Does Transparency Lead to Pay Compression?

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Abstract

This paper asks whether pay disclosure changes wage setting at the top of the public sector distribution. I examine a 2010 California mandate that required municipal salaries to be posted online. Among top managers, new disclosure led to approximately 7 percent average compensation declines, and a 75 percent increase in their quit rate. The wage cuts were largely nominal. Wage cuts were larger in cities with higher initial compensation, but not in cities where compensation was initially out of line with (measured) fundamentals. The response is more consistent with public aversion to high compensation than the effects of increased accountability.

Pay transparency policies are growing in importance. Recent examples include states and cities that have increasingly disclosed worker salaries as part of sunshine initiatives, a U.S. presidential memorandum that recommends requiring federal contractors to submit summary data on employee compensation by race and sex, and a proposed rule that would require companies to compare CEO pay with the pay of the median worker. In an extreme example, Norway now publishes all of its residents' tax returns online.

While the literature has made progress in understanding how preferences about inequality and redistribution are shaped by available information (e.g. Bartels 2005; Card et al. 2012; Cruces, Perez-Truglia, and Tetaz 2013; Kuziemko et al. 2013; Karadja, Mollerstrom, and Seim 2014), little is known about whether there are "real" effects of more information on pay. Two reasons why salary transparency might change the compensation structure in organizations, particularly at the top of the distribution, are (1) greater accountability and (2) public aversion to perceived excessive salaries, without accounting for market factors that determine wages.

Advocates for transparency polices have stressed that increased disclosure should lead to increased accountability.[1] Given new information, the public might hold elected officials more accountable for gaps between pay and productivity.[2] Increased accountability could result in lower manager compensation if capture and managerial

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[1] See, for example, *The Economist*, "Sunshine or colonoscopy?" November 19, 2011. http://www.economist.com/node/21538774

[2] Greenstone, Oyer, and Vissing-Jorgenson (2006) find evidence that mandated disclosure requirements in the 1964 Securities Act led managers to focus more on maximizing shareholder value.

power are restrained.[3] Consistent with this explanation, using survey data from across 175 countries, Djankov et. al. (2010) find that public disclosure of politicians' income is associated with lower perceived corruption and better government.

Transparency may also lead to pay compression if there is public sentiment against high levels of compensation, even if compensation is in line with fundamentals. Such a response would be consistent with inequality aversion on the part of the public (Fehr and Schmidt 1999).[4] There has been speculation in the executive compensation literature that this type of "populist" response to seemingly high levels of compensation has contributed to lower executive pay in publicly traded companies where top salaries are disclosed (Jensen and Murphy 1990), though there is little evidence of this phenomenon.

Estimating the relationship between pay transparency and the wage structure in organizations is challenging, as it requires finding variation in transparency at an organizational level, as well as data on wages. This paper seeks to overcome these difficulties by examining how the 2010 California mandate that required cities to disclose municipal salaries affected the compensation of the Chief Administrative Officer ("city manager") position—typically the highest paid city employee. The research design

[3]See Di Tella and Fisman (2004), Diamond (2013) and Brueckner and Neumark (2014) for evidence of rent extraction in the public sector.

[4] A related mechanism is morale considerations on the part of workers. Card et al. (2012) find that access to information had a negative effect on the job satisfaction of workers who were relatively lower paid within their departments. Being paid below expectations can lead to declines in productivity and product quality (eg. Greenberg 1990; Krueger and Mas 2004; Mas 2006; Mas 2008; Cohn et al. 2014). Employers might internalize these fairness concerns when setting pay, thus compressing wages (Frank 1984, Akerlof and Yellen 1990, and Bartling and von Siemens 2010). Transparency might also reduce gender and race wage gaps by making it easier to compare wages of workers in similar jobs; this was, in fact, the stated motivation behind the 2014 presidential memorandum referenced above (//www.whitehouse.gov/the-press-office/2014/04/08/presidential-memorandum-advancing-pay-equality-through-compensation-data).

exploits the fact that prior to the mandate a subset of cities ("previous disclosure" cities) had already disclosed the salaries of their top managers.[5] Using the Internet *Wayback Machine* and archives of more than three hundred local newspapers, I identify cities where the pay of city managers were already disclosed to the press or on their websites at the time of the mandate.[6] Prior to the mandate, 62 percent of cities already disclosed the salary of the city manager. I compare these cities to other cities where the online mandate represented the first recorded disclosure of city manager salary ("new disclosure" cities). I also make comparisons to wages in Arizona cities, where there were no changes in disclosure policy.

One challenge that arises when studying the effects of disclosure policies is that pre-disclosure information is not typically available. In order to examine pre-mandate trends, I made a public records request to all 482 cities in California for 1999-2012 payroll records and contracts of city managers, and to the California Public Employees' Retirement System (CALPERS) for 2001-2012 earnings records of employees who contributed to CALPERS pensions.[7]

The evidence suggests that maximum compensation is sensitive to increased transparency. Comparing the evolution of wages in cities that previously did and did not disclose salaries, I find that salary disclosure reduced compensation of city managers by an average of approximately 7 percent, largely through nominal wage cuts. These cuts

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[5] This strategy is similar to the one used by Bo, Slemrod, and Thoresen (2014) who study the effects of disclosure on tax avoidance in Norway.

[6] The *Wayback Machine* is a digital archive of websites (web.archive.org/).

[7] As discussed below, cities are now obligated to comply with public records requests whereas in the past they were not. I use data for the 2001-2012 period because only 35 percent of cities had data as far back as 2000, and even fewer for 1999 due to Y2K IT conversions.

occurred both in cases where managers remained in their position, and in cases where managers changed. Wage cuts were substantially larger in cities where compensation was initially higher, particularly cities where the city managers were paid more than $200,000 annually prior to disclosure (the mean salary was $193,000 in 2009). There was no relative decline in the 50th and 75th percentiles of the city wage distributions, on average, implying that reductions at the top of the wage distribution reflect pay compression.

Importantly, this wage effect does not appear to be the result of citywide furloughs or budget cuts following the 2007 recession. I find no evidence of differential changes between new and previous disclosure cities in average earnings of municipal employees excluding the city manager, number of municipal employees, or the average income levels of residents. Additionally, the wage reductions came after furloughs peaked in California. The estimates are also robust to inclusion of a rich set of city characteristics interacted by year, within-county comparisons, city-specific time trends, and comparisons with Arizona.

To assess whether these wage cuts are the result of greater accountability, I test whether the reduction in salaries is larger in cities with greater positive residuals in a pre-disclosure cross-sectional regression of log salary on city characteristics. I find that that this is *not* the case: wages fall as much in positive and negative residual cities on average. This finding suggests that wage cuts were not the result of the discovery of managers who exploited secrecy to inflate their wages, in general.

I also examine the effects of disclosure on manager turnover. I document that the policy was associated with a 75 percent increase in voluntary manager separations. The

evidence points to either managers who had limited surplus in their positions, or that they responded badly to nominal pay cuts. These estimates suggest that a possible drawback to transparency policies in the public sector is that wages fall to the point that cities cannot retain incumbent managers.

Overall, the evidence is more consistent with the "populist" explanation of an aversion to large salaries. Disclosure pressured cities to lower salaries, particularly large salaries, but not salaries that were higher than predicted by fundamentals. The high levels of voluntary separation following disclosure provide further support for this conclusion, since in these cases it was not possible to renegotiate compensation to transfer surplus to taxpayers.

In addition to shedding light on the role of information on the wage structure, this paper contributes to the literature on the causes and effects of government transparency (e.g. Djankov et al. 2010) and to the related literature on the relationship between the press and political accountability. The findings in this paper suggest that exposure to media had a significant effect in restraining wages at the top of the wage distribution. This finding is consistent with Snyder and Stromberg (2010) who find a relationship between press coverage and politicians' actions and policies.[8]

I also find robust evidence that the wage effect is entirely concentrated on male managers and I discuss possible reasons for this asymmetry.

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[8] On the role of the media and information on electoral outcomes and political accountability, see also Mondak (1995), Besley, Burgess, and, Prat (2002), Arnold (2004), Reinikka and Svensson (2004), Besley and Pratt (2006), Brunetti and Weder (2003), Gentzkow (2006), DellaVigna and Kaplan (2007), Ferraz and Finan (2008), Gentzkow, Shapiro and Sinkinson (2011), and Pande (2011).

Section I. Pay Disclosure in California

In July 2010, an investigative report by the *Los Angeles Times* revealed that the city manager of Bell, California (population 35,000) was being paid close to $800,000 annually.[9] While there were no legal limits on city manager compensation in California (though in this case there turned out to be illegally written employment contracts), this compensation was considered by many to be excessive. As a direct consequence of this scandal, in August, 2010 the State Controller John Chiang initiated the "Local Government Compensation Reporting Program." This program required salary information for elected officials and other municipal and county employees to be clearly stated on city websites, and that the information be transmitted to the State Controller and posted on its website (gcc.sco.ca.gov). The website went online in October 2010 with almost universal compliance. By the end of 2012 the website had almost 6 million online views.[10,11]

Prior to the 2010 mandate there was one notable event relating to transparency. In August 2007 the California Supreme Court issued two rulings requiring disclosure of individual public employee names, salaries and other employment information, but only when requested by the public.[12] Before this ruling, cities were under no obligation to disclose their compensation of city employees by name to the public or to the press. The

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[9] "Is a Manager Worth $800,000," Jeff Gottlieb and Ruben Vives, *Los Angeles Times*, July 15, 2010.

[10] http://www.acwa.com/news/state-legislation/state-controller's-public-pay-website-gets-overhaul

[11] The Bell scandal led to increased attention and media interest in public sector salaries, and a number of newspapers requested salaries of municipal employees from cities in their markets. As a result, the treatment is broader than the State Controller mandate and includes all post-Bell media exposure.

[12] "State Supreme Court: Local Government Salaries are Public Record," Howard Mintz, *San Jose Mercury News*, August 28, 2007.

ruling led to several news outlets obtaining and reporting compensation information, notably a consortium of San Francisco Bay Area newspapers that published an online database in 2009 that included employee compensation for fifty cities and other public entities.[13]

In principle, it would also be interesting to study the effects of the 2007 ruling; however, I focus on the 2010 mandate because it does not appear that these rulings led to significant new disclosure, and certainly not close to universal disclosure. Using the data collection procedure described below, I find that 38 percent of cities had not disclosed city manager salaries before 2010. Even the 2009 database mentioned above led to *new* disclosure of city manager compensation for only ten cities. Figure 1 plots mentions of "city manager" and "salary" in California by year as a share of all articles published by California newspapers in NewsLibrary.com, an online newspaper archive.[14] There is little visual evidence of increased coverage of city manager salaries following the 2007 Supreme Court ruling. There is a small increase in mentions in 2009, and a larger increase in 2010.[15] Thus, the distinction between passive disclosure (no obligation to post salaries) versus active disclosure (obligation to post salaries) appears to be important.

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[13] As discussed in the Data Appendix, I will drop from the analysis cities for which first wage disclosure occurred in 2009, as there is some ambiguity about whether they are treated. The estimates are robust to inclusion of these cities.

[14] Specifically, for every year I search for keywords ("City Manager" OR "City Administrator" OR "Town Manager") in the first paragraph of the article and salary anywhere else in the text. I divide the resulting number of search results by the total number of articles in that year for the California press in the archive and then divide this ratio by its value in 2000. Restricting the first set of key words to appear in the first paragraph reduces noise. Taking a random sample of 40 search results in 2006 I find that the restricted search has 27 relevant search results while the unrestricted search that allows the terms ("City Manager" OR "City Administrator" OR "Town Manager") to appear anywhere in the text has only 10 relevant results.

