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ARTICLE

Sectoral employment effects of state fiscal relief: evidence from the Great Recession

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Abstract
We document that the employment effects of financial aid to US states during the Great Recession were strongly unevenly distributed across sectors, the construction sector being the main beneficiary. State fiscal relief not only preserved a substantial number of jobs but it also fostered employment most strongly in the sectors hit hardest by the recession. Exploiting across-state differences, we conclude that the sectoral employment effects of state fiscal relief reflect the typical spending patterns of state and local governments, who usually spend large shares of their discretionary expenditures on construction.

Keywords: State fiscal relief; ARRA; fiscal policy; employment composition; regional labor markets; construction sector

1. Introduction

Fiscal distress of state and local governments in the USA during the COVID crisis has renewed interest in state fiscal relief programs, that is, intergovernmental transfers from the federal government to state governments. The Coronavirus Aid, Relief, and Economic Security (CARES) Act appropriated $150 billion in direct aid for state and local governments early in the crisis. The National Governors Association soon called for substantially more funds and was supported by Democrats in Congress and Fed Chairman Jerome H. Powell but opposed by then-Senate majority leader Mitch McConnel and then-President Donald Trump. The American Rescue Plan enacted in March 2021 included another $350 billion to help state and local governments bridge budget shortfalls caused by the pandemic.

The context of the Great Recession is particularly suited to learn about the labor market consequences of state fiscal relief. During the Great Recession, state fiscal relief was one of the major components of the American Recovery and Reinvestment Act (ARRA), 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 channeled mainly 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. Almost all transfers were fungible, that is, states could effectively use the money as they wished [Chodorow-Reich et al. (2012) and 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, for example, 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.[1] Chodorow-Reich et al. (2012), Wilson

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(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 estimate how the employment effects of financial aid to states were distributed across sectors. We find that there is 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. We document a positive relationship between the severity with which a sector was affected by the Great Recession and the number of jobs created in that sector through state fiscal relief. For example, the construction sector was hit most severely by the labor market downturn and benefited the most from the effects of state fiscal relief. This can be seen as a benefit of this program, since it stabilized the employment prospects of the most hard-pressed groups of workers and thus prevented further acceleration of the distributional costs of the crisis. Fostering employment particularly in the sectors that are hit hardest by an economic downturn is valuable because it is difficult for workers to switch industries [Weinberg (2001) and Artuç and McLaren (2015)]. The costs of a recession can be reduced when displaced workers are enabled to find new jobs in their old industries such that losses of industry-specific human capital [Neal (1995) and Sullivan (2010)] are avoided. Yet, one might be concerned that stabilization policies help the very sectors in structural decline. Thus, the policies may get in the way of the cleansing effect of recessions [Caballero and Hammour (1994)]. However, we find no significant relationship between *pre-crisis* employment growth and the effects of the state fiscal relief program in a sector. For example, state fiscal relief created few jobs in the manufacturing sector, which had shrunk the most before the crisis.

We then investigate whether the documented relationship between crisis exposure and the effects of state fiscal relief can be expected to occur in other recessions that differ in terms of the distribution of recessionary job losses. This can be expected only if specific sectors benefitted much from ARRA payments *because* they were hit hard by the Great Recession—be it because of sectoral slack or because state governments actively channeled funds to their hardest-hit sectors. We do not find evidence that a sector's exposure to the crisis is responsible for the employment effects of the ARRA in this sector. Specifically, 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. For example, the construction sector benefitted disproportionately from ARRA transfers even in states where it was not disproportionately affected by the Great Recession, such as New York State.

Our results rather indicate that the relationship between employment effects of state fiscal relief and crisis exposure in the Great Recession reflects rigidity in spending patterns on the side of state governments. Since funds obtained through state fiscal relief were fungible, the money was effectively distributed in the political process. Our results suggest that state policymakers used the funds freed up through state fiscal relief primarily in the same way as they usually spend discretionary funds. We provide direct evidence that the employment effects of state fiscal relief were more strongly clustered in the construction sector in states where, before the crisis, state governments spent higher shares of their budgets for construction—presumably because these state governments stuck to their spending habits also during the Great Recession. This interpretation is corroborated by our evidence showing that surges in state governments' construction spending shares were unrelated to the construction sector's fate during the recession and, in this sense, indeed "sticky." Hence, the ARRA's overall strong employment effects in the construction sector can be understood as a consequence of construction spending constituting a large share of state governments' budgets and particularly so of their discretionary expenditures.

We conclude that state fiscal relief transfers are likely to stimulate employment mostly in sectors where state governments typically spend a lot of money, independent of whether these sectors

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are hit hard by the particular downturn. The Great Recession was typical in terms of the distribution of recessionary job losses [Hoynes et al. (2012)]. For most recessions, one, therefore, may expect that channeling stabilization funds through state governments would stabilize employment particularly in the hardest-hit sectors. However, in recessions that lead to a different distribution of job losses across sectors, state fiscal relief will not automatically have this property.

Next to the literature on the effects of state fiscal relief, our paper also contributes to the literature on fiscal policy and heterogeneity on the production side. Acemoglu et al. (2016), Baqaee and Farhi (2018), Cox et al. (2020), Bouakez et al. (2021, 2022), Proebsting (2021), Ramey and Shapiro (1998), and Slavtchev and Wiederhold (2016), among others, show that sectoral heterogeneity shapes the propagation of fiscal policy and its sectoral and aggregate implications. We complement this literature by providing cross-sectional evidence of pronounced heterogeneity in the sectoral employment effects of fiscal policy during recessions.[2]

The remainder of this paper is organized as follows. In Section 2, we estimate sector-specific employment effects. In Section 3, we relate these differences to recessionary job losses and sectoral growth before the crisis. Section 4 investigates mechanisms. Section 5 concludes.

2. Sector-specific employment effects

First, we estimate sector-specific job-year coefficients, that is, 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.

Methodology. To estimate sector-specific job-year coefficients, we use the Chodorow-Reich (2019) approach, which exploits variation in ARRA outlays across US 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 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) and Conley and Dupor (2013)], and the formulaic component of all ARRA spending with local-recipient reporting which were allocated by federal agencies independently of state-specific developments in the recession [Dupor and Mehkari (2016) and Dupor and McCrory (2018)].

