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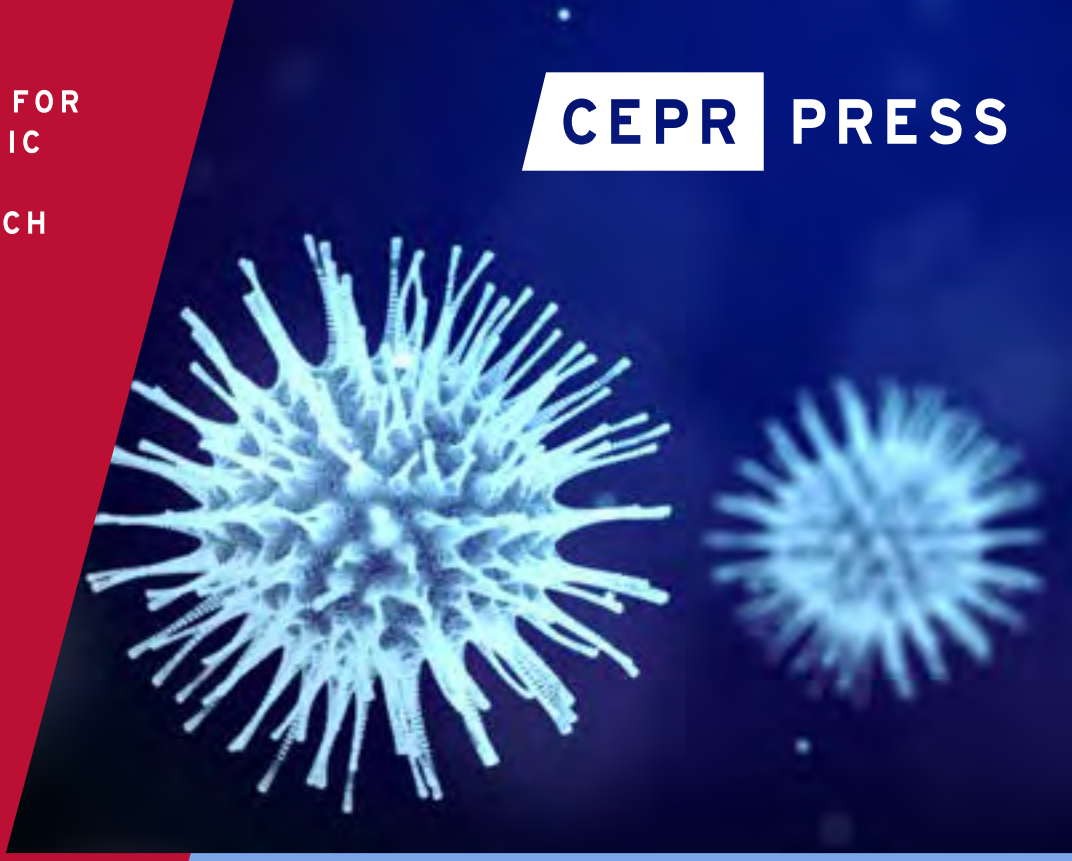

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Sectoral Employment Effects of State Fiscal Relief: Evidence from the Great Recession, Lessons for the Covid-19 Crisis

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This paper documents that the employment effects of financial aid to U.S. states during the Great Recession were strongly unevenly distributed across sectors. We show that state fiscal relief had a double dividend: not only did it preserve a substantial number of jobs, but it also fostered employment most strongly in the sectors hit hardest by the recession. We exploit differences in the distribution of recessionary job losses across states to draw conclusions for the Covid-19 recession. Our results suggest that the double dividend of state fiscal relief cannot be taken for granted.

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1 Introduction

For state governments in the U.S., the Covid-19 crisis has led to precipitous declines in revenues and soaring expenses. Since states are effectively required by law to run balanced budgets, many of them will have to slush costs or raise taxes if not supported financially by the federal government. Spending contractions or tax hikes on the part of state governments are likely to deepen the economic downturn further. Accordingly, there are prominent calls for Congress to provide financial help to state governments beyond the $150 billion Coronavirus Relief Fund established in the Coronavirus Aid, Relief, and Economic Security (CARES) Act. The National Governors Association requested an additional $500 billion in federal aid. The Coronavirus supplemental spending bill proposed by the Democratic majority in the House of Representatives in May 2020 includes $1 trillion in funding for state and local governments. While Fed Chairman Jerome H. Powell warned that leaving state governments fight for themselves would make the economic crisis worse, extending the federal aid to state governments faces powerful opposition from, e.g., Senate majority leader Mitch McConnel and President Donald Trump.

In this paper, we seek to learn from the Great Recession about the labor-market consequences of state fiscal relief and to draw conclusions for the Covid-19 crisis. During the Great Recession, state fiscal relief was one of the major components of the American Recovery and Reinvestment Act (ARRA), the around $800 billion fiscal stimulus package signed by President Obama in February 2009. The spending component of the ARRA stimulus (which also included about $350 billion in transfers and tax cuts) was largely channeled through state and local governments who received close to $250 billion from the federal government. A considerable fraction of this money was explicitly intended to relax the strain on states' budgets, and almost all transfers were fungible, i.e., states could effectively use the money as they wished (Chodorow-Reich et al., 2012, Conley and Dupor, 2013). Most transfers to states took the form of relieving state governments from payment obligations, either through increasing federal spending shares in, e.g., Medicaid, or through waiving states' cost shares in (e.g., infrastructure) projects financed by the federal government. In both cases, the respective funds effectively increased states' budgetary leeway. State fiscal relief has also been implemented in the course of previous recessions, but the context of the Great Recession is

particularly suited to learn about its effects due to the detailed documentation of the outlays.[1]

Chodorow-Reich et al. (2012), Wilson (2012), Conley and Dupor (2013), and Chodorow-Reich (2019), among others, have shown that the financial transfers to state governments implemented in the ARRA had positive employment effects, including substantial effects in the private economy. In this paper, we look beyond this aggregate effect and study its distribution across sectors. Specifically, we investigate to what extent state fiscal relief fostered employment in those sectors that had been hit hardest by the economic downturn. This is important because it is difficult for workers to switch industries (Weinberg, 2001; Artuç and McLaren, 2015). As a consequence, promoting employment is particularly valuable in sectors that have been affected severely by the crisis because this will improve the labor-market prospects of the hardest-hit groups of workers. From an aggregate perspective, the costs of a recession can be reduced when displaced workers are enabled to find new jobs in their old industries such that the loss of industry-specific human capital (Neal, 1995; Sullivan, 2010) are avoided. This is reflected in the statement of purpose of the ARRA which includes the goals to preserve and create jobs and to assist those most impacted by the recession.