[15] It is also possible that the disclosure of Bell salaries had an additional effect in turning the public's attention towards city compensation.

One reason why the 2007 ruling engendered a limited response is likely that cities found ways to delay fulfilling requests. For example, one of the *Los Angeles Times* reporters who broke the Pulitzer Prize winning story on compensation in Bell, CA describes the process by which they obtained the information:

> "Literally every day, I'm calling the city clerk," Gottlieb said. "I'm telling her, 'Listen, are we getting the documents? I really don't want to sue you, but we will, and when we go to court, and we win, because we will, we'll ask the judge to make you pay our legal bills, because that's what the [public records] statute says.'"

> The city manager, Robert Rizzo, finally relented, but they had to meet him at a conference room near a city park for kids. That was weird enough — but nine city officials and lawyers showed up.[16]

Given these obstacles, the typical resident (or even reporter without access to a legal department) would likely have had a difficult time obtaining compensation information if city officials were inclined to prevent disclosure. There may also have been cities that would not obstruct disclosure of this information but may not have been covered regularly by the press. I will show in Section III that cities without disclosure at the time of the mandate had significantly less coverage on all topics, suggesting that one factor in prior non-disclosure is lack of press coverage.

Section II. Municipal Governance and Compensation

Most California cities have a "Council-Manager" form of governance. Under this arrangement, the city council, which is elected by voters, is responsible for setting broad policies. The city council appoints a professional manager who is the head of administration. City managers are typically in charge of day-to-day operations in the city, as well as developing a budget, promoting economic development, collective bargaining,

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[16] http://www.npr.org/templates/story/story.php?storyId=130108851

managing staff, and hiring. [17] City managers are usually the highest paid municipal employees. Their compensation is negotiated with the city council, often in closed session. Contract terms vary from setting pay annually, to contracts that specify compensation over two or three year terms. Based on a random draw of ten contracts obtained through public records requests, the average term over which compensation is pre-specified is two years. Pay raises can be left to the discretion of the city council or can be linked to cost of living indices, compensation of other city employees (e.g. at least 10 percent above the next highest paid employee) or city managers in other cities. City managers are at-will employees and can be fired at any time, though contracts may specify severance payment depending on the term and circumstances of separation.

There is no limit to how much city managers in California can earn. City manager compensation is driven at least in part by competitive forces, as shown by Enikolopov (2012) who provides evidence that city manager compensation in the US is sensitive to manager performance. There may also exist institutional factors that decouple compensation from market forces. Even when the city council has discretion to set a salary, their incentives may not align with voters, particularly if salaries are not public. In the most extreme cases, the city council and city managers may collude to boost each others' compensation—this is what appears to have happened in the City of Bell. [18]

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[17] An alternative form of governance is Mayor-Council where an elected mayor serves as the city's chief administrative officer. These cities often have professional city managers that report directly to the mayor. See Levin and Tadelis (2010) and Enikolopov (2012) for additional background on forms of city governance and the role of the city manager.

[18] California has two types of cities: general law and charter cities. In general law the compensation of city council is regulated while in charter cities council compensation is unregulated. City type is determined by referenda.

Section III. Data

The data for this project comes from multiple sources, including from public records act requests and newly digitized archival documents. I describe them briefly here. Additional details on data and sample selection are available in the Data Appendix.[19]

Compensation

City manager compensation for 2000-2012 was obtained from public records act requests for payroll records and contracts of all 482 California cities.[20] Since there was not universal compliance to these requests, these records are supplemented with data obtained through a public records act request to CALPERS for earnings histories of all municipal employees who were employed during 2001-2012 and contributed to CALPERS pensions.[21] Whenever possible I construct longitudinal histories of city manager compensation using these sources. When there are gaps, I use publicly available data in the State Controller website for years 2009-2012, salaries found from *Wayback Machine* historical snapshots of city websites that reported city manager salary, as well as salaries found in newspaper archives. This data collection effort resulted in compensation histories for the city manager position for 76, 92 and 98 percent of cities for 2001-2012,

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[19] All data collection relied heavily from the help of the research assistants acknowledged above. Implicit in the discussion below, research assistants did much of the arduous data collection work.

[20] This public records request is only possible because of the Supreme Court ruling that city employee wages are in the public domain.

[21] CALPERS has earnings records for employees who contribute to pension benefits through this system. Enrollment in CALPERS depends on city and occupation. In 2009, 38 percent of California municipal employees were enrolled in CALPERS and 90 percent of cities had at least one employee enrolled.

2005-2012, and 2009-2012 respectively.[22] The Arizona League of Cities provided hardcopies of city manager salary histories for years 2004-2012 that I digitized for this study. In what follows, city manager compensation will refer to the compensation for the city manager position for a given city. For example, if there are different managers in consecutive years, the change in compensation is the difference in their earnings.

I construct measures of compensation for other municipal employees using CALPERS data for years 2001-2012 and public data from the Local Government Compensation Reporting Program for years 2009-2012. I compute average earnings (excluding the city manager), median earnings, and 75th percentile earnings of CALPERS enrolled employees over years 2001-2012. Because CALPERS enrollees are only approximately 38 percent of all municipal employees, to assess robustness I also construct these measures using the universe of municipal employees for 2009-2012 using the Local Government Compensation Reporting Program data, which includes one complete year of pre-mandate data.

City Characteristics

Point-in-time city characteristics are five-year averages from the 2009 American Community Survey and the 2007 Census of Governments. The controls utilized in the main analysis are log population, log median housing values, log average household income, percent of residents who are Hispanic, percent of residents who are black,

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[22] The reasons given for cities not providing complete information include the time period requested exceeding the record retention policy, data being contained on old IT systems, lack of staff to conduct the search, as well as non-response to my inquiry. For all requests I compensated the cities when necessary for the costs of retrieving the records. Cities varied from not charging anything to charging upwards of $500 for the information.

percent of homes that are renter occupied, percent of population (25+) with at most a high school degree, percent of employed working in construction, and number of full-time equivalent city government workers.

Because many of the cities in the sample are small, I cannot use Census data to construct time-varying city characteristics.[23]   I derive average resident income by year for the period 2004-2012 using annual zip-code level data on resident income from the Statistics of Income program of the Internal Revenue Service (SOI/IRS) that are then aggregated to the city level using zip code population weights.

Separation, City Council and Election Data

Data on manager turnover was obtained by digitizing the *California Roster*, a directory of municipal elected officials and high-level managers published by the California Secretary of State supplemented with online searches of city manager biographies since the roster is not always up to date. These data span the years 2005-2013. To identify quits versus other reasons for separation I conducted an online search for every record of separation for press reports giving background information on the reasons for the change. I code a separation as a quit if the manager is reported to be leaving for another position, or if he or she is reported to be voluntarily retiring (there is no mandatory retirement age).[24] I read the articles to verify whether the manager was voluntarily resigning versus being pushed out, but it remains possible that some of the coded resignations were actually involuntary. Reassuringly, the share of all separations

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[23] In the Census ACS data small city characteristics are only available as five year averages.
[24] Retirement was determined by checking if the manager subsequently collected pension in the California Public Employees' Retirement System, which is in the public record.

coded as quits for city managers (67 percent in 2007) is close to the share in the Panel Survey of Income Dynamics (68 percent in 2007), though obviously these are different populations. For every separation, I also code whether the new city manager had previous experience as a city manager and whether the replacement is an internal hire.

I coded managers' gender by comparing their first names to common female and male names in the Social Security Administration names database. Where there was ambiguity, I conducted an online search of the managers to identify their gender.

Data on City Council composition comes from digitized copies of the *California Roster* for the years 2003-2012.

Disclosure Data

A key variable in this study is whether city salaries were in the public domain prior to the 2010 mandate. To obtain this information, I consult historical snapshots of each city's website on the *Wayback Machine* and investigate whether the city manager's salary was posted on the city's website as of the end of 2008. Specifically, for every California city, I searched the last available snapshot in 2008 in the Human Resources Department, Finance Department, and the Administration directories of the city website searching for the city manager salary.[25] When no salary was found in any of these directories, I recorded the city as not posting the city manager salary online.

I also searched California newspapers over the period 2003-2009 for city manager salary disclosure. The primary archive used is NewsLibrary.com, which at the time of search had articles and transcripts for 338 California newspapers and TV stations. For

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[25] When available, this information is usually posted in the Human Resources department directory.

every city in California I searched for articles with the name of the city in the lead paragraph or title of the article and (("City Manager" OR "City Administrator" OR "Town Manager" OR "Town Administrator") AND ("Salary")) anywhere in the text over January 1, 2003 to December 31, 2009.[26] NewsLibrary.com displays an excerpt of the first paragraph in the search results. I first verified whether the salary was available there. If not, I assessed whether the article appeared relevant to the question and, if so, I read the entire article to look for this information.[27]

I coded a city as having prior city manager pay disclosure if either they posted the data on their website, or if the press reported city manager salary sometime in the 2003-2009 period. These criteria result in 178 new disclosure cities and 296 previous disclosure cities.[28] Three appealing aspects of this definition are that a resident who wished to find city manager salary could do so if the city had disclosed this information to the press at some point in the recent past, it reflects the dichotomous nature of disclosure, and past disclosure to the press likely signals that the city has a stance towards transparency. To the extent that I missed posted salary information or if the information was disclosed by other means (not on websites or newspapers), the operating assumption is that in such cases the information would have been relatively more difficult to access than in cities coded as disclosing. My measure of disclosure is therefore best thought of as an index that is related to previous transparency. If cities are misclassified as not

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[26] This window was chosen to balance capturing news coverage over a recent period and feasibility, as reviewing newspaper articles for mentions of city manager salaries is a time intensive process.

[27] The *Los Angeles Times* is not archived in NewsLibrary.com and I did a separate search through their search engine using the same methodology.

[28] These figures are before excluding cities for which first disclosure was in 2009. See discussion below and in the Data Appendix.

disclosing, this should lead to attenuation bias in the estimates. While the focus of the analysis is on new disclosure based on this definition, I will show that there are also wage effects, though smaller in magnitude, when the criteria for new disclosure is whether the city had not previously posted wages on their website, ignoring the role of the press.[29]

In Table 1 I examine the relationship between this measure of new disclosure and media coverage as well as other city characteristics. In column (1) I relate an indicator for new disclosure on the log total number of articles that mention the city between 2003-2008, log city population, and the percent of residents with no more than a high school degree. I include the latter two variables since exploratory analyses revealed that they are highly predictive of whether there is new disclosure. Note that any relationship found with the log total number of articles will not be mechanical since the explanatory variable includes coverage on any topic (e.g. high school sports team scores) and city manager/salary mentions are a miniscule share of all articles that refer to the city (averaging 0.14 percent of all articles). The coefficient on the log total number of articles is negative and significant. The coefficient implies that a 10 percent increase in prior coverage is associated with a 0.7 percentage point decrease in the probability of new disclosure, or a 2 percent decrease from a base of 37 percent. This estimate suggests that the disclosure variable is partially related to media coverage in the city since cities with previous disclosure have more press coverage on any topic, even conditional on population and other city characteristics.

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[29] In what follows, I drop the City of Bell from all of the analyses so as to not confound the effect of disclosure with the LA Times report and subsequent investigation into this city. See Data Appendix for sample selection criteria.

The estimates on the other characteristics in column (1) imply that smaller cities and cities with less educated residents are more likely to have new disclosure. While these estimates should not be interpreted causally, the signs of the relationships are sensible. A larger city will have more resources available to make information available as well as more interest groups who demand it. Likewise, there might be more demand for salary information in places where residents are more educated. In columns (2) and (3) I add additional characteristics namely the log of average household income, percent of population that is black and percent of population that is Hispanic. Conditional on the first three variables, these characteristics do not significantly predict new disclosure and their addition does not change the relationships on the three variables in model (1), which I have found to be robust.