This IV strategy exploits that time pressure led Congress to use existing spending formulas to allocate the ARRA transfers and that these formulas were largely independent of states' economic performance during the crisis. The Medicaid instrument captures that the ARRA's Federal Medical Assistance Percentage (FMAP) allocated more transfers to states with higher long-run Medicaid spending but is not subject to the FMAP's dependence on the unemployment rate during the crisis. The highway instrument captures that the distribution of highway grants under the ARRA depended on pre-recession formulas. The Dupor-Mekhari instrument complements the set of pre-recession determinants of ARRA spending and exploits that the ARRA included spending by federal agencies that were not instructed to channel transfers toward regions hit harder by the recession.

For our purpose, it is important that the instruments do not mechanically affect the industry mix of employment. As discussed by Chodorow-Reich et al. (2012) and Conley and Dupor (2013), funds received by states through the ARRA were fungible, that is, could be used by state governments as they wished. This means that, for example, Medicaid relief did not constitute a direct stimulus to the health sector.

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We run separate regressions for each NAICS supersector.[3] For supersector i, the baseline cross-sectional 2SLS regression is given by

$$n_{i,s} = \alpha_i + \beta_i F_s + \gamma_i' X_s + \varepsilon_{s,i}, \tag{1}$$

with

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

where s denotes federal states and i denotes sectors. The dependent variable $n_{i,s}$ is the cumulated monthly employment level by state and sector from December 2008 (when important components of the ARRA became known publicly) through December 2010, net of the level in December 2008, normalized by the adult population, and translated into job-years, that is,

$$n_{i,s} \equiv \frac{\frac{1}{12}\sum_{t=12/2008}^{12/2010}\left(\text{Employment}_{s,i,t} - \text{Employment}_{s,i,12/2008}\right)}{\text{Working age population}_{s,12/2008}}. \tag{3}$$

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. 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.[4] 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 the first-stage regressions (2), ARRA outlays are instrumented by the vector Z_s that includes three variables. The first instrument is pre-recession Medicaid spending as an instrument for the FMAP component of the ARRA. As a second instrument, we take Wilson's (2012) linear projection of ARRA highway obligations based on pre-recession total federal highway miles, total miles driven on federal highways, taxes paid to the federal highway trust fund, and Federal Highway Administration obligation limitations. The third instrument is the sum of the agency-reported pre-recession spending in the programs where ARRA funds were allocated according to pre-recession formulas identified by Dupor and Mehkari (2016), whose list excludes FMAP and highway spending because of the authors' reliance on local-recipient reporting. All instruments are used as defined and provided by Chodorow-Reich et al. (2019) and are normalized by the adult population in December 2008. As required for IV identification, the first-stage regressions also include the control variables from the second-stage regressions, X_s.

The coefficient on ARRA outlays in the second-stage regressions (1), 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. Since total ARRA payouts are on the right-hand side, we cannot directly disentangle how many ARRA dollars flew into a particular sector from how many jobs each of these dollars preserved. The approach further 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.

Data. Monthly employment data by state and industry come from the Current Employment Statistics (CES) of the Bureau of Labor Statistics (BLS).[5] For a few sector-state combinations, the required monthly employment information is missing (see Table A.1 in the appendix). Population data are from the BLS Local Area Unemployment Statistics and GSP data are from the Bureau of Economic Analysis (BEA) Regional Data, GDP by state.[6] We use the data on ARRA outlays and the instruments from Chodorow-Reich (2019).

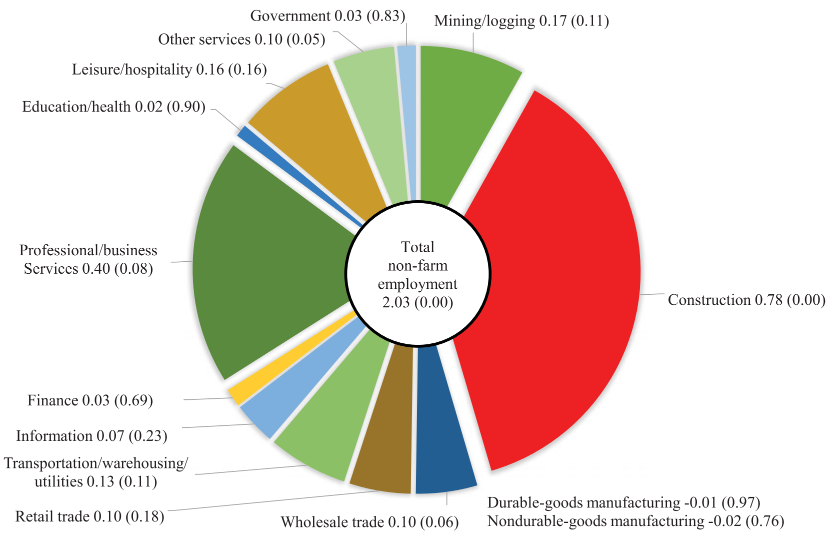

Figure 1. Sector-specific employment effects of ARRA outlays (job-years per marginal $100,000; *p*-values in parentheses). *Notes:* Coefficients on ARRA outlays from sector-specific 2SLS regressions. Dependent variable: sector-specific cumulated monthly employment from December 2008 through December 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.

Results. Figure 1 displays the estimated sector-specific job-year coefficients (the full regression results are shown in Table A.1 in the appendix).[7] 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 1 year.[8] 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, 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. It should be emphasized that these estimates are not informative about whether a particular sector profited strongly from state fiscal relief due to large fractions of the transfers having been channeled to this sector or because fiscal policy is disproportionately effective in this sector. In Section 4, we provide evidence indicating a prominent role for the former, that is, the importance of state governments' spending patterns for explaining our results.

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.[9]

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. Evidence reported by Conley and Dupor (2013) suggests strong employment effects in the public sector, which seems to stand in contrast to our findings. Yet, these large public sector effects relate to a specification where ARRA spending is considered net of lost tax revenue such that a balanced budget assumption is effectively applied, see Chodorow-Reich (2019). In a specification that does not apply such an

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assumption, the total employment effect estimated by Conley and Dupor (2013) becomes closer in magnitude to other papers, and their estimate for the public sector effect is considerably smaller.

Overall, our estimates imply that, had yearly ARRA payments to states (which averaged \$131.5 billion in 2009 and 2010) been lower by \$10 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.

As an additional perspective on the sectoral effects of state fiscal relief, we have also considered sectoral GDP as an outcome variable, again following the econometric approach by Chodorow-Reich (2019). We find that one dollar of ARRA outlays (spent across all sectors) increases GDP in the construction sector by (a statistically significant) \$0.73, which amounts to roughly 48% of the \$1.53 increase in total GDP reported by Chodorow-Reich (2019). Put differently, every \$10 billion in ARRA payments increased the economy's output by about a tenth of 1% of its pre-crisis level but output of the construction sector by about 1% of its pre-crisis level. Thus, the strong employment effects in the construction sector are accompanied by strong output effects in this sector.

