DEMAND STIMULUS AS SOCIAL POLICY

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Abstract: We exploit a recent 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 generated by defense spending accrues predominantly to households without a bachelor's degree. These households as well as Black 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. These effects vary across demographic groups.

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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 take into account 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

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

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 examine the broader distributional and social implications of DOD spending. To do so, 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. The majority of our social outcomes are based on 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. We also examine data on local mortality rates from the Center for Disease Control (CDC), which provides detailed information on underlying causes of mortality as well as the age of the deceased, and data on crime rates compiled by the Federal Bureau of Investigation (FBI).

We begin by documenting how the income generated by local DOD spending is distributed locally across demographic groups. The majority of 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. But, adjusting for shares of existing income, increases in DOD spending increase the relative income of Blacks 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 a slightly larger increase in average earnings for Black households.[2] Thus, DOD spending can contribute to achieving one of the important objectives of many tax expenditures and direct transfers targeted to Americans with low levels of 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, implying large benefits for otherwise unemployed workers. Our empirical setting also addresses the pressing

[2] Income from DOD spending accrues to workers and owners of capital. Since wage and salary income accounts for less than 100% of local income, it is expected that the response of wage and salary income to a DOD spending increase of 1% of local earnings is less than 1%.

policy question of whether demand stimulus can increase labor force participation rates. While labor force participation does not change for most demographic groups, there are large increases for some groups.

Many of the public programs targeted toward low-income households not only support distributional objectives but they 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 unemployment 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, significantly for young children, while health insurance coverage increases across the population, indicating that DOD spending substitutes for costly in-kind benefits while promoting social objectives such as health insurance coverage and 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 with the objective of enhancing 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 (Krolikowski 2017).[3] We examine the effect of DOD spending on occupational prestige—a summary measure of the quality of workers' jobs—and find strong positive effects, with the benefits concentrated among households without a bachelor's degree.

As for programs not targeted primarily toward the poor, the U.S. devotes considerable resources to subsidies to homeownership, through the mortgage interest deduction (or, alternatively, the lack of taxation of imputed rent) and the partial deductibility of property taxes,

[3] 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)

often supported with the argument that homeownership promotes community stability and engagement. But these tax expenditures have been criticized as having relatively little impact on the actual rate of homeownership, as opposed to the amounts of mortgage borrowing or housing owned (e.g., Gruber, Jensen, and Kleven 2021). We estimate 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, which is consistent with the higher homeownership and marriage rates observed for this group.

One of the most economically impactful benefits of a local DOD spending shock is a reduction in the time it takes workers to travel to work. An increase in DOD spending by a percent of local earnings reduces travel times to work by nearly 10 minutes per day overall, which implies that even at a relatively low value of time of $10/hour, the annual benefit is over $500 per worker per year. The reduction in transportation time is apparent in small CBSAs but not large CBSAs.

We also examine the effect of DOD spending on mortality by age group and cause of death. 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 deaths by assault (murders) and accidental deaths. While Case and Deaton emphasize the consequences of declining labor market prospects over prolonged periods of time, our study provides 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 heath 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, we cannot rule out that DOD spending can influence crime rates for certain demographic groups.

Comparison to General Demand Shocks. 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. Do DOD spending shocks and representative demand shocks differentially effect other social outcomes? If there are differential effects, 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, relative to DOD spending shocks they have less of an effect on the extensive margin of employment, especially for households without a bachelor's degree. Their effects on other social outcomes (e.g., disability, occupational prestige) are negligible compared to the effects of DOD spending shocks, and they lead to increases in mortality (consistent with Ruhm 2000) and crime. These differential 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 for the majority of the stronger employment effect of DOD spending shocks.

2. 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 AGM).[4]

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.[5] 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) and the Local Area Unemployment Statistics (LAUS). The underlying data for crime rates comes from the Federal Bureau of Investigation (FBI).

[4] Auerbach, Gorodnichenko, and Murphy (2021) document that the variation in CBSA-level DOD spending is orders of magnitude larger than that at the state and national level.

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

A. Government Spending Data

Our measure of government spending shocks, from a data set developed in AGM, uses data on DOD contracts, available at USAspending.gov. This data source contains detailed information on contracts signed since 2000, including the name and location (zip code) of the primary contractor, 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. Our data run through 2016.

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). To help isolate the component of DOD contracts associated with new production, DLM and AGM 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. [6] 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*. AGM discuss the merits of the instrument, and we provide further details in the discussion of the econometric specification below.