Appendix Table A1 reports additional summary statistics organized by whether cities had new disclosure. As already seen in Table 1, there are clear differences between these sets of cities. These comparisons suggest that it is important to ensure robustness by controlling for a rich set of city characteristics to verify whether the estimates pick up differential trends in characteristics rather than the effects of disclosure. I show that the estimated changes in compensation are unaffected by inclusion of these covariates interacted by year.

Table A1 also presents means of city compensation. City managers in new disclosure cities earned 21 percent less in 2009 than managers in previous disclosure cities, on average. This gap is largely accounted for by differences in population and

average income between these sets of cities.[30] Both sets of California cities have higher manager earnings than Arizona. Between 2009 and 2012 real city manager compensation fell by 11.6 percent in new disclosure cities as compared to 4.6 percent in previous disclosure California cities and 4.1 percent in Arizona cities. In the next section I will estimate these changes more systematically, accounting for trends, city characteristics, and regional shocks.

In column (1) of Table 2 I probe how disclosure affected news reports in these two sets of cities. The column presents estimates of search results for mentions of city manager and salary by new and previous disclosure cities, before and after the mandate period.[31] For this search I used a web scraper to count the number of search results with the above criteria, and I did not manually verify whether the salary was actually reported. Therefore, there are some false positives in the data using the search terms. I estimate a negative binomial model due to the low counts and many zeros (particularly for new disclosure cities). I control for the log of the total number of articles written about the city to ensure that any changes in counts are not driven by a change in overall reporting. The estimates confirm that prior to 2010 new disclosure cities have substantially fewer search results, with 63 percent (=(exp(-1)-1)*100) fewer results in new disclosure cities relative to prior disclosure cities. This gap in the pre-mandate period is mechanical based on how I constructed the new disclosure variable. The gap in search results closes considerably, however, after the mandate in 2010, with the new disclosure gap declining

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[30] Controlling for log population and log mean household income alone reduces this gap to four percent.
[31] The underlying data are city by year observations of search counts for ("YYY" AND (("City Manager" OR "City Administrator" OR "Town Manager" OR "Town Administrator") AND ("Salary")), where "YYY" is the city name and the first two search terms are restricted to appear in the first paragraph of the article.

to 9 percent (=(exp(-1+0.91)-1)*100) fewer search results. This change, which is not mechanical, is significant at conventional levels. These estimates confirm that the mandate led to considerably more press coverage on compensation for cities that had previously not been mentioned in the press. These estimates, however, offer only a partial view of how the mandate affected the diffusion of information as they do not account for the people who accessed the salaries directly online.

Section V. Disclosure and City Manager Outcomes

Earnings

Figure 2a presents visual evidence on the evolution of city manager salaries. The figure presents *nominal* compensation of city managers in new disclosure and previous disclosure cities for years 2001-2012. Specifically, for both sets of cities I regress log nominal city manager compensation on year dummies, excluding the 2009 dummy, and city fixed-effects.[32] The figure plots the estimated coefficients on the year dummies for new and previous disclosure cities normalizing 2009 to 0. I also estimate and plot the coefficients from the fitted model for new disclosure cities where the sample has been weighted to match a set of city characteristics (those listed in Section III) of the previous disclosure sample. I use DiNardo, Fortin, and Lemieux (1996) (DFL) weights.

Prior to the mandate the growth rates of city manager compensation in previous and new disclosure cities were close, with a slightly higher growth rate of compensation in new disclosure cities in the mid-2000s. In 2010 nominal wage growth plateaued for both sets of cities, but in 2011-2012 nominal compensation declined sharply in new

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[32] City fixed-effects are necessary since the panel of cities is unbalanced.

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disclosure cities while for previous disclosure cities wage growth remained stable in 2011, and then rose slightly in 2012. By 2012 the difference in compensation relative to 2009 was close to 7 percent. Reweighting the new disclosure sample yields an almost identical pattern.

Figure 2b plots the difference in the (weighted) series with 95 percent confidence intervals. The difference in compensation relative to 2009 in years prior to 2011 is never significant, but the declines in new disclosure relative to prior disclosure cities in 2011 and 2012 are significant.

The observed patterns in these figures are consistent with the mandate lowering wages in 2011 and 2012. With respect to timing of the estimated effect, as previously discussed, city manager contracts typically pre-specify compensation for a period of one or two years. We would therefore expect to see the effect grow over the first two years following the mandate in new disclosure cities as new contracts are negotiated. It is unclear whether we should expect to see a divergence in compensation as early as 2010. While some cities may have reacted quickly, it is likely that for most cities the changes would have taken more than a few months to affect manager compensation.

Figure 3 plots both of the raw series against Arizona. One added value of this comparison is that Arizona was completely unaffected by the mandate, whereas previous disclosure cities in California may still be treated by the mandate to some extent. From the previous figures we cannot tell if the flattening of nominal wages in the previous disclosure cities in California is due to the aftermath of the recession or the effects of disclosure. Figure 3 shows that the growth rates of city manager compensation in Arizona

and previous disclosure cities in California line up well, both in the pre- and post-mandate periods. It appears that the time pattern of wages in the previous disclosure cities in California is consistent with the state of the economy, and there is no evidence that they are experiencing a partial treatment from disclosure.

I now turn towards estimating the average effect of disclosure in 2010, 2011, and 2012 relative to the pre-disclosure period. Table 3 reports estimates from variants of the following base specification:

$$(1) \quad \ln(y_{it}) = \alpha_i + \delta_t + \beta_t * X_i + \theta_1 1(t = 2010) * \text{NewDisclosure}_i$$

$$+\theta_2 1(t = 2011) * \text{New Disclosure}_i + \theta_3 1(t = 2012) * \text{NewDisclosure}_i + \varepsilon_{it,}$$

where i denotes city, t denotes year, y_{it} is manager compensation in 2012 dollars, α_i are city fixed-effects, δ_t are year dummies, X_i are time-invariant city characteristics that are allowed to have a different effect in a different year, and New Disclosure$_i$ is an indicator that is equal to 1 if the city previously did not have the city manager salary on its website and not reported in the press. The city characteristic controls are the same as those used in the reweighting in Figure 2. I also consider specifications with the interaction of year and county dummies, so that the effects are identified by within county comparisons, linear trends interacted with city, and manager fixed-effects. Standard errors are clustered on city.

The parameters of interest are the interactions of the 2010-2012 dummies with the New Disclosure dummy. Column (1) includes city and year fixed-effects and the sample is limited to California. The estimated effect of new disclosure on log city manager income is −0.052 (s.e.=0.017) and −0.066 (s.e. = 0.018) in 2011 and 2012 respectively.

The estimates and significance levels are largely invariant with addition of city characteristics interacted by year (column 2), counties interacted by year (column 3), city*linear trends (column 4), and limiting the sample to the 2009-2012 period for which there are almost no missing observations (column 5). The estimated disclosure effect in the specification with city-specific trends is a 7.6 percent decline in wages by 2012. In column (6) I compare California new disclosure cities to all cities in Arizona, which were not treated. The point estimates are -0.072 log points (s.e. = 0.035) in 2011 and -0.094 log points in 2012 (s.e. = 0.036). The larger standard errors are due to the smaller number of Arizona cities. In column (7) I include manager*city fixed-effects. In this specification new disclosure is associated with a 7.1 percent reduction in compensation in 2012 within manager, implying that changes in compensation occur even without manager turnover.

The primary disclosure variable used in this analysis is whether the city had not previously posted salary information online or reported it to the press. In column (8) I define the new disclosure variable to be that the city did not post the information online only (that is, ignoring news reports). I find a similar pattern of estimates, but with smaller magnitudes. New disclosure is associated with 4.2 percent lower wages in 2011 and 4.5 percent lower wages in 2012. These estimates are smaller than when using the stricter criterion of new disclosure where I impose that they do not have news coverage for compensation, suggesting that such news coverage was putting downward pressure on manager wages in previous disclosure cities prior to the mandate.

To examine whether the changes at the top of the city distribution represent compression, columns (2) and (3) of Table 2 report estimates of the base model (column (2) of Table 3) using the log of the 50th and 75th percentiles of the city wage distributions of CALPERS enrolled employees. There is no discernable relative decline in these percentiles. For 2012, the point estimate on the disclosure effect is -0.009 for the log of the 50th percentile of the CALPERS city distribution (s.e. = 0.016) and -0.005 for the log of the 75th percentile (s.e.=0.011). Figures 4a and 4b show the log of these percentiles by year for new and previous disclosure cities, normalized to 2009 = 0. These figures provide visual confirmation that the growth of the 50th and 75th income percentiles of CALPERS enrolled employees are quite similar between these two groups of cities.

One concern with this analysis is that CALPERS enrolled employees are only a subset of all municipal employees. I address this issue by estimating the same models for 2009-2012 (all years with data available) using compensation measures derived from the universe of employees from the Local Government Compensation Reporting Program. The estimates, which are presented in Appendix Table A2, also show no relative declines in the lower percentiles of the wage distribution.

Relation Between Cuts and Initial Compensation

Figure 5a shows the relationship between compensation in 2009 and the change in compensation from 2009-2012 for new disclosure cities using local linear regression. The figure reveals that it is cities with larger initial levels of compensation that experience cuts. There is no statistically significant decline in compensation below $200,000, but

there are marked declines in compensation for initial compensation levels greater than $200,000.[33] Figure 5b shows the same relationship for previous disclosure cities where we do not observe this pattern. These figures provide additional evidence that large salaries in particular are sensitive to transparency.

Another way to assess the magnitude of this relationship is that in new disclosure cities that paid their city managers more than $200,000 per year in 2009, 40 percent paid their city managers less than $200,000 in 2012. By contrast, in previous disclosure cities where 2009 compensation was at least $200,000, only 10 percent paid their city managers less than $200,000 in 2012. The difference between these two proportions (40 percent vs. 10 percent) is significant (p-value = 0.00).

Budget Cuts and Furloughs

A possible concern in interpreting the estimates is that rather than estimating the effect of new information, perhaps we are seeing a residual effect of the 2007 recession that differentially affected the finances of new disclosure cities relative to other cities in California and Arizona. This is unlikely to be the explanation for several reasons.

First, the point estimates barely change when we control for rich city characteristics interacted by year; if anything the new disclosure effect is larger. If the wage effect were the result of the recession we would expect to see the estimates become smaller when controlling for characteristics that are correlated with financial stress, such as average household income and the percent of employment in the construction sector.

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[33] The sample becomes sparse above earnings levels of $250,000, resulting in large standard errors, however, the general pattern of declining wages remains.

Second, if the negative compensation effects are due to financial stress or furloughs, we would expect to see relative declines in average municipal earnings in new disclosure cities. I find no evidence of this. Column (4) of Table 2 present the baseline model with log average municipal earnings excluding the city manager over 2001-2012, constructed with the CALPERS data. The point estimate of new disclosure relative to previous disclosure cities in 2012 is close to zero and insignificant. Column (5) shows that there is also no significant differential change in the number of CALPERS enrolled workers over the period. Figures 6a and 6b show these outcomes by year. Again, there is no visual evidence of divergence in 2011 or 2012. Appendix Table A2 shows estimates for the same models for years 2009-2012 using the universe of municipal employees. We reach the same conclusion that there is no evidence that cities were differentially trending in these dimensions. I also find no evidence of differential changes in the average income of residents from the IRS/SOI data between these sets of cities. The point estimate for new disclosure relative to previous disclosure in 2012 for the dependent variable of log average income is 0.001 (s.e. = 0.008) in Column (6) of Table 2.

