Gridlock: Ethnic diversity in government and the provision of public goods

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How does ethnic diversity in government impact public good provision? We construct a novel dataset linking the ethnicity of California city council candidates to election outcomes and expenditure decisions. Using a regression discontinuity approach, we find that increased diversity on the council leads to less spending on public goods. This is especially true in cities with high segregation and economic inequality. Those serving on councils that experience an increase in diversity also receive fewer votes when they run for reelection. These results point towards disagreement within the council generating lower spending.

Cities in the United States and elsewhere are increasingly ethnically diverse. A growing body of empirical work has considered the political and economic consequences of this trend, highlighting the possibility for both positive and negative effects.[1] For instance, Ottaviano and Peri (2005 and 2006) show that ethnic diversity increases productivity, resulting in higher wages and higher home values. On the negative side, Alesina, Baqir and Easterly (1999) argue that ethnic diversity generates disagreement that reduces government service provision. Alesina et al. assume that preferences over spending vary by ethnic group. Consequently, greater heterogeneity leads to the adoption of a "compromise"

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[1] See Alesina and La Ferrara (2005) for a detailed survey of this literature.

good, which citizens are not willing to fund because of its distance from their preferred type of good.

Because local public spending decisions are ultimately made by city councils, we analyze the extent to which ethnic diversity within the council affects spending. This contrasts with previous empirical studies, which only consider diversity in the city as a whole and not government itself. While some research has examined how the election of a candidate of a particular identity (e.g., ethnic identity, gender, etc.) affects spending and other outcomes, our paper is distinct in highlighting the impact of the composition (or the interaction between distinct identities) of a governmental body.

The relationship between ethnic diversity in government and public spending is theoretically ambiguous. Within a governing body (in this case, the city council), diversity may lead to "gridlock": different preferences lead to disagreement over the type of public good to provide and, therefore, a reduction in spending. Alternatively, electoral pressure may result in councilmembers who (regardless of their ethnic identities) hold relatively similar policy positions (as would be suggested by a Downsian model of electoral competition). In that case, the entire council would agree and we would not expect a relationship between diversity and spending. We discuss a simple theoretical framework that elaborates upon this tension in Section II.

To examine the relationship between ethnic diversity in government and public good spending, we study city councils in California. We construct a novel dataset that identifies the ethnicity of city council members and candidates in California from 2005 to 2011. We then pair these data with detailed annual city budgets. Our focus on city council composition naturally lends itself to a quasi-experimental design. Following Lee, Moretti, and Butler (2004), we focus on close elections that could potentially shift the diversity of the council (i.e., an election between members of two different ethnic groups). In close elections, the

winner is plausibly random and, as a result, so too is the resulting change in diversity. We show that the narrow election of a candidate whose ethnicity does not match the city's modal ethnicity is a plausibly exogenous shock to diversity within the city council. Relying on this fact, we implement a regression discontinuity design that allows us to measure the extent to which these random shocks to diversity affect spending on public goods.

Our results indicate that diversity leads to gridlock. Cities reduce the amount they spend on public goods as their city council becomes increasingly diverse. These effects are largest for segregated cities and cities with more income inequality (where the potential for disagreement may be largest). We also find that all members of a council that experienced an exogenous shock to diversity receive fewer votes when they run for re-election. This latter point suggests that the city's population is dissatisfied with the decline in public goods, ruling out the possibility that diverse councils simply achieve greater efficiency in public good provision.

Our results contribute to the existing literature in two important ways. First we add to the literature on diversity within a city and public good provision. Beginning with Alesina et al. (1999), this literature has debated whether ethnic heterogeneity in cities reduces spending on public goods. Analyzing a cross section of U.S. cities, Alesina et al. document a negative relationship between ethnically diverse cities and spending on public goods.[2] However, recent empirical work finds that this relationship is not robust when estimated in a panel (Boustan et al., 2013; Hopkins, 2011) or with additional controls (Gisselquist, 2014).[3] By taking a different angle and exploring the way that diversity in

[2] They find that diverse cities spend less on productive goods (education, roads, and sewerage) but spend more on police protection. Some of their specifications attempt to account for the potential endogeneity of diversity using lagged diversity as an instrument, which yields similar results.
[3] It should be noted that Boustan et al. (2013) revisit the relationship between diversity and public good expenditures as a subsection of a paper otherwise addressing a different question.

government impacts government spending decisions, we hope to bridge this gap in the literature. Ultimately, our results suggest that in the context of a small elected decision-making body, Alesina et al.'s predictions are realized: diversity in the council negatively impacts public good spending.

Second, our results also speak to a more general literature on the impact of diversity within small groups, teams, or organizations, which has been studied in economics, psychology, and human resources. That literature tests competing hypotheses: diversity may lead to disagreement and a decline in performance, or diversity may lead to a variety of skills and ideas, generating improved performance. Results are mixed, but more papers in the literature point towards a negative effect of diversity rather than a positive effect (see Shore et al. 2009 for a review of this literature). Most recently, Ben-Ner et al. (2014) finds that diversity may have positive *or* negative effects depending on the group's goals.[4]

The remainder of this paper is organized as follows: Section I discusses institutional details related to city councils in California. In Section II, we present a theoretical framework for how diversity within a governing body might affect public good provision. Section III discusses the data that we draw on, and Section IV describes our empirical approach. Our main results appear in Section V. Section VI presents results from several tests aimed at illustrating that the underlying mechanism for the fall in public spending is indeed "gridlock." Section VII concludes.

I. City Councils in California

[4] In particular, they find that diversity has a negative impact when the group's goal is self-promotion (documented empirically in the context of soccer players on offense), but a negative impact when the group's goal is preventing a negative outcome (empirically: soccer players on defense).

California state law provides a number of guidelines for the structure of municipal governments. City councils must contain five councilmembers and councilmembers are elected "at-large" during a general municipal election. Councilmembers serve staggered four-year terms, with elections filling multiple seats every two years.[5] Elections are nonpartisan, so neither the voters nor we observe a candidate's political party. California state law defines the mayor as simply another member of the city council and does not provide for any additional powers. The mayor is typically selected by the city council from amongst its own members. In these "council-manager" cities, the council (including the mayor) dictates policy for the city, which is in turn carried out by the city manager.

There are two ways that a city can deviate from the above guidelines. If the city is "general law" – the default form of government for incorporated cities – then it can submit a ballot measure to be approved by the electorate. For "chartered" cities, any deviation must be specified in the city's charter. Nevertheless, a 2006 survey conducted by the International City/County Management Association (ICMA) provides a number of statistics illustrating that most cities conform to the state's guidelines.[6] Specifically, 93 percent of cities are council-manager cities and the mayor serves on the city council for 98 percent of the cities – because of this, when we calculate the ethnicity of the city council we include mayors. 88 percent of cities only have five councilmembers and councilmembers are elected at-large for 92 percent of cities. A city's institutional structure also tends to be relatively stable over time. In the five years preceding the survey fewer than seven percent of cities attempted to alter their form of government. When cities do attempt to alter their form of government, it is

[5] For instance, there may an election in 2004 to fill 3 of the 5 seats followed by an election in 2006 to fill the remaining two seats.

[6] The survey in question is ICMA's 2006 *Municipal Form of Government* survey, which is the source of all of the statistics in this section.

typically to switch from at-large to district based elections (or vice versa). Many of these attempts, however, were ultimately unsuccessful.

II. Theoretical framework

The theoretical model presented by Alesina, Baqir, and Easterly (1999) is an important motivation for our empirical work. In their model, preferences over public goods vary across ethnic/racial lines. Consequently, they conclude that increased diversity within a group (in their case: a city's population) leads to increased disagreement and ultimately lower spending. We apply this model to the small five-person groups that form California city councils: if there is disagreement over the type of public good within the council (perhaps associated with ethnicity), we may expect lower public good spending.

