DEMAND STIMULUS AS SOCIAL POLICY

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Abstract: We exploit a panel of city-level data with rich demographic information to estimate the distributional effects of Department of Defense spending and its effects on a range of social outcomes. The income and employment generated by defense spending accrue predominantly to households without a bachelor's degree. These households as well as Black and Hispanic households tend to disproportionately benefit from this spending. Defense spending also promotes a range of beneficial social outcomes that are often targeted by government programs, including reductions in poverty, divorce rates, disability rates, and mortality rates, as well as increases in homeownership, health insurance rates, and occupational prestige. We compare the effects of defense spending with the effects of general demand shocks and explore reasons for the differential effects of the shocks.

JEL Keywords: Fiscal Policy; Inequality; Social Policy

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1. Introduction

The economic benefits of achieving full employment are not controversial and, indeed, are reflected in stated government policy objectives, such as the dual mandate of the Federal Reserve. It is also well understood that job losses associated with recession are especially severe among lower-income groups and racial minorities, whose unemployment rates are not only higher than for other groups but also generally more cyclically sensitive.[1] Thus, maintaining a strong economy does not simply serve the objective of increasing overall well-being, but potentially improves distributional outcomes as well through the pattern of employment gains.

However, the discussion of policies to address inequality typically does not focus on general macroeconomic stimulus, nor does the discussion of the macroeconomic effects of fiscal and monetary policy typically concentrate on distributional outcomes. For example, fiscal stimulus policies focus on aggregate demand and its components (e.g., consumption, investment), but their design typically does not account for how these policies can lessen inequality, particularly with respect to broader socioeconomic outcomes beyond employment status.

Consider defense spending, the largest single category of discretionary government spending in the United States. Department of Defense (DOD) contract spending is widely used as a source of variation to study the effects of fiscal stimulus, both because it is a large source of aggregate demand and because this type of spending is predominantly driven by forces unrelated to business cycles and hence provides a natural laboratory for assessing its economic impacts. Despite the importance of DOD spending from an economic and academic perspective, the literature has almost exclusively concentrated on estimating aggregate government spending multipliers (i.e., by how much GDP—or another measure of income—changes in response to a dollar increase in DOD spending), implicitly taking DOD spending as neutral in terms of distributional outcomes. Furthermore, defense spending is usually interpreted as tying up resources in ways that do not help address social issues. In his famous "Chance for Peace" speech (1953), President Eisenhower observed, "Every gun that is made, every warship launched, every rocket fired signifies, in the final sense, a theft from those who hunger and are not fed, those who are cold and are not clothed." In other words, DOD spending can impede the ability of the government to reduce inequality and help the disadvantaged.

In this paper, we pull back the curtain on broad distributional and social implications of government spending by jointly examining a large set of outcomes. Our approach expands upon

[1] For recent evidence, see Aaronson et al. (2019).

traditional studies of fiscal stimulus that focus on GDP and employment, and it provides a comprehensive view of the social effects of government spending based on a unified empirical setting. We aim to catalogue the sensitivity of these outcomes to changes in government spending and make a tentative attempt at aggregating potential returns that usually do not enter academic and policy debates on the effectiveness of government spending programs.

Specifically, we exploit detailed data on the location and timing of DOD contracts along with city-level data on economic and social outcomes across a large range of demographic categories. Our estimates of the distributional effects of DOD spending are based primarily on Quarterly Workforce Indicators (QWI) data, which provides a panel of local labor market outcomes by demographic group based on job-level administrative data. Most of our social outcomes use data from the American Community Survey (ACS), which since 2005 has reported survey respondents' Core Based Statistical Area (CBSA) of residence (alongside detailed demographic, economic, and social data). The CBSA-level panel from 2005 onward provides rich variation to estimate many dimensions of the social and distributional effects of DOD spending.[2] Local effects are of interest in their own right and are indicative of national-level effects under conditions highlighted in Chodorow-Reich (2019) and Auerbach, Gorodnichenko, and Murphy (2024; hereafter AGM24). National-level fiscal effects differ from CBSA-level effects in the presence of migration, and we find no evidence that DOD spending induces short-run local population changes. Therefore, our effects can be interpreted as affecting residents rather than composition effects due to migration, and they are informative of the national-level social effects of DOD spending.

We begin by documenting how the income generated by local DOD spending is distributed locally across demographic groups. Most of the wage and salary income created by DOD spending accrues to those with little formal education, those who are White, and those who are middle-aged. It is to be expected that minority groups will receive a small share of total income on account of being a small share of the workforce. Therefore, to determine the distributional effects of DOD spending, we also estimate the effects on average earnings. Adjusting for shares of existing income, increases in DOD spending increase the relative income of Blacks, Hispanics, and those without a bachelor's degree more than other demographic groups. We find that a DOD spending increase equal to a percent of local income generates an increase in overall average earnings of less than 0.5 percent, but a 0.7 percent increase in the average earnings of households without a bachelor's degree and

[2] We also examine other data such as local mortality rates and crime rates.

even larger increases in average earnings for Black and Hispanic households.[3] Thus, DOD spending can contribute to achieving one of the important objectives of many tax expenditures and direct transfers targeted at Americans with low levels of formal education.

Even within a demographic category, people have varying degrees of attachment to the labor force, with potentially different responses to DOD spending by employment status. We find that DOD spending increases employment rates across demographic groups and especially for those without a bachelor's degree, implying large benefits for otherwise unemployed workers.

Many of the public programs targeted toward low-income households not only support distributional objectives but also target outcomes associated with strong externalities. For example, as shown in the recent comprehensive survey by Aizer, Hoynes, and Lleras-Muney (2022), programs aimed at supporting the health and income of low-income families with children not only reduce childhood poverty and improve childhood nutrition but also have beneficial long-term effects in terms of education, earnings, health, and mortality. Indeed, the benefits may extend beyond those directly measured. For example, low earnings and employment have been found to lead to increases in crime (Raphael and Winter-Ebmer 2001, Machin and Meghir 2004).

We find that DOD spending reduces poverty, both for children and adults. Consistent with the decline in poverty, we find a diminished dependence on government programs that support low-income families. The share of households enrolled in the SNAP program (i.e., food stamps) decreases. Medicaid participation declines while health insurance coverage increases, indicating that DOD spending substitutes for costly in-kind benefits while promoting social objectives such as poverty alleviation. Respondents are also less likely to report being disabled, an effect that is most apparent among those without a bachelor's degree, the middle-aged, and Whites.

A separate set of programs targets job training and education, aiming to enhance Americans' earnings and career trajectories, as those on the lower rungs of the job ladder suffer persistent displacements and struggle to climb the job ladder (e.g., Krolikowski 2017).[4] We find that DOD spending has strong positive effects on occupational prestige—a measure of the quality of workers' jobs—with the benefits concentrated among households without a bachelor's degree. These households also experience a reduction in routine task intensity of their jobs.

[3] Since wage and salary income accounts for less than 100 percent of local income, it is expected that the response of wage and salary income to a DOD spending increase of 1 percent of local earnings is less than 1 percent.
[4] For example, the Department of Labor's Employment and Training Administration spends approximately $4 billion per year on grants to support workforce development (https://www.dol.gov/sites/dolgov/files/ETA/budget/pdfs/FY2022BIB_ETA.pdf)

As for programs not targeted primarily toward the poor, the U.S. devotes considerable resources to subsidies for homeownership, through the mortgage interest deduction (or, alternatively, the lack of taxation of imputed rent) and the partial deductibility of property taxes, often supported by the argument that homeownership promotes community stability and engagement. But these tax expenditures have been criticized as having relatively little impact on the homeownership rate, as opposed to the amounts of mortgage borrowing or housing owned (e.g., Gruber, Jensen, and Kleven 2021). We find that DOD spending increases homeownership, significantly so for some groups. Other measures of household formation increase along with homeownership. Marriage rates increase for White households, while divorce rates decrease noticeably for middle-aged households and Black households. White households also become less likely to live in multi-family homes, consistent with their higher homeownership and marriage rates.

We also examine the effect of DOD spending on mortality. We examine separately what Case and Deaton (2020) refer to as "deaths of despair" – drug-and-alcohol-related deaths and deaths by suicide – as well as health-related deaths, murders, and accidental deaths. While Case and Deaton emphasize the consequences of declining labor market prospects over prolonged periods of time, we provide a higher-frequency estimate of the relationship between labor market earnings (induced by aggregate demand stimulus) and deaths of despair. The expected effect of (DOD-induced) labor market improvements on health and mortality at higher frequencies is not obvious. For example, Ruhm (2000) finds that most sources of fatalities (with the exception of suicides) are procyclical, as are other measures of adverse health such as smoking and obesity. We find that increases in DOD spending *lower* rates of death. Health-related death reductions account for the majority of the overall decline in deaths, and mortality improvements are concentrated among those over age 45. Finally, we explore how defense spending affects crime rates. By and large, we find little evidence that DOD spending changes the intensity of crime. While the aggregate effect is not statistically significant, there is a significant reduction in vehicle thefts.

In summary, these social outcomes are associated with substantial social benefits that are not included in traditional measures such as output multipliers. These benefits accrue disproportionately to minorities and disadvantaged groups of the population. DOD spending reduces government outlays on social transfers and produces outcomes with positive externalities. We derive the social return on a dollar of DOD spending for each outcome that is relatively precisely estimated and only for cases in which the value of the outcome can be reasonably approximated. Even taking this conservative approach, our analysis implies that a dollar of DOD spending returns $0.27 in social value beyond the value implied by an increase in GDP.

The contrast between prior evidence on the procyclicality of mortality and our evidence from DOD spending shocks raises the possibility that there is something special about DOD spending shocks, which may also affect other social outcomes differently than representative demand shocks. If so, what about DOD spending shocks makes them special? To begin addressing these questions, we separately examine the social effects of traditional Bartik spending shocks constructed from local shares of two-digit industries and national industry growth rates. We refer to these Bartik spending shocks as "general demand shocks" since they are based on information across all private-sector industries. We find that although general demand shocks have similar effects on local total earnings, there are important differences for other social outcomes. Most strikingly, general demand shocks lead to increases in mortality (consistent with Ruhm 2000) and crime. We present evidence that these differences can be attributed to the differential distribution of shocks across U.S. cities. General demand shocks affect larger cities and lead to increases in pollution and traffic congestion (as measured by average travel time to work). Consistent with these observations, we find that mortality increases are concentrated among internal (health-related) and accidental causes of death. DOD shocks, by comparison, disproportionately benefit smaller cities, do not increase pollution, and are associated with a reduction in average travel time to work.[5]

The effect of general demand shocks on other social outcomes (e.g., disability, occupational prestige) are negligible compared to the effects of DOD spending shocks. These effects lead us to conjecture that the stronger social effects of DOD spending shocks are due to their ability to pull those without a bachelor's degree into employment. We explore this possibility by predicting changes in social outcomes among those without a bachelor's degree based on differential social outcomes among the employed and non-employed (and changes in the employment rate). These employment margins of social outcomes account for a large share of the differential social effects of DOD spending shocks compared to the effects of general demand shocks. Finally, we decompose changes in employment among no-bachelor's households into those arising from industry, city, and occupational composition of DOD spending shocks and general demand shocks. We find that, while industry composition accounts for some of the differential employment effects, city and occupational composition account

[5] Finkelstein et al. (2024) present a model of procyclical mortality; our results call for models in which mortality can be *countercyclical* depending on the aggregate shock and its distribution across space.

for the majority of the stronger employment effect of DOD spending shocks. A potential implication of our evidence is that demand shocks directed toward small cities and occupations/industries with larger shares of non-bachelor's households are likely to have strong social benefits while avoiding large costs of congestion and pollution.

The paper begins by presenting a simple formal framework that rationalizes our main results and categorizes outcomes into externalities of different types. The framework and subsequent empirical evidence focus on social outcomes not emphasized in prior studies of city-level demand shocks. For example, AGM24 documents the effects of DOD spending on city-level aggregates, such as consumption, hours, prices, and GDP. That paper presents a framework to rationalize these aggregate outcomes. The contribution of this paper is to provide a range of new estimates and synthesize their social implications.

2. Organizing Framework

We begin with a formal framework that can rationalize the main empirical results that we document below: a) DOD shocks disproportionately decrease unemployment for informally-educated workers in locations exposed to the shock and reduce the reliance on safety net transfers, b) DOD shocks improve social outcomes for informally-educated workers in these locations, and c) general demand shocks increase congestion more than DOD shocks because they disproportionately affect large, dense cities.

Our objective is to highlight the main model ingredients that can qualitatively capture these patterns. Therefore, we focus on a static economy with two types of workers (those with and without a bachelor's degree) and two types of locations (densely and sparsely populated).[6] Our model generates unemployment by assuming that rigid wages are above the market-clearing wage.[7] Furthermore, we assume that the production of DOD goods is weighted toward the labor provided by workers without a bachelor's degree (who are concentrated in sparsely populated cities), whereas the production of private goods is weighted toward workers with a bachelor's degree (who are concentrated in dense cities).

[6] Our model relates to contributions that includes heterogeneous exposure to shocks by occupation and level of formal education, for example Bergman, Jaimovich, and Saporta-Eksten (2024).

[7] Rigid wages are sufficient but not necessary to generate an employment margin response to demand shocks. An alternative but notationally intensive approach would be to explicitly model matching frictions. For example, AGM24 present a model in which DOD shocks reduce unemployment in the presence of employment matching frictions and flexible prices and wages. The rigid-wage model presented here can rationalize many but not all of the aggregate (across households) city-level outcomes explored in AGM24.

Under these assumptions, the model generates stronger employment-rate responses among less-educated workers in response to DOD shocks. Model extensions can rationalize the rest of the main results. For example, means-tested welfare transfers generate safety-net savings from demand shocks. Similarly, incorporating household-level externalities from higher income (e.g., more spending on children, which improves lifetime outcomes) generates social externality benefits of DOD spending. Finally, the differential mortality effects of general demand shocks and DOD shocks arise because in dense cities the congestion externalities of employment density are stronger than agglomeration benefits of employment density. In addition to rationalizing our main empirical results, the model implies that government spending (DOD or otherwise) can yield the strongest social benefits when it purchases goods produced by less-educated workers in small cities.

The economy consists of two types of locations, densely populated and sparsely populated, which are indexed by $\ell \in \{D, S\}$. There are two types of workers living in each location. Worker types are indexed by $e \in \{E, U\}$, where E indicates workers with a bachelor's degree and U indicates workers with less than a bachelor's degree. Workers are immobile across locations, and they inelastically supply labor $L_{\ell e}$ to produce exported goods under a linear production function and rigid wages $w_{\ell e}$. The sparsely populated location has fewer workers per unit of land. Normalizing land size across locations to unity, this assumption implies that $L_{DE} + L_{DU} > L_{SE} + L_{SU}$. As discussed in Section 3, workers without a bachelor's degree are disproportionately represented in sparsely populated cities, which implies that $\frac{L_{DU}}{L_{DE}} < \frac{L_{SU}}{L_{SE}}$.

Production

A perfectly competitive production sector combines the different labor outputs to create location-specific final goods that are purchased by the Private sector or by the DOD. The production function for the Private sector is

$$Y_\ell^{Private} = \left(\phi^{\frac{1}{\sigma}} L_{\ell U}^{\frac{\sigma-1}{\sigma}} + (1-\phi)^{\frac{1}{\sigma}} L_{\ell E}^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}},$$

and the production function for the DOD is

$$Y_\ell^{DOD} = \left(\theta^{\frac{1}{\sigma}} L_{\ell U}^{\frac{\sigma-1}{\sigma}} + (1-\theta)^{\frac{1}{\sigma}} L_{\ell E}^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}.$$

The DOD uses low-education labor more intensively (consistent with our evidence below): $\theta > \phi$. Firm's cost-minimization implies demand for labor by workers without a bachelor's degree is

$$L_{\ell U}^{d} = \frac{\theta}{1-\theta} w_{\ell U}^{-\sigma} P_{\ell}^{DOD^{\sigma-1}} \mathbb{Z}_{\ell}^{DOD} + \frac{\phi}{1-\phi} w_{\ell U}^{-\sigma} P_{\ell}^{Private^{\sigma-1}} \mathbb{Z}_{\ell}^{Private}, \qquad (1)$$

where $\mathbb{Z}^{j} \equiv P^{j} Y^{j}$ is aggregate spending by sector $j \in \{DOD, Private\}$ and P^{j} is the price index of sector j. Since wages are fixed, so are price indices, and we can simplify notation by collecting constant terms: $\Theta \equiv \frac{\theta}{1-\theta} P_{\ell}^{DOD^{\sigma-1}}, \Phi \equiv \frac{\phi}{1-\phi} P_{\ell}^{Private^{\sigma-1}}$. Hence, we can re-write equation (1) as

$$L_{\ell U}^{d} = w_{\ell U}^{-\sigma}\left(\Theta\, \mathbb{Z}_{\ell}^{DOD} + \Phi \mathbb{Z}_{\ell}^{Private}\right), \qquad (2)$$

Demand for high-education workers is defined analogously.

Employment

Wages are rigid above the market-clearing wage for each worker type. Therefore, the employment rate for worker type e in location ℓ is $\mathcal{E}_{\ell e} = \frac{L_{\ell e}^{d}}{L_{\ell e}}$ where $w_{\ell e} \gg w_{\ell e}^{*}$ and the market-clearing wage is implicitly defined by $L_{\ell e} = L_{\ell e}^{d}(w_{\ell e}^{*})$.

Households

We consider representative households for each location and worker type. Specifically, households of type e in location ℓ pool income and consume collectively. Household members are exposed to two types of externalities – those that operate at the household level and those that operate at the city level. As employment and income rise, households spend more on children's health and education, i.e., things that have relatively little effect on the immediate success of children now but have a large effect on their lives in the long run.[8] This additional spending is associated with positive externalities that were not internalized in the prior consumption allocation. To preserve notational simplicity, we assume that the positive externalities depend directly on household income rather than operate through components of the individual members' consumption bundle.