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, relationships to people with whom they live, and income support from the government, among other information. Detailed geographic information is available starting in 2005.

[6] To construct this spending/outlay measure by location, AGM 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.

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), total population, and rates of employment, labor force participation, 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). We construct a CBSA-level measure of occupational prestige by averaging the score across respondents within a demographic category.

We construct CBSA-level measures using representative population weights provided by IPUMS. For some small CBSAs there are instances in which a small number of people from a demographic group are interviewed in a year. To prevent such small samples from driving any results we limit our sample to observations with at least 100 respondents in a demographic category. This restriction does not typically bind except for racial minorities and young children. Results are similar when using higher respondent thresholds.

Table 1 reports summary statistics for each of our social outcomes across the 286 CBSAs in our sample along with information on DOD spending characteristics.

C. Mortality Data

The CDC provides county-level mortality data by age group and cause of death since 1999. We focus on what Case and Deaton (2020) refer to as "deaths of despair" – drug-and-alcohol-related deaths and deaths by suicide. While Case and Deaton emphasize the consequences of declining labor market prospects over prolonged periods of time, our study provides a higher-frequency estimate of the relationship between labor market earnings (induced by aggregate demand stimulus) and deaths of despair.

Finally, we examine deaths that are classified by the CDC as accidental. Accidental deaths include those caused by automobile accidents or other unintended mishaps. 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.[7] The NACJD data includes 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 extend the data to 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 aggregation. The FBI currently provides data at the city agency and county agency level. To get a complete count of all crimes committed in a county, we summed the number of crimes reported from 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

[7] For any agency reporting data for all 12 months in a year, there was no imputation process conducted. For any agency reporting data for anywhere between 3 and 11 months in a year, the final data used was imputed by multiplying the agencies 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 and has a full 12 months of reporting, crimes are imputed using the crime rates of that similar agency.

provides crime counts and crime rates for the 1984 – 2016 period with the exception of 1993 which neither the NACJD nor the FBI provides data for.

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 AGM and Nakamura and Steinsson (2014) and estimate

$$\frac{Y_{d,\ell,t} - Y_{d,\ell,t-2}}{Y_{\ell,t-2}} = \beta \frac{G_{\ell,t} - G_{\ell,t-2}}{Y_{\ell,t-2}} + \psi_\ell + \alpha_t + error_{\ell t}, \quad (1)$$

where d, ℓ and t index demographic groups, locations (CBSA) and time (year), Y is wage and salary earnings, G is DOD spending, and ψ_ℓ and α_t are location and time fixed effects. Coefficient β measures the local DOD earnings multiplier, that is, the dollar amount of earnings for demographic group d produced by a dollar of local DOD spending over a two-year period of time. Whereas AGM focus on one-year effects, we examine two-year effects, as some social outcomes are likely to respond over multiple years. 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.

We instrument for variation in government spending $\frac{G_{\ell,t} - G_{\ell,t-2}}{Y_{\ell,t-2}}$ using a Bartik 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 AGM, the Bartik IV approach not only addresses potential endogeneity concerns but it 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. AGM argue that the Bartik 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.[8]

3. 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 smaller share of the workforce. Therefore, to determine the distributional effects of DOD spending, we also estimate effects on average earnings.

Labor market earnings can increase through various adjustment margins, including the extensive margins of employment and labor force participation as well as population inflows (migration). 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 the labor force (hence benefitting those on the margin of labor force participation) and out of unemployment, or do the benefits accrue exclusively to previously employed workers or to workers from other jurisdictions? To answer these questions we estimate effects of DOD spending on various subgroups.

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. Each of these estimates is based on changes over two-year periods; estimates from longer (5-year) horizons do not differ qualitatively from our main results.

A. Distributional Effects of DOD Spending

Table 2 reports the effect of DOD spending on total labor market earnings and average labor market earnings. 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

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

sample population, as in the ACS), which will tend to imply lower average earnings effects than in the ACS. Estimates from the two different data sources are comparable and not statistically distinguishable, which lends credibility to the estimates.[9] The remaining rows present estimates by demographic groups in the ACS.

According to column (1), a dollar of local DOD spending increases ACS labor earnings by $0.56.[10] The estimate in column (2) implies that a percent increase in DOD spending (as a share of local earnings) generates a $0.43 increase in average ACS earnings.

