - c_{a(i)}$). Additionally, let b_i be an indicator for applicant i having a score below the allocation period a's cutoff (i.e., $b_i = \mathbb{1}(s_i \leq 0)$), and $f_g(s_i)$ be a polynomial term of degree g in the (recentered) applicant score that allows for separate polynomials on either side of the cutoff. Finally, let μ_a represent allocation period fixed effects and $\mathbf{X}_i$ represent a vector of time-invariant applicant characteristics. Then, each outcome $y_{i\tau}$ for applicant i in event year τ is modeled as

$$y_{i\tau} = \alpha_\tau + \beta_\tau b_i + f_g(s_i) + \mu_a + \mathbf{X}_i \Omega_\tau + \varepsilon_{i\tau} \tag{2}$$

The inclusion of allocation period fixed effects μ_a ensures that estimates of β_τ are identified off of variation within allocation periods – a restriction that effectively controls for any cross-allocation period differences in the composition of applicants close to each score cutoff, which may vary due to the data generating process discussed previously. We include in $\mathbf{X}_i$ a vector of industry (two-digit NAICS) dummies and, in some specifications, an indicator for single-unit (vs. multi-unit) status, an indicator for being an S-corporation, an indicator for C-corporation, and an indicator for whether the firm is publicly traded.[24] These controls are intended to account for compositional differences across periods that arise due to small samples.[25,26] For comparison,

[24] Our industry codes are 2012 vintage NAICS codes associated with the "focal" establishment of the firm as identified in the LBD (see Appendix E). Entity type controls help reduce noise by making tighter comparisons within C-corporations (eligible for 100% of credits) and S-corporations (eligible for only 33% of credits if a pass-through entity).

[25] We clarify the influence of small samples in Appendix Figures D.7-D.9 by reproducing the exact process by which GO-Biz determines cutoffs in each allocation round (i.e., cumulating the credits requested from lowest to highest first-phase score until twice is budget is met). Ideally, we would observe many small steps in the empirical CDFs near each cutoff. In most allocation periods, this is exactly what we see. However in a handful of periods, there appear to be larger steps (presumably owing to quite small samples within each period), warranting the inclusion of controls.

[26] We follow Lee and Lemieux (2010) and work with the residuals of $y_{i\tau}$ after regressing on the controls. Under the Frisch-Waugh-Lovell theorem, this two-step procedure is equivalent to estimating equation (2), assuming that the covariates are asymptotically uncorrelated with the RD terms. Lee and Lemieux (2010) also show that standard errors are equivalent using this two-step procedure. Calonico et al. (2019) provide a method to directly estimate equation (2) including additional covariates. However, Calonico et al.'s (2019) method does not accommodate using bin scatterplots to assess visual fit with covariates, as the controls shift the bin means.

we also consider sparser models that include limited sets of controls (or no controls) and find qualitatively similar results. We use the same estimating equation for first-stage estimates of the effect of having a score below the allocation period-specific cutoff on the probability of a CCTC award.

For each outcome in the tables, we report the estimated discontinuity ($\hat{\beta}_\tau$), heteroskedasticity-robust standard error, and the control group mean ($\hat{\alpha}_\tau$).[27] We follow Calonico et al. (2014), who use an IMSE-optimal bandwidth that trades off "smoothing bias" and variance, as IMSE is linearly separable in each term.[28] Their approach allows for an asymmetric bandwidth on each side of the cutoff, choosing both the left- and right-side bandwidth boundaries to minimize IMSE. We consider both "fixed-bandwidth" specifications in which the IMSE-bandwidth is invariant to the choice of covariates as well as "varying-bandwidth" specifications in which we allow the IMSE-optimal bandwidth to vary with controls.[29]

As most outcomes appear to be best approximated by a linear fit, and following Gelman and Imbens (2019), we use a first-order polynomial in our pooled RD specifications. However, we allow the slope to vary above vs. below the cutoff.[30] Calonico et al. (2014) note that while the choice of bandwidth (such as IMSE-optimal) can have material effects on point estimates, the type of kernel weight chosen is generally inconsequential when using an IMSE-optimal bandwidth. We use a triangular kernel in which linear weights are applied from 0 to 1 from the bandwidth boundary to the cutoff.

4.2 Dynamic RD Estimator

The pooled RD method leverages variation only among a relatively small set of applicants close to their allocation round-specific cutoffs, and thus exhibits some sensitivity to compositional controls and bandwidth selection approaches. It also ignores the possible influence of firms that apply in multiple allocation periods, whose prior or subsequent experience with the CCTC could contaminate estimates of the dynamic treatment effects. In order to address concerns related to compositional issues and repeat applicants, we adopt the "one-step" dynamic RD method developed by Cellini et al. (2010). As described below, Cellini et al.'s (2010) one-step RD estimator

[27]In equation (2), when $score_i > c_{a(i)}$ (i.e., $s_i > 0$), α_τ is the intercept of the polynomial (i.e., the mean of the data just to the right of the cutoff).

[28]We thank Gray et al. (2023) for sharing their initial RD code.

[29]Using a fixed-bandwidth facilitated FSRDC disclosure of results with different sets of controls by limiting the number of samples for release.

[30]Formally, in equation (2), $f_g(s_i) = \omega_{1\tau} s_i + \omega_{2\tau} b_i s_i$.

uses the full sample and incorporates EIN fixed effects; the latter account for time invariant differences across firms and hence help address any concern that pre-treatment level differences between applicants above and below round-specific cutoffs could be driving the observed effects. Their methodology also dynamically controls for past treatment events among repeat applicants, effectively accounting for an applicant's entire application history in estimating treatment effects. Following their approach, we create a panel dataset in which each EIN appears once each year.[31] We let $p_{i,t-k}$ be an indicator that takes a value of 1 when EIN i applies for a tax credit in time $t-k$ (where t denotes calendar year), and $b_{i,t-k}$ be an indicator that takes a value of 1 when EIN i's score at time $t-k$ is below the cutoff for the relevant allocation period. Following Cellini et al. (2010), we include EIN fixed effects θ_i and year fixed effects η_t.[32] We estimate the model

$$y_{it} = \sum_{k=-5}^{2} \left(\psi_k p_{i,t-k} + \pi_k b_{i,t-k} p_{i,t-k} + p_{i,t-k} f_g(s_{i,t-k}) \right) + \theta_i + \eta_t + e_{it} \tag{3}$$

The outcome in this case, y_{it}, is expressed as a function of the entire history of applications, such that π_k will reflect the impact of having a score below the cutoff k years earlier, controlling for any interceding allocations in other years. Put differently, π_k reflects differences in outcomes for EINs that applied in year $t-k$, identifying off of variation in those outcomes between applicants below vs. above the score cutoff but with similar histories of applications and scores. When k is positive, the π_k terms capture the effects of past program interactions on current outcomes; when k is negative, the π_k terms reflect the effects of future program interactions on current outcomes (i.e., they represent falsification estimates). Mirroring our approach with the pooled RD, we specify $f_g(s_{i,t-k})$ as a linear polynomial with coefficients that are allowed to vary on either side of the score cutoff. In these regressions, we cluster standard errors at the EIN level.

An advantage of Cellini et al.'s (2010) approach is that it permits us to isolate the impact of CCTC eligibility in time $t-k$ on firm outcomes measured at time time t, removing the potentially confounding influence of any intermediate applications. By necessity, the methodology involves including all applicants in all years, including ones with scores far from the cutoff. In that sense, the Cellini et al. (2010) method shares features of a difference-in-differences design. Equation (3) includes controls for the score that help absorb variation from firms with scores at the tail

[31] In instances when an EIN applies more than once in the same year, we keep the application where the score was below the relevant cutoff (if there was such an application) and otherwise the application where the score was closest to the relevant cutoff.

[32] Note that the EIN fixed effects subsume the characteristics we include in $\mathbf{X}_i$ in our pooled RD approach.

ends of the score distribution, but may not fully account for differences between those further from the threshold relative to those closer to the threshold. Therefore, relative to the pooled RD design, which arguably better exploits the exogenous variation generated by the score cutoff for identification by restricting attention to applicants with scores close to the threshold but does not account for repeat applicants, we view the dynamic RD approach as providing an alternative treatment effect estimate that leans more heavily on parametric assumptions but addresses repeat applicants and uses more data.

5 Descriptive Results and Validity Checks

5.1 Descriptive Results

We begin with some descriptive results on the evolution of employment for awardees and non-awardees as well as applicants below and above the score cutoff, before and after the application date. We do so based on the event year τ as defined above. For these descriptive figures, we analyze applicants' total firm employment in California.

In our main results that follow in Section 6, we lean on the more credibly exogenous variation afforded by the score cutoff to identify the effects of the CCTC on a broader array of establishment and firm outcomes, including outcomes measured across different geographies. The figures in this section instead serve two purposes: (1) they help characterize levels and trends of employment among applicants, and (2) they motivate an RD approach that exploits applicants' scores relative to the cutoff. They additionally suggest that possible alternative empirical strategies, such as a simple difference-in-differences approach that compares outcomes for award winners and losers, would tend to generate biased estimates of the program's impacts.

Panel (a) of Figure 2 shows raw means for California-wide employment for awardees and non-awardees. Panel (a) indicates that, based on applicants' total employment in California, those ultimately awarded tax credits under the CCTC program are smaller than those not awarded credits – about 400 employees for awardees compared to 500 employees for non-awardees. Taken at face value, panel (a) also suggests that despite CCTC administrators' efforts to screen out applicants that would likely grow absent incentives, there is also some indication of a relative increase in employment among awardees two years prior to the time of application. The stronger pre-application growth for awardees points to potential differences that might, if anything, lead a difference-in-differences strategy using the pre-post contrast between awardees and non-awardees over the time span shown to overstate the effects of the awards themselves.[33]

Rather than comparing outcomes for awardees and non-awardees, we take advantage of the identification afforded by the rule-based design of the program. To shed some light on the difference this makes, panel (b) of Figure 2 shows employment measured over event time for all applicants with scores below vs. above the cutoff for proceeding to the second phase of review. In this case, there is less evidence of any differential pre-trend in California employment, presumably due to a greater portion of the variation being randomized when relying on the cutoff. In contrast to the results for awardees and non-awardees in panel (a), in panel (b) there is an indication of a relative increase in employment among those with qualifying scores (below the cutoff) as compared to those with non-qualifying scores (above the cutoff) in years following the application year.

Notably, panel (b) of Figure 2 shows a pronounced level difference in employment among applicants with scores above vs. below the score cutoffs in general. This is partly mechanical – the larger the applicant's proposed employment growth in levels, all else equal, the lower the applicant's score. In our main RD analysis, we narrow in on applicants whose scores were local to the cutoff for their respective allocation period. To illustrate the effects of using our pooled RD approach, in panel (c) of Figure 2, we show trends using the bandwidth, triangular weights, and controls (residualizing) from our fixed-bandwidth RD specification for employment in California. As expected, this eliminates the pre-application imbalance in firm size. It also makes more apparent the post-application divergence in California-wide employment among CCTC-eligible and ineligible firms. Building on this, in what follows we take further advantage of the threshold for application advancement in the review process to credibly identify the causal effects of the program on firms.

5.2 Tests for RD Identifying Assumptions

The identifying assumption for the RD analysis to recover a causal effect is that factors that might influence selection and potentially relate to post-award employment growth are continuous at the score cutoff. This is what allows us to control for endogenous selection based on future firm

[33]In Appendix Figure G.1, we show employment for awardees and non-awardees focusing on activity in the applicant's proposed location ZIP code. Consistent with Freedman et al.'s (2023) finding of significant employment effects of the CCTC measured at the tract level using the American Community Survey, this figure points to a meaningful treatment effect associated with an award. However, ZIP code-level estimates could miss important program-induced activity elsewhere in California. Additionally, they do not capture any intra-firm reallocation of activity within the state in response to the program.

growth. While we cannot directly test this assumption for unobservables, we can examine whether predetermined firm characteristics evolve smoothly through the cutoff. This is direct evidence for observables, and suggestive evidence for unobservables.

We test for pre-treatment discontinuities for a large set of applicant- and firm-level characteristics, including the outcomes of interest. We measure these characteristics either at the time of application (for variables drawn directly from firms' applications) or two years prior to the application year (for outcome measures drawn from the LBD). We use the fixed-bandwidth pooled estimator described above and present results from these tests in Table 2. Each discontinuity estimate in this table corresponds to a $\hat{\beta}$ from a separate RD regression of equation (2) prior to treatment. The fixed-bandwidth specification includes industry and allocation period fixed effects as well as baseline controls (single-unit vs. multi-unit status, an indicator for being an S-corporation, an indicator for C-corporation, and an indicator for whether the firm is publicly traded). The choice of controls for the fixed-bandwidth approach is informed by small sample compositional differences across industries and allocation periods; these help to achieve balance on baseline covariates, but as we discuss in the next section, their inclusion is not necessary to achieve statistical significance in the main results, and if anything, attenuates our estimates.[34]

We first focus on control means, which indicate that the typical firm near the cutoff in our sample applied for $795,500 in tax credits. About one-third of applicants indicated that they would terminate employees or leave the state absent the credit, underscoring the marginality of the sample.[35] As of two years prior to application, the average CCTC applicant in our sample had 455 employees across 4.40 establishments within California, and 1,973 employees across 20.32 establishments outside California. Particularly in the case of activity outside California, the averages are influenced by a small number of very large firms; for example, while average payroll outside California is nearly four times that inside California, the share of payroll outside California for the typical firm in our sample is only 15%.

Turning to the estimated discontinuities in the first column of Table 2, we find very little statistical evidence of discontinuities in baseline applicant and firm characteristics at the score cutoff. The most significant estimated effects are for inputs into the score itself (projected compensation and projected investment), which are mechanically different on either side of the

[34] We show parallel results for baseline application characteristics (panel (a) of Table 2) using different specifications with and without controls in Appendix Table G.2. We show complete results for pre-application outcomes (panel (b) of Table 2) in Appendix Table G.4.

[35] In Appendix Table D.1 and Appendix Figure D.2, we show further summary statistics on tax credits received. These reveal a pattern of increasing mean credit receipts over time, driven by the largest recipients.

cutoff and therefore, in a small sample, could create spurious evidence of a discontinuity. The estimated discontinuity for projected compensation is also sensitive to our bandwidth selection approach, becoming substantially smaller and statistically insignificant in our varying-bandwidth specification that adjusts the optimal bandwidth based on the controls included.[36] As we show in subsequent sections, the time path of treatment effects after credit allocation is qualitatively and quantitatively similar regardless of the bandwidth selection method.

Pre-treatment applicant outcomes otherwise exhibit small and generally statistically insignificant discontinuities. For example, in our main specifications with logged outcomes, firms whose scores were below the cutoff (making them eligible for tax credits) had $\tau = -2$ employment in California that was a statistically insignificant 2% greater than firms with scores above the cutoff, and payroll that was a statistically insignificant 1% smaller. We also show graphical versions of these tests for discontinuities prior to applications for a subset of these outcomes in Figure 5 (which also show our core results for $\tau = +2$, as discussed later).[37] The results in Table 2 and accompanying figures point to no meaningful pre-treatment differences in our main outcome measures at the cutoff. The lack of discontinuities in these and other variables prior to applications supports the validity of our RD approach in this context.

Finally, we conduct a McCrary (2008) style test for continuity of the density of applicants at the score cutoffs. Specifically, in Figure 3, we present results from a manipulation test using a local polynomial density estimation that relies on optimal (data-driven) bandwidths and robust bias-corrected standard errors (Cattaneo et al., 2020). As in the analysis sample, we restrict attention to "large" applicants and the ten application rounds between 2014-15 period 1 and 2017-18 period 1.[38] Based on the histogram, there does not appear to be a spike just to the left of the score cutoff, nor an obvious missing mass to the right of the cutoff, which would be expected if applicants were able to manipulate their scores to pass the first phase of the review process. If anything, a spike appears on the wrong side (i.e., to the right) of the cutoff, though the increase between these two bins is well within the range of changes between any two neighboring bins

[36] See Appendix Table G.3 for a balance table analogous to Table 2, but using a varying-bandwidth. Additionally, in Appendix Figures D.10 and D.11, we find that the estimated discontinuities in projected compensation and investment at the actual cutoff are well within the range of discontinuities estimated at placebo cutoffs.

[37] Appendix Figure G.5 and Appendix Figure G.6 show graphical versions of these tests for discontinuities in outcomes measured in high-unemployment, high-poverty areas within California as well as outside California.

[38] We perform density tests on application data from GO-Biz accessed outside the FSRDC as we are not permitted to release discrete histograms using Census microdata as per disclosure rules. We additionally drop 1% of outliers (20 applicants) with unusually high relative scores in order to zoom in on a tighter window around the cutoff and produce more sensible histogram bins. Our final sample size for these figures is 2,004 applicants. In Appendix Figure D.3, we also provide the raw distribution of scores overlaid with each of the 10 cutoffs contributing to the pooled relative score cutoff.

across the histogram's support. The manipulation test's confidence intervals from the left and right of the cutoff almost entirely overlap, consistent with applicants' inability to precisely target the cutoff in any given allocation round. This again suggests that the assumptions underlying our RD approach hold.[39]

6 RD Results

6.1 First-Stage Results

We illustrate the first-stage RD for the probability of a CCTC award in Figure 4, where we show results from the fixed-bandwidth model adjusted for industry and allocation period fixed effects as well as baseline controls. Here, we see clear evidence of a discrete jump in the probability of an award as one passes from above to below the cutoff. Having a score just below the cutoff increases the probability of receiving a CCTC award by a statistically significant 16 percentage points, or 84%. That is, scoring just below the cutoff roughly doubles the award rate relative to scoring just above the cutoff (note that the award rate to the right of the cutoff is not zero due to automatic advancers). Using alternative sets of controls or bandwidth selection methods, we arrive at very similar estimates of the impact of score eligibility on the probability of an award, ranging from 14 to 20 percentage points.[40] Notably, though, the award rate among applicants to the left of the cutoff remains below 40%.

The pattern of awards observed as one moves away from the threshold is consistent with information CCTC program administrators provided, which suggests that during the second phase of the review process, they pre-screen infeasibly low and high scores at the very edges of the distribution. These would correspond to applicants that requested very little in tax credits for a large investment, or (among those automatically advancing) substantial tax credits for a small investment. This explains why the first-stage RD plot is upward sloping to the left of the cutoff, and mildly downward sloping to the right of the cutoff. The shape of the RD to the left of the cutoff also indicates that conditional on having a first-phase passing score, an *even lower* score has little marginal bearing on the ultimate receipt of an award. This suggests that while the first-phase

[39] In Appendix D, we present additional tests of our core identifying assumptions, including further checks for balance and manipulation (see Appendix Figures D.4-D.6).

[40] We report a complete set of first-stage estimates in Appendix Table G.1. These results are based on running equation (2) with alternative sets of fixed effects, controls, and bandwidth methods. We show results using as the dependent variable (i) the probability the applicant receives a CCTC award as well as (ii) the cumulative number of CCTC awards received by the firm. Note that when we discuss elasticities in Section 7, we also estimate the first stage in terms of the change in tax liability faced by each applicant.

score can shift the award rate, it has little informational value in the second phase of review, when other aspects of the application become more salient.[41]

6.2 Main Results

We first present results for firm activity in California as a whole, as the program's main objective is to attract and retain jobs in the state. We then consider whether the program is disproportionately impacting high-poverty and high-unemployment areas in California, as the application process favors firms that propose investments in such areas. This preference echoes the incentive structure in many place-based programs that explicitly target distressed communities, often under the assumption that subsidies to those areas are less likely to crowd out private investment and that positive externalities might be larger in those areas (Glaeser and Gottlieb, 2008; Bartik, 2019). Finally, we examine the effects of the tax credits for investment in California on firm activity outside California, which speaks to the aggregate consequences of state business incentives and interjurisdictional tax competition.

6.2.1 Activity in California

Graphical RD results of the effects of a just-eligible score for log firm employment, payroll, and establishments in California as of $\tau = +2$ (i.e., 2-3 years after the application date) appear in Panel A of Figure 5. There is a large and discrete jump in each of these outcomes at the cutoff, indicating that the program induced growth in the state among applicants whose scores rendered them just-eligible for the CCTC as compared to applicants whose scores rendered them just-ineligible. These plots are based on our fixed-bandwidth model with allocation and industry fixed effects as well as baseline controls; the jumps at the cutoff are even more pronounced when using less saturated specifications.[42] As shown in Panel B of Figure 5 (and noted earlier), there are no statistically significant discontinuities in these outcomes in the placebo period, two years prior to allocation ($\tau = -2$). Also of note, comparing the results in Panels A and B of Figure 5, there is a clear level shift upward in employment and payroll among applicants on both sides of the cutoff between $\tau = -2$ and $\tau = +2$, consistent with the CCTC attracting firms with expansion plans more generally. However, expansions in California tend to be discretely larger among those firms

[41] Moving forward, we present intent-to-treat estimates, consistent with the strategy of evaluating policy effects with imperfect take up.

[42] In Appendix Figure G.2, we demonstrate the underlying strong variation in outcomes around the RD cutoff by showing RD plots for these less saturated models.

just-eligible for the CCTC based on the score cutoff.

We present estimates for California activity measured at $\tau = +2$ in Table 3. Each cell in the table is from a different regression. The first column shows RD estimates for log employment, log payroll, and log establishments within California using our fixed-bandwidth approach, where we include industry and allocation period fixed effects as well as baseline controls; these estimates match those depicted in Panel A of Figure 5. The second column shows RD estimates for the same outcomes using our varying-bandwidth approach; a key difference with this approach is that baseline controls are not required to achieve smoothness in pre-treatment outcomes, and as such, the specification only includes industry and allocation period fixed effects.[43] Finally, the third column shows results for the same outcomes using our dynamic RD approach (equation (3)). This approach follows Cellini et al. (2010) in estimating a model that dynamically controls for application histories to address possible bias from repeat applicants.

The RD estimate for log employment within California using our fixed-bandwidth approach (column (1) of Table 3) is a statistically significant 0.26, or 30%. Total payroll increases commensurately. We also see growth in the number of establishments in California among those just eligible for tax credits, although the effect for establishments is smaller than that for employees and payroll and statistically insignificant.[44] We arrive at similar results with our varying-bandwidth approach (column (2) of Table 3), although the estimated effect on log employment is less precise and the estimated effect on establishments is larger and statistically significant. Our dynamic RD estimates, which help to address repeat applicants but rely on stronger parametric assumptions, are qualitatively similar, but smaller in magnitude, than our pooled RD estimates (column (3) of Table 3). The smaller estimated effects are more a function of the difference in specification and sample than a function of the repeat applicants; running equation (3) treating each application as a separate EIN (i.e., ignoring repeats) generates very similar results as those shown in column (3) of Table 3.[45] We conclude that the presence of repeat applicants is not biasing the estimates materially.

[43] See Appendix Table G.3 for tests of discontinuities in baseline characteristics at the cutoff using the varying-bandwidth approach.

[44] Alternative specifications using a fixed-bandwidth but including fewer controls point to even larger effects of CCTC eligibility on activity within California; the addition of controls serves to attenuate the estimates, largely by helping to balance firms on pre-application size. Appendix Table G.4 illustrates how the inclusion of controls eliminates the pre-application ($\tau = -2$) differential in firm size among those with scores below the cutoff. Appendix Table G.5 shows results using outcomes measured in levels, again using several alternative specifications. The effects for outcomes measured in logs are more muted relative to those for outcomes measured in levels. This reflects the fact that the level results are in part driven by several very large expansions of CCTC-eligible applicants.