We build on the approach by Chodorow-Reich (2019) to estimate how the employment effects of financial aid to states were distributed across sectors. This approach uses *pre-recession* information on the size of states' obligations to avoid endogeneity due to states in worse shape receiving more federal assistance. We find substantial heterogeneity in the employment effects of state fiscal relief across sectors. Most strikingly, about 40% of the employment effects (roughly 0.8 out of a total of 2 job-years per additional $100,000 in aid) materialized in the construction sector, which made up only about 5.5% of pre-crisis employment. Importantly, we find that the positive employment effects of transfers to states occurred disproportionately in sectors that were hit harder by the recession. For example, the construction sector was the sector in which employment had declined strongest in the early phase of the recession, and our results show that this sector benefitted most strongly from additional intergovernmental transfers. Hence, state fiscal relief during the Great

[1] In smaller volume than in the Great Recession, state fiscal relief measures were also implemented in the 1972 State and Local Fiscal Assistance Act and the 2003 Jobs and Growth Tax Relief Reconciliation Act. The ARRA included an unusually strict provision on documentation – section 1512 of the bill requires federal agencies to report outlays in each state and all prime recipients to report the funds received – as part of President Obama's transparency and open government promises.

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Recession had a double dividend. Not only did it preserve a substantial number of jobs, but it also protected employment most strongly in the hardest-hit parts of the economy.

We then investigate in how far the double dividend of state fiscal relief can also be expected in the Covid-19 crisis. This time, most job losses accrued in industries that are characterized by a high intensity of face-to-face contact between workers and clients such as the leisure and hospitality sector and retail trade (Adams-Prassl et al., 2020). To shed light on whether extending state fiscal relief measures would again help most strongly the hardest-hit parts of the economy, we exploit differences across states in the extent to which the Great Recession hit different sectors disproportionately. We find that, in states where a specific sector had been hit harder, federal transfers did not have a significantly more pronounced effect on employment in this sector. This result hints at the strong employment effects in these sectors, e.g., the construction sector, being mostly systematic and unrelated to the specifics of the Great Recession. We therefore conclude that to support the sectors which are hit hardest by the Covid-19 recession, state lawmakers would have to use intergovernmental transfers in a sharply different way than they did during the Great Recession.

The remainder of this paper is organized as follows. Section 2 summarizes the econometric approach, and Section 3 presents the results and discusses the implications of our findings for the Covid-19 crisis. Section 4 concludes.

2 Methodology

To determine the relationship between the effects of state fiscal relief in a sector and the degree to which the Great Recession hit the sector, we proceed in two steps. First, we estimate sector-specific job-year coefficients, i.e., we estimate, sector by sector, the number of additional job-years in this sector per additional $100,000 of ARRA spending. As discussed in the Introduction, transfers received through the different programs of the ARRA were essentially alike from the perspective of a state's government as they increased budgetary leeway. We therefore analyze the effects of total ARRA payouts to states. In the second step, we translate the estimated sector-specific job-year coefficients from the first step into percentage employment effects and regress those on the percentage job losses before ARRA by sector.

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To estimate sector-specific job-year coefficients (step 1), we use the Chodorow-Reich (2019) approach, which exploits variation in ARRA outlays across U.S. states. To address that outlays were endogenous to a state's economic condition in the crisis, they are instrumented by states' pre-crisis payments in domains where the federal government took over parts of the states' obligations. Chodorow-Reich (2019) has harmonized the instrumental-variable approaches developed in the literature, and his updated analysis provides a template for studies on the effects of ARRA intergovernmental transfers. We follow Chodorow-Reich (2019)'s preferred specification and combine three instruments: states' pre-recession Medicaid spending (as proposed by Chodorow-Reich et al., 2012), the formulaic component of states' highway spending (Wilson, 2012; Conley and Dupor, 2013), and the formulaic component of all ARRA spending by federal agencies allocated independently of state-specific developments in the recession (Dupor and Mehkari, 2016; Dupor and McCrory, 2018). For our purpose, it is important that the instruments do not directly affect the industry mix of employment. The studies cited above carefully demonstrate that funds received by states through ARRA were fungible, i.e., could be used by state governments as they wished. This means that, e.g., Medicaid relief did not constitute a stimulus directed to the health sector.

We run separate regressions for each NAICS supersector.[2] For supersector i, the baseline cross-sectional 2SLS regression is given by

$$\sum_{h=0}^{H} (Y_{s,i,t+h} - Y_{s,i,t}) = \alpha_i + \beta_i F_s + \gamma_i' X_s + \varepsilon_{s,i}, \tag{1}$$

with

$$F_s = \Pi_0 + \Pi_1' Z_s + \Pi_2' X_s + \nu_{s,} \tag{2}$$

where s denotes federal states, i denotes sectors, and t is the start of the treatment period (in our case, this is December 2008, when important components of the ARRA became known publicly). The dependent variable is the cumulated monthly employment level from December 2008 through December 2010 (by state and sector), net of the level in December 2008, normalized by the adult

[2]We separate both retail trade and wholesale trade from the trade, transportation, and utilities supersector. We label the remaining group of industries in this supersector the transportation, warehousing, and utilities sector. We further separate the manufacturing supersector into durable goods and non-durable goods manufacturing.

