Fiscal Difficulties of Cities, the Labor Market, and Health Care

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Abstract

We investigated possible labor force and health effects from cities in fiscal difficulty, using bond downgrades as a measure of difficulty. We matched 23 cities with downgrades and 31 cities that maintained stable ratings to sampling units in the Medical Expenditure Panel Survey. Using a standard difference-in-difference analysis, we found that in the year of the downgrade and for the three subsequent years, the rate of separation from public employment in the cities with downgrades fell. We found suggestive evidence of an adverse effect on health, but did not find effects on health care use and spending, very likely from a lack of power.

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American cities face fiscal challenges in part from large unfunded future pension and rising retiree health care commitments (Brown, et al. 2011; Novy-Marx and Rauh 2011; Lutz and Sheiner 2014; The Pew Charitable Trusts 2018). Because a large share of spending goes toward public employees, any adjustments to bring these jurisdictions' current spending and future liabilities into better alignment with their revenue seem likely to fall in part on active and retired public employees. For example, 40 percent of local government spending is on elementary and secondary education, and another 6 percent is on police, both of which are labor intensive (Urban Institute 2015). The constitutional protections afforded public employee pensions, however, make adjustments on other margins larger than would otherwise be the case. Exacerbating the situation are rising health insurance costs and premiums; already health insurance for active and retired employees is a major expense for many cities. In this paper we explore possible adjustments to margins related to employment and health insurance and possible effects on health outcomes.

More specifically, we define a group of cities as in fiscal difficulty based on municipal bond rating downgrades and a comparison group of matched cities with stable ratings. In the most extreme case, some of the cities in difficulty have declared bankruptcy, Detroit being a well-known example. We use a standard difference-in-difference analysis to compare active and retired public employees in these two groups of cities before and after the bond rating change on measures of employment status, health insurance status, health care spending and use, and self-rated health status fair or poor, as well as self-rated physical and mental health. We call the group of cities with bond downgrades the "Shock" group and the group with stable ratings the "Control" group. We match the cities in the Shock and Control groups to cities in the Medical Expenditure Panel Survey (MEPS) to obtain data on employment, health care, and health outcomes.

We expect cities and towns under fiscal pressure to potentially alter employment practices and adopt less generous health insurance plans. Public employees may also alter their behavior. Unfortunately theory does not let us sign effects on employment, use of health care services, or health outcomes. Although cities may be more reluctant to hire, workers may be more averse to leaving public employment, especially if stress in the public sector is related to a loss of tax revenue from a decline in the local private sector. With respect to health insurance, less generosity may take the form of a smaller premium subsidy. If the employee continues to purchase insurance, however, theory would suggest only a small income effect on use that would likely not be detectable. Even in those cases in which a smaller subsidy led the employee to drop coverage, the employee in some cases might obtain coverage through am employed spouse, again mitigating the effects.[1]

A bleak fiscal situation could cause a public sector employee to retire sooner than initially planned, potentially moving from the area, and some cities could offer early retirement packages. On the other hand, if the employee thought his or her pension might be less than previously expected or that finding private employment locally was less likely, the employee might remain employed longer than initially planned. If the employee did retire, however, both the monetary and time costs of health care could change, and even if the monetary cost rose, the time cost could fall. Both costs are known to affect use (Phelps and Newhouse 1974; Manning, et al. 1987; Brot-Goldberg, et al. 2017). Change in the cost of time may also affect health habits such as exercise, alcohol consumption, and use of drugs including opioids (Ruhm 2000; Cawley and Ruhm 2012; Ruhm, et al. 2017). Even in the absence of changes in the local labor market or a public employee's insurance status, there could be other pathways through which municipal fiscal difficulty might alter the health status of employees, including increased stress leading to detrimental effects on both physical and mental health.

[1] Our time period is largely before the Affordable Care Act's exchanges, which were introduced in 2014, so we do not account for them.

Leads and lags are also problematic. For example, an employee anticipating a less generous health insurance plan may seek care earlier, at which time an asymptomatic condition may be discovered that is subsequently treated. Theory is of little help in specifying a lag structure.

In short, the overall effects of fiscal difficulty in the public sector on employment and health are not clear from theory. We therefore specified many dimensions that could be affected by fiscal difficulty without prespecifying those we expected to be affected; thus, we regard our analyses as exploratory or hypothesis generating.

We find some evidence of a labor market effect. In the unadjusted results the percentage of public employees annually leaving public employment (retired, quit, fired) in the Control group was reasonably stable in the high teens and was at a roughly similar rate in the Shock group before the bond downgrade, but in the Shock cities it fell around 10 percentage points at the time of the shock and remained at about that amount below the Control cities for the three years following the shock, rebounding in the fourth year. The adjusted results are more ambiguous, likely because, when carrying out the adjusted analysis, we excluded the shock year and the years immediately before and after it as washout years.

With regard to health insurance, we looked at the 9 year window from 4 years before to 4 years after the shock. In the year of the shock and the four subsequent years, the proportion with no insurance at some point in the year was lower in the Shock cities than in the Control cities following the shock. But it was also lower in three of the four years preceding the shock, and the difference is not significant in the adjusted results.

We assumed that the differences in separation rates in the year of the shock and the three subsequent years stemmed from an exogenous event, so we went on to determine if there were effects on a variety of health care use and outcomes. There was no evidence of effects on health care use, spending, or the proportion of various services paid out-of-pocket. One dimension where it appeared that there might be a difference between the Shock and the Control groups was the percentage of the sample that rated itself in fair or poor health at some time during the year, which was greater in the Shock cities after the shock. Assuming this effect on self-rated global health is real, we could not determine if it was operating through poorer physical or poorer mental health.

We believe our analyses suffer from a lack of power. We had only 23 Shock cities and 31 Control group cities in our final sample because of the difficulty of finding data at the city level that would match with downgrade status, and for each city, we had only a few hundred MEPS respondents. Furthermore, to obtain even 23 cities with downgrades, we had to include cities that still had "superior" credit quality after the downgrade. We could not analyze even smaller samples of cities with very low ratings after the downgrade for confidentiality reasons. Since we believe that workers in cities experiencing fiscal difficulties could well exhibit health and medical care effects, both positive and negative, we hope this line of work can be pursued with other approaches or other data to confirm or disconfirm these exploratory analyses.