Other household-level externalities may include benefits from employment that affect outcomes such as divorce but are not fully internalized by the household decision-makers. For example, unemployment may be associated with financial stress that strains relationships (Poortman 2005). For ease of exposition, we consider a single concave function ξ that captures the

[8] Poverty reductions that lead to more spending on children can improve lifetime outcomes (e.g., Aizer et al. 2022). Given the dearth of evidence on the mechanism behind these improvements, we assume that the spending on kids is used to purchase imported goods such as nutritious food. An alternative approach would be to assume these goods are purchased locally, in which case the net effect would depend on feedback between labor demand for each worker type and workers' demand for local goods (e.g., Murphy 2016; Bergman, Jaimovich, and Saporta-Eckstein 2024).

net effects of these positive externalities of employment.[9]

Demand shocks also generate externalities that operate at the city level. We capture these costs and benefits through the function $\kappa(\mathrm{E}_\ell)$, where E_ℓ is total employment (and hence employment density) in location ℓ. We assume κ is concave-downward with a slope that is initially positive but eventually turns negative. The initial upward slope indicates that agglomeration externalities dominate at low employment densities. These agglomeration externalities include reductions in transportation time to work that may operate through thick market effects. The downward slope indicates that congestion externalities dominate at higher employment densities. That is, $\kappa'(\mathrm{E}_S) > 0 > \kappa'(\mathrm{E}_D)$. [10], where *S* and *D* are sparsely and densely populated locations

Household Problem. An individual worker i of type e in location ℓ maximizes utility

$$\mathcal{W}_{\ell e i} = \log C_{\ell e i} + \xi(I_{\ell e}) + \kappa(\mathrm{E}_\ell), \tag{3}$$

subject to the budget constraint

$$C_{\ell e i} = I_{\ell e i} + T_{\ell e i}(I_{\ell e}), \tag{4}$$

where $I_{\ell e i} \equiv w_{\ell e} L^d_{\ell e}$ is the individual's labor income (which on average equals household income). $T_{\ell e i}$ are means-tested safety net transfers, and we assume $-1 < T'_{\ell e i} < 0 \; \forall \; I_{\ell e}$ to capture the fact that transfers are falling in income and have a replacement rate less than unity. We also assume that transfers phase out at high household incomes: $T_{\ell e i}\left(w_{\ell E} L^d_{\ell E}\right) = 0$.

Equilibrium

Equilibrium consists of employment levels for each worker type in each location, given labor demand from the DOD and the private sector. These employment levels determine labor income, which determines transfers and household externalities.

The household's problem implies that increases in employment and income improve welfare both by increasing consumption and through positive household-level externalities captured by ξ. They can also improve welfare through city-level externalities, depending on whether agglomeration externalities dominate congestion externalities.

[9] An alternative approach is to assume these benefits of employment are internalized but are not captured by GDP. For example, workers may be aware that stable employment facilitates stable relationships. If wages were flexible, then these benefits would be internalized in the wage bargaining process. In our setting with fixed wages, workers cannot internalize the benefits. For simplicity, we subsume such benefits (external or internal but not captured in GDP) in ξ.

[10] Note that while the traditional treatment of city-level externalities abstracts from unemployment and models externalities as a function of the workforce, our evidence indicates the importance of explicitly modeling city externalities as a function of employment.

Demand Shocks

Given the allocations of households across locations and the DOD and Private labor demand functions, we can examine the effect of spending shocks on employment, welfare, and safety net outlays. We begin by examining the distribution of employment effects.

Lemma 1: *DOD shocks increase employment among workers without a bachelor's degree more than they increase employment among formally educated workers*.

This follows from differentiation of Equation 1: $\frac{dL_{\ell U}^{d}}{d\mathbb{Z}_{\ell}^{DOD}} = \Theta w_{\ell U}^{-\sigma}$, $\frac{dL_{\ell E}^{d}}{d\mathbb{Z}_{\ell}^{DOD}} = \Phi w_{\ell E}^{-\sigma}$. Since $\Theta > \Phi$ and $w_{\ell U} < w_{\ell E}$, it follows that $\frac{dL_{SU}^{d}}{d\mathbb{Z}_{\ell}^{DOD}} > \frac{dL_{SE}^{d}}{d\mathbb{Z}_{\ell}^{DOD}}$. This is consistent with evidence presented below.[11]

Lemma 2: *Demand shocks that increase demand for low-education workers lead to a reduction in safety-net transfers*. *Furthermore, DOD shocks result in a larger reduction in safety net transfers.* The first statement follows from the fact that demand shocks increase income, and transfers are inversely related low-income households' income. This model prediction is consistent with evidence below that demand shocks lead to lower food stamps and welfare receipt among low-education workers. The second follows from the fact that DOD shocks have stronger employment effects for low-education workers (Lemma 1), consistent with evidence below.

Lemma 3: *DOD shocks that increase demand for low-educated workers lead to increased social benefits (e.g., lower divorce and disability rates) that stem from the household income externality* $\omega_{\ell U} \equiv \xi(I_{\ell U})$.

This follows from $\frac{d\omega_{\ell U}}{d\mathbb{Z}_{\ell}^{DOD}} = \xi'(I_{\ell U})\frac{dI_{\ell U}}{d\mathbb{Z}_{\ell}^{DOD}} > 0$.

Lemma 4: *Demand shocks in uncongested cities increase social welfare through net agglomeration externalities, whereas demand shocks that disproportionately affect congested cities lead to decreased social welfare through congestion externalities*.

This follows from the properties of $\kappa(\mathrm{E}_{\ell})$: At first, κ is upward sloping due to agglomeration externalities. At higher employment density, κ slopes downward due to congestion externalities.

[11] We have assumed for simplicity that the elasticity of substitution across workers is the same in both sectors. In a more flexible setting, the relative effects of DOD shocks would depend on the relative elasticities.

Lemma 5: *Suppose the government plans to spend marginal dollars to maximize social welfare. It can purchase the DOD good or the Private good, and it can spend on any location. Under a utilitarian social welfare function, the welfare-maximizing decision is to purchase the DOD good from the uncongested city.*

The government solves $\max_{\{\mathbb{Z}_\ell^j\}} \mathcal{W} = \sum_e \sum_\ell \mathcal{W}_{\ell e}$ subject to $X = \sum_\ell \sum_j \mathbb{Z}_\ell^j$. To prove that the welfare maximizing allocation is $\mathbb{Z}_S^{DOD} = X$, it is sufficient to show that a) direct consumption benefits weakly dominate other spending allocations, b) household externalities weakly dominate the alternatives, and c) city externalities weakly dominate the alternatives. Result a) follows from concavity of welfare with respect to consumption and the fact that DOD shocks are tilted toward the labor of low-income households. This implies that purchasing the DOD good has the strongest effect on income (and hence marginal utility of consumption) of low-income households. Result b) follows from concavity of $\xi(I_{\ell U})$ and the fact that DOD shocks disproportionately increase income among low-income households. Finally, c) follows from $\kappa'(\mathrm{E}_S) > 0 > \kappa'(\mathrm{E}_D)$ which stems from the downward-parabolic shape of κ.

3. Data and Methodology

Our analysis exploits variation in DOD spending, which is derived from detailed data on the location and timing of DOD contracts. DOD spending provides an ideal setting through which to examine the effects of demand stimulus. Typically, it neither contributes directly to local infrastructure nor enters households' utility functions, thus isolating aggregate demand stimulus as the potential channel through which it can affect economic and social outcomes. DOD spending is also the largest category of discretionary government spending and is, therefore, among the most relevant components of aggregate demand controlled by the government.

Prior research has faced limitations on the outcomes that could be studied with DOD spending. One strand of the literature has examined national time series data, which can be combined with national economic data but has the limitation that national variation is relatively insignificant and confined to military buildups around wars (e.g., Auerbach and Gorodnichenko 2012; Ramey and Zubairy 2018). Another strand of the literature has focused on state-level spending (e.g., Nakamura and Steinsson 2014), which provides stronger variation and stronger identification but cannot be combined with as broad a range of outcomes as with national data. More recent work has exploited strong CBSA-level variation in DOD spending to examine fiscal multipliers over a shorter time span (Demyanyk, Loutskina, and Murphy 2019, hereafter DLM; and Auerbach, Gorodnichenko, and Murphy 2020; hereafter AGM20).[12]

Recent data advancements have made it possible to combine the short CBSA-level panel data on DOD spending with data on a range of social, economic, and demographic characteristics, a feature that we exploit in this study. In particular, the American Community Survey (ACS) contains respondent-level demographic, economic, social, and geographic information.[13] Detailed geographic information is available starting in 2005, including respondents' CBSA of residence for 290 different CBSAs. We use the ACS to create a CBSA-by-year panel of data on economic and social outcomes by demographic group. Data on other social outcomes are from the Centers for Disease Control and Prevention (CDC), which provides county-level information on mortality by age and cause of death.

We combine these data with Bureau of Labor Statistics data on earnings and employment from the Quarterly Census for Employment and Wages (QCEW), the Local Area Unemployment Statistics (LAUS), and Quarterly Workforce Indicators (QWI). A useful feature of the QWI is that it provides local labor market statistics by demographic group based on job-level administrative data and therefore provides stronger data coverage (both across workers within a CBSA and across CBSAs) on labor market outcomes than does the ACS. Specifically, the QWI combines labor market information from the Longitudinal Employer-Household Dynamics (LEHD) with demographic information from the Census to produce a time-series of earnings and employment by age, race, and sex. We compute average earnings and employment rates from the QWI data by demographic group using group-level population estimates provided by Census through the CDC. The resulting dataset covers 853 CBSAs, nearly three times the coverage of the ACS. The limitation of the QWI is that the education information is imputed based on other demographic characteristics (Abowd et al. 2005). Population estimates are also not available by education group, so we focus on QWI-based labor market outcomes by age, race, and sex. Other social outcomes are not available in the QWI. The underlying data for crime rates come from the Federal Bureau of Investigation (FBI). To gain further insights, we supplement these sources with data on pollution (Environmental Protection Agency) and voting outcomes (Chenoweth et al. 2020)[14].

[12] AGM24 document that the variation in CBSA-level DOD spending is orders of magnitude larger than that at the state and national level.

[13] The ACS data is provided through IPUMS (Ruggles et al. 2021).

[14] Chenoweth et al. (2020) compile election data from the U.S. Election Assistance Commission (https://www.eac.gov/research-and-data/datasets-codebooks-and-surveys) and MIT Election Science and Data Lab (https://electionlab.mit.edu/data).

A. <u>Government Spending Data</u>

Our measure of government spending shocks, from a data set developed in AGM24, uses data on DOD contracts, available at USAspending.gov. This data source contains detailed information on contracts signed since 2000, including the identity of the primary contractor, the location (zip code) where the majority of work is performed, the total contracted amount (obligated funds), and the duration of the contract. In most cases, we also observe the primary zip code in which contracted work was performed.[15]

The timing of contract obligations need not correspond with the timing of outlays to contractors nor with the timing of new production (production that would not have occurred in the absence of the contract).[16] To help isolate the component of DOD contracts associated with new production, DLM and AGM20 use information on the duration of each contract to construct a proxy for outlays associated with each contract over time. We use this proxy as our measure of DOD spending.[17] We also instrument for this DOD spending measure with a Bartik-type shock, which further isolates the component of DOD contracts associated with *new production*. AGM20 discuss the merits of the instrument, and we provide further details in the econometric discussion

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[15] To enable comparisons with prior work using a similar empirical design, our data run through 2016. Post-2016 data appears to exhibit a structural break in the distribution of DOD spending across locations, which limits their viability given our empirical design discussed below. This break is important because we use a Bartik shock as an instrument and this approach works only if we have a stable composition of aggregate spending based on military needs. Various potential reasons for the break can undermine the validity of our approach.

[16] Recent evidence has raised concerns that NIPA-based DOD measures do not accurately reflect the timing of production. For example, Brunet (2022) and Briganti and Sellemi (2023) show that government spending is often recorded at delivery, which occurs after production. We are less concerned about a potential timing mismatch in our study for several reasons. First, Brunet's Budget Authority measure (which corrects for timing mismatch) closely tracks NIPA measures of production post-2000. Relatedly, the timing mismatch emphasized in Briganti and Sellemi (2023) is highly relevant at the quarterly frequency but is less likely to be relevant at the two-year horizon in this study. Our results are similar over even longer (five-year) horizons, which further mitigates concerns about high-frequency timing mismatch. Finally, as emphasized by Briganti and Sellemi, any remaining timing mismatch will tend to bias the estimated effects of DOD shocks toward zero, which would imply that our results provide a lower bound for the benefits of DOD spending.

[17] To construct this spending/outlay measure by location, AGM20 and DLM derive a flow spending measure for each contract by allocating the contracted amount equally over the contract's duration. For example, for a \$3 million contract that lasts three years we assign \$1 million in spending for each year of the contract. We then aggregate spending across contracts in a location at each point in time to construct local measures of DOD spending. In addition to new contract obligations, the dataset also contains modifications to existing contracts, including downward revisions to contract amounts (de-obligations) that appear as negative entries. Many of these de-obligations are very large and occur subsequent to large obligations of similar magnitude. Furthermore, in many cases, de-obligations happen within days after obligations appear in the reporting system. When obligations and de-obligations with magnitudes within 0.5 percent of each other, both elements of the pair are considered to be null and void as it is unlikely that any outlays were associated with these temporary obligations. This restriction removes 4.7 percent of contracts from the sample. Although our DOD spending and NIPA national defense (purchases of intermediate goods + investment – R&D spending) have different coverage and hence different levels (see Appendix Figure 2), the correlation between the series is 0.99. To be clear, our analysis relies only on our contract-based measure of DOD spending and we do not use NIPA data.

below and in Appendix A. DOD contracts overwhelmingly flow to large companies – the top 10 percent of contractors receive over 98 percent of DOD spending – so the effects of DOD spending should be considered to be driven by typical defense spending rather than by requirements that some contracting be directed toward small and/or minority-owned businesses. Furthermore, DOD set-aside programs that favored minority-owned businesses or promoted affirmative action were cancelled or scaled down (see e.g., Enchautegui et al. 1997).

B. Data from the ACS

ACS respondents report labor force information, including pre-tax earnings, occupation, employment status, and labor force status. They also report demographic information, educational attainment, health insurance status, disability status, location of work (including the time it takes to travel to work), homeownership status, relationship status, and income support from the government, among other information. Detailed geographic information is available starting in 2005.

We aggregate the respondent-level information to create CBSA-level measures of economic and social outcomes by demographic group (education level, age, race, and gender). These measures include total earnings, average (across respondents) earnings, average transportation time to work (among those who are employed), and rates of employment, disability, homeownership, marriage, divorce, and health insurance.

We also examine poverty rates and occupational status, each of which is constructed by IPUMS based on other respondent-level information. IPUMS reports each respondent's income as a share of the Federal poverty line, and we consider a respondent to be poor if his or her income falls below 100 percent of this threshold. IPUMS also constructs a measure of occupational prestige (the Siegel prestige score) based on perceptions among survey participants at the National Opinion Research Center (Siegel 1971). To assess job-ladder effects, we also compute CBSA-level measures of the Autor and Dorn (2013) Routine Task Index (RTI) based on occupation-level measures of routine, manual, and abstract task inputs. We construct CBSA-level measures using representative population weights provided by IPUMS.[18] Table 1 reports summary statistics for each of our social outcomes across the CBSAs in our sample along with information on DOD spending characteristics.

[18] For some small CBSAs, there are some instances in which a small number of people from a demographic group (racial minority or young children) are interviewed in a year. To determine the influence of these instances on our estimates, we limited our sample to observations with at least 100 respondents in a demographic category and found that the results are stable to this and higher thresholds.

C. Mortality Data

The CDC provides county-level mortality data by age group and cause of death since 1999. One category is what Case and Deaton (2020) refer to as "deaths of despair" – drug-and-alcohol-related deaths and deaths by suicide. We also examine health-related deaths and deaths that are classified by the CDC as accidental. Accidental deaths include those caused by automobile accidents or other unintended mishaps. Such deaths could increase in response to DOD spending, as higher employment and work effort cause distractions that lead to accidents. Alternatively, higher income could reduce stress and decrease the likelihood of accidents.

We derive death rates by dividing total deaths by population counts provided by the CDC. When there are fewer than ten deaths in a county the CDC suppresses the actual death count. We derive lower and upper bounds on the number of deaths (by age and cause of death) by setting the number of deaths to 0 or 10, respectively, when the data are suppressed. We report the results for the lower-bound mortality rates and indicate the few instances in which estimates based on upper-bound mortality rates differ.

D. Crime Data

As discussed above, increases in wages and employment can also affect crime. The National Archive of Criminal Justice Data (NACJD), which is hosted by the University of Michigan, aggregates crime reports from the FBI's Uniform Crime Reporting (UCR) program to the county level. In this UCR program, police departments across the United States can voluntarily report the number of crimes committed in their jurisdictions. According to the FBI, over 18,000 law enforcement agencies report their data to the UCR. Relative to the data publicly available from the FBI, the NACJD has access to agency-level data from the FBI at a monthly frequency which allows the NACJD to impute missing data for incomplete records.[19] The NACJD data include crime statistics for violent crime, murder, aggravated assault, rape, property crime, robbery, burglary, larceny, vehicle theft, and arson from 1984 – 2016 with the years 1993 and 2015 missing.

To incorporate the more recent data available and in order to fill in the missing year of NACJD data that are still available from the FBI (2015), we created our own method of

[19] For any agency reporting data for all 12 months in a year, there was no imputation process. For any agency reporting data for between 3 and 11 months in a year, the final data used was imputed by multiplying the agency's crime data by a factor of [12 / number of months reported]. For any agency reporting data for 2 months or less, the final data used was set to zero. In the situation, however, that an agency resides in a state where another agency in that same state has a similar population measure and has a full 12 months of reporting, crimes are imputed using the crime rates of that similar agency.

aggregation. The FBI currently provides data at the city agency and county agency levels. To get a complete count of all crimes committed in a county, we summed the number of crimes reported by the county agency and all the city agencies that exist inside that county. In contrast to the NACJD data, since the FBI does not publicly provide monthly level data, we are constrained by not being able to impute data for city or county agencies that do not report data. The final product provides crime counts and crime rates for the 1984 – 2016 period with the exception of 1993, for which neither the NACJD nor the FBI provides data.

E. Econometric Specification

Our objective is to estimate the effects of DOD spending on the earnings of different demographic groups and on a range of social outcomes. When estimating effects on earnings, we adapt the specifications in AGM20 and Nakamura and Steinsson (2014) and estimate

$$\frac{Y_{d,\ell,t} - Y_{d,\ell,t-2}}{Y_{\ell,t-2}} = \beta^d \frac{G_{\ell,t} - G_{\ell,t-2}}{Y_{\ell,t-2}} + \psi_\ell + \alpha_t + error_{d\ell t}, \quad (5)$$

where d, ℓ, and t index demographic groups, locations (CBSA) and time (year), Y is wage and salary earnings, G is DOD spending, ψ_ℓ and α_t are location and time fixed effects, and the denominator on both sides of the regression is lagged city-level earnings. β measures the local DOD earnings multiplier, that is, the dollar amount of earnings for group d produced by a dollar of local DOD spending over a two-year period of time. Whereas AGM20 focus on one-year effects, we examine two-year effects, as some social outcomes are likely to respond over multiple years. As we discuss below, we also examine longer-run (5-year) effects of DOD spending and find that they are generally similar to our reported 2-year effects. When estimating effects on growth in average earnings, we replace the dependent variable with $\frac{\bar{Y}_{d,\ell,t} - \bar{Y}_{d,\ell,t-2}}{\bar{Y}_{d,\ell,t-2}}$, where $\bar{Y}$ is average earnings. When estimating rates of change of other social outcomes, we replace the dependent variable with $X_{\ell,t} - X_{\ell,t-2}$, where X represents for example rates of poverty, death, divorce, etc.[20]

We instrument for variation in government spending $\frac{G_{\ell,t} - G_{\ell,t-2}}{Y_{\ell,t-2}}$ using a Bartik-type instrumental variable (IV) shock, $\frac{s_\ell \times (G_t - G_{t-2})}{Y_{\ell,t-2}}$, where s_ℓ is the location's average share of DOD contract spending over the relevant period and G_t is aggregate contract spending in period *t*. As discussed in AGM20, this Bartik-type IV approach not only addresses potential endogeneity

[20] To limit the influence of extreme observations, we winsorize all variables at the 1% and 99% levels.

concerns but also isolates the component of DOD contracts that is actually associated with new production. Many DOD contracts represent payment for new production as well as payment for production that would have occurred anyway, either because the specific contract was anticipated or because firms smooth production over lumpy contracts. AGM20 argue that the Bartik-type IV approach isolates the relevant component of $\frac{G_{\ell,t}-G_{\ell,t-2}}{Y_{\ell,t-1}}$ associated with *new* production by using information on contemporaneous changes in national production.[21] Thus, we report only IV-based estimates for each outcome. DLM and AGM20 find that OLS estimates tend to be considerably smaller than IV estimates when estimating CBSA-level DOD output multipliers, consistent with the noise in the raw DOD spending measure. Our OLS results are in line with these prior studies.

