



# Business as usual: Politicians with business experience, government finances, and policy outcomes



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## ABSTRACT

Are government finances and policy outcomes different under politicians with business experience? We study California city councils and implement a regression discontinuity strategy to provide causal evidence on this issue. Ultimately, we find no evidence that the election of a candidate with business experience impacts city expenditures, revenues, unemployment rates, and other outcomes. Future vote shares for candidates with business experience are also unaffected, which suggests that these politicians are not having an impact that is observed to voters but unobserved in our data.

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## 1. Introduction

Do the personal characteristics of political candidates have any bearing on the policies they ultimately enact, or do politicians (regardless of their background and identity) merely enact the preferences of the electorate? Existing evidence on this question is mixed. Some characteristics clearly matter. For instance, [Besley et al. \(2011\)](#) find that educated political leaders generate higher economic growth. [Alesina et al. \(2015\)](#) find that spending is higher in Italian local governments when a younger mayor is elected, which they argue is driven by career concerns. Gender, on the other hand, appears to matter in some settings ([Anzia and Berry, 2011](#); [Clots-Figueras, 2012](#); [Bhalotra and Clots-Figueras, 2014](#)), but not others ([Ferreira and Gyourko, 2014](#)). Similarly, a politician's partisan affiliation impacts outcomes at some levels of government ([Lee et al., 2004](#); [Albouy, 2013](#)), but not others ([Leigh, 2008](#); [Ferreira and Gyourko, 2009](#)). Finally, a related literature finds evidence that personal characteristics affect how politicians vote on issues specifically related to those characteristics.<sup>1</sup>

Despite the richness of this literature, the general impact of a candidate's professional background and experience has received relatively little attention (with some notable exceptions, which we discuss later). This is surprising for two reasons. First, intuitively, just as [Besley et al. \(2011\)](#) show that better educated leaders generate economic growth, a candidate with

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<sup>1</sup> For example: how women vote on women's issues ([Frankovic, 1977](#); [Swers, 1998, 2002](#)), the willingness of politicians with military experience to use military force ([Gelpi and Feaver, 2002](#)), how lawyers vote differently than other representatives on issues like tort reform ([Matter and Stutzer, 2015](#)) and no-fault insurance ([Dyer, 1976](#)), how politicians with business experience are more likely to support pro-business legislation ([Witko and Friedman, 2008](#)), and how politicians with experience in farming are more likely to vote for farm subsidies ([Bellemare and Carnes, 2015](#)).

professional experience relevant to the tasks faced in government may also improve policy outcomes. Second, professional background is a characteristic that candidates and voters find important. One does not need to look far to find candidates at all levels, from city council to the presidency,<sup>2</sup> touting “business experience” as a positive during their campaigns.<sup>3</sup> Professional experience – and business experience in particular – matters to voters as well; in a recent Gallup poll, 81 percent of respondents indicated that the country would be better governed if someone with “business or management experience” were in office.<sup>4</sup> Both McDermott (2005) and Mechtel (2014) document that revelation of a candidate’s occupation impacts voters’ choices; their evidence suggests that voters use occupation as a signal of competency in low-information elections.

In this paper, we contribute to the literature by asking whether policy outcomes differ under politicians with business experience. To answer this question, we use a regression discontinuity approach in the context of California city council elections. California city councils provide a useful setting to explore this question for several reasons. First, the typical council includes only five members.<sup>5</sup> Thus, unlike elections to state or federal legislatures, this is an environment where the election of a single member can (and has been shown to) have a large and immediate impact on policy outcomes.<sup>6</sup> Second, for all cities in California, rich data are available for both local elections and annual spending patterns. Finally, almost all council elections in California are non-partisan; instead of partisan affiliation appearing on a ballot, candidate’s occupation or profession is listed as a “ballot designation.”<sup>7</sup> Ballot designation appears in our data, which allows us to identify the city council elections involving candidates with or without a business background. In our empirical approach, we focus our attention on races where one candidate has business experience and the other does not. We take advantage of the fact that, in sufficiently close elections, selection of the candidate with (or without) business experience is plausibly random, which in turn allows us to assess the causal impact of business experience in government.

Ultimately, we find no evidence that the election of a candidate with business experience generates an observable change in policy – expenditures, revenues, likelihood of maintaining a surplus (revenues exceed expenditures), and unemployment rates are all unchanged. The distribution of spending is also unaffected. City governments do not spend more or less on public goods (roads, parks, etc.) or on themselves (salaries, benefits for government workers) when a candidate with business experience is elected to the council. Moreover, there is no evidence that candidates with business experience receive a higher or lower vote share than candidates without business experience when they are up for reelection. This suggests that there are no benefits (or costs) from electing a businessperson that are observable only to voters. This absence of evidence persists when we assess whether there is heterogeneity in the treatment effect across cities. We test whether the impact of a businessperson on the council is different in larger cities, cities with pre-existing economic/financial difficulties, or city-year observations where the elected businessperson is in the final years of his or her term. In all of these cases, the main finding persists: there is no evidence that the election of a businessperson has a causal impact on a city’s policy outcomes.

While professional experience, and business experience in particular, has received relatively little attention by economists, two related papers from the political science literature are particularly worth noting. Carnes (2012) explores whether there are systematic differences in roll call voting patterns for Congressional representatives from different social classes. In doing so, he assesses the relationship between occupation (including business-related occupations) and roll call voting. He finds that legislators from certain professions (businesspeople, farm owners, and professionals) tend to have more conservative voting records than industrial, farm, and union workers. Witko and Friedman (2008) focus explicitly on the impacts of business experience amongst members of Congress. Like Carnes, they also find that businesspeople have distinct patterns of roll call voting: businesspeople are more likely to support bills supported by the Chamber of Commerce and less likely to support bills supported by union groups. They also provide some evidence that businesspeople are more likely to sponsor business-related bills. Thus, there is some evidence of a possible relationship between business experience and political behavior. Of course, in any such study, it can be difficult to disentangle the impacts of a politician’s characteristics from the characteristics of the electorate who supported that politician. We therefore contribute to this existing literature in two ways. First, by taking advantage of natural experiments provided by close elections, we identify the causal impact

<sup>2</sup> For instance, a Columbia, SC city councilmember has campaigned under the slogan “Businessman for city council” and is quoted as saying: “Business experience is a much-needed commodity in government. Business people often have unique insights, particularly on economic development and on issues facing the business community. And I think business people also bring common sense and innovative problems solving abilities to the table.” (<http://www.wistv.com/story/21585009/councilman-first-to-announce-challenge-to>).

<sup>3</sup> In a Republican debate during the 2016 primary election cycle, candidate and businessman Donald Trump claimed: “What I am ... is a businessman, and that’s the kind of mindset this country needs to bring it back, because we owe \$19 trillion right now, \$19 trillion, and you need this kind of thinking to bring our country back.” (<http://cnnpressroom.blogs.cnn.com/2015/09/16/cnn-reagan-library-debate-later-debate-full-transcript/>).

<sup>4</sup> This poll asked about a variety of candidate characteristics (gender, partisan leaning, religion, ‘outsider status’) and whether respondents thought those characteristics would generate “better governing.” “Business experience” generated the most positive response by a large margin. The next most favorable type of candidate was a female candidate, who was expected to “govern better” by 63 percent of respondents. Much further down the list was “people who have never held elected office,” which was viewed as a characteristic that would lead to better governing by 49 percent of respondents. (<http://www.gallup.com/poll/174002/americans-say-business-background-best-governing.aspx>).

<sup>5</sup> Institutional details that determine council size are discussed in greater detail in Section 3.

<sup>6</sup> For instance, Beach and Jones (2016) also study California city councils and find that the narrow election of a candidate who increases the ethnic diversity of the council leads to a dramatic reduction in spending on public goods and services.

<sup>7</sup> California state law requires candidates to provide not just their occupation, but also information that would allow the State of California to verify that the listed occupation is accurate.

of electing a politician with business experience. Second, unlike these papers, we focus on the general policy impacts of business experience in government, rather than voting patterns of individual politicians.

The paper proceeds as follows: In Section 2, we draw on existing theoretical and empirical literature to generate hypotheses on the possible impact of a businessperson in government. Relevant institutional details for California city councils are discussed in Section 3. In Section 4, we describe the data we use, including our method of identifying candidates with business experience. Section 5 describes the regression discontinuity approach used in the paper. Section 6 reports results; we first document that the regression discontinuity approach is valid in this setting. Then, we assess the causal impact of the election of a businessperson using both simple graphical analysis and the empirical strategy described in Section 4. Section 7 concludes.

## 2. Drawing hypotheses from existing literature

In this section, we draw from literature to suggest two ways in which business experience might impact policy. First, businesspeople may have different preferences over policy (Witko and Friedman, 2008). Of course, as we discuss in this section, whether this translates into different enacted policy depends on which model of electoral competition better describes California city councils. Second, even if businesspeople do not differ in their policy preferences, they may differ in their ability to propose successful and feasible policies, particularly with respect to budgets.