As in Alesina et al. (1999), we assume citizen preferences vary across ethnic/racial lines. That is, individuals from particular ethnic groups have correlated preferences that differ from the preferences of other groups. This might be genuine differences in preferences, or may simply be linked to disagreements over the physical location of public goods that generate very localized benefits (e.g., disagreement over which neighborhood to build a park within).[7] Different models of electoral competition make different predictions on whether a diverse population leads to diverse views on the city council. In the remainder of this section, we review two prominent theoretical models of electoral competition that provide contrasting answers. From there, predictions follow from Alesina et al.'s model.

[7] Obviously, if there are no differences in preferences across ethnic groups in citizens or candidates, the theoretical prediction is that diversity within the council should not lead to changes in public good provision.

A. Downsian competition

In the standard Downsian model of electoral competition (Downs, 1957), two vote-maximizing candidates are competing for an office, and in competing for votes, both candidates ultimately adopt the policy position preferred by the median voter. That is, the standard model predicts "policy convergence." Of course, the elections we study have more than two candidates and more than one winner. Cox (1984) extends the Downsian model to settings with multiple winners, and again predicts policy convergence. If these models describe our setting, then the necessary prerequisite for gridlock is not met; all councilmembers – regardless of ethnicity – hold the same view on what policy should be enacted, and that view is likely the view of the median voter.

> Hypothesis 1: As in the Alesina et al. model, if preferences do not vary (because all candidates, regardless of ethnicity, cater to the median voter) increased diversity within the council should not impact public goods expenditures.

B. Citizen-candidate models

In part because candidates have been found to deviate from the pure policy convergence prediction[8], many alternative models on the relationship between voter preferences and policy outcomes have arisen to better accommodate this fact. Especially relevant are "citizen-candidate" models of electoral competition (Osborne and Slivinski, 1996; Besley and Coate, 1997). In those models, candidates are policy-motivated. Individuals ("citizens") decide to run if the

[8] For instance, policy divergence has been documented amongst US Representatives (Lee, Moretti, and Butler, 2004), local governments in Sweden (Pettersson-Lidbom, 2008), governors in the US (Hill and Jones, 2016).

benefits they experience from the opportunity to (possibly) enact their preferred policy outweigh the costs of running, and therefore – if elected – they continue to maintain their individually preferred policy as their policy platform. Thus, unlike under the Downsian model, individuals running for (and being elected to) office do not necessarily propose the same policy and instead maintain their personally preferred policy position.[9]

> Hypothesis 2: If there is an increase in diversity in the council (and if different ethnic groups have different preferences), then there will be disagreement over public goods expenditures. Following from Alesina et al., this will lead to lower spending on public goods.

III. Data

In this paper, we study the link between city government spending and ethnic diversity within city government. We rely on three broad sources of data to identify: (1) the names and vote totals of individuals who served on a city council or ran for city council but lost; (2) the ethnicities of those councilmembers and candidates; and (3) how much the city council spent and the allocation of those expenditures amongst various categories.

The first source of data (and the reason we focus on California) is the *California Election Data Archive* (CEDA), which provides the names and number of votes for every candidate in every local government election occurring between 1995 and 2011. Because ethnicity is not listed in this dataset, we supplement the

[9] It is worth noting that models of "distributive politics" (e.g., Cox and McCubbins, 1986) lead to similar predictions, albeit through very different channels. In those models, politicians are vote-maximizing but instead of catering to the median voter they promise transfers to (or spending that benefits) particular identifiable groups in order to encourage turnout. For the sake of brevity, we omit discussion of other models and instead highlight that citizen-candidate models are one of several possible alternatives to the standard Downsian framework.

CEDA election returns with novel data (discussed below). Together, these datasets allow us to identify the ethnic composition of city councils and the counterfactual composition (what the composition would have been had the losing candidate won). Finally, we obtain expenditure data from the California State Controller's Office city budget records. These records provide detailed annual accounts of expenditures and revenues for every city in California between 2005 and 2011.

A. Ethnicities

We focused our data collection efforts for ethnicity on the 5,177 individuals who either (1) served on the city council between 2005 and 2011 or (2) ran for city council but just lost. Our ethnicity data come from three sources. First, we contacted 450 cities to inquire about the availability of ethnicity information for city councilmembers and candidates. 230 cities responded to this request but only 96 were able to provide us with any information. Those 96 cities, however, provided ethnicity information for 714 councilmembers and candidates.

To fill in the gaps we collected pictures of councilmembers and candidates from candidate websites, newspaper articles, voting pamphlets, and other sources. We successfully located photos for 3,615 councilmembers and candidates. After collecting these photos we conducted a survey on Amazon's "Mechanical Turk" website where we asked workers to report the candidate's ethnicity based on the candidate's name and picture.[10] The worker could choose from the following options: White, Black, Native American, East Asian or Pacific Islander, Indian, Middle Eastern or North African, or Hispanic. We also asked the worker to identify the gender of the candidate.

[10] Workers were specifically asked to indicate the race/ethnicity/ancestral background that provides the best description of the individual based on their name and photograph.

We collected ten unique responses for each candidate. There was no limit to the number of photographs a worker could code, but they never observed a candidate more than once. For the sake of incentive compatibility, workers were told that the responses from other workers would be used to judge the accuracy of their work. Specifically, workers were not paid unless a majority of their responses matched the modal response.

We use these responses to code ethnicity only if a majority of workers agreed on a single response. This restriction removes 31 individuals from our sample. The average rate of agreement for the remaining 3,584 individuals was 94 percent, which implies that on average 9.4 of the ten workers chose the same ethnicity. The average rate of agreement for gender was 99 percent.

Because our contact with cities and our collection of photos occurred simultaneously, there is some overlap between the two samples. Specifically, for 263 individuals we have ethnicity information obtained from both the city and from our "Mechanical Turk" methods. This provides an opportunity to assess the accuracy of our "Mechanical Turk" method. In general, the Mechanical Turk responses matched the response from cities 95 percent of the time. The correlation between whether the city identified a candidate as White and whether the "Mechanical Turk" identified the candidate as White is 0.89. The correlations for Black, Asian, and Hispanic response are 1, 0.84, and 0.88, respectively.[11]

These correlations suggest that mismatches between city and "Mechanical Turk" responses likely result from the Mechanical Turk choosing "White" instead of "Asian" or "Hispanic". To remedy this, we contacted the *National Association of Latino Elected and Appointed Officials* and the *Asian Pacific American Institute for Congressional Studies* to obtain ethnicity information for Asian and

[11] We are unable to compute correlations for Native American, Middle Eastern, and Indian because they are a much smaller share of the population and do not appear in our "overlap" sample.

Hispanic councilmembers and candidates. These organizations maintain lists of government officials that are of Hispanic or Asian origin, and together they provided us with the ethnicity of 571 Asian and Hispanic candidates (191 of which were not listed in either our "city" or "Mechanical Turk" sources). Because we have ethnicity information from three sources (city, Mechanical Turk, and ethnic lists), we assign candidates an ethnicity in the following way: we use the information provided by cities as the true ethnicity whenever possible. We then rely on the lists obtained from ethnic organizations to identify any remaining Hispanic or Asian candidates. Mechanical Turk responses are then used to fill in any gaps. This further increases the accuracy of the Mechanical Turk responses because we are only relying on their ability to determine whether a candidate is White, Black, Native American, Indian, or Middle Eastern. Our final sample includes ethnicities for 4,226 of the 5,177 councilmembers and candidates who either served on the city council between 2005 and 2011 or ran for city council but just lost. Put another way, our sample allows us to identify the ethnicity of each councilmember for 1,750 of the 2,316 council-year pairs between 2005 and 2011.