Methods

Municipal bond ratings from credit rating agencies provide a unified measure of the fiscal health of the municipality at the time of a debt issue and have been shown to be associated with economic, financial, debt, and administrative conditions (Hajek 2011; Palumbo and Zaporowski 2012). Additionally, bond ratings provide a consistent rating system by which to compare cities. This is particularly useful given that uniform financial data for municipalities are scarce. Nonetheless, the ratings are specific to a given debt issue and so are an imperfect proxy for a municipality's financial well-being.

We began identifying cities experiencing fiscal difficulties using downgrades in Moody's bond ratings. Of the three main credit rating agencies, Fitch, Moody's, and Standard & Poor's, we selected Moody's for having the fewest unrated cities and other missing values in our primary dataset. Moody's long-term municipal bond ratings rank from Aaa (highest) to C (lowest); its short-term ratings are in four groups, VMIG1 (highest) to SG. Appendix 1 shows all Moody's ratings with a mapping of the ratings to qualitative descriptions.

We limited our scope to city long-term general obligation bond ratings both for consistency and to have the most direct view possible of municipal finances. There are two primary types of general obligation municipal bonds: General obligation limited tax (GOLT) and general obligation unlimited tax (GOULT) bonds. Ratings for both types of bonds are based on the same underlying methodology, although GOULT bonds may be more stable (Seymour 2014). To maximize our sample, we included cities with either GOLT or GOULT bonds; in those cases where data on both are available, we used GOULT ratings as the more conservative estimate of fiscal difficulties.

We used a three-pronged strategy to identify municipalities with downgrades. First, we identified downgrades in the Moody's general obligation bond ratings using data from the *Statistical Abstract of the United States* (United States Bureau of the Census various years). The *Statistical Abstract* publishes the general obligation bond ratings for the 80 most populous US cities from 1995-2010. These data are reported in the fourth quarter of each year to the Census Bureau. The published bond ratings have a two year lag, and the *Statistical Abstract* stopped publication after 2012; the 2012 edition thus provides bond ratings from 2010, the most recent year in our sample.

Second, we used a press report search on Factiva, a global news and business database operated by Dow Jones & Company, to identify downgrades in cities outside the scope of the *Statistical Abstract*, that is from smaller cities. We conducted a review using the following keywords: city; bond rating; downgrade; general obligation; and Moody's. All keywords were required for a search result to be included. We restricted the date range from January 1, 2000 to December 31, 2015. This search yielded 613 non-duplicated results. We reviewed these 613 reports individually to see if they fit the inclusion criteria of a non-duplicate city general obligation bond with a Moody's credit downgrade. Although press reports are likely to have high sensitivity, their specificity may be low; that is, not all downgrades may be identified in press reports.

Third, we identified cities that had defaulted on municipal bonds using a Moody's Investors Services report (Moody's Investor Services 2017). In the date range of interest, 1995-2015, this report identified defaults and/or bankruptcies in seven municipalities: Detroit, MI; Harrisburg, PA; Mammoth Lakes, CA (unrated default); Moberly, MO (unrated default); San Bernardino, CA (unrated default); Stockton, CA; and Vadnais Heights, MN (unrated default). Which of these cities was sampled in the MEPS is confidential, but not all of them were.

We constructed a comparison group for all the communities with downgrades by matching a group of communities using propensity scores, according to the year of the downgrade, initial bond rating, population, $(\text{population})^2$, and census region. We explicitly did not match based on state because this would have limited our power considerably. We applied weights proportional to the probability of each observation's being in the opposite group (multiplying the MEPS sample weights); this effectively downweighted communities that are not likely to have observable characteristics that overlap with communities in the other group and improved balance (Li, et al. 2007).

Our outcome data come from the MEPS, a large nationally, representative household survey of the civilian, noninstitutionalized population conducted annually since 1996. To align our date range of bond ratings with the availability of data from the MEPS, we restricted our date range of interest to 2000-2015. The MEPS dataset offers several advantages for this type of work. First, it captures consistent data over time for residents in a large sample of US cities with information about each survey subject's current and past employer; thus we are able to identify current local government employees, retirees who formerly worked for cities, and their spouses and dependents. Second, the MEPS survey captures information about each subject's current employment status and employer, health insurance coverage, medical use, and health status, all of which are of interest to us. Third, the survey includes a cross-section of city residents during each wave, with annual waves over multiple years; fortuitously, MEPS captured at least nine years of data on persons in many of our shock cities.[2] Fourth, the MEPS includes information about every member of the household so we could examine the potential effects of fiscal shocks on both public employees (current and retired) and their spouses and dependents.

We linked the data on the cities with bond downgrades, those with stable bond ratings, and the MEPS data using the five-digit zip codes corresponding to each city. This left us with 23 shock and 31 control cities in our final sample. We had to exclude over two-thirds of the full MEPS sample of public employees and dependents because we did not have bond rating data for their cities. Notably, most of these MEPS subjects resided in smaller cities, i.e., not among the most populous 80 cities in the United States.

Our original analysis included all 31 cities with downgrades. This produced many null results, and we therefore focused on a smaller sample of cities that were downgraded to a rating Aa3 or lower. This yielded a sample of 627 employees (current workers or retirees) in the Shock cities and 1,988 in the Control cities. Eliminating the cities with downgrades that left them at ratings above Aa3 reduced the sample size by almost half. Although Aa3 is a relatively high rating, using only cities with ratings of Baa1 or lower ("acceptable credit quality or lower") would have resulted in more than an 80 percent reduction in the original sample size, and power is problematic even with the sample size that we use. Another way to say this is that about 75 percent of the cities with any downgrade remained in the VMIG1 group ("superior credit quality") even after the downgrade. For confidentiality reasons we cannot show the detailed distribution of ratings.