Models of electoral competition speak to the issue of how the distinct policy preferences of businesspeople, insofar as they exist, would translate into policy.<sup>8</sup> The classic Downsian model suggests that business experience should not matter (Downs, 1957). Under that model (and related models), all candidates respond to the preferences of the median voter in an attempt to maximize votes. As a result, the policy a candidate adopts will be independent of their identity (business background, gender, partisan affiliation, etc.), as all competing candidates adopt similar policies – the policy that is most preferred by the median voter.

Other models of electoral competition allow for the possibility that candidates are not strictly vote-maximizers, but instead run with the goal of enacting their personally preferred policies. For instance, in “citizen-candidate” models (Osborne and Slivinski, 1996; Besley and Coate, 1997), running is only worthwhile if the potential benefit of running (the chance to enact personally preferred policies) outweighs the costs. This implies it is unlikely that two candidates with the same policy proposals compete, as the benefit to running in that case is low (given that the election of one candidate still generates the losing candidate’s preferred policy outcome). Thus, under these models, the identity (in this case, professional background) of the candidate may matter.

Perhaps more relevant is the idea that candidates can also vary on a second dimension such as ability or quality (Besley, 2005; Alesina and Tabellini, 2007). Under this view, businesspeople may impact policy not because they have systematically different ideological positions or policy preferences, but instead because they may differ in their ability to propose and guide the implementation of successful policies. Several empirical papers highlight the importance of candidate quality or ability, independent of ideology, in influencing policy outcomes. As previously noted, Besley et al. (2011) find that better educated political leaders (which may serve as a proxy for ability) generate more economic growth. Relatedly, Whalley (2013) finds that borrowing costs are lower for cities in California with appointed (rather than elected) city treasurers. This is important because appointed treasurers are more likely to be selected based on expertise than elected treasurers, as voters may have a more difficult time assessing the quality of alternative candidates.

Still, even if quality is the dimension upon which businesspeople differ from other candidates, the direction in which they differ is ambiguous. Businesspeople may enter the council with a better understanding of budgets, suggesting objective improvements in the city’s budget and policy outcomes. On the other hand, it is equally possible that skills necessary in business (and budgeting) do not translate well into the city government setting; in that case, the election of a businessperson may have no impact (or perhaps even a negative impact) on the city’s budget.

It is also worth noting that, as with other legislative offices, councilmembers are generally responsible for establishing policy, but are not responsible for implementing policy. As we discuss in the next section, in California, city managers typically implement and manage policies. Thus, the potential impacts of business experience are not likely to come through the experience businesspeople have in managing organizations.

In short, there is substantial theoretical ambiguity as to whether we should expect professional experience (and business experience in particular) to impact policy outcomes. As the literature currently stands, there is little empirical work to help resolve this ambiguity. We aim to fill this gap, while also contributing more generally to the literature on the relationship between political candidate selection and resulting policy outcomes.

<sup>8</sup> There is, of course, a large theoretical and empirical literature aimed at identifying which model best explains how politicians behave (Carnes and Lauderdale, 2011; Bellemare and Carnes, 2015; Lee et al., 2004; Jones and Walsh, 2016). We, however, are only concerned with the predictions of those models (i.e., whether a candidate with business experience governs differently than a candidate without business experience) and so for the sake of brevity we only consider two prominent models that produce opposing conclusions.

### 3. City councils in California

California state law provides a number of guidelines for the structure of municipal governments. If a city were to simply conform to these guidelines their council would contain five councilmembers, and councilmembers would be elected “at-large” during a general municipal election. Councilmembers would also serve staggered four-year terms, with elections filling multiple seats every two years.<sup>9</sup> The default form of government is also “council-manager” where the council sets policies that are implemented by a city manager. Finally, the mayor generally serves on the council and does not have any additional powers. In fact, most mayors are simply selected by the city council from amongst its own members.

Although it is possible for cities to deviate from the above guidelines, most cities conform to these guidelines. The 2006 *Municipal Form of Government* survey conducted by the International City/County Management Association (ICMA) illustrates this point. Specifically, the mayor serves on the city council for 98 percent of the cities, 88 percent of city councils contain five councilmembers, councilmembers are elected at-large for 92 percent of cities, and the term length is four years for 99 percent of cities. A city’s institutional structure also tends to be relatively stable over time. In the five years preceding the survey less than seven percent of cities attempted to alter their form of government. When cities do attempt to alter their form of government, it is typically to switch from at-large to district based elections (or vice versa). Most of these attempts, however, were ultimately unsuccessful.

### 4. Data

#### 4.1. Election data and identification of businesspeople

Our first source of data is the *California Election Data Archive* (CEDA), which provides the names, number of votes, and occupation for every candidate in every local government election from 1995 to 2011. While occupational status is reported in the data, there is of course no variable for “business experience.” Broadly, we classify a candidate as having business experience if their occupation falls into one of the following categories: executive (CEO, CFO, COO, executive, president), entrepreneur (business owner, entrepreneur), or investor (investor, venture capitalist).<sup>10</sup> Of the 14,232 candidates that ran for city council between 1995 and 2011, roughly 31 percent (4393) are identified as having business experience. To put this in perspective, 5 percent of candidates report having experience as a lawyer while another 7 percent report having experience as an educator.<sup>11</sup>

#### 4.2. Outcome variables and additional controls

Our outcome variables are mostly drawn from annual city-level financial transaction reports. Reporting of these retrospective data is mandatory and the data are freely available from the California State Controller’s Office.<sup>12</sup> We focus our attention on the fiscal years running from 1999 to 2000 until 2010–2011. These annual reports provide a governmental-wide accounting of expenditures, revenues, assets, and liabilities for every city in California. They also indicate which services (policing, transit, garbage collection, etc.) are provided by the city itself, which services are contracted out to a private firm, and which are contracted out to a different government (e.g., the county government).

We analyze the impact of a businessperson’s victory on the following outcomes: total revenue, total expenditure, total government administration expenditure, and total public goods expenditures. The “total revenue” variable includes cities’ general revenues (e.g., taxes) and revenues from business-type activities (e.g., charges for services).<sup>13</sup> The “total expenditure” variable includes all of a government’s expenditures (including operating expenditures, capital outlay, and debt service). Government administration includes costs of running the government (e.g. salaries, wages, and benefits for city employees). Public goods expenditures include the sum of expenditures on public safety (police and firefighters), transportation (public transit, and roads), community development, health, and culture/leisure. All of these variables are measured at a per capita level (using city-level population drawn from the 2000 and 2010 Census and interpolated for intercensal years). They are also transformed into real dollars, taking 2010 as a base year. In our regression analysis, we take the log of each of these variables.

Several other outcomes we consider are constructed from the financial transaction data. Wang et al. (2007) and Rivenbark et al. (2010) point to the ratio of revenues to expenditures as an important measure of a government’s financial condition, with a ratio of one (implying that revenues exceed expenditures) indicating that a government “lived within its financial means” (Rivenbark et al.). We construct an indicator for whether the city’s revenues exceed expenditures, which we use as

<sup>9</sup> For instance, there may an election in 2004 to fill 3 of the 5 seats (where the resulting councilmembers would face reelection in 2008), followed by an election in 2006 to fill the remaining two seats.

<sup>10</sup> Specifically, we classify a candidate as having business experience if their reported occupation includes any of the following strings “business”, “owner”, “entrep” (which is truncated to account for common misspellings of entrepreneur), “executive”, “president”, “investor”, or “capitalist”.

<sup>11</sup> Lawyers were identified based on the occupation strings “lawyer”, “attorney”, “litigator”, or “judge”. Educators were identified from the strings “educator”, “teacher”, “professor”, or “instructor”.

<sup>12</sup> The data can be retrieved from: <https://bythenumbers.sco.ca.gov>.

<sup>13</sup> The revenue variable excludes pension trust fund financial transactions and revenues associated with the internal service fund.

