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Does Transparency Lead to Pay Compression?

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**ABSTRACT**

This paper asks whether disclosing wages to the public changes wage setting at the top of the public sector income distribution. I evaluate a 2010 California mandate that required cities to submit municipal salaries to the State, to be posted on a public website. City managers—typically the highest paid employees—in cities that had not previously disclosed salaries experienced average compensation declines of approximately 8 percent relative to cities where at the time of the mandate manager wages were already in the public domain. This decline was largely accomplished through nominal pay cuts. The wage cuts were not the result of relatively greater financial stress, as the overall wage bill did not diverge between these sets of cities. Wages were cut irrespective of whether or not they were out of line with (measured) fundamentals. Consequently, the residual variance of manager wages did not decline. Following new disclosure the city manager quit rate increased by 75 percent, suggesting that transparency pressured cities to lower the wages that were already close to reservation levels. The evidence is more consistent with a “populist” response to perceptions of excessive salaries than with the effects of increased accountability.

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Pay transparency policies are growing in importance. Recent examples include a U.S. presidential memorandum that recommends requiring federal contractors to submit summary data on employee compensation by race and sex, a proposed rule that would require companies to compare CEO pay with the pay of the median worker, and states and cities that have increasingly disclosed worker salaries as part of sunshine initiatives. 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 are shaped by available information (e.g. Bartels 2005; Card et al. 2012; Cruces, Perez-Truglia, and Tetaz 2013; Kuziemko et al. 2013), little is known about whether there are “real” effects of more information on pay. Two reasons why salary transparency might change the compensation structure are greater accountability and public aversion to perceived excessive salaries, without accounting for market factors that determine wages. Advocates for these policies have stressed the role of the accountability benefits of increased transparency.<sup>1</sup> Given new information, the public might hold elected officials more accountable to gaps between pay and productivity.<sup>2</sup> Increased accountability could result in lower city manager compensation if capture and managerial power are restrained.<sup>3</sup> Transparency might also lead to compression because of public perceptions of excess pay, even if wages are in line with fundamentals. 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

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<sup>1</sup> See, for example, *The Economist*, November 19, 2011.

<sup>2</sup> Consistent with this explanation, 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.

<sup>3</sup> See Diamond (2013) and Brueckner and Neumark (2014) for evidence of rent extraction in the public sector.

companies where top salaries are disclosed (Jensen and Murphy 1990), though there is little evidence.<sup>4</sup>

Estimating the relationship between pay transparency and the wage structure in organizations is challenging because 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 top city managers.<sup>5</sup> The research design exploits the fact that prior to the mandate a subset of cities (“previous disclosure” cities) had already disclosed the salaries of their top managers.<sup>6</sup> Using the Internet *Wayback Machine* and archives of more than three hundred local newspapers, I identify cities where the pay of top city administrative officers was already disclosed to the press or on their websites at the time of the mandate.<sup>7</sup> Based on this procedure, I estimate that prior to the mandate approximately 60 percent of cities already disclosed the salary of the chief administrative officer (“city manager”), typically the highest paid city employee. 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.

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<sup>4</sup> A related mechanism is morale considerations. 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](http://www.whitehouse.gov/the-press-office/2014/04/08/presidential-memorandum-advancing-pay-equality-through-compensation-data))

<sup>5</sup> I use “city manager” as the generic term for the chief administrative officer position.

<sup>6</sup> 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.

<sup>7</sup> The *Wayback Machine* is a digital archive of websites ([web.archive.org/](http://web.archive.org/)).

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 481 cities in California for 1999-2012 payroll records and contracts of city managers. (The State Controller requested that cities disclose 2009 salaries, which yields one full year of pre-mandate data.)<sup>8</sup>

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 8 percent, largely through nominal wage cuts. This finding is evidence that maximum compensation is sensitive to increased transparency. Importantly, this wage effect does not appear to be the result of citywide furloughs or budget cuts following the 2007 recession as I find no evidence of differential changes in the overall wage bill in new and previous disclosure cities, and 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, and city-specific time trends.

To assess whether these wage cuts are the result of greater accountability I test whether new disclosure results in a lower residual variance of wages. I find that the reduction in salaries is not larger in cities with more positive residuals in a pre-disclosure cross-sectional regression of log salary on city characteristics, suggesting that wage cuts were not the result of the discovery of managers who exploited secrecy to inflate their wages through capture. New information also did not lead to compression of city manager salaries across cities, as would be the case if there were more information to be used for arbitrage or benchmarking. The evidence points more to a

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<sup>8</sup> 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.

populist mechanism, whereby cities were pressured to reduce top salaries irrespective of whether the salaries were appropriate.

Complementing the earnings analysis, I examine the effects of disclosure on manager turnover. I find that the policy was associated with an increase of 75 percent in manager voluntary separations. Together with the wage estimates, this estimate implies a high elasticity of quits with respect to the wage of approximately 9. This finding suggests that managers had limited surplus in their positions. This finding goes against another accountability explanation for the observed wage cuts, that pay transparency compels employers to renegotiate compensation agreements that awarded the managers surplus that could be transferred to taxpayers. The findings instead suggest that managers had little surplus to begin with. Therefore, a possible drawback to transparency policies in the public sector is that wages fall to the point that cities cannot retain incumbent managers.

In addition to shedding light on the role of information on the wage structure, these findings contribute to the literature on 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 political accountability.<sup>9</sup> I also find that the wage effect is entirely concentrated on male managers and I discuss possible reasons for this asymmetry.