[45] See the $\tau = +2$ estimates in Appendix Figure G.4. This figure provides comparable estimates to Panel C of Figure 6, discussed next.

In Figure 6, we show RD estimates for a range of τ (time relative to application year), from -5 to $+2$, using each of our three approaches (fixed-bandwidth in Panel A, varying-bandwidth in Panel B, and dynamic RD in Panel C). The results in Panels A and B of Figure 5 correspond to the estimates depicted at $\tau = +2$ and $\tau = -2$ in Panel A of Figure 6. With all three approaches, in years prior to the application year, the estimated effects for employment, payroll, and establishments in California are close to zero and statistically insignificant, as would be expected (and consistent with the continuity tests discussed in Section 5.2). However, in years after the application, the RD estimates using all three methods point to statistically significant growth, particularly in employment and payroll.[46] While data limitations preclude examination of the extent of persistence in these effects beyond $\tau = +2$, firms are required to maintain employment at their contractually specified levels for three years beyond the end of their five-year contracts (so for eight years after $\tau = 0$), or risk forsaking the tax credits.

Overall, measured 2-3 years after application, our results suggest CCTC eligibility increases firm employment within California by 11-32%, firm payroll within California by 15-39%, and the number of firm establishments within California by 1-21%. We return to additional interpretation of this range of estimates and their implications in our discussion of the implied tax elasticities in Section 7.

6.2.2 Activity in High-Poverty and High-Unemployment Areas

Businesses locating or expanding anywhere in California can receive tax credits under the CCTC program. However, the program prioritizes high-poverty and high-unemployment cities and counties in the state. In early years of the program, this was operationalized by giving preference to applications with proposed investment in such areas in the discretionary review phase. After 2017, applicants could automatically advance to the discretionary review phase if they committed to investing in a designated high-poverty or high-unemployment city or county.

To analyze the effects of the CCTC on firms' employment, payroll, and establishments in high-poverty and high-unemployment areas, we turn directly to interpreting the time path of RD estimates across the range of τ (time relative to application year) in Figure 7.[47] The pooled RD estimates using fixed- and varying-bandwidth approaches in Panels A and B show that in

[46] We observe the same patterns, albeit with an even stronger upward trajectory in the post-application period, if instead we use employment and payroll levels as opposed to logs; we show dynamic RD results in levels in Appendix Figure G.3.

[47] We provide supporting regression discontinuity plots for $\tau = +2$ and $\tau = -2$ in Appendix Figure G.5. Regression results for each outcome measured in $\tau = +2$ using each of our three estimators appear in Appendix Table G.6.

years following the application, firms with scores below the cutoff grew more in high-poverty or high-unemployment parts of California than firms with scores above the cutoff. However, the imprecision of these estimates preclude us from making definitive statements about the CCTC's effects in distressed parts of the state relative to its effects in the state as whole. Notably, in Panels A and B, for employment and even more so for establishments, there is some evidence of pre-treatment trends, though the estimated effects prior to application are statistically indistinguishable from zero. Furthermore, as shown in Panel C, our estimates for employment, payroll, and establishments from the dynamic approach are near zero and statistically insignificant. Overall, while the time path and direction of effects is qualitatively consistent with positive impacts of the CCTC in high-poverty and high-unemployed parts of California, the combination of imprecision and instability across estimators warrants caution when interpreting the magnitude of these results relative to effects in California as a whole.

6.2.3 Cross-State Spillovers and Allocative Inefficiency

A major concern with state and local business incentive programs is that they may come not only at the expense of taxpayers, but also other jurisdictions, if subsidies merely shift the location of activity. In that case, states and localities may be expending large sums for what ultimately generates little to no net growth in the aggregate.

We consider the role of within-firm reallocation of activities across establishments in potentially giving rise to such an effect. If the positive impacts of the CCTC for a firm's operations in California come at the expense of that firm's operations in other states, it would lend support to concerns about the broader consequences of interjurisdictional tax competition. On the other hand, if businesses do not merely reallocate activities in response to this state-specific incentive, but rather expand overall operations, it would suggest that at least within-firm reallocation does not dampen the aggregate effects of the CCTC.

We present RD estimates for employment, payroll, and establishments outside California across our three estimators in Table 4. Measured in logs, we fail to detect statistically significant negative effects of CCTC-eligibility on employment, payroll, and establishments outside California 2-3 years after allocation, as shown in the first three rows of the table. However, given the noise owing to combining data from the rest of the U.S., we cannot rule out sizable negative or positive effects; based on these results alone, it is possible that there is indeed a negative reallocation effect or positive scale effect, but the signal is too small to detect against such a noisy backdrop.

The imprecision of our estimates for log employment, payroll, and establishments outside California may stem from some firms having substantial employment scattered across multiple other states and many firms having little to no employment in other states. Therefore, we consider an alternative outcome, the share of a firm's employment outside California, that just captures the extent to which CCTC eligibility tilts the geographic concentration of a firm's employment toward (or away from) California. Given the prior results indicating an increase in activity in California among CCTC-eligible firms, we would expect the average share of employment outside California to fall on the order of 3 percentage points (from 14% to 11%) in the absence of any reduction in employment in other states in response to the tax credits.[48] The expected negative effect would be even more pronounced if firms narrowly to the right of the cutoff expanded outside of California as the result of not meeting the Phase I cutoff.

In the bottom three rows of Table 4, we find that CCTC eligibility is associated with, at worst, a statistically insignificant 1 percentage point decrease in the share of employment outside California. We can rule out with over 95% confidence a decrease in the share of employment outside California of 3 percentage points in all three models. For payroll and establishment effects, model (1) rejects, with 95% confidence, decreases greater than 4.9 percentage points each, while models (2) and (3) can reject payroll and establishment declines greater than 3.0 percentage points each. Figure 8, which shows RD estimates across the full range of event times, suggests no pre-application differentials and little detectable post-application impacts of score eligibility on employment, payroll, and establishment shares outside California in Panels A and C. While the varying-bandwidth estimator in Panel B exhibits positive point estimates prior to allocation, confidence intervals for the most part contain zero, and there is no evidence of a strong differential decline (or increase) in the share of activity outside of California years beyond the allocation.

The estimate paths in Figure 8 categorically reject strong negative reallocation effects from the CCTC beyond those expected by the increase in activity in California. That is, the results point to growth in California that does not come at the expense of CCTC winners reallocating activity to the state, or CCTC losers reallocating away from the state. One concern, however, is that our specification is diluted by including firms with no prior presence outside California, thereby mechanically over-weighting firms with the least scope to reallocate. This could attenuate estimates toward zero. A related concern is if automatic advancers indicating they would relocate

[48] We arrive at this 3 percentage point reduction by applying the estimated increase in California employment to the baseline share of employment outside of California reported in Appendix Table G.5.

absent the incentive disproportionately appear on either side of the cutoff, it could bias local estimates. Two pieces of evidence rule out the concerns. First, in results not shown, we re-estimate the fixed-bandwidth RD model on the share of employment occurring outside of California, but conditioning on firms having pre-existing activity outside of California at the time of application. In these tests, we fail to find effects statistically different from zero.[49] Second, we find that automatic advancers do not disproportionately appear on one side of the cutoff.[50]

Finally, in Figure 9, we show RD plots for a range of τ for firm-level measures of total revenue, revenue per employee, and revenue per payroll dollar. These results point to some evidence of expansions in overall firm activity as well as productivity following the application year for those applicants whose scores fell just below the cutoff as compared to those whose scores fell just above the cutoff. While estimates for some of the pre-allocation periods are statistically significant, there is a clear pattern of increasing revenue across all three panels, with weaker evidence in support of positive productivity effects in periods following the allocation year. The results for labor and payroll productivity indicate that the additional growth in employment in California also does not entail allocative inefficiency. If anything, there are positive effects on firm-wide revenue and measures of labor productivity.

Our results for revenue and productivity are potentially linked to several important channels, including possibly the financing of innovation (Howell, 2017) or price-induced technological change in which giving incentives to redirect the firm's input mix can lead to gains in productivity (which we measure as revenue per worker) (Popp, 2002). These results are also consistent with work by Giroud and Mueller (2015), who show that financially unconstrained firms (such as those in our sample) expand in locations with lower costs of capital without substituting away from locations with higher costs. Given the large investments associated with the top tax credit offers and the prevalence of technology firms in the CCTC context (see Table 1), innovation and a lack of financial constraints could help to explain the observed revenue effects.

7 Implied Local Tax Elasticities

In this section, we discuss the implications of our estimates for firms' responsiveness to changes in their tax burdens in the short run. This allows us to better compare the effects of the CCTC to those of other place-based subsidy programs and tax rate changes. Specifically, we apply the monetary

[49]This description of our results was released in a Census Bureau Sign and Significance release.
[50]See Appendix Figure D.12.

increase associated with each applicant's subsidy to the applicant's estimated state tax liability, and then use our prior estimates to calculate local tax elasticities associated with the CCTC. While several important assumptions are required to conduct this exercise, we view it as fruitful for contextualizing the magnitude of our results.

We first need to characterize the immediate change in annual tax liability (or alternatively effective net-of-tax rate) faced by each CCTC applicant i prior to application. While we do not directly observe state tax liabilities, we do observe LBD revenue and labor expenditures at the firm-year level, and can further estimate capital expenditures from applicants' reported planned capital expenditures that are provided to GO-Biz. We write profits in California for applicant i at $\tau = -1$ (i.e., one year before applying for the CCTC) as

$$\Pi_{i,\tau=-1} = Rev_{f(i),\tau=-1} \times \omega_{i,\tau=-1} - Pay_{i,\tau=-1} - Inv_{i,\tau=-1} \tag{4}$$

In equation (4), Rev is U.S.-wide nominal revenue for the firm f of which applicant i is potentially just a part; these revenues are observed only at the firm level in the LBD. We apportion firm revenues measured in $\tau = -1$ to each applicant i using ω, for which we use the share of firm f's LBD employment in California in $\tau = -1$. Pay is nominal LBD payroll in California for applicant i, which we also measure at $\tau = -1$. Finally, we proxy applicant i's capital costs, Inv, using planned annual investments as reported in its CCTC application.[51] Specifically, we take the average capital investment that is reported in the CCTC data for each of the five years following the application year, converted to present value using a discount rate of 5%.

Next, we apply California's flat 8.84% corporate tax rate to baseline profits to obtain each applicant's tax liability. For CCTC awardees, we subtract annualized cumulative credits (total credits offered/5 years, denoted C) from their tax liabilities.[52]

$$\text{Tax Liability}_{i,\tau=-1} = 0.0884 \times \Pi_{i,\tau=-1} - C_i \tag{5}$$

Based on this approach and the fixed-bandwidth estimator, we estimate that the mean applicant receives an approximately 4% decrease in tax liability when below the cutoff, as shown in Panel A of Table 5. This estimate forms the denominator of our tax elasticities. Using the estimates in column (1) of Table 3, the implied elasticity of employment in California with respect to corporate

[51] This requires the assumption that there is no opportunity cost of renting out currently used capital, only the cost of new investments.

[52] Tax liability can also be recast as the applicant's effective net-of-tax rate (equal to 0.9116 when no credits are received).

taxes is -7.6, and the implied elasticity of payroll in California is -7.2 (see Panel B of Table 5).[53] For the same estimated reduction in tax liability, our varying-bandwidth and dynamic employment estimates imply local labor demand elasticities of -8.3 and -2.8, respectively. The payroll tax literature offers benchmarks for our estimates. For example, Guo (2023) finds labor demand elasticities ranging from -1.1 to -2.4 in the context of UI payroll taxes. In the same context, Johnston (2021) estimates an elasticity of -4. Ku et al. (2020), meanwhile, estimate an elasticity of -3.6 using place-based payroll taxes. Benzarti and Harju (2021) estimate labor demand elasticities of -2.9 to -4.2 in the context firm-specific payroll tax increases. We discuss the implications of our set of estimates for the marginal value of public funds for the CCTC program in Appendix H.

Based again on our fixed-bandwidth RD approach, our cross-state firm mobility semi-elasticity estimate is -0.26. The varying-bandwidth point estimate is the same sign but even larger in absolute value, pointing again to limited negative external effects of the CCTC on firms' activities in other states. The dynamic estimator, on the other hand, points to reallocation in response to the CCTC, but at a positive 0.26, the cross-state firm mobility semi-elasticity is close to zero. Taken together, our results are in line with the estimates of relatively small cross-state firm reallocation elasticities in response to corporate taxes from, for example, Giroud and Rauh (2019) and Suárez Serrato and Zidar (2016).

Overall, the sizable local labor demand elasticities we estimate are consistent with our interpretation of the CCTC as a "best-practice" upper-bound for firms with expected growth. As previously discussed, the CCTC's discretionary tools particularly target firms that are on the margin of making large capital investments in California. While our results offer credible short-term employment estimates from firm subsidies, our setting and data preclude longer-run analysis. The design of CCTC requires firms to uphold employment levels for an additional three years beyond the initial five-year contract period, but it is possible that firms exit or downsize when subsidies expire. Patrick and Partridge (2022) study the long-run effects of (subsidized) million-dollar plant entrants, and while they find that large plant openings do not push locations into new productivity equilibria as predicted by the "Big Push" literature (see e.g., Kline and Moretti (2014)), they present evidence consistent with persistent direct employment effects in which subsidized firms do not exit in the long run. Similarly, Freedman (2017) finds evidence of substantial persistence in the employment impacts of large subsidized plant sitings in the 1930s.

[53] A caveat is that our elasticity calculations require plugging in the estimate no matter its statistical significance. While prior first-stage results were always statistically significant, subsidies received usually represent a very small number relative to total tax liabilities. Nevertheless, a money-denominated metric is required for such calculations.

Nonetheless, the extent to which direct employment effects of the CCTC persist in the long run remains an open question.

8 Conclusion

This paper examines firm responses to a large-scale hiring subsidy program in California. The CCTC is one of a new generation of business incentive policies that attempts to incorporate best practices from prior government efforts to attract and retain businesses. The design of the CCTC, and in particular the formulaic component of the application process, also facilitates rigorous empirical evaluation.

Taking advantage of confidential administrative data on CCTC applicants combined with restricted-use microdata from the LBD on establishments and their parent firms, we find that the CCTC program induces employment and payroll growth in California, primarily due to business expansions but also to a lesser extent by new business sitings. Furthermore, we do not find strong evidence that the program leads to significant reallocation of employment or payroll across states within firms nationwide. Consistent with this, using data on firm-wide revenue, we find no evidence of allocative inefficiency associated with CCTC-induced growth in California.

Our results suggest that the CCTC is more effective than many previous business incentive programs at inducing firm expansions locally. The difference may arise because the CCTC not only bundles hiring and investment incentives together, but also targets marginal firms with pre-existing expansion plans through its application process and discretionary tools. Unlike many previous state industrial policies, the CCTC also features audited benchmarking and enforceable credit recaptures. Our results imply that it is possible that any state unilaterally offering a mix of investment and hiring incentives such as the CCTC program could achieve local gains without reallocation effects. However, there may also be unique advantages to the California economy or aspects of the CCTC's implementation that make the credit particularly effective locally. It is also likely that wider adoption across states could dampen the program's positive effect in any given state. A full equilibrium accounting would additionally need to consider impacts on housing markets, competitive implications for firms not participating in the CCTC, and more. While such an analysis is beyond the scope of this paper, our results nonetheless have important implications for policy, and in particular in informing future efforts to design and implement effective economic development initiatives.

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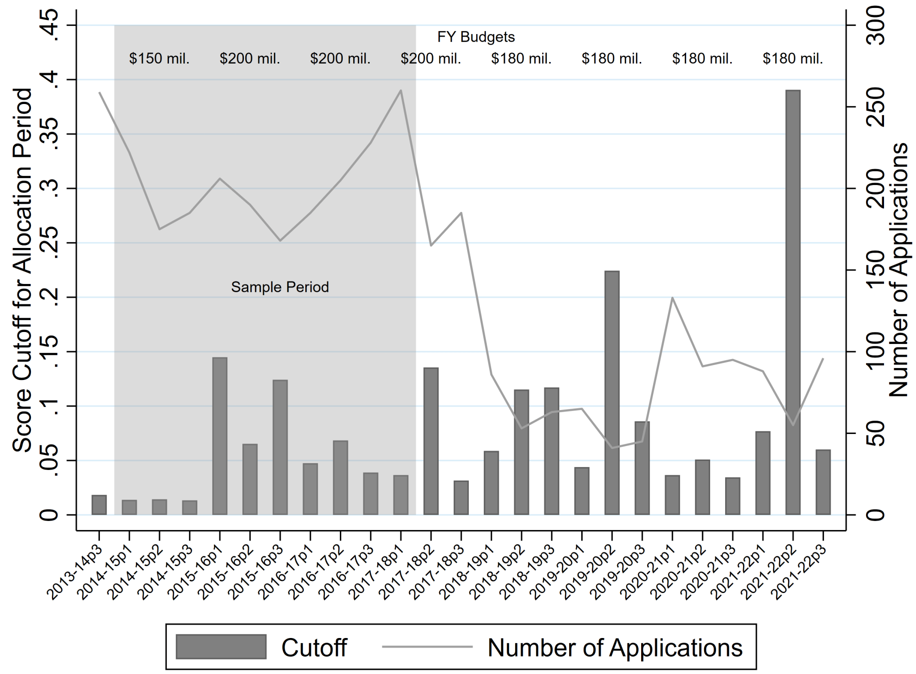
Figures and Tables

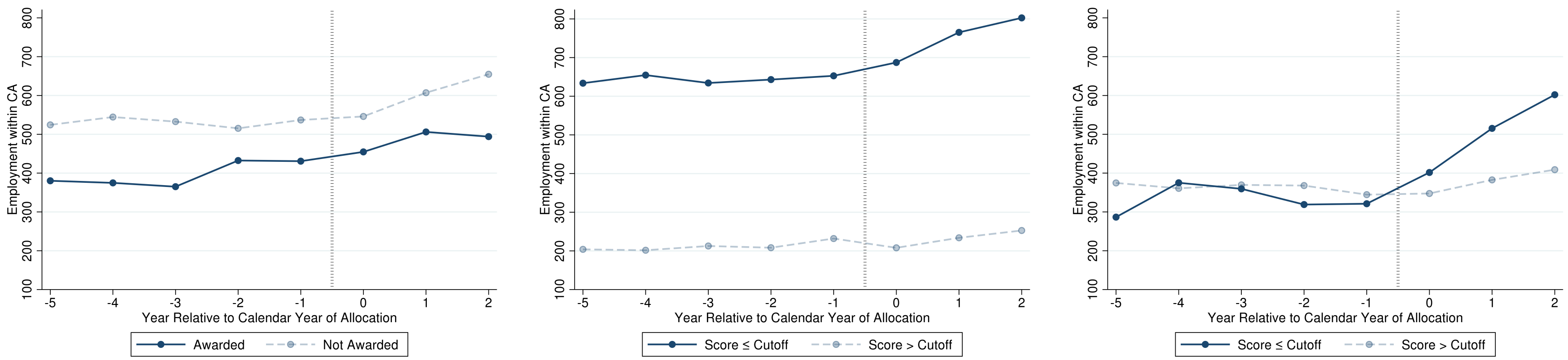
Figure 1: CCTC Score Cutoffs and Applicant Pool Sizes by Allocation Period



NOTES—Figure shows score cutoffs and application volumes for all allocation periods in our data. Our analysis sample is 2014-15p1 to 2017-18p1 (p denotes one of three allocation periods each fiscal year). Cutoffs and application counts are shown for "large" applicants only, to match our main sample. The drop in number of applications after our sample period is associated with statutory changes around this period. See text for additional details.

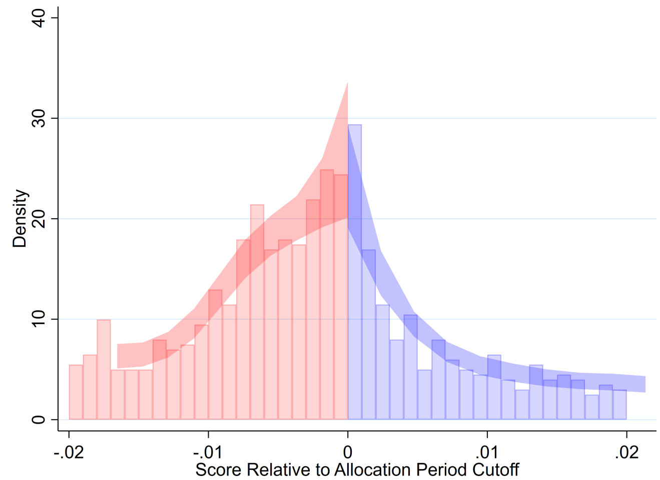
Figure 2: CCTC Applicant Firm Employment by Awardee and Cutoff Status

(a) Awardees vs. Non-Awardees (b) Below vs. Above Score Cutoff (c) Below vs. Above Score Cutoff, Pooled RD Sample



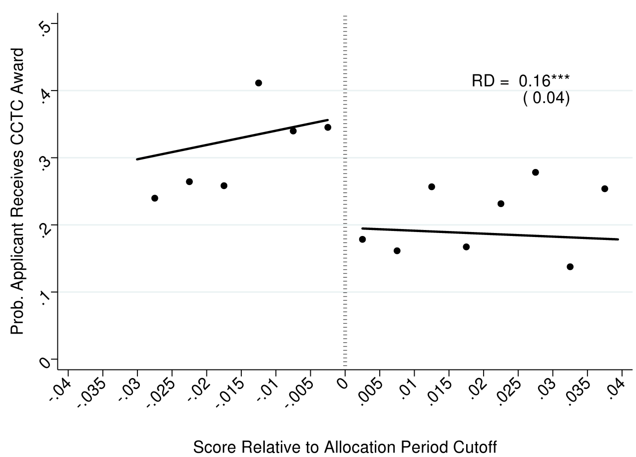
NOTES—Figures show firm-wide employment level in California relative to calendar year of application. Panels (a) and (b) reflect the full sample, while panel (c) reflects the bandwidth, weights, and controls (residualized) from our fixed-bandwidth RD specification for employment in California.

Figure 3: Manipulation Test over Relative Score Running Variable



NOTES—Figure shows manipulation test output following Cattaneo et al. (2020), using a linear fit and triangular kernel with asymmetric bandwidths (estimated without controls). The output range of the figure is three times the default bandwidth on either side of the cutoff. The estimation package (Cattaneo et al., 2018) produces smoothing-bias corrected 95% confidence intervals.