The remaining estimates in columns 1 and 2 provide information on the demographic groups that benefit the most from DOD spending shocks. Those without a bachelor's degree benefit the most from DOD increases, both in terms of total amounts (column 1) and in terms of percent increase in average earnings (column 2). The middle-aged (41-61) also disproportionately benefit compared to other age groups. In terms of race, Whites receive the majority of income generated by DOD spending, but Blacks experience the largest increase in average earnings.

B. Margins of Labor Market Adjustment

Even within a demographic category, people have varying degrees of attachment to the labor force. Does DOD spending increase employment and labor force participation, or do the benefits of DOD spending accrue predominantly to employed workers or migrants?

Columns 3 through 5 of Table 2 report the effect of DOD spending on changes in the employment rate (the share of the labor force that is employed –column 3), changes in the labor force participation rate (column 4), and local population growth (column 5) across demographic groups. There is a clear increase in employment rates. For example, a percent increase in DOD spending (as a share of local earnings) generates an increase in the local employment rate by 0.22 percentage points, implying that DOD spending is particularly beneficial to the unemployed.

Can demand stimulus pull detached workers back into the labor force? Labor force participation rates have declined rapidly following each of the last two recessions, only to ultimately increase with accelerating GDP growth. The nature of these changes in labor force

[9] 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.

[10] 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.

participation is of paramount interest to policymakers, as they have direct implications for the amount of slack in the labor market. If labor force participation has been responding to aggregate demand (rather than other structural factors), this suggests there may be more policy space for demand stimulus even as unemployment rates decline, for example.

We find no detectable effect on labor force participation rates across all demographic groups in the ACS, suggesting that there are limited overall effects of demand stimulus over short horizons. However, this lack of an overall labor force participation rate response masks important heterogeneity in the response within demographic groups. A percent increase in DOD spending (as a share of local earnings) generates a 0.08 percentage point increase in labor force participation among the middle-aged (column 4, age 41-61) and a 0.14 increase among those with a bachelor's degree.

To what extent does local DOD spending generate a local population response? The population response is strongest among those without a bachelor's degree, which is often identified as the least mobile group of Americans (Moretti 2013), although the estimate is imprecise. A shock that increases average earnings among the less-formally-educated by 71 percent (Table 2, column 2) induces a 17 percentage point increase in that group's local population (implying an elasticity of local population to earnings of over one-fourth among those without a bachelor's degree).

C. Social Outcomes (ACS)

The earnings, employment rate, and labor force participation rate responses of lower-income demographic groups indicates 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 presents the estimated effects for a range of social outcomes. We begin by reporting results based on adult ACS respondents between age 20 and 70. Since outcomes such as poverty can have very different externalities for children than for adults, we subsequently present a relevant subset of results for children by different age groups.

Poverty and welfare. According to column 1 of Table 3, a percent increase in DOD spending (as a share of local earnings) reduces poverty rates 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.

In addition to potentially increasing longer-run outcomes for previously poor households, particularly children (Aizer, Hoynes, and Lleras-Muney 2022), the reduction in poverty naturally

reduces dependence on in-kind transfers. In particular, 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). A percent increase in DOD spending (as a share of local earnings) implies that food stamp receipt declines by 0.08 percentage points. So if average earnings in a city is $30,000 and the average food stamp benefit is $1,500[11], then DOD spending of $300 saves 0.0008X$1,500=$1.20 of food stamp payments.

While food stamp receipt is 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 3), especially for some demographic groups that receive the most earnings benefit from DOD spending. A percent increase in DOD spending (as a share of income) leads to a 0.001 percentage point reduction in disability rates among households without a bachelor's degree, which implies that it takes approximately $4.5 million (average earnings/(0.01X(1-bachelors share)) of untargeted DOD spending to prevent one person without a bachelor's degree from being disabled.

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. Whites are more likely to be married in response to a DOD spending shock. They 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.

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

Work-related outcomes. Jobs confer immediate income-related benefits to workers. They also affect workers' lifetime trajectory of income and other life outcomes (e.g., Blanchflower and Oswald 2004). Job losses tend to have permanent 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? Table 4 demonstrates a substantial increase in occupational prestige, with an increase of DOD spending equal to a percent of local earnings causing a 0.024-point increase in a location's average occupational prestige score. This is nearly identical to the average biennial change in occupational prestige in our sample. Alternatively, it would take a DOD spending shock equal to 77.8 percent of local income to increase occupational prestige by a standard deviation of the score across cities (1.79, Table 1) The effect is particularly strong among household without a bachelor's degree (0.037).