We then conducted an analysis of several outcomes potentially affected by fiscal difficulties. The sample consisted of active and retired public employees in those communities that are common between the communities with rating downgrades, which we term the Shock group, the Control group, and sampling units in those communities that are included in the MEPS. The initial set of outcomes we examined included: separation rates, percentage of persons without health insurance during the year, measures of total medical spending, health insurance status, percentage of medical expense paid out- of-pocket, self-rated health, global measures of physical and mental health from the SF-12, preventive health measures, and a survey measure of consumer satisfaction with health care (Agency for Healthcare Research and Quality 2018).

We defined the fiscal shock as occurring in the year of the bond rating downgrade. As noted above, we used a window from four years before the shock to four years after it. Because of uncertainty about leads and lags, in the difference-in-difference analyses we deleted the year of the Shock and both years on either side of it as washout years. The elimination of years surrounding the bond downgrade focused our analyses on more persistent changes potentially associated with the shock and excluded more transient effects. Because the definition of the timing of the initial shock could vary as could the timing of potential municipal responses, we

[2] The MEPS periodically adjusts its sampling frame, i.e., the cities it samples from. This caused an additional loss of sample since some cities with downgrades were not observed for a sufficient number of years before and after the downgrade.

also examined annual raw differences between the treatment and control groups for hints of anticipatory and lagged effects.

The analyses we present include only those with the sample of active and retired public employees. We also examined a sample that included dependents, but those results were similar and we do not show them. In the health services domain some of the spouses and dependents could have been covered by a spouse's insurance policy, which would dilute our power.

All standard errors and statistical tests account for clustering at the city level. We alternatively computed these tests using the MEPS PSU/cluster structure, but found similar results largely because clustering at the city level is a close approximation.

Results

Table 1 shows the balance between the Shock and Control groups for the analytic sample of employees and retirees. Despite the effort to match, the Shock cities have a notably higher proportion of females and blacks and a smaller proportion of whites, have a less well educated population, and are disproportionately in the Midwest. Most of the difference between the unweighted sample and weighted sample stems from application of the MEPS sampling weights; after applying the MEPS sampling weights the additional propensity score matching did not much change the numbers shown in Table 1 (not shown). We will show both raw (unadjusted) annual results and regressions with the variables in Table 1 as covariates.

Although the raw rate of public employee separation (retired, quit, fired) was reasonably constant in the Control group and reasonably similar in the Shock group prior to the shock, it fell roughly 10 percentage points in the Shock group at the time of the shock and remained at roughly that lower level for the following three years, rebounding in year 4 (Figure 1). As seen in the confidence bars in Figure 1, however, we had limited precision in our estimates of the Shock group.

Table 2 shows difference-in difference results for the rate of separation. Not surprisingly given the rebound in separation rates in the fourth year after the shock, the difference-in-difference results are sensitive to the inclusion of that year, so Table 2 shows one panel that compares years two and three after the shock with years two and three before the shock and another panel that adds in the fourth year before and after the shock. The results are marginally significant omitting the fourth year and undoubtedly would be much more so if we had not excluded the three washout years from the sample.

After the shock there was a lower raw rate of persons who were uninsured for part of the year in the Shock group (Figure 2), but the difference-in-difference analyses show no measurable effect, suggesting the differences in the raw rate are attributable to imbalances between the Shock and Control groups, especially the differences in the proportion of minorities and the regional differences (Table 3).

Table 4 shows that there could be a difference in those reporting themselves to be in fair or poor health after the shock. The sample for Table 4 is cities with downgrades to Aa3 or lower, but the result on poor or fair health shown in Table 4 is little changed by using the full sample of 31 cities with downgrades. The result, however, does not appear if dependents are included in the sample (not shown). We went on to see if this effect could be further narrowed to differences in physical or mental health, but did not see effects (Tables 5 and 6).

We looked at a variety of effects on use and spending, but no effects emerged that would survive a multiple comparison correction (Table 7).

Discussion

During the years 2000 to 2015, 31 US cities experienced drops in their bond ratings and a few declared bankruptcy. We found that this fiscal shock to a municipality appears to result in a lower rate of separation from public employment in the year of the shock and the three years following it. Any drop in separation from a steady state rate should be transitory, so the change in the raw rate in fourth year after the shock back to something that resembles a steady state rate seems plausible. We also found a lower raw rate of having a spell of uninsurance during a year among employees in the Shock group. Although consistent with a lower rate of separation from public employment, that result appears attributable to observable differences between the two samples. In addition, there is some evidence of a higher rate of those employees and retirees assessing themselves to be in fair or poor health in the Shock group compared with the Control group. We examined several other measures of health care use and spending and health outcomes, however, and did not find any strong effects. Nor did we find effects when we included dependents.

We do not regard these mainly null results as necessarily the absence of effects but rather the likely lack of power to detect a true effect. Our analytic sample of cities was limited to 23 cities in the Shock group and 31 in the Control, and the post-downgrade bond rating in many of the cities in the Shock group still left them with ratings that Moody's considered to be "superior" credit quality. If there is another economic downturn, there may be more cities with severe fiscal problems that can shed more light on the questions we sought to answer. Moreover, as more cities face their pension obligations and experience rising health insurance premiums, we anticipate that the numbers of cities in fiscal difficulty and the degree of difficulty could increase. Arguably, the pension accounting standards change in 2012 should lead to more accurate reporting, with potentially deleterious effects for those cities with overly optimistic estimates of their obligations.

The current and former employees and dependents of cities in fiscal difficulty almost certainly will bear some of the responses to this pressure. To the extent that there are clear pathways between these fiscal shocks and health, there could be opportunities for policy interventions. For example, if we had observed large increases in early retirement and decreases in health insurance coverage, then policies facilitating transitions into private insurance markets, for example, limited premium differential age bands or risk adjustment modifications, could help mitigate any adverse health effects.