Figure 4: First-Stage RD Plot

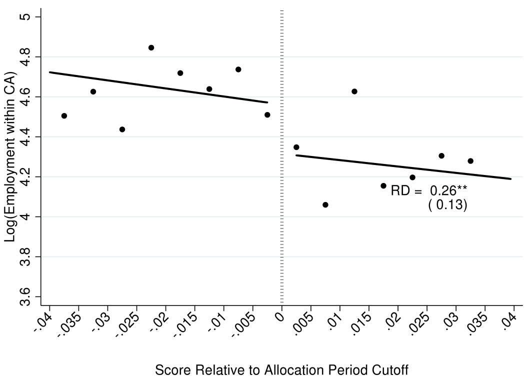


NOTES—Figure shows fixed-bandwidth regression slopes and intercepts overlaid on top of equally spaced pre-binned outcome data with a bin size of 0.005. Plots are shown over the optimal bandwidth selected using the IMSE-procedure, which produces asymmetric optimal bandwidth boundaries for each sample. Fixed-bandwidth estimates correspond to our specification that residualizes outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation with a fixed optimal bandwidth. Observations to the left of the cutoff reflect applicants whose scores qualify them for further consideration. Observations to the right of the cutoff include applicants who do not meet the score cutoff but who may automatically advance to the second phase of consideration (see text for details). Discontinuity estimate and standard error correspond to the fully saturated model in Appendix Table G.1 (column (5)).

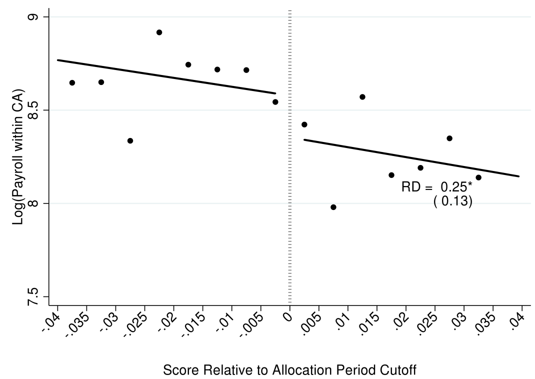
Figure 5: Pooled RD Results for Activity within California (Fixed Bandwidth)

Panel A. Two Years After Credit Allocation Year ($\tau = +2$)

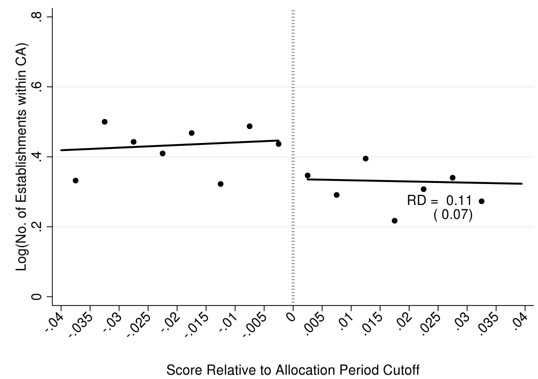
(a) Employment



(b) Payroll

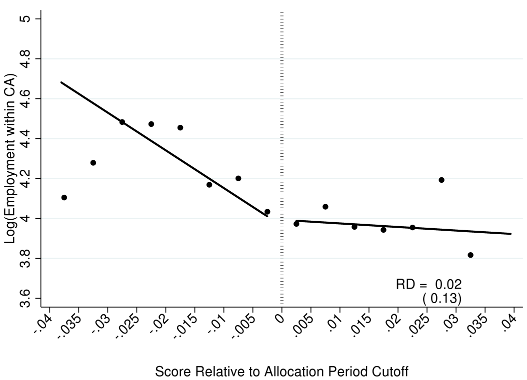


(c) Establishments

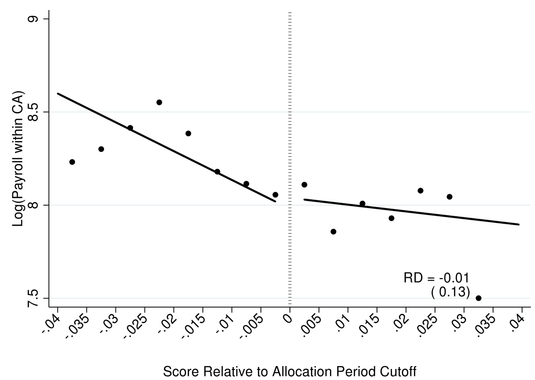


Panel B. Two Years Prior to Credit Allocation Year (Placebo, $\tau = -2$)

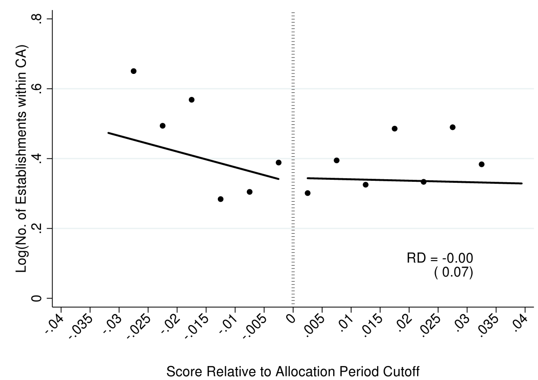
(d) Employment



(e) Payroll



(f) Establishments

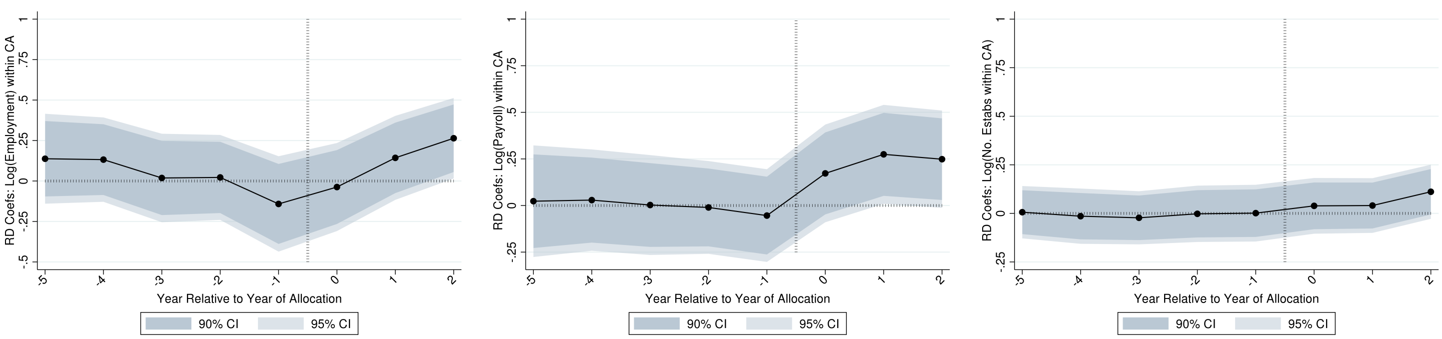


NOTES—Figures show fixed-bandwidth regression slopes and intercepts overlaid on top of equally spaced pre-binned outcome data with a bin size of 0.005. Plots are shown over the optimal bandwidth selected using the IMSE-procedure, which produces asymmetric optimal bandwidth boundaries for each sample. Fixed-bandwidth estimates correspond to our specification that residualizes outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation with a fixed optimal bandwidth.

Figure 6: RD Coefficient Plots over Event Time for Activity within California

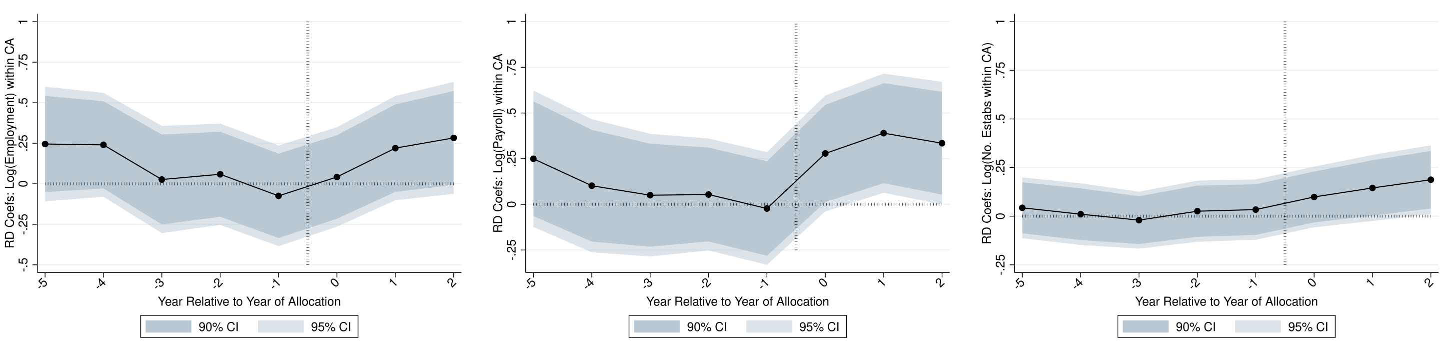
A. Pooled RD using Fixed Bandwidth

(a) Employment (b) Payroll (c) Establishments



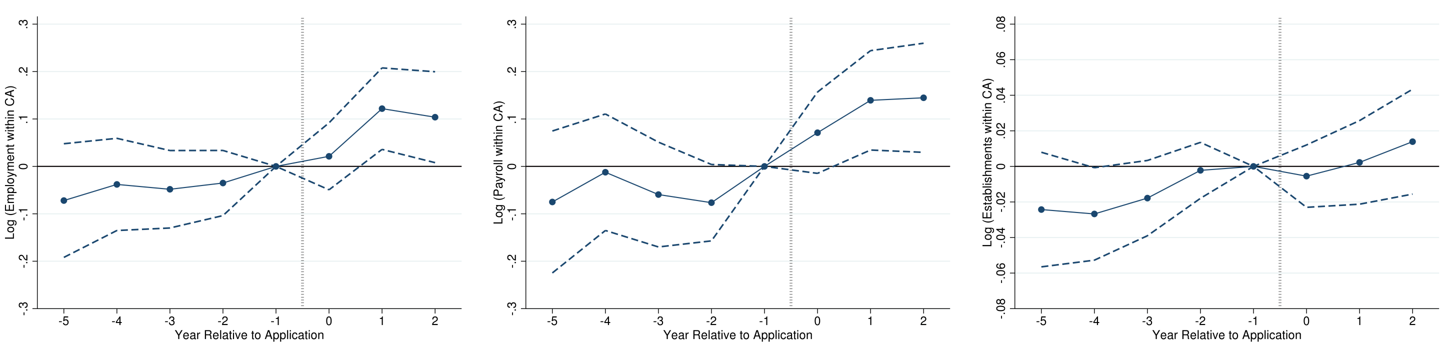
B. Pooled RD using Varying Bandwidth

(a) Employment (b) Payroll (c) Establishments



C. Dynamic RD

(a) Employment (b) Payroll (c) Establishments

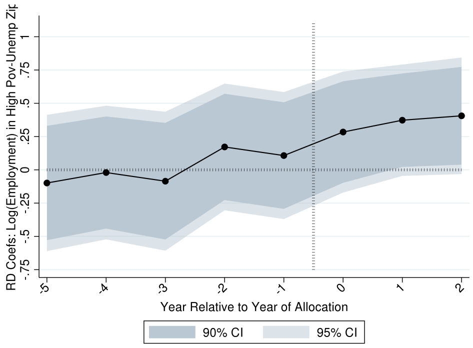


NOTES—Each point in the figures in Panel A and Panel B is from a separate regression for the corresponding outcome and event year, shown with 90% and 95% confidence intervals. Estimates and standard errors for $\tau = -2$ (two years prior to credit allocation) and $\tau = +2$ (two years after credit allocation) are equivalent to those found in the table output. Fixed-bandwidth estimates first select the IMSE-optimal bandwidth, then residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation. Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. Dynamic estimates in Panel C follow the Cellini et al. (2010) method to account for repeat applicants, shown with 95% confidence intervals.

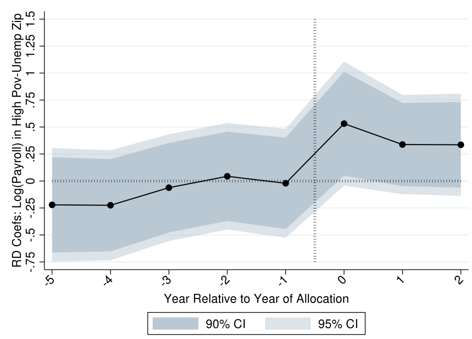
Figure 7: RD Coefficient Plots over Event Time for Activity in High Poverty/Unemployment ZIP Codes in CA

A. Pooled RD using Fixed Bandwidth

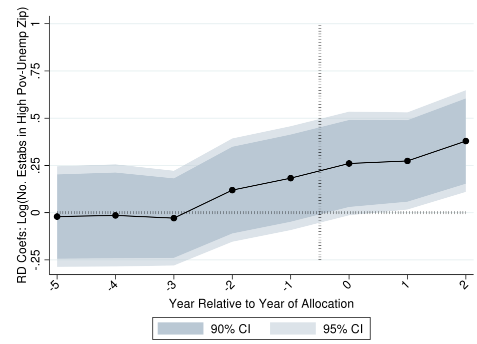
(a) Employment



(b) Payroll

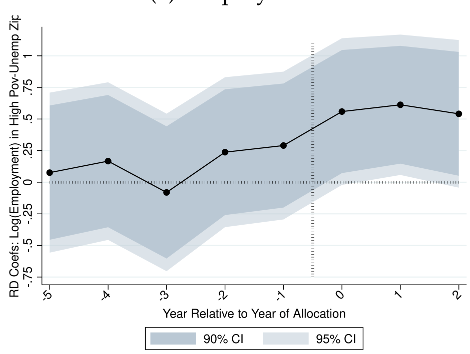


(c) Establishments

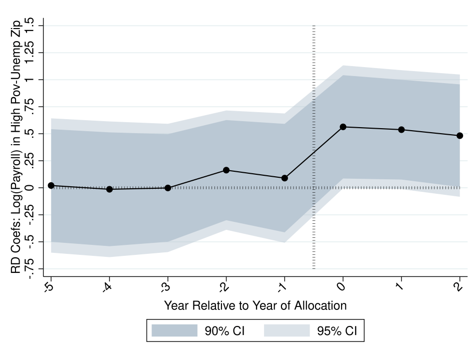


B. Pooled RD using Varying Bandwidth

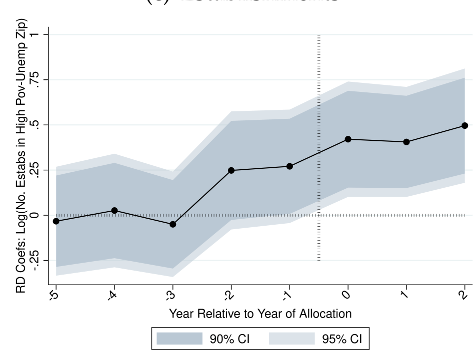
(a) Employment



(b) Payroll

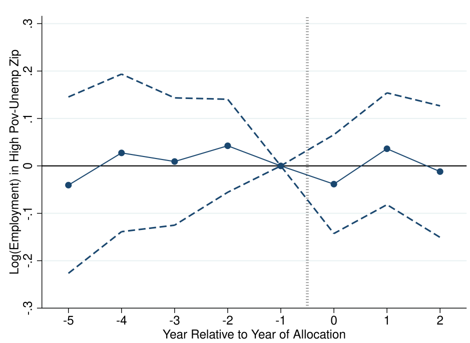


(c) Establishments

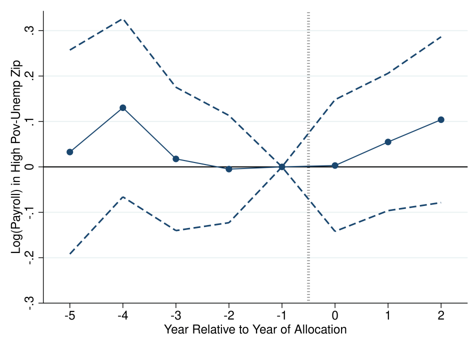


C. Dynamic RD

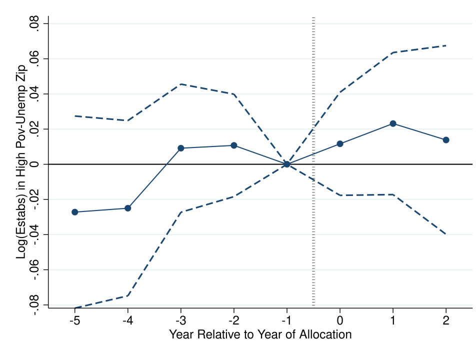
(a) Employment



(b) Payroll

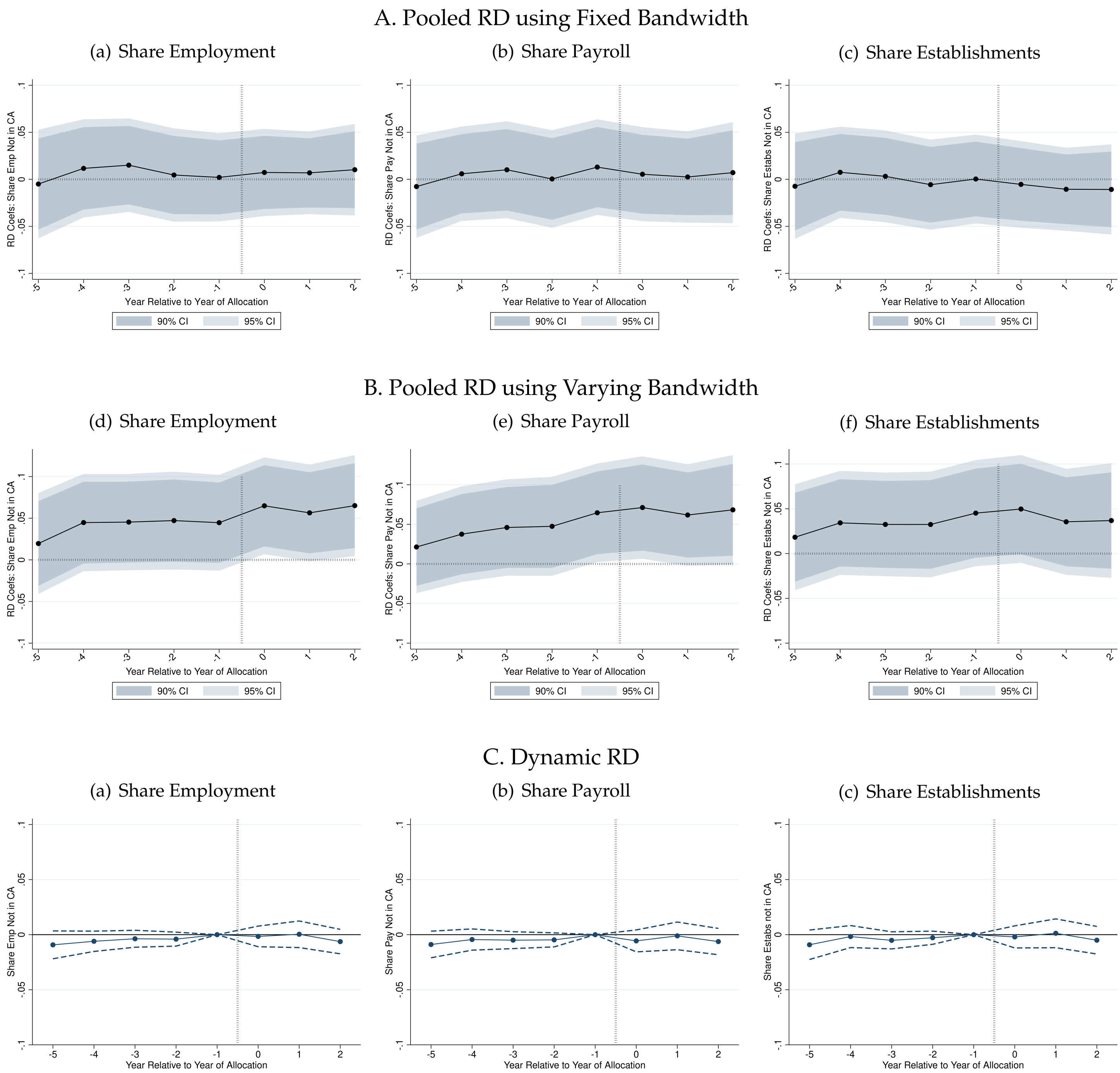


(c) Establishments



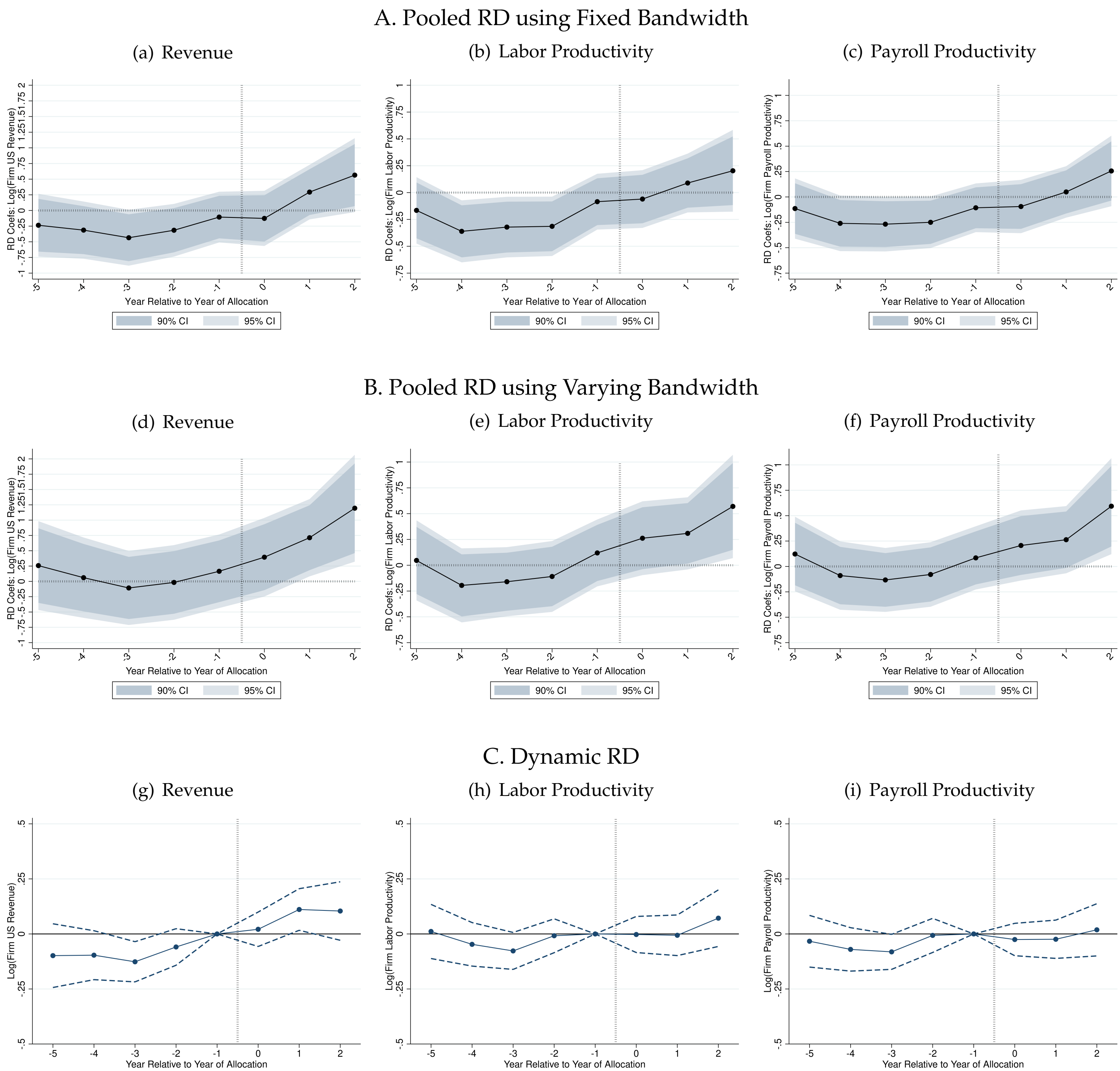
NOTES—Each point in the figures in Panel A and Panel B is from a separate regression for the corresponding outcome and event year, shown with 90% and 95% confidence intervals. Estimates and standard errors for $\tau = -2$ (two years prior to credit allocation) and $\tau = +2$ (two years after credit allocation) are equivalent to those found in table output. Fixed-bandwidth estimates first select the IMSE-optimal bandwidth, then residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation. Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. Dynamic estimates in Panel C follow the Cellini et al. (2010) method to account for repeat applicants, shown with 95% confidence intervals.