**Table 1**

Regression discontinuity estimates of the impact of businessperson victory on city financial and policy outcomes.

| OUTCOME VARIABLES                                                                                         | (1)<br>Mean of variable | (2)<br>RD: Linear | (3)<br>RD: Quadratic | (4)<br>RD: Cubic  | Obs.  |
|-----------------------------------------------------------------------------------------------------------|-------------------------|-------------------|----------------------|-------------------|-------|
| Per- capita expenditures<br>(Logged in regression)                                                        | 1,684.945<br>(23.749)   | −0.029<br>(0.047) | −0.021<br>(0.052)    | −0.014<br>(0.057) | 3,453 |
| Per-capita pub. good exp.<br>(Logged in regression)                                                       | 1,241.439<br>(15.006)   | −0.046<br>(0.042) | −0.024<br>(0.047)    | −0.012<br>(0.051) | 3,453 |
| Per-capita gov. admin. exp.<br>(Logged in regression)                                                     | 196.114<br>(4.150)      | 0.068<br>(0.053)  | 0.076<br>(0.061)     | 0.057<br>(0.071)  | 3,453 |
| Per-capita revenue<br>(Logged in regression)                                                              | 1,732.813<br>(24.697)   | −0.026<br>(0.046) | −0.008<br>(0.051)    | −0.006<br>(0.056) | 3,453 |
| Rev. > Exp.? (=1 if yes)                                                                                  | 0.544<br>(0.008)        | −0.022<br>(0.024) | 0.009<br>(0.029)     | 0.004<br>(0.034)  | 3,453 |
| Debt ratio<br>(Total liab./Total assets)                                                                  | 0.248<br>(0.003)        | −0.001<br>(0.012) | −0.010<br>(0.015)    | −0.002<br>(0.016) | 3,450 |
| Quick ratio<br>(Cash+Inv./Current liab.)                                                                  | 3.898<br>(0.206)        | −0.667<br>(0.574) | −0.735<br>(0.676)    | −0.808<br>(0.741) | 3,123 |
| Unemployment rate                                                                                         | 0.073<br>(0.001)        | −0.002<br>(0.002) | −0.002<br>(0.002)    | −0.002<br>(0.003) | 2,262 |
| Method of provision of city services: City, contract with private firm, or contract with other government |                         |                   |                      |                   |       |
| Fraction of services provided<br>by city                                                                  | 0.481<br>(0.003)        | −0.014<br>(0.017) | −0.028<br>(0.019)    | −0.014<br>(0.021) | 3,453 |
| Fraction of services provided<br>through private contract                                                 | 0.191<br>(0.002)        | 0.010<br>(0.009)  | 0.014<br>(0.011)     | 0.014<br>(0.013)  | 3,453 |
| Fraction of services provided<br>by other governments                                                     | 0.329<br>(0.003)        | 0.004<br>(0.016)  | 0.014<br>(0.019)     | 0.000<br>(0.022)  | 3,453 |
| Degree of polynomial                                                                                      |                         | 1st               | 2nd                  | 3rd               |       |
| Additional controls                                                                                       |                         | Yes               | Yes                  | Yes               |       |

Notes: Each row reports an outcome variable of interest. Even when we take the log of the variable in the regression analysis (as indicated), Column 1 reports the mean of the actual (not log-transformed) value of the variable. Columns 2–4 report the regression results. Each column differs only in the degree of polynomial through which “Businessperson margin of victory” enters. Thus, in Columns 2–4 each cell reports a coefficient from a different specification. Coefficients on “margin of victory” terms and additional controls are not reported for space. Additional controls as described in main text. All monetary outcomes are real (2010) dollars.

Robust standard (clustered at city level) in parentheses.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

an outcome (referred to as “Rev. > Exp.” in the tables that follow). We also construct the city’s total debt-to-asset ratio as total liabilities divided by total assets. Finally, we construct a commonly used measure of liquidity: the “quick ratio” (Rivenbark et al., 2010), defined as a city’s cash and investments (that is, readily accessible assets) divided by current liabilities. The quick ratio measures a government’s ability to meet short-term obligations, with a higher ratio indicating a higher ability to do so.

Lastly, we identify the fraction of services provided by the city, private firms, or other governments. Here, we simply divide the number of services that a city reports being provided in a particular way (city, private, or other government) by the total number of services provided. Summary statistics for all outcome variables appear in the first column of Table 1.

We should note that the standards of local governmental financial reporting changed substantially in the early 2000’s as a result of Governmental Accounting Standards Board (GASB) Rule No. 34, which was issued in 1999. Many city governments did not fully implement changes required by the rule until fiscal year 2004.<sup>14</sup> In our data, many new variables (and therefore aggregations of variables) were added to the financial transactions reports in the early 2000’s, but are generally unchanged thereafter. Importantly, the set of available variables is unchanged throughout the entire time period we use. Moreover, to ensure that changes to accounting do not drive our results, in additional analyses we restrict our sample to FY2004 and beyond (Appendix Table A1). We ultimately find that results from those additional analyses are nearly identical to our main results.

One additional outcome variable, unemployment rate, is not drawn from the city financial reports or CEDA data. This variable is drawn from the Bureau of Labor Statistics’ Local Area Unemployment Statistics (LAUS). LAUS measures unemployment rates at the city-year level, but only for cities with more than 25,000 people. Thus, in analyses incorporating unemployment (either as an outcome or lagged unemployment as a right-hand side variable) we lose roughly one third of our sample.

Finally, we draw on both the Census and the 2007–10 American Community Survey samples to obtain additional city-level controls. These include: total population, median income, income per capita, share of residents in each ethnic/racial category, and the share of residents in each of the following education categories: college degree, some college (but no

<sup>14</sup> See Johnson et al. (2012) for an overview of the changes brought about by GASB Rule No. 34 and the impacts of the rule. We thank an anonymous referee for bringing this to our attention.

degree), high school degree, some high school (but no degree), and no high school. This set of controls is similar to those used by Ferreira and Gyourko (2014) in their regression discontinuity approach to assessing whether a candidate's gender matters in city politics.

## 5. Empirical approach

Throughout the paper, our main empirical strategy is a parametric regression discontinuity design. We focus on city council elections where one candidate is a businessperson and the other is not. Because many elections will have more than one winner, we focus on elections where exactly one of the marginal candidates (those that were either narrowly elected or narrowly lost the election) is a businessperson. In sufficiently close elections, the winner (businessperson or not) is randomly assigned to serve on the council, which allows us to identify the causal effect of business experience on policy outcomes. Throughout our sample period (fiscal years 2000 through 2010), 88 percent of California cities experience an election between two candidates where one candidate has business experience and the other does not.

Drawing from our annual city financial transactions data, we estimate variations of the following equation:

$$y_{ct} = \alpha + \beta_1 \mathbf{1}[\text{Businessperson wins}_{ct}] + \beta_2 F(\text{Businessperson margin}_{ct}) + \beta_3 \mathbf{1}[\text{Businessperson wins}_{ct}] \times F(\text{Businessperson margin}_{ct}) + \beta \mathbf{X}_{ct} + \varepsilon_{ct}$$

where the subscript “c” indicates the city and subscript “t” indicates the year. The variable “Businessperson wins” is an indicator that is equal to one in cities and years where a businessperson won an election against a non-businessperson in the most recent election and is serving on the council. In city-year observations where the business candidate lost, this variable is equal to zero. The variable “Businessperson margin” represents the difference between the businessperson's vote share and the competitor's vote share and is only positive if the businessperson wins. “Businessperson wins”, “Businessperson margin”, and the interaction of the two mirror the relatively standard parametric regression discontinuity approach (e.g., Ferreira and Gyourko, 2009, 2014). Also note that, as is relatively standard in this approach, vote margin enters through  $F(\cdot)$ , which is a polynomial function. In our analyses,  $F(\cdot)$  will either be a first, second, or third-degree polynomial.

Because “Businessperson margin” is the difference between the vote share received by the candidate with business experience and the vote share received by the competitor,  $\beta_1$  can be interpreted as the impact of a businessperson's victory when the margin was zero; in practice, we interpret this as the impact of a victory in a very close election. Thus, the “Businessperson wins” coefficient is of primary interest in this specification, as it can be interpreted as the causal impact of electing a candidate with business experience to the council.

Our analysis also includes a vector of additional controls,  $\mathbf{X}_{ct}$ . This vector includes year fixed effects and the following city-level demographic controls: total population, median income, income per capita, the share of residents in each ethnic/racial category, and the share of residents in each education category (college degree, some college (but no degree), high school degree, some high school (but no degree), and no high school). Results are robust to the exclusion of these controls. Results are also robust to the inclusion of city fixed effects, which we report in the appendix.<sup>15</sup> The outcome variable  $y_{ct}$  captures the outcomes discussed in the data section: revenue, expenditure, debt ratio, etc.

One potential complication for our analysis is identifying the timing of treatment. The California fiscal year runs from July to July while elections typically occur in November. Thus, a candidate elected in November of 2005, for instance, would have absolutely no effect on fiscal decisions made in the first half the 2005 fiscal year (which runs from July 2005 to July 2006) and would likely have a very minimal impact on the remaining fiscal year. To deal with this, our outcome variables always begin in the fiscal year following the “businessperson vs not a businessperson” election. Sticking with our above example, we would consider fiscal years 2006, 2007, 2008, and 2009 as the potential outcome years for the four-year term that a candidate elected in November of 2005 has the potential to influence.