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<sup>9</sup> 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), Besley and Pratt (2006), Brunetti and Weder (2003), Gentzkow (2006), DellaVigna and Kaplan (2007), Ferraz and Finan (2008), Gentzkow, Shapiro and Sinkinson (2011), 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 \$800,000 annually.<sup>10</sup> While there were no legal limits on city manager compensation in California, this compensation was considered by many to be excessive (though in this case there turned out to be illegally written employment contracts). As a direct consequence of this scandal, in August, 2010 the State Controller John Chiang initiated the “Local Government Compensation Reporting Program” that 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 to be posted on its website ([gcc.sco.ca.gov](http://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.<sup>11,12</sup>

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 name, salary and other employment information, but only when requested by the public.<sup>13</sup> Before this ruling cities were under no obligation to disclose their compensation of city employees by name to the public or the press. The ruling led to several news outlets obtaining and reporting compensation information, notably a consortium of San Francisco Bay Area

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<sup>10</sup> “Is a Manager Worth \$800,000,” Jeff Gottlieb and Ruben Vives, *Los Angeles Times*, July 15, 2010.

<sup>11</sup> <http://www.acwa.com/news/state-legislation/state-controller’s-public-pay-website-gets-overhaul>

<sup>12</sup> 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.

<sup>13</sup> “State Supreme Court: Local Government Salaries are Public Record,” Howard Mintz, *San Jose Mercury News*, August 28, 2007.

newspapers that published an online database in 2009 that included employee compensation for fifty cities and other public entities.<sup>14</sup>

In principal, 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 approximately 40 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.<sup>15</sup> 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.<sup>16</sup> Thus, the distinction between passive disclosure (no obligation to post salaries) versus active disclosure (obligation to post salaries) appears to be important.

One reason for the limited response to the 2007 ruling likely has to do with cities finding ways to stall in fulfilling requests. For example, one of the *Los Angeles Times* reporters who

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<sup>14</sup> 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, but the estimates are robust to inclusion of these cities.

<sup>15</sup> 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. For example, 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.

<sup>16</sup> It is also possible that the disclosure of Bell salaries had an additional effect in turning the public’s attention towards city compensation.



broke the 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.<sup>17</sup>

These reporters won a Pulitzer Prize for this investigation. Needless to say, the average resident (or even reporter without access to a legal department) would 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, managing staff, and hiring.<sup>18</sup> 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

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<sup>17</sup> <http://www.npr.org/templates/story/story.php?storyId=130108851>

<sup>18</sup> 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) for additional background on forms of city governance.

annually, to contracts that specify compensation over two or three year terms. Based on a random draw of ten contracts I 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 specified in contracts as depending on Cost of Living Adjustments or depend on compensation for other city employees (eg. 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 what city managers in California can earn. City manager compensation is driven by supply and demand factors, but there are also 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.)<sup>19</sup>

### **Section III. Data**

The data for this project comes from multiple sources, including multiple public records act requests and newly digitized archival documents. I describe them briefly here and additional details on data and sample selection are available in the Data Appendix.<sup>20</sup>

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<sup>19</sup> 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.

<sup>20</sup> 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 time-consuming data collection.

### *Compensation*

City manager compensation data comes from four sources. City manager compensation for years 2000-2012 was obtained from a public records act request for payroll records and contracts to all 481 California cities.<sup>21</sup> These data are supplemented with 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 66, 86 and 98 percent of cities for years 2001-2012, 2005-2012, and 2009-2012 respectively. 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.<sup>22</sup> 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 in 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.

### *City Characteristics*

City characteristics are five-year averages from the 2009 American Community Survey, Census American Factfinder, and the 2007 Census of Governments. The controls utilized in the main analysis are log population, log housing density, log median household income, percent Hispanic, percent black, percent of homes that are renter occupied, unemployment rate, percent

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<sup>21</sup> This public records request is only possible because of the Supreme Court ruling that city employee wages are in the public domain.

<sup>22</sup> 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.

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.

### *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 2005-2013 supplemented with online searches of city manager biographies. To identify quits versus other reasons for separation (eg. termination, death) 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). A research assistant read the articles to verify whether the manager was voluntarily resigning versus being pushed out, but it remains possible that some of the resignations are actually involuntary. Reassuringly, the share of all separations coded as quits for city managers (67 percent in 2007) is close to the share in the PSID (68 percent in 2007).

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.

### *Disclosure Data*

A key variable in this study is whether city salaries were in the public domain prior to the 2010 mandate. For city managers I look at whether as of the end of 2008 the city manager's salary was posted on the city's website through a search of the city website on the *Wayback*

*Machine*. Specifically, for every city we searched the last available snapshot in 2008 looking for the city manager salary in the Human Resources Department, Finance Department, and the Administration directories of the city website.<sup>23</sup> 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 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.<sup>24</sup> NewsLibrary.com displays an excerpt of the first paragraph in the search results, and I first verified whether the salary is available there. If not, I assessed whether the article appeared relevant to the question, and if so we read the entire article to check for this information.<sup>25</sup>

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. Two 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, and it reflects the discrete nature of disclosure since 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

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<sup>23</sup> When available, this information is usually posted in the Human Resources department directory.

<sup>24</sup> 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.

<sup>25</sup> The *Los Angeles Times* is not archived in NewsLibrary.com and I did a separate search through their search engine using the same methodology.

newspapers), the operating assumption is that in such cases the information would have been relatively more difficult to access than cities coded as disclosing. The measure is therefore best thought of as an index that is related to previous transparency. If cities are misclassified as not 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.

In Table 1 I examine the relationship between this measure of new disclosure and broader media coverage of the cities. Column (1) relates the log total number of articles that mention the city between 2003-2008 to the new disclosure dummy. New disclosure cities had 85 percent fewer articles in the California press mentioning the city from 2003-2008. This relationship is not mechanical since the dependent variable includes coverage on any topic (eg. 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).

City characteristics such as population and median household income are correlated with media exposure and have an independent effect on whether city manager salary is disclosed. When I add detailed city characteristic controls (column 2) the relationship between disclosure and overall coverage falls, but remains a sizable with 33 percent fewer press mentions in new disclosure cities. These estimates imply that the disclosure variable is related to media coverage in the city. Cities with previous disclosure have significantly more press coverage on any topic, even conditional on population and other city characteristics.