Figure 8: RD Coefficient Plots over Event Time for Activity outside California



NOTES—Each point in the figures in Panel A and Panel B is from a separate regression for the corresponding outcome and event year, shown with 90% and 95% confidence intervals. Estimates and standard errors for $\tau = -2$ (two years prior to credit allocation) and $\tau = +2$ (two years after credit allocation) are equivalent to those found in table output. Fixed-bandwidth estimates first select the IMSE-optimal bandwidth, then residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation. Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. Dynamic estimates in Panel C follow the Cellini et al. (2010) method to account for repeat applicants, shown with 95% confidence intervals.

Figure 9: RD Coefficient Plots for Firm-Level (National) Revenue Outcomes



NOTES—Each point in the figures in Panel A and Panel B is from a separate regression for the corresponding outcome and event year, shown with 90% and 95% confidence intervals. Revenue measures are only available at the firm-year level for the entire US, and are not apportioned by geography. Fixed-bandwidth estimates first select the IMSE-optimal bandwidth, then residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation. Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. Dynamic estimates in Panel C follow the Cellini et al. (2010) method to account for repeat applicants, shown with 95% confidence intervals. Revenue is in real terms, deflated by the 2009 PPI, whereas productivity measures are fractions of nominal values.

Table 1: Top 20 CCTC Tax Credit Awards, 2014-2017 (Awards in Analysis Period)

Applicant Name	Tax Credits Awarded	Proposed Investment Increase	Proposed Employment Increase	Industry	Year
Tesla Motors, Inc.	15,000,000	2,389,000,000	4,426	Automobile Manufacturing	2015
Faraday & Future, Inc.	12,725,000	311,100,000	1,990	Automobile Manufacturing	2016
Nordstrom, Inc.	11,000,000	171,000,000	367	Online Order Fulfillment Warehouse and Retail Distribution	2016
NextEV USA, Inc.	10,000,000	138,300,000	917	Automobile Manufacturing	2016
Northrop Grumman Systems Corp.	10,000,000	520,300,000	1,359	Aircraft Manufacturing	2015
Samsung Semiconductor, Inc.	9,000,000	194,700,000	327	Semiconductor R&D	2015
General Motors Company	8,000,000	14,000,000	1,163	Automobile Manufacturing	2017
Ulta, Inc.	8,000,000	48,300,500	542	Online Order Fulfillment Warehouse and Retail Distribution	2016
Boehringer Ingelheim Fremont, Inc.	7,500,000	122,000,000	258	R&D in Biotechnology	2017
Proterra, Inc.	7,500,000	85,967,500	432	Electric Automobile Manufacturing	2017
SF Motors, Inc.	7,500,000	10,884,910	357	Autonomous Vehicle R&D	2017
Kite Pharma, Inc.	7,000,000	114,800,000	621	Biopharmaceutical R&D and Manufacturing	2016
Centene Corporation	7,000,000	100,100,000	1,532	Healthcare Administration	2016
LuLaRoe LLC	6,400,000	120,000,000	1,362	Clothing Manufacturing and Wholesaler	2017
OWB Packers LLC	6,000,000	38,500,000	605	Beef Processing	2016
Samsung Semiconductor, Inc.	6,000,000	357,800,000	400	Semiconductor R&D	2014
Scopely, Inc.	5,500,000	53,468,069	309	Mobile Application Development	2016
Renovate America, Inc.	5,475,000	24,400,000	542	Energy Efficiency Consulting Services	2017
Snapchat, Inc.	5,000,000	32,000,000	1,194	Mobile Application Development	2016
Planet Labs, Inc.	4,340,000	60,000,000	216	Earth Imaging Satellite Design, Manufacturing and Operation	2015

NOTES—Data on awardees is publicly available on the GO-Biz website https://business.ca.gov/california-competes-tax-credit/awardee-list/ (accessed on 04/04/22). Investments refer to the five-year qualified investment per each applicant's contract commitment, while employment refers to the net five-year full time equivalent employee contract commitments.

Table 2: Continuity in Baseline Application Characteristics and Outcomes (Fixed Bandwidth)

Dep. Variable	Discontinuity ($\hat{\beta}$)	Standard Error	Control Mean ($\hat{\alpha}$)	N
Panel A. Application Characteristics				
Tax Credits Requested	-157,600	164,900	795,500	1,600
AA Relocate	-0.01	0.02	0.03	1,600
AA Terminate or Leave	0.03	0.04	0.33	1,600
AA Occur Other State	-0.02	0.03	0.07	1,600
Log Baseline Employees	-0.07	0.13	4.16	1,600
Log Projected Compensation Next 5 Years	0.30**	0.12	15.48	1,600
Log Projected Investment Next 5 Years	0.38**	0.16	14.51	1,600
Panel B. Pre-Treatment Applicant Outcomes ($\tau = -2$)				
Activity in California				
Employment within CA	64	195	455	1,600
Payroll within CA (Ths. $)	4,999	12,870	28,350	1,600
Establishments within CA	-0.18	1.67	4.40	1,600
Log Employment within CA	0.02	0.13	3.99	1,600
Log Payroll within CA	-0.01	0.13	8.03	1,600
Log Establishments within CA	0.00	0.07	0.34	1,600
Activity in High-Poverty/High-Unemployment California ZIPs				
Employment in High Pov-Unemp CA ZIPs	75	82	109	1,600
Payroll in High Pov-Unemp CA ZIPs (Ths. $)	3,777	4,057	5,698	1,600
Establishments in High Pov-Unemp CA ZIPs	-0.04	0.67	1.58	1,600
Log Emp. in High Pov-Unemp CA ZIPs	0.17	0.24	4.2	1,600
Log Payroll in High Pov-Unemp CA ZIPs	0.04	0.25	8.23	1,600
Log Establishments in High Pov-Unemp CA ZIPs	0.12	0.14	0.49	1,600
Activity outside California				
Employment outside CA	611	1,216	1,973	1,600
Payroll outside CA (Ths. $)	36,480	72,710	115,000	1,600
Establishments outside CA	-3.98	12.26	20.32	1,600
Log Employment outside CA	-0.71	0.48	6.36	1,600
Log Payroll outside CA	-0.73	0.51	10.50	1,600
Log Establishments outside CA	-0.69**	0.34	2.20	1,600
Share Employment outside CA	0.00	0.03	0.15	1,600
Share Payroll outside CA	0.00	0.03	0.15	1,600
Share Establishments outside CA	-0.01	0.02	0.16	1,600

NOTES—Each row corresponds to a separate regression discontinuity with the listed variable as the dependent variable. The control mean is the estimated intercept ($\hat{\alpha}_\tau$) from equation (2). Fixed-bandwidth estimates first select the IMSE-optimal bandwidth, then residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation. Baseline controls include single-unit (vs. multi-unit) status, an indicator for being an S-corporation, an indicator for C-corporation, and an indicator for whether the firm is publicly traded. N represents the rounded observation count prior to IMSE-optimal bandwidth adjustments and log transformations. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** p$\leq$0.01, ** p$\leq$0.05, * p$\leq$0.1.

Table 3: RD Estimates for California Activity, $\tau = +2$

Dep. Variable	(1)	(2)	(3)
Log(Employment within CA)	0.26**	0.28	0.10**
	(0.13)	(0.18)	(0.05)
Log(Payroll within CA)	0.25*	0.33*	0.14**
	(0.13)	(0.17)	(0.06)
Log(No. Establishments within CA)	0.11	0.19**	0.014
	(0.07)	(0.09)	(0.02)
Pooled, Fixed Bandwidth	X		
Pooled, Varying Bandwidth		X	
Dynamic			X

NOTES—Each row and column corresponds to a separate regression discontinuity estimate with the dependent variables listed in the first three rows. Fixed-bandwidth estimates first select the IMSE-optimal bandwidth, then residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation. Baseline controls include single-unit (vs. multi-unit) status, an indicator for being an S-corporation, an indicator for C-corporation, and an indicator for whether the firm is publicly traded. Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. In columns (1) and (2), there are approximately 1,700 (rounded) applicants prior to IMSE-optimal bandwidth adjustments and log transformations, which deflates observation counts by a range depending on covariates. The observation count is slightly larger here than in the placebo period due to new entrants. In column (3), there are repeated observations on each applicant. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** p≤0.01, ** p≤0.05, * p≤0.1.

Table 4: RD Estimates for Activity Outside of California, $\tau = +2$

Dep. Variable	(1)	(2)	(3)
Log(Employment outside CA)	-0.33	0.30	0.04
	(0.51)	(0.57)	(0.094)
Log(Payroll outside CA)	-0.33	0.39	0.02
	(0.54)	(0.61)	(0.097)
Log(No. Establishments outside CA)	-0.21	-0.14	0.18**
	(0.39)	(0.42)	(0.07)
Share Employment outside CA	0.01	0.07**	-0.01
	(0.02)	(0.03)	(0.01)
Share Payroll outside CA	0.01	0.07*	-0.01
	(0.03)	(0.04)	(0.01)
Share Establishments outside CA	-0.01	0.04	-0.01
	(0.02)	(0.03)	(0.01)
Pooled, Fixed Bandwidth	X		
Pooled, Varying Bandwidth		X	
Dynamic			X

NOTES—Each row and column corresponds to a separate regression discontinuity estimate with the dependent variables listed in the first three rows. Fixed-bandwidth estimates first select the IMSE-optimal bandwidth, then residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation. Baseline controls include single-unit (vs. multi-unit) status, an indicator for being an S-corporation, an indicator for C-corporation, and an indicator for whether the firm is publicly traded. Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. In columns (1) and (2), there are approximately 1,700 (rounded) applicants prior to IMSE-optimal bandwidth adjustments and log transformations, which deflates observation counts by a range depending on covariates. The observation count is slightly larger here than in the placebo period due to new entrants. In column (3), there are repeated observations on each applicant. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** p≤0.01, ** p≤0.05, * p≤0.1.

Table 5: Tax Elasticity Calculation Results (Fixed Bandwidth)

Dep. Variable	$\hat{\beta}$ (SE)	Tax Elasticity Parameter	Elasticity Estimate
Panel A. First Stage RD			
Log(Applicant Tax Liability)	-0.04		
	(0.24)		
Baseline Mean: Log(Tax Liability)	14.92		
Panel B. Intent-to-Treat RD Estimates			
Log(Employment within CA)	0.26**	Local Labor Demand	$\frac{exp(0.26)-1}{exp(-0.04)-1} = -7.57$
	(0.13)		
Log(Payroll within CA)	0.25*	Local Payroll Demand	$\frac{exp(0.25)-1}{exp(-0.04)-1} = -7.24$
	(0.13)		
Log(Establishments within CA)	0.11	Local Firm Expansion	$\frac{exp(0.11)-1}{exp(-0.04)-1} = -2.96$
	(0.07)		
Sh(Employment outside CA)	0.01	Firm Mobility (Semi-Elas.)	$\frac{0.01}{exp(-0.04)-1} = -0.26$
	(0.02)		
Industry FEs	X		
Allocation Period FEs	X		
Baseline Controls	X		
N	1,700		

NOTES—Panel A reports the estimate of β using equation (2) for the outcome listed with a fixed bandwidth, along with the outcome's baseline mean. The four rows in Panel B are estimates drawn from previous tables, which are then used in the elasticity (or semi-elasticity) calculation in the final column.

Appendices

A High-Poverty / Unemployment Areas

Figure A.1: Example of High Poverty / Unemployment List (2021-22 Period 2)

GOVERNOR'S OFFICE OF BUSINESS AND ECONOMIC DEVELOPMENT

STATE OF CALIFORNIA • OFFICE OF GOVERNOR GAVIN NEWSOM

California Competes Tax Credit
List of High Poverty and High Unemployment Areas

Applicable to applications submitted January 3, 2022 – January 24, 2022

"High poverty area" means a city and/or county within California with a poverty rate of at least 150% of the California statewide poverty rate per the most recently updated data available from the U.S. Census Bureau's American Community Survey 5-Year Estimates thirty days prior to the first day of the applicable application period.

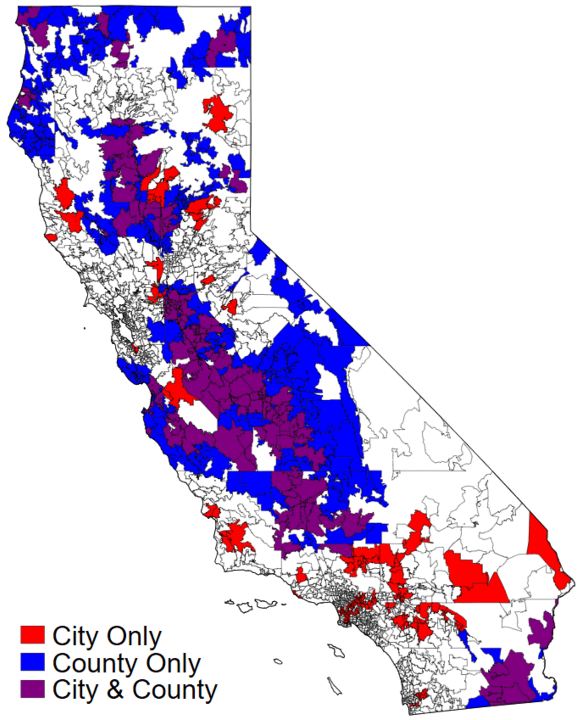
"High unemployment area" means a city and/or county within California with an unemployment rate of at least 150% of the California statewide unemployment rate per the most recently updated data available from the California Employment Development Department on http://www.labormarketinfo.edd.ca.gov/ or the equivalent website thirty days prior to the first day of the applicable application period.

Cities with High Poverty

Adelanto
Arcata
Arvin
Atwater
Avenal
Banning
Barstow
Bell
Bell Gardens
Biggs
Blythe
Brawley
Calexico
California City
Calipatria
Cathedral City
Chico
Chowchilla
Clearlake
Coachella
Coalinga
Compton
Corcoran
Corning
Crescent City
Cudahy
Davis
Delano
Desert Hot Springs
Dinuba
Dorris
El Centro
Exeter
Farmersville
Firebaugh
Fort Jones
Fresno
Grass Valley
Hawaiian Gardens
Hemet
Holtville
Huntington Park
Huron
Lancaster
Lindsay
Madera
Maricopa
Maywood
McFarland
Mendota
Merced
Mount Shasta
Needles
Nevada City
Orange Cove
Orland
Oroville
Parlier
Point Arena
Porterville
Red Bluff
Reedley
San Bernardino
San Joaquin
San Luis Obispo
Sanger
Santa Cruz
Selma
Shafter
Sonora
Taft
Tehama
Tulare
Tulelake
Twentynine Palms
Victorville
Wasco
Weed
Westmorland
Willits
Woodlake
Yreka

1 of 4
2021-22 Application Period 2

Figure A.2: California ZIP Codes Ever Designated High Poverty or High Unemployment



NOTES—Figure shows California ZIP codes ever designated a high-poverty or high-unemployment area under the CCTC program. To tag each California ZIP code, we assemble high-poverty and high-unemployment city and county lists provided online by GO-Biz from 2016p2 (January 2017, the first allocation period where poverty and unemployment were used as automatic advancement criteria) to 2020p3 (March 2021). We map cities to ZIP codes using a custom crosswalk, and counties to ZIP codes using HUD's County to ZIP code Crosswalk. Of 2,451 California ZIP codes in the data, 810 (33%) are flagged as either high unemployment or high poverty during our sample period, while the remaining 1,641 (67%) are never flagged as either high unemployment or high poverty. In the map, we further indicate whether the ZIP code is flagged based on city, county, or both. We were unable to locate two lists during this period, those from July 2019 and July 2021. *Source:* GO-Biz.

Table A.1: Pre-CCTC Demographics of High-Poverty / High-Unemployment Areas in California (2010)

	Counties Ever High Poverty/Unemp.			**All Other Counties**		
	Mean	SD	N	Mean	SD	N
Population (1,000s)	216.99	270.18	25	964.53	1,809.16	33
Median income ($1,000s)	43.39	6.10	25	57.84	12.86	33
Share aged 0-24	0.35	0.06	25	0.32	0.05	33
Share aged 25-54	0.39	0.03	25	0.41	0.04	33
Share aged 55+	0.26	0.08	25	0.28	0.07	33
Share White non-Hisp.	0.55	0.21	25	0.59	0.19	33
Share Hispanic	0.34	0.20	25	0.24	0.13	33
Share Black	0.02	0.02	24	0.04	0.04	33
Share Asian	0.04	0.04	25	0.09	0.09	33
Share Am. Ind.	0.02	0.04	25	0.01	0.02	33
Share HI or PI	0.00	0.00	24	0.00	0.00	33
Share multiple races	0.02	0.01	25	0.03	0.01	33
Unemployment rate	0.17	0.04	25	0.12	0.03	33
Share in poverty	0.19	0.04	25	0.14	0.03	33
Share children in poverty	0.28	0.05	25	0.19	0.05	33

NOTES—Data on population, median income, age compositions, racial/ethnic compositions, and poverty shares are from the 2010 Decennial Census. Unemployment rates are from the April 2010 BLS Local Area Unemployment Statistics.

B Additional Policy Considerations

In this appendix, we provide qualitative evidence on the objectives and concerns of officials involved in the CCTC applicant review process. We are not privy to discussions among the GO-Biz staff about discretionary factors, and our conversations with them lead us to believe that at the discretionary phase they consider primarily information on whether the award is likely to lead to job creation that would not occur absent the award. However, there is evidence that policymakers involved in decisions about CCTC awards pay attention to other factors as well. In particular, other policymakers weigh in via the CCTC Committee that approves each CCTC award. We have read the minutes of the CCTC Committee meetings that occur after each round of applications to approve each CCTC award.[i] While all awards are voted on by the committee, the minutes often reflect specific discussion of a subset of awards – to the best of our knowledge, as requested by committee members – with separate votes on these awards. Thus, the discussion of these specific awards highlights issues that current policymakers view as important.[ii]

We first verified that the discussion of these individual awards at the CCTC Committee meetings accurately reflect what we know about the applicants from the data. Given that the minutes are public and identify specific companies, while the company-identified data are confidential, we cannot provide specific documentation of this accuracy. However, we can report that discussions in the minutes match very closely the various flags in the data regarding whether the company claims it will relocate to another state or terminate employment absent an award, and whether it is promising to create jobs in high-unemployment/high-poverty areas.

More substantively, we identified issues that arise frequently in the minutes in relation to awards discussed by the CCTC Committee. We supplemented this information from the minutes with a search of news stories for ten companies for which the CCTC Committee's discussion reflected these common issues.

The meeting minutes reflected discussion of some factors that were clearly within the purview of GO-Biz in making their discretionary decisions, such as whether the applicant really needed the credit to create jobs,[iii] and whether applicants were just playing localities or states off each other to get tax breaks.[iv] However, other policy considerations emerged in the meeting minutes as well. One issue that surfaced in a number of cases was whether credits were going to companies that were going to automate and/or use artificial intelligence to eliminate jobs.[v] A second was whether the industries were strategically important, such as contributing to the state's zero carbon emissions goals.[vi] A third issue was whether the recipient companies were pursuing or

[i]The committee is chaired by the Director of GO-Biz, and includes the State Treasurer, the Director of the Department of Finance, and an appointee from the State Senate and Assembly. The minutes are available at `https://business.ca.gov/about/publications/`.

[ii]It is likely that GO-Biz staff pay attention to these factors in their discretionary decisions, knowing that the CCTC Committee is attuned to them.

[iii]See, for example, the discussions of Tesla at the June 2015 meeting and Aetna Health at the November 2015 meeting. These concerns were echoed in media reports, including a 2021 *Bloomberg* article that claimed that CCTC awards were increasingly going to large companies, and that "Nine out of 10 of these big companies are going to do these projects regardless" (Mahoney and Brady, 2021).

[iv]See, for example, the discussion of an award to Nordstrom at the April 2016 meeting. As related in an article in the *Sacramento Business Journal* (Young, 2016), one CCTC Committee member said regarding the Nordstrom case that "I get nervous when huge corporations pit communities against each other... I hope Nordstrom is taking a high-road approach and not trying to whipsaw poor cities against each other."

[v]For example, this was raised at the November 2019 meeting in relation to an award to Kroger for warehouse facilities.

[vi]This was raised at the April 2016 meeting in relation to an award to Faraday & Future, and discussed in relation to awards to Proterra, Tesla, and BYD in a statement from the GO-Biz director (`https://business.ca.gov/statement-`

contributing to workforce diversity and inclusion.[vii] A fourth issue was whether the award was likely to be associated with training opportunities.[viii] And a fifth issue was general employer treatment of workers, such as resistance to unions and employee benefits.[ix]

Appendix B References

MAHONEY, L. AND C. BRADY (2021): "California's Tax Incentive's Success Is in Its Failures," *Bloomberg Tax*, August 17.

YOUNG, A. (2016): "Battle Over State Tax Credits Pits California Cities Against Each Other," *Sacramento Business Journal*, April 15.

from-the-governors-office-of-business-and-economic-development-director-panorea-avids-following-governor-browns-final-state-of-the-state-address/).

[vii] This came up often – for example, in discussions at the April 2016 meeting about awards to Faraday & Future and Snapchat, and at the June 2016 meeting about awards to NextEV and GreenPower Motor Company.

[viii] This also came up often – for example, in discussions at the April 2017 meeting about awards to General Motors and Proterra, and at the November 2019 meeting about awards to Systems Machine Automation Components and Northrop Grumman Systems.

[ix] For example, issues of worker treatment were raised in discussing an award to National Steel and Shipbuilding at the April 2015 meeting, and issues of nonunion labor were raised in discussing an award to Northrop Grumman at the November 2019 meeting.

C Examples of Annual Milestones: Tesla Motors and Centene Corp.

Exhibit A Milestones

Taxpayer: Tesla Motors, Inc.