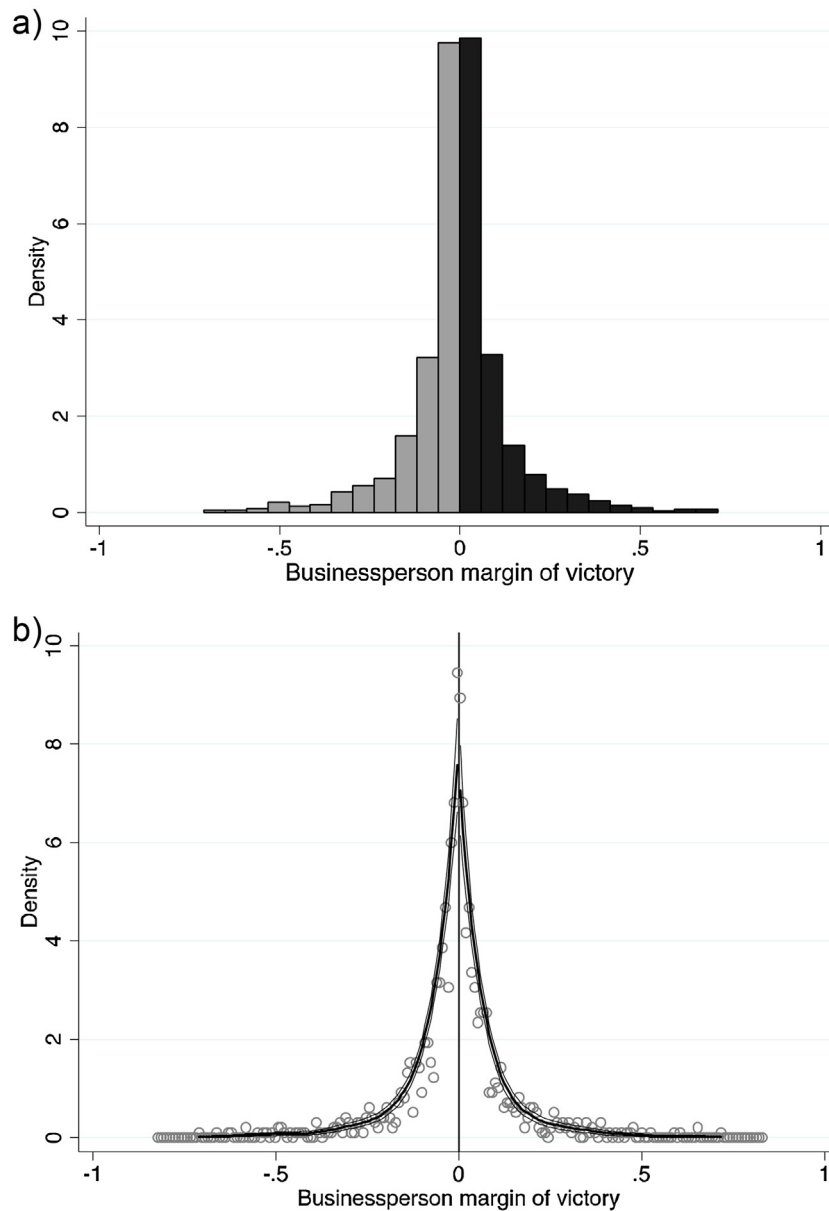
## 6. Results

### 6.1. Assessing validity of regression discontinuity design

Before turning to the main results, we report the results of several tests aimed at probing the validity of our regression discontinuity design. First, we address a common concern in regression discontinuity designs: the “forcing variable” (in this case, businessperson margin of victory) should be balanced around the cutoff (the point where one candidate barely wins the primary).<sup>16</sup> This implies that there should be roughly the same number of observations just to the left of *Businessperson margin*=0 as there are just to the right of *Businessperson margin*=0. If this is not the case, we cannot be sure that the design is randomly assigning a businessperson to the council. Instead, we would be concerned that some unobserved factor determines the winner of a close election (e.g., businesspeople have more resources to sway voters in a close election).

<sup>15</sup> Note that we do not include city fixed effects in our main specifications, as a large fraction of cities in our sample (26 percent) experience only one election involving a businessperson and non-businessperson competing for the marginally elected seat on the council. In those cities, city fixed effects would absorb our identifying variation.

<sup>16</sup> Recall that businessperson margin is the difference between the businessperson's vote share and the competitor's vote share.



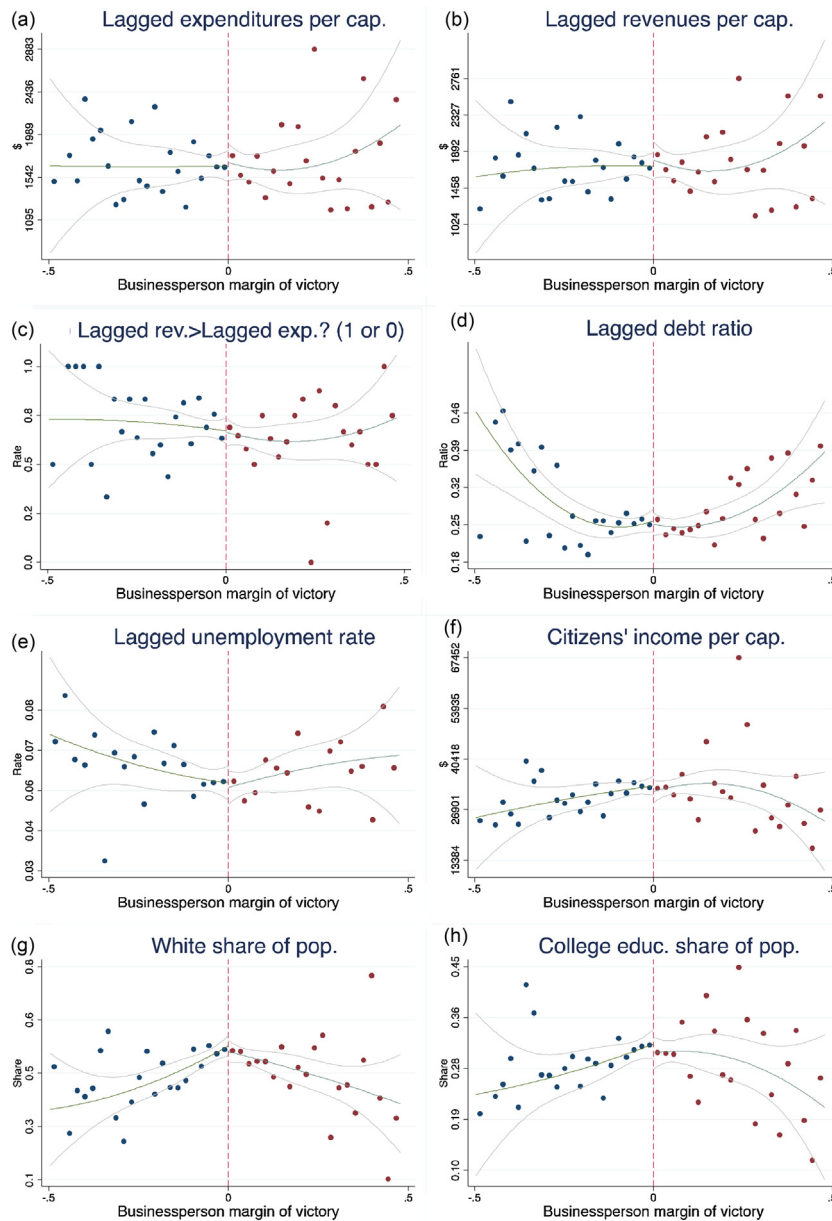
**Fig. 1.** Is a businessperson more likely to win a close election?

Notes: “Businessperson margin of victory” is on x-axis in both panels. This variable is the businessperson’s vote share minus the competitor (non-businessperson) vote share. If businessperson margin of victory  $> 0$ , the businessperson has won.

Fig. 1 provides two graphical assessments of this assumption. Panel A presents a simple histogram documenting the distribution of margin of victory. The light gray bars to the left of “Margin = 0” represent instances where the candidate with business experience loses the election, while the dark gray bars to the right represent instances where the candidate with business experience won. Two things are worth noting: first there are many elections close to the cutoff (these are narrowly contested elections). More importantly, the number of close elections resulting in a businessperson victory is roughly the same as the number of close elections resulting in a businessperson loss. This is further documented in Panel B where we follow McCrary (2008) and plot a discontinuous density function around the cutoff.<sup>17</sup> The figure demonstrates that the density just to the left of the cutoff is statistically indistinguishable from the density just to the right of the cutoff.

A second critical assumption for the RD design is that observable characteristics of the city (other than likelihood of a businessperson serving on the council and outcome variables of interest) are smooth around the cutoff. Of particular concern

<sup>17</sup> In fact, the figure is constructed using code from McCrary’s 2008 paper, which is available on his website.



**Fig. 2.** RD Validity: Binned scatterplots assessing continuity of observables at cutoff.

Notes: “Businessperson margin of victory” is on x-axis in all panels. This variable is the businessperson’s vote share minus the competitor (non-businessperson) vote share. If businessperson margin of victory > 0, the businessperson has won. All panels are binned scatterplots taking the binned average of different variables on the y-axis. All variables described as “lagged” (a–e) are the mean of outcome in the two years prior to the relevant election. “Lagged rev. > Lagged exp.” (panel c) equals 1 if city’s lagged total revenues > lagged total expenditures. Debt ratio (panel d) is defined as “Total liabilities/Total assets”. All monetary outcomes are real (2010) dollars.

in this case, for instance, is the possibility that candidates with business experience are more likely to be elected when the city is experiencing economic distress (e.g. high unemployment, large deficits, etc.) under the assumption they are better suited to solve these issues. If any of these other variables (e.g. unemployment rate at the time of election) vary discontinuously at the cutoff, we cannot be sure that we are identifying the causal impact of a councilmember with business experience or if any results are driven by having high pre-existing unemployment.