B. Measuring diversity

To measure diversity we focus on fractionalization and polarization, the most prominent measures used within the literature.[12] Both indices range from zero to one where zero corresponds to a situation where there is no diversity. Fractionalization is maximized when each councilmember is of a different ethnicity. Polarization, on the other hand, is maximized when the seats are

[12] $Fractionalization_{ct} = 1 - \sum_e (share_{cte})^2$ and $Polarization_{ct} = 4 * \sum_e (share_{cte})^2 (1 - share_{cte})$ where $share_{cte}$ is the share of the city council in city *c* during year *t* that is of ethnicity *e*.

distributed into two ethnic groups. The typical council has five seats, where 3.9 of those seats are held by white councilmembers. Fractionalization ranges from 0 to 0.75. The mean is 0.229 and the median is 0.32. Polarization ranges from 0 to 1 with a mean of 0.408. The median is 0.64. Complete summary statistics for the composition of city councils are reported in Panel A of Table 1.

<Table 1 about here>

C. Outcome variables

Our outcome variables are drawn from annual city budgets, which we obtained from the California City Controller's Office. These datasets report detailed expenditure and revenue categories for every city in California. For instance, we observe the amount spent on parks, servicing debts, police, etc. in addition to revenue from various sources (general revenue, intergovernmental revenue, etc.).

Ultimately, we are interested in the impact of diversity on public good spending. One challenge is that there is substantial variation across cities in the types of public goods offered. To achieve uniform measures of spending across cities, we collapse spending into two broad categories: public goods and non-public goods. The non-public goods category includes spending on government administration and debt servicing, both of which are provided by the Controller's Office. "Public goods" is a category of spending we created by taking a city's total expenditures for the year and removing expenditures on "government administration" and debt repayment. The "Public goods" category therefore includes all spending on roads, parks, police protection, sewerage, public transportation, etc. This aggregated measure of public good expenditures provides us with a common measure across all of the cities in our sample.

The first two columns of Table 1 report summary statistics for all city-year pairs that appear in the California Controller dataset and the subset of city-year pairs where we were able to obtain ethnicity information for each councilmember.[13] Although the cities with completed councils are slightly larger, ethnic composition and spending patterns across the two samples are similar. Specifically, the city populations in both samples are roughly 50 percent white with fractionalization indices of 0.50 and polarization indices of 0.70. In both samples, per-capita spending on public goods is roughly $1450 and per-capita spending on non-public goods is about $240. In the third column, we report summary statistics for the sample of city-year pairs that will be used in our regression discontinuity analysis (this approach is described in greater detail in Section IV). Again, relative to the rest of California, these cities are slightly larger and have a smaller white share but exhibit similar spending patterns and have a similar level of overall diversity. The distribution of total spending and the distribution of year-to-year changes in spending on public goods are reported in Figure 1.[14] It is perhaps worth noting that California's Proposition 13 established limits on local taxation. Thus variation in spending is likely larger in states without these types of restrictions.

<Figure 1 about here>

IV. Empirical approach

A. Empirical approach and data

[13] The city of Vernon appears in the California Controller dataset but we exclude it from this table and our analysis as it is an extreme outlier. Vernon has a population of 112, and when it is included in Column 1 average spending across all California cities rises from roughly $1,500 per capita to $6,000 per capita.

[14] Figure 1 reports the distribution for the RD sample. The distribution for the full sample is nearly identical once the sample is truncated at the 99th percentile in order to remove the long right tail.

To assess the relationship between diversity and public good spending in our panel of data, we might simply regress spending on fractionalization or polarization (and include city and year fixed effects).[15] Of course, in doing so, we may be concerned about endogeneity between diversity and spending. To deal with this, we use close elections with the potential to impact council-level diversity as a source of random variation.[16] Specifically, we adopt a regression discontinuity design, focusing on elections between a candidate whose ethnicity differs from the modal ethnicity of the city and a candidate whose ethnicity matches the modal ethnicity.[17] We do so under the assumption that the election of the "non-modal" candidate increases the diversity of the council. This assumption is tested and overwhelmingly confirmed in the next section.

In California, most city councilmembers are elected "at large", which means that it is usually the case that multiple candidates are competing for multiple seats. For example, five candidates might compete for three seats on the council with the seats being assigned to the candidates with the three highest vote shares.[18] For the purposes of this study, whether an election is classified as being between a modal and non-modal candidate depends on the ethnicity of the candidate who narrowly won the election and the ethnicity of the candidate who narrowly lost. In the five candidates competing for three seats example, the race

[15] These simple regressions reveal a positive but statistically insignificant relationship between diversity and public spending.

[16] The idea of using close elections as a source of random variation in political composition was made famous by Lee et al. (2004). Recent work analyzing elections in the U.S. House of Representatives (Caughey and Sekhon, 2011; Grimmer et al., 2011) has challenged the validity of this design, arguing that even close elections are often nonrandom. However, Eggers et al. (2014) cast doubt on this claim. Eggers et al. study elections at several levels and across time periods, and find no evidence that incumbents are more likely to win in a close election.

[17] We focus on modal ethnicity because the majority group can vary from city to city, particularly in California where it is possible to have majority Hispanic cities.

[18] For at-large elections occurring between 2006 and 2009 (our sample period), the number of seats that were available for any given election ranged from 1 to 5 with a mean of 2.4. The number of candidates ranged from 1 to 16 with a mean of 5.4.

would only be classified as "modal vs non-modal" if the ethnicity of the candidate with the third highest vote share matched the city's modal ethnicity and the ethnicity of the candidate with the fourth highest vote share differed from the city's modal ethnicity (or vice versa). Some cities hold district-based elections where each seat is decided by a separate election. Our empirical strategy requires at most one election for each city-year pair. For these cities, we use the closest election between two candidates of different ethnicities as the election of interest.

We have ethnicity data for both the winning and losing candidate for 684 of the 817 competitive city-council elections that occurred between 2006 and 2009.[19] For the remaining incomplete elections, 76 are incomplete because we could not find the ethnicity of the losing candidate while 16 are incomplete because we could not identify the winning candidate's ethnicity. An additional 41 elections are incomplete because we could not identify the ethnicity of either candidate. Although one might be concerned that the candidates we could find pictures for are of better quality than candidates for which no picture could be obtained, it is not immediately clear how this should bias our results as our empirical strategy relies on knowing the ethnicity of both the winning and losing candidate. Furthermore, the fact that ethnicity data were obtained from two additional sources should help alleviate this concern.

We restrict our sample to cities that experienced an election between a "modal" and a "non-modal" candidate *at any point* between 2006 and 2009, as these are the cities where an election has the potential to change the diversity of the council. For these cities, we construct a panel that spans from fiscal year 2005-06 until fiscal year 2010-11.[20] A city faces the potential for becoming "treated" following the first election between a "modal" and "non-modal" candidate. Prior to this, all cities are considered "untreated". If the non-modal

[19] "Competitive" elections are elections involving more candidates than open seats on the council.
[20] The fiscal year runs from July to July.

candidate wins the election, then the city is treated; in the notation of our empirical models, an indicator called "non-modal wins" is set to 1. If the modal candidate wins, the city remains untreated and "non-modal wins" remains 0.[21]

B. Empirical model

We estimate the impact of a non-modal victory on spending outcomes using a regression discontinuity approach within a narrow bandwidth (selected in accordance with Calonico et al.'s (2014) optimal bandwidth selection procedure). Specifically, we estimate variations of the following equation:

$$\begin{aligned} y_{ct} \quad = \quad & \propto + \beta_1 \mathbf{1}[Non-modal\ wins_{ct}] + \beta_2 margin\ of\ victory_c \\ & + \beta_3 \mathbf{1}[Non-modal\ wins_{ct}] * margin\ of\ victory_c \\ & + \varepsilon_{ct} \end{aligned} \tag{1}$$

where subscript "c" indicates the city and "t" indicates the year. The dependent variable, y_{ct}, is generally ln(per-capita spending) and standard errors are clustered at the city-council level (i.e. the city-year observations where decisions are made by the same city council) as that is the level at which treatment occurs. A candidate is classified as "non-modal" if their ethnicity does not match the modal ethnicity within the city.[22] The variable $\mathbf{1}[Non-modal\ wins_{ct}]$ is an indicator that turns to one if the non-modal candidate (in city c) wins (or has won) the election by year t.