Our IV approach relies on cross-sectional variation in s_ℓ along with time-series variation in national DOD spending. Appendix Figure 1 illustrates the rich cross-sectional variation in s_ℓ, and Appendix Figure 2 depicts the 2000-2009 run-up and subsequent draw-down of national DOD spending. To evaluate whether other CBSA-level covariates are driving variation in the DOD shock, AGM24 estimate specification (1) using local characteristics in place of s_ℓ, as recommended by Goldsmith-Pinkham et al. (2020). None of the characteristics yields statistically significant output effects, nor do any yield first-stage F-statistics of significance. This offers reassurance that the DOD shock is indeed capturing exogenous DOD spending rather than other CBSA-level characteristics. Furthermore, AGM20 provide case-study evidence that is consistent with the exogenous nature of our DOD shocks.[22]

Our DOD shocks are highly persistent (0.96 first-order serial correlation), and it is possible that our estimates include the lagged effects of past shocks. Our intention is to capture the net effects across time. We conjecture that our empirical specification achieves this objective since contemporaneous effects of lagged shocks that are captured by our estimates are offset by lagged effects of contemporaneous shocks that are not captured by our estimates. We assess the conjecture empirically by estimating the effects of long-horizon (five-year) shocks on outcome changes over five years, and our estimates are generally close to our short-horizon estimates. The long-horizon

[21] Appendix A presents the full argument from AGM20. The new production shocks may nonetheless be anticipated if residents of CBSAs with large shares of DOD spending expect that they will receive disproportionate shares of spending from an anticipated war build up. Such anticipation effects, if present, would bias our coefficients toward zero by rendering a portion of the effects prior to our observation of the shock (Ramey 2011).

[22] After Lockheed Martin was awarded $200 billion to build a new fighter jet in Fort Worth, its labor market expanded significantly compared to neighboring Dallas, while Dallas and Fort Worth shared the same pre-trends before the contract was awarded.

estimates for our primary outcomes of interest are in Online Appendix Tables 1 and 2.

Our tables report standard errors based on hypothesis tests from individual regressions. We also jointly computed p-values among regressions in our main table using Romano and Wolf's (2016) multiple-hypothesis test and the statistical significance of our estimates is generally similar.

4. Empirical Results

To highlight heterogeneity in the effects of DOD spending on socioeconomic outcomes, we report the effects of a local DOD spending shock on labor market outcomes and social outcomes by demographic group. We begin by addressing the important yet straightforward question: who benefits from DOD spending? We report total earnings to provide a sense of which demographic groups receive the most income generated by DOD spending. It is to be expected that minority groups will receive a small share of total income on account of being a small share of the workforce. Therefore, to determine the distributional effects of DOD spending, we also estimate the effects on average earnings.

Labor market earnings can increase through the intensive or extensive margins of employment. The prevalence of each of these adjustment margins delivers important information on the distributional effects *within* each demographic group. Does DOD spending pull workers into employment, or do the benefits accrue exclusively to previously employed workers? To answer these questions, we estimate effects of DOD spending on employment rates by subgroup. We also examined labor force participation and local population margins. The responses are generally negligible and not statistically different from zero (and hence we do not report them), consistent with the evidence in AGM24. The negligible population response implies that the earnings benefits of DOD spending accrue to pre-existing residents rather than reflecting composition effects due to migrants. We then turn to the social implications of DOD spending by estimating effects on outcomes from the ACS data and then on mortality rates from the CDC.

For each demographic category we present the p-value of F-tests of equality of coefficients across demographic groups within that category. Appendix Table 3 reports the magnitude of group-by-group differential effects along with p-values of the difference.

A. Labor Market Effects of DOD Spending

Table 2 reports the effect of DOD spending on total labor market earnings, average labor market earnings, and the employment rate. The top row reports the effect on ACS-reported earnings for the whole ACS sample. For comparison, the second row reports results from the QCEW. The measure of average earnings from the QCEW is total earnings divided by the number of employed (rather than the sample population, as in the ACS), which will tend to imply lower average earnings effects than in the ACS because of the increase in employment. Estimates from the two different data sources are comparable and not statistically distinguishable, which lends credibility to the estimates.[23] The remaining rows present estimates by demographic groups in the ACS and QWI.

According to column 1, a dollar of local DOD spending increases ACS labor earnings by \$0.55.[24] The estimate in columns 2 and 3 imply that a percent increase in DOD spending (as a share of local earnings) generates a \$0.42 increase in average ACS earnings and an increase of the local employment rate by 0.21 percentage points.

B. <u>Distributional Effects</u>

The remaining estimates in Table 2 provide information on the demographic groups that benefit the most from DOD spending shocks. Those without a bachelor's degree benefit more than formally educated workers in terms of total amounts (column 1), percent increase in average earnings (column 2), and the increase in the employment rate (column 3). The effects in columns 1 and 3 are statistically stronger for those without a bachelor's degree.[25]

The labor market estimates for other demographic groups are based on QWI data, which provide more power than do the ACS data; the estimates from the ACS are qualitatively similar but less precise (Appendix Figure 5). The first-stage F-statistic for the QWI regressions is 143.4, a reflection of the much larger set of CBSAs than in the ACS regressions. Appendix Table 4 presents estimates of the magnitude of the differential effects across demographic groups along with the statistical significance of the differences. Blacks and Hispanics experience much larger (and statistically significant) increases in average earnings than do Whites. For example, a percent increase in DOD spending as a share of local earnings is associated with a 0.36 (0.46) percent larger increase in average earnings for Black (Hispanic) households than for White households. Average earnings also disproportionately increase for Males compared to Females. The groups that disproportionately benefit

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[23] While not statistically distinguishable, the earnings estimates from the ACS are lower than those from the QCEW. This could be due to the fact that ACS earnings is based on survey respondents' self-reported earnings, while QCEW earnings are based on administrative data. For example, even though both datasets intend to capture pre-tax earnings, it is possible that ACS respondents tend to report observed (post-tax) earnings.

[24] The dependent variable in the total earnings regressions is change in total earnings (from ACS or QCEW) divided by lagged QCEW earnings. In all regressions, DOD spending and its instrument are divided by lagged QCEW earnings.

[25] Appendix Figures 3 and 4 present graphical evidence of these differential effects. In particular, the differences in labor market outcomes are increasing in the (residualized) DOD instrument, across time periods (Appendix Figure 3) and exclusively during the initial five-year run-up in DOD spending (Appendix Figure 4).

from higher average earnings also experience larger increases in employment rates. With the exception of Blacks (compared to Whites), these employment-rate differences are statistically significant. (See Appendix Table 4.) In addition, young households experience much larger (and statistically significant) increases in employment rates than do middle-aged households.

C. Social Outcomes (ACS)

The earnings and employment rate responses of lower-income demographic groups indicate that DOD spending helps achieve distributional social objectives. To what extent do these income effects lead to other desirable social objectives and/or reduce dependence on government-funded programs? Table 3 through 7 presents the estimated effects for a range of social outcomes. We begin by reporting results based on adult ACS respondents between ages 20 and 70. Since outcomes such as poverty can have very different short- and long-run effects for children than for adults, we subsequently present a relevant subset of results for children by different age groups.

When possible, we infer the dollar value of the social benefit of DOD spending based on the amount of DOD spending it takes to change the social outcome for a person and the per-person value of the change. These calculations depend on average QCEW earnings per capita, which in our data is just under $17,000. They also depend on the social value of the outcomes, which tend to be tentative in nature. Table 8 reports our best estimate of the social return for various outcomes for which reasonable approximations of social value can be derived. Typically, effects on social outcomes are more precisely estimated for subgroups than are effects for the whole population, a reflection of likely heterogeneous effects of DOD spending by demographic group. When computing social benefits, we limit our analysis to the demographic groups that exhibit relatively precisely estimated effects for an outcome. Specifically, for all outcomes except Medicaid coverage, we limit our analysis to demographic groups for which estimated effects have a p-value<0.1. For Medicaid, we slightly relax this threshold and focus on the sub-groups with the most precisely estimated effects (p-value<0.17). Our statistical tests for Medicaid have less power due to the shorter panel of data. But we also have strong priors that Medicaid coverage should respond mechanically to the poverty reduction caused by DOD spending, consistent with the poverty, food stamp, and welfare receipt effects based on longer panel data.

Our social benefits fall into two categories: improvements in well-being not already included in increases in before-tax earnings, and reductions in expenditures on government safety-net programs, either directly measured in our empirical results or based on estimates of potential cost savings. We include these estimates of potential cost savings in lieu of estimates of improvements in well-being for outcomes that are broad in nature and/or difficult to quantify, e.g., reductions in child poverty. The logic is that existing government programs directed, e.g., at reducing child poverty reflect the policy judgment that such expenditures have social benefits at least as large. Each type of social benefit increases the net benefits per dollar of DOD spending, either by increasing benefits or by reducing net government costs.

Poverty and welfare. According to column 1 of Table 3, a percent increase in DOD spending (as a share of local earnings) reduces the poverty rate by 0.08 percentage points. The effects are entirely accounted for by those without a bachelor's degree and are also particularly strong among Whites and males. The reduction in poverty naturally reduces dependence on in-kind transfers. Eligibility for food stamps is tied to income, and as expected the increase in income and decline in poverty translates into a reduction in food stamp rates that is of a similar magnitude as the reduction in poverty rates (column 2). So, if the average per-person earnings in a city is $17,000 and the average food stamp benefit is $1,500,[26] then DOD spending of $17,000 saves 0.08×$1,500=$120 of food stamp payments (i.e., a dollar of DOD spending saves ~$0.01 of food stamp outlays). The reduction in poverty also reduces eligibility for welfare payments and Medicaid. The ACS reports total welfare payments, which permits us to directly estimate that a dollar of DOD spending saves $0.005 among households without a bachelor's degree (column 3). And according to column 4, a percent increase in DOD spending is associated with a reduction in Medicaid take-up that is (relatively) precisely estimated among White households. DOD spending equal to local earnings reduces Medicaid receipt among White households by 20 percentage points, which implies that the DOD can spend $\frac{\$17{,}000}{\text{person}} \times \frac{1\text{ person}}{0.71\text{ White persons}} \times \frac{1\text{ White person}}{0.2\text{ White Medicaid recipients}} \approx \$119k$ to reduce the Medicaid rolls by one person. According to Medicaid.gov, the median (across states) per-person cost of Medicaid is $8,436, which implies that a dollar of DOD spending yields a return of 8,436/119k=$0.07 in social net savings. The estimates for other demographic groups are more imprecise, so we do not include them in the savings return to DOD spending.

Other Social Benefits. This reduction in Medicaid take-up is more than offset by increases in health insurance coverage (column 5), although the estimates are imprecise, a reflection of the shorter time span for which health insurance is reported in the ACS. Our estimate of the net gain in health

[26] See https://www.fns.usda.gov/sites/default/files/resource-files/34SNAPmonthly-7.xls

insurance coverage is 0.34 among the full population and 0.65 (statistically significant) for those age 41-61, which implies that the DOD must spend $\frac{\$17{,}000}{\text{person}} \times \frac{1 \text{ person}}{0.44 \text{ Person age 41-61}} \times \frac{1 \text{ Person age 41-61}}{0.65 \text{ insured}} =$ $\$59k$ to extend health insurance coverage to another person. The extent to which increases in private health insurance provide social benefits is unclear. If health insurance is provided as part of compensation, then one might argue that its social value is already incorporated in the measured increase in income. Even if those newly enrolled in health insurance value the insurance less than the cost of that insurance (as suggested, for example, by Finkelstein, Hendren, and Luttmer 2019), that discount could reflect either cross-subsidization of other, sicker workers or increased profits of health care providers, in both instances representing an increase in real private incomes. However, there is also evidence that increases in health insurance coverage relieve pressure on government safety-net programs. Coughlin et al. (2013) estimate that at least 65 percent of the unreimbursed costs to health care providers for uninsured individuals are reimbursed by government programs targeted at covering these costs. Using their estimate of $1,257 unreimbursed care per uninsured individual, this amounts to a reduction in government safety-net spending of $817 per newly insured individual. Therefore, the savings associated with extended health insurance coverage from a dollar of DOD spending is 817/59k=$0.014.

While social transfers are directly tied to income and poverty, other social outcomes are less directly related to income. Disability in particular is a health condition with an ex ante unclear relationship to short-term economic conditions. Maestas et al. (2021) document a strong effect of the Great Recession on applications for disability insurance. While the incentives to file for disability insurance during a downturn (conditional on potential disability) are clear, it is less apparent whether self-reported disability responds to economic conditions, including DOD spending. We find that DOD spending indeed affects self-reported disability rates (column 6), especially for some demographic groups that receive the most earnings benefit from DOD spending. Our estimate of 0.115 implies that the DOD must spend $\frac{\$17{,}000}{\text{person}} \times \frac{1 \text{ person}}{0.73 \text{ Non-bachelor's}} \times$ $\frac{1 \text{ Non-bachelor's}}{0.115 \text{ Non-bach disability}} \approx \$202k$ to prevent a (non-bachelor's) self-reported disability.[27]

Work-related outcomes. Jobs confer immediate income-related benefits to workers. They also

[27] Lacking any measure of the potential benefit from this disability reduction, we do not include it in our overall accounting in Table 8.

affect workers' lifetime trajectory of income and other life outcomes (e.g., Blanchflower and Oswald 2004). Job losses tend to have highly persistent adverse effects on workers, as displaced workers tend to be hired in lower-ranking and lower-paying jobs and only slowly work their way back up the job ladder. These adverse consequences are mirrored by the benefits to workers who maintain their jobs and climb the job ladder. To what extent do DOD spending shocks affect workers' occupational status? Column 7 demonstrates a substantial increase in occupational prestige, with an increase in DOD spending equal to local earnings causing a 2.2-point increase in a location's average occupational prestige score. By point of comparison, the standard deviation of occupational prestige across CBSAs is 1.8 (Table 1). Similarly, DOD shocks reduce the RTI (column 8), significantly for those with no bachelor's degree.

In addition to benefitting from the increase in occupational standing, which we do not attempt to value, households also benefit from a reduction in transportation times to work: DOD spending equal to local earnings causes a 6.8-minute-per-day reduction in average transportation time to work across workers and a 14-minute reduction in daily (to-and-from work) transportation time. This implies that if the DOD spends \$17k (the average local QCEW earnings per worker), it saves the average worker $\frac{14 \text{ minutes}}{\text{day}} \times \frac{1}{60} \frac{\text{hour}}{\text{minutes}} \times 5 \frac{\text{days}}{\text{week}} \times 50 \frac{\text{weeks}}{\text{year}} = 58.3$ hours per year. Even if the value of time is as low as \$10 an hour, this implies a massive annual economic benefit to workers of approximately $\$583$ and the DOD generates $\$583/\$17k = \$0.034$ of economic benefit for each dollar spent. The reduction in average travel time is consistent with the notion that DOD spending facilitates spatial agglomeration among workers and employers. For example, there may be more job opportunities closer to workers' residences. Alternatively, workers may have the resources to move to locations closer to job clusters. The latter would be consistent with the increase in homeownership and reduction in multi-family housing for some demographic groups. Appendix Table 5 presents evidence that workers and firms indeed are more spatially proximate after a DOD shock. Workers are less likely to drive to work (column 1), and the reduction in driving is consistent with increases in driving alternatives such as taking public transportation and/or walking/biking to work (columns 2 through 4). To evaluate the proximity hypothesis, we compute annual measure of employment-weighted density using zip-code-level employment counts from the County Business Patterns dataset. CBSA-level employment-weighted density is the employment-weighted average of employees per square mile across zip codes in a CBSA. Our estimates (column 5) indicate that a percent increase in DOD spending (as a share of local earnings) leads to a 0.75 percent increase in employment-weighted density, that is, employment becomes more spatially concentrated.

Marriage, divorce, and household formation. Individual incomes have been shown to have a variety of effects on marriage and divorce rates (Burgess, Propper, and Aassve 2003). If marriage is a path to financial security, higher income may reduce incentives to marry. Alternatively, if marriage is a signal that one is financially stable enough to support children and afford a home, then higher income may result in higher likelihood of marriage. We find that DOD spending shocks have differential effects on marriage across demographic groups (Table 4). Whites are more likely to be married and more likely to own a home. In particular, a DOD shock that raises White households' average earnings by 56.0 percent (Table 2) is associated with a 9.1 percentage-point increase in homeownership among this group. White households are also more likely to own a home, less likely to live in a multi-family home, and less likely to be a single parent, which suggests that the income generated by the DOD spending shock indeed facilitates household formation for people in this demographic category. For Black and Hispanic households, our estimates are imprecise. Home values tend to increase, significantly so for non-bachelor's and young households, even though homeownership does not increase for these groups. One possible explanation is that new bachelor's homeowners drive up home values for pre-existing non-bachelor's homeowners.[28]

Divorce falls substantially for middle aged households. Our estimate of -0.124 implies that the DOD can spend \$322k to prevent a divorce. Thomas and Sawhill (2002) estimate that sustaining marriage is associated with an annual benefit equal to 43 percent of earnings. Based on this estimate, the annual return to a dollar of DOD spending is $\left(\frac{\$17k}{\text{person}} \times 0.43\right)/322\text{k} = \$0.023$.