Column (3) of Table 1 presents the difference-in-difference estimate of search results for mentions of city manager and salary by new and previous disclosure cities. The underlying data

are city and year observations of search counts for (“YYY” AND (“City Manager” OR “City Administrator” OR “Town Manger” 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. 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.<sup>26</sup> The estimates confirm that prior to 2010 new disclosure cities have substantially fewer search results, with 67 percent  $(=\exp(-1.1)-1)*100$  fewer search results in new disclosure cities relative to prior disclosure cities. This gap in the pre-mandate period is mechanical by how I constructed the new disclosure variable.<sup>27</sup> The gap in search results closes considerably, however, after the mandate in 2010, with the new disclosure gap declining to 25 percent 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 how accessed the salaries directly online.

#### **Section IV. Summary Statistics**

Table 2 presents city characteristics in new disclosure cities (column 1), previous disclosure cities (column 2), and Arizona (column 3). There are clear differences between these sets of cities. Previous disclosure cities are significantly larger, denser, have higher average

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<sup>26</sup> Since a non-disclosure city is one where there is limited media presence, and post-mandate coverage is both a function of new information as well as media presence it need not be the case that the increase in new disclosure coverage is larger after the mandate given the low baseline media presence.

<sup>27</sup> While new disclosure is defined as lack of city manager salary reports prior to 2010, the search results are not strictly zero in the period as there are some false positive results using the search terms.

household income, more municipal employees, a lower share Hispanic, and a lower share of residents with at most a high school degree than new disclosure cities in California. I cannot reject differences in percent black, percent of housing that is renter occupied, percent of population employed in construction, and percent of population employed in retail. Arizona cities are closer in size to the new disclosure cities, but there are also differences in a number of other dimensions. These comparisons suggest that it will be important to ensure robustness by controlling for a rich set of city characteristics to verify whether I am picking up differential trends in characteristics rather than the effects of disclosure. It will turn out that the estimated changes in compensation are unaffected by inclusion of these covariates interacted by year.

Table 2 also presents means of various measures of city compensation. City managers in new disclosure cities earned 22 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.<sup>28</sup> Both sets of California cities have higher manager earnings than Arizona. The table also reports the log change in city manager compensation between 2009 and 2012, allowing for a simple differences-in-differences estimate of the effect of the mandate. Between 2009 and 2012 real city manager compensation fell by 12 percent in new disclosure cities as compared to 4 percent in previous disclosure and Arizona cities. These differences are statistically significant at the 1 percent level. In the next section I will estimate these changes more systematically, accounting for trends, city characteristics, and regional shocks.

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<sup>28</sup> Controlling for log population and log mean household income reduces this gap to four percent.



## Section V. Disclosure and City Manager Outcomes

### *Earnings*

Figure 2 presents initial 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 every city I plot demeaned annual log compensation, normalizing both sets of cities to 0 in 2009.<sup>29</sup> I also plot demeaned city manager log compensation in new disclosure cities where the sample was been reweighted to match a set of city characteristics (those listed in Section III) of the previous disclosure sample. I use DiNardo Fortin 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 2010 nominal wage growth plateaued for both sets of cities, but in 2011-2012 nominal compensation declined in new disclosure cities while for previous disclosure city wage growth remained flat in 2011, and then rose slightly in 2012. By 2012 the difference in compensation was close to 8 percent relative to 2009. Reweighting the new disclosure sample yields an almost identical pattern.

Figure 3 plots the difference in the (weighted) series with 95 percent confidence bands. 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

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<sup>29</sup> Demeaning the data is necessary since the panel of cities is unbalanced.

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 change compensation.

Figure 4 plots both of the raw series against Arizona. The 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 4 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. Thus, 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:

$$(1) \quad \ln(y_{it}) = \alpha_i + \delta_t + \delta_t * X_i + \theta_1 1(t = 2010) * NewDisclosure_i + \theta_2 1(t = 2011) * NewDisclosure_i + \theta_3 1(t = 2012) * NewDisclosure_i + \varepsilon_{it},$$

where  $y_{it}$  is log manager compensation in 2012 dollars,  $\alpha_i$  are city fixed-effects,  $\delta_t$  are year dummies, and  $\delta_t * X_i$  are year dummies interacted with time invariant city characteristics. The 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 income is  $-0.045$  ( $se=0.017$ ) and  $-0.066$  ( $se = 0.02$ ) in 2011 and 2012 respectively. The estimates and significance are largely invariant (and tend to be a bit larger) 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 few missing observations (column 5). The estimated disclosure effect in the specification with city-specific trends is an 8.2 percent decline in wages by 2012. In column (6) I compare just California new disclosure cities to all cities in Arizona, which were not treated. The point estimates grow to  $-0.083$  log points ( $se = 0.041$ ) in 2011 and to  $-0.095$  log points in 2012 ( $se = 0.059$ ). The larger standard errors are due to the smaller number of Arizona cities (89 versus 297 previous disclosure cities in California). In column (7) I include manager\*city fixed-effects. In this specification new disclosure is associated with an 8 percent reduction in compensation within manager, suggesting 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 limit the disclosure variable to whether the city had not posted the information online (that is, ignoring news reports). I find a similar pattern of estimates, but with smaller magnitudes. New disclosure is associated with 3.8 percent lower wages in 2011 and 3.3 percent lower wages in 2012, both of which are significant at conventional levels. This is about half of the magnitude of the effects

when using the more strict criterion that they also do not have news coverage. This suggests that news coverage was putting downward pressure on manager wages in previous disclosure cities prior to the mandate.

### *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, and 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.

Second, if the negative compensation effect is due to financial stress or furloughs, we would expect to see relative declines in the city wage bill. I find no evidence of this. Column (9) of Table 3 shows that the point estimate of new disclosure relative to previous disclosure cities in 2012 is 0.005 (se = 0.025). (For this analysis I limit the sample to 2009-2012, as these are the years for which I have complete wage bill information for all cities.) Total payroll did not decline by more in new disclosure cities than previous disclosure cities, on average, over this period.