Third, we can also look at the timing of furloughs in California relative to the timing of the estimated mandate effects. Appendix Figure A1 plots press mentions of furloughs for cities in California newspapers from NewsLibrary.com, normalized by the total number of articles in California by year. References to furloughs spike in 2009 and 2010. While there are elevated levels in 2011 and 2012 relative to the pre-recession years, they are only half as large as the mentions in 2009-2010. This pattern is consistent with furlough actions at the state level; California state employees were furloughed

starting in July 2009, and University of California employees beginning September 2009, ending a year later in September 2010. If the wage cuts were due to furloughs, we would expect to see cuts in city manager salaries in 2009 and 2010 as well as 2011 and 2012, which we do not.

Test of the Accountability Mechanism

Cities with higher initial compensation accounted for most of the cuts after disclosure. However, high compensation need not reflect "excess" compensation since cities vary in many dimensions and some cities, for example larger ones, are more difficult to manage and may merit higher salaries. To test whether city manager salaries decreased due to an improved accountability mechanism, I ask whether city managers with larger salaries than predicted by city characteristics, like population and income, should have wages reduced closer to predicted levels. Positive wage residuals in the pre-disclosure period might reflect omitted city and manager characteristics, but they would also represent cases where managers who have captured the pay process used secrecy to elevate their pay. For example, in a regression of log city manager wage on city characteristics (described below), the manager of the City of Bell, where wrongdoing was uncovered, had the largest wage residual of all California cities prior to disclosure.[34] If disclosure has the effect of revealing managers who have captured the pay process, or unwinding managerial power, we would expect to see wage declines for managers who were paid more than what is predicted given the characteristics of where they work.

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[34] As discussed earlier, the City of Bell is dropped from the main analysis.

To implement this test, using only previous disclosure cities, I regress log city manager salary in 2009 on log population, log average household income, and the number of city employees. These three variables alone have significant explanatory power (R-squared = 0.54).[35] I then calculate the log wage residual on the full sample, and I interact a dummy for whether the city's residual is greater than zero with the interaction of the new disclosure and 2010-2012 dummies. To simplify presentation I only report the 2012 interactions since the estimates from the earlier years do not alter the conclusion.[36] Column (1) of Table 4 reports this estimate. The interaction is close to zero and insignificant. Wages declined as much in cities with negative residuals as positive residuals, on average. This finding holds for residuals calculated with more characteristics, including the addition of percent of residents with no post-secondary schooling (column 2) and log median housing values, city housing density and percent of city homes that are renter occupied (column 3). These estimates suggest that the Bell case is the exception rather than the rule. Instead of uncovering and correcting wages of managers who were paid more than predicted by city characteristics, transparency led cities to lower wages whether or not the wages were out of line with fundamentals. This analysis suggests that the mechanism behind the wage effect is not greater accountability.[37,38]

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[35] Estimates from this regression, as well as the two alternative regressions, are available in Appendix Table A3.

[36] The complete estimates are reported in Appendix Table A4.

[37] Another possible effect of disclosure is that it reduced city manager dispersion of wages across all cities, as would be the case if transparency allowed cities to arbitrage, along the lines of Jensen (2007). This is unlikely to be the case since more than half of California cities already disclosed wages providing ample public data on the distribution of wages. Supporting this, I find that both the standard deviation and the

Gender Differences

Table 5 includes interactions of the information shock with a dummy for whether the city manager is female.[39] In the model, the interaction term 1(2012)*New Disclosure represents the log compensation of male managers relative to previous disclosure cities and 2000-2009, and the interaction of 1(2012)*New Disclosure*1(Female) gives the female-male differences in the disclosure effect.[40] The table shows that the entire decline in compensation seen in Table 3 is accounted for by the wage reduction experienced by male managers.[41] Compensation does not decline for female managers, on average, and the male/female differences are significant. It is possible that female managers are in a different type of city than male managers. However, controlling for detailed city characteristics interacted by year and the new disclosure indicator (column 5), which controls for differences in the disclosure response by city characteristic type, does not change the estimates appreciably. In these models I have also included indicators for whether the manager was paid at least $200,000 per year in 2009 interacted by year and the new disclosure indicator (as well as all main effects) to account for the observed

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residual standard deviation (using the characteristics in Table 4) of city manager log wages across all California cities were unchanged between 2009 and 2012.

[38] I have also examined whether post-disclosure wage cuts were larger in cities where members of the city council had longer tenures. I found no significant relationship between the magnitudes of cuts in new disclosure cities and the fraction of the city council that was in office 4 years prior, as well as the average tenure of the council. These estimates are available in Appendix Table A6. There is also no significant relationship between new disclosure and electoral outcomes as measured by the share of the city council of 2009 that was in office in 2012 (Appendix Table A7).

[39] Over 2009-2012, 14 percent of city mangers were female. Gender is the only manager characteristic I coded for this analysis.

[40] I focus on the 2012 effects, but 2011 shows a similar pattern. The complete estimates are in Table A5.

[41] In new disclosure cities female city managers earned approximately 5 percent less than male managers in 2009, but this difference is not significant and the exact magnitude of the gap is sensitive to included controls.

heterogeneity in this dimension. Estimates are also robust to within-county comparisons and inclusion of city*year trends (cols. 2, 4 and 6).

While one can only speculate as to why there is a differential effect of pay secrecy on male and female managers, this evidence is consistent with male managers doing more to take advantage of pay secrecy to inflate their pay than female managers. This might be the case if there are differences between men and women in aggressiveness in bargaining as has been found by some papers in the literature (eg. Stuhlmacher and Walters 1999; Bowles et al., 2007; Babcock et al., 2006; Fortin 2008; Card, Cardoso and Kline 2013; see also Bertrand 2011 for a review of this literature). Another interpretation is that the mandate provided information to female managers about what other city managers earned, thus allowing them negotiate a higher wage. I view this explanation as less plausible since female managers had access to city manager compensation in the cities that already disclosed this information. A third explanation, which cannot be ruled out, is that city councils believed there to be a higher risk of a lawsuit by a female city manager on the basis of discrimination when cutting nominal wages.

Separations

Next I investigate how downward wage adjustment resulting from increased transparency affected manager separations. If managers accrued surplus in their job, we should find that manager separations are relatively insensitive to wage cuts. By contrast, if there are limited rents, we should see a higher sensitivity of voluntary separation with respect to wage.

I divide the sample into two periods, 2007-2009 and 2010-2012, and code a city as having a separation over these periods if there is at least one city manager separation in the interval. This approach was taken because when a manager departs, I observe in the data that there are a number of new managers over a short period of time, likely reflecting the employment of interim managers. By using an outcome variable that indicates whether there was at least one separation over the period, I eliminate double counting within any one period. Collapsing the data to these two periods, I then estimate a linear probability model for separations and quits with indicators for new disclosure, the 2010-2012 ("post") period, and their interaction.

Table 6 reports the estimates from this analysis. Column (1) shows that in the pre-disclosure period overall separations were 20 percentage points lower in new than previous disclosure cities. The separation rate over the next three-year period increased by 12.4 percentage points in new disclosure cities while it decreased by 7.9 percentage points in previous disclosure cities. The difference between these changes is statistically significant at conventional levels. Columns (2) and (3) show that the positive post-mandate*new disclosure interaction is robust to inclusion of city characteristics and county dummies interacted by the post disclosure period.

If managers were separating as a result of wage cuts we should see this effect in the quit margin. Column (4) uses as the dependent variable whether the manager voluntarily separated either by quitting or retiring. The new disclosure by post interaction is estimated as 0.16 (s.e. = 0.05) without controls, and 0.15 (s.e. = 0.06) and 0.13 (s.e. = 0.07) with inclusion of city characteristics*post and county dummies*post respectively.

The mean quit rate for the new disclosure group over the three year period 2007-2009 is 0.2, so new disclosure is associated with approximately a 75 percent increase in the quit rate relative to the counterfactual.

The quit estimates imply a high sensitivity of quits with respect to the wage. The estimated wage effect of new disclosure of 7 percent implies an elasticity of quits with respect to the wage of approximately 11. This magnitude is substantially higher than previous estimates in the literature.[42] However, this elasticity should only be viewed as a simple way to benchmark the magnitude of the response since the steady-state elasticity might be different than the one implied by this policy.

The high sensitivity of quits with respect to the wage cuts imply either that a significant share of managers have wages close to the margin of their next best option or are unhappy at having received a nominal pay cut, as discussed in Bewley (1999).[43,44] The finding is consistent with estimates on public/private wage differentials tend to show that there is a private sector wage premium for more skilled workers that makes it difficult to attract high-quality workers to public sector jobs (Borjas 2002). If this were the case, based on previous studies on the relationship between applicant skill and offered wages in the public sector, such as Krueger (1988) and Dal Bó, Finan and Rossi (2013), we might expect that the wage cuts lead to a less qualified set of applicants for vacant

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[42] The non-experimental elasticities summarized in Manning (2011) never exceed 2.

[43] It should be noted that since the baseline quit rate is relatively low, even with a large increase in quits there are inframarginal cases where wage cuts were accepted. Inclusion of manager*city fixed-effects in specification (1) shows similar magnitudes as the estimates, as reported in Table 3.

[44] It is also possible that managers quit because they didn't want their wage to be made public, but this is unlikely to be the case since many cities, particularly larger cities that pay higher salaries and attract more experienced managers already disclosed pay. A city manager unwilling to take a job where the wage is public would have considerably limited his or her career trajectory.

manager positions.[45] While city manager productivity is difficult to measure, I make some limited progress on this question by coding whether new city manager hires had prior city manager experience, or whether they were an internal hire. The latter is an indication that the city could not find a candidate to fill the vacancy. Using these measures, there is some suggestive evidence that following wage cuts cities downgraded city manager quality. The share of hires with previous city manager experience declined from 34.5 to 31 percent in new disclosure cities following disclosure, while the share increased from 40 to 44 percent in previous disclosure cities over the same period. In new disclosure cities the share of internal hires increased from 40 to 45 percent, while in previous disclosure cities the share declined from 38 to 34 percent.[46]

Conclusion

This paper has presented evidence that making wages public compresses the top of the public sector wage distribution. One possible explanation for this effect is that disclosure makes public officials more accountable, and these officials are subsequently compelled to negotiate more favorable contracts for taxpayers that were previously inflated because of managerial power or skimming. The evidence assembled does not point to this mechanism. Rather, cities cut wages whether or not they appear high as predicted by fundamentals, and the evidence is consistent with city managers already having compressed wages before the mandate. The evidence is more in line with a

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[45] This relationship might be different for elected positions. Fisman et al. (2012) find that higher salaries for European Union legislators led to lower educated elected members of parliament.
[46] These changes in the proportion of managers with previous experience and internal hires between new and previous disclosure cities are not significant at conventional levels.

"populist" response to visibility of top salaries: salaries are cut because they appear excessive, regardless of whether or not they actually are. This conclusion is consistent with the existence of inequality aversion in society that constrains wage setting for highly visible positions.

The paper shows that transparency policies in the public sector have the potential to cause significant turnover. The high responsiveness of quits to the wage cuts may have limited downward pressure on wages since any additional compression would lead to substantial turnover. Absent the high level of quits, wages may have fallen further.[47] It is also possible that where the economy was stronger at the time of the mandate, the wage effect would have been smaller since managers would have had even better outside options.