Childhood Poverty. The effects of poverty are particularly severe for the life trajectories of children (Aizer, Hoynes, and Lleras-Muney 2022). Therefore, it is helpful to examine poverty responses for children separately than for adults. Table 5 (columns (1)-(3)) reports that poverty rates tend to decline for children, especially for those age 6 to 10. One way to measure the value of poverty reduction is based on the cost of government programs targeted at poverty reduction. According to Burns and Fox (2022), the 2021 expansion of the Child Tax Credit lifted 2.1 million children out of poverty at a cost of \$105.1 billion. Thus, the cost per child lifted out of poverty for one year was \$50,000. Our estimates in Table 5 indicate that the DOD can spend

[28] This would be consistent with the evidence in Murphy (2024) that during CBSA-wide housing expansions, housing demand by bachelor's households increases home values for non-bachelor's households.

$\frac{\$17{,}000}{\text{person}} \times \frac{\text{1 people age 6-10}}{\text{0.188 Poor age 6-10}} \times \frac{1}{0.104} \frac{\text{people}}{\text{people age 6-10}} \approx \$869k$ to move a young child out of poverty. If the value of this reduction in poverty is $50k, then a dollar of DOD spending yields $0.058 in poverty-reduction value; put another way, each dollar of DOD spending would obviate the need for $0.058 of spending on the child credit aimed at lifting children out of poverty.

Consistent with poverty reductions, Medicaid receipt among children falls substantially. There is no detectable change in health insurance rates.[29] Our estimates imply that the DOD must spend $233k to prevent Medicaid receipt among a child aged 6 to 10. According to Medicaid.gov[30], the median (across U.S. states) cost per child of Medicaid is $3,556. This implies that a dollar of DOD spending saves $0.015 in kids' Medicaid costs.

D. Mortality

Table 6a (columns 1 and 2) reports the effect of DOD spending shocks on various categories of mortality. To maintain consistency with our reporting of other social outcomes, the reported dependent variable is the change in mortality rate. In contrast with the ACS social outcomes (for which rates are directly inferred from respondent-level data), the mortality rates are based on population estimates.[31] Mortality rates tend to decline in response to an increase in DOD spending (although this estimate is imprecise), with internal (health-related) deaths accounting for nearly all of the decline. When the sample is restricted to ACS cities, there is a noticeable reduction in drug-and-alcohol-related deaths, although this estimate is also imprecise.

When examining mortality by age category (columns 1 and 2 in Table 6b), there is a quantitatively and statistically significant decline in deaths among those over age 45. A percent increase in DOD spending as a share of local income leads to 2.41 fewer deaths among those between ages 45 and 65 per 100,000, and to 7.60 fewer deaths among those over age 65 per 100,000. This implies that with average earnings of approximately $17,000, the DOD can spend $0.01 \times \frac{\$17{,}000}{\text{person}} \times \frac{\text{100,000 people age 45-65}}{\text{2.41 deaths age 45-65}} \times \frac{\text{people}}{\text{0.31 people age 45-65}} \approx \$22.8$ million to save the life of someone age 45-65 and

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[29] Information on health insurance and Medicaid status are available starting in 2008. The First-stage F-statistic for regressions based on this shorter time span of data are considerably lower, which reflects the much lower variation in national DOD spending in the post-recession period. National DOD spending surged through the Great Recession, peaked in 2010, and then decreased through the remainder of our sample. The post-2008 data do not exploit this large fluctuation in national spending.

[30] https://www.medicaid.gov/state-overviews/scorecard/how-much-states-spend-per-medicaid-enrollee/index.html

[31] We separately examine mortality growth (not reported), which exhibits an economically and statistically significant decline of -0.138 (standard error 0.067).

can spend $0.01 \times \frac{\$17{,}000}{\text{person}} \times \frac{100{,}000 \text{ people age } 65+}{7.60 \text{deaths age } 65+} \times \frac{\text{people}}{0.18 \text{ people age } 65+} \approx \$12.4$ million to save a life of someone age 65+. The social value of this effect depends on the (unknown) persistence of mortality reduction. As a lower bound we assume the life is saved for only a year. And using the value of a life-year of \$369k from Kniesner and Viscusi (2019), this implies that a dollar of DOD spending yields a life-saving benefits of 0.369/22.8=\$0.016 (45-64) and 0.369/12.4=\$0.03 (65+). Of course, the number could be much higher if the mortality reduction were more persistent. These potentially substantial benefits are opposite in sign to the cyclicality of mortality that has been documented in prior work. General economic expansions appear to be associated with *increased* mortality (Ruhm 2000), while DOD-induced expansions appear to *decrease* mortality.[32]

E. Crime

Although one may naturally think that economic prosperity reduces crime, the reality may be more complex; for example, uneven growth could increase social tension and encourage property crime. To explore how DOD spending shocks affect crime, we use various crime rates (violent crime, murder, aggravated assault, rape, property crime, robbery, burglar, larceny, vehicle theft, arson) as outcome variables in specification (1) and report results in Table 7). We find that these shocks generally have no statistically significant effects on crime. The only exception to this pattern is vehicle theft, which declines statistically significantly after a positive DOD spending shock when we consider all CBSAs. Our estimate of 171.2 (per 100,000) implies that the DOD must spend \$9.9 million to prevent a vehicle theft. Even if the value of preventing a car theft is \$10,000, the social return on a dollar of DOD spending is only \$0.001.[33]

F. Summary of Social Benefits

Summing over the potential savings from safety-net programs and the value of additional social benefits yields a total return on a dollar of DOD spending of \$0.273 (Table 8).[34] This tentative estimate

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[32] The online appendix provides a more detailed comparison of our results and Ruhm's, for DOD-induced expansions as well as those due to general demand shocks discussed below.

[33] Although these results suggest that on average DOD spending shocks do not have a systematic effect on crime rates, these aggregate estimates may mask important heterogeneity. Unfortunately, neither the FBI nor NACJD provide information on who commits crime and thus cannot shed more light on hypotheses that emphasize potential distributional effects of DOD spending on crime.

[34] One may also express these benefits of DOD spending in terms of the "marginal value of public funds" (MVPF), as defined and applied in Hendren and Sprung-Keyser (2020). Assuming a dollar of defense spending yields a dollar of social value excluding the additional benefits studied here, the net cost to the government is reduced by the safety net savings in Table 8 (0.112 per dollar spent) and the social value is increased by the 0.161 other social benefits per

is likely a lower-bound on the social savings from DOD spending. For example, we have assumed that mortality effects are temporary. Furthermore, we have not incorporated savings from demographic groups with large but imprecisely estimated effects on social outcomes, nor have we included any potential benefits associated with home value appreciation. There may also be improvements in health care and nutrition that may be associated with the reduced need to rely on related government programs that we are not accounting for. Finally, several outcomes are likely to be associated with large social benefits even though an estimate of the magnitude is not available.

5. Not all Demand Shocks are Alike: Comparison to a General Demand Shock

DOD spending has well-established advantages for understanding the effects of fiscal stimulus on the economy. We have documented social effects that are heterogeneous across demographic groups and in many instances economically substantial. Are these effects unique to DOD-induced aggregate demand expansions? Or are DOD spending shocks representative of typical local aggregate demand expansions?

To address these questions, we replace the DOD spending shock series with a series of general demand shocks – the inner product of industry-CBSA shares and national industry-level growth rates – that are typically exploited to isolate exogenous shifts in local labor demand (e.g., Autor et al. 2013; Beaudry et al. 2018; Goldsmith-Pinkham et al. 2020, henceforth GSS). Specifically, we adapt our baseline specification (1) by replacing government spending growth $\frac{G_{\ell,t}-G_{\ell,t-2}}{Y_{\ell,t-2}}$ with local earnings growth $\frac{Y_{\ell,t}-Y_{\ell,t-2}}{Y_{\ell,t-2}}$, where we instrument for local earnings growth with the inner product (over 20 two-digit industries) of industry-location shares and national-level industry earnings growth: $B_{lt} = \sum_{k=1}^{20} \frac{Y_{k,\ell,0}}{Y_{\ell,0}} \times \frac{Y_{k,t}-Y_{k,t-2}}{Y_{k,t-2}}$ (a traditional Bartik instrument[35]):

$$\frac{X_{d,\ell,t} - X_{d,\ell,t-2}}{Y_{\ell,t-2}} = \beta^d \frac{Y_{\ell,t} - Y_{\ell,t-2}}{Y_{\ell,t-2}} + \psi_\ell + \alpha_t + error_{d\ell t}. \quad (6)$$

Our measure of industry-level earnings is limited to earnings from private-sector employment, which limits any potential correlation between government employment shocks and DOD shocks.[36] The resulting demand shock series is relatively independent of our DOD spending shock

dollar, for an MVPF of 1.301 (= 1.155/(1-0.112)). This value, based on short-term outcomes, cannot be compared to those for the programs evaluated by Hendren and Sprung-Keyser, which also account for long-term consequences.

[35] While we also referred to the instrument for our DOD shocks as a Bartik-type instrument, that refers to the motivation for the IV methodology rather that to the instrument itself, which differs in the two cases.

[36] Since our Bartik instrument is constructed with only private-sector earnings, the sum of earnings shares across industries does not sum to total earnings, as is often the case in applications of Bartik shocks. See GSS for a further

series (correlation -0.07). Since our demand shock exploits variation across all 2-digit industries, we will refer to it as a general demand shock.

In the terminology of GSS, the research design implicit in the use of our Bartik instrument is based on differential exposure to common shocks. The typical concern in this context is that differential exposure to national industry shocks (based on different pre-period local industry shares) leads to different *changes* in local earnings due to channels other than local demand. Industry shares may be correlated with other local characteristics that predict upcoming changes in local earnings. Such concerns are particularly relevant in empirical settings with only two periods (pre- and post-shock). Our setting, however, is based on multiple time periods when the common shock exhibits strong fluctuations, which permits us to use location fixed effects to control for CBSA characteristics. The main threat to our identification assumption would be CBSA-level supply-side factors that are both correlated with local industry shares *and* coincidentally fluctuate with national industry growth rates, after controlling for CBSA fixed effects.[37] GSS recommend highlighting the industries driving the Bartik shock by reporting weights that depend on the covariance between an industry's fitted value of total earnings and actual earnings (the "Rotemberg weight"). We report a similar statistic – the response of industry earnings to Bartik-instrumented total earnings – that is conveniently interpreted as the effect of a general demand shock on industry earnings.[38] Appendix Table 6 reports the NAICS 2-digit industries that experience the largest increase in QCEW earnings in response to a general demand shock. Mining (NAICS 21, which includes oil and gas extraction) and manufacturing (NAICS31-33) are by far the most important industries, consistent with the dominant industries in other applications of traditional Bartik shocks (GSS).

Our general demand shock is less persistent than the shock based on DOD spending: 0.65 vs. 0.96 first-order serial correlation.[39] Below we document that local general demand shocks have effects that differ from those of DOD shocks for some social outcomes. We uncover potential

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discussion. We examine relatively aggregate industry classifications (2-digit) since their shares are more stable over time than disaggregate classifications. Indeed, our pre-period industry shares are nearly identical to industry shares over our sample period (correlation 0.99).

[37] It is possible that the Bartik shock is driven by national industry-specific supply shocks. Since such shocks would increase labor demand in a CBSA, we consider them to be local (CBSA-level) demand shocks.

[38] Reporting industry-level effects also conveniently summarizes average industry-level relevance across years in a panel setting (whereas there is a Rotemberg weight for each industry/year). Note that industry effects are inclusive of input-output linkages and other general equilibrium effects. According to the estimates in AGM20, city-level input-output linkages are quite strong, while general equilibrium effects tend to be small but positive in response to local demand shocks.

[39] We also examined a "China shock" (see Autor et al. 2013 for a discussion), which is more persistent than the general demand shock, but this shock has small year-to-year variations thus making it unsuitable for our analysis.

reasons for these differential effects, although persistence is also a candidate explanation.[40]

A. Effects of a General Demand Shock

Table 9 reports the effects of the general demand shock and, for reference (from Table 2), the effects of the DOD spending shock on labor market outcomes across CBSAs. The aggregate earnings effects are very similar: a one percentage point DOD spending shock raises total earnings by $0.55, while a general demand shock raises earnings by $0.63. However, there are substantial differences in the allocation of these earnings across demographic groups. The earnings benefits of the general demand shock accrue more to households with a bachelor's degree, younger households, and White households, relative to the earnings benefits of DOD spending shocks.

Despite similar aggregate earnings effects across the types of demand shocks, there are large differences in the employment rate response, with the general demand shock leading to an employment rate response of just over half that of the DOD spending shock. This lower employment response implies that the earnings produced by a general demand shock accrue more to those who are already employed. When examining employment-rate responses by educational attainment, it is apparent that the different aggregate employment-rate response is accounted for entirely by those without a bachelor's degree. In short, DOD spending shocks exhibit stronger labor market effects for the less-educated than do general demand shocks, and this difference is especially stark for the less-educated who would otherwise be unemployed.

The distributional effects of general demand shocks are qualitatively similar to those of DOD spending shocks. Appendix Figure 6 reports the effects of general demand shocks by race, sex, and age from the QWI data, for which statistical tests have more power. For example, Black and Hispanic households exhibit stronger average earnings effects than do White households.

Social Effects of General Demand Shocks. Table 10 and Appendix Table 7 report the social effects of the general demand shock. As with the DOD shock, there is a substantial decline in poverty and food stamp receipt. However, these broader demand shocks exhibit milder effects on other social outcomes reported in Table 10 than the DOD shocks, particularly for disability rates. In contrast to DOD spending shocks, general demand shocks tend to <u>increase</u> routine task intensity among non-bachelor's households and lead to increases in average transportation time to work.

[40] The general demand shock is also less persistent than is typically documented for national aggregate demand shocks. One reason for the low persistence of the general demand shock relative to well-documented persistence of national demand shocks is that our inclusion of time fixed effects differences out persistent aggregate shocks. Therefore, one should interpret our general demand shocks as representing "typical" *differential* demand shocks across cities.

Whereas the social effects of DOD spending shocks and general demand shocks on adults are in some instances distinct, the effects on children's outcomes are generally aligned. Columns (4)-(6) in Table 5 report the effects of general demand shocks on the young. As with the DOD spending shock, children experience less poverty and are less likely to be on Medicaid.

Mortality. Turning to mortality (columns (3) and (4) in Tables 6a and 6b), we see the starkest differences between the effects of DOD spending shocks and general demand shocks. In response to a general demand shock, mortality rates increase substantially (by approximately 100 deaths per 100,000 people), with most deaths being due to internal health factors or accidents. The effects of general demand shocks are consistent with Ruhm's (2000) evidence that mortality is procyclical. Furthermore, mortality increases are driven by those over age 45, the same demographic groups that experienced a *decline* in mortality in response to DOD spending shocks. In Appendix B, we compare the magnitudes of our estimates with those from Ruhm (2000) and find that our results for the general demand shock are generally of the same sign and order of magnitude as Ruhm's, whereas those of the DOD shock are of the opposite sign.

Ruhm (2000) attributes procyclical mortality in part to a deterioration in diet and exercise as the economy expands. Another plausible factor is pollution, which we expect to increase with economic activity (see e.g., Dasgupta et al. 2002 for a discussion). We do not have CBSA-level data on health outcomes,[41] but the Environmental Protection Agency publishes highly disaggregated measures of the Air Quality Index (AQI), for which higher values indicate lower air quality.[42] Table 11 shows that there is indeed a differential effect of the demand shocks on AQI. Both the median value (over days in a year) and the 90th percentile of a city's AQI increase substantially in response to a general demand shock but are relatively unaffected by a DOD shock, consistent with the differential response of health-related mortality. Why might pollution respond more strongly to a general demand shock? One possibility is that even though both shocks increase local earnings by similar amounts, different responses of commuting and congestion lead to different responses of pollution. Indeed, average transportation time to work falls in response to a DOD shock but increases in response to a general demand shock. The differential responses of transportation time and

[41] the Center for Disease Control (CDC) provides only estimates on the rate of diagnosed diabetes at the county-year level (https://www.cdc.gov/diabetes/data/index.html). These county-level estimates are created using data from the US Census Bureau's Population Estimates Program. Rates are given by instances of diagnosed diabetes per 100 people and include a point estimate, lower limit, and upper limit estimate for each county. We do not find any evidence that general demand or DOD shocks lead to higher (much less differential) prevalence of diabetes.

[42] https://www.epa.gov/air-trends/air-quality-cities-and-counties. Increases in the AQI represent a worsening of air quality.

pollution are likely driven by differences in the types of cities that are affected by the different shocks. Below we present evidence that DOD shocks disproportionately affect smaller cities, for which congestion is less of a concern. Demand shocks, by contrast, disproportionately affect larger cities that are more likely to exhibit congestion constraints.

Crime. Whereas DOD shocks lead to reductions in vehicle thefts (and insignificant effects on other types of crimes), general demand shocks appear to increase vehicle theft and aggravated assault (columns (3) and (4) in Table 7). Obviously, some estimates may be statistically significant by chance, but one can contemplate mechanisms, based on the differential distributional effects of the spending shocks, that rationalize the differential response of vehicle theft and assault to different types of spending shocks.

One possibility is that crime reflects a more general deterioration in positive social engagement. To explore this possibility, we examine voter turnout as a proxy for civic engagement. According to Table 12, voter turnout falls substantially in response to a general demand shock, whereas the effects of a DOD shock on voter turnout are not distinguishable from zero. The relative decline in turnout does not appear to reflect differential effects on political party affiliations: both types of demand shocks lead to decreasing vote shares for Democratic candidates. The similarity of voting outcome responses to DOD and general demand shocks suggests that specific political economy considerations (e.g., defense contracts stimulate voters to support the Republican party which is perceived as being more hawkish on national security), rather the influences of general improvements in economic conditions on party allegiance, are unlikely to explain the differences between DOD and general demand shocks.

Appendix Table 8 reports the social value of a dollar of spending from general demand shocks, based on the same approach used in Table 8 for valuing DOD shocks. The gains are approximately one-third smaller,[43] which is driven by the fact that general demand shocks increase mortality and transportation time to work. The social values do not account for differential effects on occupational prestige, the RTI, aggravated assault, and violent crime. These outcomes are difficult to value, but doing so would amplify the relative benefits of DOD shocks.

B. Differential Social Effects of DOD and General Demand Shocks: The Employment Margin

[43] That is, the MVPF corresponding to the results in Appendix Table 8, 1.201 (= 1.179/(1-0.018)) indicates a value in excess of 1.0 of 0.20, rather than the excess of 0.30 for the DOD shocks.

Local demand shocks that have similar effects on local earnings have social effects that in some cases differ drastically. DOD spending shocks improve many social outcomes, whereas general demand shocks increase mortality while generating mild or non-existent social improvements.

To explore the underlying reasons for these differential social effects, we focus on those with low levels of formal education, as this demographic category accounts for a large share of the population, exhibits worse social outcomes than those with a bachelor's degree, *and* exhibits the strongest differential social response to the two types of demand shocks. Why might DOD spending shocks improve social outcomes more than general demand shocks for those without a bachelor's degree? Each type of demand shock has similar average earnings effects for those without a bachelor's degree (0.71 for a DOD spending shock compared to 0.69 for a general demand shock), suggesting that the differential social effects do not operate through earnings alone. However, this group experiences a large differential employment response: DOD spending shocks increase employment rates among those without a bachelor's degree by 24.5 percentage points, whereas general demand shocks only lead to only a 14.3 percentage point increase.