3. Relation to sectoral employment developments before and during the Great Recession

Having shown that the employment effects of state fiscal relief differ substantially across sectors, we now investigate the relationship between the effects of state fiscal relief in a sector and the sectoral employment developments before and during the Great Recession. As a first step, we translate the estimated sector-specific job-year coefficients into percentage employment effects and regress those percentage effects on percentage sector-specific job losses during the first year of the recession before the ARRA. Note that 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. We will investigate mechanisms in the next section.

We calculate, for each sector i, relative employment gains from an additional \$10 billion in yearly ARRA payments,

$$\text{Gains}_i \equiv \frac{\widehat{\beta}_i \cdot \kappa}{\text{Employment}_{i,11/2007}}, \tag{4}$$

where $\widehat{\beta}_i \cdot \kappa$ with $\kappa \equiv \$10\text{ billion/year}/\$100,000$ is the absolute employment gain from an additional \$10 billion, which we divide by the sector's pre-crisis employment level in November 2007. We then regress these relative employment gains on sector-specific relative employment changes during the first year of the recession, that is, the part of the downturn before the ARRA between November 2007 and November 2008:

$$\text{Gains}_i = \delta + \zeta \cdot \frac{\text{Employment}_{i,11/2008} - \text{Employment}_{i,11/2007}}{\text{Employment}_{i,11/2007}} + \epsilon_i. \tag{5}$$

To take into account estimation uncertainty from the estimation of the job-year coefficients, we weigh observations by the statistical significance (one minus p-value) of the estimated job-year coefficients $\widehat{\beta}_i$.

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 the 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

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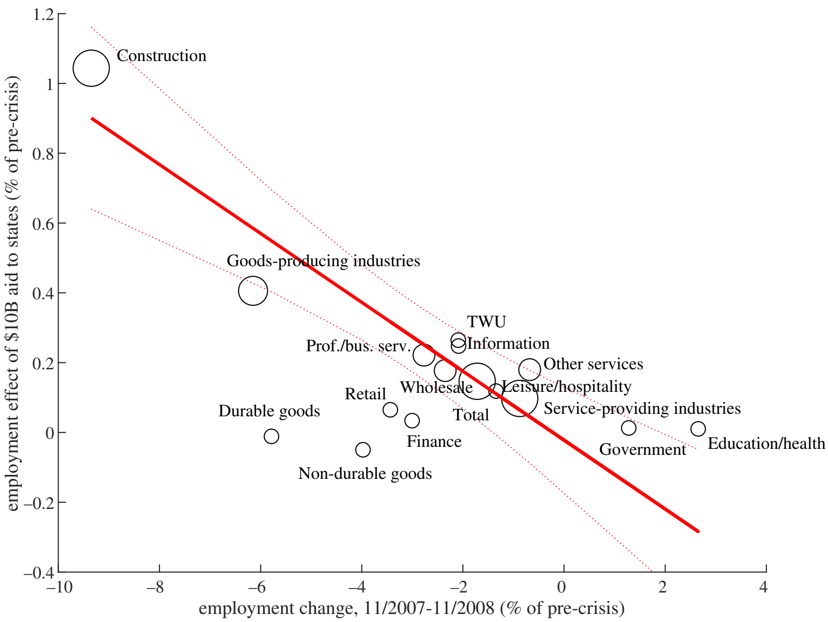

Figure 2. Employment effects of ARRA outlays by sector's exposure to downturn.
Notes: Vertical axis shows relative employment gains of \$10 billion of additional ARRA outlays, as defined in equation (4). The size of circles indicates the statistical significance of the underlying job-years coefficient. 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.

disproportionately in the sectors that had 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 \$10 billion in ARRA outlays (i.e. $\widehat{\zeta} = 0.099$, p-value<0.001).[10]

The relationship displayed in Figure 2 can be regarded as a desirable property of a state fiscal relief program since it prevented the labor market prospects of the hardest-hit groups of workers from deteriorating further and thereby dampened the distributional costs of the crisis. Raising the chances of laid-off workers to find work in their industry avoids switching costs for workers and reduces the loss of industry-specific human capital in a recession. This view 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." Our results show that the state fiscal relief programs in the ARRA contributed to achieving both these goals.

However, the sectoral employment effects of state fiscal relief might conflict with long-run developments that policy may not want to lean against. In particular, policymakers may want to avoid protecting employment in sectors that are in structural decline. It is often argued that recessions can have a cleansing effect on the structural composition of the economy in that they accelerate structural change by pushing struggling firms in shrinking industries over the cliff, thereby freeing up resources for modern and growing industries.

To analyze this aspect, we now relate the effects of state fiscal relief in a sector to the sector's long-run employment growth before the crisis. Specifically, we regress relative sectoral employment gains from an additional \$10 billion in yearly ARRA payments, Gains_i, on sectoral employment growth from the end of the early 2000's recession in November 2001 to the eve of

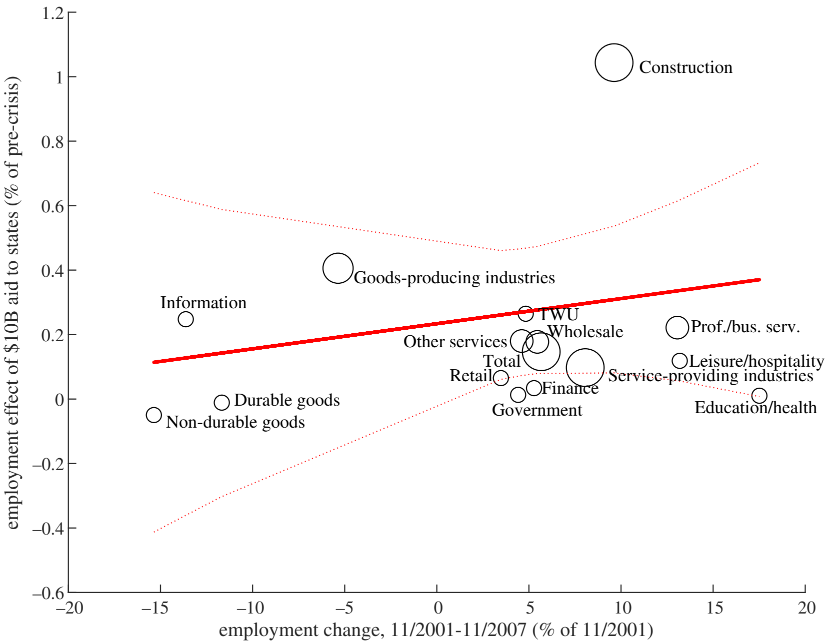

Figure 3. Employment effects of ARRA outlays vs. sector's long-run employment growth.
Notes: Vertical axis shows relative employment gains of $10 billion of additional ARRA outlays, as defined in equation (4). The size of circles indicates the statistical significance of the underlying job-years coefficient. 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.