population, and translated into job-years, i.e.,

$$Y_{s,i,t+h} - Y_{s,i,t} = \frac{1}{12}\left(\frac{\text{Employment}_{s,i,t+h} - \text{Employment}_{s,i,t}}{\text{Working age population}_{s,t}}\right). \tag{3}$$

Accordingly, the time span is $H = 24$ months. The endogenous variable F_s is total ARRA outlays to state s from December 2008 to December 2010, measured in \$100,000 increments and per person of working age in December 2008. It is instrumented by the vector Z_s, as described above, where instruments are normalized by the adult population in December 2008. Following Chodorow-Reich (2019), we include as control variables (captured in vector X_s) states' pre-ARRA employment-to-population ratio as well as pre-ARRA trends in employment and production to account for the potential threat to identification that states' differential pre-crisis trends were correlated with the pre-crisis spending levels measured by the instruments. Specifically, the regressions account for the December 2008 employment-to-population ratio, the change in employment from December 2007 to December 2008, and the change in gross state product (GSP) from the fourth quarter of 2007 to the fourth quarter of 2008. As in Chodorow-Reich (2019), the control variables are normalized to have unit variance. In robustness checks, we also control for sector-specific employment trends within states. The coefficient on ARRA outlays, β_i, measures the number of additional job-years in sector i due to an additional \$100,000 spent across all sectors. It compares the actual employment development in a sector to the counterfactual with fewer ARRA transfers. The approach does not allow us to disentangle between prevented job destruction and induced job creation. As discussed by Chodorow-Reich et al. (2012), relief payments were used in two ways: to avoid or alleviate spending cuts and to prevent or lower tax and fee increases. Accordingly, we phrase our results in terms of job-years preserved through state fiscal relief.

To determine the relationship between the sectoral employment effects of ARRA transfers to states and sectoral job losses during the recession (step 2 of our analysis), we calculate, for each sector i, relative employment gains from an additional \$1 billion in yearly ARRA payments,

$$\text{Gains}_i \equiv \frac{\widehat{\beta}_i \cdot \kappa}{\text{Employment}_{i,t-13}}, \tag{4}$$

where $\widehat{\beta}_i \cdot \kappa$ with $\kappa \equiv \$1 \text{ billion/year}/\$100,000$ is the absolute employment gain from an additional \$1 billion, which we divide by the sector's pre-crisis employment level in November 2007,

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$\text{Employment}_{i,t-13}$. We then regress these relative employment gains on sector-specific relative employment changes during the first year of the recession (i.e., the part of the downturn before ARRA):

$$\text{Gains}_i = \delta + \zeta \cdot \frac{\text{Employment}_{i,t-1} - \text{Employment}_{i,t-13}}{\text{Employment}_{i,t-13}} + \epsilon_i. \tag{5}$$

To take into account estimation uncertainty from step 1, we weigh observations by the statistical significance (one minus p-value) of the estimated job-year coefficients $\widehat{\beta}_i$. This regression does not aim at identifying a causal relation between recessionary job losses and the sectoral effects of the ARRA, but it is merely an accounting tool that helps to summarize the descriptive relationship between the two.[3]

Data. Monthly employment data by state and industry come from the Current Employment Statistics (CES) of the Bureau of Labor Statistics (BLS).[4] For a few sector-state combinations, the required monthly employment information is missing (see Table A.1 in the Appendix for sample sizes). We use the data on ARRA outlays and the instruments from Chodorow-Reich (2019). Population data are from the BLS Local Aea Unemployment Statistics and GSP data are from the Bureau of Economic Analysis (BEA) Regional Data, GDP by state.[5]

3 Results

Sector-specific employment effects. The results from the first step of our analysis are summarized in Figure 1, which displays the estimated sector-specific job-year coefficients (the full regression results are shown in Table A.1 in the Appendix).[6] As Chodorow-Reich (2019), we estimate that an additional $100,000 in ARRA payouts increased total employment by the equivalent of about two jobs, each of which lasts for one year.[7] Figure 1 illustrates that there was substantial heterogeneity in the effects across sectors. Close to 0.8 job-years, or nearly 40% of the total impact, accrued in the construction sector. Professional and business services, wholesale trade, and the residual "other services" sector also experienced significant employment effects. Furthermore,

[3] As emphasized in Chodorow-Reich et al. (2012), it is unlikely that employment developments until November 2008 already reflected anticipated effects of the ARRA stimulus. Important components of the ARRA became apparent no sooner than in December 2008.

[4] See `https://www.bls.gov/sae/data/home.htm`

[5] See `https://www.bls.gov/lau/` and `https://www.bea.gov/data/gdp/gdp-state`, respectively.

[6] First-stage F-statistics range from 39.8 to 46.1.

[7] The difference between our estimate (2.03) and Chodorow-Reich (2019)'s estimate (2.01) is due to data revisions.

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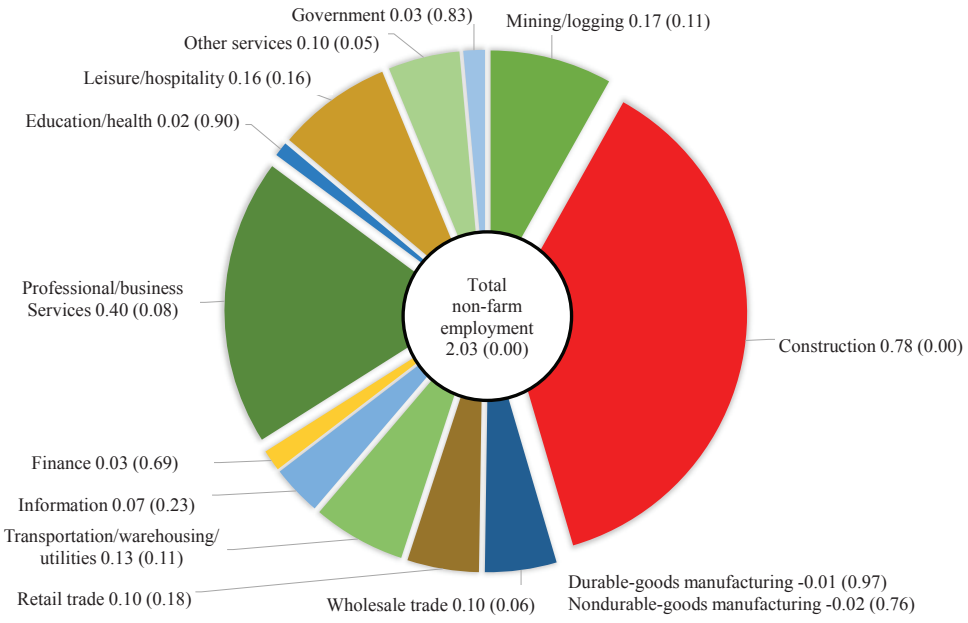
Figure 1: Sector-specific employment effects of ARRA outlays (job-years per marginal $100,000; p-values in parentheses)