Those without a bachelor's degree are more likely to be unemployed than those with a bachelor's degree and more likely to experience adverse social outcomes. Among the group without a bachelor's degree, the unemployed are even more likely to experience adverse social outcomes.[44] Therefore, we conjecture that much of the differential social effects are due to the differential ability to pull households into employment.

In Appendix C, we explore the role of the employment margin by decomposing changes in social outcomes to isolate the component associated with changes in the employment rate. We document that the employment margin explains a large share of the declines in poverty, food stamp receipt, and disability. It also accounts for increases in marriage rates and occupational prestige. We then examine how employment effects for households without a bachelor's degree depend on the city, occupation, and industry composition of DOD and general demand shocks. DOD shocks have much stronger employment effects, and we find that the city and occupational composition of DOD shocks account for most of this difference. Compared to general demand shocks, DOD spending shocks are directed toward cities that are relatively smaller and have relatively lower employment rates, lower earnings, and fewer residents with a bachelor's degree. DOD shocks are also

[44] For example, 27 percent of those not employed and without a bachelor's report being disabled, compared to 13 percent of those employed without a bachelor's and 5 percent of those with a bachelor's degree.

disproportionately directed toward Production and Maintenance occupations. Given this differential composition of shocks, it is not surprising that DOD shocks disproportionately benefit those without a bachelor's and those who would otherwise be unemployed.

These findings suggest that the social benefits of demand shocks depend on the occupational and locational exposure to the shocks. Therefore, fiscal policies that target Production and Maintenance and smaller cities are likely to generate the largest social benefit. Targeting cities may be more feasible than targeting occupations. But given the concentration of production occupations in small cities, targeting in only one dimension may be sufficient. To evaluate this conjecture, we compute predicted employment based on the city *and* occupation variation on no-bachelor's shares. This responds only slightly more to DOD shocks than predicted employment based on either city or occupation alone (columns 7 and 8 of Appendix Table 10), which suggests that targeting cities can be sufficient.

6. Conclusion

The fiscal policy literature has generally focused on the magnitude and timing of effects on key macroeconomic aggregates, such as GDP, employment, earnings, and interest rates. But beneath the surface of these aggregates lie important distributional consequences (e.g., which groups benefit relatively more or less from policy shocks). These distributional consequences are of considerable importance as the U.S. confronts an environment in which there is significant economic inequality and a host of associated social problems. Moreover, the consequences of fiscal policy extend far beyond the economic outcomes commonly examined. Improvements in employment and earnings can bring with them other positive outcomes, for the individuals themselves and, through effects on the take-up of government benefits, the government's fiscal health. Indeed, the stronger economy that fiscal stimulus generates may complement a vast array of social policies.

In the results presented above, we find that arguably exogenous fiscal policy shocks, coming through the award of contracts by the Department of Defense, provide a strong stimulus to earnings and employment, consistent with previous results in the literature. However, we also find that the increase in earnings is proportionally higher for non-White individuals and for those without a bachelor's degree, and that those without a bachelor's degree also experience a proportionally larger increase in employment. Consistent with this increase in earnings, the less educated also experience a significant decline in rates of poverty and disability, as well as an improvement in working conditions, as measured by occupational prestige and travel time to work. Other population subgroups experience particular beneficial outcomes as well. And, for the older population as a whole, there is a significant decline in mortality rates.

These positive outcomes are not a necessary consequence of a general improvement in the economic environment. Comparing them to the outcomes of a standard (Bartik) general demand shock, we find that the general demand shock has smaller effects on employment among the less educated, less of an impact on disability and, echoing results from earlier studies, *adverse* effects on mortality. A decomposition of the differences in these results indicates that they are substantially explained by differences in the locations and occupations that benefit directly from the two types of shocks. Thus, although not by design, defense-related government spending is a particularly strong force not just for economic stimulus, but also for improving economic equity and a broader set of measures of well-being.

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Table 1. Summary Statistic

	Mean	Median	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
CBSA characteristics					
Population	706,285	262,938	1,380,204	69,612	12,400,000
Average Earnings	28,828	28,038	5,598	17,242	56,836
Share of DOD spending	0.003	0.000	0.010	0.000	0.128
DOD share of Total Earnings	0.037	0.012	0.068	0.000	0.534
Social Indicators					
Employment Rate	0.93	0.93	0.02	0.85	0.99
Labor Force Participation Rate	0.73	0.73	0.04	0.54	0.83
Poor	0.12	0.11	0.04	0.06	0.27
Food stamp receipt	0.13	0.13	0.04	0.05	0.32
Disabled	0.13	0.13	0.03	0.06	0.21
Lives in multi-family home	0.08	0.08	0.03	0.03	0.21
Married	0.55	0.55	0.04	0.44	0.68
Divorced	0.13	0.13	0.02	0.07	0.18
Single Parent	0.11	0.11	0.02	0.06	0.17
Occupational Prestige Index	40.55	40.48	1.80	34.99	46.75
Homeowner	0.68	0.69	0.06	0.49	0.83
Transportation time to work	23.16	22.78	3.44	15.93	41.72
Health Insurance	0.83	0.83	0.06	0.52	0.95
Crime Rate (per 100,000)					
Murder Rate	4.71	4.26	2.81	0.45	21.17
Rape Rate	35.18	32.84	14.37	5.04	111.92
Robbery Rate	97.21	92.08	55.69	10.09	314.45
Aggravated Assault Rate	269.10	244.78	141.50	33.36	1,028.87
Burglary Rate	718.67	690.37	298.85	223.63	2,461.45
Larceny Rate	2,143.04	2,100.27	570.07	1,059.38	3,714.54
Vehicle Theft Rate	220.09	182.97	131.11	29.98	687.59
Arson Rate	18.84	17.12	11.79	2.82	121.83
Air Quality Index					
Median	44.73	43.70	10.11	17.00	173.70
90^{th} percentile	73.19	71.10	19.53	29.50	173.70
Voting outcomes					
Voter turnout	0.60	0.61	0.08	0.36	0.80
Democratic Index	0.45	0.45	0.12	0.16	0.77
Changes (as share of lagged earnings)					
DOD spending	0.002	0.000	0.021	-0.200	0.230
Predicted DOD spending	0.001	0.000	0.005	-0.035	0.055
Earnings	0.063	0.056	0.045	-0.115	0.354
Growth in Average Earnings	0.034	0.032	0.024	-0.067	0.177
Change in Employment rate	-0.001	-0.000	0.007	-0.030	0.028
Change in Labor Force Participation Rate	-0.005	-0.004	0.007	-0.046	0.025

Note: This table displays summary statistics for the 286 CBSAs with data from the ACS, USAspending.gov, CDC, EPA, FBI and Chenoweth et al. (2020).

Table 2. Earnings Response by Demographic Group

	Total Earnings	Average Earnings	Employment Rate
	(1)	(2)	(3)
Panel A: ACS or LAUS			
All (ACS)	0.551**	0.422**	0.214***
	(0.249)	(0.198)	(0.061)
All (QCEW or LAUS)	0.844***	0.379***	0.170***
	(0.229)	(0.093)	(0.063)
Education (ACS)			
No Bachelors	0.545***	0.707***	0.242***
	(0.162)	(0.220)	(0.073)
Bachelors	0.033	0.271	0.068
	(0.132)	(0.251)	(0.051)
p-value (equality)	[0.006]	[0.163]	[0.044]
N	2,542	2,542	2,542
First-Stage F statistic	28.577	28.577	28.577
Panel B: Demographic Group (QWI)			
Race			
White	0.369***	0.558***	0.136***
	(0.094)	(0.124)	(0.032)
Black	0.073***	0.915***	0.295**
	(0.022)	(0.263)	(0.142)
Hispanic	0.033**	1.015***	0.311***
	(0.013)	(0.233)	(0.073)
p-value (equality)	[0.000]	[0.014]	[0.008]
Age			
22-44	0.256***	0.648***	0.230***
	(0.067)	(0.148)	(0.050)
45-64	0.186***	0.495***	0.124***
	(0.053)	(0.113)	(0.036)
65-99	0.026***	0.927***	0.043***
	(0.005)	(0.167)	(0.010)
p-value (equality)	[0.004]	[0.000]	[0.000]
Sex			
Male	0.365***	0.777***	0.209***
	(0.088)	(0.146)	(0.041)
Female	0.147***	0.381***	0.094***
	(0.042)	(0.122)	(0.029)
p-value (equality)	[0.000]	[0.000]	[0.000]
N	11,927	11,912	11,912
First-Stage F statistic	143.438	143.398	143.398

Note: This table reports the effect of increases in DOD spending (instrumented by the DOD Bartik shock) on labor market outcomes over a two-year time span. All variables are winsorized at the 1% and 99% level. Fixed effects for CBSA and year are included but not reported. P-values of equality of estimates across subgroups are reported in square brackets. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Social Outcomes by Demographic Group.

Social Outcomes (rates):	Poverty	Food Stamp Receipt	Welfare Income	Medicaid Receipt	Health Insurance	Disabled	Occupational Prestige	Routine Task Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Demographic Group								
All	-0.075*	-0.080	-0.004*	-0.069	0.335	-0.081*	2.193*	-0.145
	(0.044)	(0.056)	(0.002)	(0.180)	(0.211)	(0.047)	(1.275)	(0.097)
Education								
No Bachelors	-0.115*	-0.104	-0.005*	-0.105	0.306	-0.115**	3.627***	-0.244*
	(0.059)	(0.071)	(0.002)	(0.216)	(0.226)	(0.052)	(1.194)	(0.132)
Bachelors	0.010	-0.024	0.000	-0.019	-0.015	-0.004	0.251	0.178
	(0.037)	(0.026)	(0.000)	(0.146)	(0.170)	(0.036)	(3.015)	(0.308)
p-value (equality)	[0.058]	[0.216]	[0.041]	[0.735]	[0.175]	[0.012]	[0.335]	[0.278]
Race								
White	-0.093**	-0.116*	-0.002	-0.204	0.414	-0.075*	2.555*	-0.056
	(0.037)	(0.060)	(0.002)	(0.149)	(0.252)	(0.041)	(1.346)	(0.116)
Black	0.092	0.136	-0.002***	0.498	2.087	0.038	12.997*	-0.378
	(0.237)	(0.234)	(0.001)	(0.852)	(1.333)	(0.189)	(7.304)	(1.030)
Hispanic	-0.113	-0.187	0.001	0.771	0.158	-0.034	-1.150	0.065
	(0.193)	(0.254)	(0.001)	(0.977)	(0.824)	(0.168)	(8.532)	(0.605)
p-value (equality)	[0.730]	[0.451]	[0.019]	[0.455]	[0.074]	[0.808]	[0.300]	[0.934]
Age								
20-40	-0.067	-0.096	-0.003*	-0.188	0.249	-0.013	2.677	0.033
	(0.071)	(0.087)	(0.002)	(0.272)	(0.282)	(0.043)	(2.336)	(0.273)
41-61	-0.068	-0.060	-0.002	0.073	0.652*	-0.135*	0.881	-0.111
	(0.055)	(0.051)	(0.001)	(0.210)	(0.368)	(0.068)	(1.704)	(0.138)
62-70	-0.100	-0.146*	0.000	-0.221	-0.126	-0.093	1.505	-0.718**
	(0.070)	(0.079)	(0.001)	(0.277)	(0.149)	(0.097)	(3.571)	(0.346)
p-value (equality)	[0.921]	[0.488]	[0.042]	[0.470]	[0.002]	[0.099]	[0.794]	[0.287]
Sex								
Male	-0.101*	-0.085	-0.002*	0.089	0.254	-0.069	0.651	-0.238
	(0.048)	(0.057)	(0.001)	(0.246)	(0.212)	(0.056)	(2.206)	(0.169)
Female	-0.057	-0.085	-0.002	-0.187	0.445	-0.092**	3.617*	-0.067
	(0.052)	(0.065)	(0.002)	(0.203)	(0.277)	(0.044)	(2.018)	(0.149)
p-value (equality)	[0.364]	[0.996]	[0.780]	[0.119]	[0.407]	[0.539]	[0.393]	[0.511]
N	2542	2542	2542	1756	1756	2542	2542	2542
First-Stage F statistic	28.577	28.577	28.577	4.716	4.716	28.577	28.577	28.577

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) on social outcomes by demographic category over a two-year time span. DOD spending is instrumented with a Bartik shock. All variables are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. P-values of equality of estimates across subgroups are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Social Outcomes by Demographic Group

Social Outcomes (rates):	Transportation time to work	Multi-family home	Homeowner	Home Value (growth)	Married	Divorced	Single parent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Demographic Group							
All	-6.862**	-0.039	0.094	0.655*	0.037	-0.036	-0.010
	(2.604)	(0.030)	(0.059)	(0.359)	(0.049)	(0.032)	(0.023)
Education							
No Bachelors	-6.604**	-0.068	0.078	0.655*	0.083	-0.044	-0.015
	(3.075)	(0.045)	(0.075)	(0.359)	(0.070)	(0.032)	(0.026)
				0.655*			
Bachelors	-9.895**	0.002	0.181**	(0.359)	-0.066	-0.032	-0.031
	(4.671)	(0.052)	(0.089)	0.655*	(0.130)	(0.066)	(0.056)
p-value (equality)	[0.533]	[0.338]	[0.404]	[0.460]	[0.369]	[0.862]	[0.787]
Race							
White	-7.409**	-0.103**	0.093	0.531	0.146***	-0.042	-0.037
	(2.990)	(0.041)	(0.059)	(0.333)	(0.047)	(0.049)	(0.023)
Black	11.061	0.233	-0.031	0.515	-0.298	-0.209	0.088
	(10.998)	(0.256)	(0.345)	(1.204)	(0.294)	(0.156)	(0.147)
Hispanic	-15.573**	0.247	-0.148	0.448	-0.407*	0.055	0.080
	(7.141)	(0.175)	(0.317)	(0.720)	(0.237)	(0.175)	(0.149)
p-value (equality)	[0.183]	[0.139]	[0.689]	[0.990]	[0.032]	[0.508]	[0.618]
Age							
20-40	-2.071	-0.060	0.058	0.861**	-0.035	0.020	-0.007
	(3.436)	(0.053)	(0.118)	(0.376)	(0.107)	(0.057)	(0.044)
41-61	-10.332***	-0.016	0.083	0.606	0.068	-0.124**	-0.013
	(3.372)	(0.029)	(0.063)	(0.396)	(0.068)	(0.053)	(0.044)
62-70	-13.384	-0.039	0.141	0.675	0.066	0.043	0.020
	(8.347)	(0.043)	(0.097)	(0.441)	(0.097)	(0.049)	(0.051)
p-value (equality)	[0.188]	[0.696]	[0.822]	[0.521]	[0.675]	[0.021]	[0.865]
Sex							
Male	-8.283**	-0.028	0.110	0.720*	0.005	-0.030	-0.022
	(3.588)	(0.038)	(0.083)	(0.379)	(0.063)	(0.036)	(0.024)
Female	-5.988*	-0.051	0.067	0.587*	0.070	-0.037	-0.004
	(3.351)	(0.038)	(0.057)	(0.347)	(0.058)	(0.035)	(0.035)
p-value (equality)	[0.511]	[0.598]	[0.559]	[0.173]	[0.377]	[0.835]	[0.630]
N	2,542	2,542	2,542	2,542	2,542	2,542	2,542
First-Stage F statistic	28.577	28.577	28.577	28.577	28.577	28.577	28.577

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) on social outcomes by demographic category over a two-year time span. DOD spending is instrumented with a Bartik shock. All variables are winsorized at the 1% and 99% levels. Data on health insurance and Medicaid status are only available as of 2008. Fixed effects for CBSA and year are included but not reported. P-values of equality of estimates across subgroups are reported in square brackets. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Child Poverty and Health Insurance

Social Outcomes (rates):	DOD demand shock			General demand shock		
	Poor	Health Insurance	Medicaid	Poor	Health Insurance	Medicaid
	(1)	(2)	(3)	(4)	(5)	(6)
Demographic Group						
Age 6 to 10	-0.188*	-0.200	-0.694	-0.177***	-0.162*	-0.258**
	(0.111)	(0.442)	(0.465)	(0.057)	(0.092)	(0.106)
Age 10 to 15	-0.107	0.280	-0.432	-0.278***	-0.076	-0.084
	(0.167)	(0.535)	(0.494)	(0.083)	(0.154)	(0.083)
Age 16 to 20	-0.043	0.468	-0.072	-0.218**	0.057	-0.184***
	(0.179)	(0.588)	(0.397)	(0.062)	(0.082)	(0.068)
N	2542	1756	1756	2542	1756	1756
First-Stage F statistic	28.577	4.716	4.716	160.103	194.316	194.316

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) and general demand on social outcomes by demographic category over a two-year time span. DOD spending is instrumented with a Bartik shock. All variables are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6a. Mortality

	DOD demand shock		General demand shock	
	All CBSAs	ACS cities	All CBSAs	ACS cities
	(1)	(2)	(3)	(4)
All	-47.1	-27.3	109.5**	156.8***
	(58.0)	(68.6)	(41.5)	(37.6)
Mortality by Cause of Death				
Suicide	9.5**	-3.6	-3.2	-0.9
	(4.3)	(5.9)	(4.8)	(4.6)
Drug/Alcohol	-6.3	-24.3	12.7**	13.4
	(8.4)	(24.3)	(5.6)	(13.3)
Assault	0.4	-0.7	0.2	2.2
	(2.1)	(5.6)	(1.1)	(4.0)
Internal	-50.5	-21.6	83.2**	114.8**
	(56.4)	(67.2)	(38.1)	(38.0)
Accident	3.7	-2.3	25.9**	37.0**
	(10.4)	(17.8)	(12.7)	(14.6)
N	13304	3114	14,055	3,114
First-Stage F statistic	155.2	39.5	187.2	175.6

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings; columns (1) and (2)) and general demand (columns (3) and (4)) on death rates by age category over a two-year time span. Death rates are per 100,000 people. Data is suppressed for county-year observations with fewer than 9 deaths. We report results in which the number of deaths in these counties is set to zero; with the exception of Mortality Growth for Drug&Alcohol and for 25-44 the results are very similar to instead setting the number of deaths in suppressed counties to 9. CBSA-level data is derived by aggregating the county-level data. All estimates are based on the instrumental variable approach. All variables are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6b. Mortality

	DOD demand shock		General demand shock	
	All CBSAs	ACS cities	All CBSAs	ACS cities
	(1)	(2)	(3)	(4)
Mortality by age				
0-14	33.0	21.7	20.8	16.9
	(24.8)	(24.4)	(13.0)	(25.4)
15-24	22.5	6.9	1.0	88.7***
	(40.7)	(55.4)	(23.9)	(29.5)
25-44	27.2	-45.8	38.3	94.6*
	(30.0)	(39.0)	(40.6)	(52.1)
45-65	-240.9***	-121.4*	91.6*	137.7***
	(75.1)	(61.9)	(47.8)	(49.9)
65-99	-759.8**	-253.4	531.8**	657.4**
	(293.4)	(309.4)	(214.4)	(265.5)

See Note to Table 6a above.