Third, we can also look at the timing of furloughs in California relative to the timing of the estimated mandate effects. Figure 5 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-Great Recession years, they are only half as large as the mentions in 2009-2010. This pattern seen here 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 2009 and 2010 as well as 2011 and 2012, which we do not.

### *Changes in Residual Wage Variance*

Under the improved accountability mechanism, 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 also would also represent cases where captured managers 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. If disclosure has the effect of revealing and unwinding capture or managerial power we would expect to see wage declines for managers who are paid more than what is predicted given the characteristics of where they work.

To implement this test I regress log city manager salary in 2009 on log population, log median household income, city unemployment rate, percent of residents (25+) with at most a high school degree, log of full-time equivalent city employees, percent of homes that are renter occupied, and log housing density using only previous disclosure cities (R-squared = 0.5). I then interact a dummy for whether the city's residual is greater than zero with the interaction of the

new disclosure and 2010-2012 dummies. Column (1) of Table 4 reports these estimates. To simplify presentation I only report the 2012 interactions but the earlier years do not alter the conclusion. The interaction shows that wages in positive residual cities are not declining by relatively more than other cities. This finding holds for other measures of the residual, such as an indicator for the top residual quartile and for inclusion of more or less characteristics.<sup>30</sup> These estimates suggest that the Bell case is more 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 points suggests that the mechanism behind the wage effect is not greater accountability.

Further evidence of this general tendency to lower wages can be seen by looking at the R-squared of regressions of log city manager wages on the city characteristics used above. Dividing the sample into four groups, new and previous disclosure cities in 2009 and 2012, and regressing the log city manager wage on the city characteristics results in almost the same R-squared in each of these groups (0.5 and 0.49 for previous disclosure cities in 2009 and 2012 respectively, and 0.47 and 0.47 for new disclosure cities in 2009 and 2012 respectively) and no meaningful changes over time.

It is also not the case that the policy reduced city manager dispersion of wages across all California cities, as would be the case if transparency led to more information that cities could use to arbitrage, along the lines of Jensen (2007). Using the same covariates as in the previous analysis, the R-squared of a regression of log city manager wage is 0.53 in both 2009 and 2012.

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<sup>30</sup> I also estimated a more parsimonious model with log population and log median household income with similar results. A third model I estimated included the base set of characteristics as well as log housing values and log rents, also with similar results, but these are not included in the main specification because these variables are missing for 25 cities.

This last finding is perhaps not surprising since more than half of California cities already disclosed wages providing ample public data on the distribution of wages.

### *Gender Differences*

Table 4 includes interactions of the information shock with a dummy for whether the city manager is female. In the model, the interaction  $1(2012)*new$  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. The table shows that the entire decline in compensation seen in Table 3 is coming from male managers. 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 new disclosure (column 6) does not change the estimates appreciably, nor do within-county comparisons or inclusion of city\*year trends (columns 3, 5 and 7).

These findings suggest that pay secrecy had a differential effect on male and female managers. While one can only speculate as to why such an asymmetry is present, this evidence is consistent with male managers doing more to take advantage of pay secrecy to inflate their pay than female managers, as 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.<sup>31</sup> 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 high degree of 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 as an outcome whether there was at least one separation over the period I eliminate double counting. 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 4 reports the estimates from this analysis. Column (1) shows that in the pre-disclosure period overall separations were approximately 20 percentage points lower in new than previous disclosure cities. The separation rate over the next three-year period increased by 12 percentage points in new disclosure cities while it decreased by 7 percentage points in previous disclosure cities. The difference between these changes is statistically significant at conventional

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<sup>31</sup> Female city managers earned approximately 5 percent less than male managers in 2009, but this difference is not statistically significant and the exact magnitude of the gap is sensitive to included controls.



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 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 0.15 (se = 0.05) without controls, and 0.14 (se = 0.06) and 0.12 (se = 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 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 8 percent implies an elasticity of quits with respect to the wage of 9. This magnitude is substantially higher than previous estimates in the literature.<sup>32</sup> The finding suggests that a significant share of managers have wages close to the margin of their next best option. A caveat is that the high quit rate might due to unhappiness at having received a nominal pay cut, as in Bewley (1999), rather than managers leaving to better alternatives.<sup>33,34</sup> Notwithstanding, a conclusion from this analysis is that transparency policies in the public sector have the potential to cause significant turnover.

The separation analysis provides additional evidence against the accountability explanation for the observed wage compression. It does not appear that additional information

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<sup>32</sup> The elasticities summarized in Manning (2011) never exceed 2.

<sup>33</sup> 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. For example, inclusion of manager\*city fixed-effects in specification (1) shows similar magnitudes as the estimates in Table 3.

<sup>34</sup> It is also possible that managers quit because they didn't want their wage to be 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.

led to renegotiation that transferred surplus from managers to their employers. Rather, the evidence suggests that wages were already close to manager reservation levels. This finding is consistent with estimates on public/private wage differentials that 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). The high responsiveness of quits to the wage cuts may limit downward pressure on wages since any additional compression would lead to substantial turnover. Absent the high level of quits, wages may have fallen further.<sup>35</sup> It is also possible that were the economy stronger at the time of the mandate, the wage effect would have been smaller since managers would have had better outside options.