The variable "margin of victory" is simply the difference between the vote share received by the winner and the vote share received by the loser. In elections

[21] Because the fiscal year runs from July to July and elections are held in November, a non-modal candidate elected in November of 2006 will not have any input on the 2006-2007 fiscal year budget. The first budget the non-modal candidate will have the opportunity to affect is the 2007-2008 budget. Our treatment indicator is modeled in such a way that it would remain "0" for the 2006-2007 fiscal year and would turn to "1" starting with the 2007-2008 fiscal year.

[22] The modal ethnicity within a city is drawn from decennial Census data. We use the modal ethnicity from 2008 (the midpoint of our sample), which was calculated by interpolating ethnic shares between the 2000 and 2010 censuses.

with "multiple winners" (as in a city council election to fill multiple seats), the margin victory is measured for marginal candidates: the difference between vote share of the last-placed winner and the first-placed loser. Because of this, β_1 can be interpreted as the impact of a non-modal victory when the margin is zero; in practice, we interpret this as the impact of a victory in a very close election. Thus, the "non-modal wins" coefficient is of primary interest as it can be interpreted as the causal impact of a non-modal victory.

Notice that $margin\ of\ victory_c$ is not time varying and is thus not indexed by t. Although coding the margin as zero before the election may seem natural, it does not make sense in our panel framework. Specifically, coding margin of victory as zero prior to the treatment-inducing election implies that the counterfactual to a non-modal win is not just a non-modal loss (which is our intention), but also all observations prior to the election. Of course, setting the "margin of victory" as constant generates complications if a city experiences more than one election that might cause a shift in diversity. Thus, for some cities, it is necessary to truncate their panel so that each city in our sample experiences only one election between a modal and non-modal candidate. For the 40 cities in our sample that experience a second election between a modal and non-modal candidate, we truncate their panel, dropping all observations coinciding with and following the year that the second potentially treatment-inducing election occurs.

This approach is equivalent to estimating a local linear regression with a uniform kernel (Hahn et al., 2001). We estimate these specifications within a narrow bandwidth around the cutoff. The bandwidth is selected in accordance with Calonico et al.'s (2014) optimal bandwidth selection procedure. We identify the optimal bandwidth associated with our main specification and use that throughout. In practice, our analysis includes observations where the margin of victory falls within the range: [-0.071, 0071]. As we show in the next section, our results are robust to smaller and larger bandwidths.

While we estimate some specifications that follow the conventional cross-sectional regression discontinuity approach (described above), our main specifications take advantage of the panel nature of the data to provide additional precision and yield more conservative estimates. In some specifications, we modify the above specification simply by taking the first-difference of the outcome variable. However, the primary specification used through most of the paper fully adopts a panel framework. Specifically, we estimate the same basic equation described above, but with city and year fixed effects:

$$\begin{aligned} y_{ct} \quad = \quad & \propto + \beta_1 \mathbf{1}[Non-modal\ wins_{ct}] + \beta_2 margin\ of\ victory_c \\ & + \beta_3 \mathbf{1}[Non-modal\ wins_{ct}] * margin\ of\ victory_c \\ & + [city\ FEs]_c + [year\ FEs]_t + \varepsilon_{ct} \end{aligned} \tag{2}$$

This specification is similar to the "differences-in-discontinuities" approach employed by Grembi, Nannicini, and Troiano (2016) and intuitively can be thought of as a differences-in-differences specification where treatment is randomly assigned by the narrow election of a non-modal candidate. As in the simpler specifications, equation (2) is estimated within the optimal bandwidth.

V. Empirical results

A. Assessing non-modal victory as an exogenous shock to diversity

In this section, we conduct tests to assess the validity of our research design. First, we note that our outcome variable of interest (within-government diversity) is not binary as the extent to which any candidate affects the diversity of the city council depends both on the candidate's ethnicity and the ethnicity of the other councilmembers. Because of this, we show that the election of a non-modal candidate increases diversity within the city council in Panel A of Table 2. Specifically, we estimate variations of equation (2) taking measures of council-level diversity as the outcome variable. Regardless of whether we measure

diversity with fractionalization (column 1) or polarization (column 2), there is a strong and positive relationship between the election of a non-modal candidate and the diversity of the city council. To get a sense of the magnitude, consider shifting from a council with four White members and one Hispanic member to a council with three White members and two Hispanic councilmembers. This would change fractionalization from 0.32 to 0.48, which is roughly equivalent to the magnitude observed for the "non-modal" indicator in Table 2. Having established that a narrow non-modal victory indeed generates a positive shock to the diversity of the council, we simply use the narrow non-modal victory as our treatment in the remainder of the paper.

<Table 2 about here>

In Panel b of Table 2 we ask whether it is appropriate to interpret the narrow election of a non-modal candidate as an exogenous shock to council-level diversity. A standard concern for the regression discontinuity approach is that other observable characteristics vary at the cutoff; one might be concerned, for instance, that non-modal candidates are more likely to be female or are more likely to be elected in diverse cities and that these facts – rather than the shock to the diversity of the council – explain any differences in public goods spending. In column 2, we test whether there are discontinuities in a variety of observables at the cutoff.[23] Specifically, we employ the cross-sectional regression discontinuity approach on the set of cities that ever experience a close election between a "modal" and "non-modal" candidate in the year that the relevant election occurs.[24] We take a variety of city-level variables as outcomes, which – if our design is

[23] Column 1 of that table reports means of the variables we consider.
[24] Graphical evidence using the Calonico et al. (2014) cross-sectional non-parametric approach is presented in appendix Figure A1.

valid – should not vary discontinuously at the cutoff. These include: fractionalization, polarization, segregation, income inequality, the share of voters registered as a Democrat, the share of registered voters, a measure of political competition taken from Besley et al. (2010) that reaches a maximum when there is exactly the same number of Democrats and Republicans in a city, and pre-election spending on public goods. In addition to these variables, we also consider the diversity of the rest of the council members, and the gender of the winning candidate. We find no statistically-significant differences associated with the narrow election of a non-modal candidate, which indicates that these potentially important confounds are balanced around the cutoff.

Finally, we address another common concern in regression discontinuity designs: the running variable (in this case, non-modal margin of victory) should also be balanced around the cutoff (the point where one candidate barely wins). If we define the running variable as [*non-modal margin=non-modal vote share – modal vote share*], this implies that there should be roughly the same number of observations to the left of *non-modal margin* as there are just to the right of *non-modal margin*. This issue is especially important to our research design. Not only have some questioned the "randomness" near the cutoff when implementing regression discontinuity designs to electoral outcomes (Caughey and Sekhon, 2011; Grimmer et al., 2011) but Vogl (2014) documents concerns specifically in the context of race and city politics.

In Figure 2, we follow McCrary (2008) and plot a discontinuous density function around the cutoff (non-modal margin=0). Figure 2 is similar to graphs used by Vogl (2014).[25] 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. We can also document that modal candidates are not more likely to win

[25] Code for this procedure is available from McCrary's website.

close elections using simple statistical tests. Ideally, when the election is close, the probability of a non-modal victory should be 0.5. When an election is decided by a margin of 5 percent of less, the observed proportion of non-modal victories is 0.5062. Using a binomial test, this is statistically indistinguishable from 0.5 (the p-value is effectively one).[26]

<Figure 2 about here>

B. Preliminary results

Figure 3 presents our first piece of causal evidence that an increase in the diversity of the governing body decreases expenditures on public goods. Figure 3 is a binned scatter plot where cities are organized by the non-modal candidate's margin of victory (on the x-axis) and spending is on the y-axis. Following Calonico et al. (2014), we non-parametrically assess the relationship between "non-modal win margin" and per-capita spending. In the first panel of Figure 3, we see that spending on public goods falls by roughly 30 percent at the cutoff (where the non-modal candidate just wins the election). Spending on non-public goods (panel three) is largely unaffected. In the second panel of Figure 3 we adopt a first-differences approach that is more conservative and more in line with our preferred specification. In that panel, we see that per-capita spending on public goods falls by roughly 12 percent when the non-modal candidate is narrowly elected. First difference estimates for non-public good spending (panel four) indicate that spending on non-public goods increases by about 15 percent following the election of a non-modal candidate. While this result is actually in

[26] The same holds when we tighten the definition of a "close election". When an election is decided by a margin of 2 percent of less, the observed proportion of non-modal victories is 0.4898. Using a binomial test, this too is statistically indistinguishable from 0.5.

line with a prediction of Alesina et al. (1999) – that patronage payments increase in response to gridlock– we hesitate to lean too hard on this result as it is not robust (as we will show in Table 4).