We address this concern in Fig. 2. There we present a series of binned scatterplots reporting the relationship between businessperson margin of victory (x-axis) and a variety of city-level characteristics that should not be expected to vary discontinuously at the cutoff (y-axis). The y-axis variables are reported as binned averages, binned within small ranges of businessperson margin of victory.

One set of observable characteristics we consider are lagged values of outcomes variables we use in our main results. For instance, Panel (a) reports “lagged expenditures per capita”. This is measured as the average expenditures in the city in the two years prior to election that had the potential to randomly assign a businessperson to the council. Council elections are every two years, so this can alternatively be thought of as expenditures in the previous council term. All “lagged” variables in this figure are measured in the same way. Panels (b)–(e) report lagged revenues, indicator of revenues exceeding expenditures, debt ratio, and unemployment rates respectively. All of these pre-treatment outcomes are smooth at the cutoff.

The remaining panels of Fig. 2 assess the relationship between demographic characteristics of the population and a businessperson’s margin of victory. In particular, we consider: (f) income per capita, (g) white population share, and (h) share of population with a college degree. We do not observe any discontinuities in the cutoff in these variables.

## 6.2. Assessing the causal impacts of electing a businessperson on financial and policy outcomes

Having established the validity of our regression discontinuity design, we now turn to the main question of the paper: does the election of a candidate with business experience impact policy outcomes? In short, the answer seems to be “No.” In this subsection, we provide two types of evidence to support this conclusion: simple graphical results and regression analysis.

We begin by graphically assessing the causal impact of a businessperson’s election to the council. Fig. 3 presents a series of binned scatterplots similar to those in Fig. 2. Each panel reports binned averages of an outcome variable that the elected candidate could potentially influence. The outcomes we consider are: (a) expenditures per capita, (b) revenue per capita, (c) an indicator for whether city’s revenues exceed expenditures, (d) debt ratio, and (e) unemployment rate. If any of these variables were causally impacted by the election of a businessperson, then we would expect to see a discontinuous jump near the cutoff that determines whether a businessperson wins (businessperson margin of victory = 0). Ultimately, all five of these variables are smooth and continuous through the cutoff, suggesting that the election of a businessperson does not impact any of these variables.

In Columns 2–4 of Table 1, we report the results of the parametric regression discontinuity approach as described in Section 5. Each row reports results for a different outcome variable. To assess the sensitivity of the results, we vary the function that we use to model “businessperson margin of victory”. In Column 2, we report results where margin of victory to enters (and interacts with businessperson victory) as a linear function. Margin of victory is modeled as a 2nd and 3rd degree polynomial in Columns 3 and 4 respectively. For all outcome variables, results are consistent across all three specifications.<sup>18</sup>

We assess the impact of a businessperson’s victory on the five outcome variables that we considered in Fig. 3 (per-capita expenditures, per-capita revenues, indicator that revenues exceed expenditures, debt ratio, and unemployment rate). We also consider several additional outcomes: public goods expenditures per capita, government administration expenditures per capita, quick ratio, and the fraction of services provided either by the city or through other means. In the regression analysis, all monetary outcomes (expenditures, revenues, and specific expenditure categories) are in logs.

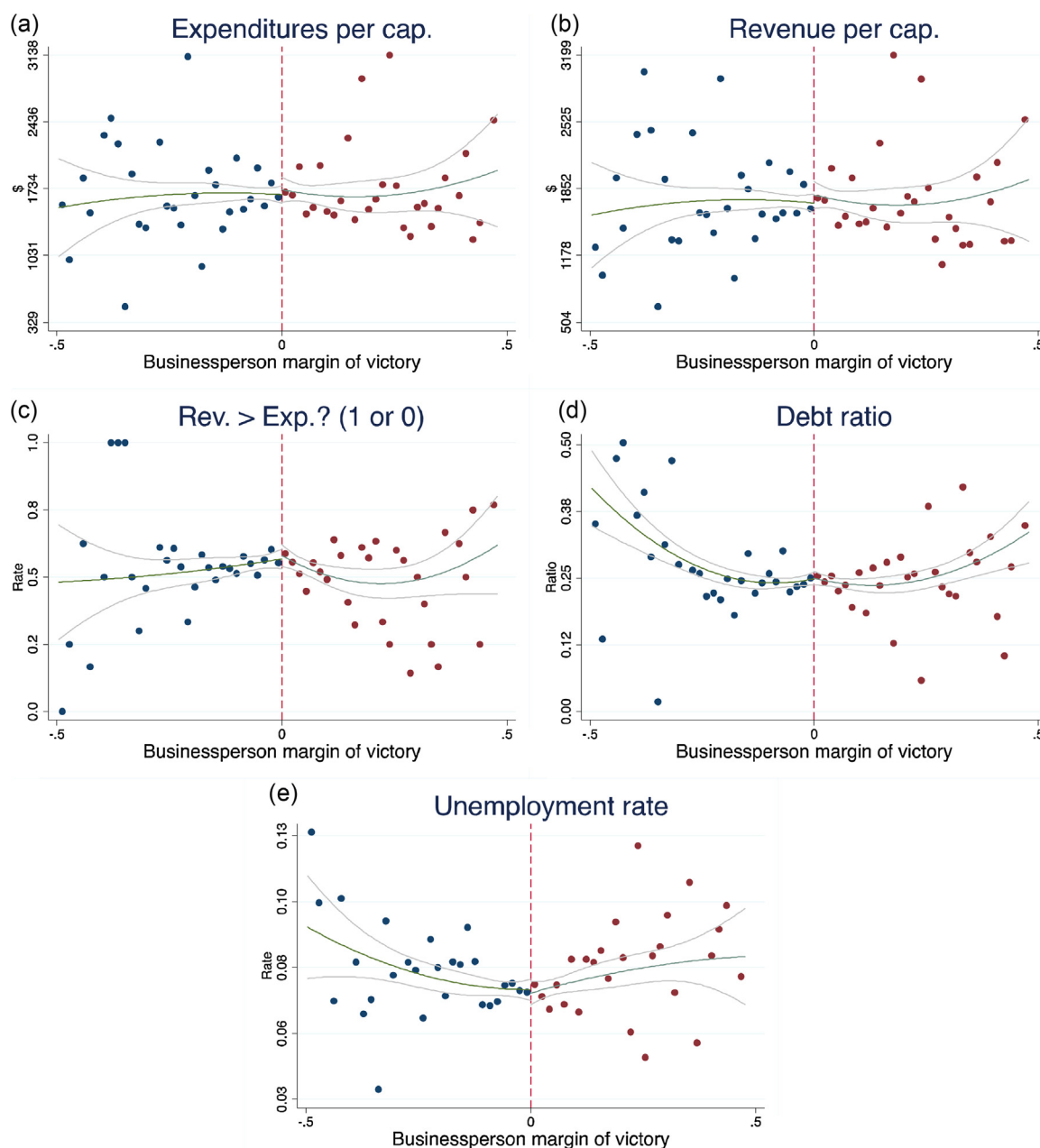
Table 1 reveals the same finding as Fig. 3: in general, there is no evidence that the election of a candidate with business experience has any impact – positive or negative – on policy outcomes. Governments do not experience any clear change in their expenditures, revenues, debt ratios, liquidity (as measured by the quick ratio), unemployment rates, or likelihood that revenues exceed expenditures. There are also no clear changes in the way governments provide services: the fraction of services provided by the city, other governments, or a private firm remains constant. Moreover, on most of the outcomes, not only are estimated coefficients not significantly different than zero, they are relatively close to zero in magnitude. Two exceptions to this are government administration expenditures and the quick ratio. While still imprecisely estimated, the coefficients suggest a change of spending of between 6 and 8 percentage points. The impact on the quick ratio is similarly imprecisely estimated, but – if anything – suggests a negative impact of business experience on liquidity.

Thus, our general conclusion from Table 1 is that there is little to no evidence that politicians with business experience impact spending and other policy outcomes. To further assess the sensitivity of these findings to different approaches, we report the results of two additional specifications in the appendix. Appendix Table A3 repeats the basic approach of Table 1, but includes city fixed effects. Appendix Table A4 employs a nonparametric regression discontinuity approach. There, we estimate local linear regressions with bandwidths selected optimally as in Imbens and Kalyanaraman (2011). In both cases, as in Table 1, we find no evidence that business experience impacts any of these outcomes.

Of course, one possibility is that there is heterogeneity in the impact of electing a businessperson that is masked in Table 1. For instance, business experience may have an impact on city outcomes only when the city is in economic distress but not otherwise. Alternatively, one might expect that a businessperson’s influence is not experienced until the final two years of his or her term, as policies may take time to take effect.