the Great Recession in November 2007. That is, we replace the independent variable in (5) by

$$\frac{\text{Employment}_{i,11/2007} - \text{Employment}_{i,11/2001}}{\text{Employment}_{i,11/2001}}.$$

Figure 3 shows no significant relationship between sectoral trends before the crisis and the employment effects of state fiscal relief. In contrast to Figure 2, the estimated slope is positive and statistically insignificant (*p-value* 0.49). Two sectors are particularly interesting. Employment in manufacturing (of both durable and non-durable goods) had declined substantially during the noughties but did not benefit from the state fiscal relief transfers in the ARRA. In contrast, construction employment, which profited heavily from state fiscal relief, had not shown a downward trend before the crisis. Overall, Figure 3 ameliorates concerns that state fiscal relief did counteract the cleansing effect of recessions.[11]

4. Exploring potential mechanisms

We now investigate reasons for the distribution of employment effects of state fiscal relief across sectors. Understanding *why* employment effects of the ARRA were distributed across sectors the way they were is important for assessing whether a similar relationship between crisis exposure and state fiscal relief effects can be expected also in other crises. We can expect this only if specific sectors benefitted disproportionately from state fiscal relief *because* the crisis (here: the Great Recession) hit them hard.

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To organize thoughts, consider how employment in sector i and state s depends on fiscal spending in state s. In general, employment in a sector depends on the entire distribution of ARRA payouts across sectors, including spillover effects from the spending of ARRA funds in other sectors. Hence, we can write

$$n_{i,s} = n\left(F_{1,s}\left(F_s\right), \ldots, F_{i,s}\left(F_s\right), \ldots, F_{I,s}\left(F_s\right), \Lambda_{i,s}\right),$$

where I is the set of sectors, $F_{i,s}$ is the amount of ARRA payouts that were effectively distributed to sector i by state s (in forms of government purchases, subsidies, tax breaks, or otherwise) which is a function of total ARRA payouts to this state, F_s, and $\Lambda_{i,s}$ summarizes other determinants of sector state-specific employment. Taking the derivative with respect to F_s gives

$$\beta_i = \frac{\partial n_{i,s}}{\partial F_s} = \underbrace{\frac{\partial n_{i,s}}{\partial F_{i,s}}}_{\text{sectoral multiplier}} \cdot \underbrace{\frac{\partial F_{i,s}}{\partial F_s}}_{\text{sectoral impulse}} + \underbrace{\sum_{j \neq i} \frac{\partial n_{i,s}}{\partial F_{j,s}} \frac{\partial F_{j,s}}{\partial F_s}}_{\text{spill-over effect}}. \tag{6}$$

Equation (6) decomposes the effect of ARRA transfers to state s on employment in sector i of this state into three components:

(i) the *sectoral fiscal multiplier* in this sector, $\partial n_{i,s}/\partial F_{i,s}$,

(ii) the size of the *sector-specific impulse*, that is, the share of (marginal) ARRA dollars that have effectively flown into this sector, $\partial F_{i,s}/\partial F_s$, and

(iii) the size of the *spillover effect* from spending in other sectors, $\sum_{j\neq i} \partial n_{i,s}/\partial F_{j,s} \cdot \partial F_{j,s}/\partial F_s$.

Data limitations do not allow us to disentangle (i)–(iii) for each sector directly. For this, one would need data on how state governments spent the ARRA transfers they received. Due to the fungibility of the transfers, this would require observing the counterfactual sector-specific spending and tax behavior that state governments would have shown without the transfers. Yet, we can investigate *indirectly* which elements of equation (6) are predominantly driving the estimated employment effects. For this, we have to take into account that all three terms in equation (6) are affected by sectors' *exposure to the crisis* as well as sectors' *structural characteristics*.

Exposure to crisis. There are several reasons why sectors' exposure to the crisis can affect the sectoral employment effects of fiscal policy. Regarding *(i) sectoral multipliers*, economic slack is often suggested as a reason for elevated fiscal policy effects.[12] If productive capacity lies idle in a sector, an increase in demand may induce full-scale increases in output and employment without pressuring wages and prices upward. Regarding *(ii) sector-specific impulses*, state policymakers may have actively channeled funds into the sectors suffering most from the crisis to provide relief in these sectors. Regarding *(iii) spillover effects*, usual supply chains can be disturbed in times of economic distress, as witnessed during the recovery from the COVID crisis, changing how demand in downstream industries affects upstream industries. Independent of which element of condition (6) is thought to be predominantly affected by cyclical conditions, the hypothesis that the distribution of employment effects across sectors is *driven* by sectors' cyclical conditions has a common testable implication: if sectoral exposure to the crisis was responsible for the employment effects of state fiscal relief in a sector, this sector should have benefitted disproportionately from ARRA in places where it was hit disproportionately strongly by the recession.

To investigate this implication empirically, we exploit that, in the Great Recession, the distribution of job losses across sectors differed between *states*. For example, in the first 12 months of the Great Recession, the construction sector was about six percentage points more affected by job losses than the US economy as a whole. However, in New York State and Texas, 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

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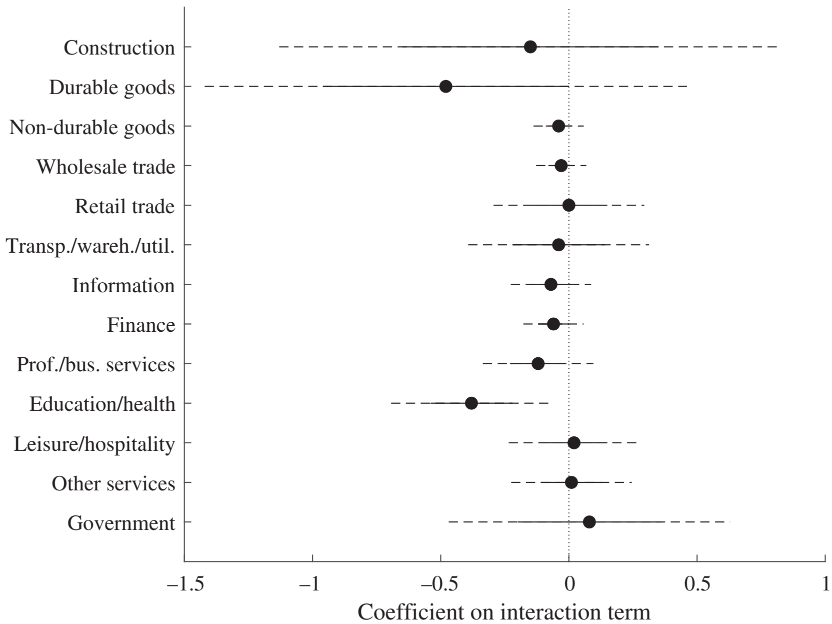

Figure 4. Estimated coefficients on the interaction between ARRA payouts and sector-specific excess exposure to the Great Recession.