Notes: Coefficients on ARRA outlays from sector-specific 2SLS regressions. Dependent variable: sector-specific cumulated monthly employment from December 2008 through December 2010 net of December 2008 employment. Regressor of interest: Total ARRA outlays between December 2008 and December 2010, instrumented as described in the text. Control variables: December 2008 total employment, change in total employment from December through December 2009, 2007Q4-2008Q4 change in gross state product.

there are noticeable, yet statistically insignificant, employment effects in retail trade, the leisure and hospitality sector, mining and logging, as well as the trade, warehousing, and utilities sector. Employment effects in other sectors tend to be small.

To put the small estimate for government employment into perspective, recall that our cross-sectional analysis determines the effects of the ARRA payments that some states received more than others. Our results do not rule out that inframarginal ARRA dollars were used to preserve government jobs across states, they rather indicate that the marginal ARRA dollar was used otherwise and affected employment most strongly in the private sector. The estimate for mining and logging should be considered with caution due to the fracking boom that is potentially confounded with the effects of ARRA transfers because of similar timing.[8]

[8]The fracking boom cannot easily be accounted for by, e.g., including pre-crisis trends as control variables because fracking hit off almost simultaneously with the ARRA stimulus, especially in small states where this development may be particularly influential. E.g., in North Dakota, production of shale gas rose more than eightfold from 2008 to 2009 after being virtually constant at low levels before (according to data from the U.S. Energy Information Agency).

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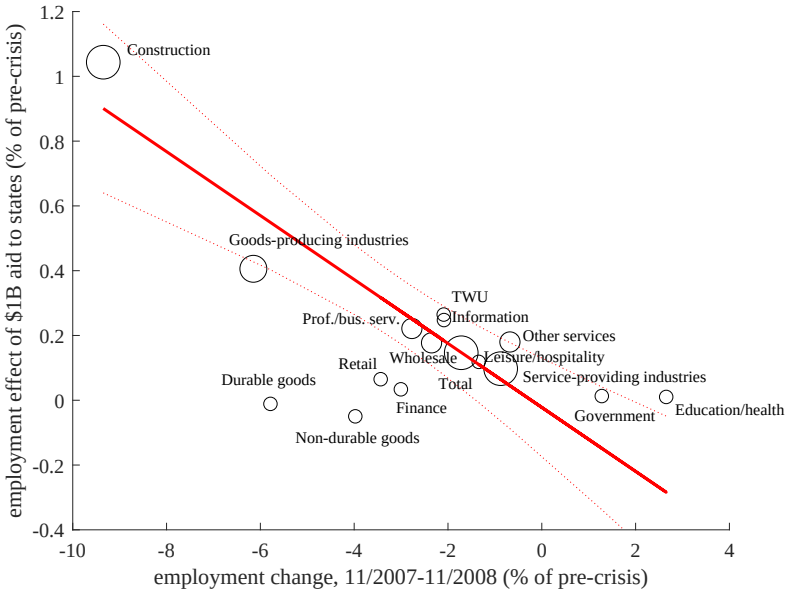
Figure 2: Employment effects of ARRA outlays by sector's exposure to downturn.



Notes: Vertical axis shows relative employment gains of $1 billion of additional ARRA outlays, as defined in equation (4). The size of circles indicates the statistical significance of the underlying job-years coefficient. Large circles: p-value≤0.01; medium circles: p-value≤0.05; small circles: p-value≤0.10, tiny circles: p-value>0.10. The regression uses one minus p-value as weights. Estimated employment gains for total employment and supersector groups (goods-producing and services-providing industries) are shown in the scatter plot for comparison but omitted from the regression. TWU = Transportation, warehousing, and utilities. Prof./bus. serv. = Professional and business services

Overall, our estimates imply that, had yearly ARRA payments to states (which averaged $131.5 billion in 2009 and 2010) been lower by $1 billion, total employment would have been lower by 0.15% of pre-crisis employment (equivalent to 20,000 jobs) and employment in the construction sector would have been lower by as much as 1.04% of its pre-crisis level.

Figure 2 plots estimated relative employment gains due to ARRA payments, as defined in equation (4), against sector-specific relative employment changes during the first year of the recession (i.e., the part of the downturn before ARRA), see equation (5). Larger circles indicate more precise estimates of the underlying job-year coefficients, and the red lines show the fitted linear relation from regression (5), along with a 95% confidence interval. The figure shows that, in general, additional ARRA transfers to states had more substantial employment effects in sectors that had been hit harder by the crisis. Hence, ARRA payments to states stimulated the labor market especially in the sectors that had already suffered the most. On average, a one percentage

point stronger decline in employment during the first year of the recession is associated with a roughly 0.1 percentage points stronger estimated employment effect of an additional $1 billion in ARRA outlays (i.e., $\widehat{\zeta} = 0.099$, p-value<0.001).[9] This reveals the double dividend of state fiscal relief in the Great Recession: many jobs were preserved, and disproportionately so in sectors that were affected disproportionately by the downturn. This way, state fiscal relief prevented further deterioration of the labor-market prospects of the hardest-hit groups of workers.