Future efforts to examine the health and labor impact of municipal fiscal difficulty could benefit from more refined measures of fiscal difficulty and certainly from larger samples. As noted earlier, our bond rating data was mainly limited to the 80 largest US cities, which resulted in the exclusion of the majority of public employees in the MEPS sample, because most of the MEPS sample resides in smaller cities. Moreover, most of the 80 largest cities did not experience downgrades, limiting the size of the Shock group. While the bond ratings provide a consistent measure of fiscal health across cities and time, they also blur the range of causes that could increase the likelihood of heterogeneous effects, e.g., some cities may have made poor investment decisions whereas others faced more insidious revenue challenges. Moreover, the type and nature of the reason for the bond rating change also could impact the timing of the true shock to cities, e.g., in some cases with more insidious fiscal challenges, credit rating agencies provide early warnings of potential bond rating changes to cities, which then could implement changes or not before any rating change.

More detailed linked data on municipal behavior, health insurance decisions, and medical outcomes also could be valuable. For example, the household portion of the MEPS survey, which we used, offered limited information on the generosity of health insurance offered over time by cities, e.g., changes in actuarial value, benefit design, provider networks, or cost-sharing amounts for specific services. The insurance portion of the survey provides such details and could be analyzed.[3] Conceivably, cities might be more likely to alter insurance generosity than to reduce employment (except through attrition) in response to fiscal shocks. As noted earlier, however, the observed employment and insurance decisions also reflect decisions by employees and households. And in some cases, there could be relevant third or fourth parties, e.g., unions or spouses, which would influence the ability or timing of city changes or the responses by employees. Increases in the amounts of data on these variables could improve future work.

A major limitation of the current work is the lack of precision in our estimates, which reflected in large part the relatively small sample sizes available on public employees in cities where we had available bond-rating data. As discussed above, a more complete set of bond-ratings might identify public employees in additional cities that could be included in both the Shock and Control groups. Including more years of MEPS data as more cities experience difficulties over time could also be helpful. Larger samples with more information at the city and individual levels also could improve the comparability of the Control group. Other national datasets, including the Current Population Survey, the American Community Survey, or the Health and Retirement Survey might also be helpful in examining certain outcomes. Claims data from aggregators such as IBM/Truven alone or in combination with state-based entities, e.g., CALPERS, also could increase the sample Claims data may be particularly helpful for estimating the effects of fiscal shock on health care utilization and spending, which owing to their skewed nature, are particularly sensitive to sample size.

Conclusion

In sum, we examined the potential labor market and health outcomes following a fiscal shock to cities in the United States. We found a number of cities that experienced bond rating downgrades in recent years, and expect that these numbers will increase in future years. Using linked data from bond ratings and the MEPS household survey, we found likely lower rates of separation from public employment in these cities compared with cities with stable bond ratings. We also found some evidence of adverse effects on health status. To reach more definitive answers to these questions, however, will require better data lest the search under the proverbial lamppost leaves too much in the shadows.

[3] This sample is less readily available, however.

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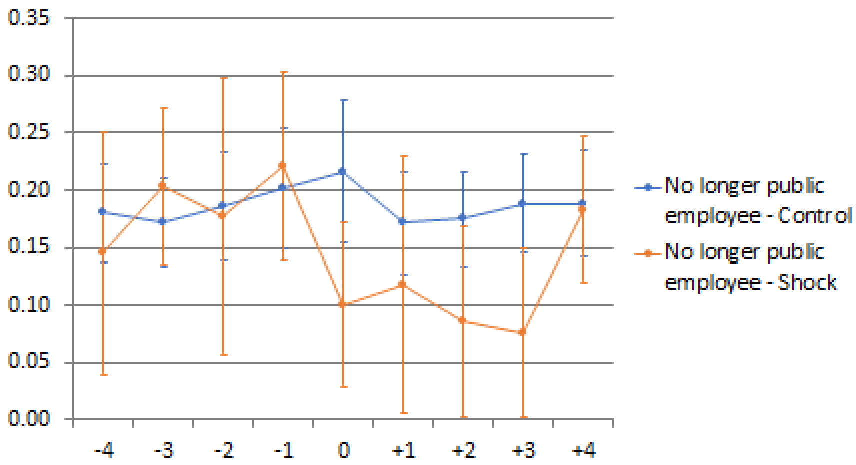
Table 1. Control vs. Shock Group– Public Employees & Retirees

	Propensity Score Weighted				**Unweighted**			
	CONTROL n=1988		SHOCK n=722		CONTROL n=1988		SHOCK n=722	
	Mean	Std. Error	Mean	Std. Error	Mean	Std. Error	Mean	Std. Error
Age (years)	48.09	1.09	47.93	0.97	47.57	0.81	47.13	0.84
Female	0.62	0.02	0.52	0.03	0.62	0.02	0.55	0.01
Race/Ethnicity								
Hispanic	0.12	0.03	0.13	0.03	0.21	0.05	0.21	0.06
Black	0.28	0.04	0.38	0.08	0.37	0.04	0.46	0.10
White	0.55	0.05	0.43	0.06	0.35	0.05	0.28	0.05
Education Level								
Less than HS	0.08	0.01	0.09	0.02	0.13	0.02	0.12	0.02
HS diploma	0.24	0.04	0.28	0.05	0.26	0.03	0.30	0.05
Some college	0.22	0.03	0.22	0.03	0.23	0.02	0.24	0.02
Bachelor's	0.25	0.02	0.19	0.02	0.21	0.01	0.17	0.02
≥ Masters	0.21	0.02	0.21	0.03	0.17	0.02	0.18	0.03
Married	0.48	0.03	0.47	0.04	0.49	0.03	0.47	0.03
Region								
Northeast	0.17	0.11	0.19	0.08	0.12	0.08	0.15	0.07
Midwest	0.16	0.07	0.27	0.14	0.14	0.06	0.29	0.16
South	0.40	0.10	0.32	0.17	0.43	0.11	0.33	0.18
West	0.27	0.09	0.22	0.14	0.32	0.11	0.23	0.14
Census population	1,031,022	181,920	850,956	266,769	1,199,592	238,141	953,855	315,642