<Figure 3 about here>

C. Main results: Regression Discontinuity Design

In Table 3 we formally estimate the relationships depicted in Figure 3. We start with a cross-sectional non-parametric approach following Calonico et al. (2014). Specifically, we estimate the relationship between the election of a non-modal candidate and the level of per-capita spending (within the optimal bandwidth). As in the first panel of Figure 3, column 1 of Table 3 indicates that per-capita spending is approximately 30 percent lower in cities where a non-modal candidate barely wins, a result that is significant at the 5-percent level. The impact on non-public good spending (column 4) is small, negative, and statistically insignificant, as in Figure 3. In columns 2 and 5 we use first-differenced spending as our outcome variable, an approach that is more in line with the panel approach that we ultimately adopt. Consistent with panels two and four of Figure 3, per-capita spending on public goods falls by roughly 11 percent while spending on non-public goods increases by about 21 percent when the non-modal candidate is narrowly elected.

Turning to our parametric, panel-based approach, columns 3 and 6 formally estimate equation (2) and yields results consistent with the preceding estimates; per-capita spending on public goods falls by approximately 13 percent (significant at the 1-percent level) following the election of a non-modal candidate. The effect on non-public goods spending remains positive (roughly 14 percent) but is not significant at conventional levels.

<Table 3 about here>

All of these specifications yield results that are consistent with the Alesina et al. (1999) argument that diversity in a group generates disagreement over public goods, which in turn implies a reduction in the willingness to spend. For the remainder of the paper we will focus our attention on the parametric panel-based approach as the results are more conservative and the inclusion of both year and city fixed effects increases the precision of our estimates.

In Table 4, we test the robustness of our result to different specifications. Column 1 again uses the optimal bandwidth; we see that our results are qualitatively identical when spending is measured in levels rather than logs (rows two and four). Columns 2 and three demonstrate the robustness of our results to other bandwidths. In column 2 we restrict to the set of cities where the treatment-inducing election was decided by a margin of less than 3.6 percent (half the optimal bandwidth), and in column 3 we expand our sample to include elections that were decided by less than 14.2 percent (twice the optimal bandwidth). Although we see slight variation in precision, the point estimates are largely unaffected: the election of a non-modal candidate decreases per-capita spending on public goods by roughly 10 percent. In column 4, we interact non-modal wins with a second degree polynomial function of margin of victory (as opposed to the linear interaction in the main specification). Again, results are largely unaffected. Our main specification clusters standard errors at the council level (the interaction of city and the two year period that a particular form of a council exists) as this is the unit of treatment, but results are robust to clustering at the city-level (column 5). Results are also robust to restricting to the samples to cities where the council consists of five members – thereby dropping from the sample cities with notably different forms of city council organization – (column 6), and restricting sample

so that the "pre-treated" period is taken to be the year just before treatment, and no years before that (column 7).

<Table 4 about here>

Two other sets of results are reported in the appendix. First, in appendix Table A1, we run a series of specifications taking specific spending categories (community development, culture/leisure, health, public safety, and public transit) as outcomes. Results are less precise, as there is variation across cities in what the city government actually provides, but we consistently estimate a negative relationship between non-modal victory and spending for each of these categories Second, in appendix Table A2, we show that tax revenue (column 1) and transfers from the state government (column 2) are unaffected by the election of a non-modal candidate. Thus, the drop in expenditures cannot be explained by a restricted budget. This is worth noting, especially in California where cities' budgets may be more restricted than elsewhere due to Proposition 13.

VI. Probing the mechanism

A. Heterogeneity in treatment effects

Thus far, we have seen that diversity in a city council leads to a reduction in spending on public goods. This result is consistent with the argument that more diversity within a council leads to disagreement and "gridlock," but other potential explanations do exist. In Table 5 we probe the mechanism by interacting our treatment variable (non-modal wins) with various city and council-level characteristics. Specifically, we assess whether there is heterogeneity in the effect of diversity on the basis of: city-level segregation, city-level inequality, city-level

diversity, city-level non-white share, rest-of-council diversity, council-level non-white share, and spending in the term prior to the treated council. Recall that these characteristics are balanced around the cutoff (see Table 2), so any heterogeneity observed here is not simply driven by a failure of randomization.

Two words of caution are warranted. First the dimension of heterogeneity may simply correlate with the underlying differences in effect, rather than cause them. Second, in no column can we reject equality between the two coefficients at conventional levels of statistical significance. Thus, we take these results as suggestive, but not definitive.

In the first column we partition the sample based on the median level of segregation in the city.[27] For gridlock to occur, it must be that there is disagreement between councilmembers of different ethnicities. We do not have direct evidence on differences in preferences over types of public goods across ethnic lines. However, there may be disagreement within the council over the location of public goods. For instance, councilmembers may agree on the type of park they would like to fund, but councilmembers may have a preference for positioning the park so that it is accessible to their own group. Of course, in order for this to be a source of disagreement across ethnic lines, there must be some degree of segregation within the city. Results indicate that the decline in public spending is indeed more pronounced in segregated cities, pointing towards gridlock as a mechanism.[28]

[27] We use tract-level census data to construct a measure of segregation for each city in the sample. In particular, we measure segregation using the *multi-group dissimilarity index,* as proposed by Reardon and Firebaugh (2002). The index runs from 0 to 1, where 0 indicates that a city is totally integrated and segregation increases as the index approaches 1.

[28] This result is roughly consistent with (and may help speak to mechanisms driving) Alesina and Zhuravskaya's (2011) results. In a cross-country comparison, they find that ethnic segregation is associated lower quality of government along a number of dimensions including "government effectiveness", which captures citizen satisfaction with government provision of goods, services, and infrastructure.

Following similar logic, it is reasonable to expect that preferences over public goods may vary depending on socioeconomic status. Insofar as there are correlations between ethnic group and socioeconomic standing[29], disagreement across socioeconomic groups in a city can generate disagreement across ethnic groups within the council. Thus, if gridlock drives the decline in spending, we might expect a larger decline when there is more opportunity for disagreement; in this case, that means there may be a larger decline when there is more income inequality within the city. This appears to be the case empirically, as we document in column 2.[30]

<Table 5 about here>

The remaining columns of Table 5 further assess heterogeneity in our result across cities. Column 3, which partitions the sample at the median level of fractionalization amongst the city's population (0.57), suggests that our result is more pronounced in more homogenous cities. In column 4, we see that the effect is driven by cities with a larger non-white share. It is possible for results to be larger in homogenous cities and cities with large non-white shares as there are many cities with a large share of Hispanics. However, because we cannot reject that the "high" and "low" coefficients are different, we do not intend to draw too much from this.

Column 5 splits the sample based on the diversity (fractionalization) of the council members *excluding* the one elected in the treatment-inducing election. We lose precision in these estimates because there are many councils for which we can observe the ethnicities of the modal and non-modal candidates, but cannot

[29] Given widespread evidence of black-white wage gaps, as well as other ethnic/racial wage gaps (Altonji and Blank, 1999), this is a reasonable assumption.
[30] We measure inequality using the Theil index.

observe the ethnicity of *all* members of the council they are elected to. Nonetheless, the resulting coefficients are very similar in magnitude; an increase in diversity leads to a decrease in spending whether councils are otherwise homogenous or diverse. This is the same conclusion that we draw from Column 6. In that column, we partition the sample based on the non-white share of the council (again excluding the winner of the treatment-inducing election) and again find remarkably similar magnitudes.