We corroborated our results in several robustness checks. In particular, we applied the Medicaid instrument suggested by Chodorow-Reich et al. (2012) rather than the baseline combination of instruments controlled for sector-specific pre-ARRA employment trends, and accounted for the full set of control variables considered in Chodorow-Reich (2019)'s sensitivity analysis. See Appendix for details.

Lessons for the Covid-19 crisis. While the Great Recession was a typical recession regarding the distribution of job losses across sectors with construction and manufacturing being the hardest-hit sectors (Hoynes et al., 2012), the Covid-19 crisis is different. This time, other sectors are most strongly affected by the downturn, such as retail trade, leisure, and hospitality. We now discuss whether extending financial aid to states in the Covid-19 crisis would also yield a double dividend in the sense that the additional funds would save or create jobs disproportionately in the sectors that are hit hardest by this recession. One the one hand, we have documented that the overall employment gains due to state fiscal relief during the Great Recession were moderate in those sectors that are now hit hard by the Covid-19 crisis, see Figure 1. On the other hand, we have shown that, during the Great Recession, there was a positive relationship between the employment gains due to state fiscal relief and how strongly a sector was affected by the downturn, see Figure 2. If the latter finding applies to recessions in general, we can expect that extending state fiscal relief in the Covid-19 crisis would support employment predominantly in the hardest-hit parts of the economy, in particular retail trade, leisure, and hospitality. To shed light on this, we exploit that, in the Great Recession, the distribution of job losses across sectors differed between *states*. For example, in the first twelve months of the Great Recession, the construction sector in the U.S.

[9] In line with our previous results, the construction sector is an important driver of this result, being the sector most affected by the crisis and the strongest beneficiary of the relief money. Leaving out this sector weakens the relation between crisis exposure and employment effects of ARRA outlays, but the relationship continues to be negative.

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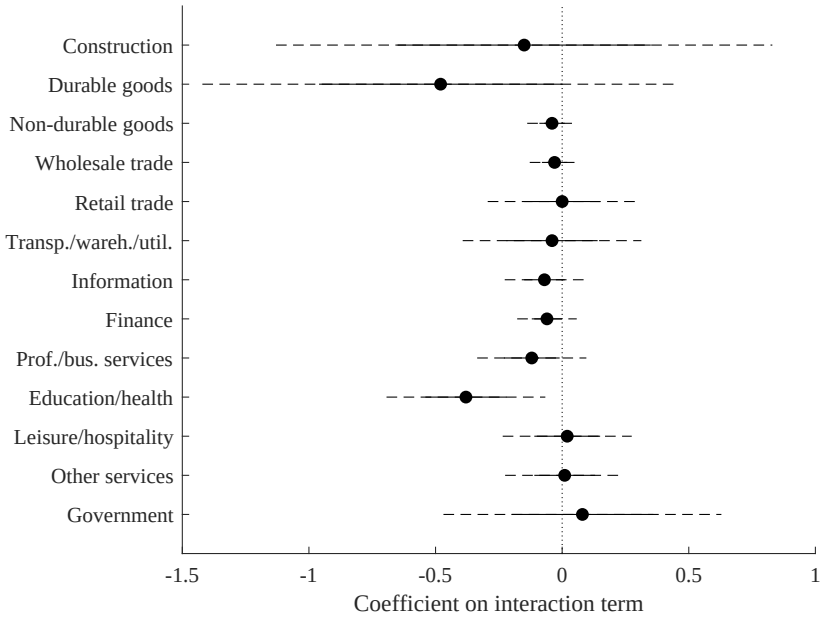
was about six percentage points more affected by job losses than the U.S. economy as a whole. In New York State and Texas, however, job losses in the construction sector were less than two percentage points higher than the drop in total employment in these states, while in California and Florida, they were over 11 percentage points higher than the state-specific average. Exploiting this variation between state-specific recessions allows us to investigate how the distribution of job losses in a downturn affects the distribution of the effects of state fiscal relief. Our empirical approach is to examine whether a particular sector tended to benefit more strongly from ARRA payments in those states where it had previously suffered more severely from the crisis. If this is the case, then it can be concluded that systematic forces ensured that specific sectors benefited strongly from the ARRA payments *because* they had been strongly affected by the crisis. Then, we may expect the double dividend of state fiscal relief to occur also in the Covid-19 crisis.

Technically, we consider an additional set of regressions where we interact ARRA payouts with the pre-ARRA drop in sector-specific employment relative to total employment. We define, for each sector i in each state s, a measure of the excess exposure to the downturn in 2007/08 as the percent employment change for sector i in state s between December 2007 and December 2008 minus the percent change in total employment in state s, and normalize this variable to have mean zero and variance one. Our baseline empirical model (1) is then augmented by the interaction between ARRA outlays and the excess-exposure measure, and the excess-exposure measure further enters the second stage as an additional control variable. Here, we use the three instruments from the baseline regressions as well as their respective interactions with the excess exposure measure as instruments (giving a total of six instruments) to instrument ARRA outlays and the interaction term.