With this in mind, in Table 2 we report the results of specifications testing for differences across different types of cities and/or pre-existing financial conditions. Specifically, we modify the main estimating equation, interacting “businessperson

<sup>18</sup> In Table 1, we suppress coefficients and R-squared for space. Appendix Table A2 reports results from three specifications of Table 1 (2nd degree polynomial for outcomes: revenue, expenditures, and debt ratio), reporting R-squared and all coefficients. Also, as noted in the data section, standards for local government financial reporting changed in the early 2000’s. We assess whether this impacts our results in Appendix Table A1, we restrict the sample to fiscal years 2004 and beyond (after changes were fully installed). Results are nearly identical to our main results.



**Fig. 3.** Graphical RD results (binned scatterplots): Impact of businessperson victory on city financial and policy outcomes.

Notes: “Businessperson margin of victory” is on x-axis in all panels. This variable is the businessperson’s vote share minus the competitor (non-businessperson) vote share. If businessperson margin of victory  $> 0$ , the businessperson has won. All panels are binned scatterplots taking the binned average of different variables on the y-axis. “Rev.  $>$  Exp.” (panel c) equals 1 if city’s total revenues  $>$  total expenditures. Debt ratio (panel d) is defined as “Total liabilities/Total assets”. All monetary outcomes are real (2010) dollars.

wins” and all interacted “margin of victory terms” with a dummy indicating either that: the elected councilmember is in the last two years of his or her term (Panel A), the city is large (above-median population) (Panel B), the city’s revenues exceeded expenditures in its previous council term (Panel C), the city had an above-median debt ratio in its previous council term (Panel D), or the city experienced above-median unemployment in its previous council term (Panel E). Each panel reports results from a different specification using each of the specified interactions. Each column reports results from a different outcome variable. Here we report the five main outcome variables we considered in Fig. 3. In this table, we report results only from the 2nd degree polynomial RD specification, but results are consistent with 1st and 3rd degree as well.

Ultimately, Table 2 reveals that the lack of evidence of a businessperson impact is fairly constant across cities. For expenditures, revenues, debt ratio, unemployment, and likelihood that revenue exceeds expenditures, the election of a

**Table 2**

Assessing heterogeneity in impact of businessperson victory (RD).

| VARIABLES                                                                                       | (1)                    | (2)                 | (3)                      | (4)               | (6)                |
|-------------------------------------------------------------------------------------------------|------------------------|---------------------|--------------------------|-------------------|--------------------|
|                                                                                                 | ln(Expend. per capita) | ln(Rev. per capita) | Rev. > Exp.? (=1 if yes) | Debt ratio        | Unemp. rate        |
| Panel A: Differential impact in second half (last two years) of term?                           |                        |                     |                          |                   |                    |
| Businessperson wins                                                                             | −0.016<br>(0.052)      | 0.003<br>(0.051)    | 0.037<br>(0.036)         | −0.014<br>(0.014) | −0.000<br>(0.003)  |
| ... X 2nd half of term                                                                          | −0.012<br>(0.028)      | −0.026<br>(0.029)   | −0.061<br>(0.050)        | 0.007<br>(0.011)  | −0.003<br>(0.002)  |
| Panel B: Differential impact in large (above-median pop.) cities?                               |                        |                     |                          |                   |                    |
| Businessperson wins                                                                             | −0.044<br>(0.072)      | −0.029<br>(0.071)   | 0.049<br>(0.041)         | −0.013<br>(0.026) | −0.008<br>(0.007)  |
| ... X Large city                                                                                | 0.004<br>(0.103)       | 0.004<br>(0.101)    | −0.064<br>(0.059)        | 0.003<br>(0.030)  | 0.006<br>(0.007)   |
| Panel C: Differential impact in cities where pre-existing revenues > pre-existing expenditures? |                        |                     |                          |                   |                    |
| Businessperson wins                                                                             | −0.035<br>(0.094)      | −0.022<br>(0.092)   | −0.003<br>(0.054)        | 0.011<br>(0.031)  | −0.006<br>(0.005)  |
| ... X Lagged surplus                                                                            | 0.024<br>(0.111)       | 0.021<br>(0.109)    | 0.014<br>(0.064)         | −0.027<br>(0.035) | 0.006<br>(0.005)   |
| Panel D: Differential impact in cities with pre-existing debt ratios above median?              |                        |                     |                          |                   |                    |
| Businessperson wins                                                                             | −0.016<br>(0.075)      | −0.005<br>(0.075)   | 0.017<br>(0.039)         | 0.006<br>(0.010)  | 0.002<br>(0.003)   |
| ... X Lagged high debt                                                                          | 0.022<br>(0.101)       | 0.024<br>(0.099)    | −0.010<br>(0.059)        | −0.000<br>(0.023) | −0.009*<br>(0.005) |
| Panel E: Differential impact in cities with pre-existing unemployment above median?             |                        |                     |                          |                   |                    |
| Businessperson wins                                                                             | −0.053<br>(0.104)      | −0.044<br>(0.101)   | −0.061<br>(0.052)        | −0.001<br>(0.020) | −0.000<br>(0.002)  |
| ... X Lagged high unemp                                                                         | −0.009<br>(0.124)      | 0.008<br>(0.121)    | 0.118<br>(0.076)         | −0.029<br>(0.031) | −0.004<br>(0.005)  |
| Degree of polynomial                                                                            | 2nd                    | 2nd                 | 2nd                      | 2nd               | 2nd                |
| Additional controls                                                                             | Yes                    | Yes                 | Yes                      | Yes               | Yes                |
| Observations (see note)                                                                         | 3,448                  | 3,448               | 3,448                    | 3,445             | 2,260              |

Notes: All specifications employ a 2nd degree polynomial parametric RD approach. Each column reports results on a different outcome variable. Each panel represents a different set of regressions employing different interactions with the “Business person wins” dummy. For example, Panel A, ‘businessperson wins’ is interacted with a dummy indicating that a councilperson is in the final two years of the term. Coefficients on “margin of victory” terms and additional controls are not reported for space. Additional controls as described in main text. There are fewer observations in the regressions reported in Panel E, because we only observe unemployment rate for a subset of city-years. In Panel E, we have 2,257 observations in all five specifications. All monetary outcomes are real (2010) dollars.

Robust standard (clustered at city level) in parentheses.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

businessperson has the same effect regardless of whether we are looking at: large vs. small cities, cities with pre-existing financial issues vs. those without, the first two years of a councilmember’s term vs. the last two, or cities with high unemployment vs. those with low unemployment. Thus, there is no support for a narrative wherein business experience can help “fix” cities with financial problems.

The preceding results document a lack of support for the idea that business experience impacts observable policy outcomes. Of course, the election of a businessperson could impact in ways that are observed to voters but unobserved in our data. To assess this, we consider how well a candidate fares when they run for re-election. We repeat the same basic regression discontinuity approach, but take “vote share in the next election” as an outcome. If time  $t$  is the year that a candidate (with or without business experience) was randomly assigned to council, vote share here is measured as *votes received by randomly assigned candidate at time  $t + 4$  divided by total votes cast at time  $t + 4$* . Thus, the variable runs from 0 to 1. Ultimately, we are measuring whether voters are any more or less willing to re-elect a businessperson than a non-businessperson.

Table 3 reports the results. Columns 1, 2, and 3 differ only in the degree of polynomial used in modeling margin of victory. The specifications include the same set of controls used in the previous analyses (though, again, results are consistent without these controls). We find no differential re-election prospects of incumbents with business experience relative to those without. In Appendix Table A5, we report the results of a nonparametric regression discontinuity approach; the general conclusion is the same. In an alternative specification we use whether or not the candidate is successfully re-elected as the outcome variable of interest, and again find that candidates with business experience are no more likely to be re-elected than candidates without business experience.

## 7. Conclusion

Does professional experience prior to serving in an elected office matter for policy outcomes? Candidates frequently argue that their backgrounds in business, as community organizers, or as seasoned politicians will impact how they govern. Voters,

**Table 3**

Vote share of narrowly elected businesspeople in the next election they face (RD).

| VARIABLES            | (1)                      | (2)                      | (3)                      |
|----------------------|--------------------------|--------------------------|--------------------------|
|                      | Next election vote share | Next election vote share | Next election vote share |
| Businessperson wins  | 0.015<br>(0.020)         | 0.008<br>(0.021)         | 0.008<br>(0.022)         |
| Degree of polynomial | 1st                      | 2nd                      | 3rd                      |
| Additional controls  | Yes                      | Yes                      | Yes                      |
| Observations         | 606                      | 606                      | 606                      |
| R-squared            | 0.316                    | 0.326                    | 0.326                    |

Notes: The unit of observation in this table is a candidate-by-election. The sample is restricted to elections occurring four years after the narrow election that randomly assigned whether a businessperson is on the council. "Next election vote share"=(votes at time t for candidate randomly assigned at time t-4)/(total votes at time t). Coefficients on "margin of victory" terms and additional controls are not reported for space. Additional controls as described in main text.