Finally, one plausible alternative explanation for our results (which we explore further in Subsection C.) is that spending falls because new councilmembers enter and increase the efficiency of council spending (rather than generating disagreement). If that were the case, we might expect larger reductions in spending in cities where there was high spending in recent council terms. In column 7, we split our sample by cities with high or low pre-existing spending on public goods and find nearly identical effects of diversity. This provides evidence against the "improved efficiency" mechanism as an explanation for our results.

B. Diversity or minority representation?

Next, we consider whether our results are driven by diversity of the council per se (an impact coming from a change in the *composition of the council*), or whether they are driven by a change in minority representation (an impact coming from the identity of a particular candidate). The empirical finding is of interest either way, but our hypothesized mechanism (disagreement driven by multiple opinions on the council, leading to a decline in spending) is most plausible if the result is driven by diversity. To assess this, we report regression discontinuity estimates of the impact of a member of a particular group (Asian, Black, Hispanic, or White) winning an election against someone from a different

group.[31] Results (reported in Table 6) reveal no clear pattern. While the results in Table 6 are underpowered, we should note that expanding the sample to include elections that were decided by less than 14.2 percent produces nearly identical results.

<Table 6 about here>

The lack of group-specific effects helps reconcile our results with recent research finding that the identity of a (narrowly) winning candidate does not impact spending patterns in city government (e.g., Ferreira and Gyourko, 2009, 2014). Our results in Table 6 suggest this to be the case in our setting as well. Instead, our results highlight that while the identity of a particular candidate may not impact outcomes, a shift in the *composition of government* can impact outcomes. This again points to "gridlock" as a potential mechanism, as gridlock is a phenomenon that necessarily depends not just on the identity/preferences of an individual, but on the general composition of the group in question.

C. Electoral consequences of treatment

Thus far we have interpreted a decline in public spending as evidence of gridlock, a negative outcome. As noted, it may instead be that diverse councils find ways to provide the same public goods more efficiently, which would be a positive outcome for the city. This falls in line with the strand of literature documenting that within-group diversity can lead to more creative solutions to problems. Note that providing the same level of goods and services more

[31] The empirical methodology is the same as before, however, the relevant election becomes a narrow election between an Asian and non-Asian candidate (for instance) instead of a narrow election between a modal and non-modal candidate.

efficiently would be perceived as a "positive" outcome by voters, whereas gridlock would be perceived as "negative". Thus, getting a sense of whether voters support "treated" councils may help focus in on certain classes of explanations.

To assess whether a decline in spending is perceived as a negative outcome by voters, we turn to a measure of voter satisfaction: the electoral success of city council members in the *next* election that they face. Specifically, we estimate a variation of equation (2) taking each councilmember's vote share when they run for re-election as the outcome variable. Note that we obtain the future vote share of every councilmember that served with the winner of the close election between a modal and non-modal candidate.

We first set the data up as a panel, where we observe each candidate's vote share twice. The first vote share comes from the election that brought them into office and the second vote share comes from the election following the potentially modal vs. non-modal election. This allows us to control for candidate ability by including candidate fixed effects. Because the number of candidates in the race mechanically impacts vote share, we include indicator variables that account for the number of candidates seeking election. Results (reported in Column 1 of Table 7) indicate that for councils that experienced an increase in diversity (because the non-modal candidate won, councilmembers have vote shares that are four percentage points smaller when they seek re-election.

In column 2 we simply analyze future vote shares for councilmembers that seek re-election in a cross-section. Again, we see that councilmembers that served on (relatively) more diverse councils have a smaller vote share when they seek re-election. Of course, it may be that vote share falls mechanically because more candidates run when there is a more diverse council. We find that this is not the case. In columns 3 and 4 we take number of incumbents seeking re-election and total number of candidates as the outcome variables. In both cases, we find no

clear impact of a diverse council on the number of candidates running for office. Based on these results, it seems that the decline in public good spending is indeed an outcome that is viewed as dissatisfactory to voters.

<Table 7 about here>

VII. Conclusion

We analyze the relationship between ethnic diversity and public good provision by constructing a novel dataset linking the ethnicity of city councilmembers to election outcomes and expenditure decisions. This allows us to exploit close elections as a source of random variation in the ethnic composition of a city council. We first show that the narrow election of a candidate whose ethnicity is *not* the city's modal ethnicity is a plausibly exogenous shock to diversity within the city council. More precisely, the election of a non-modal candidate is associated with an increase in diversity at the government level and is not associated with systematic differences in other city-level characteristics (e.g., ethnic diversity within the city or city-level political composition). We then implement a regression discontinuity design, which allows us explore how this increase in diversity affects the provision of public goods.

We find that an increase in diversity on the city council leads to a reduction in the amount spent on public goods. This result is consistent with an application of Alesina et al.'s (1999) theoretical model to the small five-person groups that form the council. Their model suggests ethnic diversity in a group leads to disagreement over the type of public good being provided and ultimately lower spending. We call this the "gridlock" hypothesis, which we probe further to assess if this mechanism explains our main result. The decline in spending is largest in cities with high levels of segregation. In cities with high ethnic

segregation, the physical location of (very local) public goods offers a source for disagreement (even where there is no other ethnic disagreement over "type" of public good being offered). Moreover, we find that members of diverse councils face diminished re-election prospects, ruling out the possibility that the drop in spending is perceived as a positive outcome by citizens. We take these results as evidence of the gridlock hypothesis as a mechanism driving our results.

We should add several caveats to the interpretation of our main result. Most importantly, we emphasize that we are examining the impact of diversity on a single outcome, which certainly does not represent the entirety of government's influence on citizens' wellbeing. A bit more specifically, although *spending* falls, we are unable to say anything about actual provision of the public good or changes in distributional outcomes. To more fully assess the welfare and equity implications of this decline in spending, future research should find group-specific outcomes to assess whether certain groups disproportionately benefit or suffer from a more diverse government. Data on consumption or enjoyment of publicly provided services, for instance, would allow future researchers to more fully address these issues.

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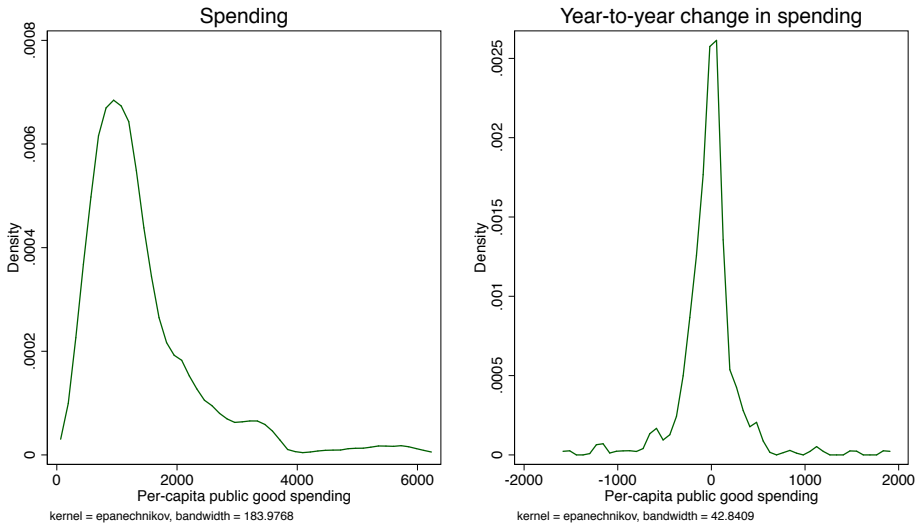
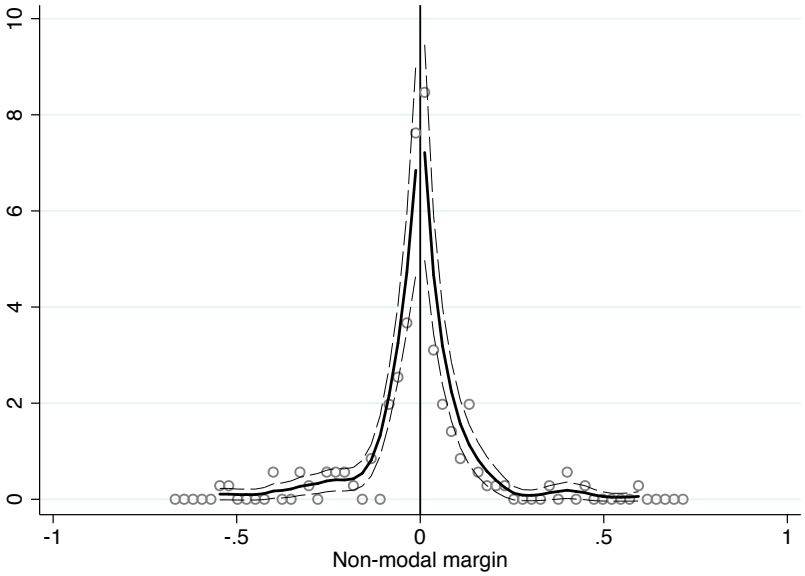
Figure 1: Distribution of per-capita public good spending patterns (RD sample)