	2014 Tax Year (Base)	2015 Tax Year	2016 Tax Year	2017 Tax Year	2018 Tax Year	2019 Tax Year	Total
Total California Full-Time Employees[1]	6,463	8,058	9,126	10,011	10,548	10,889	
Net Increase of Full-Time Employees Compared to the Base Year		1,595	2,663	3,548	4,085	4,426	
Minimum Annual Salary of California Full-Time Employees Hired		$35,000	$35,000	$35,000	$35,000	$35,000	
Cumulative Average Annual Salary of California Full-Time Employees Hired		$55,000	$55,000	$55,000	$55,000	$55,000	
Investments		$693,280,000	$357,700,000	$430,750,000	$419,160,000	$488,590,000	$2,389,480,000
Tax Credit Allocation		$0	$500,000	$1,500,000	$2,500,000	$10,500,000	$15,000,000

[1] Determined on an annual full-time equivalent basis

NOTES—Milestones are from GO-Biz negotiated agreement "California Competes Tax Credit Allocation Agreement." Tesla milestones accessible at `https://static.business.ca.gov/wp-content/uploads/2019/08/teslaamended.pdf`.

Exhibit A
Milestones

Taxpayer: Centene Corporation (Credit Recipient), et al.

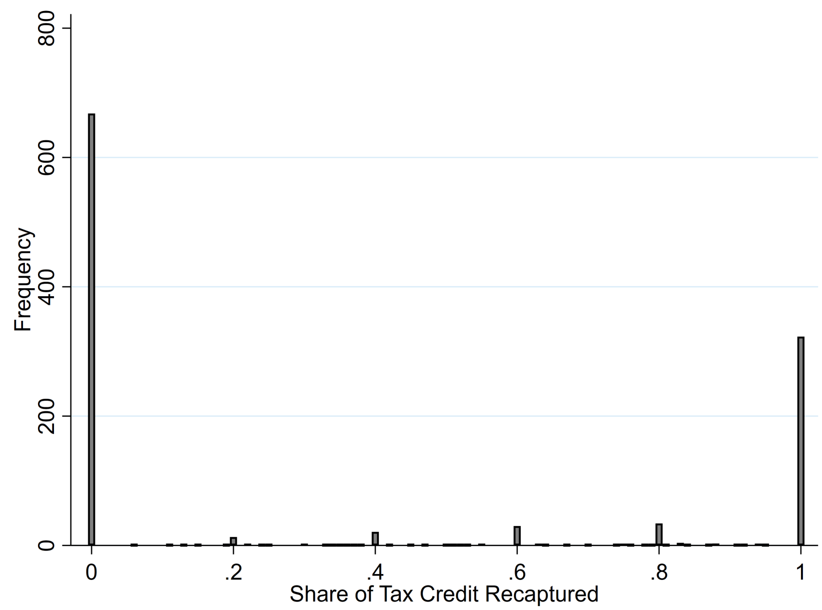
	2015 Tax Year (Base)	2016 Tax Year	2017 Tax Year	2018 Tax Year	2019 Tax Year	2020 Tax Year	Total
Total California Full-Time Employees[1]	5,848	5,880	6,415	7,120	7,278	7,380	
Net Increase of Full-Time Employees Compared to the Base Year		32	567	1,272	1,430	1,532	
Minimum Annual Salary of California Full-Time Employees Hired		$39,000	$39,000	$39,000	$39,000	$39,000	
Cumulative Average Annual Salary of California Full-Time Employees Hired		$83,000	$83,000	$83,000	$83,000	$83,000	
Investments		$0	$19,400,000	$28,000,000	$7,000,000	$45,721,000	$100,121,000
Tax Credit Allocation		$500,000	$1,625,000	$1,625,000	$1,625,000	$1,625,000	$7,000,000

[1] Determined on an annual full-time equivalent basis

NOTES—Milestones are from GO-Biz negotiated agreement "California Competes Tax Credit Allocation Agreement". Centene milestones accessible at https://static.business.ca.gov/wp-content/uploads/2020/10/Centene-Corporation-CCTC-AgreementAmended.pdf.

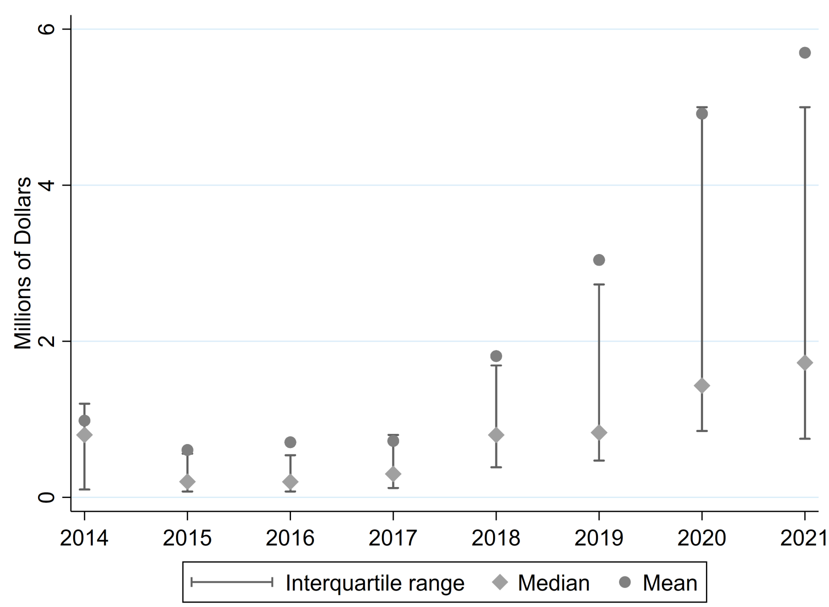
D Supplementary Descriptive Statistics

Figure D.1: Histogram of Share of Tax Credits Recaptured by CCTC



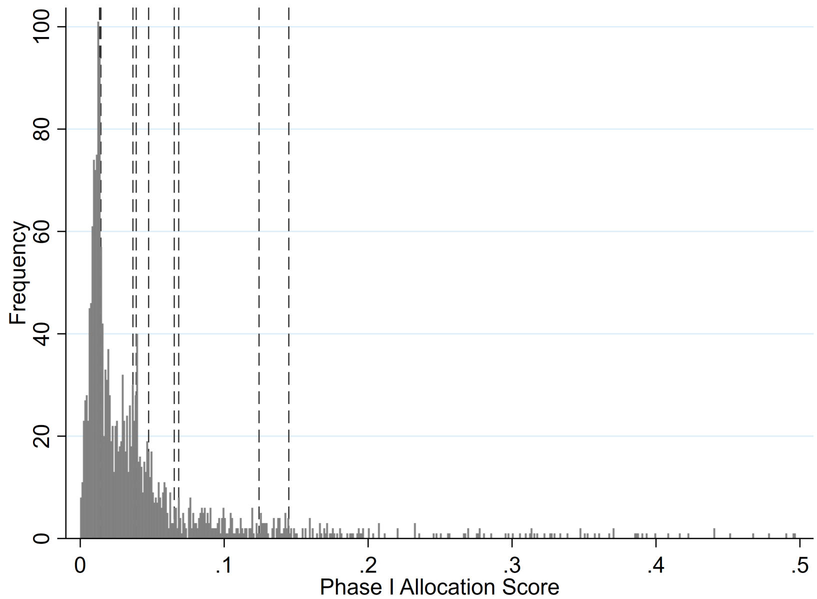
NOTES—Data on awardees is publicly available on the GO-Biz website at https://business.ca.gov/california-competes-tax-credit/awardee-list/ (accessed on 04/04/22). In cases where credits are fully captured by the end of the five-year commitment, recapture may occur at each year, likely resulting in some bunching at each 20% interval until the full amount is recouped.

Figure D.2: Whisker Plot of Tax Credits Allocated by Calendar Year



NOTES—Data on awardees is publicly available on the GO-Biz website at https://business.ca.gov/california-competes-tax-credit/awardee-list/ (accessed on 04/04/22). Means are skewed right by a very large right tail in each year.

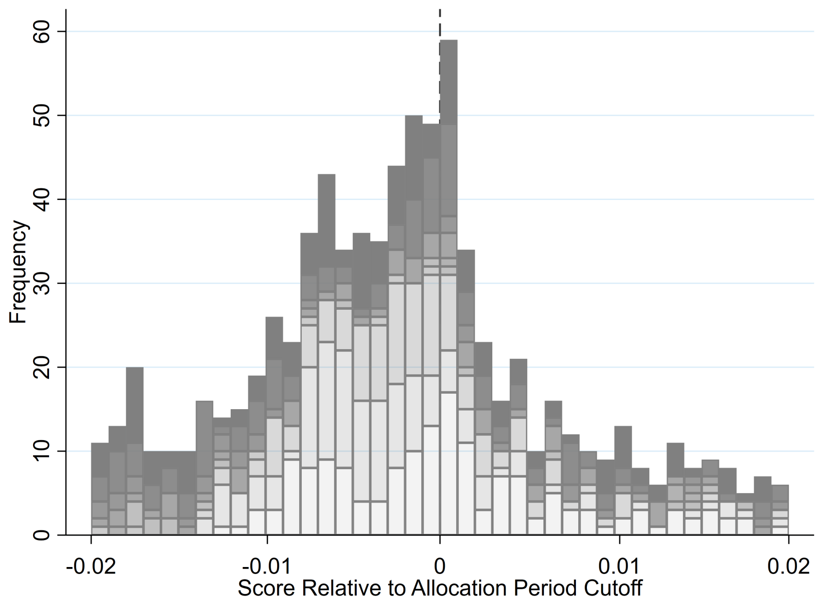
Figure D.3: Raw Score Distribution with Overlaid Cutoffs



NOTES—Figure shows the distribution of raw scores overlaid with the score cutoffs for the 10 allocation periods in the analysis sample. Vertical dashed lines denote the 10 allocation period cutoffs considered in the analysis sample.

In Appendix Figure D.4, we decompose the underlying density by each allocation period. While consistency of the RD estimator only relies on sufficient mass close to the pooled cutoff, this figure also reveals that densities do not appear to be dramatically higher just to the left of the cutoff relative to the right for any given period.

Figure D.4: Stacked Histogram for Relative Scores by Allocation Period



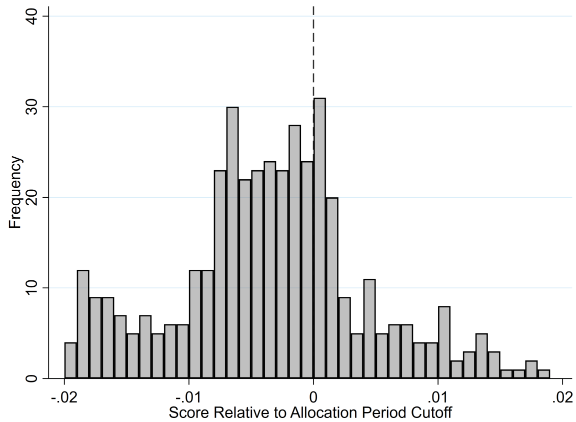
NOTES—Figure decomposes the histogram of relative scores by allocation period, where lighter shades denote earlier periods and darker shades denote more recent periods.

In Appendix Figure D.5, we also show histograms for two subgroups of applicants for which one might be especially concerned about manipulation. In panel (a), we show effects separately for applicants that paid a consultant a fee to fill out their application to the CCTC (an indicator that we observe for all applicants, and amounts to 46% of applicants). Neither those who use or do not use a consultant appear to bunch immediately below the cutoff. Panel (b) also separates applicants by whether they are a first-time applicant (66% of applicants), or repeat applicant (34% of applicants) – the latter being potentially susceptible to learning from past allocations. Again, here, we see no obvious evidence of excess mass to the left of the cutoff for repeat applicants. The pattern in the figure points to more of a level shift to lower scores for all repeat applicants as opposed to any precise manipulation around score cutoffs. This is consistent with Appendix Figure D.6, which shows that to the extent learning occurs, it tends to shift the entire distribution lower rather than lead to a large differential mass just to the left of the cutoff.

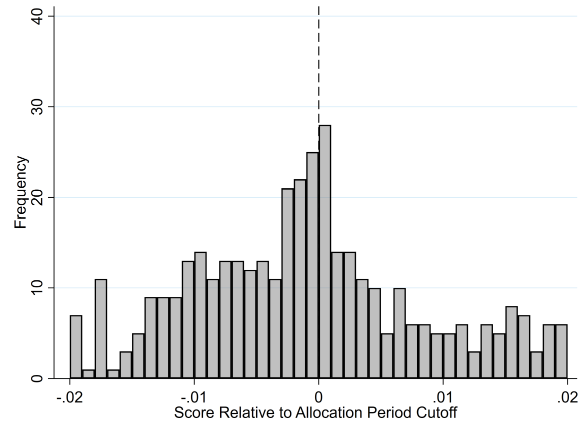
Figure D.5: Histograms by Consultant and Repeat Applicant Status

Panel A. Density by whether Paid Consultant Filled Out CCTC Application

(a) Used Consultant

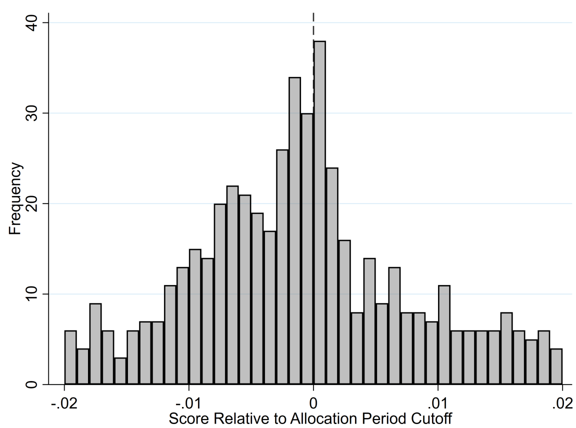


(b) No Consultant

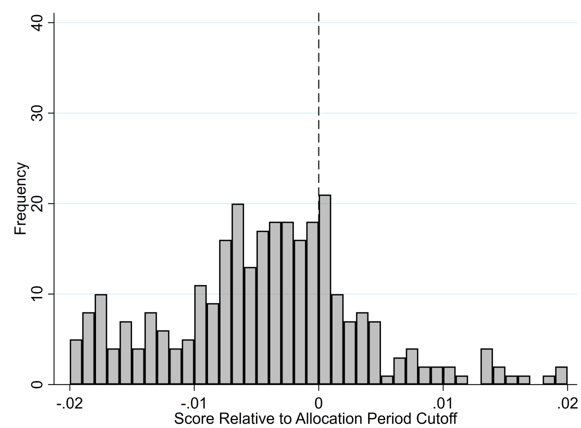


Panel B. Density by First Time vs. Repeat Applicants:

(c) First Time Applicants

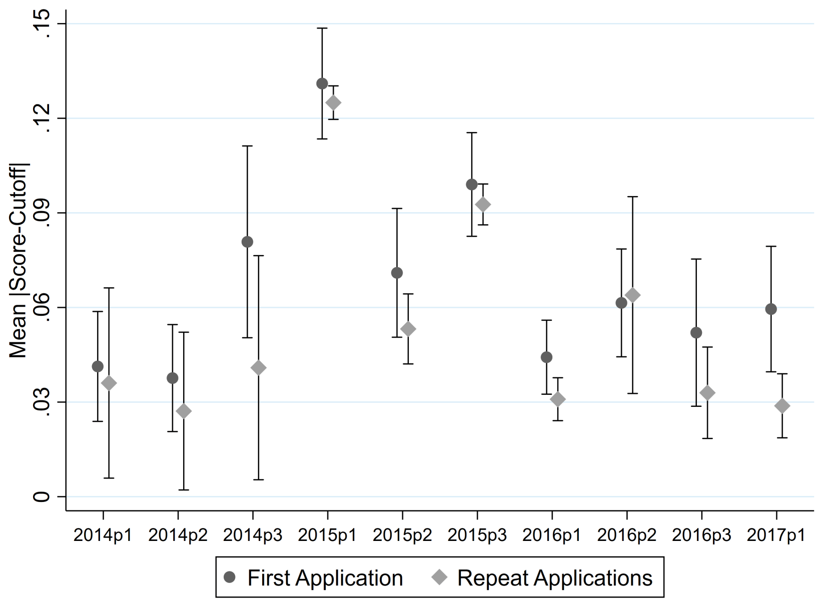


(d) Repeat Applicants



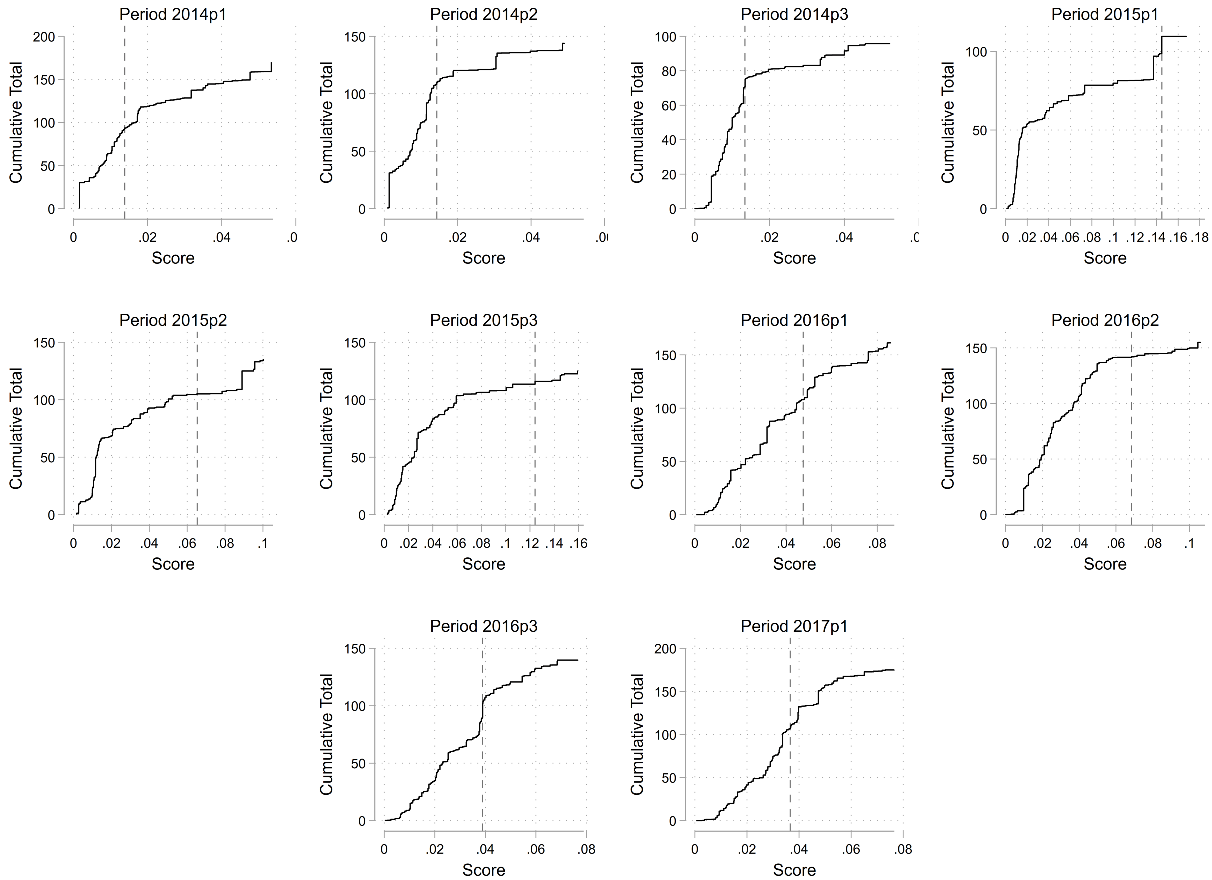
NOTES—Figures show densities of applicants by whether applicant had consultant (46% of applicants) or no consultant (54% of applicants), and by whether applicant was a first-time (66%) versus repeat (34%) applicant.

Figure D.6: Learning Among Repeat Applicants



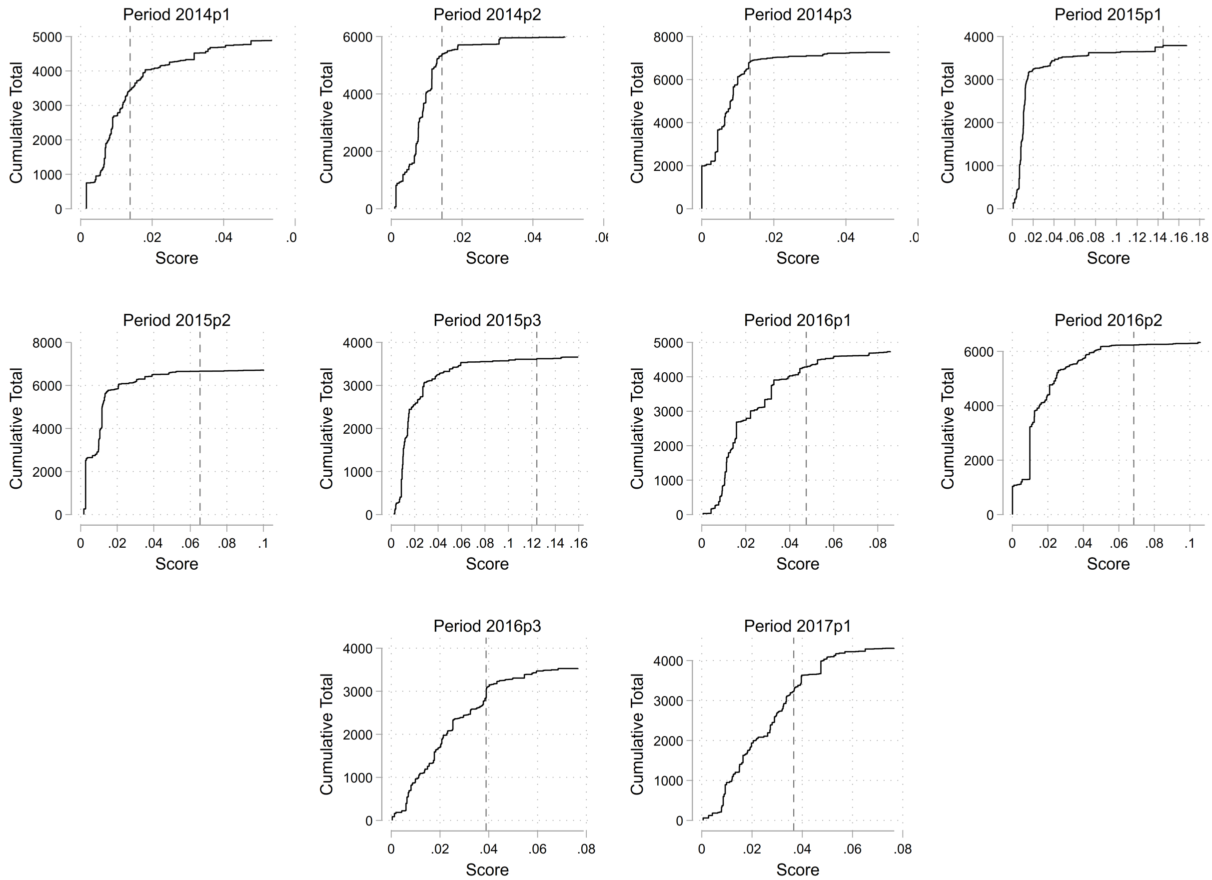
NOTES—Figure shows mean absolute value of distance between each applicant's score and allocation period cutoff, separately for first time applicants and repeat applicants (within firm). While 66% of overall applicants are first-time applicants within firm (34% are repeat applicants), roughly 50% of all applicants apply to the CCTC only once. The figure shows a small downward level shift, indicating that repeat applicants get marginally closer to the score cutoff, albeit they remain far away from zero (bunching around which would be more consistent with manipulation).