A question for further research is whether this kind of response extends to the private sector as well, as has been suggested by Jensen and Murphy (1990) and others. There is some evidence, summarized in Kaplan (2012), that the growth of publicly traded CEO pay (where wages are public) has trailed private CEO pay (where wages are not public) and other high-earner professions. The findings here suggest that disclosure could potentially contribute to this tendency. More work could also be done to investigate other effects of pay disclosure, including compression in other parts of the wage distribution, gender and race wage gaps, and whether transparency changes the relative bargaining of workers and employers in wage setting.

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[47] This conclusion is reminiscent of the findings of the local public finance literature and Abramitzky (2008) (in the context of the Israeli Kibbutzim) that mobility limits the ability to redistribute.

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Figure 1. Share of all articles in the California Press referring to "City Manager" and "Salary" (2000=1)
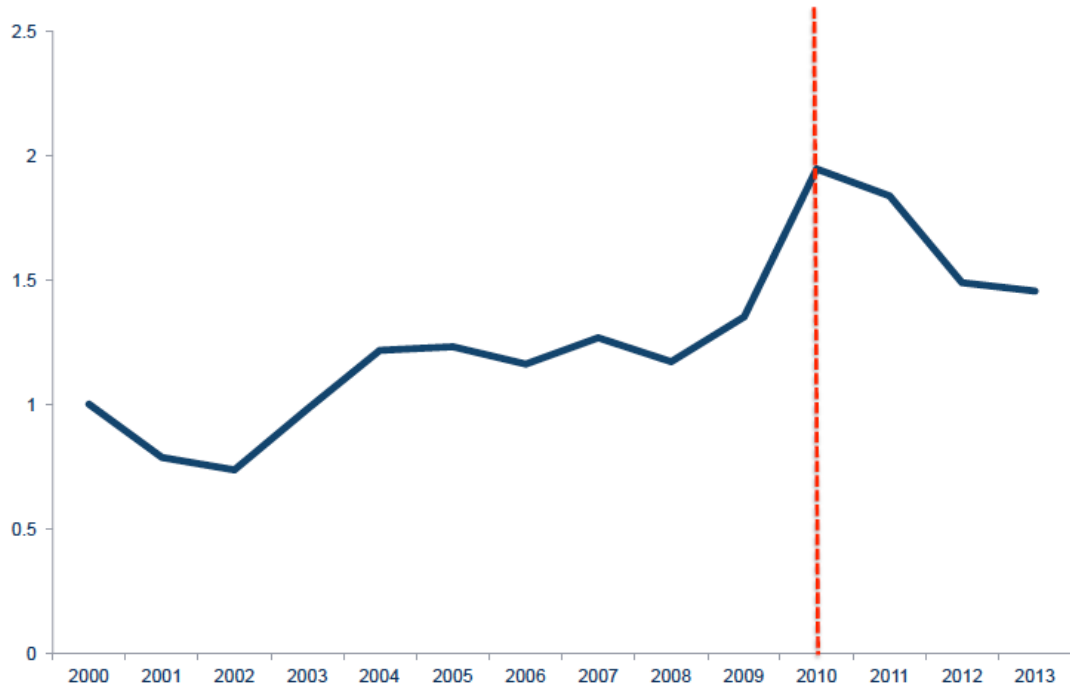
Notes: The data are from searches of California newspapers in NewsLibrary.com. The numerator of the share is the annual number of articles referring to ("City Manager" OR "City Administrator" OR "Town Manager") in the Lead/First Paragraph of the article and "Salary" anywhere in the text. The denominator of the share is the total number of articles in California for that year. The figure plots this ratio as a multiple of the ratio in 2000. The dashed vertical line is the year that the mandate went into effect.

Figure 2a. City Manager Nominal Compensation Growth by Disclosure Status
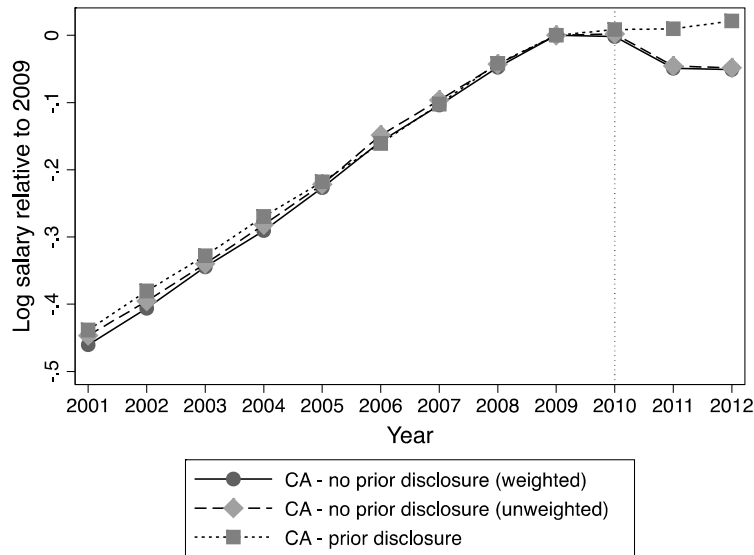
Figure 2b. City Manager Earnings Growth; New Disclosure relative to Prior Disclosure Cities (2009=0)
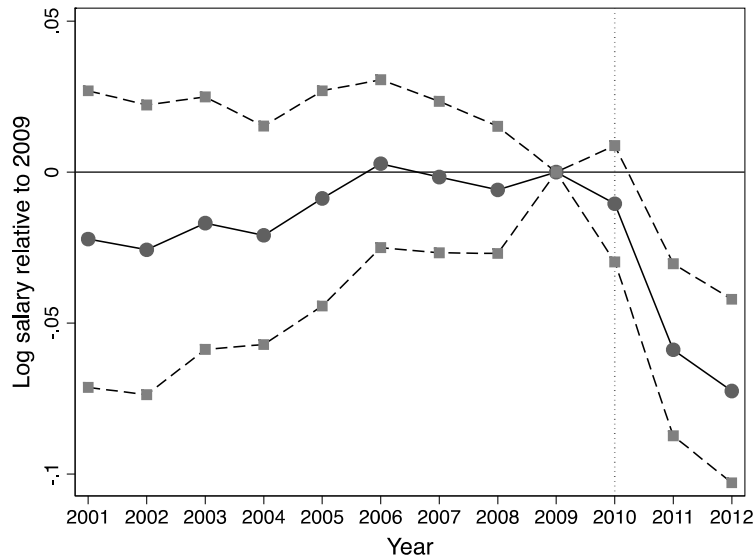
Notes: Figure 2A plots demeaned log nominal compensation of city manager compensation relative to 2009 for the new and prior disclosure cities in California. The weighted series reweights the new disclosure sample to match characteristics in the previous disclosure sample. The characteristics used are log population, log median housing values, log average household income, percent of residents who are Hispanic, percent of residents who are black, percent of homes that are renter occupied, percent of population (25+) with at most a high school degree, percent of employed working in construction, and number of full-time equivalent workers in the city government. The dotted vertical line is the year that the mandate went into effect. Figure 2B plots the difference between the previous disclosure series and the weighted new disclosure series with 95% confidence intervals.

Figure 3. City Manager Earnings Growth; New and Prior Disclosure California Cities relative to Arizona (2009=0)
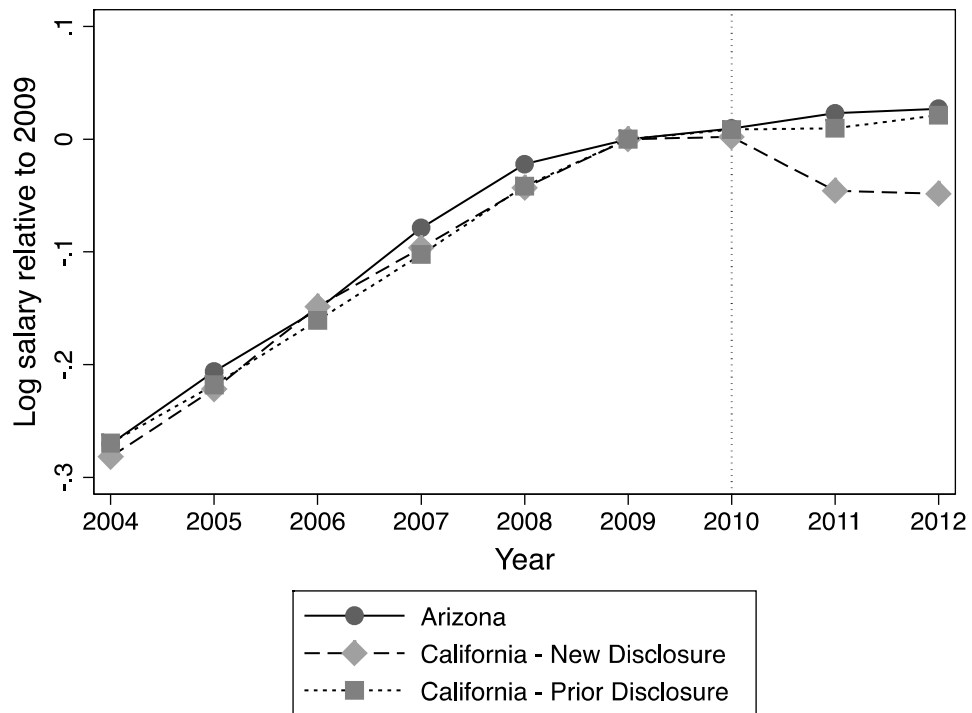
Notes: This figure plots demeaned log nominal compensation of city manager compensation relative to 2009 for Arizona, new disclosure, and prior disclosure cities. The dotted vertical line is the year that the mandate went into effect. The Arizona data are only available beginning in 2004.

Figure 4a. Log 50th percentile of annual earnings of CALPERS enrolled municipal workers (2009=0)
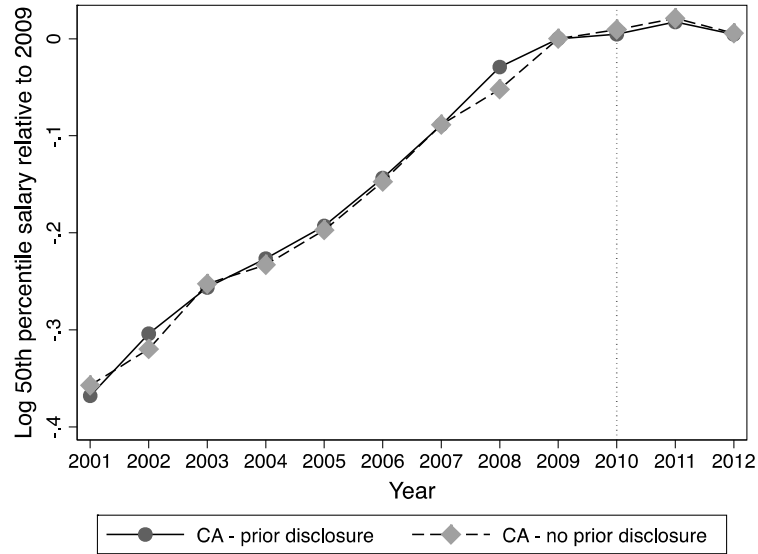
Figure 4b. Log 75th percentile of annual earnings of CALPERS enrolled municipal workers (2009=0)
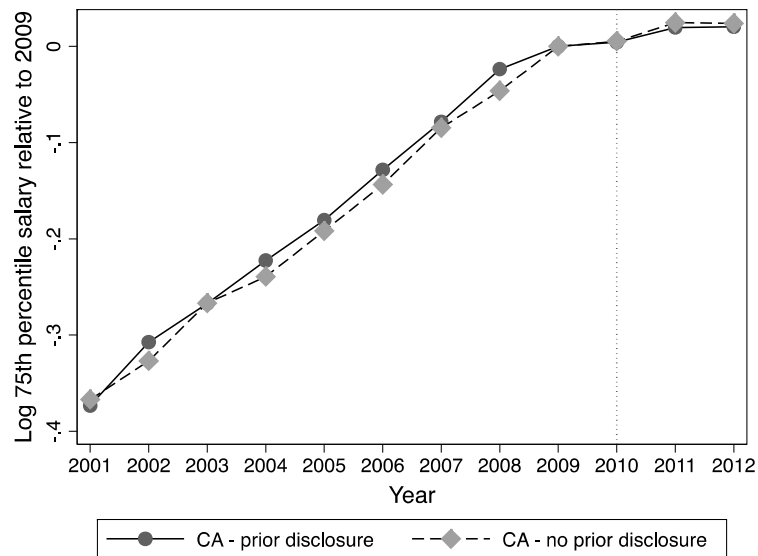
Notes: Figure 4a plots the demeaned log 50th percentile compensation of municipal compensation for workers who contribute to CALPERS pensions relative to 2009. Figure 4b plots the demeaned log 75th percentile compensation of municipal compensation for workers who contribute to CALPERS pensions relative to 2009. The dotted vertical line is the year that the mandate went into effect.