In addition to benefitting from the increase in occupational standing, households also benefit from a reduction in transportation times to work: a percent increase in DOD spending (as a share of local earnings) causes a 6.7-minute-per-day reduction in transportation time to work (which implies a 13.4-minute reduction in total transportation time to and from work). Even if the value of time is as low as $10 an hour, this implies a massive annual economic benefit to workers of approximately $\$558 \approx \frac{\$10}{hour} \times \frac{13.4}{60} \frac{hours}{day} \times 5 \frac{days}{week} \times 50 \frac{weeks}{year}$.

A number of mechanisms could account for this lower transportation time. For example, there may be more job opportunities closer to workers' residence. 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.

Childhood Poverty. As already discussed, the effects of poverty are particularly severe for the life trajectories of children. Therefore, it is helpful to examine poverty responses for children separately than for adults. Table 5 reports that poverty rates tend to decline for children. The effect is economically double that for adults, although the estimates are less precise. Consistent with the reductions in poverty, Medicaid receipt among children falls substantially. There is no detectable decline in health insurance rates, which suggests that children substitute from Medicaid to private health insurance.

D. Mortality

Table 6 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 changes in mortality rates. 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.[12] When examining changes in mortality rates, mortality tends 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 restricting the sample 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 (Table 7), there is an economically 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 a 2.61 fewer deaths among those between age 45 and 65 per 100,000, and to 8.49 fewer deaths among those over age 65 per 100,000. This implies that with average earnings of approximately \$30,000, the DOD can spend $0.01 \times \frac{\$30{,}000}{\text{person}} \times \frac{100{,}000 \text{ people age 45-65}}{2.61 \text{ deaths age 45-65}} \times 2.2 \frac{\text{people}}{\text{people age 45-65}} \approx \$25$ million to save a life of someone age 45-65 and can spend $0.01 \times \frac{\$30{,}000}{\text{person}} \times \frac{100{,}000 \text{ people age 65+}}{8.49 \text{deaths age 65+}} \times 12.5 \frac{\text{people}}{\text{people age 65+}} \approx \$45$ million to save a life of someone age 65+, which amounts to $\approx \$16$ million to save a life of someone age 45+. These life-saving effects of DOD spending indicate that the spending cannot be justified *solely* on the basis of mortality reduction, as the cost exceeds the typical value of a statistical life.[13] Nonetheless, they represent a substantial benefit, and 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.

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.

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

[13] For example, Federal Emergency Management Agency used a value of \$7.5 million as of 2020. (https://www.fema.gov/sites/default/files/2020-08/fema_bca_toolkit_release-notes-july-2020.pdf).

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 8. 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. 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, the FBI or NACJD do not provide information on who commits crime and thus cannot shed more light on hypotheses that emphasize potential distributional effects of DOD spending on crime.

4. 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, Dorn, and Hanson 2013; Beaudry, Green, and Sand 2018; Goldsmith-Pinkham, Sorkin, and Swift 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):

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

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.[14] The resulting demand shock series is relatively independent of our DOD spending shock 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 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. 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.[15] Appendix Table A1 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).

[14] 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 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).

[15] 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 AGM, city-level input-output linkages are quite strong, while general equilibrium effects tend to be small but positive in response to local demand shocks.

A. Effects of a General Demand Shock

Table 9 reports the effects of the general demand shock and, for reference, the effect 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 $0.56, 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 rates 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.

Social Effects of General Demand Shock. Table 10 reports the social effects of the general demand shock. As with the DOD spending shock, there is a substantial decline in poverty and food stamp receipt. However, these broader demand shocks exhibit milder effects on other social outcomes than the DOD spending shocks, particularly for disability rates.

These aggregate effects mask meaningful heterogeneity across demographic groups. Those with a bachelor's degree are *less* likely to own a home or be married in response to a general demand shock, whereas Blacks are more likely to own a home and be married and are less likely to be divorced. Somewhat paradoxically, Blacks are also more likely to be single parents in response to a general demand shock.

Turning to other social outcomes (Table 11), we find that general demand shocks tend to increase occupational prestige, although by far less than DOD spending shocks. And, in contrast to DOD spending shocks, general demand shocks lead to increases in average transportation time to work.

Whereas the social effects of DOD spending shocks and general demand shocks on adults are distinct, the effects on children's outcomes are aligned. Table 12 reports effects of general demand shocks on the young. As with the DOD spending shock, children experience less poverty, are more likely to have health insurance, and are less likely to be on Medicaid.