Table 7. Crime Rates

	DOD demand shock		General demand shock	
	All CBSAs	ACS cities	All CBSAs	ACS cities
	(1)	(2)	(3)	(4)
Violent Crime Rate	109.1	134.0	135.0*	148.9
	(93.3)	(175.2)	(79.8)	(105.3)
Murder Rate	0.7	0.5	3.1	4.4
	(3.0)	(3.7)	(2.0)	(2.9)
Aggravated Assault Rate	104.4	190.5	130.5*	89.2
	(88.2)	(165.0)	(69.1)	(73.5)
Rape Rate	-3.8	-38.6	-6.4	-12.4
	(19.6)	(32.1)	(12.6)	(13.3)
Property Crime Rate	-85.7	-58.1	86.5	86.4
	(682.6)	(1305.2)	(384.7)	(496.2)
Robbery Rate	17.4	6.1	10.2	60.5
	(18.4)	(50.7)	(13.0)	(45.5)
Burglary Rate	35.2	-160.8	-102.8	-118.1
	(243.7)	(369.4)	(109.0)	(242.5)
Larceny Rate	-379.5	716.2	-329.6	-463.3
	(417.4)	(1201.6)	(266.6)	(336.4)
Vehicle Theft Rate	-172.1*	44.1	121.6**	176.2**
	(86.3)	(182.2)	(49.7)	(84.2)
Arson Rate	-5.2	47.6	7.3	4.7
	(11.9)	(30.5)	(8.3)	(9.3)
N	12892	3045	13,610	3,045
First-Stage F statistic	143.0	38.5	328.3	265.7

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings; columns (1) and (2)) and general demand (columns (3) and (4)) on crime rates over a two-year time span. DOD spending is instrumented with a Bartik shock. All variables are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Savings from DOD Spending.

	Estimate	Population share	DOD must spend	Value per person	Benefits per DOD dollar
	(1)	(2)	(3)	(4)	(5)
Safety Net Savings					
Food Stamp Receipt White Adult	-0.116	0.71	$206,411	$1,500	$0.007
Welfare Payments (direct savings)	-0.005				$0.005
Medicaid Receipt White Adult	-0.2	0.71	$119,718	$8,436	$0.070
Medicaid Receipt 6-10	-0.7	0.104	$233,516	$3,556	$0.015
Health Insurance age 41-61 (net)	0.65	0.44	$59,258	$817	$0.014
Subtotal					**$0.112**
Other Social Benefits					
Transportation Time (hours per year)	-58.3	1	$292	$10	$0.034
Divorce age 41-61	-0.12	0.44	$321,970	$7,310	$0.023
Child Poverty age 6-10	-0.188	0.104	$869,476	$50,000	$0.058
Mortality age 45-64 (per 100k)	-241	0.31	$22,754,651	$369,000	$0.016
Mortality age 65-99 (per 100k)	-760	0.18	$12,426,901	$369,000	$0.030
Vehicle Theft (per 100k)	-171.2	1	$9,929,907	$10,000	$0.001
Subtotal					**$0.161**
Total					**$0.273**

This table derives social benefits per dollar of DOD spending. Unless otherwise specified, outcomes are changes in rates in response to DOD spending equal to local earnings. Benefits per DOD dollar (column 5) is the value per person of the value of the outcome (column 4) divided by the amount the DOD must spend to produce that outcome (column 3). The amount in column 3 is average QCEW earnings (17k) divided by the (negative of the) estimate from column 1 and the population share from column (2).

Table 9. Labor Force Responses by Demographic Group, General Demand Shock.

Labor Market Outcomes:	Total ACS Earnings		Average ACS Earnings		Employment Rate	
Shock:	DOD	General Demand	DOD	General Demand	DOD	General Demand
	(1)	(2)	(3)	(4)	(5)	(6)
Demographic Group						
All	0.551**	0.626***	0.422**	0.563***	0.214***	0.125***
	(0.249)	(0.077)	(0.198)	(0.075)	(0.061)	(0.034)
Education						
No Bachelors	0.545***	0.460***	0.707***	0.691***	0.242***	0.143***
	(0.162)	(0.060)	(0.220)	(0.074)	(0.073)	(0.038)
Bachelors	0.033	0.169***	0.271	0.284**	0.068	0.063**
	(0.132)	(0.061)	(0.251)	(0.125)	(0.051)	(0.026)
N	2,542	2,542	2,542	2,542	2,542	2,542
First-Stage F statistic	28.6	160.1	28.6	160.1	28.6	160.1

Note: This table reports the effect of increases in DOD spending (instrumented by the DOD Bartik shock) and earnings (instrumented by the traditional Bartik shock) on labor market outcomes over a two-year time span. The sample is limited to CBSA-years with at least 100 respondents for the given category. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Social Outcomes by Demographic Group, General Demand Shock.

Social Outcomes (rates):	Poverty	Food Stamp Receipt	Welfare Income	Medicaid Receipt	Health Insurance	Disabled	Occupational Prestige	Routine Task Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Demographic Group								
All	-0.108***	-0.122***	-0.002**	0.019	0.129**	-0.006	0.577	0.092*
	(0.024)	(0.028)	(0.001)	(0.036)	(0.053)	(0.014)	(0.979)	(0.051)
Education								
No Bachelors	-0.150***	-0.155***	-0.002**	0.013	0.153**	-0.006	1.660**	0.133**
	(0.030)	(0.035)	(0.001)	(0.044)	(0.057)	(0.017)	(0.771)	(0.058)
Bachelors	0.033	-0.019	0.000	0.080***	0.007	0.010	-2.125*	-0.055
	(0.038)	(0.029)	(0.000)	(0.020)	(0.042)	(0.022)	(1.146)	(0.123)
Race								
White	-0.053*	-0.098**	0.000	0.055	0.161***	-0.024	-0.057	0.073
	(0.030)	(0.037)	(0.001)	(0.033)	(0.035)	(0.022)	(1.096)	(0.099)
Black	-0.221*	-0.172	-0.001***	-0.185	0.079	0.074	-7.340	1.107***
	(0.125)	(0.129)	(0.000)	(0.119)	(0.115)	(0.092)	(5.246)	(0.343)
Hispanic	-0.170	-0.403***	0.000	-0.220	-0.030	-0.037	1.633	0.354
	(0.106)	(0.130)	(0.001)	(0.172)	(0.134)	(0.074)	(4.097)	(0.496)
Age								
20-40	-0.162***	-0.190***	-0.001*	0.010	0.217***	-0.003	1.685	0.056
	(0.048)	(0.042)	(0.001)	(0.050)	(0.059)	(0.021)	(1.733)	(0.157)
41-61	-0.087***	-0.092***	-0.001	0.040	0.060	-0.023	-0.622	0.124
	(0.023)	(0.033)	(0.001)	(0.038)	(0.061)	(0.025)	(1.094)	(0.125)
62-70	-0.011	-0.067**	0.001**	-0.044	0.076*	-0.004	0.811	0.173
	(0.044)	(0.031)	(0.000)	(0.035)	(0.038)	(0.035)	(2.013)	(0.321)
Sex								
Male								
	-0.095***	-0.124***	-0.001	0.027	0.132**	-0.025	0.611	0.177**
	(0.021)	(0.036)	(0.001)	(0.037)	(0.050)	(0.015)	(1.715)	(0.080)
Female								
	-0.116***	-0.123***	-0.001*	0.010	0.119**	0.016	0.542	0.038
	(0.031)	(0.033)	(0.001)	(0.038)	(0.057)	(0.022)	(0.986)	(0.088)
N	2,542	2,542	2,542	1,756	1,756	2,542	2,542	2,542
First-Stage F statistic	160.1	160.1	160.1	194.3	194.3	160.1	160.1	160.1

Notes: This table reports the effect of a percent increase in earnings (instrumented by the general demand shock) on social outcomes by demographic category over a two-year time span. All variables are winsorized at the 1% and 99% levels. Data on health insurance and Medicaid status are only available as of 2008. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 11. Air Quality

	DOD demand shock		General demand shock	
	All CBSAs	ACS cities	All CBSAs	ACS cities
	(1)	(2)	(3)	(4)
90th percentile	-0.0	19.8	28.9**	30.7*
	(13.9)	(20.5)	(12.1)	(17.9)
Median	-2.1	2.8	13.9***	12.6
	(8.6)	(11.1)	(4.7)	(9.7)
N	5,570	2,740	5,617	2,740
First-Stage F statistic	106.6	30.5	71.9	176.5

Note: This table reports the effect of a percent increase in CBSA earnings (instrumented by the general demand shock) on the Air Quality Index (AQI) over a two-year time span. 90th percentile and Median are the relevant percentiles of the daily AQI in a city over the span of a year. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 12. Civic engagement

	DOD demand shock		General demand shock	
	All CBSAs	ACS cities	All CBSAs	ACS cities
	(1)	(2)	(3)	(4)
Turnout	-0.014	-0.260	-0.154**	-0.105
	(0.097)	(0.352)	(0.061)	(0.100)
N	4,112	1,240	4,299	1240
First-Stage F statistic	42.1	15.6	226.6	227.0
Democratic party index	-0.139**	-0.085	-0.085**	-0.040
	(0.053)	(0.123)	(0.033)	(0.029)
N	4,112	1,240	3,667	1,052
First-Stage F statistic	42.1	15.6	139.0	390.4

Note: This table reports the effect of a percent increase in CBSA earnings (instrumented by the general demand shock) on the voter turnout and the Democratic Partisan indexover a two-year time span. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Demand Stimulus as Social Policy

By Auerbach, Gorodnichenko, and Murphy

ONLINE APPENDIX

Appendix A. Discussion of IV from AGM20

For the reader's convenience, we present here modified language from AGM20 related to the advantage of the Bartik-type DOD instrument.

Our objective is to assess the effect of new DOD spending on production of goods and services that would not have occurred in the absence of new spending. For example, during the Iraq war build-up, the DOD increased total orders for fighter jets relative to what had been anticipated before the war. Many DOD contracts, however, represent payment for production that would have occurred anyway, either because the contract was anticipated or because firms smooth production over lumpy contracts. For example, assume that Lockheed accurately forecasts average orders for fighter jets over the next three years. The timing of a contract simply indicates when DOD receives cash but does not correspond to actual new production demand.

More formally, consider $\Delta G_{\ell,t} \equiv G_{\ell,t} - G_{\ell,t-1} = \Delta G^{W}_{\ell,t} + \Delta G^{P}_{\ell,t}$, where $\Delta G^{P}_{\ell,t}$ is an outlay that induces new production and $\Delta G^{W}_{\ell,t}$ are contracts that that are not associated with new production but rather only contain information on the timing of outlays (e.g., because they were anticipated). By mixing $\Delta G^{W}_{\ell,t}$ and $\Delta G^{P}_{\ell,t}$, we are likely to have a downward bias in the size of the multiplier to government spending shocks. Our strategy is to find a variable that is correlated with $\Delta G^{P}_{\ell,t}$ and uncorrelated with $\Delta G^{W}_{\ell,t}$ and use this variable as an instrument for $\Delta G_{\ell,t}$.

The Bartik instrument effectively provides such a filter: aggregate DOD spending represents new production of goods and services and thus $\frac{s_{\ell} \times (G_t - G_{t-1})}{Y_{\ell,t-1}}$ picks up only spending-related changes in $\frac{G_{\ell,t} - G_{\ell,t-1}}{Y_{\ell,t-1}}$ and filters out the cash transfers (including anticipated contracts). In other words, the Bartik instrument helps us to isolate the component of contracts that corresponds to new production by relating location-specific contracts to changes in aggregate production/spending.

Our DOD Bartik instrument exploits changes in national production ($G_t - G_{t-1}$) rather than changes in total contract obligations, ΔO_t. This is because the timing of contract obligations does not correspond to the timing of outlays or new production, and many contracts specify production and outlays over horizons of over five years. One might expect that aggregating across contract obligations would yield an aggregate measure that smoothly tracks DOD production. However, Appendix Figure 2 demonstrates that aggregate obligations are lumpy, whereas our measure of DOD spending smoothly tracks the shape of DOD production. For comparison, Appendix Figure 2 also reports NIPA national defense production, modified to follow the recommended approach from Cox let al. (2024). Despite level differences between our aggregate spending measure and NIPA production, the trends are highly related (correlation 0.99).

Recent evidence has raised concerns that NIPA-based DOD measures do not accurately reflect the timing of production. For example, Brunet (2022) and Briganti and Sellemi (2023) show that government spending is often recorded at delivery, which occurs after production. We are less concerned about a potential timing mismatch in our study for several reasons. First, Brunet's Budget Authority measure (which corrects for timing mismatch) closely tracks NIPA measures of production post-2000. Relatedly, the timing mismatch emphasized in Briganti and Sellemi (2023) is highly relevant at the quarterly frequency but is less likely to be relevant at the two-year horizon in this study. Our results are similar over even longer (five-year) horizons, which further mitigates concerns about high-frequency timing mismatch. Finally, as emphasized by Briganti and Sellemi, any remaining timing mismatch will tend to bias the estimated effects of DOD shocks toward zero, which would imply that our results provide a lower bound for the benefits of DOD spending.

Appendix B. Comparing Effects on Mortality with Those in Ruhm (2000)

Ruhm (2000) considers the impact of a change in the state unemployment rate, controlling for income in some specifications. As the main impact works through the unemployment rate, consider his results for his specification excluding income as an explanatory variable. For a 1 percentage point increase in the state unemployment rate, the change in the number of deaths per 100,000 are:

All	-4.57	presented in Table II; also computable from the effect on log deaths in Table II, -.0052, multiplied by the number of deaths per 100,000 in Table I, 879.8
20-44	-3.36	Effect on log deaths in Table III, -.0203, multiplied by the number of deaths per 100,000 in Table I, 165.4
45-64	+0.28	+.0003 x 934.2 (same approach as above)
>65	-16.77	-.0032 x 5240.0 (same approach as above)

where the value for all deaths is provided in his Table II, and those for specific age ranges computed by multiplying the effect on log deaths per 100,000 in his Table III by the number of deaths per 100,000 in his Table I.

In our results above, we consider the effects of a change in DOD spending or a general demand (Bartik) income shock on mortality. In each case, a unit change is an increase in defense spending or income equal in magnitude to the level of local income, rather than a percentage point of defense spending or income, so we need to divide the coefficients in Table 6 by 100 and multiply them by -1 to make them of a comparable scale and sign to Ruhm's. Also, to convert these effects of a percentage point change in DOD spending or income to those of a change in the unemployment rate, we divide them by the employment-rate responses in the last two columns of Table 9 (0.214 and 0.125 respectively, for DOD shocks and general demand shocks). The results for effects on mortality (for all CBSAs, based on the first and third columns of Tables 6a and 6b) are:

	DOD demand shock	General demand shock
All	+2.20	-8.76
25-44	-1.27	-3.06
45-64	+11.26	-7.33
>65	+35.50	-42.54

(Note that we have ages 25-44 whereas Ruhm has 20-44.)

Our results for the general demand shock are generally of the same sign and order of magnitude as Ruhm's, whereas those of the DOD shock are of the opposite sign.

Appendix C. Differential Social Effects of DOD and General Demand Shocks: The Employment Margin.

Here we examine the extent to which improvements in social outcomes among households without a bachelor's degree can be attributed to changes in the employment rate. We then examine how the employment rate response depends on the composition of demand shocks across cities, occupations, and industries.

A. <u>Differential Social Effects of DOD and General Demand Shocks: The Employment Margin</u>

We can obtain an approximation of the role of the employment margin by decomposing changes in rates of social indicators for households without a bachelor's degree. First, note that the rate of a social outcome among no-bachelor's residents of city ℓ at time t is

$$\frac{O_{\ell,t}}{Pop_{\ell,t}} = \frac{Pop^{E}_{\ell,t}}{Pop_{\ell,t}} \times \frac{O^{E}_{\ell,t}}{Pop^{E}_{\ell,t}} + \frac{Pop^{NE}_{\ell,t}}{Pop_{\ell,t}} \times \frac{O^{NE}_{\ell,t}}{Pop^{NE}_{\ell,t}},$$

where $Pop_{\ell,t}$ is the population of people without a bachelor's degree in city ℓ at time t and $O_{\ell,t}$ is the number of these people with a social outcome of interest. $Pop^{E}_{\ell,t}$ is the number of no-bachelor's residents that are employed, $Pop^{NE}_{\ell,t}$ is the number that are not employed, and $O^{E}_{\ell,t}$ and $O^{NE}_{\ell,t}$ are defined analogously. Then, to a first-order approximation, we can write:

$$\Delta\left(\frac{O_{\ell,t}}{Pop_{\ell,t}}\right) \approx \sum_{e\in\{E,NE\}} \left\{\frac{Pop^{e}_{\ell,t-2}}{Pop_{\ell,t-2}} \times \Delta\left(\frac{O^{e}_{\ell,t}}{Pop^{e}_{\ell,t}}\right) + \frac{O^{e}_{\ell,t-2}}{Pop^{e}_{\ell,t-2}} \times \Delta\left(\frac{Pop^{e}_{\ell,t}}{Pop_{\ell,t}}\right)\right\}$$

Note that (ignoring migration) since $\Delta\left(\frac{Pop^{E}_{\ell,t}}{Pop_{\ell,t}}\right) = -\Delta\left(\frac{Pop^{NE}_{\ell,t}}{Pop_{\ell,t}}\right)$, we can write

$$E^{O}_{\ell,t} \equiv \sum_{e\in\{E,NE\}} \frac{O^{e}_{\ell,t-2}}{Pop^{e}_{\ell,t-2}} \times \Delta\left(\frac{Pop^{e}_{\ell,t}}{Pop_{\ell,t}}\right) = \Delta\left(\frac{Pop^{E}_{\ell,t}}{Pop_{\ell,t}}\right) \times \left[\frac{O^{E}_{\ell,t-2}}{Pop^{E}_{\ell,t-2}} - \frac{O^{NE}_{\ell,t-2}}{Pop^{NE}_{\ell,t-2}}\right],$$

which captures the portion of changes in rates of outcome O that can be attributed to changes in the employment rate (and differences in rates of O among the employed and unemployed). We will refer to $E^{O}_{\ell,t}$ as the employment margin of social outcome O.