## **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 capture. The evidence assembled does not point to this mechanism. Rather, cities cut wages whether or not they appear high as predicted by fundamentals, and city managers appear to already have had compressed wages before the mandate. The evidence is more consistent with a “populist” response to top salaries: top salaries are cut because they appear excessive, regardless of whether or not they actually are. 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

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<sup>35</sup> 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.

growth of public sector CEO pay (where wages are public) has trailed private sector 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 investigating 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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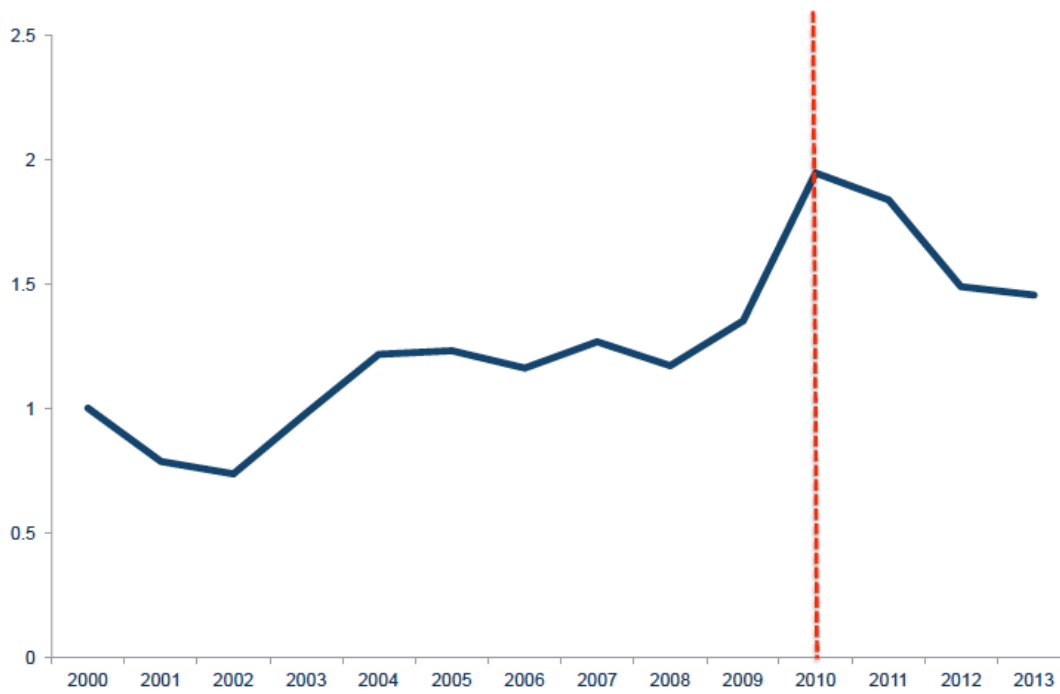
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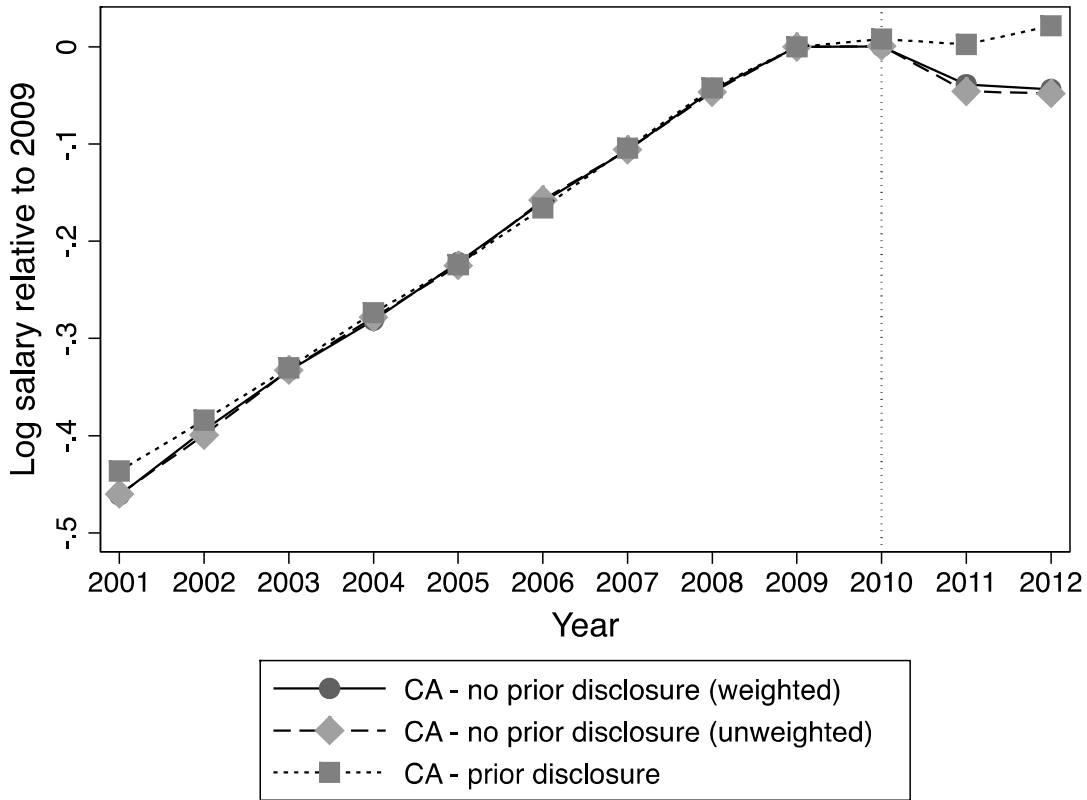
Figure 1. Share of all articles in the California Press referring to “City Manager” and Salary (2000=1)



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 proportional differences relative to 2000. The dashed vertical line is the year that the mandate went into effect.