Robust standard (clustered at city level) in parentheses.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

at least in their responses to polling, seem convinced that professional experience (and business experience in particular) will improve outcomes. Yet, there is little existing evidence to support this view. In this paper we focus on the impact of business experience in California city councils and find no evidence that candidates with business experience govern differently than candidates without business experience. Specifically, we employ a regression discontinuity design that exploits the fact that in sufficiently close elections the winner is plausibly randomly assigned. We then consider a range of outcomes that one might expect business experience to matter: spending, revenue, likelihood that revenues exceed expenditures, debt ratios, and unemployment. We find no statistically significant effects and for most outcomes the point estimates are not economically meaningful.

Our paper contributes to the more general literature on the relationship between candidate characteristics and resulting policy outcomes. At least in the context of city councils, we find that business experience does not influence policy outcomes. This is ultimately consistent with other papers that find that, particularly in local governments, characteristics that one might expect to matter in fact do not (e.g. partisan affiliation and gender – [Ferreira and Gyourko 2009, 2014](#)). Similar to these papers, our result can be interpreted as consistent with a Downsian model of electoral competition.

On the other hand, our results differ – to some extent – with findings that politicians with business experience exhibit distinct voting patterns in Congress ([Carnes, 2012](#)), especially on business-related issues ([Witko and Friedman, 2008](#)). However, our study is distinct in several ways. First, we take advantage of natural experiments that generate quasi-random assignment of businesspeople to elected office. We therefore strip away the potential that areas that elect businesspeople hold different preferences, which in turn, explains the distinct voting patterns of their representatives. Second, our focus is less on the voting patterns of individual politicians and more on the resulting policy outcomes. It is possible that city council members with business experience express distinct views relative to other members, but these views do not translate into policy differences.<sup>19</sup>

Of course, it should be noted that the regression discontinuity strategy does imply we are estimating a local effect wherein we are testing whether *narrowly elected* city council members have an impact on outcomes. It is possible that, by virtue of being narrowly elected, politicians lack the political capital to enact their preferred policies, which could explain our results. However, we note that existing studies have applied the regression discontinuity design to municipal council elections (e.g., [Folke, 2014; Beach and Jones, 2016](#)) and have found that even narrowly elected council members can influence outcomes. We also note that many city council elections are close. In our sample, roughly half of all elections are determined by 5 percent or less. Thus, in this context, a narrow election may not come with the same lack of political capital that might be true of elections to higher offices.

<sup>19</sup> Alternatively, it is of course possible that our results simply do not generalize from city councils to Congress. There is scope for further research on this front.

## Appendix A.

**Table A1**

Regression discontinuity estimates of the impact of businessperson victory on city financial and policy outcomes, restricted to FY2004 and beyond.

|                                                                                                           | (1)               | (2)                | (3)               |       |
|-----------------------------------------------------------------------------------------------------------|-------------------|--------------------|-------------------|-------|
| OUTCOME VARIABLES                                                                                         | RD: Linear        | RD: Quadratic      | RD: Cubic         | Obs.  |
| Per- capita expenditures<br>(Logged in regression)                                                        | −0.024<br>(0.048) | −0.024<br>(0.053)  | −0.011<br>(0.057) | 3,156 |
| Per-capita pub. good exp.<br>(Logged in regression)                                                       | −0.038<br>(0.043) | −0.023<br>(0.048)  | −0.007<br>(0.052) | 3,156 |
| Per-capita gov. admin. exp.<br>(Logged in regression)                                                     | 0.071<br>(0.054)  | 0.080<br>(0.061)   | 0.070<br>(0.071)  | 3,156 |
| Per-capita revenue<br>(Logged in regression)                                                              | −0.019<br>(0.047) | −0.009<br>(0.051)  | −0.001<br>(0.056) | 3,156 |
| Rev. > Exp.? (=1 if yes)                                                                                  | −0.023<br>(0.025) | 0.013<br>(0.030)   | 0.010<br>(0.035)  | 3,156 |
| Debt ratio<br>(Total liabs./Total assets)                                                                 | 0.000<br>(0.013)  | −0.008<br>(0.015)  | 0.001<br>(0.016)  | 3,153 |
| Quick ratio<br>(Cash+Inv./Current liabs.)                                                                 | −0.255<br>(0.330) | −0.215<br>(0.361)  | −0.240<br>(0.390) | 2,853 |
| Unemployment rate                                                                                         | −0.001<br>(0.002) | −0.000<br>(0.003)  | −0.001<br>(0.003) | 2,080 |
| Method of provision of city services: City, contract with private firm, or contract with other government |                   |                    |                   |       |
| Fraction of services provided<br>by city                                                                  | −0.016<br>(0.017) | −0.032*<br>(0.020) | −0.022<br>(0.021) | 3,156 |
| Fraction of services provided<br>through private contract                                                 | 0.009<br>(0.009)  | 0.013<br>(0.011)   | 0.014<br>(0.013)  | 3,156 |
| Fraction of services provided<br>by other governments                                                     | 0.007<br>(0.016)  | 0.020<br>(0.020)   | 0.007<br>(0.022)  | 3,156 |
| Degree of polynomial                                                                                      | 1st               | 2nd                | 3rd               |       |
| Additional controls                                                                                       | Yes               | Yes                | Yes               |       |

Notes: Data are restricted to years 2004 and beyond. Each row reports an outcome variable of interest. Columns 2–4 report the regression results. Each column differs only in the degree of polynomial through which “Businessperson margin of victory” enters. Thus, in Columns 2–4 each *cell* reports a coefficient from a different specification. Coefficients on “margin of victory” terms and additional controls are not reported for space. Additional controls as described in main text. All monetary outcomes are real (2010) dollars.

Robust standard (clustered at city level) in parentheses.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table A2**

Replicating main results from 2nd degree polynomial RD, displaying all coefficients.

|                                           | (1)                         | (2)                    | (3)                 |
|-------------------------------------------|-----------------------------|------------------------|---------------------|
| VARIABLES                                 | Ln(Expenditures per capita) | Ln(Revenue per capita) | Debt ratio          |
| Businessperson wins                       | −0.021<br>(0.052)           | −0.008<br>(0.051)      | −0.010<br>(0.015)   |
| Businessperson margin                     | −0.067<br>(0.595)           | −0.156<br>(0.571)      | 0.162<br>(0.152)    |
| Businessperson wins * Margin              | 0.093<br>(0.856)            | −0.013<br>(0.832)      | −0.128<br>(0.205)   |
| (Businessperson margin) <sup>2</sup>      | 0.100<br>(1.102)            | −0.057<br>(1.052)      | 0.380<br>(0.310)    |
| Businessperson wins * Margin <sup>2</sup> | 0.394<br>(1.712)            | 0.923<br>(1.633)       | −0.528<br>(0.379)   |
| Total population                          | 0.000***<br>(0.000)         | 0.000***<br>(0.000)    | 0.000**<br>(0.000)  |
| Median income                             | −0.000***<br>(0.000)        | −0.000***<br>(0.000)   | −0.000**<br>(0.000) |
| Income per capita                         | 0.000***<br>(0.000)         | 0.000***<br>(0.000)    | 0.000<br>(0.000)    |
| Share pop.: Hispanic                      | 0.816**<br>(0.363)          | 0.808**<br>(0.359)     | 0.128<br>(0.104)    |
| Share pop.: Other                         | 5.593**<br>(2.826)          | 5.107*<br>(2.841)      | −0.233<br>(0.854)   |
| Share pop.: Asian                         | −0.235<br>(0.276)           | −0.285<br>(0.271)      | −0.041<br>(0.066)   |

Table A2 (Continued)

| VARIABLES                      | (1)<br>Ln(Expenditures per capita) | (2)<br>Ln(Revenue per capita) | (3)<br>Debt ratio  |
|--------------------------------|------------------------------------|-------------------------------|--------------------|
| Share pop.: Native American    | 13.076***<br>(4.837)               | 13.757***<br>(4.821)          | 3.535**<br>(1.390) |
| Share pop.: Black              | 0.186<br>(0.372)                   | 0.177<br>(0.352)              | 0.516**<br>(0.210) |
| Share pop.: College degree     | 1.920**<br>(0.838)                 | 1.835**<br>(0.832)            | 0.098<br>(0.238)   |
| Share pop.: Some college       | 1.475<br>(0.964)                   | 1.366<br>(0.955)              | −0.071<br>(0.291)  |
| Share pop.: High school degree | −0.176<br>(0.882)                  | −0.173<br>(0.881)             | −0.090<br>(0.227)  |
| Share pop.: Some high school   | −1.009<br>(1.632)                  | −1.559<br>(1.594)             | 0.133<br>(0.581)   |
| Constant                       | 6.260***<br>(0.799)                | 6.395***<br>(0.792)           | 0.297<br>(0.235)   |
| Observations                   | 3,453                              | 3,453                         | 3,450              |
| R-squared                      | 0.259                              | 0.269                         | 0.154              |

Year fixed effects included but not reported.