Figure D.7: Cumulative Totals for Aggregate Tax Credits Requested by Allocation Period



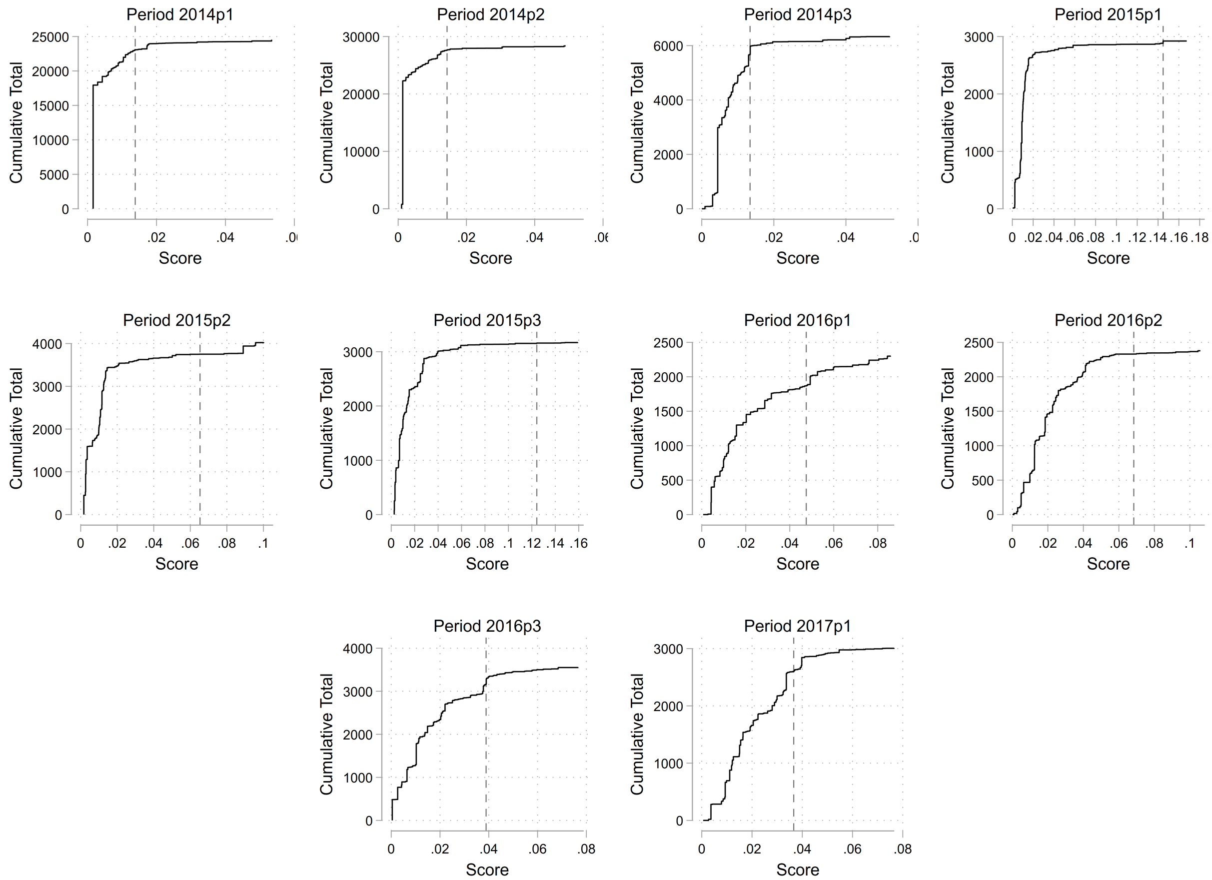
NOTES—Figure shows the cumulative running totals for aggregate tax credits (in millions) requested by applicants ordered by their cost-benefit score (low to high), separately for each allocation period. Vertical dashed lines denote the CCTC cutoff where tax credits cumulatively total 200% of the budgeted amount for each allocation round.

Figure D.8: Cumulative Totals for Aggregate Compensation by Allocation Period



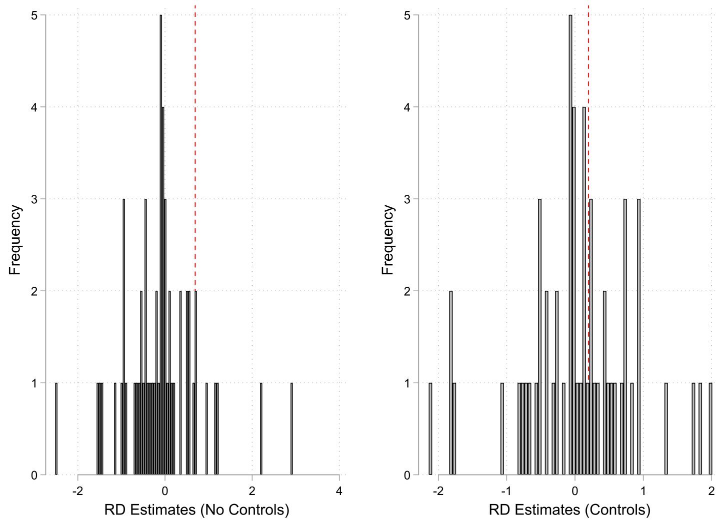
NOTES—Figure shows the cumulative running totals for aggregate compensation (in millions) proposed by applicants ordered by their cost-benefit score (low to high), separately for each allocation period. Vertical dashed lines denote the CCTC cutoff where tax credits cumulatively total 200% of the budgeted amount for each allocation round.

Figure D.9: Cumulative Totals for Aggregate Investment by Allocation Period



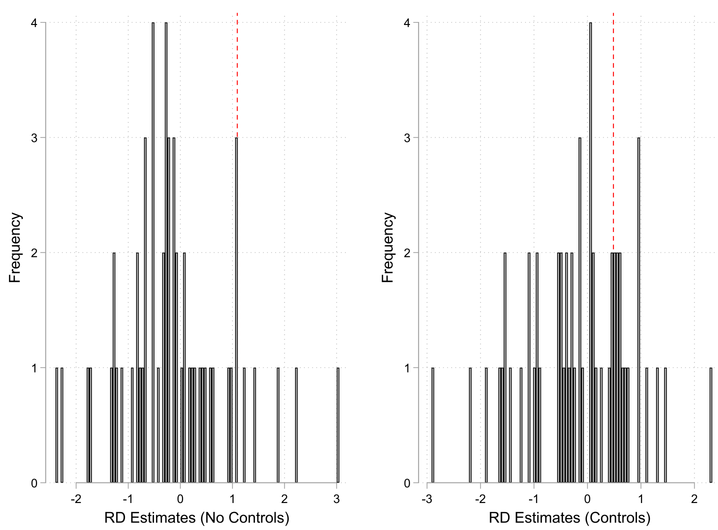
NOTES—Figure shows the cumulative running totals for aggregate investment (in millions) proposed by applicants ordered by their cost-benefit score (low to high), separately for each allocation period. Vertical dashed lines denote the CCTC cutoff where tax credits cumulatively total 200% of the budgeted amount for each allocation round.

Figure D.10: Distribution of RD Estimates for Log Aggregate Compensation at Placebo Cutoffs



NOTES— The histograms show the distribution of regression discontinuity estimates for log aggregate compensation at various (placebo) cutoffs. There are 57 estimates in total, corresponding to cutoffs at scores ranging from -0.14 to 0.14 in increments of 0.005. The specification for estimates in the left panel includes no controls. The right panel corresponds to RD estimates using the covariate-adjusted optimal bandwidth (Calonico et al., 2019) procedure, and includes industry fixed effects, allocation period fixed effects, and baseline controls. The vertical dashed red lines represent the estimated discontinuity at the actual cutoff (at score zero), which lies well within the range of estimates.

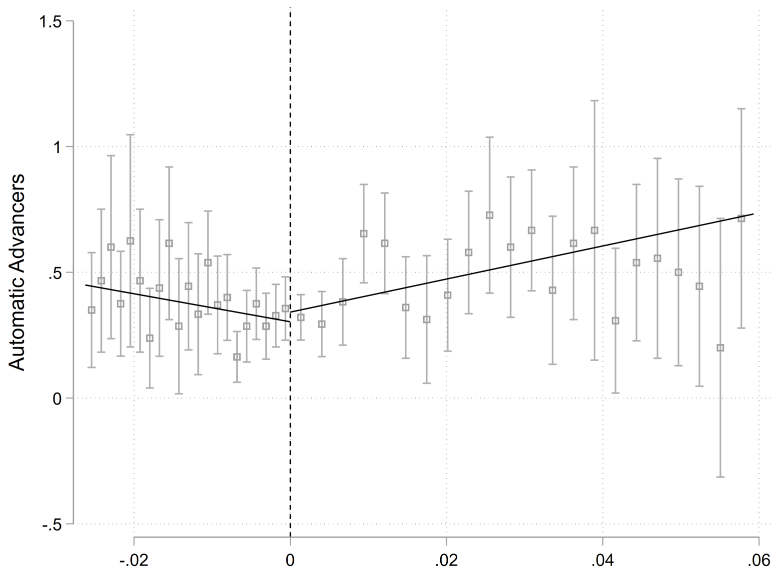
Figure D.11: Distribution of RD Estimates for Log Aggregate Investment at Placebo Cutoffs



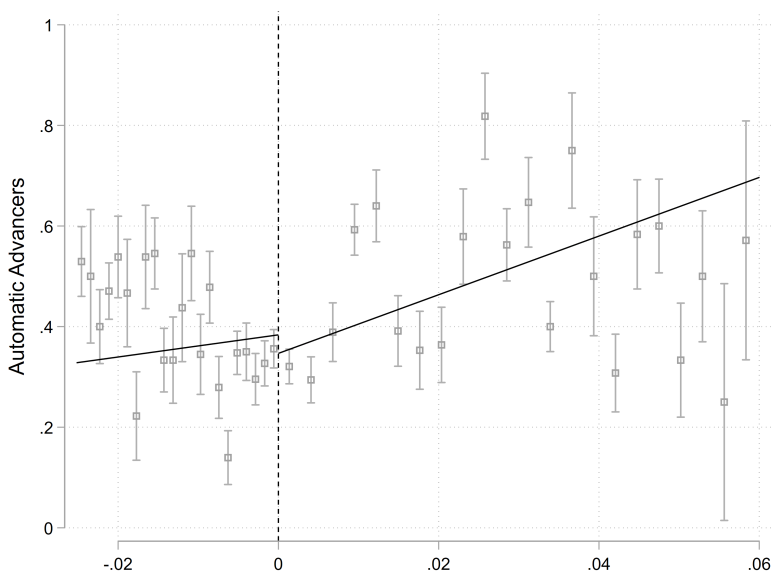
NOTES— The histograms show the distribution of regression discontinuity estimates for log aggregate investment at various (placebo) cutoffs. There are 57 estimates in total, corresponding to cutoffs at scores ranging from -0.14 to 0.14 in increments of 0.005. The specification for estimates in the left panel includes no controls. The right panel corresponds to RD estimates using the covariate-adjusted optimal bandwidth (Calonico et al., 2019) procedure, and includes industry fixed effects, allocation period fixed effects, and baseline controls. The vertical dashed red lines represent the estimated discontinuity at the actual cutoff (at score zero), which lies well within the range of estimates.

Figure D.12: RD Results for Automatic Advancers

(a) No Controls



(b) Full Set of Controls



NOTES— Figure shows RD plots for the dummy variable that indicates whether a firm advances to phase two of the evaluation process automatically (irrespective of score). The RD estimate is not significant. Estimates use covariate-adjusted IMSE-optimal bandwidths following the Calonico et al. (2019) procedure. Panel (a) includes no controls, whereas panel (b) includes industry fixed effects, allocation period fixed effects, single-unit (vs. multi-unit) status, an indicator for being an S-corporation, an indicator for C-corporation, and an indicator for whether the firm is publicly traded. The figure is based on application data obtained from GO-Biz (accessed outside the Census Research Data Center).

Table D.1: Tax Credit Awards and Recaptures by Year

Year	Number of Awards	Average Award ($Mil)	Total Credits Awarded ($Mil)	Total Credits Recaptured ($Mil)	Share Recaptured
2014	29	0.98	28.52	15.15	0.531
2015	301	0.61	182.64	87.32	0.478
2016	244	0.71	172.22	79.98	0.464
2017	291	0.72	210.32	35.28	0.168
2018	109	1.81	197.29	29.99	0.152
2019	69	3.04	209.88	3.93	0.019
2020	46	4.92	226.17	1.20	0.005
2021	52	5.70	296.29	0.00	0.000
Total	1,141	1.34	1,523.32	252.85	0.166

NOTES—Data on awardees is publicly available on the GO-Biz website at https://business.ca.gov/california-competes-tax-credit/awardee-list/ (accessed 04/04/22). For 2014-2017, the share recaptured is 36.7%, higher than the 16.6% for the 2014-2021 timeframe. Given that more recent awardees have not yet reached the end of their five-year contracts, the former estimate may better reflect the ultimate recapture rate.

Table D.2: Top 20 CCTC Tax Credit Awards, 2014-2021 (All Awards)

Applicant Name	**Tax Credits Awarded**	**Proposed Investment Increase**	**Proposed Employment Increase**	**Industry**	**Year**
Lockheed Martin Corp.	39,500,000	150,000,000	500	Aerospace Manufacturing	2019
Lockheed Martin Corp.	39,500,000	100,000,000	450	Aircraft Development and Manufacturing	2018
Microsoft Corporation	35,000,000	175,000,000	2,085	Software Development	2021
Relativity Space, Inc.	30,000,000	319,800,000	964	Aerospace R&D and Manufacturing	2021
Northrop Grumman Systems Corp.	30,000,000	1,147,000,000	1,001	Aircraft Manufacturing	2019
Lockheed Martin Corp.	29,800,000	100,000,000	450	Aerospace Manufacturing	2020
Stripe, Inc.	28,500,000	85,000,000	2,086	Software Development	2021
EnerVenue, Inc.	25,000,000	406,100,000	1,692	Battery Manufacturing	2021
Better Holdco, Inc.	25,000,000	55,428,938	3,500	Consumer Lending	2020
Sorrento Therapeutics, Inc.	25,000,000	68,200,000	1,034	Biopharmaceutical R&D and Manufacturing	2021
VinFast Dealer San Francisco #1 LLC	20,500,000	200,400,000	1,065	Electric Vehicle Manufacturing Headquarters and Sales	2021
Samsara Networks, Inc.	20,000,000	189,800,000	2,098	Sensor R&D	2019
Cue Health, Inc.	20,000,000	150,000,000	1,667	Medical Device Manufacturing	2021
NBCUniversal LLC	20,000,000	132,000,000	800	Online Streaming Media Service Provider	2020
Cepheid	20,000,000	250,000,000	2,510	Diagnostic Test Manufacturing	2020
Atieva USA, Inc.	18,000,000	46,400,000	1,858	Electric Automobile R&D	2020
Universal Standard Housing LLC	17,238,000	100,000,000	560	Prefabricated Building Manufacturing	2020
Northrop Grumman Innovation Systems, Inc.	15,000,000	57,000,000	337	Aerospace R&D and Manufacturing	2020
Tesla Motors, Inc.	15,000,000	2,389,000,000	4,426	Automobile Manufacturing	2015
MP Materials Corp.	14,790,000	190,500,000	203	Mineral Extraction	2021

NOTES—Data on awardees is publicly available on the GO-Biz website `https://business.ca.gov/california-competes-tax-credit/awardee-list/` (accessed on 04/04/22). Investments refer to the qualified investment per each applicant's contract commitment.

E Matching CCTC Applicants to the Longitudinal Business Database

Our goal is to match each CCTC application to the set of establishments spanned by the applicant's EIN(s) over time. To this end, we define the "focal" establishment associated with the application, which is generally the best establishment based on rules described below. The focal establishment helps verify that the applicant has an existing high-quality match in the data (precluding "ghost applicants" with zero economic activity throughout the panel, while accommodating new startups). Matching on EINs allows us to measure how much LBD economic activity (i.e., number of establishments, March 12 employment stock, annual payroll flow, annual revenue flow) is located in a particular geography for both winning and losing CCTC applicants.

Retaining all EIN(s) spanned by the focal establishment facilitates the estimation of substitution patterns within US firms across locations, and allows for the appropriate tracking of economic activity even when a focal establishment expansion is accompanied by an organizational change in EIN.[x]

We begin with a list of approximately 3,800 CCTC applicants across 13 allocation periods from 2014 to 2018, which we first match to the U.S. Census Bureau's Business Register / Standard Statistical Establishment List (BR/SSEL) for 2013-2016 applicants, and County Business Patterns Business Register (CBPBR) for 2017-2018 applicants.[xi] The CCTC data provide the applicant's company name and address, legal name, and the address of the (non-binding) location for the proposed expansion of economic activity tied to a potential tax credit. In addition to these fields, about 97% of applicants report the federal EIN associated with the applicant firm.[xii]

Step 1: Broad Match. We first use the federal EIN to search for all potential establishment candidates in all lagged, current, and lead years, separately across single-unit and multi-unit establishment lists in the BR/SSEL and CBPBR from 2012 to 2018, keeping all establishment candidates for whom an applicant is found in either single- or multi-unit files. For those not yet matched, we match the applicant's proposed project location address with the BR/SSEL and CBPBR primary physical address in both the current and one-year lagged application years. For those remaining unmatched, we match the applicant's company name in both current and one-year lagged application years. Finally, for the remaining set, we perform a fuzzy match on the concatenated string address + company name, and keep all *reclink2* scores > 0.75.[xiii] This procedure results in well over 90% of of applicants having at least one focal establishment candidate, the vast majority of which are matched based on EIN. We carry these many-to-one matches over to Step 2 of our matching procedure.[xiv]

Step 2: Precise Match. In Step 2, we develop a set of heuristics to reduce these many-to-one broadly matched focal candidates to unique establishments, whittling down the final list of match

[x] In such cases, the LBDNUM panel identifier could have a different EIN across years; examining a non-focal establishment at the applicant EIN risks missing economic activity when the EIN changes, which could potentially be important when a tax incentive induces an expansion or reorganization.

[xi] The CBPBR supplanted the BR/SSEL in 2017. See Chow et al. (2021) for further details.

[xii] Per our data use agreement, GO-Biz shared EINs with the research team via a secure encrypted transfer, and the integration with LBD was conducted on secure servers in Federal Statistical Research Data Centers.

[xiii] *Reclink2* was developed by Wasi and Flaaen (2015) for Stata, and uses a bigram string comparator algorithm that computes the total share of matched pairs of consecutive characters.

[xiv] For those unmatched after Step 1, the majority report no baseline employees, suggesting that these are new startups. Most are tagged as "growth projects" in CCTC administrative data, which can include establishments registering with new EINs, as well as those which may not have launched yet. Roughly one third are single-member LLCs or sole proprietorships, which may also have a lower likelihood of opening, making them less likely to appear in Census business register data.

candidates that need to be matched by hand in Step 3 (described below) to a more manageable set. We proceed in a series of sub-steps:

(A) We consider all current one-to-one applicant-establishment matches as "focal-matched" (which we put aside to hand check later), and move to applicants that still have multiple establishment candidates to choose from.

(B) Starting with Step 1 matches based on EIN, we drop all lagged and lead candidate matches except the previous, current, and subsequent year, providing a maximum of three possible match candidates. If a perfect address match results in a one-to-one unique match among these three, we use that match and tag the application as focal-matched.

(C) For the remaining unmatched applicants, we iterate through each Step 1 match based on address and company name, and tag the application as focal-matched if:

- the exact company name and address results in a one-to-one match;
- of the remaining unmatched, exact company name and address matching results in multiple matches across potential match years, but is one-to-one in the application year (we drop all other years);
- of the remaining unmatched, the state or project location ZIP code results in a one-to-one match;
- of the remaining unmatched, throwing out all but the top 10 potential address or name candidates based on Levenshtein string distance results in a one-to-one match.[xv]

(D) Next, we import the panel identifier of interest for all candidates (the 2020 LBDNUM vintage), and use this to further map any remaining unmatched candidates that would otherwise have equivalent company name and address information, one-to-one to establishments.[xvi] If there is only one populated LBDNUM among possible candidates, we assign the application to that establishment as the focal match.[xvii] In the handful of cases where there remain multiple LBDNUM's each year with otherwise equivalent information, we arbitrarily keep the first LBDNUM and drop the rest, noting that these are multi-establishment firms.[xviii]

(E) Finally, for the remaining unmatched applicants, we implement a second fuzzy *reclink2* match on the concatenated string address + company name, keeping *reclink2* scores > 0.60 (the default), and keep all one-to-one matches resulting from this step, tagging the quality of each match based on *reclink2* score, and whether the match was in the application year or a different year.

Step 3: Manual Match. At this point, we have generated match flags corresponding to each sub-step (A) – (E), resulting in a small subset of applicants that need to be hand-matched to

[xv] We use the Stata command *strdist* (Barker and Pöge, 2017). To break ties, we also keep the top name (address) match when address (name) produces 10 or more top matches with the same *strdist* score.

[xvi] This sub-step also includes the previously unincorporated Step 1 "Fuzzy" matches based on *reclink2*.

[xvii] If there is only one LBDNUM in the proposed ZIP, we make that the focal establishment. If not, we move on to unique establishments within state, the highest payroll LBDNUM, the highest payroll LBDNUM within state, and finally the highest payroll LBDNUM overall, to assign these cases a focal establishment.

[xviii] Because we ultimately pull all LBDNUM's associated with the EIN, we are able to preserve the collapsed "wide" information detailing other LBDNUM's associated with the arbitrarily chosen focal establishment in this case, such that no information is lost.

focal establishments, and another set that has been put aside to be hand-checked. Of these, we drop any applicant that is a clear mismatch, and map all remaining many-to-one candidates to a single focal establishment. When the company name or address appears to be a mismatch, but the EIN matches in either the previous, current, or subsequent match year, we let the EIN take precedence and do not drop observations. When the only source of the mismatch appears to be that one of either the Census or CCTC application data report an individual's name while the other reports a company name, we also do not drop this observation as the difference likely amounts to a simple reporting inconsistency (most common in sole-proprietorships and LLCs). Finally, we draw three random samples of 100 applicants and hand check their match quality. After applying our matching algorithm and sampling restrictions, we match nearly all of the 1,900 applicants.

Appendix E References

BARKER, M. AND F. PÖGE (2017): "STRDIST: Stata Module to Calculate the Levenshtein Distance, or Edit Distance, Between Strings," Boston College Department of Economics.

CHOW, M., T. FORT, C. GOETZ, N. GOLDSCHLAG, N. LAWRENCE, E. R. PERLMAN, M. STINSON, AND T. K. WHITE (2021): "Redesigning the Longitudinal Business Database," Center for Economic Studies Working Paper CES-21-08, U.S. Census Bureau.