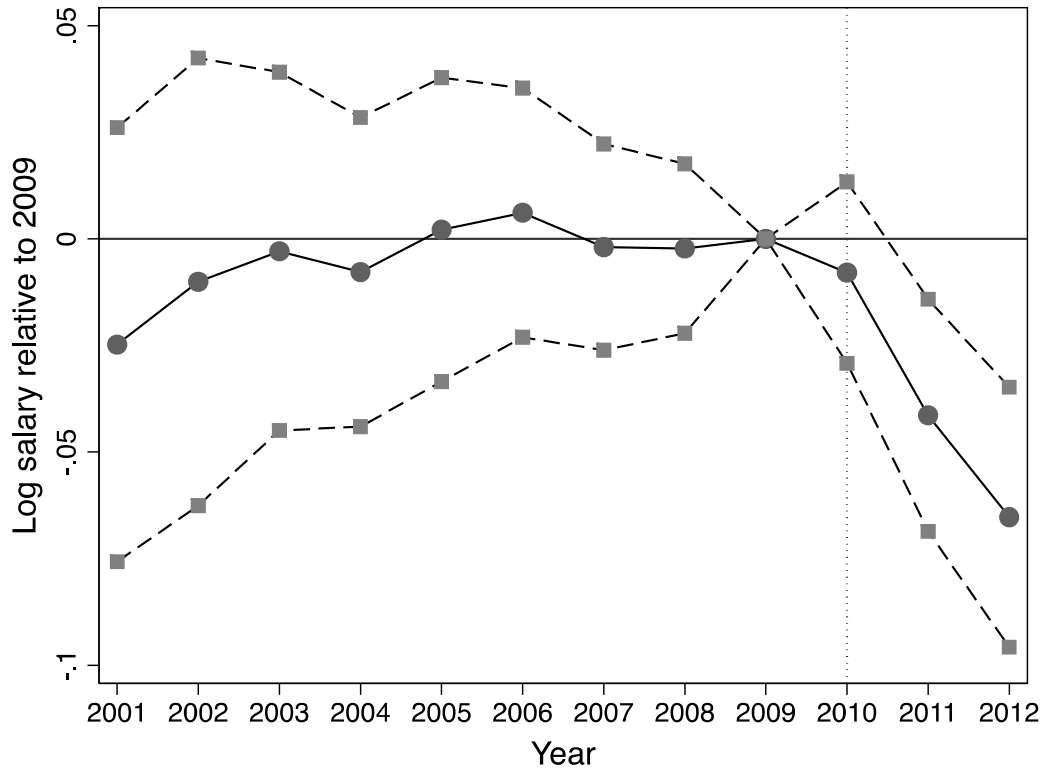


Figure 2. City Manager Nominal Compensation Growth; New Disclosure and Prior Disclosure CA Cities



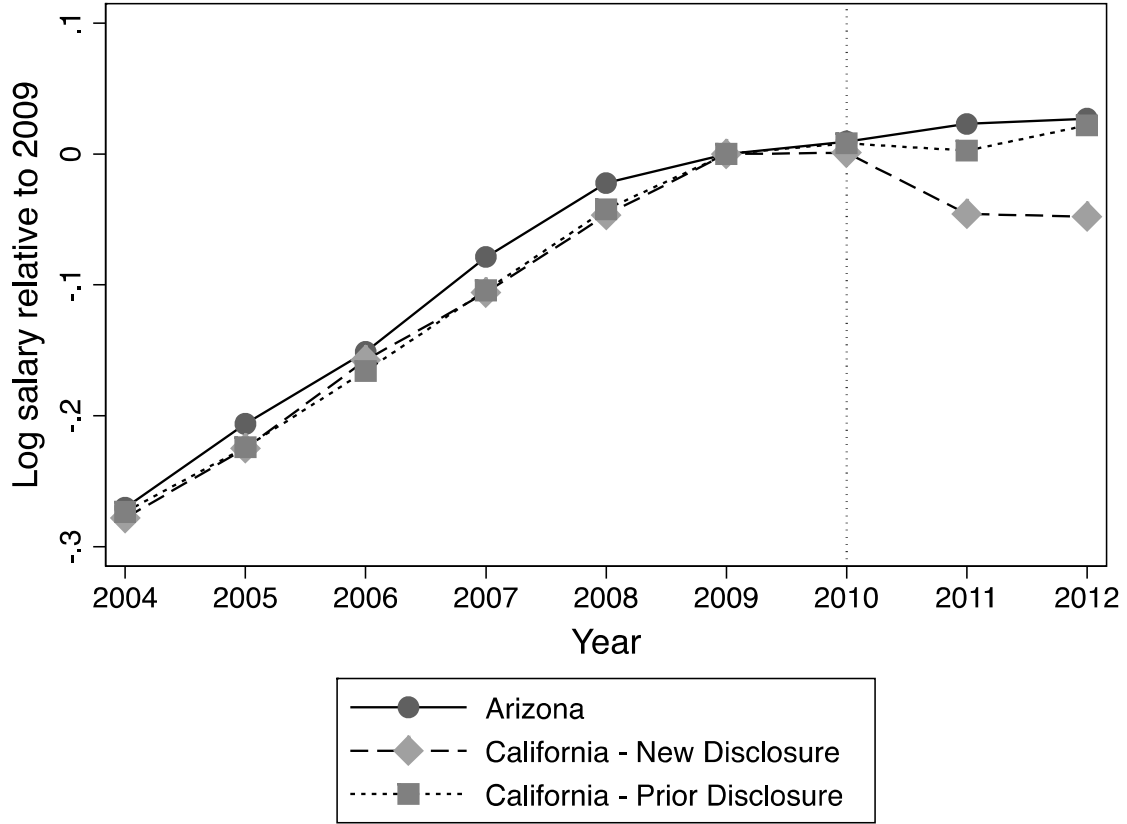
Notes: This figure plots demeaned log nominal compensation of city manager compensation relative to 2009 for the new and prior disclosure cities. The weighted series reweights the new disclosure sample to match the log population, log housing density, log median household income, percent Hispanic, percent black, percent of homes that are renter occupied, unemployment rate, 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 prior disclosure sample. The dotted vertical line is the year that the mandate went into effect.