Robust standard errors (clustered at city-level) in parentheses.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

Table A3

Regression discontinuity estimates of the impact of businessperson victory on city financial and policy outcomes, with city fixed effects.

| OUTCOME VARIABLES                                                                                          | (1)<br>Mean of variable | (2)<br>RD: Linear  | (3)<br>RD: Quadratic | (4)<br>RD: Cubic    | Obs.  |
|------------------------------------------------------------------------------------------------------------|-------------------------|--------------------|----------------------|---------------------|-------|
| Per-capita expenditures<br>(Logged in regression)                                                          | 1,684.945<br>(23.749)   | 0.002<br>(0.011)   | −0.007<br>(0.014)    | 0.004<br>(0.016)    | 3,453 |
| Per-capita pub. good exp.<br>(Logged in regression)                                                        | 1,241.439<br>(15.006)   | −0.003<br>(0.013)  | −0.011<br>(0.017)    | −0.003<br>(0.020)   | 3,453 |
| Per-capita gov. admin. exp.<br>(Logged in regression)                                                      | 196.114<br>(4.150)      | 0.016<br>(0.027)   | 0.026<br>(0.032)     | 0.066*<br>(0.035)   | 3,453 |
| Per-capita revenue<br>(Logged in regression)                                                               | 1,732.813<br>(24.697)   | −0.012<br>(0.012)  | −0.021<br>(0.016)    | −0.016<br>(0.018)   | 3,453 |
| Rev. > Exp.? (=1 if yes)                                                                                   | 0.544<br>(0.008)        | −0.032<br>(0.028)  | −0.036<br>(0.035)    | −0.034<br>(0.041)   | 3,453 |
| Debt ratio<br>(Total liabs./Total assets)                                                                  | 0.248<br>(0.003)        | −0.001<br>(0.008)  | 0.001<br>(0.009)     | 0.001<br>(0.010)    | 3,450 |
| Quick ratio<br>(Cash+Inv./Current liabs.)                                                                  | 3.898<br>(0.206)        | −0.138<br>(0.245)  | −0.253<br>(0.264)    | −0.310<br>(0.320)   | 3,123 |
| Unemployment rate                                                                                          | 0.073<br>(0.001)        | 0.001<br>(0.001)   | −0.000<br>(0.001)    | −0.001<br>(0.002)   | 2,262 |
| Method: of provision of city services: City, contract with private firm, or contract with other government |                         |                    |                      |                     |       |
| Fraction of services provided<br>by city                                                                   | 0.481<br>(0.003)        | 0.001<br>(0.003)   | 0.003<br>(0.004)     | 0.003<br>(0.004)    | 3,453 |
| Fraction of services provided<br>through private contract                                                  | 0.191<br>(0.002)        | −0.006*<br>(0.003) | −0.008**<br>(0.004)  | −0.009**<br>(0.004) | 3,453 |
| Fraction of services provided<br>by other governments                                                      | 0.329<br>(0.003)        | 0.005<br>(0.004)   | 0.005<br>(0.005)     | 0.005<br>(0.005)    | 3,453 |
| Degree of polynomial                                                                                       |                         | 1st                | 2nd                  | 3rd                 |       |
| Additional controls                                                                                        |                         | Yes                | Yes                  | Yes                 |       |

Notes: Table differs from Table 1 (in the main text) only in that city fixed effects are included in specifications reported in this table. Each row reports an outcome variable of interest. Even when we take the log of the variable in the regression analysis (as indicated), Column 1 reports the mean of the actual (not log-transformed) value of the variable. Columns 2–4 report the regression results. Each column differs only in the degree of polynomial through which “Businessperson margin of victory” enters. Thus, in Columns 2–4 each cell reports a coefficient from a different specification. Coefficients on “margin of victory” terms and additional controls are not reported for space. Additional controls as described in main text. All monetary outcomes are real (2010) dollars.

Robust standard (clustered at city level) in parentheses.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

**Table A4**

Nonparametric regression discontinuity approach—impact of businessperson victory on city financial and policy outcomes.

| OUTCOME VARIABLES                                                                                          | (1)<br>Mean of<br>variable | (2)<br>Nonpara. RD:<br>Optimal<br>bandwidth | (3)<br>Nonpara. RD: ½<br>X Optimal<br>bandwidth | (4)<br>Nonpara. RD: 2<br>X Optimal<br>bandwidth | Obs.  |
|------------------------------------------------------------------------------------------------------------|----------------------------|---------------------------------------------|-------------------------------------------------|-------------------------------------------------|-------|
| Per- capita expenditures<br>(Logged in regression)                                                         | 1,684.945<br>(23.749)      | −0.006<br>(0.028)                           | −0.001<br>(0.031)                               | −0.005<br>(0.025)                               | 3,453 |
| Per-capita pub. good exp.<br>(Logged in regression)                                                        | 1,241.439<br>(15.006)      | 0.037<br>(0.042)                            | 0.059<br>(0.055)                                | 0.029<br>(0.035)                                | 3,453 |
| Per-capita gov. admin. exp.<br>(Logged in regression)                                                      | 196.114<br>(4.150)         | 0.040<br>(0.065)                            | 0.094<br>(0.085)                                | 0.028<br>(0.053)                                | 3,453 |
| Per-capita revenue<br>(Logged in regression)                                                               | 1,732.813<br>(24.697)      | 0.061<br>(0.045)                            | 0.082<br>(0.057)                                | 0.046<br>(0.037)                                | 3,453 |
| Rev. > Exp.? (=1 if yes)                                                                                   | 0.544<br>(0.008)           | 0.051<br>(0.043)                            | 0.038<br>(0.056)                                | 0.028<br>(0.034)                                | 3,453 |
| Debt ratio<br>(Total liabs./Total assets)                                                                  | 0.248<br>(0.003)           | 0.014<br>(0.012)                            | 0.010<br>(0.015)                                | 0.010<br>(0.009)                                | 3,450 |
| Quick ratio<br>(Cash+Inv./Current liabs.)                                                                  | 3.898<br>(0.206)           | −0.374<br>(0.364)                           | −0.094<br>(0.715)                               | −0.819<br>(0.563)                               | 3,136 |
| Unemployment rate                                                                                          | 0.073<br>(0.001)           | 0.003<br>(0.004)                            | 0.003<br>(0.006)                                | 0.001<br>(0.004)                                | 2,262 |
| Method: of provision of city services: City, contract with private firm, or contract with other government |                            |                                             |                                                 |                                                 |       |
| Fraction of services provided<br>by city                                                                   | 0.481<br>(0.003)           | 0.015<br>(0.015)                            | 0.020<br>(0.020)                                | 0.011<br>(0.013)                                | 3,453 |
| Fraction of services provided<br>through private contract                                                  | 0.191<br>(0.002)           | −0.010<br>(0.010)                           | 0.020<br>(0.014)                                | −0.008<br>(0.008)                               | 3,453 |
| Fraction of services provided<br>by other governments                                                      | 0.329<br>(0.003)           | −0.007<br>(0.017)                           | −0.011<br>(0.023)                               | −0.016<br>(0.014)                               | 3,453 |

Notes: Each row reports an outcome variable of interest. All expenditures and revenue outcomes are measured per capita. Even when we take the log of the variable in the regression analysis (as indicated), Column 1 reports the mean of the actual (not log-transformed) value of the variable. Columns 2–4 report the regression results. Each column differs only in the bandwidth. Column 2 employs the optimal bandwidth as in [Imbens and Kalyanaraman \(2011\)](#); columns 3 and 4 halve or double this bandwidth for robustness. In Columns 2–4 each cell reports the estimated impact of a businessperson from a different specification. All monetary outcomes are real (2010) dollars.

Standard errors in parentheses.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

**Table A5**

Nonparametric regression discontinuity approach – Vote share of narrowly elected businesspeople in the next election they face (RD).

| VARIABLES           | (1)<br>Nonpara. RD: Optimal bandwidth | (2)<br>Nonpara. RD: ½ X Optimal bandwidth | (3)<br>Nonpara. RD: 2 X Optimal bandwidth |
|---------------------|---------------------------------------|-------------------------------------------|-------------------------------------------|
| Businessperson wins | −0.005<br>(0.025)                     | −0.007<br>(0.027)                         | −0.019<br>(0.022)                         |
| Observations        | 637                                   | 637                                       | 637                                       |

Notes: The unit of observation in this table is a candidate-by-election. The sample is restricted to elections occurring four years after the narrow election that randomly assigned whether a businessperson is on the council. Outcome variable is “Next election vote share”, which is measured as (votes at time t for candidate randomly assigned at time t-4)/(total votes at time t). Each column differs only in the bandwidth. Column 1 employs the optimal bandwidth as in [Imbens and Kalyanaraman \(2011\)](#); columns 2 and 3 halve or double this bandwidth for robustness.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

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