Turning to mortality (Table 13), 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 (perhaps due to changes in diet and exercise). Furthermore, mortality increases are driven by those over age 45 (Table 14), the same demographic groups that experienced a *decline* in mortality in response to DOD spending shocks.

In a similar spirit, we find (Table 15) that general demand shocks appear to increase vehicle theft while DOD spending shocks can reduce vehicle theft. This pattern is unusual because other types of crime seem to be equally insensitive (i.e., not statistically significant) to general and DOD spending shocks. 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 to different types of spending shocks.

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

Local demand shocks that have similar effects on local earnings have drastically different social effects. 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.[16] Therefore, we conjecture that much of the differential social effects is due to the differential ability to pull households into employment.

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 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.

Table 16 reports regression coefficients when $E^{O}_{\ell,t}$ is the dependent variable in specification (1) for various social outcomes for which DOD spending shocks have meaningful effects among

[16] 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.

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 disability decline for those with no bachelor's degree of -0.114. 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.

C. 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. Changes in employment in city ℓ can be written as

$$\Delta Emp_{\ell,t} = \sum_i \left(\frac{Emp_{i,t-2}^{\text{NoBach}}}{Emp_{i,t-2}^{\text{Total}}} + \frac{Emp_{i,t-2}^{\text{Bach}}}{Emp_{i,t-2}^{\text{Total}}} \right) \times \Delta Emp_{i,\ell,t}, \qquad (3)$$

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

$$\Delta \widehat{Emp}_{\ell,t}^{\text{NoBach}} = \sum_i \frac{Emp_{i,t-2}^{\text{NoBach}}}{Emp_{i,t-2}^{\text{Total}}} \times \Delta Emp_{i,\ell,t}. \qquad (4)$$

Similarly, we can predict employment based only on variation in city-level allocations of bachelor's workers:

$$\Delta \widehat{Emp}_{\ell,t}^{\text{NoBach,City}} = \frac{Emp_{\ell,t-2}^{\text{NoBach}}}{Emp_{\ell,t-2}^{\text{Total}}} \times \Delta Emp_{\ell,t} \qquad (5)$$

Panel A of Table 17 reports coefficients from using each of these measures of predicted no-bachelor's employment as the dependent variables in regressions (1) and (2). Panel B presents analogously defined effects on predicted earnings (rather than employment). For comparison, we also report the (previously reported) effects on actual employment and earnings (columns 7 and 8).

The actual differential employment effect is 0.21 (0.46-0.25). A quarter of this difference is associated with differences in non-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 accounts 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.

Table 18 reports results underlying those in Table 17 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 military occupations (defined broadly to include anyone enlisted in the military) and Production and Maintenance occupations. Production and Maintenance 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. Table 19 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, earnings, and 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.

5. 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 distributional 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 tool 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	667,719	259,002	1,202,035	90,354	12,100,000
Average Earnings	28,732	27,960	5,604	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	0.14	0.13	0.04	0.07	0.28
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	40.54	40.44	1.80	34.99	46.28
Homeowner	0.68	0.69	0.06	0.49	0.83
Transportation time to work	23.16	22.76	3.42	15.93	41.72
Health Insurance	0.83	0.83	0.06	0.52	0.95
Crime Rate (per 100,000)					
Murder Rate	4.70	4.26	2.77	0.53	21.17
Rape Rate	35.20	32.98	14.33	5.04	111.92
Robbery Rate	97.24	91.89	55.61	10.09	314.45
Aggravated Assault Rate	269.22	243.76	141.16	33.36	1,028.87
Burglary Rate	721.15	694.28	297.08	223.63	2,461.45
Larceny Rate	2,143.87	2,116.37	566.60	1,116.87	3,714.54
Vehicle Theft Rate	220.09	182.97	130.84	29.98	687.59
Arson Rate	18.86	17.12	11.77	3.01	121.83
Changes (as share of lagged earnings)					
DOD spending	0.002	0.000	0.023	-0.251	0.230
Predicted DOD spending	0.001	0.000	0.005	-0.036	0.055
Earnings	0.063	0.056	0.045	-0.115	0.354
Growth in Average Earnings	0.019	0.018	0.013	-0.032	0.101
Change in Employment rate	-0.004	-0.003	0.008	-0.037	0.031
Change in Labor Force Participation Rate	-0.005	-0.004	0.007	-0.046	0.025

Note: This table displays summary statistics for the 282 CBSAs with data from the ACS, USAspending.gov, and FBI.