Notes: Estimated coefficients $\widehat{\xi}_i$ from sector-specific 2SLS regressions $n_{i,s} = \alpha_i + \beta_i F_s + \xi_i F_s \widetilde{E}_{i,s} + \omega_i \widetilde{E}_{i,s} + \gamma_i' X_s + \varepsilon_{s,i}$. $\widetilde{E}_{i,s}$ is $(E_{i,s} - \text{mean}(E_{i,s}|i))/(\text{var}(E_{i,s}|i)^{1/2})$, where $E_{i,s} = (\text{Employment}_{s,i,11/2008} - \text{Employment}_{s,i,11/2007})/\text{Employment}_{s,i,11/2007} - (\text{Employment}_{s,11/2008} - \text{Employment}_{s,11/2007})/\text{Employment}_{s,11/2007}$. 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.

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.

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 percentage employment change for sector i in state s between December 2007 and December 2008 minus the percentage 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. 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 4 shows 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). The estimates for the interaction terms are insignificant for most sectors. When calculating the marginal effect of ARRA payouts from these regressions, we find that they are similar across the range of our excess exposure measure. Considering the construction sector as an example, our results show that jobs were created in this sector mostly irrespective of whether states suffered from a particularly strong "construction sector recession." In other words, also in states where the construction sector was hit only slightly more strongly than the economy as a whole, employment gains in construction

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due to ARRA payments were sizeable. This hints at the strong employment effects in the construction sector being mostly systematic and unlikely to be explained by the degree to which sectors had been affected by the Great Recession.

Structural characteristics. We now assess the importance of sectors' structural characteristics in explaining the distribution of ARRA effects.

Structural characteristics can influence *(i) sectoral fiscal multipliers* such that ARRA may have helped those sectors more that, for structural reasons, are more responsive to fiscal policy. For example, one can expect employment reactions to an impulse to be large in sectors that have relatively weak employment protection or where employment is more fluid, that is, more easily adjustable. In line with this, Nekarda and Ramey (2011) have documented larger effects of government spending in industries that have low unionization rates. Further, employment effects can be stronger in sectors that are more labor-intensive or where labor is less substitutable with capital services. The latter point is emphasized by Bredemeier et al. (2020, 2022) who show that fiscal policy is more effective in sectors with large shares of pink-collar workers.

Block (a) of Table 1 presents the relative employment gains by sector from \$10B in ARRA layouts, Gains_i, along with selected sectoral characteristics that have been discussed in the literature to affect sectoral fiscal multipliers. The last row summarizes the connection between ARRA effects and the respective characteristics by giving the slope coefficient from a linear regression of ARRA effects on characteristics. As can be seen, the employment gains from ARRA have no statistically significant relation to sectoral measures of labor market regulation, fluidity, labor intensity, or pink-collar intensity. These findings do not contradict the arguments above but instead indicate that sectoral fiscal multipliers do not explain the distribution of ARRA gains. To be clear, the literature has already identified the sectors where sectoral fiscal multipliers are large but these sectors are not the ones for which we find elevated effects of the ARRA, which implies that the reason why specific sectors benefitted strongly from the ARRA must rather be related to the size of the sector-specific impulses under the ARRA or the strength of the spillover effects.

The *(iii)* spillover effect can also be related to the structural characteristics of an industry. Such spillovers across sectors arise in the presence of intermediate inputs [see, e.g. Long and Plosser (1983)] as well as in the presence of capital flows across sectors [see, e.g. Horvath (2000)]. Sectoral spillovers imply that sectors that are central in the input-output and capital flow networks are more responsive to developments in other sectors, as demonstrated by Foerster et al. (2021). Specifically, Bouakez et al. (2021) have shown that fiscal policy has larger effects in more central (or "upstream") sectors. To investigate whether intermediate-input and capital flow centrality play a role in explaining our results, we calculate, for all sectors, the Katz-Bonacich centrality measure from make-use tables and the Katz-Bonacich centrality measure from capital flow tables. In block (b) of Table 1, we document the relationship between employment gains from ARRA and a sector's centrality in the intermediate-input and capital networks, respectively. Only centrality in the capital flow network is related to ARRA gains in the expected direction, but the connection is statistically insignificant.

A final hypothesis is that a sector may structurally attract large shares of government demand so that shrinking or inflating state budgets will significantly affect that sector, making *(ii) the sector-specific impulse* during ARRA large. Recall that the funds received through state fiscal relief were fungible, which made it a priori unclear which sectors will benefit from relief payments that materialized at the state level and whose distribution was determined in the political process (Leduc and Wilson, 2017). Most likely, it is easiest for state policymakers to adjust spending where a large amount of money is spent anyway. This is in line with Nekarda and Ramey (2011) who exploit for the identification of government spending effects that expansions and contractions of government purchases leave their distribution over industries largely unchanged. Such rigidity of the government spending mix can explain our key findings because construction spending accounts for a large share of discretionary spending by state governments. Usually, construction spending

Table 1. Estimated employment gains from $1B in ARRA payouts and selected structural characteristics of sectors