Figure 3 illustrates the estimated coefficients on the interaction terms in the sector-specific regressions (the full regression results are documented in Table A.2 in the Appendix). In most sectors, the estimated coefficient on the interaction term is negative. This result implies that, in states where the respective sector was hit harder by the Great Recession, ARRA transfers to this state tended to have a more substantial effect on employment in this sector. The strong employment effects in the construction sector may thus partly result from this sector's significant exposure to the crisis, for example, because state governments deliberately decided to use the

Figure 3: Estimated coefficients on the interaction between ARRA payouts and sector-specific excess exp



Notes: Estimated coefficients $\widehat{\xi}_i$ from sector-specific 2SLS regressions $\sum_{h=0}^{H}(Y_{s,i,t+h} - Y_{s,i,t}) = \alpha_i + \beta_i F_s + \xi_i F_s \widetilde{E}_{i,s} + \omega_i \widetilde{E}_{i,s} + \gamma_i' X_s + \varepsilon_{s,i}$. $\widetilde{E}_{i,s}$ is $(E_{i,s} - \text{mean}(E_{i,s}|i))/(\text{var}(E_{i,s}|i)^{1/2})$, where $E_{i,s} = (\text{Employment}_{s,i,t-1} - \text{Employment}_{s,i,t-13})/\text{Employment}_{s,i,t-13} - (\text{Employment}_{s,t-1} - \text{Employment}_{s,t-13})/\text{Employment}_{s,t-13}$. F_s and $F_s\widetilde{E}_{i,s}$ are instrumented as described in the main text. Dots: point estimates. Solid lines: point estimate plus/minus one standard deviation. Dashed lines: 95% confidence intervals. Transp./wareh./util. = transportation, warehousing, and utilities. Prof./bus. services = professional and business services.

ARRA payments to support their hardest-hit sectors. However, the estimates for the interaction terms are insignificant for most sectors. When calculating the estimated marginal effect of ARRA payouts from these regressions, we find that they are similar across the range of our excess-exposure measure. Using again the construction sector as an example, our estimates imply that, in states where the construction sector was hit only slightly more strongly than the economy as a whole, employment gains in construction due to ARRA payments were sizeable, too. This hints at the strong employment effects in the construction sector being mostly systematic and unrelated to the specifics of the Great Recession. For the sectors particularly relevant in the context of the Covid-19 crisis (leisure, hospitality, retail trade), we find that the interaction is insignificant and point estimates are close to zero. This indicates that these sectors would not profit more strongly from state fiscal relief in the Covid-19 recession, which has hit them substantially harder than did the Great Recession.

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An explanation for our findings is that state governments used additional ARRA payments to a large degree to extend construction-related spending or to alleviate cuts in this type of spending, largely irrespective of how job losses were distributed across sectors in their states. Leduc and Wilson (2017) document, for one component of the ARRA stimulus, that intergovernmental transfers were used mostly for infrastructure spending. This can be a consequence of, e.g., the relative easiness of cutting back on construction expenditures and these cuts being avoided due to the relief payments or intensive lobbying of firms in the construction sector (as documented by Leduc and Wilson, 2017). An alternative explanation for the distribution of the employment effects of ARRA could be that structural characteristics of the construction sector make employment in this sector distinctly responsive to demand changes. This explanation seems unlikely as the literature has identified several sources of strong sector-specific employment reactions to changes in government demand – an upstream position in the production network (Bouakez et al. 2020), low unionization (Nekarda and Ramey 2011), and large shares of pink-collar workers (Bredemeier et al., 2020a, 2020b) – none of which apply to the construction sector.

The finding that ARRA outlays did not have more substantial employment effects in a sector in states where this sector was hit harder by the recession implies that the double dividend of state fiscal relief cannot be taken for granted in other recessions. To help the industries that have been struck this time, such as retail trade, leisure, and hospitality, the funds would have to be used in a distinctly different way than during the Great Recession. For example, these sectors could be exempted from tax or fee increases, or the relief payments could be used for direct subsidies to the severely affected industries.

4 Conclusion

We have provided evidence of pronounced heterogeneity in the employment effects of the ARRA's state fiscal relief program during the Great Recession, with the construction sector being the main beneficiary. Our findings imply that intergovernmental transfers to states did not only protect jobs, but they also protected jobs in the industries hit hardest, thereby preventing further accelerations in the distributional costs of the crisis. We have argued that such a double dividend could be generated in the Covid-19 recession only if states use relief payments in distinctly different ways than they did during the Great Recession.

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Appendix

Table A.1 shows the full results of our baseline 2SLS regressions, which we use to estimate sector-specific job-year coefficients as displayed in the pie chart in Figure 1. Each column corresponds to a sector-specific regression.

Figures A.1 through A.3 show the results of robustness checks. In Figure A.1, we used the Medicaid relief instrument suggested by Chodorow-Reich et al. (2012) rather than the baseline combination of instruments. In Figure A.2, we controlled for sector-specific pre-ARRA employment trends. In Figure A.3, we accounted for the full set of control variables considered in Chodorow-Reich (2019)'s sensitivity analysis. In all specifications, we find that sectors hit harder by the recession were more strongly affected by state fiscal relief, as in our baseline specification.

Table A.2 shows the full results of the augmented sector-specific 2SLS regressions with interaction terms, given by

$$\sum_{h=0}^{H}(Y_{s,i,t+h}-Y_{s,i,t})=\alpha_i+\beta_i F_s+\xi_i F_s\widetilde{E}_{i,s}+\omega_i\widetilde{E}_{i,s}+\gamma_i' X_s+\varepsilon_{s,i},$$

where

$$\widetilde{E}_{i,s}=\frac{E_{i,s}-\text{mean}(E_{i,s}|i)}{\text{var}(E_{i,s}|i)^{1/2}}$$

with

$$E_{i,s}=\frac{\text{Employment}_{s,i,t-1}-\text{Employment}_{s,i,t-13}}{\text{Employment}_{s,i,t-13}}-\frac{\text{Employment}_{s,t-1}-\text{Employment}_{s,t-13}}{\text{Employment}_{s,t-13}}.$$

Each column of Table A.2 corresponds to a sector-specific regression.