Figure 3. Manager Earnings Growth; New Disclosure relative to Prior Disclosure (2009=0)



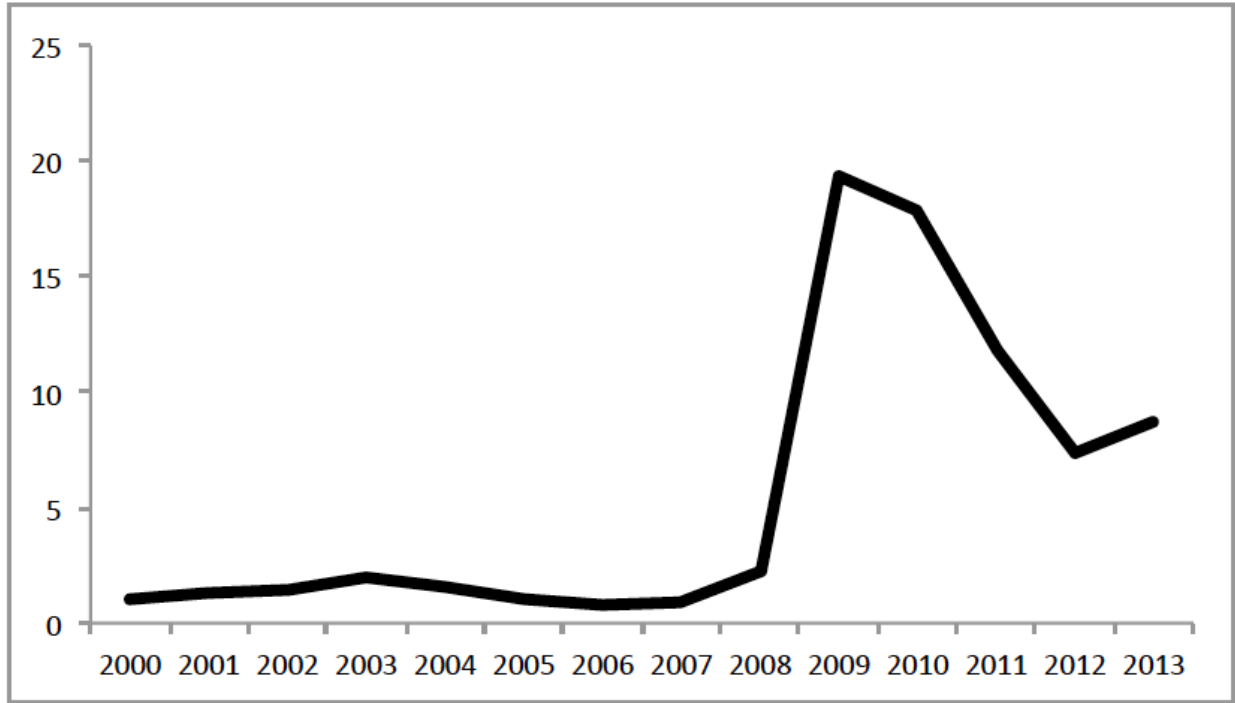
Notes: This figure plots the difference between the average demeaned log city manager salary in new disclosure cities relative to prior disclosure cities. The new disclosure sample is weighted using the weights described in the notes to Figure 1. The dashed lines are 95 percent confidence intervals. The confidence band is larger for earlier years due to smaller sample sizes. The dotted vertical line is the year that the mandate went into effect.

Figure 4. City Manager Earnings Growth; New and Prior Disclosure 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.

Figure 5. 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.

Table 1. Relation between New Disclosure Measure and Media Coverage

	OLS		Negative Binomial Model
	log(Number of articles 2003-2008)	log(Number of articles 2003-2008)	City manager mentions
	(1)	(2)	(3)
New Disclosure	-0.847 (0.112)	-0.334 (0.110)	-1.104 (0.121)
New Disclosure * 2010			0.823 (0.284)
New Disclosure * 2011			0.284 (0.198)
New Disclosure * 2012			0.569 (0.237)
log(Number of articles 2003-2008)			0.559 (0.035)
City characteristics		X	
R-squared	0.108	0.436	
Observations	465	456	5580

Notes: New disclosure 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 NewsLibrary.com database on any topic for years 2003-2008 for the city. City manager mentions is the number of articles that contain terms "City manager" and "Salary" by year and city. The unit of analysis is city in columns (1)-(2) and city\*year in column (3). City characteristics are log full time government employees, log housing density, log population, log average household income, Hispanic share, black share, percent of housing that is renter occupied, percent of residents (25+) that have at most a high school degree, percent employed in construction sector.

Table 2. Summary Statistics

	(1) California New Disclosure	(2) California Previous Disclosure	(3) Arizona	(5) p-value (1)=(2)	(6) p-value (1)=(3)
ln(population)	9.50	10.47	9.34	0.00	0.42
ln((Housing Density)	6.82	7.17	5.60	0.00	0.00
ln(Mean Household Income)	10.88	11.08	10.71	0.00	0.00
ln(Government FTEs)	4.29	5.27	4.79	0.00	0.01
% Hispanic	39.09	30.33	30.97	0.00	0.02
% Black	3.445	3.944	2.07	0.36	0.01
% at most HS Degree	48.13	37.76	46.60	0.00	0.51
% Housing that is Renter Occupied	39.05	38.35	29.24	0.62	0.00
% Employed in construction	7.438	7.101	9.61	0.37	0.00
% Employed in Retail	11.01	11.14	11.92	0.69	0.06
Female Manager (Yes = 1)	0.137	0.121		0.63	
ln(2009 city manager compensation)	12.04	12.26	11.67	0.00	0.00
ln(2012 city manager comp.) -ln(2009 city manager comp.)	-0.116	-0.045	-0.04	0.00	0.00
ln(2012 Wage Bill) -ln(2009 Wage Bill)	-0.209	-0.204		0.79	

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

Table 3. Disclosure and City Manager Salaries

	ln(City Manager Salary)							Online Only Disclosure	ln(Wage Bill)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
New Disclosure * 2010	-0.003 (0.014)	-0.005 (0.013)	0.001 (0.014)	-0.013 (0.013)	-0.012 (0.012)	-0.024 (0.027)	-0.018 (0.014)	-0.017 (0.014)	-0.022 (0.019)
New Disclosure * 2011	-0.045 (0.017)	-0.050 (0.016)	-0.046 (0.017)	-0.059 (0.021)	-0.059 (0.017)	-0.083 (0.041)	-0.055 (0.019)	-0.038 (0.015)	-0.003 (0.023)
New Disclosure * 2012	-0.066 (0.018)	-0.074 (0.017)	-0.060 (0.019)	-0.082 (0.025)	-0.073 (0.021)	-0.095 (0.059)	-0.082 (0.022)	-0.033 (0.018)	0.005 (0.025)
City fixed-effects	X	X	X	X	X	X	X	X	X
City characteristics*year		X	X	X	X	X	X	X	X
County*year			X		X				
City*linear trend				X		X			
2009-2012 sample					X				
Arizona Comparison group						X			
Manager*city fixed-effects							X		
R-squared	0.904	0.910	0.927	0.948	0.956	0.975	0.944	0.910	0.996
Observations	4919	4854	4854	4854	1797	1996	3287	4854	1797