		(a) determinants of sectoral multipliers				*(b) determinants of spillover effects*	
Sector *i*	Gains$_i$	unionization	labor intensity	fluidity	pink collar occ. share	intermediate input centrality	capital flow centrality
Construction	1.04	0.14	0.42	0.05	0.02	0.04	0.13
Manufacturing	−0.03			0.03	0.05		
Durable goods	−0.01	0.12	0.67			0.09	0.30
Non-durable goods	−0.05	0.12	0.32			0.09	0.03
Wholesale and retail	0.10				0.55		
Wholesale trade	0.18	0.06	0.39	0.05		0.06	0.06
Retail trade	0.06	0.06	0.34	0.12		0.04	0.04
TWU	0.26	0.23	0.61	0.00	0.06	0.06	0.04
Information	0.25	0.13	0.35	0.02	0.15	0.06	0.05
Finance	0.03	0.02	0.33	0.02	0.27	0.12	0.03
Prof. and bus. serv.	0.22	0.03	0.37	0.06	0.23	0.15	0.08
Education/health	0.01	0.10	0.66	0.10	0.22	0.03	0.03
Leisure/hospitality	0.12	0.03	0.32	0.14	0.71	0.04	0.03
Other services	0.18	0.03	0.81	0.12	0.41	0.04	0.03
Government	0.01	0.40	0.41	0.07	0.33		
Est. slope coefficient		0.08	−0.08	−0.96	−0.54	−1.62	0.50
		(0.92)	(0.88)	(0.62)	(0.18)	(0.47)	(0.65)

Notes: Unionization is the 2007 share of jobs covered by collective-bargaining agreements (source: BLS, Union affiliation data from the CPS). Labor intensity is 2009 employment per output measured in jobs per $10K (source: BLS, employment projections, available in 10-year intervals). Intermediate-input centrality is the Katz-Bonacich centrality measure calculated from the make-use table reported in Foerster et al. (2021). Capital flow centrality is the Katz-Bonacich centrality measure calculated from the capital flow table reported in Foerster et al. (2021). For both centrality measures, we set the attenuate factor to 0.5, following Carvalho (2014). Fluidity is the 2007 separation rate (source: BLS, Job Openings and Labor Turnover Survey). Pink-collar occupation share is the share of workers in sales and related occupations or in office and administrative support occupations (source: CPS). Some characteristics are not available at our baseline disaggregation, in which we case consider the closest available alternative. TWU = transportation, warehousing, and utilities. Estimated slope coefficient is the estimate for λ_1 in regression $\text{Gains}_i = \lambda_0 + \lambda_1 \times \text{Characteristic}_i$, *p*-value in parentheses.

accounts for about 10% of state spending, and estimates of the share of unavoidable expenditures (primarily welfare spending) in total expenditures averages at about 60%, putting the share of construction spending in discretionary spending at about one-quarter. Gordon et al. (2019) find that about 80% of states' spending is fixed in some form (i.e. not easy for state legislatures to change), implying that construction spending may account for up to 50% of relatively easy-to-change state spending.

To investigate the hypothesis that the employment effects of ARRA occurred mainly in the construction sector because this sector typically receives a large share of discretionary spending, we exploit heterogeneity in the usual spending patterns of state governments. We have to restrict the subsequent analysis to the construction sector due to data availability. The Annual Surveys of State and Local Government Finances report state and local government expenditures by character and object, but the data cannot be mapped to NAICS industries. However, capital outlays for construction are explicitly listed for each state in each fiscal year. Under our hypothesis, we should observe that the effects of ARRA on construction employment were even larger in states that typically spend larger shares on construction than other states do. To test this implication, we run a similar interaction term regression as before, but this time, we use as interaction term

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Table 2. Regression results for employment change in construction sector, 12/2008 through 12/2010, ARRA payouts interacted with states' pre-ARRA construction expenditure as share of total expenditures (*p*-values in parentheses)

Total ARRA payouts × construction expenditure share	0.18
	(0.00)
Total ARRA payouts	0.43
	(0.02)
Dec-08 state employment/ population 16+	−0.60
	(0.50)
State employment change, Dec-07 to Dec-08	4.23
	(0.00)
GSP change, 2007Q4-2008Q4	1.54
	(0.02)
Construction expenditure share	−3.41
	(0.00)
Constant	−3.00
	(0.80)

Notes: Table shows results of the sector-specific 2SLS regression $n_{i,s} = \alpha_i + \beta_i F_s + \xi_i F_s \widetilde{D}_{i,s} + \omega_i \widetilde{D}_{i,s} + \gamma_i' X_s + \varepsilon_{s,i}$, where $\widetilde{D}_{i,s}$ is demeaned expenditures of state s in sector i as a share of the state's total expenditures in fiscal year 2008, $D_{i,s} = 100 \cdot \text{Expenditures}_{s,i,FY08} / \text{Expenditures}_{s,FY08}$. The endogenous terms F_s and $F_s \widetilde{D}_{i,s}$ are instrumented by the vectors Z_s and $Z_s D_{i,s}$. Due to data availability, this regression is feasible only for the construction sector.

the *pre-crisis* share of government expenditures for construction in total state and local government expenditures. Specifically, we augment our baseline empirical model (1) by the interaction between ARRA outlays and the construction expenditure share, and the expenditure share further enters the second stage as an additional control variable. We use the three instruments from the baseline regressions as well as their respective interactions with the expenditure share as instruments (giving a total of six instruments) to instrument ARRA outlays and the interaction term. For the pre-crisis construction expenditure share, we use data for the fiscal year 2008, which ends June 30, 2008, in most states and no later than September 30 in any state and hence before the ARRA.

The results of this interaction term regression are displayed in Table 2. The estimated coefficient on the interaction term is positive and statistically significant at the 1% level. Hence, ARRA transfers did indeed have more substantial effects on construction employment in states that had spent larger shares of their budgets on construction activities already before the ARRA. Quantitatively, the results suggest that a one percentage point increase in the pre-crisis expenditure share on construction enlarges the effect of $100,000 in ARRA transfers by almost 0.2 job-years. This finding supports the hypothesis that ARRA transfers were distributed across sectors in a way state governments distribute most discretionary funds.

To corroborate this view, Figure 5 shows that surges in states' construction sector spending during the ARRA period were *not* related to the drop in employment in that sector in the *pre-ARRA* recession period. The horizontal axis shows the construction sector's excess exposure in a state (as before, measured by the percentage sectoral employment change between December 2007 and December 2008 net of the percentage change in total employment). The vertical axis shows the rate of change in a state government's expenditures on construction activities net of the rate of change in the states' total government expenditure between the fiscal years 2008 and 2010. If anything, there is a slight *positive* relationship between the two variables, while an active channeling of ARRA funds to harder-hit sectors would imply a *negative* relation. This lends additional support to the "stickiness" of the sectoral composition of government spending.