Appendix Appendix Table9 reports regression coefficients when $E^{O}_{\ell,t}$ is the dependent variable in specification **Error! Reference source not found.** for various social outcomes for which DOD spending shocks have meaningful effects among those without a bachelor's degree. The employment margin explains large shares of the declines in poverty, food stamp receipt, and disability. For example, the employment margin component of disability effects is -0.051, nearly half of the decline in disability for those with no bachelor's degree of -0.115 (Table 3, column (6)). The employment margin also accounts for increases in marriage rates and occupational prestige, although for a smaller share of the total change in these outcomes in response to a DOD spending shock.

B. Differential Employment Effects: The Role of Industry, City, and Occupational Composition

Here, we examine the role of the industry, location, and occupational composition of DOD spending shocks and general demand shocks in driving the differential employment response. Assuming no changes in the relative employment of households with and without bachelor's degrees in any city by industry cell, changes in employment in city ℓ can be written as

$$\Delta Emp_{\ell,t} = \sum_i \left(\frac{Emp_{i,t-2}^{\text{NoBach}}}{Emp_{i,t-2}^{\text{Total}}} + \frac{Emp_{i,t-2}^{\text{Bach}}}{Emp_{i,t-2}^{\text{Total}}} \right) \times \Delta Emp_{i,\ell,t}, \quad (1)$$

where i indexes industries or occupations. Based on this decomposition, we can write predicted employment (based on pre-period industry or occupation shares of no-bachelor's workers) for households without a bachelor's degree as

$$\Delta \widehat{Emp_{\ell,t}}^{\text{NoBach}} = \sum_i \frac{Emp_{i,t-2}^{\text{NoBach}}}{Emp_{i,t-2}^{\text{Total}}} \times \Delta Emp_{i,\ell,t}. \quad (2)$$

Similarly, we can predict employment based only on variation in city-level allocations of no-bachelor's workers:

$$\Delta \widehat{Emp_{\ell,t}}^{\text{NoBach,City}} = \frac{Emp_{\ell,t-2}^{\text{NoBach}}}{Emp_{\ell,t-2}^{\text{Total}}} \times \Delta Emp_{\ell,t} \quad (3)$$

Panel A of Appendix Table 10 reports coefficients from using each of these measures of predicted no-bachelor's employment as the dependent variables in regressions **Error! Reference source not found.** and **Error! Reference source not found.**. Panel B presents analogously defined effects on predicted earnings (rather than employment). The actual differential employment effect is 0.21 (0.46-0.25). A quarter of this difference is associated with differences in no-bachelor's shares across industries (0.059=0.185-0.126). Differences across cities and across occupations account for much larger shares of the actual difference, each to similar degrees.

Turning to earnings, DOD spending shocks have stronger effects, but the difference is small compared to the differential employment effects. Furthermore, neither industry, occupation, nor city shares of no-bachelor's earnings explain any of this (small) difference. In short, the city and occupation composition of DOD spending shocks account for a large share of its stronger effect on employment. Differential effects of the demand shocks on earnings are smaller and not accounted for by the industry, occupation, or city composition of the shocks.

Appendix Table 11 reports results underlying those in Appendix Table 10 for the industries and occupations with the largest differential employment effect (of DOD spending shocks compared to general demand shocks). Within industries, the DOD-induced employment change among those with no bachelor's degree is strongest in the construction and manufacturing industries, whereas general demand shocks have much milder employment effects in these industries. The mild employment effect of general demand shocks on no-bachelor's employment in the manufacturing industry is surprising, given that manufacturing is highly tradable and accounts for much of the variation in the general demand shock. The mild employment (Panel A) and earnings (Panel B) effects of general demand shocks among those with no bachelor's degree in the manufacturing industry implies that manufacturing-industry workers with a bachelor's degree are by far the strongest beneficiaries of general increases in demand for manufactured goods.

The occupations that benefit the most from DOD spending shocks are Production and Maintenance occupations. These occupations have among the lowest occupational prestige scores among those with no bachelor's degree. Given that previously unemployed workers typically find jobs on lower rungs of the job ladder (e.g., Krolikowski 2017), it is unsurprising that employment gains would be concentrated in low-rung occupations such as Production and Maintenance.

As discussed above, the city composition of shocks also explains the differential employment effects of the demand shocks (Appendix Table 12 reports correlations between the demand shocks, using national growth rates from 2005-2007, and CBSA characteristics). General demand shocks are directed toward cities that are larger, richer (based on housing value and average earnings), have a less elastic housing supply, have a greater share of formally educated residents, and have higher employment rates. DOD spending shocks, in contrast, are directed toward cities that are relatively smaller and have relatively lower employment rates, lower earnings, and fewer residents with a bachelor's degree. Given this differential city composition of shocks, it is not surprising that DOD shocks disproportionately benefit those without a bachelor's and those who would otherwise be unemployed.

These findings suggest that the social benefits of demand shocks depend on the occupation and city distribution of exposure to the shocks. Therefore, fiscal policies that target Production and Maintenance occupations and smaller cities are likely to generate the largest social benefit. Targeting cities may be more feasible than targeting occupations. But given the concentration of production occupations in small cities, targeting in only one dimension may be sufficient. To evaluate this conjecture, we compute predicted employment based on the city *and* occupation variation on no-bachelor's shares (columns 7 and 8 of Appendix Table 10). This responds only slightly more to DOD shocks than do $\Delta\widehat{Emp}_{\ell,t}^{\text{NoBach}}$ or $\Delta\widehat{Emp}_{\ell,t}^{\text{NoBach,City}}$, which suggests that targeting cities can be sufficient.

Appendix D. Additional tables and figures.

Appendix Table 1. Labor Force Responses by Demographic Group: 5-year outcomes in response to shocks measured over 5 years

Labor Market Outcomes:	Total ACS Earnings		Average ACS Earnings		Employment Rate	
Shock:	DOD	General Demand	DOD	General Demand	DOD	General Demand
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.776***	0.602***	0.512**	0.407***	0.202***	0.171***
	(0.260)	(0.062)	(0.198)	(0.057)	(0.069)	(0.038)
Education						
No Bachelors	0.559***	0.438***	0.737***	0.564***	0.218***	0.194***
	(0.177)	(0.051)	(0.233)	(0.062)	(0.078)	(0.043)
Bachelors	0.201*	0.177**	0.153	0.154	0.093**	0.098***
	(0.117)	(0.066)	(0.195)	(0.147)	(0.043)	(0.030)
N	1684	1684	1684	1684	1684	1684
First-Stage F statistic	21.718	95.244	21.718	95.244	21.718	95.244

Note: This table reports the effect of increases in DOD spending over a five-year time span (instrumented by the DOD Bartik shock) and earnings over a five-year time span (instrumented by the traditional Bartick shock) on labor market outcomes over a five-year time span. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 2. 5-year outcomes in response to shocks measured over 5 years

Panel A Social Outcomes:	Poverty	Food Stamp Receipt	Welfare Income	Medicaid Receipt	Health Insurance	Disabled	Occupational Prestige	Transportation Time to Work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DOD shock	-0.103**	-0.112*	-0.002	0.170	0.951*	-0.097***	2.146	-6.263**
	(0.038)	(0.064)	(0.002)	(0.442)	(0.537)	(0.036)	(1.561)	(2.343)
General Demand shock	-0.115***	-0.142***	-0.003**	0.007	0.113**	0.035	0.042	2.632
	(0.026)	(0.037)	(0.001)	(0.034)	(0.048)	(0.027)	(0.579)	(2.479)
Panel B Social Outcomes:	**Multi-Family Home**	**Homeowner**	**Married**	**Divorced**	**Single Parent**	**Mortality age 45-65**	**Mortality age 65-99**	**Median AQI**
DOD shock	-0.014	0.108**	0.053	-0.029	-0.013	-137.4**	-689.0***	-3.812
	(0.038)	(0.050)	(0.058)	(0.030)	(0.025)	(60.1)	(235.4)	(6.592)
General Demand shock	-0.010	0.021	-0.053**	-0.006	0.015	144.2***	365.6**	3.206
	(0.025)	(0.028)	(0.022)	(0.020)	(0.011)	(40.6)	(158.4)	(7.093)

Note: This table reports the effect of increases in DOD spending over a five-year time span (instrumented by the DOD Bartik shock) and earnings over a five-year time span (instrumented by the traditional Bartick shock) on social outcomes over a five-year time span. The coefficients on each shock are estimated from separate regressions. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 3. Effects of DOD Shocks: Difference between Bachelor's and non-Bachelors' Households

	Difference (bachelor's -no-bachelor's)	p-value
Outcome	(1)	(2)
Total Earnings	-0.512***	(0.002)
Average Earnings	-0.436	(0.147)
Employment Rate	-0.174*	(0.054)
Labor Force Participation Rate	0.121	(0.180)
Population	-0.517*	(0.078)
Poverty	0.125*	(0.052)
Food Stamp Receipt	0.080	(0.221)
Disabled	0.111***	(0.009)
Multi-family home	0.070	(0.367)
Homeowner	0.103	(0.384)
Married	-0.149	(0.012)
Divorced	0.012	(0.863)
Single parent	-0.016	(0.783)

Note: This table reports the differential effect of DOD shocks on outcomes for non-bachelor's households and bachelor's households (column 1). Column 2 reports the statistical significance (p-value) of the difference.

Appendix Table 4. Effects of DOD Shocks: Differences by Demographic Group, QWI Data

	Total Earnings	Average Earnings	Employment Rate
	(1)	(2)	(3)
Black compared to White	-0.296***	0.357*	0.158
	(0.001)	(0.057)	(0.203)
Hispanic compared to White	-0.337***	0.457***	0.175***
	(0.000)	(0.010)	(0.003)
Male compared to Female	0.218***	0.397***	0.116***
	(0.001)	(0.000)	(0.000)
Young compared to Middle-aged	0.071*	0.153*	0.106***
	(0.079)	(0.083)	(0.001)

Note: This table reports the differential effect of DOD shocks on labor market outcomes by demographic category. P-values of the differences between demographic groups are reported in parentheses. Young refers to ages 22 to 44, and middle-aged refers to ages 45 to 64.*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 5. Effect of DOD shock on Transportation Method and Density

	Drive	Public Transportation	Walk or Bike	Work From Home	Employment-weighted Density
	(1)	(2)	(3)	(4)	(5)
DOD shock	-0.069*	0.022	0.009	0.022	0.746**
	(0.036)	(0.017)	(0.018)	(0.025)	(0.336)
N	2542	2542	2542	2542	2542
First-Stage F statistic	28.6	28.6	28.6	28.6	28.6

Note: This table reports the differential effect of DOD shocks on changes in the share of workers driving to work (column 1), taking public transportation to work (column 2), walking/biking to work (column 3), and working from home (column 4); and employment-weighted density (column 5). Employment-weighted density is employment-weighted average of employment density across zip codes in a CBSA. Standard errors clustered at the state level are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 6. Top Five Industry Responses to General Demand Shock

	Mining	Manufacturing	Construction	Wholesale Trade	Professional Services
	(1)	(2)	(3)	(4)	(5)
General Demand	0.209***	0.192***	0.129***	0.073***	0.071***
	(0.055)	(0.041)	(0.014)	(0.016)	(0.010)
N	2460	2502	2502	2502	2502
First-Stage F statistic	147.47	151.10	151.10	151.10	151.10

Note: This table reports the response of industry-level earnings to changes in CBSA-level earnings (instrumented with the general demand shock) for industries with the strongest response. All variables are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 7. Social Outcomes by Demographic Group, General Demand Shock

Social Outcomes (rates):	Transportation time to work	Multi-family home	Homeowner	Home Value (growth)	Married	Divorced	Single parent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Demographic Group							
All	4.298**	0.009	-0.017	0.377*	0.005	-0.022	-0.027
	(1.991)	(0.022)	(0.026)	(0.202)	(0.030)	(0.020)	(0.027)
Education							
No Bachelors	3.418	-0.003	0.016	0.507**	0.016	-0.018	-0.029
	(2.101)	(0.024)	(0.033)	(0.206)	(0.036)	(0.021)	(0.033)
Bachelors	6.537*	0.042	-0.147***	0.252	-0.088*	-0.035	-0.025
	(3.299)	(0.036)	(0.044)	(0.209)	(0.048)	(0.029)	(0.017)
Race							
White	4.140*	-0.009	-0.020	0.272	-0.016	-0.025	-0.031
	(2.312)	(0.022)	(0.028)	(0.181)	(0.040)	(0.031)	(0.028)
Black	16.361**	0.126	-0.160	0.372	0.040	-0.034	0.088
	(7.448)	(0.123)	(0.147)	(0.468)	(0.206)	(0.083)	(0.105)
Hispanic	11.925*	-0.080	-0.226	-0.226	-0.072	0.045	0.035
	(6.004)	(0.110)	(0.179)	(0.179)	(0.093)	(0.057)	(0.103)
Age							
20-40	3.931	0.020	-0.021	0.268	0.007	-0.034	-0.031
	(3.174)	(0.040)	(0.037)	(0.242)	(0.052)	(0.033)	(0.061)
41-61	4.937	-0.003	-0.012	0.555**	0.010	-0.034	-0.027
	(2.967)	(0.037)	(0.030)	(0.212)	(0.031)	(0.025)	(0.018)
62-70	4.504	0.065**	-0.073	0.202	-0.120*	0.014	0.003
	(5.376)	(0.025)	(0.062)	(0.197)	(0.068)	(0.033)	(0.032)
Sex							
Male	5.296*	0.028	-0.024	0.379*	-0.024	-0.018	-0.009
	(3.110)	(0.026)	(0.034)	(0.194)	(0.032)	(0.034)	(0.017)
Female	1.937	-0.007	-0.006	0.389*	0.022	-0.026	-0.046
	(1.869)	(0.025)	(0.033)	(0.209)	(0.032)	(0.020)	(0.045)
N	2542	2542	2542	2542	2542	2542	2542
First-Stage F statistic	160.1	160.1	160.1	160.1	160.1	160.1	160.1

Note: This table reports the effect of a percent increase in earnings (instrumented by a general demand shock) by demographic category over a two-year time span. CBSA-level earnings growth is instrumented with a traditional Bartik shock. All variables except the Bartik shock are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 8. Savings from General Demand Shocks.

	Estimate	Population share	Required Spending	Value per person	Benefits per dollar
	(1)	(2)	(3)	(4)	(5)
<u>Safety Net Savings</u>					
Food Stamp Receipt	-0.122	1	$139,344	$1,500	$0.011
Welfare Payments (direct savings)	-0.002				$0.002
Medicaid Receipt Bachelor's	0.08	0.27	-$787,037	$8,436	-$0.011
Medicaid Receipt 6-10	-0.258	0.104	$633,572	$3,556	$0.006
Medicaid Receipt 16-20	-0.184	0.121	$763,564	$3,556	$0.005
Health Insurance White (net)	0.217	0.71	$110,339	$817	$0.007
Subtotal					**$0.020**
<u>Other Social Benefits</u>					
Transportation Time (hours per year)	35.8	1	-$474	$10	-$0.021
Child Poverty age 6-10	-0.18	0.104	$908,120	$50,000	$0.055
Child Poverty age 11-15	-0.28	0.106	$572,776	$50,000	$0.087
Child Poverty age 11-15	-0.22	0.121	$638,618	$50,000	$0.078
Mortality (per 100k)	109.5	1	-$15,525,114	$369,000	-$0.024
Vehicle Theft (per 100k)	-121.6	1	$13,980,263	$10,000	-$0.001
Subtotal					**$0.175**
Total					**$0.195**

This table derives social benefits per dollar of spending from a general demand shock. Unless otherwise specified, outcomes are changes in rates in response to DOD spending equal to local earnings. The method for determining the value of benefits is analogous to the method used to determine the benefits of DOD spending in Table 8. When there are statistically significant effects across overlapping subgroups, we compute value based on the larger subgroup. Benefits per dollar (column 5) is the value per person of the value of the outcome (column 4) divided by the amount the DOD must spend to produce that outcome (column 3). The amount in column 3 is average QCEW earnings (17k) divided by the (negative of the) estimate from column 1 and the population share from column (2).

Appendix Table 9. Employment Margin among Households without a Bachelor's Degree

Social Outcomes (rates):	Poverty	Food Stamp Receipt	Disabled	Multi-family home	Homeowner	Married	Divorced	Occupational Prestige
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DOD shock	-0.046***	-0.030***	-0.055***	0.001	0.010	0.014**	-0.006	0.775***
	(0.015)	(0.011)	(0.018)	(0.003)	(0.007)	(0.007)	(0.004)	(0.248)
N	2542	2542	2542	2542	2542	2542	2542	2542
First-Stage F statistic	28.6	28.6	28.6	28.6	28.6	28.6	28.6	28.6

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) on the employment margin of social outcomes among those without a bachelor's degree over a two-year time span. DOD spending is instrumented with a Bartik shock. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 10. Predicted Earnings and Employment of No-Bachelor's based on Industry, City, and Occupation Composition of Demand Shocks

Prediction based on:	Industry composition		City Composition		Occupation composition		City-and-Occupation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Predicted Employment								
DOD shock	0.185***		0.248**		0.258***		0.279***	
	(0.061)		(0.094)		(0.081)		(0.086)	
General demand shock		0.126***		0.143***		0.163***		0.169***
		(0.029)		(0.039)		(0.035)		(0.038)
Panel B: Predicted Earnings								
DOD shock	0.317**		0.363*		0.380**		0.453**	
	(0.150)		(0.195)		(0.163)		(0.199)	
General demand shock		0.362***		0.392***		0.397***		0.445***
		(0.050)		(0.060)		(0.059)		(0.063)

Note: This table reports the effect DOD shocks and general demand shocks on predicted employment (Panel A) and predicted earnings (Panel B) of workers without a bachelor's degree. Predicted outcomes in Column 1 and 2 are based on national variation in the no-bachelor's share across industries. Predicted outcomes in Columns 3 and 4 are based on city variation in the share of no-bachelor's workers. Predicted outcomes in Columns 5 and 6 are based on occupation variation in the share of no-bachelor's workers. Columns 7 and 8 are based on city and occupation variation. All variables are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 11. Industries and Occupations with Strongest Differential Employment Effect of DOD Shocks among Those with No Bachelor's Degree

Prediction based on:	Industries				Production and Maintenance Occupations	
	Construction		Manufacturing			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Employment (no bachelor's)						
DOD shock	0.132**		0.124***		0.106***	
	(0.062)		(0.040)		(0.037)	
General demand shock		0.067***		0.060***		0.088***
		(0.020)		(0.022)		(0.026)
Panel B: Predicted Earnings (no bachelor's)						
DOD shock	0.154		0.248***		0.173**	
	(0.092)		(0.086)		(0.074)	
General demand shock		0.130***		0.063*		0.138***
		(0.040)		(0.037)		(0.028)
N	1406	1406	1837	1837	2304	2304
First-Stage F statistic	8.68	130.94	19.93	97.53	26.85	97.24