Figure 5a. Relationship Between the 2009-2012 Change in City Manager Compensation
and Initial 2009 Compensation; New Disclosure Cities
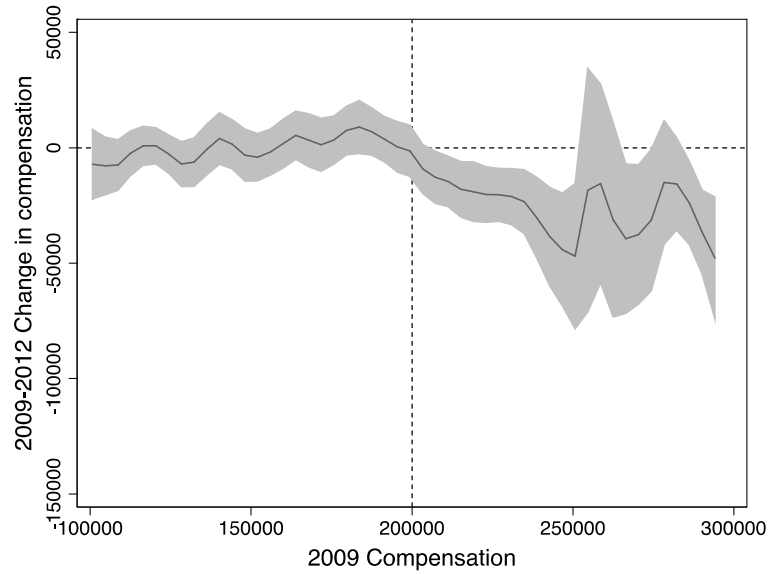
Figure 5b. Relationship Between the 2009-2012 Change in City Manager Compensation
and Initial 2009 Compensation; Previous Disclosure Cities
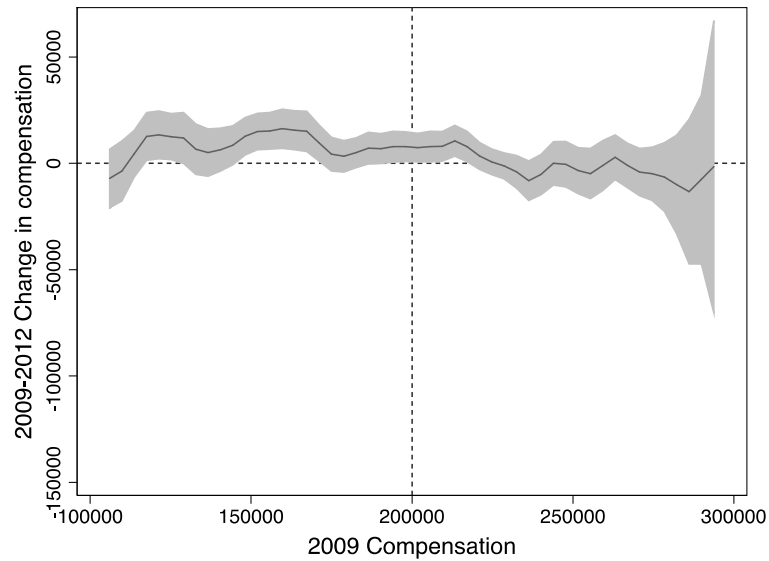
Notes: Figure 5 plots local linear regression estimates of the relationship between the 2009-2012 change in city manager compensation and initial 2009 compensation. The bandwidth is $4000. Shaded areas represents the 95 percent confidence interval.

Figure 6a. Log average earnings of municipal workers enrolled in CALPERS excluding the city manager (2009=0)
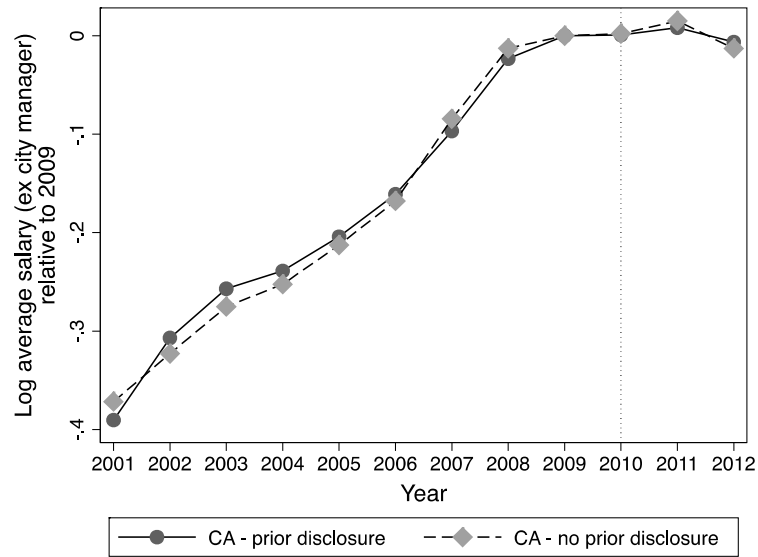
Figure 6b. Log number of municipal workers enrolled in CALPERS excluding the city manager (2009=0)
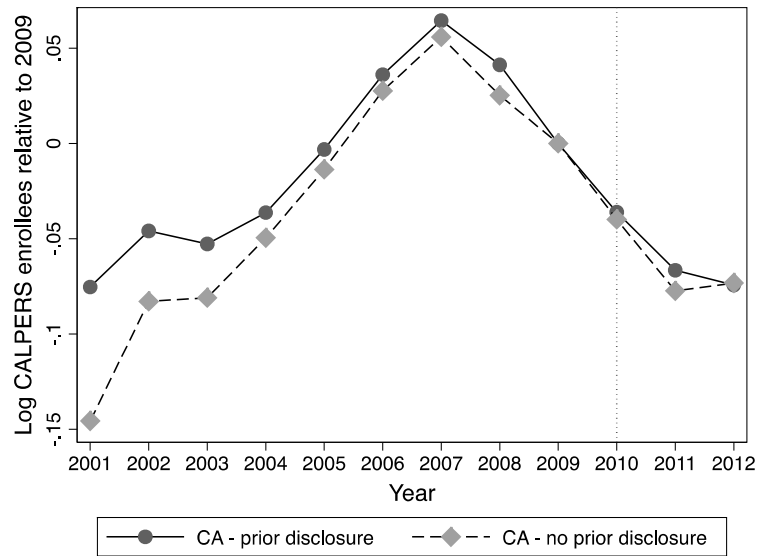
Notes: Figure 5a plots the demeaned log average annual earnings of municipal workers excluding the city manager who contributed to CALPERS pensions relative to 2009. Figure 5b plots the demeaned log number of municipal workers excluding the city manager who contributed to CALPERS pensions relative to 2009. The dotted vertical line is the year that the mandate went into effect.

Table 1. Determinants of New Disclosure

	New Disclosure		
	(1)	(2)	(3)
log(Number of articles 2003-2008)	-0.072	-0.071	-0.072
	(0.020)	(0.020)	(0.020)
% of residents with at most a HS degree	0.006	0.005	0.006
	(0.001)	(0.002)	(0.003)
log(population)	-0.081	-0.084	-0.082
	(0.017)	(0.018)	(0.019)
log(average HH income)		-0.014	0.001
		(0.088)	(0.091)
% of residents who are black		0.004	0.003
		(0.004)	(0.004)
% of residents who are Hispanic			-0.001
			(0.002)
R-squared	0.184	0.184	0.185
Observations	458	457	457

Notes: This table reports linear probability model estimates for dependent variable New Disclosure which is 1 if the city did not have city manager salary reported in the press between 2003-2008 or on its website in 2008. log(Number of articles 2003-2008) is the log of the number of articles in the NewsLibrary.com database on any topic for years 2003-2008 for the city. The unit of analysis is city. Robust standard errors in parentheses.

Table 2. Relation Between New Disclosure and Time Varying City Characteristics

	Negative Binomial Model	OLS				
	City manager mentions	ln(50th percentile city salary)	ln(75th percentile city salary)	ln(Average municipal compensation)	ln(Number of municipal workers)	ln(Average resident income)
	(1)	(2)	(3)	(4)	(5)	(6)
New Disclosure * 2010	0.906	0.002	0.002	0.001	-0.002	-0.006
	(0.257)	(0.013)	(0.012)	(0.012)	(0.019)	(0.013)
New Disclosure * 2011	0.450	-0.005	0.002	0.001	0.000	0.004
	(0.201)	(0.015)	(0.012)	(0.013)	(0.021)	(0.006)
New Disclosure * 2012	0.475	-0.009	-0.005	-0.016	0.013	0.001
	(0.247)	(0.016)	(0.011)	(0.017)	(0.022)	(0.008)
New Disclosure	-1.003					
	(0.136)					
log(Number of articles 2003-2008)	0.571					
	(0.041)					
City fixed-effects		X	X	X	X	X
City characteristics* year		X	X	X	X	X
R-squared		0.892	0.911	0.872	0.991	0.992
Observations	5520	4813	4813	4813	4813	3975

Notes: Column (1) is a negative binomial model for years 2000-2012 where the dependent variable is the number of articles in NewsLibrary.com that contain terms "City manager" and "Salary" by year and city. All dollar measures are nominal. 50th and 75th percentiles, average municipal compensation, and number of municipal workers are derived from CALPERS earnings records for years 2001-2012 for 423 cities. Average resident income is derived from IRS/SOI zip code records for years 2004-2012 for 444 cities. City characteristics are log population, log median housing values, log average household income, percent of residents who are Hispanic, percent of residents who are black, percent of homes that are renter occupied, percent of population (25+) with at most a high school degree, percent of employed working in construction, and number of full-time equivalent city government workers. All main effects are included in the model, including year dummies. Standard errors clustered on city are in parentheses.