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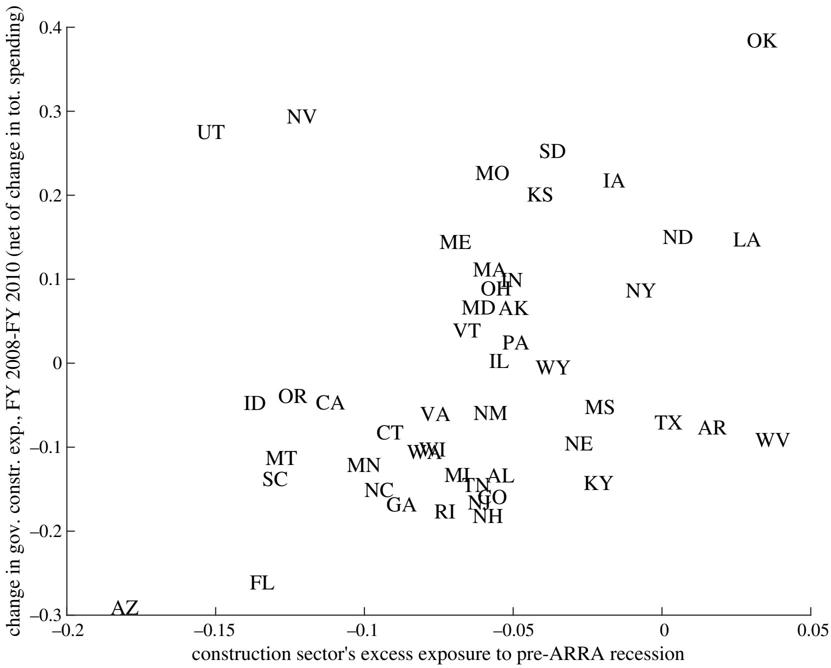

Figure 5. Changes in states' construction sector spending 2008-2010 by construction sector's excess exposure to pre-ARRA recession.

Notes: Horizontal axis: percentage employment change in construction sector between December 2007 and December 2008 minus percentage change in total employment, by state. Vertical axis: rate of change in (state and local) government expenditures on construction outlays minus rate of change in total (state and local) government expenditures, by state.

A cautious outlook. Our analysis suggests that state fiscal relief transfers are likely to stimulate employment mostly in sectors where state governments typically spend a lot of money, independent of whether these sectors are hit hard by the particular downturn. Given that the Great Recession was typical in terms of the distribution of recessionary job losses [Hoynes et al. (2012)], state fiscal relief may well lead to stabilizing the sectoral distribution of employment in most recessions. However, in recessions featuring a different distribution of job losses across sectors, state fiscal relief is unlikely to have this property automatically. This is illustrated in Figure 6, where we gauge the most likely outcome of another state fiscal relief program if the allocation of resources was similar to the one in the ARRA. Specifically, the figure plots our previously estimated employment gains through ARRA relief transfers against the job losses during the rapid employment drop at the onset of the COVID crisis (02/2020–04/2020). While the leisure and hospitality sector has been hit hardest by the COVID crisis, a program like ARRA would not have substantial employment effects in this sector, as documented by our analysis. Conversely, the construction sector, which experienced the most substantial employment gains from ARRA, was hit less hard during the COVID crisis.

To help the industries that have been struck during the COVID crisis, such as retail trade, leisure, and hospitality, 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. However, the COVID crisis is also unusual in terms of whether it was desirable or even possible to stabilize employment, especially in the sectors that are particularly hard hit by the crisis. In a pandemic, there is an additional trade-off since protecting hard-hit sectors may run against avoiding activities that contribute to infections. Also the trade-off between short-run stabilization of sectoral

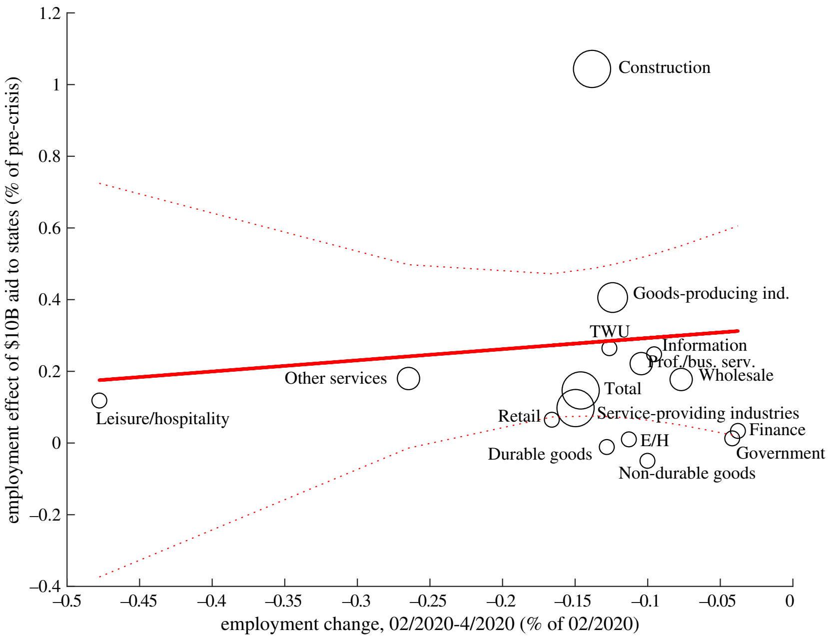

Figure 6. Employment effects of ARRA outlays vs. job losses during COVID crisis.
Notes: Vertical axis shows relative employment gains of \$10 billion of additional ARRA outlays, as defined in equation (4). The size of circles indicates the statistical significance of the underlying job-years coefficient. 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. E/H = Education/Health.

employment and long-run sectoral employment trends matters in the COVID crisis, which has hit hard the already struggling brick-and-mortar retail sector.

5. Conclusion

This paper has 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. We have provided descriptive evidence that ARRA outlays did have more substantial employment effects in sectors that experienced stronger employment losses in the crisis. At the same time, we did not find that state fiscal relief protected employment disproportionately in sectors that are in structural decline. To investigate the mechanisms behind our results, we have exploited differences between US states in the distribution of job losses in the Great Recession and in the usual spending patterns of state governments. We did not find that ARRA outlays affected a sector more strongly in states where this sector was hit harder by the recession, ruling out economic slack or the intentional channeling of the relief funds to hard-hit sectors as explanations for our results. Instead, we find that construction employment benefitted more strongly from ARRA transfers in states that, before ARRA, had spent larger shares of their budgets for construction activities, hinting at state and local governments' typical spending behavior as the likely explanation for our results.