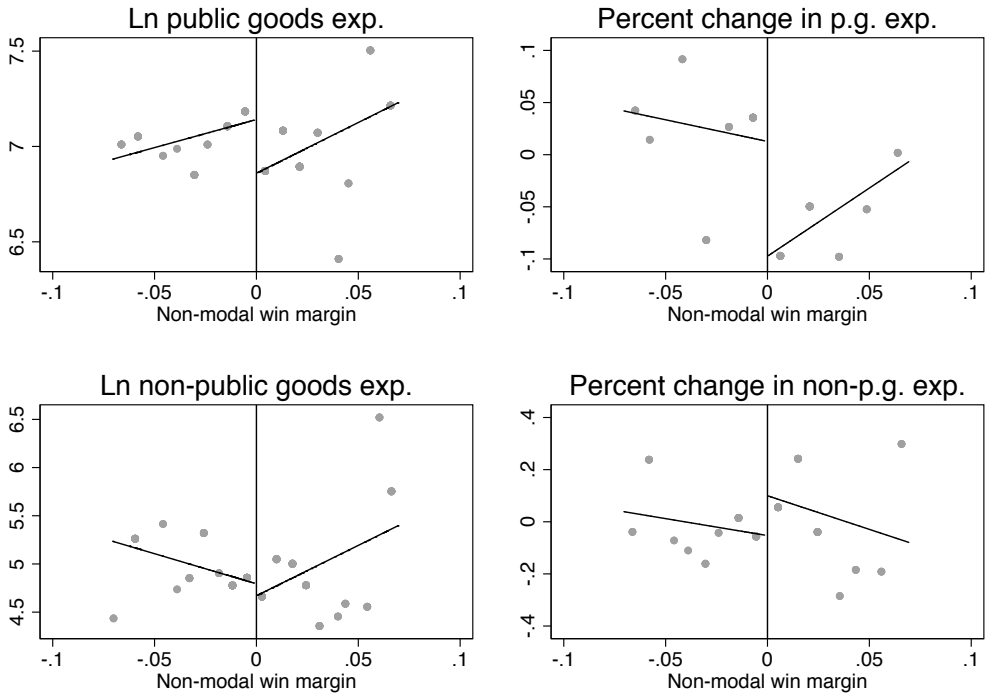
Sample restricted to the set of cities that ever experience a close election between a modal and non-modal candidate (i.e. an election that was decided by a margin of less than 7.1 percent). A modal candidate is a candidate whose ethnicity matches the city's modal ethnicity.

Figure 2: Distribution of non-modal margin of victory



A modal candidate is a candidate whose ethnicity matches the city's modal ethnicity. The "Non-modal margin" of victory is simply the *non-modal vote share – modal vote-share*. The dashed lines represent the 95% confidence interval around the estimate.

Figure 3: Binned scatter plot of per-capita spending and the non-modal candidate's margin of victory



Sample restricted to the set of cities that ever experience an election between a modal and non-modal candidate (i.e. an election that was decided by a margin of less than 7.1 percent). A modal candidate is a candidate whose ethnicity matches the city's modal ethnicity. Each "Public goods" is simply total expenditures minus expenditures on "government administration" and debt repayment. The "Public goods" category therefore includes all spending on roads, parks, police protection, sewerage, public transportation, etc.

Table 1: Summary statistics

Panel a: Council-level characteristics (completed councils only)				
	Mean	Min	Max	Standard deviation
Fractionalization	0.228	0	0.75	0.232
Polarization	0.405	0	1	0.392
Number of seats	5.303	4	11	0.981
White councilmembers	3.907	0	9	1.661
Hispanic councilmembers	0.950	0	7	1.434
Asian councilmembers	0.198	0	4	0.546
Black councilmembers	0.205	0	5	0.580
Other councilmembers	0.043	0	3	0.224

Panel b: City-level demographics				
	All cities	Completed councils	RD sample	Cities with incomplete elections
Total population	63,444	76,620	73,880	30,608
	(19,5813)	(22,2735)	(69,520)	(55,063)
White share	0.485	0.462	0.342	0.510
	(0.254)	(0.251)	(0.205)	(0.276)
Hispanic share	0.341	0.348	0.426	0.353
	(0.252)	(0.252)	(0.205)	(0.282)
Asian share	0.103	0.118	0.153	0.072
	(0.124)	(0.133)	(0.161)	(0.108)
Black share	0.037	0.040	0.049	0.031
	(0.053)	(0.055)	(0.061)	(0.041)
Other share	0.008	0.006	0.006	0.012
	(0.008)	(0.004)	(0.005)	(0.013)
Fractionalization within city	0.489	0.502	0.540	0.441
	(0.161)	(0.163)	(0.144)	(0.161)
Polarization within city	0.702	0.708	0.750	0.661
	(0.175)	(0.176)	(0.148)	(0.183)
Panel c: City-level budget information				
Per-capita public good expenditures	$1,451.23	$1,445.09	$1,405.55	$1,439.23
	(1,383.83)	(1,491.65)	(964.81)	(1,428.03)
Per-capita non-public good expenditures	$236.23	$241.50	$204.26	$268.74
	(370.06)	(400.75)	(238.57)	(577.83)
Per-capita tax revenues	$634.21	$647.99	$507.97	$651.82
	(931.36)	(991.67)	(298.42)	(1,264.27)
Per-capita revenus from state transfers	$12.26	$11.85	$9.30	$19.75
	(60.47)	(68.25)	(12.69)	(122.80)

For panels b and c, standard deviations are reported in parentheses. “All cities” refers to the 2,316 city-year pairs that reported expenditure information to the California City Controller’s office

between 2005 and 2011. “Completed councils” restricts to the 1,750 council-year pairs where the ethnicity is known for each councilmember. “RD sample” restricts to the cities that experience a close election between a modal and non-modal candidate (i.e. an election that was decided by a margin of less than 7.1 percent). A modal candidate is a candidate whose ethnicity matches the city’s modal ethnicity. Total city-year pairs is 382 in this sample. In the “cities with incomplete elections” there are 537 council-year pairs. Population characteristics are obtained by interpolating between the 2000 and 2010 decennial Censuses.