Table 2. Earnings Response by Demographic Group

	Total Earnings	Average Earnings	Employment Rate	Labor Force Participation Rate	Population
	(1)	(2)	(3)	(4)	(5)
Demographic Group					
All (ACS)	0.557**	0.427**	0.216***	0.029	0.065
	(0.249)	(0.197)	(0.061)	(0.032)	(0.090)
All (QCEW or LAUS)	0.855***	0.383***	0.171***	0.110**	0.006
	(0.228)	(0.093)	(0.063)	(0.046)	(0.054)
Education					
No Bachelors	0.548***	0.712***	0.245***	0.025	0.173
	(0.161)	(0.220)	(0.073)	(0.042)	(0.122)
Bachelors	0.037	0.309	0.085	0.138**	-0.372
	(0.132)	(0.238)	(0.051)	(0.065)	(0.224)
Age					
20-40	0.145	0.298	0.273***	-0.067	-0.018
	(0.147)	(0.268)	(0.074)	(0.061)	(0.157)
41-61	0.363**	0.509**	0.157**	0.079*	0.115
	(0.138)	(0.211)	(0.062)	(0.041)	(0.094)
62-70	0.037	0.037	0.037	0.037	0.037
	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
Race					
White	0.513**	0.503**	0.220***	0.052	0.076
	(0.217)	(0.208)	(0.058)	(0.037)	(0.106)
Black	0.092*	0.724**	0.002	0.022	0.133
	(0.048)	(0.352)	(0.203)	(0.104)	(0.260)
Hispanic	0.194**	0.907	0.097	0.173	0.125
	(0.093)	(0.605)	(0.157)	(0.226)	(0.317)
Sex					
Male	0.387*	0.449*	0.270***	0.024	0.090
	(0.203)	(0.227)	(0.078)	(0.045)	(0.098)
Female	0.176*	0.393*	0.140***	0.007	0.053
	(0.090)	(0.212)	(0.047)	(0.044)	(0.099)
N	2,541	2,541	2,541	2,541	2,541
First-Stage F statistic	28.576	28.576	28.576	28.576	28.576

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) on labor market 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. 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 3. Social Outcomes by Demographic Group.

Social Outcomes (rates):	Poverty	Food Stamp Receipt	Disabled	Multi-family home	Homeowner	Married	Divorced	Single parent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Demographic Group								
All	-0.079*	-0.081	-0.080*	-0.037	0.091	0.039	-0.037	-0.011
	(0.047)	(0.056)	(0.047)	(0.030)	(0.059)	(0.049)	(0.031)	(0.023)
Education								
No Bachelors	-0.119**	-0.107	-0.114**	-0.066	0.076	0.085	-0.045	-0.015
	(0.057)	(0.071)	(0.051)	(0.046)	(0.075)	(0.069)	(0.032)	(0.026)
Bachelors	0.002	-0.028	-0.005	0.005	0.161*	-0.072	-0.025	-0.026
	(0.035)	(0.027)	(0.036)	(0.053)	(0.087)	(0.129)	(0.067)	(0.053)
Age								
20-40	-0.093	-0.100	-0.012	-0.059	0.055	-0.032	0.019	-0.007
	(0.078)	(0.087)	(0.043)	(0.053)	(0.118)	(0.107)	(0.057)	(0.044)
41-61	-0.054	-0.061	-0.134*	-0.014	0.081	0.070	-0.124**	-0.015
	(0.053)	(0.051)	(0.068)	(0.030)	(0.063)	(0.067)	(0.053)	(0.044)
62-70	-0.079	-0.178*	-0.081	-0.026	0.098	-0.037	0.089	0.031
	(0.082)	(0.091)	(0.092)	(0.050)	(0.105)	(0.101)	(0.059)	(0.055)
Race								
White	-0.099**	-0.118*	-0.077*	-0.098**	0.086	0.146***	-0.051	-0.041
	(0.038)	(0.060)	(0.042)	(0.040)	(0.058)	(0.047)	(0.044)	(0.025)
Black	0.068	-0.060	0.030	-0.009	0.017	-0.206	-0.064	0.016
	(0.178)	(0.206)	(0.134)	(0.112)	(0.157)	(0.228)	(0.096)	(0.096)
Hispanic	-0.097	-0.101	-0.204	0.231	-0.189	-0.269	0.123	0.179
	(0.273)	(0.273)	(0.130)	(0.255)	(0.379)	(0.212)	(0.146)	(0.153)
Sex								
Male	-0.099*	-0.086	-0.069	-0.025	0.107	0.007	-0.031	-0.021
	(0.051)	(0.056)	(0.056)	(0.037)	(0.083)	(0.063)	(0.035)	(0.024)
Female	-0.072	-0.087	-0.090**	-0.051	0.066	0.072	-0.037	-0.006
	(0.054)	(0.065)	(0.043)	(0.038)	(0.057)	(0.057)	(0.035)	(0.035)
N	2541	2541	2541	2541	2541	2541	2541	2541
First-Stage F statistic	28.576	28.576	28.576	28.576	28.576	28.576	28.576	28.576