Notes

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 is particularly suited to learn about the effects of state fiscal relief due to the detailed documentation of the outlays. The act included stringent 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.
2 There is a growing, related literature, analyzing the distributional consequences of fiscal policy; see, among others, Anderson et al. (2016), Bilbiie (2020), Ferriere and Navarro (2020), Giavazzi and McMahon (2012), Glomm et al. (2018), Klein et al. (2022), Misra and Surico (2014), and McKay and Reis (2016).
3 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.
4 As emphasized by Chodorow-Reich et al. (2012), it is unlikely that employment developments until November 2008 already reflected the anticipated effects of the ARRA stimulus. Important components of the ARRA did not become known to the public before December 2008.
5 See https://www.bls.gov/sae/data/home.htm
6 See https://www.bls.gov/lau/ and https://www.bea.gov/data/gdp/gdp-state, respectively.
7 First-stage F-statistics range from 39.8 to 46.1.
8 The difference between our estimate (2.03) and Chodorow-Reich (2019)'s estimate (2.01) is due to data revisions. For the aggregate employments effects of other fiscal policy measures, see, among others, Gehrke (2019), Monacelli et al. (2010), Pappa (2009), and Rahn and Weber (2019).
9 The fracking boom cannot easily be accounted for by, for example, 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. For example, 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 US Energy Information Agency).
10 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, see the upper panel of Figure A.1 in the appendix.
11 The lower panel of Figure A.1 in the appendix shows the counterpart to Figure 3 when we exclude the construction sector from the analysis.
12 For the role of slack for the aggregate multiplier, see, among others, Auerbach and Gorodnichenko (2012), Gomes et al. (2020), Linnemann and Winkler (2016), and Ramey and Zubairy (2018).

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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.

Table A.2 shows the full results of the augmented sector-specific 2SLS regressions where ARRA payouts are interacted with the respective sector's excess exposure to the Great Recession in the respective state. The regression equation is given by

$$n_{i,s} = \alpha_i + \beta_i F_s + \xi_i F_s \widetilde{E}_{i,s} + \omega_i \widetilde{E}_{i,s} + \gamma_i' X_s + \varepsilon_{s,i},$$

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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Table A.2. Regression results for sectoral employment changes, 12/2008 through 12/2010, ARRA payouts interacted with a sectors' excess exposure to Great Recession (*p*-values in parentheses)

	Construction	Dur.-goods manufctng.	Non-d.-goods manufctng.	Wholesale trade	Retail trade	Transport., wareh., util.	Infor-mation
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)

	Financial services	Prof./bus. services	Education/ health	Leisure/ hospitality	Other services	Govern ment
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)

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.

where

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

with

$$E_{i,s} = \frac{\text{Employment}_{s,i,t-1} - \text{Employment}_{s,i,11/2007}}{\text{Employment}_{s,i,11/2007}} - \frac{\text{Employment}_{s,11/2008} - \text{Employment}_{s,11/2007}}{\text{Employment}_{s,11/2007}}.$$

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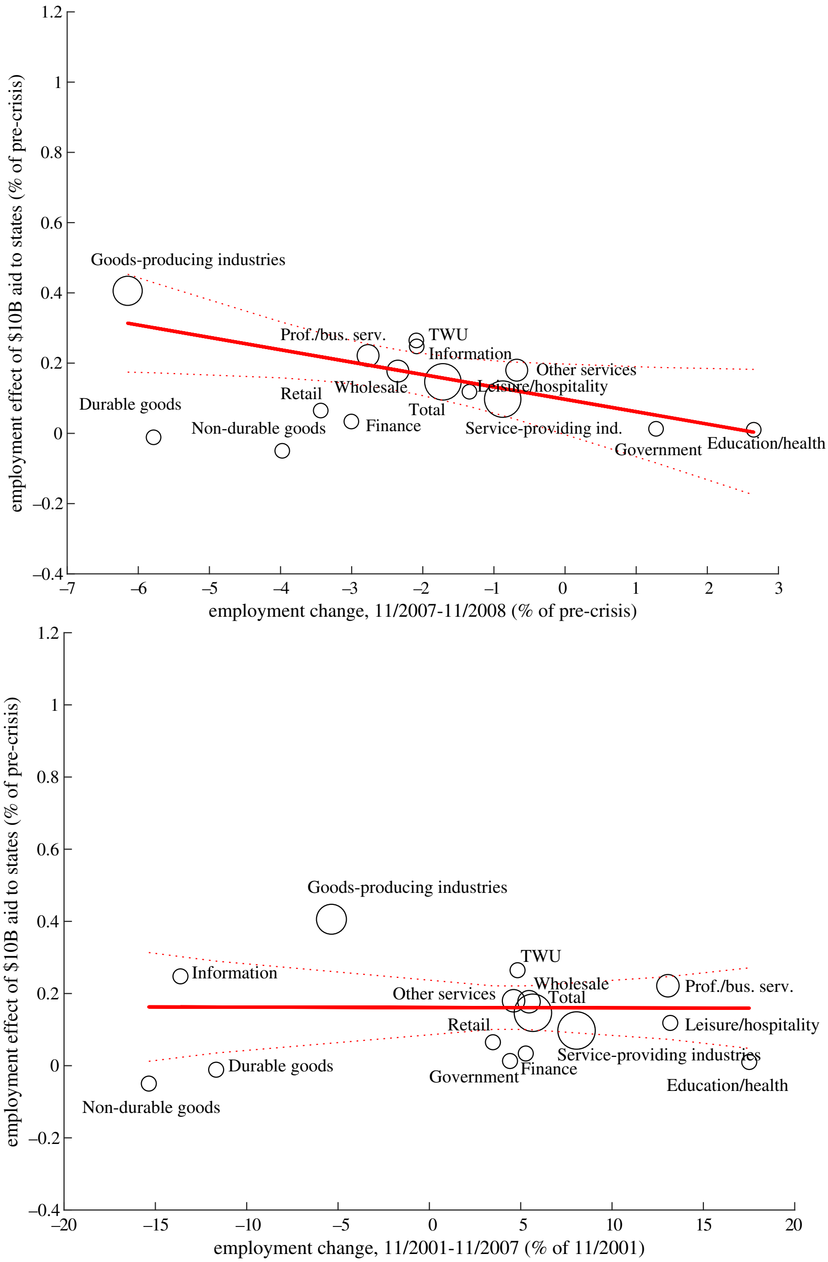

Figure A.1. Employment effects of ARRA outlays by sector's exposure to downturn and sector's long-run employment growth (w/o construction sector).

Notes: Vertical axis shows relative employment gains of $10 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. TWU = Transportation, warehousing, and utilities. Prof./bus. serv. = Professional and business services.

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

Figure A.1 repeats the information from Figures 2 and 3 in the main text but the construction sector is omitted from the scatterplots and the estimation of the fitted relations.

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