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Table A.1: Regression results for sectoral employment changes, 12/2008 through 12/2010 (p-values in parentheses)

	Total non-farm	Mining/ logging	Construction	Dur.-goods manufctng.	Non-d.-goods manufctng.	Wholesale trade	Retail trade	Transport., wareh., util.
Total ARRA payouts	2.03	0.17	0.78	-0.01	-0.02	0.10	0.10	0.13
	(0.00)	(0.11)	(0.00)	(0.97)	(0.76)	(0.06)	(0.18)	(0.11)
Dec-08 state employment /population 16+	-3.74	-0.15	-1.57	-0.20	0.14	-0.07	-0.39	-0.11
	(0.12)	(0.86)	(0.13)	(0.72)	(0.44)	(0.71)	(0.25)	(0.63)
State employment change, Dec-07 to Dec-08	11.95	0.55	5.10	-1.02	-0.05	0.43	1.28	0.10
	(0.00)	(0.50)	(0.00)	(0.28)	(0.88)	(0.03)	(0.00)	(0.52)
GSP change, 2007Q4-2008Q4	2.27	2.37	2.33	-1.73	-0.44	-0.17	0.59	-0.06
	(0.47)	(0.07)	(0.01)	(0.04)	(0.03)	(0.45)	(0.24)	(0.80)
Constant	-6.59	-2.24	4.13	-5.95	-3.49	-2.36	0.67	-2.25
	(0.82)	(0.81)	(0.76)	(0.42)	(0.18)	(0.34)	(0.86)	(0.38)
First-stage F-statistic	46.09	44.16	45.75	44.67	45.32	46.09	46.09	46.09
Number of observations	50	47	48	48	48	50	50	50

	Infor- mation	Financial services	Prof./bus. services	Education/ health	Leisure/ hospitality	Other services	Govern- ment
Total ARRA payouts	0.07	0.03	0.40	0.02	0.16	0.10	0.03
	(0.23)	(0.69)	(0.08)	(0.90)	(0.16)	(0.05)	(0.83)
Dec-08 state employment /population 16+	0.01	-0.24	-0.60	-0.19	-0.67	0.19	0.02
	(0.95)	(0.18)	(0.11)	(0.40)	(0.07)	(0.10)	(0.95)
State employment change, Dec-07 to Dec-08	-0.37	0.49	1.10	0.74	2.03	0.40	1.69
	(0.02)	(0.06)	(0.03)	(0.02)	(0.00)	(0.01)	(0.00)
GSP change, 2007Q4-2008Q4	-0.33	-0.24	0.39	-0.33	1.05	0.11	-0.99
	(0.03)	(0.37)	(0.42)	(0.25)	(0.01)	(0.56)	(0.01)
Constant	-2.61	0.82	-1.55	5.95	5.13	-3.93	2.37
	(0.03)	(0.68)	(0.78)	(0.07)	(0.29)	(0.00)	(0.61)
First-stage F-statistic	39.80	46.09	46.09	46.09	46.09	46.09	46.09
Number of observations	48	50	50	50	50	50	50

Notes: Dur.-goods manufctng. = Durable-goods manufacturing, Non-d.-goods manufctng = Non-durable-goods manufacturing. Transport., wareh., util. = Transportation, warehousing, and utilities. Prof./bus. services = Professional and business services. P-values are given in parentheses. First-stage F-statistic and number of observations differ across columns because, for a few sector-state combinations, the required monthly employment information is missing.

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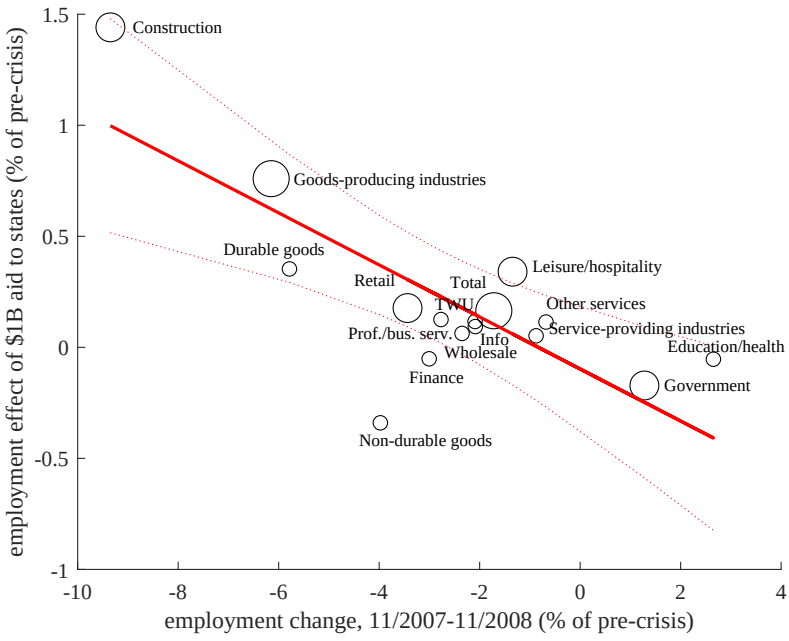
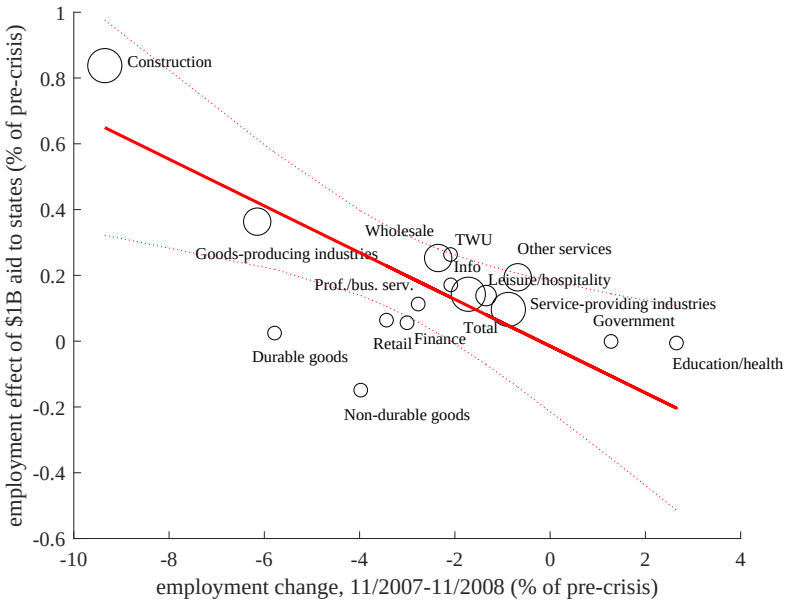
Figure A.1: Robustness check: Medicaid relief instrument



Notes: see Figure 2.