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. 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 4. Social Outcomes by Demographic Group

Social Outcomes (rates):	Occupational Prestige	Transportation time to work	Health Insurance	Medicaid
	(1)	(2)	(3)	(4)
Demographic Group				
All	2.241*	-6.876**	0.338	-0.068
	(1.280)	(2.602)	(0.212)	(0.181)
Education				
No Bachelors	3.663***	-6.663**	0.309	-0.103
	(1.192)	(3.065)	(0.226)	(0.216)
Bachelors	0.400	-10.336**	-0.018	-0.027
	(3.051)	(4.641)	(0.177)	(0.144)
Age				
20-40	2.716	-2.048	0.254	-0.188
	(2.334)	(3.436)	(0.282)	(0.272)
41-61	0.918	-10.421***	0.653*	0.074
	(1.711)	(3.350)	(0.368)	(0.210)
62-70	1.250	-7.675	-0.163	-0.147
	(3.459)	(7.519)	(0.157)	(0.238)
Race				
White	2.519*	-6.626**	0.377	-0.196
	(1.351)	(3.135)	(0.232)	(0.150)
Black	3.048	5.079	1.945	-0.487
	(4.726)	(6.143)	(2.435)	(0.947)
Hispanic	-4.696	-9.480	-0.711	1.365
	(7.607)	(8.269)	(0.904)	(1.184)
Sex				
Male	0.678	-8.285**	0.258	0.090
	(2.203)	(3.587)	(0.212)	(0.247)
Female	3.690*	-6.032*	0.447	-0.186
	(2.041)	(3.342)	(0.279)	(0.203)
N	2541	2541	1755	1755
First-Stage F statistic	28.576	28.576	4.715	4.715

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. The sample is limited to CBSA-years with at least 100 respondents for the given category. 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 5. Child Poverty and Health Insurance

Social Outcomes (rates):	Poor	Health Insurance	Medicaid
	(1)	(2)	(3)
Demographic Group			
Age 6 to 10	-0.162	-0.189	-0.994***
	(0.120)	(0.414)	(0.303)
Age 10 to 15	-0.280	0.330	-0.769
	(0.184)	(0.530)	(0.473)
Age 16 to 20	-0.231	0.355	-0.216
	(0.228)	(0.486)	(0.361)
N	2121	1458	1458
First-Stage F statistic	21.290	4.980	4.980

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. 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 6. Mortality by Cause of Death.

Cause of Death	All	Suicide	Drug/Alcohol	Assault	Internal	Accident
	(1)	(2)	(3)	(4)	(5)	(6)
All CBSAs						
Mortality Rates	-38.867	9.649**	-6.595	0.053	-41.753	7.166
	(59.789)	(4.354)	(8.538)	(2.200)	(59.337)	(11.245)
N	13303	13303	13303	13303	13303	13303
First-Stage F statistic	155.171	155.171	155.171	155.171	155.171	155.171
ACS cities						
Mortality Rates	-25.861	-3.574	-25.141	0.363	-21.752	-1.737
	(68.358)	(5.897)	(24.440)	(5.857)	(67.372)	(17.669)
N	3113	3113	3113	3113	3113	3113
First-Stage F statistic	39.513	39.513	39.513	39.513	39.513	39.513

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) 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 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. DOD spending is instrumented with a Bartik shock. All variables are winsorized at the 1% and 99% levels. The middle panel presents regressions that are weighted by a CBSAs population as of 2000. 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 7. Mortality by Age