Table 2: Validity of regression discontinuity design

Panel a: Impact of a non-modal win on council diversity		
	Fract.	Polar.
Non-modal wins	0.125***	0.210***
	(0.043)	(0.076)
Observations	339	339
R-squared	0.845	0.812

Panel b: Balance at the time of election		
	Mean in sample	RD balance (diff. at cut-off)
City fractionalization	0.545	-0.058
	(0.149)	(0.044)
City Polarization	0.749	-0.002
	(0.153)	(0.045)
City segregation	0.235	-0.019
	(0.091)	(0.028)
City income inequality	0.159	0.004
	(0.048)	(0.014)
City Democrat share	0.471	-0.046
	(0.109)	(0.032)
City registered voter share	0.439	-0.033
	(0.118)	(0.035)
City political competition	-0.093	-0.004
	(0.063)	(0.019)
Pre-election PG spending per capita	1382.450	-351.231
	(902.73)	(263.692)
Rest-of-council fractionalization	0.325	-0.060
	(0.218)	(0.064)
Female winner	0.343	-0.083
	(0.477)	(0.148)

Sample is restricted to the set of cities that ever experience a close election between a modal and a non-modal candidate (i.e. an election that was decided by a margin of less than 7.1 percent). A

modal candidate is a candidate whose ethnicity matches the city's modal ethnicity. In panel a, the sample is further restricted to the set of cities where we know the ethnicity of every councilmember. Panel a is estimated as a panel, and thus includes city and year fixed effects and the non-modal wins indicator is interacted with margin of victory, but these coefficients are not displayed. Robust standard errors (clustered at council-level) in parentheses. Panel b is estimated as a cross-section using outcome variables from the election-year. The non-modal wins indicator is interacted with margin of victory, although those coefficients are not displayed. Column 1 of panel b reports the sample mean and standard deviations are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: The impact of a non-modal victory on per-capita spending

	ln(Public good spending)			ln(Non-public good spending)		
	(1) Cross-sectional non parametric	(2) First difference non-parametric	(3) Panel parametric	(4) Cross-sectional non parametric	(5) First difference non-parametric	(6) Panel parametric
Non-modal wins	-0.302**	-0.114*	-0.127***	-0.034	0.210**	0.143
	(0.137)	(0.054)	(0.043)	(0.176)	(0.105)	(0.105)
Calonico et al. (2014) 95% CI	[-0.644 to -0.042]	[-0.233 to 0.004]		[-0.426 to 0.365]	[0.003 to 0.459]	
Observations	338	142	372	338	142	372

Sample is restricted to the set of cities that ever experience a close election between a modal and a non-modal candidate (i.e. an election that was decided by a margin of less than 7.1 percent). A modal candidate is a candidate whose ethnicity matches the city's modal ethnicity. The "public goods" spending category is simply total expenditures minus expenditures on "government administration" and debt repayment. The "Public goods" category therefore includes all spending on roads, parks, police protection, sewerage, public transportation, etc.

Table 4: Alternative specifications for assessing the impact of a non-modal victory on per-capita spending

	(1) Optimal bandwidth	(2) Half-optimal bandwith	(3) Twice optimal bandwidth	(4) Second degree polynomial
ln(per-capita public good spending)	-0.127***	-0.097*	-0.109***	-0.119**
	(0.043)	(0.051)	(0.035)	(0.055)
Per-capita public good spending	-158.379**	-182.396**	-158.514***	-206.584**
	(69.452)	(74.668)	(56.251)	(81.436)
ln(per-capita non-public good spending)	0.143	0.284**	0.041	0.312**
	(0.105)	(0.132)	(0.086)	(0.141)
Per-capita non-public good spending	32.588	71.757**	20.256	89.617**
	(29.959)	(33.312)	(24.495)	(37.117)
	(5) Standard errors clustered at the city-level	(6) Restricting to cities with five councilmembers	(7) Omitting observations more than one year before treatment	
ln(per-capita public good spending)	-0.127**	-0.101**	-0.089**	
	(0.052)	(0.048)	(0.042)	
Per-capita public good spending	-158.379*	-143.583**	-149.092**	
	(87.771)	(66.517)	(62.398)	
ln(per-capita non-public good spending)	0.143	0.089	0.182*	
	(0.140)	(0.121)	(0.105)	
Per-capita non-public good spending	32.588	30.159	47.373	
	(37.202)	(28.433)	(33.595)	

Robust standard errors (clustered at the council level) in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Sample is restricted to the set of cities that ever experience a close election between a modal and a non-modal candidate (i.e. an election that was decided by less than 7.1 percent). A modal candidate is a candidate whose ethnicity matches the city's modal ethnicity. Each regression includes city and year fixed effects. The non-modal wins indicator is also interacted with margin of victory. The "public goods" spending category is simply total expenditures minus expenditures on "government administration" and debt repayment. The "Public goods" category therefore includes all spending on roads, parks, police protection, sewerage, public transportation, etc.

Table 5: The impact of a non-modal victory interacted with city and council-specific variables on log public good spending per capita

	(1) City segregation	(2) City inequality	(3) City diversity	(4) City non-white share	(5) Rest-of-council diversity	(6) Rest-of-council non-white share	(7) Pre-election spending
Treat X Low	-0.050	-0.066	-0.151**	-0.073	-0.110*	-0.134**	-0.133*
	(0.070)	(0.053)	(0.061)	(0.052)	(0.059)	(0.062)	(0.072)
Treat X High	-0.131***	-0.191***	-0.078	-0.183***	-0.096	-0.112	-0.127**
	(0.049)	(0.064)	(0.056)	(0.065)	(0.092)	(0.073)	(0.049)
High-Low diff.	-0.081	-0.125	0.073	-0.109	0.014	0.022	0.006
P-value	0.321	0.117	0.352	0.171	0.889	0.794	0.942
Observations	313	372	372	372	262	262	372
R-squared	0.946	0.937	0.938	0.937	0.948	0.947	0.937

Robust standard errors (clustered at the council level) in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Sample is restricted to the set of cities that ever experience a close election between a modal and a non-modal candidate (i.e. an election that was decided by less than 7.1 percent). A modal candidate is a candidate whose ethnicity matches the city's modal ethnicity. Each regression includes city and year fixed effects. The non-modal wins indicator is also interacted with margin of victory. "Public goods" is simply total expenditures minus expenditures on "government administration" and debt repayment. The "Public goods" category therefore includes all spending on roads, parks, police protection, sewerage, public transportation, etc. "High-low diff." measures the difference between the "Treat X High" and "Treat X Low" coefficients. The p-value reported in the next row tests the statistical significance of the difference in these coefficients.

Table 6: The impact of a group-specific victory on log public good spending per capita

	(1) Asian	(2) Black	(3) Hispanic	(4) White
Group wins	-0.034	0.081	0.036	-0.058
	(0.075)	(0.088)	(0.047)	(0.044)
Observations	110	128	354	377
R-squared	0.974	0.958	0.915	0.935

Robust standard errors (clustered at the council level) in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Column 1 is restricted to the set of cities that ever experience a close election between and an Asian and non-Asian candidate. Column 2 is restricted to the set of cities that ever experience a close election between and a Black and non-Black candidate. Column 3 is restricted to the set of cities that ever experience a close election between and a Hispanic and non-Hispanic candidate. Column 4 is restricted to the set of cities that ever experience a close election between and a White and non-White candidate. Each regression includes city and year fixed effects. Close elections are defined as elections that were decided by a margin of less than 7.1 percent. All specifications include year and city fixed effects. The "Group wins" indicator is also interacted with margin of victory.

Table 7: The impact of a non-modal win on future electoral outcomes

	(1)	(2)	(3)	(4)
	Vote share	Vote share	Num. incumbents seeking re-election	Num. candidates
Non-modal wins	-0.037*	-0.044**	0.295	0.065
	(0.019)	(0.018)	(0.751)	(0.258)
Candidate fixed effects	Y	N		
FE's for num. of candidates in race	Y	Y		
Observations	502	276	276	276
R-squared	0.963	0.849	0.041	0.054

Robust standard errors (clustered at the council level) in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Sample is based on the set of candidates that served on a council following a close election between a modal and non-modal candidate (i.e. an election that was decided by less that 7.1 percent). A modal candidate is a candidate whose ethnicity matches the city's modal ethnicity. Each regression includes city and year fixed effects and the non-modal wins indicator is interacted with margin of victory, but these coefficients are not displayed.