Table 3. Disclosure and City Manager Salaries

	Newspaper and Online Disclosure							Only Online Disclosure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Disclosure * 2010	-0.003	-0.005	-0.002	-0.010	-0.011	-0.008	-0.019	-0.013
	(0.014)	(0.014)	(0.015)	(0.013)	(0.011)	(0.031)	(0.015)	(0.015)
New Disclosure * 2011	-0.052	-0.057	-0.054	-0.064	-0.064	-0.072	-0.064	-0.042
	(0.017)	(0.016)	(0.017)	(0.019)	(0.016)	(0.035)	(0.019)	(0.017)
New Disclosure * 2012	-0.066	-0.069	-0.064	-0.076	-0.075	-0.094	-0.071	-0.045
	(0.018)	(0.017)	(0.018)	(0.022)	(0.017)	(0.036)	(0.020)	(0.019)
City fixed-effects	X	X	X	X	X	X	X	X
City characteristics * year		X	X	X	X	X	X	X
County * year			X					
City*linear trend				X				
2009-2012 sample					X			
Arizona Comparison						X		
Manager fixed-effects							X	
R-squared	0.905	0.910	0.924	0.951	0.958	0.952	0.945	0.909
Observations	5099	5023	5023	5023	1778	2072	3334	5023

Notes: All models estimated by OLS for years 2001-2012. The dependent variable in all models is log city manager salary in 2012 dollars by city and year. Column (6) includes all Arizona cities and only new disclosure cities in California. In columns (1)-(7) new disclosure is a city that did not previously have city manager salary online or reported in the press (see text for details). In column (8) new disclosure is 1 if the city did not report the city manager salary on its website. City characteristics are log population, log median housing values, log average household income, percent of residents who are Hispanic, percent of residents who are black, percent of homes that are renter occupied, percent of population (25+) with at most a high school degree, percent of employed working in construction, and number of full-time equivalent city government workers. Manager fixed-effects are a unique fixed-effect for a manager in a city. All main effects are included in the model, including year dummies. Standard errors clustered on city are in parentheses.

Table 4. Heterogeneity by 2009 Wage Residual

	(1)	(2)	(3)
1(Residual>0)*New Disclosure*1(2012)	0.009	0.020	0.005
	(0.036)	(0.035)	(0.036)
1(Residual>0)*1(2012)	0.020	0.013	0.019
	(0.018)	(0.018)	(0.018)
New Disclosure*1(2012)	-0.073	-0.078	-0.071
	(0.026)	(0.026)	(0.028)
City fixed-effects	X	X	X
Characteristics 1[a]	X		
Characteristics 2[b]		X	
Characteristics 3[c]			X
R-squared	0.90	0.90	0.90
Observations	5062	5062	5062

Notes: Dependent variable is ln(city manager salary in 2012 dollars) by city and year for 2001-2012. In column (1) "Residual" is the residual computed using estimates from a regression of log city manager salary on city characteristics (Characteristics 1, 2, or 3) in year 2009 in the previous disclosure sample. All main effects are included in the model. The underlying regressions used to estimate the residuals are shown in Appendix Table A3. The variables New Disclosure*1(2011) and New Disclosure*1(2010) are reported in Appendix Table A4. Standard errors clustered on city are in parentheses.

[a] Characteristics 1 are log city population, log average household income, and log number of full-time municipal employee.

[b] Characteristics 2 consists of Characteristics 1 plus percent of homes that are renter occupied and percent of population (25+) with at most a high school degree.

[c] Characteristics 3 consists of Characteristics 2 plus log median housing values, percent of employed residents working in construction, and log housing density.

Table 5. Heterogeneity by Gender

	(1)	(2)	(3)	(4)	(5)	(6)
Female* New Disclosure*1(2012)	0.111	0.095	0.098	0.102	0.084	0.091
	(0.033)	(0.032)	(0.034)	(0.052)	(0.033)	(0.049)
New Disclosure*1(2012)	-0.090	-0.091	-0.091	-0.090		
	(0.018)	(0.018)	(0.019)	(0.025)		
City fixed-effects	X	X	X	X	X	X
City characteristics*year		X	X	X	X	X
County*year			X			
City*linear trend				X		X
City characteristics*year *New Disclosure					X	X
R-squared	0.93	0.93	0.94	0.96	0.93	0.96
Observations	3521	3463	3463	3463	3463	3463

Notes: Dependent variable is ln(city manager salary in 2012 dollars) by year and city for 2001-2012. The variable Female is an indicator for a female city manager. City characteristics are those used in Table 3 as well as an indicator for whether the city manager earned at least $200,000 in 2009.   All main effects are included in the models, including year dummies and the interactions of Female*new disclosure with 1(2011) and 1(2010). The complete set of estimates are reported in Appendix Table A5.

Table 6. Disclosure and City Manager Turnover

	(1) Separation	(2) Separation	(3) Separation	(4) Quit	(5) Quit	(6) Quit
New Disclosure * Post	0.203 (0.060)	0.213 (0.068)	0.200 (0.077)	0.158 (0.053)	0.146 (0.060)	0.128 (0.068)
New Disclosure	-0.198 (0.041)	-0.234 (0.046)	-0.208 (0.052)	-0.116 (0.053)	-0.109 (0.041)	-0.086 (0.047)
Post	-0.079 (0.039)			-0.076 (0.034)		
City characteristics*Post		X	X		X	X
County*Post			X			X
Mean Dependent Variable	0.29	0.29	0.29	0.20	0.20	0.20
Observations	920	902	902	920	902	902

Notes: Linear probability estimates. Sample is split into two periods: 2007-2009 and 2010-2012. The Post period is the 2010-2012 period. An observation is a city by each of these periods. The dependent variable is 1 if there was at least one separation or quit in the interval. See notes to Table 3 for list of city characteristics.

Online Appendix

Data Appendix

City Manager Compensation Data

The compensation data for California come from five sources: a public records request to all 482 California cities, earnings records from CALPERS, the State Controller public salary database, salaries recorded in the *Wayback Machine*, and news reports. I prioritize the data sent directly from cities and CALPERS, and when those are unavailable I supplement data from the other sources. Whenever possible, I use city manager Medicare earnings (W-2 Box 5) for city manager compensation, which is one of the variables requested by the State Controller for the public website and a record I requested in the public records request of cities. The reason for using Medicare earnings is that it captures additional compensation beyond salary such as bonuses and car allowances. Some cities were unable to provide this information, in which case I used the manager's twelve-month salary. For a given city, I consistently use either the twelve-month salary of total earnings so that any differences across cities are absorbed by city fixed-effects. When managers depart cities, often their Medicare earnings are inflated in their last year due to severance payments. I do not use these records and I instead impute these observations using straight-line interpolation with the old city manager's salary in the year before separation and the new city manager's salary in the year after separation. I also use straight-line interpolation to impute compensation missing for other reasons where the salary is available before and after the missing observation. Where there are no records before or after the observation the value remains missing. The estimates are robust to dropping imputed values and limiting the sample to the 2009-2012 period (as reported in the paper) where there is almost

1

universal reporting on compensation. When there is more than one city manager in the position, I sum their compensation excluding any large lump-sum payments that are likely to be separation payments. When a manager has a partial year of service I compute the full-year equivalent of the partial year salary when the date of hire is available.

Arizona City and Local Government Employee Payroll Data

Data on local government employee pay in Arizona come from the League of Arizona Cities and Towns. Each year, the League of Arizona Cities and Towns produces a Local Government Salary and Benefit Survey that contains the salary for each city manager in a given year. I obtained archives of the documents directly from the League of Arizona Cities and Towns, which I then digitized.[48]

City Characteristics Data

The city demographic and characteristics data come from the 2009 American Community Survey (ACS) 5-year estimates. I accessed this survey via the United State Census Bureau's American FactFinder search tool.[49]

The ACS 5-year estimates are multiple year estimates. The 2009 5-year estimates are the estimate for a city over the period from 2005-2009. I chose to use the ACS 5-year estimates because they provide full information on every city and town. The ACS three and one year estimates only provide information on cities and towns with populations larger than 20,000 and 65,000, respectively.[50]

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[48] http://www.azleague.org/
[49] http://factfinder2.census.gov/faces/nav/jsf/pages/index.xhtml
[50] http://www.census.gov/acs/www/guidance_for_data_users/estimates/

To accurately identify the estimates for cities and towns in California and Arizona in the ACS, I used the geographic data for places. Place data includes data for cities, towns, and census designated places.

The demographic data come from the Selected Social Characteristics, Selected Economic Characteristics, Selected Housing Characteristics, and the ACS Demographic and Housing Characteristics files. Information on household types and educational attainment come from the Selected Social Characteristics file. Information on employment status, industry, income, and poverty level come from the Selected Economic Characteristics file. Information on housing occupancy, value, and rent came from the Selected Housing Characteristics file. Information on population, age, race, and ethnicity come from the ACS Demographic and Housing Characteristics file.

Population and Housing Density Data

The population and housing density data come from the 2010 Decennial Census accessed from the United State Census Bureau's American FactFinder search tool, specifically CT-PH1 Population, Housing Unites, and Density: 2010 –State – Places. The dataset includes information on population, housing units, area in square miles, and housing and population density per square mile for every city and town in the United States.

Separation Data

Data on manager turnover was obtained by digitizing the *California Roster*, a directory of municipal elected officials and high-level managers published by the California Secretary of State, for years 2005-2012 supplemented with online searches of city manager biographies. The online searches were necessary because the roster is not always up to date. To identify quits

versus other reasons for separation I conducted an online search for every record of separation for press reports giving background information on the reasons for the change. I code a separation as a quit if the manager is reported to be leaving for another position, or if he or she is reported to be voluntarily retiring.

I also use the city manager names to code manager gender for 2005-2012. I coded gender based on a comparison of first name with common female and male names in the Social Security Administration names database. Where there was ambiguity, I conducted an online search of the managers to identify their gender.

Dropped Cases

Starting with 482 cities in California, the following exclusions are made:

--14 cities where there is no record of a Chief Administrative Officer.

--The City of Bell because it was an investigation of this city that prompted disclosure, so as to not confound an investigation with disclosure.

--14 cities where first disclosure occurred in 2009. This selection is imposed because these cities are treated in that they first disclosed after the Supreme Court ruling, mostly through the Bay Area online database, but the timing relative to the broader mandate is shifted by a year. The estimates are unaffected by their inclusion as untreated or treated cities.

--In specifications with controls, 8 cities with missing city characteristics

The main analysis utilizes 453 California cities without controls, 445 cities with controls, and 84 Arizona cities.

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Appendix Figure A1.   Share of all articles in the California Press referring to "Furloughs" (2000=1)
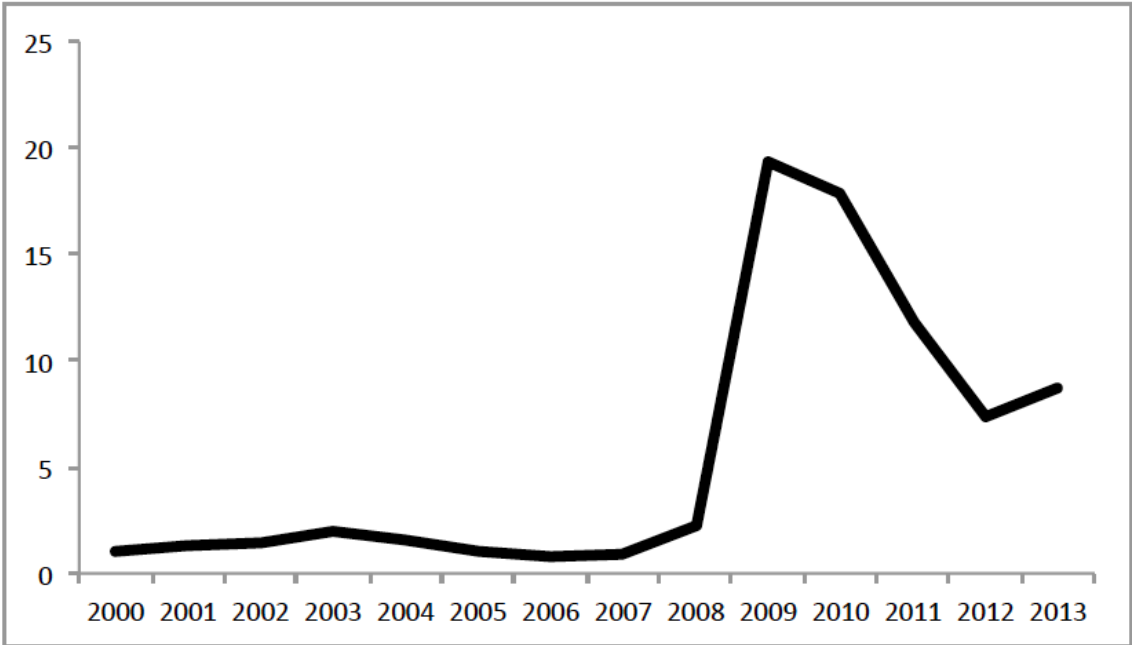
Notes: The data are from searches of California newspapers in NewsLibrary.com. The numerator is the number of articles referring to "Furloughs" in a given year and the denominator is the total number of articles in California. The share is expressed relative to 2000.