Notes: All models are estimated with OLS. Standard errors clustered on city in parentheses. The unit of analysis is city\*year. Unless otherwise noted, sample spans 2001-2012. In columns (1)-(7) and (9) “New Disclosure” is 1 if the city did not have city manager salary reported in the press between 2003-2008 or on its website in 2008 and 0 otherwise. In column (8) “New Disclosure” is 1 if the city did not report the city manager salary on its website and 0 otherwise. In column (8) the dependent variable is log(city manager salary) and the sample spans 2005-2012. Column (6) includes all Arizona cities and only new disclosure cities in California. The dependent variable in column (9) is the log of total payroll expenditures in the city and the sample is limited to 2009-2012, the years wage bill data are available. City characteristics are log population, log housing density, percent Hispanic, percent black, percent of homes that are renter occupied, unemployment rate, 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.

Table 4. Heterogeneity by 2009 Wage Residual and Gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female		0.091	0.077	0.075	0.118	0.072	0.109
*New Disclosure*1(2012)		(0.039)	(0.039)	(0.041)	(0.055)	(0.039)	(0.053)
1(Residual>median)	0.019						
*New Disclosure*1(2012)	(0.036)						
New Disclosure*1(2012)	-0.076	-0.085	-0.092	-0.081	-0.087		
	(0.026)	(0.019)	(0.019)	(0.021)	(0.028)		
City fixed-effects	X	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	0.91	0.93	0.93	0.94	0.96	0.93	0.96
Observations	4919	3478	3427	3427	3427	3427	3427

Notes: All models are estimated with OLS. Standard errors clustered on city in parentheses. Dependent variable in all specifications is ln(city manager salary in 2012 dollars). The unit of analysis is city\*year. See Table 3 notes for list of city characteristics. In column (1) "Residual" is the residual from a regression of log city manager salary on city characteristics listed in Section V. "Female" denotes a female city manager. All main effects are included in the model, as well as the interactions of 1(Residual>median)\*New Disclosure and Female\*new disclosure with 1(2011) and 1(2010).



Table 5. City Manager Turnover

	(1) Separation	(2) Separation	(3) Separation	(4) Quit	(5) Quit	(6) Quit
New Disclosure * Post	0.186 (0.059)	0.207 (0.067)	0.262 (0.096)	0.151 (0.053)	0.140 (0.060)	0.119 (0.068)
New Disclosure	-0.197 (0.040)	-0.246 (0.045)	-0.240 (0.051)	-0.112 (0.037)	-0.113 (0.041)	-0.087 (0.048)
Post	-0.067 (0.039)			-0.074 (0.033)		
City characteristics * Post		X	X		X	X
County * Post			X			X
Mean Dependent Variable	0.292	0.292	0.292	0.199	0.199	0.199
Observations	930	912	912	930	912	912

Notes: Linear probability models. Standard errors in parentheses. The unit of analysis is city\*time. There are two time periods: 2007-2009 and 2010-2012. "Post" denotes the 2010-2012 period. The dependent variable is 1 if there is at least one separation or quit in the interval. See notes to Table 3 for list of city characteristics.

## **Data Appendix**

### *City Manager Compensation Data*

The compensation data for California come from four sources: a public records request to all California cities, the State Controller public database, salaries recorded in the *Wayback Machine*, and news reports. I prioritize the data sent directly from cities, and when unavailable I supplement the data from the other sources. Whenever possible, I attempted to 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 Medicare 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 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 using the date of hire.

#### *Arizona City and Local Government Employee Payroll Data*

Data on local government employee pay in Arizona comes 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. The Local Government Salary and Benefit Survey 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.<sup>36</sup>

#### *City Characteristics Data*

The city demographic and characteristics data comes from the 2009 American Community Survey (ACS) 5-year estimates. We accessed this survey via the United State Census Bureau's American FactFinder search tool.<sup>37</sup>

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.<sup>38</sup>

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<sup>36</sup> <http://www.azleague.org/>

<sup>37</sup> <http://factfinder2.census.gov/faces/nav/jsf/pages/index.xhtml>

<sup>38</sup> [http://www.census.gov/acs/www/guidance\\_for\\_data\\_users/estimates/](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 comes 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 came from the Selected Social Characteristics file. Information on employment status, industry, income, and poverty level came 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 came from the ACS Demographic and Housing Characteristics file.

#### *Population and Housing Density Data*

The population and housing density data comes 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.

#### *Municipal Finance Data*

Data on municipal finances comes form the U.S. Census Bureau's Census of Governments: Finances. The Census of Governments: Finance covers the entire range of government financial activities (revenue, expenditure, debt, and assets). In years that end with 2

or 7 (2002 and 2007 in our sample), the entire universe is surveyed. In intervening years, a sample of the population is canvassed under the Annual Survey of Government Finances.

In order to access information on cities and towns, I used the individual unit file. For years prior to 2007, this file was publicly available online.<sup>39</sup> For 2007-2011, we obtained city and town level data directly from the United States Census Bureau.

### *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. To identify quits versus other reasons for separation (eg. termination, death) 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).

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.

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<sup>39</sup> <https://www.census.gov/govs/local/>

### *Dropped Cities*

The following cases were omitted in California:

--12 cities where there is no record of a city manager or 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.

--In specifications with controls, 7 cities without city characteristics

--15 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.

The main analysis utilizes 457 California cities without controls, 450 cities with controls, and 84 Arizona cities.