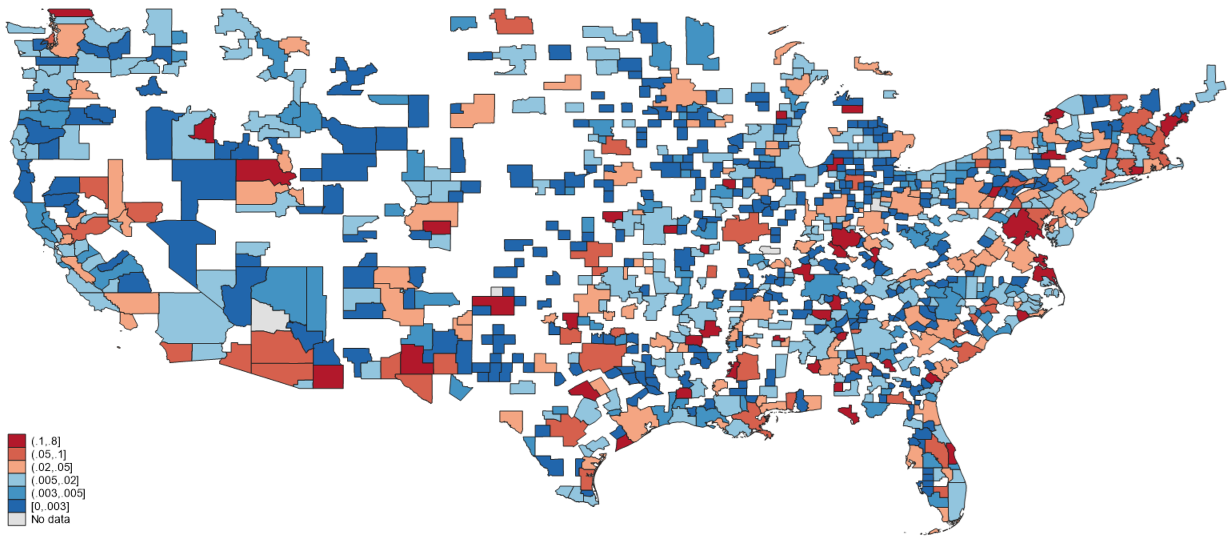
Note: This table reports the effect DOD shocks and general demand shocks on employment (Panel A) and earnings (Panel B) of workers without a bachelor's degree in industries and occupations with the strongest differential effect of DOD shocks. Industry-and-occupation-level changes in non-bachelor's employment and earnings are normalized by total (across industry and occupation) changes in non-bachelor's employment and earnings. All variables are winsorized at the 1% and 99% levels. Fixed effects for CBSA and year are included but not reported. Standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 12. Correlations between Demand Shocks and CBSA Characteristics

	Shock:	
	General Demand	DOD Spending
	(1)	(2)
log(population)	0.297	0.072
Saiz (2010) housing supply elasticity	-0.134	-0.051
Bachelor's share	0.182	0.082
White share	-0.297	-0.091
Poverty	0.044	-0.123
Employment rate	0.152	0.081
Average home value	0.210	0.012
Average wage earnings	0.192	0.135

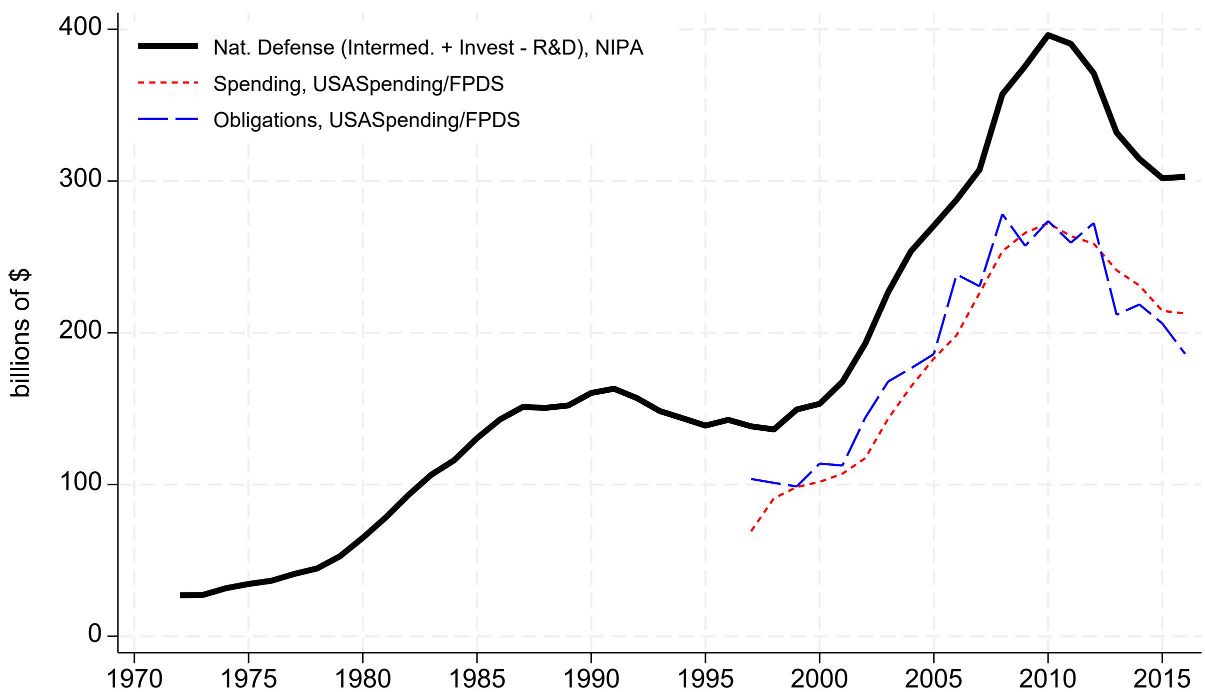
Note: This table reports correlation coefficients between the demand shocks and CBSA covariates. Column (1) reports correlations with the general demand shock, and column (2) reports correlations with the DOD spending shock. The shocks are based on national growth rates between 2005 and 2007, and with the exceptions of the Saiz (2010) housing supply elasticity and population (based on 2000 Census), the CBSA covariates are based on estimates from the 2005 ACS.

Appendix Figure 1. Cross-Sectional Variation in DOD Spending.



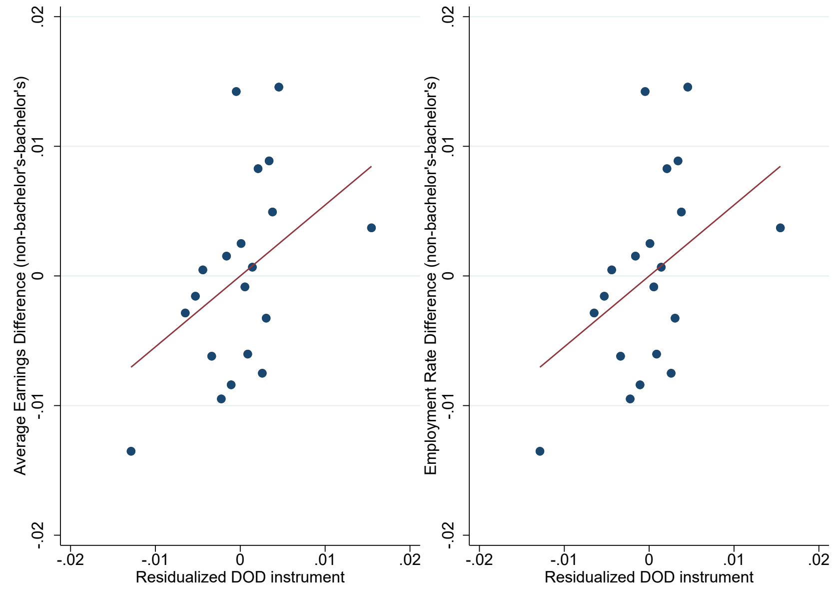
Note: this figure shows variation in $s_\ell \times (G_{2005}/Y_{\ell,2005})$, that is, the CBSA share of national spending scaled by national spending relative to CBSA-level labor earnings.

Appendix Figure 2. Time-Series Variation in National DOD Spending.



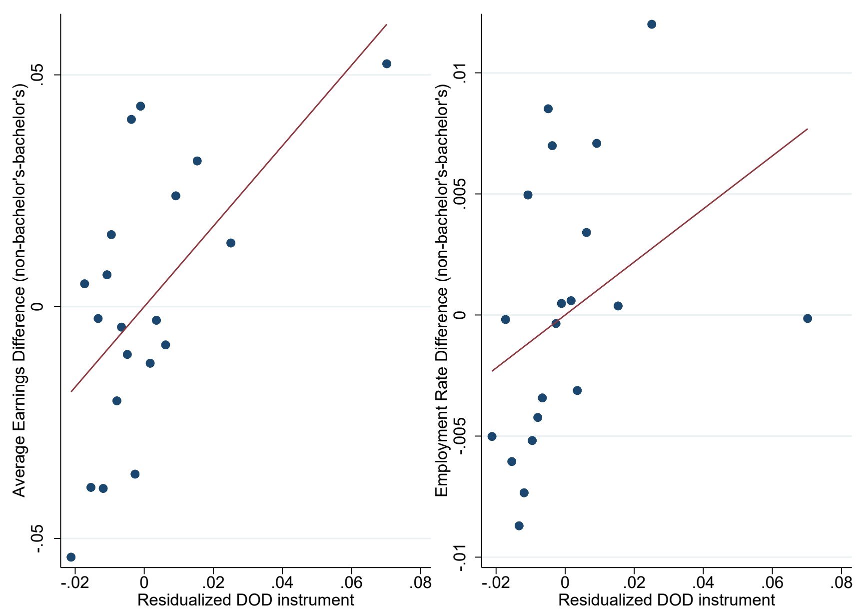
Notes: The level difference between NIPA and our contract-based DOD spending stems from several sources. First, we use contracts awarded only by the Department of Defense while national defense spending covers other agencies responsible for national defense (e.g., Department of Energy, CIA, U.S. Coast Guard, etc.). Second, we use only contacts with the place of performance in the U.S. This means we exclude military spending in overseas bases and operations (this is about 10-15% of DOD contracts). Third, there is a collection of smaller issues (e.g., missing zip codes for the place of contract performance) that contribute to the difference between the NIPA statistics and our aggregate spending.

Appendix Figure 3. Visual Evidence of Relationship between DOD shock and Differential Labor Market Effects (non-bachelor's versus bachelor's), Full Panel.



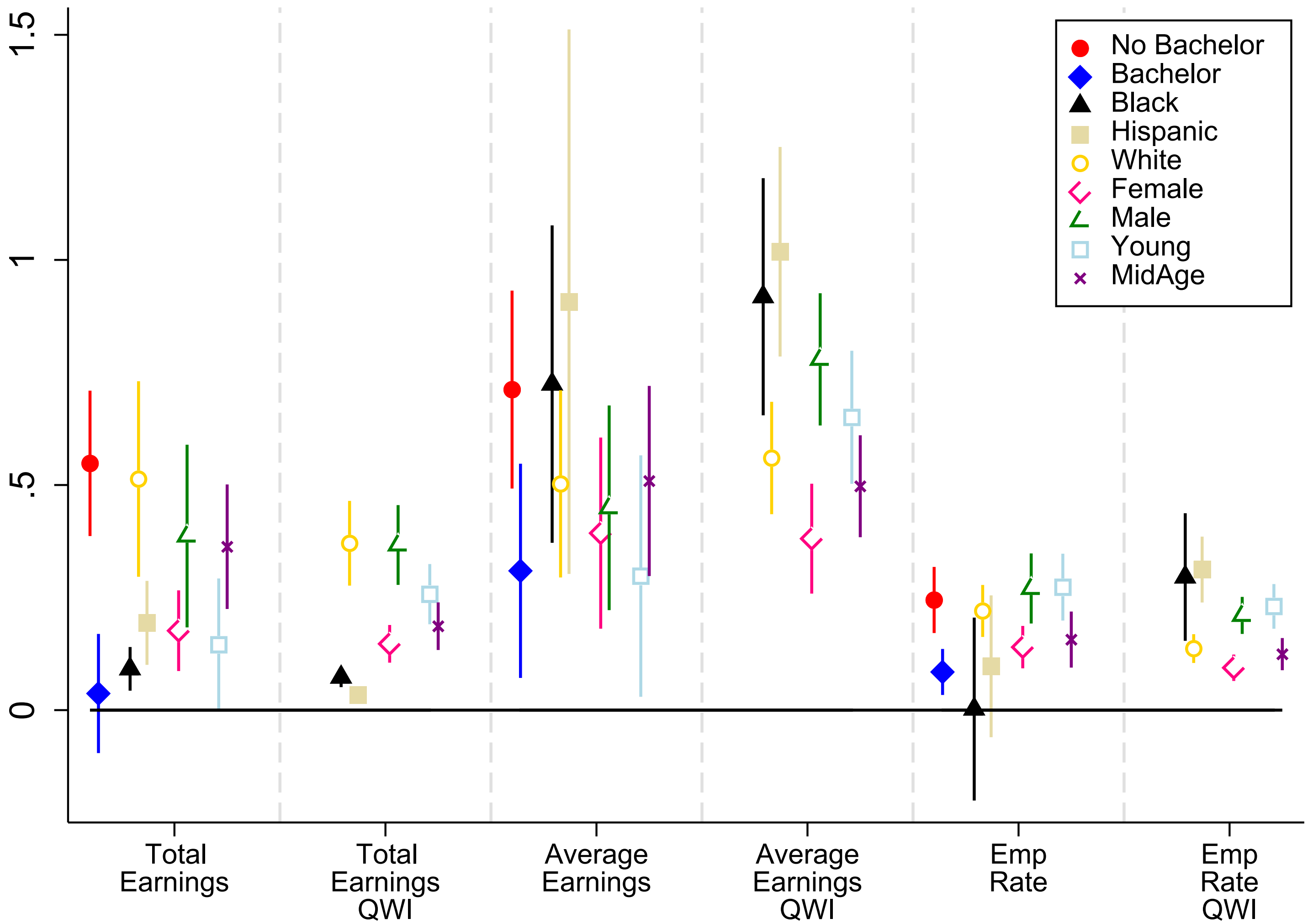
Note: This figure plots the binscatter of the difference (between non-bachelor's and bachelor's) households in Average earnings growth (left panel; employment rates, right panel) and the residuals from a regression of the DOD instrument $\frac{s_\ell \times (G_t - G_{t-2})}{Y_{\ell,t-2}}$ on time and CBSA fixed effects.

Appendix Figure 4. Visual Evidence of Relationship between DOD shock and Differential Labor Market Effects (non-bachelor's versus bachelor's), Cross-Sectional Variation.



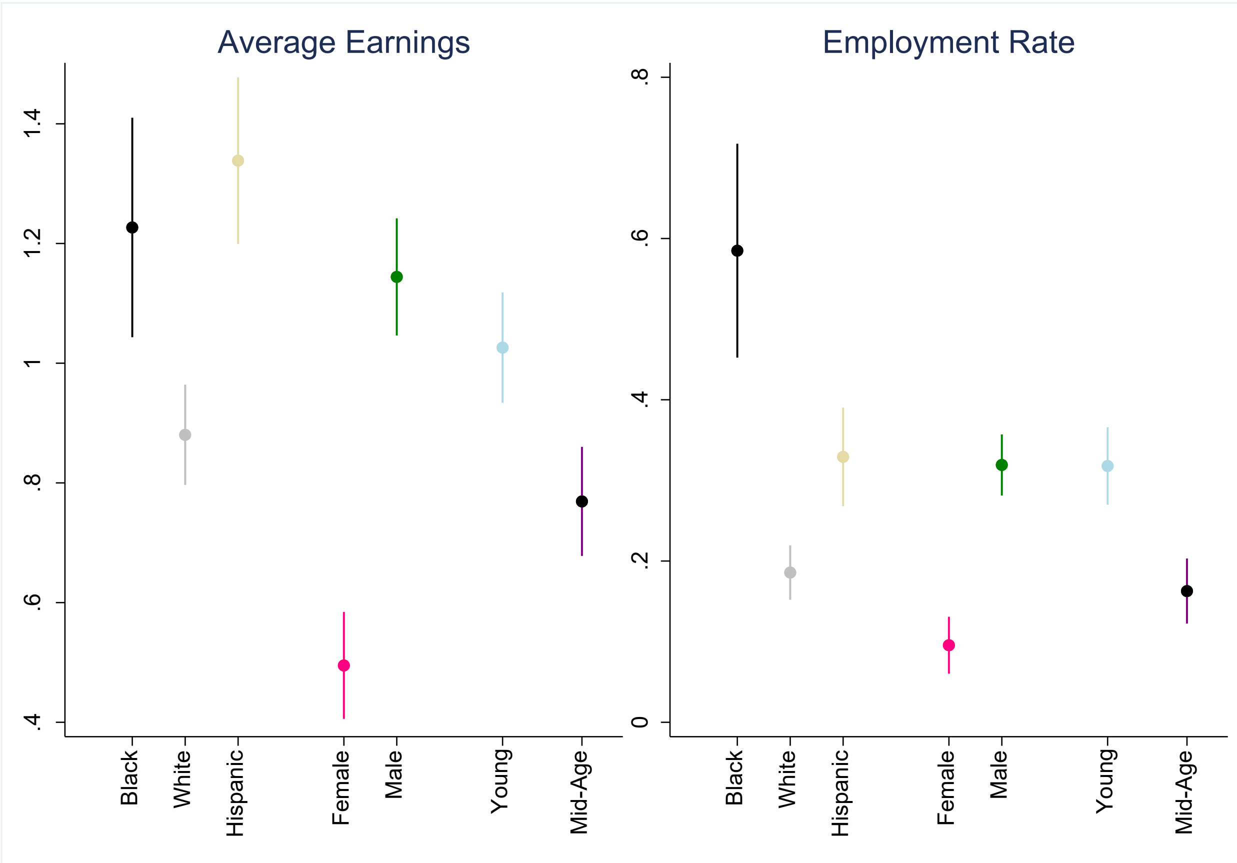
Note: This figure is similar to Appendix Figure 3 but exploits only cross-sectional variation. Specifically, we examine changes in DOD spending and labor market outcomes between 2005/06 and 2009/10. We obtain residuals from a regression of the DOD instrument on the CBSA covariates from Demyanyk at al (2019). We then plot the binscatter of the differential labor market outcomes and these residuals.

Appendix Figure 5. Labor Market Effects of DOD Spending by Demographic Group, ACS and QWI.



Note: This figure plots the regression coefficients plus and minus one standard error from regressions of labor market outcomes (by demographic group) on DOD spending. The first-stage F-statistic for the QWI-based regressions is 143.4 (N=11911).

Appendix Figure 6. Distributional Effects of General Demand Shocks by Demographic Group, QWI.



Note: This figure plots the regression coefficients plus and minus one standard error from regressions of labor market outcomes (by demographic group) on DOD spending. The first-stage F-statistic for the QWI-based regressions is 189.3 (N=12567).