Appendix Table A1. Summary Statistics

	(1)	(2)	(3)	(5)	(6)
	California New Disclosure	California Previous Disclosure	Arizona	p-value (1)-(2)	p-value (1)-(3)
ln(population)	9.52	10.41	9.34	0.00	0.38
ln(Housing value)	12.79	13.09	11.98	0.00	0.00
ln(Mean Household Income)	10.89	11.08	10.71	0.00	0.00
ln(Government FTEs)	4.29	5.20	4.79	0.00	0.01
% Hispanic	39.00	30.34	30.97	0.00	0.02
% Black	3.518	3.786	2.07	0.62	0.01
% at most HS Degree	48.02	37.76	46.60	0.00	0.54
% Housing that is Renter Occupied	38.85	38.20	29.24	0.64	0.00
% Employed in construction	7.432	7.114	9.61	0.39	0.00
% Employed in Retail	11.02	11.16	11.92	0.66	0.07
Female Manager (Yes = 1)	0.135	0.124		0.34	
ln(2009 city manager compensation)	12.05	12.26	11.67	0.00	0.00
ln(2012 city manager comp.)-ln(2009 city manager comp.)	-0.116	-0.046	-0.041	0.00	0.00
ln(2012 Wage Bill)-ln(2009 Wage Bill)	-0.213	-0.202		0.60	
Number of Cities	178	296	89		

Notes: Compensation and wage bill are in 2012 dollars. See Section III for data sources and definition of disclosure cities.

Appendix Table 2A. Relation Between New Disclosure and Time Varying City Characteristics from Local Government Compensation Reporting Program Data

	ln(50th percentile city salary)	ln(75th percentile city salary)	ln(Average municipal compensation)	ln(Number of municipal workers)
	(2)	(3)	(4)	(5)
New Disclosure * 2010	0.037	-0.015	0.001	-0.018
	(0.063)	(0.034)	(0.021)	(0.020)
New Disclosure * 2011	0.067	0.058	0.017	-0.029
	(0.069)	(0.036)	(0.026)	(0.027)
New Disclosure * 2012	0.046	0.057	0.012	-0.019
	(0.072)	(0.037)	(0.026)	(0.030)
City fixed-effects	X	X	X	X
City characteristics*year	X	X	X	X
R-squared	0.888	0.911	0.947	0.990
Observations	1791	1791	1776	1779

Notes: 50th and 75th percentile city salaries, average municipal compensation, and number of municipal workers are derived from public data for from the Local Government Compensation Reporting Program for 2009-2012. City characteristics are log population, log median housing values, log average household income, percent of residents who are Hispanic, percent of residents who are black, percent of homes that are renter occupied, percent of population (25+) with at most a high school degree, percent of employed working in construction, and number of full-time equivalent city government workers. All main effects are included in the model, including year dummies. Standard errors clustered on city are in parentheses.

Appendix Table A3. Estimates Underlying the Residual Calculation Used in Table 4

	(1)	(2)	(3)
ln(population)	0.086	0.095	0.104
	(0.015)	(0.015)	(0.016)
ln(Average Household Income)	0.205	0.244	0.403
	(0.029)	(0.061)	(0.097)
ln(Number of municipal workers)	0.080	0.065	0.062
	(0.014)	(0.016)	(0.016)
Percent of population (25+) with at most a HS degree		0.000	-0.001
		(0.001)	(0.001)
Percent of homes that are renter occupied		0.003	0.006
		(0.001)	(0.002)
ln(Housing Values)			-0.060
			(0.061)
ln(Housing Density)			-0.054
			(0.022)
% Working in Construction			0.010
			(0.004)
Observations	289	289	289
R-squared	0.54	0.55	0.57

Notes: This table show the underlying estimates used to construct the residuals in Table 4. Dependent variable is log city manager salary. Sample is limited to previous disclosure cities in 2009.

Appendix Table A4. Additional Estimates for Models in Table 4

	(1)	(2)	(3)	(4)	(5)	(6)
1(Residual>Y)*New Disclosure*1(2010)	0.010	0.023	0.007	-0.009	-0.006	-0.009
	(0.026)	(0.026)	(0.027)	(0.027)	(0.027)	(0.026)
1(Residual>Y)*New Disclosure*1(2011)	0.010	0.020	-0.004	-0.003	-0.004	-0.025
	(0.033)	(0.033)	(0.034)	(0.034)	(0.034)	(0.033)
1(Residual>Y)*New Disclosure*1(2012)	0.009	0.020	0.005	-0.006	-0.010	-0.008
	(0.036)	(0.035)	(0.036)	(0.036)	(0.036)	(0.036)
New Disclosure*1(2010)	-0.008	-0.014	-0.008	-0.003	-0.005	-0.003
	(0.019)	(0.019)	(0.021)	(0.017)	(0.018)	(0.018)
New Disclosure*1(2011)	-0.059	-0.063	-0.052	-0.054	-0.055	-0.046
	(0.025)	(0.024)	(0.026)	(0.022)	(0.022)	(0.023)
New Disclosure*1(2012)	-0.073	-0.078	-0.071	-0.067	-0.066	-0.066
	(0.026)	(0.026)	(0.028)	(0.024)	(0.024)	(0.024)
1(Residual>Y)*1(2010)	0.068	0.065	0.067	0.087	0.087	0.087
	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)	(0.014)
1(Residual>Y)*1(2011)	0.039	0.032	0.045	0.039	0.042	0.054
	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)
1(Residual>Y)*1(2012)	0.020	0.013	0.019	0.019	0.024	0.028
	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.018)
City fixed-effects	X	X	X	X	X	X
Y=0	X	X	X			
Y=65th percentile				X	X	X
City Characteristics 1	X			X		
City Characteristics 2		X			X	
City Characteristics 3			X			X
R-squared	0.90	0.90	0.90	0.90	0.90	0.90
Observations	5062	5062	5062	5062	5062	5062

Notes: The table shows the full set of coefficients for the models reported in Table 4. See Table 4 notes for additional detail. This table also shows additional specifications where the interaction is with a dummy for the residual being greater than the 65th percentile of the residual distribution.

Appendix Table A5. Additional Estimates for Models in Table 5

	(1)	(2)	(3)	(4)	(5)	(6)
Female* New	0.022	0.021	0.024	0.017	0.026	0.021
Disclosure*1(2010)	(0.027)	(0.029)	(0.032)	(0.033)	(0.028)	(0.033)
Female* New	0.086	0.083	0.068	0.088	0.078	0.077
Disclosure*1(2011)	(0.030)	(0.030)	(0.035)	(0.040)	(0.032)	(0.040)
Female* New	0.111	0.095	0.098	0.102	0.084	0.091
Disclosure*1(2012)	(0.033)	(0.032)	(0.034)	(0.052)	(0.033)	(0.049)
New Disclosure*1(2010)	-0.012	-0.016	-0.018	-0.012		
	(0.013)	(0.013)	(0.014)	(0.014)		
New Disclosure*1(2011)	-0.071	-0.077	-0.077	-0.075		
	(0.017)	(0.017)	(0.017)	(0.020)		
New Disclosure*1(2012)	-0.090	-0.091	-0.091	-0.090		
	(0.018)	(0.018)	(0.019)	(0.025)		
Female*1(2010)	-0.016	-0.020	-0.019	-0.023	-0.020	-0.024
	(0.016)	(0.016)	(0.018)	(0.021)	(0.016)	(0.021)
Female*1(2011)	-0.015	-0.023	-0.020	-0.020	-0.022	-0.023
	(0.017)	(0.016)	(0.016)	(0.026)	(0.016)	(0.025)
Female*1(2012)	-0.020	-0.028	-0.024	-0.017	-0.027	-0.020
	(0.019)	(0.018)	(0.020)	(0.035)	(0.018)	(0.034)
Female * New Disclosure	-0.016	-0.016	-0.004	-0.037	-0.001	-0.017
	(0.022)	(0.023)	(0.026)	(0.029)	(0.022)	(0.031)
Female	0.004	-0.007	0.001	0.017	0.000	0.014
	(0.014)	(0.014)	(0.016)	(0.018)	(0.014)	(0.018)
City fixed-effects	X	X	X	X	X	X
City characteristics*year		X	X	X	X	X
County*year			X			
City*linear trend				X		X
City characteristics*year *New Disclosure					X	X
R-squared	0.93	0.93	0.94	0.96	0.93	0.96
Observations	3521	3463	3463	3463	3463	3430

Notes: Dependent variable is ln(city manager salary in 2012 dollars). See Table 3 notes for city characteristics. All main effects are included in the models, including year dummies and the interactions of Female*new disclosure with 1(2011) and 1(2010).

Appendix Table A6. Relation Between Magnitude of City Manager Wage Cuts and City Council Composition

	(1)	(2)
Variable *New Disclosure*1(2010)	0.000 (0.026)	0.001 (0.006)
Variable *New Disclosure*1(2011)	-0.003 (0.031)	0.003 (0.007)
Variable *New Disclosure*1(2011)	0.003 (0.031)	-0.003 (0.007)
New Disclosure*1(2010)	-0.003 (0.021)	-0.010 (0.043)
New Disclosure*1(2011)	-0.056 (0.023)	-0.080 (0.056)
New Disclosure*1(2012)	-0.070 (0.023)	-0.042 (0.056)
Variable *1(2010)	0.026 (0.016)	-0.004 (0.003)
Variable *1(2011)	0.005 (0.017)	-0.002 (0.004)
Variable *1(2012)	0.003 (0.018)	-0.001 (0.004)
City fixed-effects	X	X
City characteristics * year	X	X
Variable= Share of 2009 city council in office in 2005	X	
Variable =Average tenure of city council in 2009		X
R-squared	0.91	0.91
Observations	5009	5009

Notes: This table shows OLS estimates of the relationship between new disclosure, and interactions of new disclosure with measures of city council tenure, and log city manager compensation in 2012 dollars. The average tenure of the city council is based on truncated city council tenure for years 2003-2009 and a member of a city council who is observed in their position for all years 2003-2009 is assigned a tenure of six years. City characteristics are reported in Table 3.

Appendix Table A7. Relation Between New Disclosure and Future City Council Composition

	Share of city council in t-4 present in t	
	(1)	(2)
New Disclosure * Post	-0.013	-0.021
	(0.046)	(0.048)
Post	0.343	
	(0.026)	
City fixed-effects	X	X
City characteristics*year		X
R-squared	0.627	0.640
Observations	901	886

Notes: This table shows OLS estimates of the effect of new disclosure on city council composition. The sample is California cities in 2008 and 2012. The post period refers to 2012. The dependent variable is the share of the city council in t (where t is either 2008 or 2012) that was present in t-4. City characteristics are those from Table 3. Robust standard errors in parentheses.