Figure 1. No Longer a Public Employee



NOTE: Includes those who retired, were fired, or quit during the previous year

Figure 2. Any Uninsured Spell During The Year

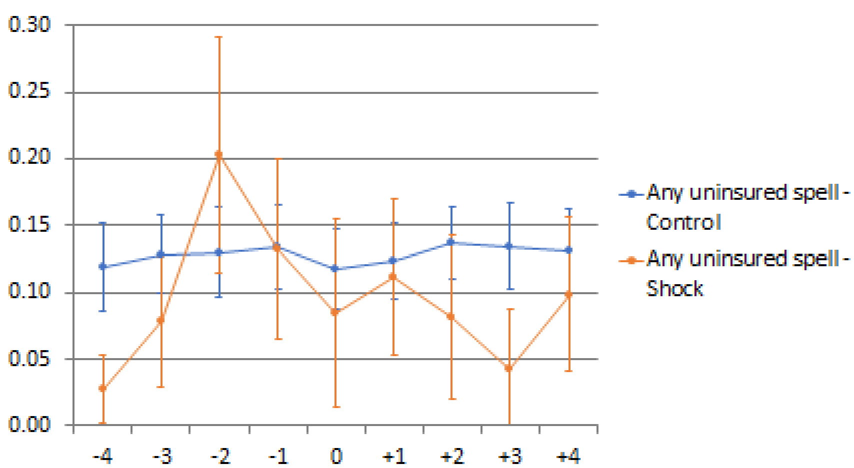
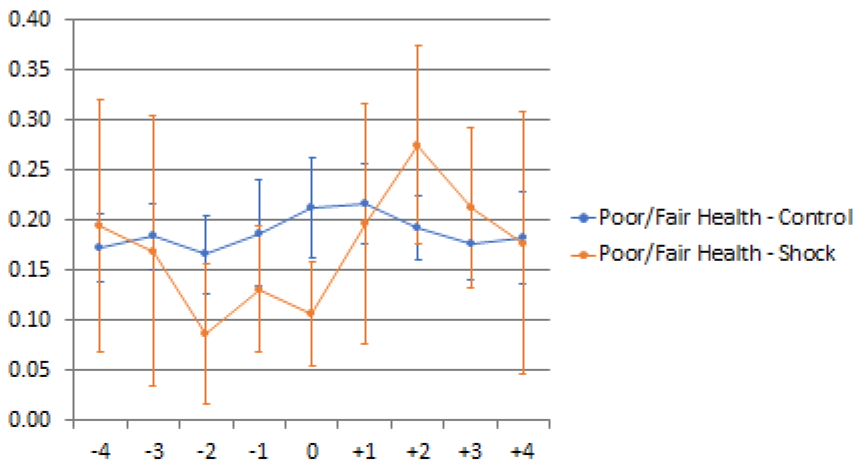


Figure 3. Poor/Fair Self-reported Health During The Year



NOTE: Reports Poor/Fair health at any point in previous year

Figure 4. Self-reported Physical Health Score (SF-12 PCS, Higher Scores=Better)

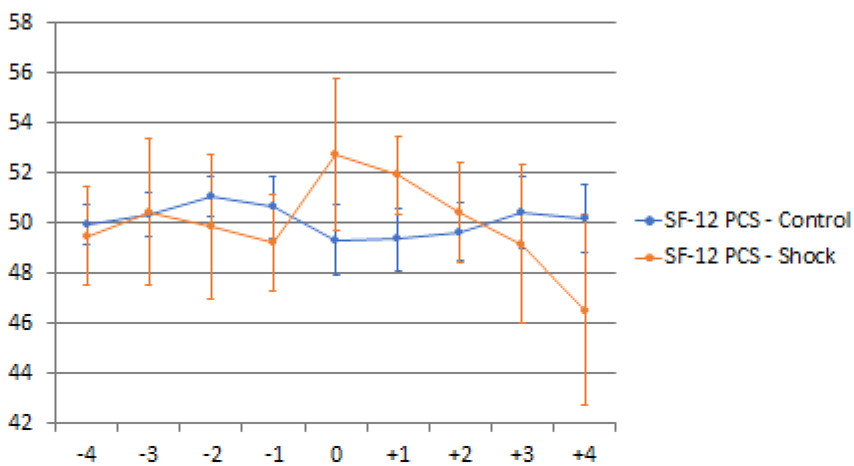
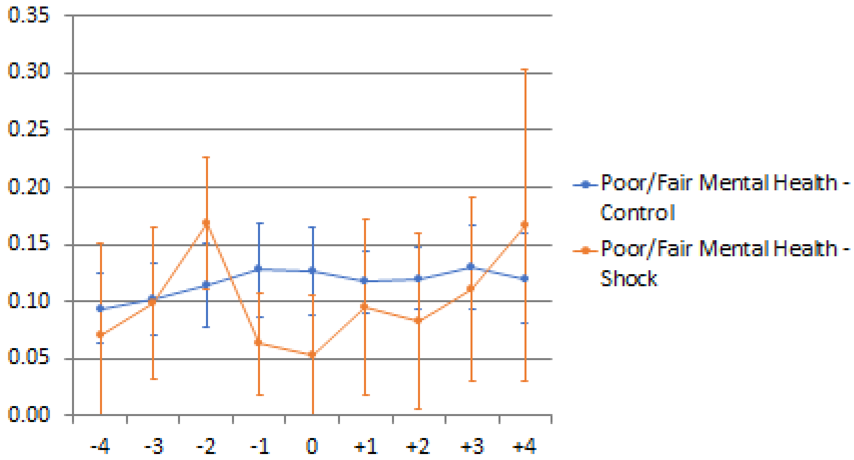


Figure 5. Poor/Fair Self-reported Mental Health During The Year



NOTE: Reports Poor/Fair mental health at any point in previous year

Figure 6. Self-reported Mental Health Score (SF-12 MCS, Higher Scores=Better)