Figure A.2: Robustness check: controlling for sector-specific employment trends



Notes: see Figure 2.

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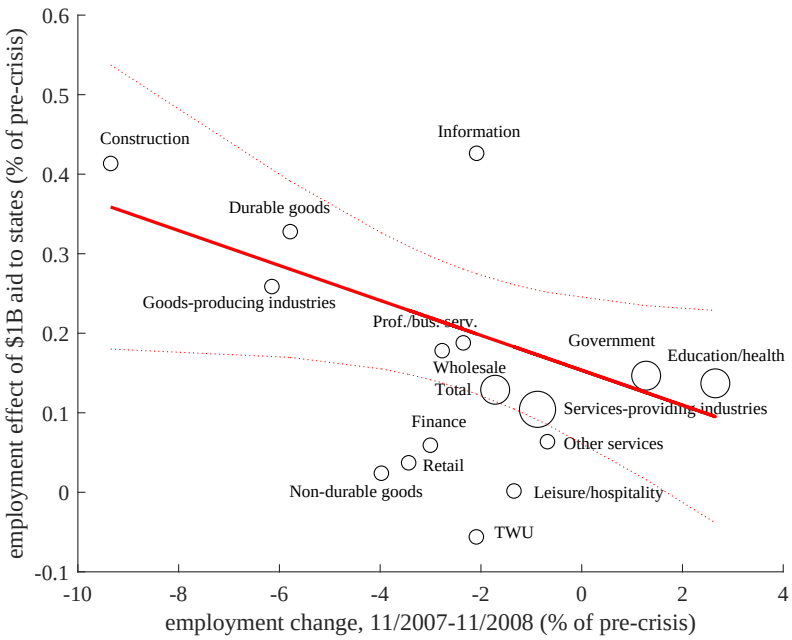
Figure A.3: Robustness check: including all control variables considered in Chodorow-Reich (2019)'s sensitivity analysis



Notes: see Figure 2.

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Table A.2: Regression results for sectoral employment changes, 12/2008 through 12/2010, interaction-term specification (p-values in parentheses)

	Construction	Dur.-goods manufctng.	Non-d.-goods manufctng.	Wholesale trade	Retail trade	Transport., wareh., util.	Information
Total ARRA payouts	0.88	0.03	-0.03	0.10	0.11	0.15	0.04
	(0.00)	(0.90)	(0.76)	(0.09)	(0.18)	(0.08)	(0.35)
Total ARRA payouts	-0.15	-0.48	-0.04	-0.03	0.00	-0.04	-0.07
× sector's excess exposure	(0.77)	(0.32)	(0.45)	(0.48)	(0.99)	(0.81)	(0.38)
Sector's excess exposure	2.83	7.09	0.72	0.52	-0.06	0.55	1.05
	(0.64)	(0.26)	(0.33)	(0.39)	(0.97)	(0.81)	(0.33)
Dec-08 state employment+	-1.21	-0.53	0.07	-0.12	-0.38	-0.09	0.05
/population 16+	(0.39)	(0.38)	(0.71)	(0.54)	(0.24)	(0.69)	(0.62)
State employment change,	4.11	-1.01	0.01	0.46	1.27	0.09	-0.28
Dec-07 to Dec-08	(0.17)	(0.28)	(0.99)	(0.02)	(0.00)	(0.55)	(0.02)
GSP change,	2.20	-1.92	-0.43	-0.18	0.61	-0.06	-0.25
2007Q4-2008Q4	(0.03)	(0.03)	(0.04)	(0.48)	(0.22)	(0.80)	(0.02)
Constant	-2.36	-2.58	-2.63	-1.84	0.48	-2.66	-2.62
	(0.90)	(0.75)	(0.37)	(0.47)	(0.90)	(0.28)	(0.04)
Number of observations	48	48	48	50	50	50	48

	Financial services	Prof./bus. services	Education/ health	Leisure/ hospitality	Other services	Government
Total ARRA payouts	0.01	0.32	-0.03	0.16	0.08	0.07
	(0.94)	(0.20)	(0.81)	(0.09)	(0.19)	(0.79)
Total ARRA payouts	-0.06	-0.12	-0.38	0.02	0.01	0.08
× sector's excess exposure	(0.33)	(0.26)	(0.02)	(0.85)	(0.93)	(0.79)
Sector's excess exposure	1.14	2.80	6.27	0.18	0.01	-1.04
	(0.13)	(0.09)	(0.00)	(0.91)	(1.00)	(0.76)
Dec-08 state employment	-0.28	-0.48	-0.51	-0.54	0.17	0.04
/population 16+	(0.07)	(0.21)	(0.02)	(0.13)	(0.16)	(0.91)
State employment change,	0.55	0.50	1.87	1.98	0.47	1.64
Dec-07 to Dec-08	(0.02)	(0.29)	(0.00)	(0.00)	(0.00)	(0.02)
GSP change,	-0.25	0.43	-0.21	0.91	0.14	-1.02
2007Q4-2008Q4	(0.37)	(0.35)	(0.48)	(0.02)	(0.42)	(0.01)
Constant	1.64	-2.49	11.36	3.66	-3.47	1.65
	(0.38)	(0.65)	(0.00)	(0.43)	(0.05)	(0.75)
Number of observations	50	50	50	50	50	50

Notes: Dur.-goods manufctng. = Durable-goods manufacturing, Non-d.-goods manufctng = Non-durable-goods manufacturing. Transport., wareh., util. = Transportation, warehousing, and utilities. Prof./bus. services = Professional and business services. P-values are given in parentheses. Number of observations differ across columns because, for a few sector-state combinations, the required monthly employment information is missing.