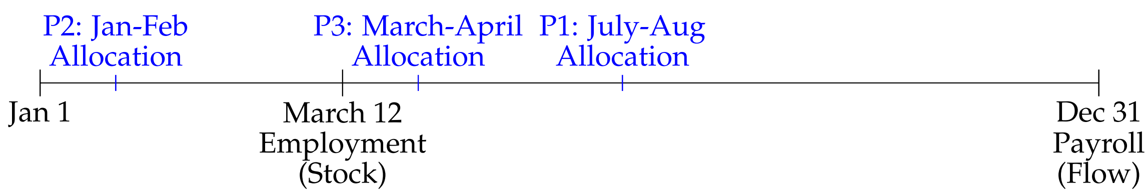
WASI, N. AND A. FLAAEN (2015): "Record Linkage Using Stata: Preprocessing, Linking, and Reviewing Utilities," *The Stata Journal*, 15, 672–697.

F Policy Timing Details

There are three periods of CCTC applications (and awards) per 12-month period, which are dated by fiscal years. Each fiscal year begins in July of the previous calendar year. We refer to each period of application (and awards) within a fiscal year as an "allocation period." We restrict attention to allocation periods that allow us to observe applicants for at least five years before and three years after applying (with the latter including the application year).

The specific dates of the application windows for each period in our data appear in Appendix Table F.1. As can be seen in the table, the first allocation period of each fiscal year (P1) is in a different calendar year than allocation periods 2 and 3 (P2 and P3). As discussed in the text, Census payroll data are year-end calendar measures of total payroll expended, while the employment data are as of March 12 of each calendar year. We summarize the policy and data timing as well as exposure information in Appendix Figure F.1.

Figure F.1: LBD Data Timing



Event Year (τ) = Calendar Year of LBD Data – Calendar Year of CCTC Allocation

Period	Employment Exposure	Payroll Exposure
P1	None in $\tau = 0$, full in $\tau = +1$	Partial in $\tau = 0$, full by $\tau = +1$
P2	Partial in $\tau = 0$, full in $\tau = +1$	Near-Full in $\tau = 0$, full by $\tau = +1$
P3	None in $\tau = 0$, full in $\tau = +1$	Near-Full in $\tau = 0$, full by $\tau = +1$

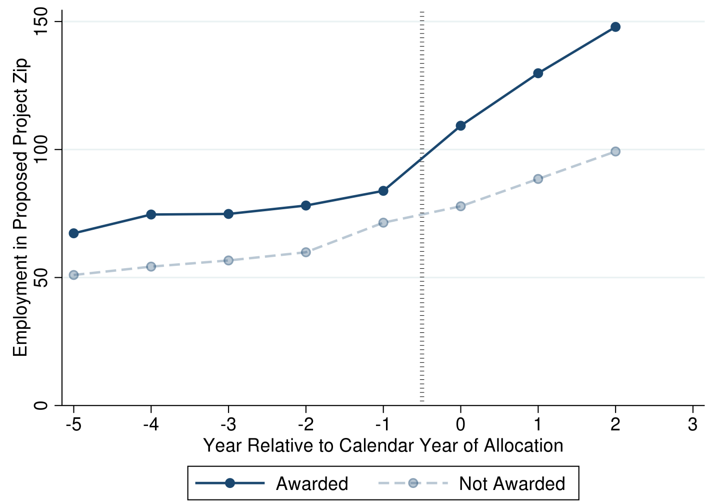
Table F.1: CCTC Application Rounds and Budgets in Main Sample

	FY 2014-15 ($150 mil.)	**FY 2015-16** ($200 mil.)	**FY 2016-17** ($200 mil.)	**FY 2017-18** ($200 mil.)
P1	Sep 29, '14 - Oct 27, '14	Jul 20, '15 - Aug 17, '15	Jul 25, '16 - Aug 22, '16	Jul 24, '17 - Aug 21, '17
P2	Jan 5, '15 - Feb 2 '15	Jan 4, '16 - Jan 25, '16	Jan 2, '17 - Jan 23, '17	
P3	Mar 9, '15 - Apr 6 '15	Mar 7, '16 - Mar 28, '16	Mar 6, '17 - Mar 27, '17	

NOTES—Budgets are determined at the beginning of the fiscal year and split across the three allocation periods. The third period in each fiscal year also includes any remaining unallocated amounts from previous allocation periods.

G Supplementary Results

Figure G.1: CCTC Applicant Firm Employment by Awardee Status in Proposed ZIP Codes

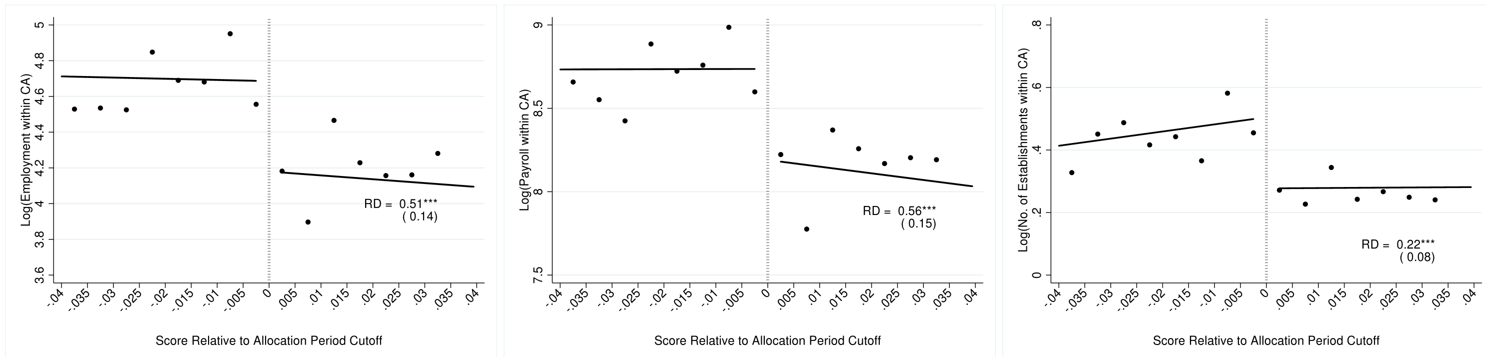


NOTES—Figure shows employment levels of for the full sample of awarded and non-awarded applicants in their proposed ZIP codes relative to the calendar year of application.

Figure G.2: Pooled RD Results for Activity within California, Fixed Bandwidth but without Baseline Controls

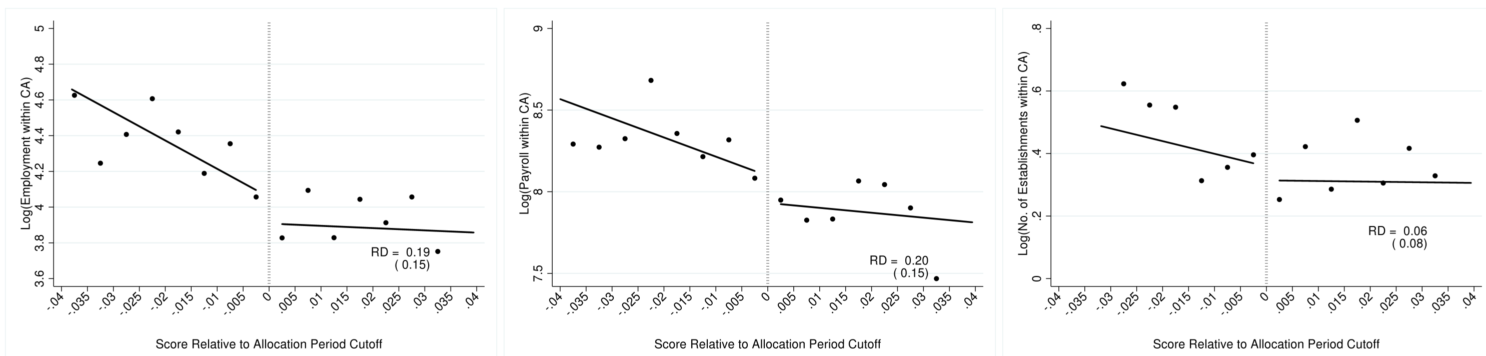
Panel A. Two Years After Credit Allocation Year ($\tau = +2$)

(a) Employment (b) Payroll (c) Establishments



Panel B. Two Years Prior to Credit Allocation Year (Placebo, $\tau = -2$)

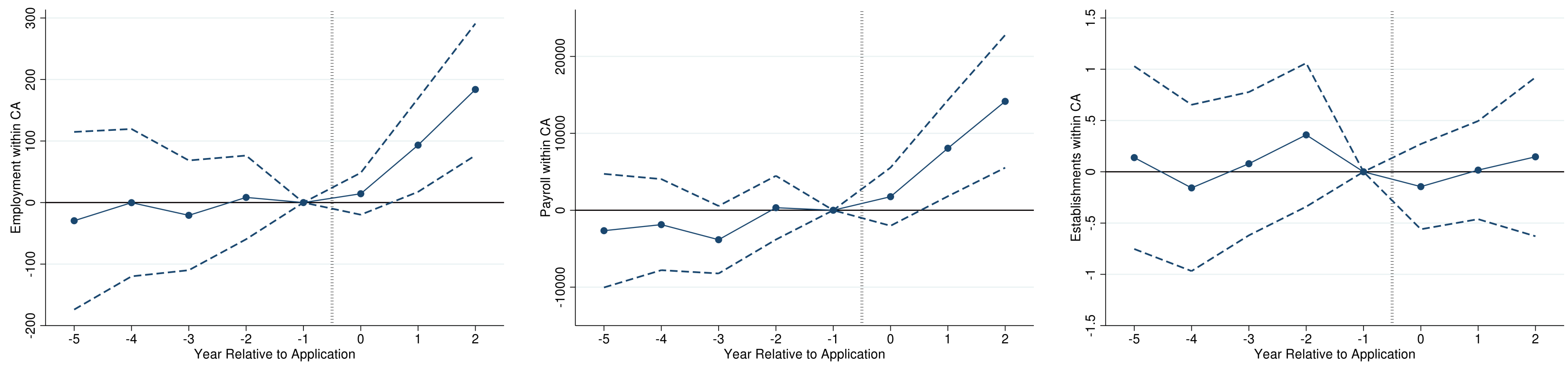
(d) Employment (e) Payroll (f) Establishments



NOTES—Figures show regression slopes and intercepts overlaid on top of equally spaced pre-binned outcome data with a bin size of 0.005. Plots are shown over the optimal bandwidth selected using the IMSE-procedure, which produces asymmetric optimal bandwidth boundaries for each sample. Plots residualize outcomes by industry and allocation period fixed effects (added back to their overall mean) prior to mean-collapsing by bin, but do not include baseline controls associated with the most saturated model.

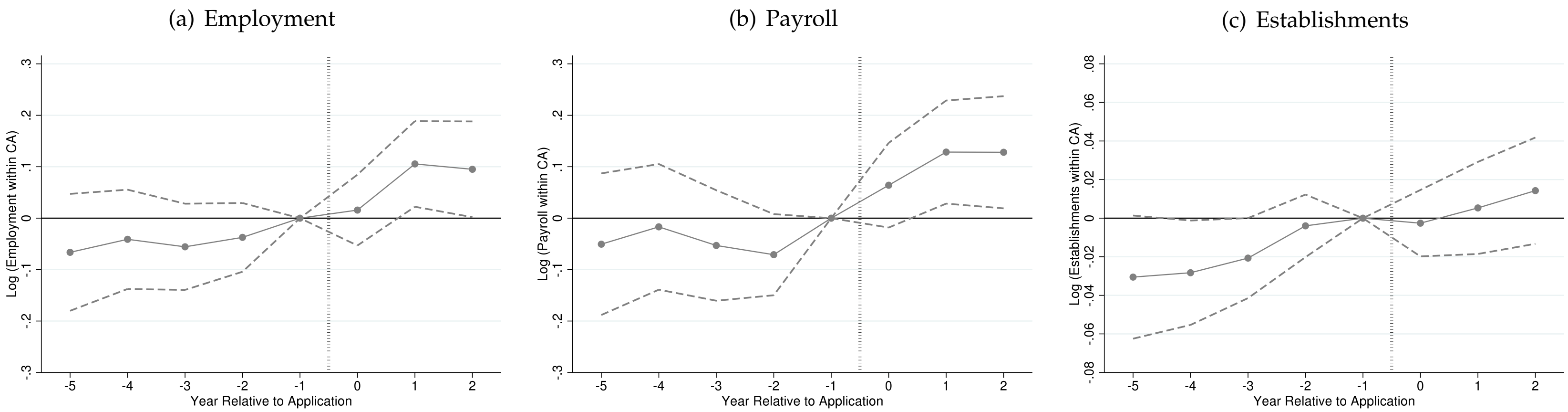
Figure G.3: Dynamic RD Coefficient Plots over Event Time for Activity within California, Levels

(a) Employment (b) Payroll (c) Establishments



NOTES—Estimates based on equation (3) using the methodology developed by Cellini et al. (2010). Regressions include the complete history of applications as well as EIN and year fixed effects. Standard errors are clustered at the EIN level.

Figure G.4: Dynamic RD Coefficient Plots over Event Time for Activity within California, Ignoring Repeats

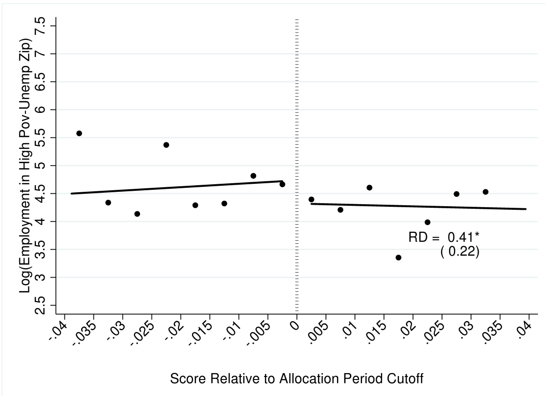


NOTES—Estimates based on equation (3) using the methodology developed by Cellini et al. (2010), but ignoring repeat applicants by treating each application as a separate EIN. Regressions include application and year fixed effects (application history drops out given applications are unique for this sample). Standard errors are clustered at the application level.

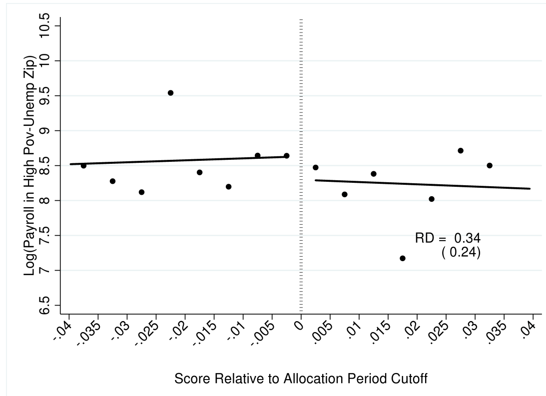
Figure G.5: Pooled RD Results for Activity in High-Poverty/High-Unemployment ZIP Codes in California (Fixed Bandwidth)

Panel A. Two Years After Credit Allocation Year ($\tau = +2$)

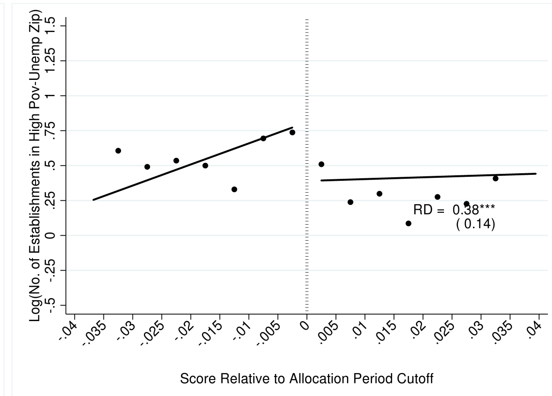
(a) Employment



(b) Payroll

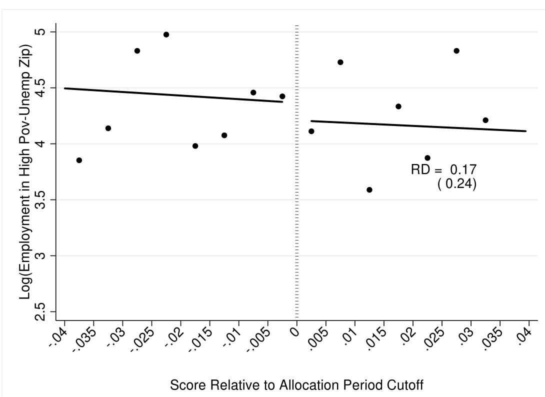


(c) Establishments

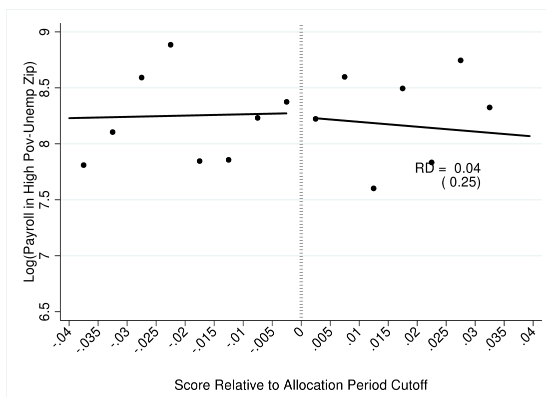


Panel B. Two Years Prior to Credit Allocation Year (Placebo, $\tau = -2$)

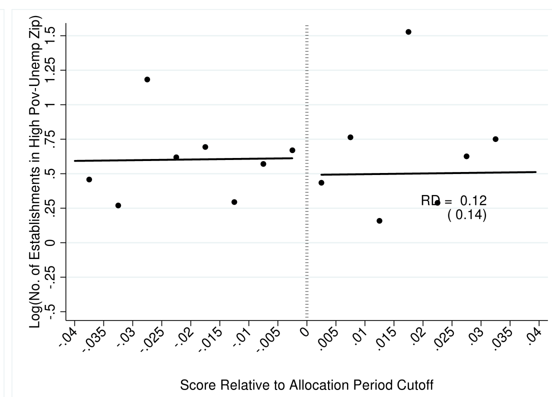
(d) Employment



(e) Payroll



(f) Establishments

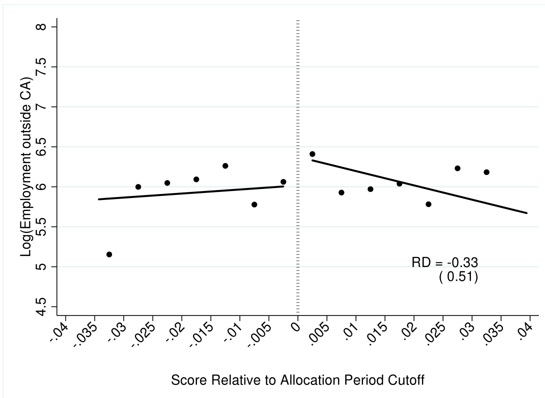


NOTES—Figures show regression slopes and intercepts overlaid on top of equally spaced pre-binned outcome data with a bin size of 0.005. Plots are shown over the optimal bandwidth selected using the IMSE-procedure, which produces asymmetric optimal bandwidth boundaries for each sample. Plots residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to mean-collapsing by bin.

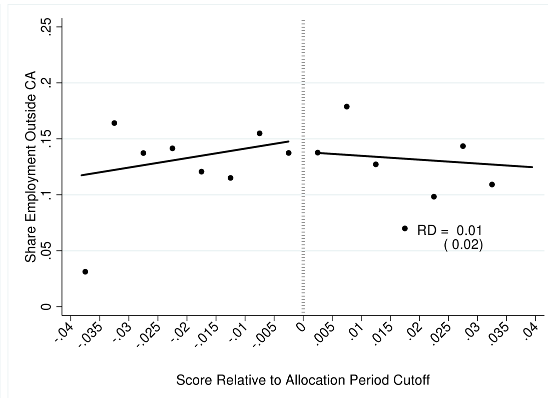
Figure G.6: Pooled RD Results for Employment outside California (Fixed Bandwidth)

Panel A. Two Years After Credit Allocation Year ($\tau = +2$)

(a) Employment

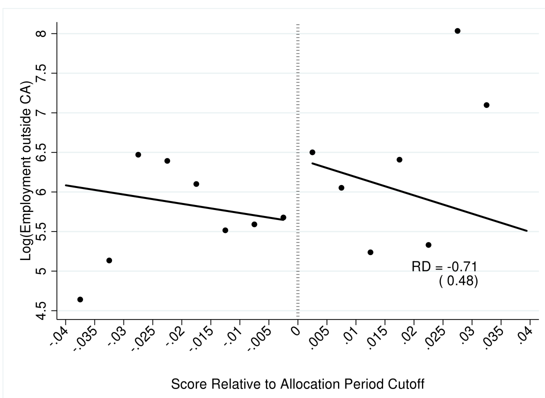


(b) Share Employment

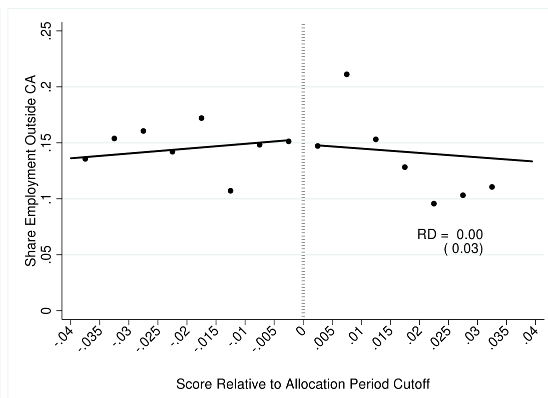


Panel B. Two Years Prior to Credit Allocation Year (Placebo, $\tau = -2$)

(c) Employment



(d) Share Employment



NOTES—Figures show regression slopes and intercepts overlaid on top of equally spaced pre-binned outcome data with a bin size of 0.005. Plots are shown over the optimal bandwidth selected using the IMSE-procedure, which produces asymmetric optimal bandwidth boundaries for each sample. Plots residualize outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to mean-collapsing by bin. Here, we show RD plots for employment, but also report estimates for remaining outcomes in the corresponding table.

Table G.1: First-Stage Results

	Fixed Bandwidth					Varying Bandwidth
Dep. Variable	(1)	(2)	(3)	(4)	(5)	(6)
Pr(Applicant Receives Award)	0.19***	0.20***	0.17***	0.18***	0.16***	0.14***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Applicant No. of Awards	0.14***	0.14***	0.11**	0.12***	0.10**	0.10**
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
Industry FEs		X		X	X	X
Allocation Period FEs			X	X	X	X
Baseline Controls					X	
Control Mean (Pr(Award))	0.16	0.15	0.19	0.19	0.19	0.19
Control Mean (No. of Awards)	0.33	0.31	0.35	0.33	0.34	0.33
N	1,800	1,800	1,800	1,800	1,800	1,800

NOTES—Each row and column corresponds to a separate regression discontinuity estimate with the dependent variables listed in the first two rows. The control means vary across models as they are estimated intercepts ($\hat{\alpha}_\tau$) from equation (2) using the fixed-bandwidth approach. Fixed-bandwidth estimates correspond to our specification that residualizes outcomes by industry fixed effects, allocation period fixed effects, and baseline controls (added back to their overall mean) prior to estimation with a fixed optimal bandwidth. Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. In undisclosed results, the varying-bandwidth first stage is robust to also including baseline controls. N represents the rounded observation count prior to IMSE-optimal bandwidth adjustments and log transformations, which deflates observation counts depending on covariates. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$.