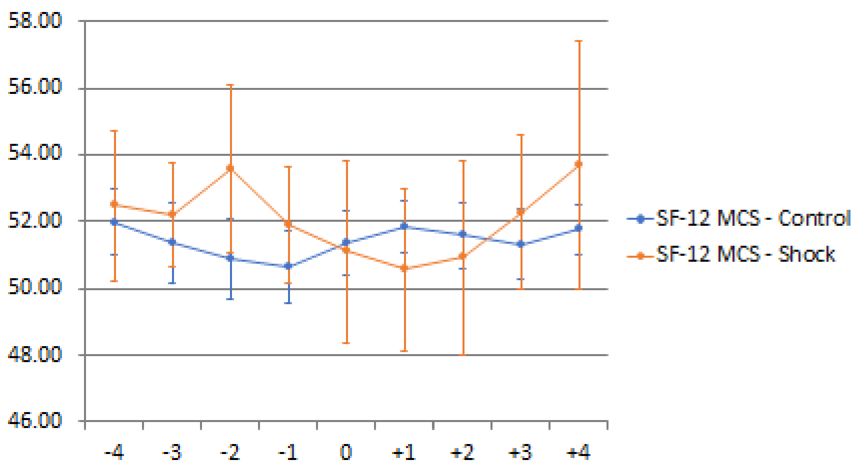


Table 2
No Longer Public Employee

	No Longer Public Employee, Years -3, -2, +2, +3*				**No Longer Public Employee, Years -4, -3, -2, +2, +3, +4***			
	Coefficient	Std. Error	t	P>|t|	Coeff	Std. Error	t	P>|t|
1.post	-0.006	0.006	-0.920	0.360	0.012	0.012	-1.090	0.281
1.treat	0.008	0.044	0.170	0.864	0.038	0.038	-0.270	0.788
Post*treat	**-0.089**	**0.049**	**-1.810**	**0.075**	**-0.053**	**0.049**	**-1.090**	**0.282**
Age	-0.005	0.001	-3.850	0.000	-0.005	0.001	-4.75	0.000
Hispanic	0.031	0.043	0.740	0.464	0.024	0.039	0.620	0.536
Black	0.008	0.030	0.250	0.804	0.008	0.029	0.280	0.778
Female	0.014	0.028	0.500	0.616	0.017	0.027	0.630	0.529
HS diploma	-0.135	0.037	-3.660	0.001	-0.136	0.040	-3.430	0.001
Some college	-0.034	0.052	-0.650	0.520	-0.059	0.047	-1.260	0.215
Bachelors	-0.178	0.038	-4.690	0.000	-0.176	0.043	-4.090	0.000
Masters or higher	-0.121	0.042	-2.860	0.006	-0.127	0.040	-3.130	0.003
Married	-0.065	0.031	-2.140	0.037	-0.056	0.027	-2.060	0.044
Region								
Midwest	0.081	0.055	1.490	0.143	0.060	0.060	1.290	0.202
South	0.022	0.044	0.490	0.624	0.050	0.050	0.290	0.775
West	-0.032	0.046	-0.700	0.487	0.049	0.049	-0.710	0.482
Year	0.002	0.004	0.470	0.637	0.003	0.003	1.030	0.306
constant	-2.877	7.117	-0.400	0.688	5.465	5.465	-0.940	0.352
R-squared	0.1024				0.0923			
N	3134				4642			
				NO CONTROLS				
1.post	0.003	0.016	0.210	0.836	0.005	0.017	0.280	0.778
1.treat	0.012	0.047	0.250	0.805	-0.001	0.040	-0.020	0.985
Post*treat	**-0.113**	**0.054**	**-2.090**	**0.041**	**-0.080**	**0.051**	**-1.550**	**0.127**
constant	0.178	0.021	8.690	0.000	0.179	0.019	9.290	0.000
R-squared	0.0473				0.0009			

*In both panels the years -1, 0, +1, where 0 is the year of the shock, are omitted. Standard errors and statistical tests adjust for clustering at the city level.

Table 3
Uninsured at Any Time During the Year*

	Coeffient	Std. Err.	t	P>|t|
1.post	0.002	0.009	0.180	0.859
1.treat	-0.018	0.019	-0.980	0.333
Post*treat	**-0.018**	**0.035**	**-0.510**	**0.611**
Already retired	0.033	0.036	0.920	0.361
Age	-0.005	0.001	-5.660	0.000
Hispanic	0.087	0.026	3.340	0.002
Black	0.064	0.032	2.030	0.048
Female	0.011	0.023	0.470	0.642
HS diploma	0.012	0.038	0.320	0.754
Some college	0.043	0.040	1.090	0.281
Bachelors	0.013	0.037	0.360	0.720
Masters or higher	-0.006	0.035	-0.160	0.873
Married	-0.018	0.023	-0.790	0.435
Region				
Midwest	-0.059	0.016	-3.680	0.001
South	0.012	0.019	0.630	0.530
West	-0.003	0.026	-0.100	0.922
Year	0.001	0.002	0.290	0.769
Constant	-1.110	4.839	-0.230	0.819
R-squared	0.0817			
N	5681			
NO CONTROLS				
1.post	0.008	0.012	0.720	0.474
1.treat	-0.016	0.029	-0.550	0.584
Post*treat	**-0.048**	**0.040**	**-1.180**	**0.243**
Constant	0.126	0.013	9.390	0.000
R-squared	0.0009			

*The years -4, -3, -2, +2, +3, +4 are included; the years -1, 0, +1, where 0 is the year of the shock, are omitted as washout years.

Standard errors and statistical tests adjust for clustering at the city level.