Table G.2: Continuity in Applicant Covariates, Sensitivity to Controls and Bandwidth Method

	Fixed Bandwidth								Varying Bandwidth		
	Model 1		Model 2		Model 3		Model 4		Model 5		N
Dep. Variable	Disc.	SE	Disc.	SE	Disc.	SE	Disc.	SE	Disc.	SE	
Single-Unit	-0.15***	0.04	-0.09**	0.04	-0.12***	0.04	-0.07*	0.04	-0.04	0.04	1,600
C-Corporation	0.18***	0.05	0.13***	0.04	0.16***	0.05	0.13***	0.04	0.11***	0.04	1,600
S-Corporation	-0.09*	0.05	-0.04	0.04	-0.08*	0.05	-0.04	0.04	-0.05	0.04	1,600
Publicly Traded	0.09***	0.03	0.06***	0.02	0.08***	0.03	0.06**	0.02	0.05**	0.02	1,600
AA Relocate	-0.02	0.02	0.00	0.02	-0.03	0.02	-0.01	0.02	-0.02	0.02	1,600
AA Terminate or Leave	-0.01	0.05	0.01	0.04	0.01	0.05	0.04	0.04	0.06	0.04	1,600
AA Occur other State	-0.03	0.03	-0.01	0.03	-0.03	0.03	-0.02	0.03	-0.03	0.03	1,600
Log Baseline Employees (App.)	0.44**	0.18	0.19	0.15	0.32*	0.17	0.12	0.15	-0.01	0.15	1,600
Log Projected Compensation Next 5 Years (App.)	0.71***	0.14	0.51***	0.13	0.6***	0.14	0.43***	0.13	0.15	0.14	1,600
Log Projected Investment Next 5 Years (App.)	0.96***	0.20	0.68***	0.18	0.81***	0.2	0.57***	0.18	0.40**	0.20	1,600
Industry FEs			X	X			X	X	X	X	
Allocation Period FEs					X	X	X	X	X	X	
Control Mean (Single-Unit)	0.75		0.73		0.76		0.74		0.76		
Control Mean (C-Corporation)	0.28		0.30		0.28		0.29		0.28		
Control Mean (S-Corporation)	0.53		0.51		0.53		0.52		0.53		
Control Mean (Publicly Traded)	0.06		0.07		0.05		0.07		0.05		
Control Mean (AA Relocate)	0.05		0.05		0.04		0.03		0.04		
Control Mean (AA Terminate or Leave)	0.33		0.32		0.34		0.32		0.34		
Control Mean (AA Occur other State)	0.09		0.09		0.08		0.07		0.08		
Control Mean (Log Baseline Employees)	4.03		4.12		3.93		4.04		3.93		
Control Mean (Log Projected Compensation)	15.31		15.39		15.32		15.40		15.32		
Control Mean (Log Projected Investment)	14.36		14.47		14.24		14.39		14.24		

NOTES—Models 1 through 4 show estimates as we add controls, prior to using the variables listed in the first four rows as baseline controls in our main analyses. Each row corresponds to a separate regression discontinuity (denoted "Disc.") for the listed dependent variable. The "control mean" varies across models as it is an estimated intercept (α_τ) from equation (2). Model 5 shows estimates using a covariate-adjusted bandwidth, with control means borrowed from the fixed-bandwidth model. N represents the rounded observation count prior to IMSE-optimal bandwidth adjustments and log transformations, which deflates observation counts depending on covariates. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** p≤0.01, ** p≤0.05, * p≤0.1.

Table G.3: Continuity in Baseline Application Characteristics and Outcomes, Alternative Estimator with Varying Bandwidths

Dep. Variable	Discontinuity ($\hat{\beta}$)	Standard Error	Control Mean ($\hat{\alpha}$)	N
Panel A. Application Characteristics				
Tax Credits Requested	-59,530	172,300	795,500	1,600
AA Relocate	-0.03	0.03	0.03	1,600
AA Terminate or Leave	0.06	0.04	0.33	1,600
AA Occur Other State	-0.02	0.02	0.07	1,600
Log Baseline Employees	-0.01	0.15	4.16	1,600
Log Projected Compensation Next 5 Years	0.15	0.14	15.48	1,600
Log Projected Investment Next 5 Years	0.40**	0.20	14.51	1,600
Panel B. Pre-Treatment Applicant Outcomes ($\tau = -2$)				
Activity in California				
Employment within CA	150	210	455	1,600
Payroll within CA (Ths. $)	6,845	13,920	28,350	1,600
Establishments within CA	0.15	1.75	4.40	1,600
Log Employment within CA	0.06	0.16	3.99	1,600
Log Payroll within CA	0.05	0.16	8.03	1,600
Log Establishments within CA	0.03	0.08	0.34	1,600
Activity in High-Poverty/High-Unemployment California ZIPs				
Employment in High Pov-Unemp CA ZIPs	82	87	109	1,600
Payroll in High Pov-Unemp CA ZIPs (Ths. $)	4,150	4,396	5,698	1,600
Establishments in High Pov-Unemp CA ZIPs	-0.09	0.71	1.58	1,600
Log Emp. in High Pov-Unemp CA ZIPs	0.24	0.30	4.2	1,600
Log Payroll in High Pov-Unemp CA ZIPs	0.16	0.28	8.23	1,600
Log Establishments in High Pov-Unemp CA ZIPs	0.25	0.17	0.49	1,600
Activity outside California				
Employment outside CA	1,092	1,228	1,973	1,600
Payroll outside CA (Ths. $)	76,570	72,710	115,000	1,600
Establishments outside CA	-0.21	12.77	20.32	1,600
Log Employment outside CA	-0.22	0.50	6.36	1,600
Log Payroll outside CA	-0.16	0.54	10.50	1,600
Log Establishments outside CA	-0.58*	0.32	2.20	1,600
Share Employment outside CA	0.05	0.03	0.15	1,600
Share Payroll outside CA	0.05	0.03	0.15	1,600
Share Establishments outside CA	0.03	0.03	0.16	1,600

NOTES—Each row corresponds to a separate regression discontinuity with the listed variable as the dependent variable. The control mean is the estimated intercept ($\hat{\alpha}_\tau$) from equation (2). Varying-bandwidth estimates residualize outcomes by industry and allocation period fixed effects and use covariate-adjusted optimal bandwidths following the Calonico et al. (2019) procedure. N represents the rounded observation count prior to IMSE-optimal bandwidth adjustments and log transformations. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** p≤0.01, ** p≤0.05, * p≤0.1.

Table G.4: Complete Placebo RD Results for Employment, Payroll, and Establishments, $\tau = -2$ (Fixed Bandwidth)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Variable			Levels					Logs		
Employment within CA	623**	368*	494*	246	64	0.49***	0.27*	0.37**	0.19	0.02
	(264)	(207)	(263)	(206)	(195)	(0.17)	(0.15)	(0.17)	(0.15)	(0.13)
Employment in High Pov-Unemp ZIP	223**	161*	180*	118	75	0.63**	0.42	0.39	0.25	0.17
	(99)	(87)	(98)	(87)	(82)	(0.31)	(0.26)	(0.30)	(0.26)	(0.24)
Payroll within CA	40,900**	25,060*	32,340**	17,730	4,999	0.54***	0.27*	0.43**	0.20	-0.01
	(16,170)	(13,840)	(16,080)	(13,770)	(36,480)	(0.17)	(0.15)	(0.17)	(0.15)	(0.13)
Payroll in High Pov-Unemp ZIP	11,230**	8,059*	8,868*	5,824	3,777	0.56*	0.30	0.34	0.16	0.04
	(4,864)	(4,310)	(4,842)	(4,288)	(4,057)	(0.31)	(0.27)	(0.31)	(0.27)	(0.25)
No. Establishments within CA	3.73*	1.55	3.01	1.06	-0.18	0.22**	0.06	0.20*	0.06	0.00
	(1.98)	(1.75)	(1.97)	(1.74)	(1.67)	(0.11)	(0.08)	(0.11)	(0.08)	(0.07)
No. Establishments in High Pov-Unemp ZIP	1.38*	0.59	0.98	0.28	-0.04	0.42**	0.22	0.33	0.15	0.12
	(0.83)	(0.72)	(0.82)	(0.71)	(0.21)	(0.67)	(0.15)	(0.21)	(0.15)	(0.14)
Employment outside CA	4,457**	2,584**	3,532**	1,717	611	0.31	0.13	0.08	-0.17	-0.71
	(1,791)	(1,269)	(1,790)	(1,266)	(1,216)	(0.59)	(0.49)	(0.57)	(0.48)	(0.48)
Share Employment outside CA	0.09***	0.05*	0.07**	0.04	0.00					
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)					
Payroll outside CA	251,500**	147,300*	200,300**	101,600	36,480	0.3	0.1	0.09	-0.18	-0.73
	(99360)	(77130)	(99160)	(76920)	(72710)	(0.61)	(0.53)	(0.59)	(0.51)	(0.51)
Share Payroll outside CA	0.09***	0.05	0.07**	0.03	0.00					
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)					
No. Establishments outside CA	26.98*	11.5	20.59	6.47	-3.98	-0.19	-0.32	-0.34	-0.43	-0.69**
	(15.49)	(12.81)	(15.48)	(12.79)	(12.26)	(0.43)	(0.35)	(0.42)	(0.34)	(0.34)
Share No. Establishments outside CA	0.09***	0.04	0.07**	0.03	-0.01					
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)					
Industry FEs		X		X	X		X		X	X
Allocation Period FEs			X	X	X			X	X	X
Baseline Controls					X					X
Control Mean (Emp within CA)	260	403	264	382	455	3.89	3.99	3.80	3.90	3.99
Control Mean (Emp High Pov-Unemp)	77	100	67	93	109	4.23	4.27	4.14	4.21	4.20
Control Mean (Payroll within CA)	16,110	24,350	16,530	23,410	28,350	7.89	7.99	7.81	7.92	8.03
Control Mean (Payroll High Pov-Unemp)	3,849	5,047	3,631	4,903	5,698	8.21	8.28	8.13	8.23	8.23
Control Mean (Estabs within CA)	3.4	4.13	3.17	3.91	4.4	0.3	0.34	0.26	0.31	0.34
Control Mean (Estabs High Pov-Unemp)	1.38	1.56	1.19	1.45	1.58	0.52	0.5	0.48	0.5	0.49
Control Mean (Emp outside CA)	677	1,704	771	1,550	1,973	6.23	6.27	6.08	6.16	6.36
Control Mean (Share Emp outside CA)	0.12	0.13	0.12	0.13	0.15					
Control Mean (Payroll outside CA)	45,330	100,900	48,760	91,090	115,000	10.38	10.41	10.23	10.29	10.50
Control Mean (Share Payroll outside CA)	0.13	0.14	0.12	0.14	0.15					
Control Mean (Estabs outside CA)	11.81	17.99	10.93	16.28	20.32	2.30	2.25	2.15	2.14	2.20
Control Mean (Share Estabs outside CA)	0.13	0.15	0.13	0.15	0.16					
N	1,600	1,600	1,600	1,600	1,600	1,600	1,600	1,600	1,600	1,600

NOTES—Each row and column corresponds to a separate fixed-bandwidth regression estimate with the listed row characteristic as the dependent variable. The control means vary across models as they are estimated intercepts ($\hat{\alpha}_\tau$) from equation (2). N represents the rounded observation count prior to IMSE-optimal bandwidth adjustments and log transformations, which deflates observation counts by a range depending on covariates. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** p≤0.01, ** p≤0.05, * p≤0.1.

Table G.5: Complete RD Results for Employment, Payroll, and Establishments, $\tau = +2$ (Fixed Bandwidth)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Variable	Levels					Logs				
Employment within CA	942***	832***	733**	598**	220	0.76***	0.6***	0.64***	0.51***	0.26**
	(305)	(259)	(304)	(257)	(241)	(0.16)	(0.14)	(0.16)	(0.14)	(0.13)
Employment in High Pov-Unemp ZIP	323***	302***	247**	222**	135	0.94***	0.75***	0.71**	0.59**	0.41*
	(103)	(92)	(102)	(91)	(85)	(0.28)	(0.25)	(0.28)	(0.24)	(0.22)
Payroll within CA	52,520***	42,150***	41,890**	31,310**	8,926	0.85***	0.64***	0.73***	0.56***	0.25*
	(16,910)	(14,800)	(16,820)	(14,700)	(13720)	(0.17)	(0.15)	(0.17)	(0.15)	(0.13)
Payroll in High Pov-Unemp ZIP	13,370**	11,310**	10,150*	7,891	3,592	0.92***	0.66**	0.75**	0.58**	0.34
	(5,449)	(4,965)	(5,426)	(4,938)	(4,663)	(0.3)	(0.26)	(0.29)	(0.26)	(0.24)
No. Establishments within CA	7.61***	6.47***	6***	4.84***	2.49	0.4***	0.27***	0.33***	0.22***	0.11
	(2.14)	(1.87)	(2.14)	(1.86)	(1.76)	(0.1)	(0.08)	(0.1)	(0.08)	(0.07)
No. Establishments in High Pov-Unemp ZIP	3.11***	2.75***	2.24**	1.9**	1.06	0.82***	0.68***	0.63***	0.52***	0.38***
	(0.96)	(0.84)	(0.95)	(0.83)	(0.76)	(0.21)	(0.16)	(0.21)	(0.16)	(0.14)
Employment outside CA	5,047***	4,301***	4,022**	3,078**	1,103	0.97	0.65	0.58	0.21	-0.33
	(1,696)	(1,331)	(1,695)	(1,327)	(1,264)	(0.63)	(0.54)	(0.59)	(0.52)	(0.51)
Share Employment outside CA	0.1***	0.07**	0.08***	0.05*	0.01					
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)					
Payroll outside CA	304,100***	242,000***	242,300**	169,800*	45,470	0.91	0.61	0.65	0.3	-0.33
	(109,400)	(90,170)	(109,300)	(89,830)	(84,700)	(0.65)	(0.57)	(0.62)	(0.55)	(0.54)
Share Payroll outside CA	0.1***	0.07**	0.09***	0.05*	0.01					
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)					
No. Establishments outside CA	53.66***	48.21***	41.13**	34.57**	15.51	0.49	0.27	0.22	-0.06	-0.21
	(16.59)	(13.84)	(16.55)	(13.76)	(13.03)	(0.48)	(0.42)	(0.46)	(0.4)	(0.39)
Share No. Establishments outside CA	0.09***	0.05*	0.07**	0.04	-0.01					
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)					
Industry FEs		X		X	X		X		X	X
Allocation Period FEs			X	X	X			X	X	X
Baseline Controls					X					X
Control Mean (Emp. within CA)	321	447	303	377	555	4.18	4.22	4.12	4.17	4.31
Control Mean (Emp. High Pov-Unemp)	73	89	58	67	102	4.36	4.35	4.27	4.28	4.31
Control Mean (Payroll within CA)	19,300	26,300	20,140	24,890	35,340	8.14	8.2	8.11	8.18	8.34
Control Mean (Payroll High Pov-Unemp)	4,788	5,872	4,223	5,150	6,814	8.26	8.28	8.2	8.24	8.29
Control Mean (Estabs within CA)	2.66	3.34	2.39	2.8	3.91	0.28	0.31	0.24	0.28	0.34
Control Mean (Estabs in High Pov-Unemp)	1.02	1.2	0.87	0.98	1.32	0.4	0.36	0.36	0.35	0.39
Control Mean (Emp. outside CA)	597	1348	644	1062	1949	6.11	6.16	5.88	6.09	6.33
Control Mean (Share Emp. outside CA)	0.11	0.12	0.1	0.11	0.14					
Control Mean (Payroll outside CA)	43,430	95,650	49,450	81,510	136,700	10.29	10.34	10.1	10.26	10.57
Control Mean (Share Payroll outside CA)	0.12	0.13	0.12	0.13	0.16					
Control Mean (Estabs outside CA)	6.11	11.94	6.6	9.26	17.8	2.17	2.26	1.96	2.12	2.19
Control Mean (Share Estabs outside CA)	0.14	0.15	0.13	0.14	0.17					
N	1,700	1,700	1,700	1,700	1,700	1,700	1,700	1,700	1,700	1,700

NOTES—Each row and column corresponds to a separate fixed-bandwidth regression estimate with the listed row characteristic as the dependent variable. The control means vary across models as they are estimated intercepts ($\hat{\alpha}_\tau$) from equation (2). N represents the rounded observation count prior to IMSE-optimal bandwidth adjustments and log transformations, which deflates observation counts by a range depending on covariates. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** p≤0.01, ** p≤0.05, * p≤0.1.

Table G.6: Pooled RD Estimates for High Poverty/Unemployment California ZIP Codes, $\tau = +2$

Dep. Variable	(1)	(2)	(3)
Log(Employment in High-Pov-Unemp Zip)	0.41*	0.54*	-0.01
	(0.22)	(0.30)	(0.07)
Log(Payroll in High-Pov-Unemp Zip)	0.34	0.48*	0.10
	(0.24)	(0.29)	(0.09)
Log(No. Establishments in High-Pov-Unemp Zip)	0.38***	0.50	0.01
	(0.14)	(0.16)	(0.03)
Pooled, Fixed Bandwidth	X		
Pooled, Varying Bandwidth		X	
Dynamic			X

NOTES—Each row and column corresponds to a separate regression discontinuity estimate with the dependent variables listed in the first three rows. The control means vary across models as they are estimated intercepts ($\hat{\alpha}_\tau$) from equation (2). In columns (1) and (2), there are approximately 1,700 (rounded) applicants prior to IMSE-optimal bandwidth adjustments and log transformations, which deflates observation counts by a range depending on covariates. The observation count is slightly larger here than in the placebo period due to new entrants. In column (3), there are repeated observations on each applicant. Each regression uses IMSE-optimal bandwidths chosen separately on each side of the cutoff, and triangular kernel weights. Standard errors are heteroskedasticity-robust. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$.

H Marginal Value of Public Funds

In this appendix, we conduct a back-of-the-envelope calculation of the marginal value of public funds (MVPF) under the CCTC in order to compare its effectiveness to alternative local hiring policies. Hendren (2016) and Hendren and Sprung-Keyser (2020) provide a framework in which the welfare effects of policies can be characterized by inframarginal agents' induced behavioral responses (used to calculate their private willingness to pay (WTP) for the policy), and government expenditures on the policy net of any fiscal externalities that might arise from the induced behavioral response. The MVPF has the advantage that private WTP for benefits and net fiscal costs combine to form a money metric (benefits-per-dollar) for the program's social return, which can then be compared to other programs without making ad hoc assumptions about deadweight loss or excess burden.

Numerator. To calculate our baseline MVPF, we must first estimate workers' WTP for the jobs created by the program. If newly demanded labor by CCTC awardees only constitutes a small potential benefit to the average California worker (such as marginal upward wage pressure for employed workers), the envelope theorem implies no private WTP for these marginal gains; the expected earnings returns from additional search effort for *currently employed* workers is already set equal to the marginal cost of search. However, as noted in Finkelstein and Hendren (2020), many policies induce large benefit changes in which workers consider large changes in their behavior – here, a sizable demand shock may induce search effort that might move a worker from unemployed to employed. Our RD estimates isolate the amount of payroll per worker induced by the CCTC program *that would otherwise not have been demanded*. If these jobs are filled by previously unemployed workers, then their private WTP for the jobs can be assumed to equal the payroll flow they would receive less their reservation wage.[xix] In our MVPF numerator, we thus combine our fixed-bandwidth log payroll and employment estimates into payroll per worker (which equals \$60,908) and subtract an estimate of the reservation wage (assumed to be equal to 52 weeks of maximum California unemployment insurance payments).

$$\text{Worker WTP for CCTC} = \frac{\overbrace{(e^{0.25}-1)\times(\$35,340,000)}^{\text{Firm Payroll Effect}}}{\underbrace{(e^{0.26}-1)\times 555}_{\text{Firm Employment Effect}}} - \underbrace{\$450 \times 52 \text{ weeks}}_{\text{Reservation Wage}} = \$37,508$$

We make two caveats. First, the numerator and denominator inputs of the first term in the expression above would both be larger using our varying-bandwidth estimator's results, and would both be smaller using our dynamic RD estimator's results. Second, we assume here that new hires are being drawn from the pool of unemployed; if instead new jobs are filled by workers with higher reservation wages (which could happen if, for example, CCTC-induced job creation induces population gains in California, as would be suggested by Beaudry et al. (2014) and Amior and Manning (2018)), the WTP would be smaller.

Denominator. Next, we need to consider the net fiscal costs of the CCTC program. Using administrative CCTC data, we calculate the cost-per-job as the net full-time increase in employees per dollar of awards actually received during our sample period, which yields a cost of \$7,721.

[xix] Agrawal et al. (2021) note that such calculations are made even more complicated if non-local workers migrate in to bid away such gains. If workers instead transition to the subsidized job from another employer, this would make vacant a new job with a similar income stream.

Note that this cost-per-job is the *programmatic* cost associated with each subsidized job – to the extent that jobs would have been created absent the incentive, this would be reflected in the numerator RD estimate, which captures *net new jobs*.[xx] We assume a 10% mark-up on this cost for administrative overhead, which results in a programmatic cost per job of $8,493. The fiscal externality of interest is a positive externality (entering the denominator negatively) from individual income tax that arises from payroll increases associated with the program. We use an average effective tax rate of 3.06%, which is California's effective state income tax rate associated with our payroll-per-worker estimate of $60,908, and apply this to the full amount of the payroll per worker since unemployment insurance benefits are not taxed at the state level in California.[xxi]

$$\text{Net Fiscal Costs} = \underbrace{\$8,493}_{\text{Cost per Job}} - \underbrace{0.0306 \times \$60,908}_{\text{Fiscal Externality}} = \$6,629 \tag{6}$$

Dividing the worker WTP by the net fiscal costs yields an estimate of the MVPF for the CCTC of 5.66. Taken at face value, this suggests California workers receive about $5.66 in benefits from the CCTC for every dollar the state spends on the program. This may overstate the actual MVPF for reasons discussed above. However, even under alternative assumptions about workers' outside options, the MVPF of the CCTC is still likely to compare favorably to other programs with similar aims. The benefit-cost ratios presented are not directly comparable, but as Slattery (2022) discusses, the estimates in Bartik (2019) suggest a substantially lower MVPF for economic development programs more generally. However, the CCTC's MVPF may be in the ballpark of Gaggl and Wright's (2017) MVPF estimate of 4.29 for investment tax credits for information and computer technology.[xxii] To the extent that CCTC awardees are also more likely to pursue other subsidies, or that overhead costs are larger than we assume for these calculations, it would reduce the MVPF under the CCTC.

Appendix H References

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[xx] We deliberately chose to estimate payroll-per-worker in the numerator of the MVPF, but used a programmatic cost-per-job in the denominator. The reason for this is that our employment and payroll effects are right-skewed, as discussed in the paper. In the numerator, the *ratio* of payroll to employment allows for a robust measure of private willingness to pay for CCTC benefits because it is a ratio of two right-skewed variables. However, to estimate cost-per-job would require scaling an average employment RD estimate across all awardees, no matter how skewed employment is. We thus chose to use programmatic cost-per-jobs that does not rely on extrapolating a local RD estimate.

[xxi] We use NBER's TAXSIM to calculate the average effective California state income tax rate over the 2014-2019 period for a single tax filer earning $50,000. This average effective rate is 3.06%.

[xxii] See https://www.policyimpacts.org/policy-impacts-library for estimated MVPFs for other policies.

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