Table 4
In Fair or Poor Health at Any Time During the Year*

	Coeffient	Std. Err.	t	P>|t|
1.post	0.009	0.011	0.800	0.428
1.treat	-0.051	0.038	-1.340	0.186
Post*treat	**0.089**	**0.035**	**2.570**	**0.013**
family member already retired	0.111	0.056	2.000	0.051
age	0.002	0.001	2.050	0.046
hispanic	0.062	0.039	1.570	0.122
black	0.024	0.039	0.610	0.545
female	-0.028	0.031	-0.900	0.375
hs diploma	0.004	0.052	0.080	0.934
some college	-0.092	0.051	-1.790	0.079
bachelors	-0.136	0.053	-2.560	0.013
masters or higher	-0.136	0.051	-2.640	0.011
married	-0.086	0.026	-3.290	0.002
region				
Midwest	0.028	0.041	0.690	0.490
South	0.027	0.036	0.760	0.452
West	0.009	0.042	0.210	0.836
year	-0.001	0.003	-0.420	0.676
constant	2.548	5.629	0.450	0.653
R-squared	0.0767			
N	5681			
NO CONTROLS				
1.post	0.010	0.017	0.580	0.561
1.treat	-0.028	0.044	-0.640	0.526
Post*treat	**0.072**	**0.040**	**1.810**	**0.076**
constant	0.174	0.015	11.480	0.000
R-squared	0.0006			

*The years -4, -3, -2, +2, +3, +4 are included; the years -1, 0, +1, where 0 is the year of the shock, are omitted as washout years.
Standard errors and statistical tests adjust for clustering at the city level.

Table 5
SF-12 Self-Rated Physical Health*

	Coeffient	Std. Err.	t	P>|t|
1.post	-0.675	0.321	-2.100	0.040
1.treat	-0.352	0.806	-0.440	0.664
Post#treat	**-0.156**	**0.944**	**-0.160**	**0.870**
already retired	-2.863	2.026	-1.410	0.164
age	-0.231	0.032	-7.220	0.000
hispanic	-0.268	1.096	-0.240	0.808
black	0.606	0.927	0.650	0.516
female	-0.834	0.553	-1.510	0.138
hs diploma	0.663	1.088	0.610	0.545
some college	1.871	1.106	1.690	0.097
bachelors	3.893	1.205	3.230	0.002
masters or higher	5.267	0.961	5.480	0.000
married	1.301	0.767	1.700	0.096
region				
Midwest	0.905	1.307	0.690	0.492
South	0.030	1.274	0.020	0.981
West	-0.322	1.385	-0.230	0.817
year	0.120	0.064	1.860	0.069
constant	-180.694	128.976	-1.400	0.167
R-squared	0.2451			
N	5139			
NO CONTROLS				
1.post	-0.323	0.587	-0.550	0.584
1.treat	-0.462	1.114	-0.420	0.680
Post*treat	**-0.569**	**1.366**	**-0.420**	**0.678**
constant	50.433	0.350	143.970	0.000
R-squared	0.0006			

Note: *The years -4, -3, -2, +2, +3, +4 are included; the years -1, 0, +1, where 0 is the year of the shock, are omitted as washout years.
Standard errors and statistical tests adjust for clustering at the city level.

Table 6
SF-12 Self-Rated Mental Health*

	Coeffient	Std. Err.	t	P>|t|
1.post	0.102	0.362	0.280	0.779
1.treat	1.053	0.714	1.470	0.146
Post*treat	**-1.001**	**0.772**	**-1.300**	**0.201**
already retired	-2.051	1.225	-1.670	0.100
age	0.097	0.028	3.480	0.001
hispanic	0.327	1.073	0.300	0.762
black	0.421	0.771	0.550	0.587
female	-1.599	0.779	-2.050	0.045
hs diploma	1.055	1.172	0.900	0.372
some college	0.611	1.083	0.560	0.575
bachelors	1.460	1.495	0.980	0.333
masters or higher	0.526	1.351	0.390	0.699
married	1.626	0.796	2.040	0.046
region				
Midwest	0.185	1.459	0.130	0.900
South	1.166	1.388	0.840	0.405
West	0.476	1.321	0.360	0.720
year	0.019	0.070	0.280	0.782
constant	6.803	139.674	0.050	0.961
R-squared	0.0466			
N	5144			
NO CONTROLS				
1.post	0.137	0.378	0.360	0.719
1.treat	1.405	0.817	1.720	0.092
Post*treat	**-0.899**	**0.751**	**-1.200**	**0.237**
constant	51.414	0.474	108.380	0.000
R-squared	0.0005			

*The years -4, -3, -2, +2, +3, +4 are included; the years -1, 0, +1, where 0 is the year of the shock, are omitted as washout years.

Standard errors and statistical tests adjust for clustering at the city level.

Table 7
Raw Health Care Utilization and Spending Results*

	mean	t-value	p-value
Retired before start of year	0.00	0.03	0.97
No longer public employee (retired, fired, quit during year)	-0.09	-1.73	0.09
Personal income	-4530	-1.05	0.30
Family income	641	0.09	0.93
Pension income	1256	0.70	0.49
Any uninsured spell	-0.06	-1.52	0.13
# of months uninsured	-0.43	-1.36	0.18
Reports dental coverage	-0.04	-0.62	0.53
Utilization & Spending			
Has usual source of care	0.08	3.08	0.00
Any office-based/opd tx	0.09	1.57	0.12
Any prescription rx fills	0.07	1.52	0.13
Any dental visits	0.19	2.65	0.01
# ambulatory visits	0.69	0.53	0.60
# rx fills	5.40	2.18	0.03
# dental visits	0.45	2.21	0.03
Total expenditures	-1011	-0.60	0.55
Ambulatory expenditures	-241	-0.56	0.58
RX expenditure	69	0.07	0.94
Dental expenditures	61	0.59	0.56
Total $ out of pocket (OOP)	47	0.17	0.87
Ambulatory $ OOP	-88	-0.56	0.58
RX $ OOP	41	0.25	0.80
dental $ OOP	41	0.54	0.59
% paid OOP all expenditures	-0.02	-0.74	0.46
% paid OOP ambulatory	-0.03	-0.53	0.60
% paid OOP RX	-0.06	-1.30	0.20
% paid OOP dental	0.02	0.25	0.81
Health Status			
Poor/fair health (any time in year)	0.06	1.56	0.13
Poor/fair/good health vs. excellent/very good	-0.03	-0.41	0.68
Poor/fair mental health	-0.04	-0.82	0.41
Poor/fair/good mental health	-0.07	-1.15	0.26
Prevention			
SF-12 PCS (higher better)	-0.61	-0.44	0.66
SF-12 MCS (higher better)	0.12	0.12	0.90
Had pap in last year (female, age>17)	-0.06	-0.56	0.58
Had pap in last 3 years (female, age>17)	-0.10	-1.17	0.25
Had mammogram past 1 year (female, age>39)	-0.03	-0.27	0.79
Had mammogram past 3 years (female, age>39)	0.00	0.04	0.96
Had PSA in last year (male, age>39)	-0.05	-0.70	0.49
had PSA in last 3 years (male, age>39)	-0.02	-0.26	0.80
Flu shot in last year (age>17)	0.01	0.16	0.87
CAHPS Satisfaction 1-10	0.52	2.47	0.02

**All income amounts are adjusted by the CPI; all expenditure amounts are adjusted by CMS PHC medical inflation index (overall for total spending, components of spending for the others)The years -4, -3, -2, +2, +3, +4 are included; the years -1, 0, +1, where 0 is the year of the shock, are omitted as washout years.

Standard errors and statistical tests adjust for clustering at the city level.

Appendix 1
Moody's Bond Ratings

VMIG Scale

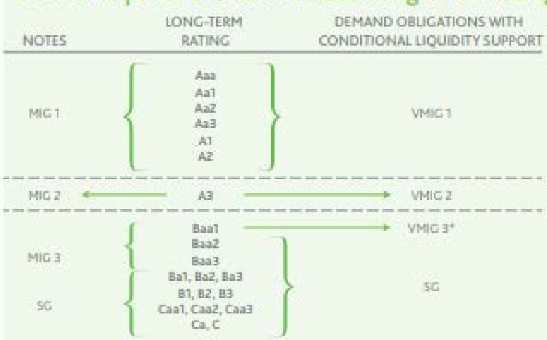
VMIG 1	This designation denotes superior credit quality. Excellent protection is afforded by the superior short-term credit strength of the liquidity provider and structural and legal protections that ensure the timely payment of purchase price upon demand.
VMIG 2	This designation denotes strong credit quality. Good protection is afforded by the strong short-term credit strength of the liquidity provider and structural and legal protections that ensure the timely payment of purchase price upon demand.
VMIG 3	This designation denotes acceptable credit quality. Adequate protection is afforded by the satisfactory short-term credit strength of the liquidity provider and structural and legal protections that ensure the timely payment of purchase price upon demand.
SG	This designation denotes speculative-grade credit quality. Demand features rated in this category may be supported by a liquidity provider that does not have an investment grade short-term rating or may lack the structural and/or legal protections necessary to ensure the timely payment of purchase price upon demand.

* For VRDBs supported with conditional liquidity support, short-term ratings transition down at higher long-term ratings to reflect the risk of termination of liquidity support as a result of a downgrade below investment grade.

VMIG ratings of VRDBs with unconditional liquidity support reflect the short-term debt rating (or counterparty assessment) of the liquidity support provider with VMIG 1 corresponding to P-1, VMIG 2 to P-2, VMIG 3 to P-3 and SG to not prime.

For more complete discussion of these rating transitions, please see Annex B of Moody's Methodology titled Variable Rate Instruments Supported by Conditional Liquidity Facilities.

US Municipal Short-Term Versus Long-Term Ratings



* For SBPA-backed VRDBs, The rating transitions are higher to allow for distance to downgrade to below investment grade due to the presence of automatic termination events in the SBPAs.

From https://www.moodys.com/researchdocumentcontentpage.aspx?docid=PBC_79004

Appendix 2
Balance Including Dependents and Full Population of Shock and Control Cities

Appendix Table 2a. Control vs. Shock Group– Public Employees, Retirees & Family Members

	CONTROL n=4891		SHOCK n=1616	
	Mean	Std. Error	Mean	Std. Error
Age (years)	37.21	1.07	37.79	1.27
Female	0.53	0.01	0.49	0.02
Race/Ethnicity				
Hispanic	0.14	0.03	0.17	0.05
Black	0.28	0.04	0.40	0.10
White	0.53	0.05	0.37	0.05
Education Level				
Less than HS	0.26	0.01	0.27	0.03
HS diploma	0.19	0.02	0.23	0.03
Some college	0.19	0.02	0.18	0.02
Bachelor’s degree	0.17	0.01	0.13	0.01
Masters or higher	0.12	0.02	0.12	0.02
Married	0.39	0.01	0.39	0.02
Region				
Northeast	0.16	0.10	0.17	0.08
Midwest	0.17	0.07	0.25	0.15
South	0.40	0.10	0.36	0.18
West	0.27	0.09	0.22	0.14
Census population	1,026,894	177,635	907,092	292,388

Appendix Table 2a. Control vs. Shock Group – Full Population

	CONTROL n=4891		SHOCK n=1616	
	Mean	Std. Error	Mean	Std. Error
Age (years)	35.64	0.52	36.22	0.49
Female	0.51	0.00	0.52	0.01
Race/Ethnicity				
Hispanic	0.22	0.04	0.22	0.04
Black	0.20	0.02	0.32	0.07
White	0.50	0.04	0.38	0.04
Education Level				
Less than HS	0.29	0.02	0.32	0.01
HS diploma	0.20	0.01	0.24	0.02
Some college	0.18	0.01	0.18	0.01
Bachelor’s degree	0.15	0.01	0.11	0.01
Masters or higher	0.08	0.01	0.07	0.01
Married	0.33	0.01	0.29	0.01
Region				
Northeast	0.14	0.09	0.17	0.07
Midwest	0.15	0.06	0.29	0.14
South	0.41	0.11	0.29	0.14
West	0.29	0.09	0.26	0.15
Census population	1,108,100	204,770	856,715	269,900