Age Category	All	0-14	15-24	25-44	45-65	65-99
	(1)	(2)	(3)	(4)	(5)	(6)
All CBSAs						
Mortality Rates	-38.867	29.667	16.230	36.532	-261.442***	-849.044**
	(59.789)	(25.250)	(41.076)	(30.892)	(82.152)	(320.744)
N	13303	13303	13303	13303	13303	13303
First-Stage F statistic	155.171	155.171	155.171	155.171	155.171	155.171
ACS cities						
Mortality Rates	-25.861	21.588	8.212	-43.271	-119.712*	-252.056
	(68.358)	(24.327)	(55.944)	(38.429)	(61.564)	(309.593)
N	3113	3113	3113	3113	3113	3113
First-Stage F statistic	39.513	39.513	39.513	39.513	39.513	39.513

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) 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 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. DOD spending is instrumented with a Bartik shock. Both the Bartik shock and DOD spending are winsorized at the 1% and 99% levels. The middle panel presents regressions that are weighted by a CBSAs population as of 2000. 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. Crime Rates

Crime Type:	Violent Crime Rate	Murder Rate	Aggravated Assault Rate	Rape Rate	Property Crime Rate	Robbery Rate	Burglary Rate	Larceny Rate	Vehicle Theft Rate	Arson Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
All CBSAs	98.509	0.381	91.831	-5.576	-111.316	17.58	46.343	-392.633	-175.965**	-3.84
	(97.262)	(3.08)	(91.839)	(20.098)	(718.046)	(17.89)	(241.842)	(426.821)	(82.325)	(11.411)
N	12891	12891	12891	12891	12891	12891	12891	12891	12891	12891
First-Stage F statistic	138.289	138.289	138.289	138.289	138.289	138.289	138.289	138.289	138.289	138.289
CBSAs in ACS	87.906	0.733	164.725	-41.885	-312.47	-9.476	-214.008	638.304	34.916	46.052
	(183.481)	(3.774)	(169.832)	(32.972)	(1333.57)	(44.295)	(371.54)	(1197.456)	(172.275)	(28.946)
N	3044	3044	3044	3044	3044	3044	3044	3044	3044	3044
First-Stage F statistic	40.065	40.065	40.065	40.065	40.065	40.065	40.065	40.065	40.065	40.065

Note: This table reports the effect of a percent increase in DOD spending (as a share of local earnings) 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. 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 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	0.557**	0.561***	0.427**	0.518***	0.216***	0.110***
	(0.249)	(0.082)	(0.197)	(0.070)	(0.061)	(0.032)
Education						
No Bachelors	0.548***	0.400***	0.712***	0.618***	0.245***	0.126***
	(0.161)	(0.064)	(0.220)	(0.065)	(0.073)	(0.036)
			0			
Bachelors	0.037	0.187***	0.309	0.279**	0.085	0.051**
	(0.132)	(0.065)	(0.238)	(0.133)	(0.051)	(0.024)
Age			0			
20-40	0.145	0.117	0.298	0.498***	0.273***	0.117***
	(0.147)	(0.088)	(0.268)	(0.115)	(0.074)	(0.043)
			0			
41-61	0.363**	0.380***	0.509**	0.558***	0.157**	0.099***
	(0.138)	(0.086)	(0.211)	(0.084)	(0.062)	(0.031)
			0			
62-70	0.037	0.049	0.037	0.450	0.037	0.106**
	(0.033)	(0.036)	(0.033)	(0.410)	(0.033)	(0.048)
Race			0			
White	0.513**	0.340***	0.503**	0.476***	0.220***	0.107***
	(0.217)	(0.085)	(0.208)	(0.083)	(0.058)	(0.027)
			0			
Black	0.092*	0.089***	0.724**	0.572***	0.002	0.166
	(0.048)	(0.025)	(0.352)	(0.198)	(0.203)	(0.123)
			0			
Hispanic	0.194**	0.125***	0.907	0.610***	0.097	0.103***
	(0.093)	(0.030)	(0.605)	(0.134)	(0.157)	(0.025)
Sex			0			
Male	0.387*	0.468***	0.449*	0.615***	0.270***	0.116***
	(0.203)	(0.070)	(0.227)	(0.084)	(0.078)	(0.030)
			0			
Female	0.176*	0.093**	0.393*	0.286***	0.140***	0.101***
	(0.090)	(0.042)	(0.212)	(0.099)	(0.047)	(0.036)
N	2541	2542	2541	2542	2541	2542
First-Stage F statistic	28.576	132.315	28.576	132.315	28.576	132.315

Note: This table reports the effect of increases in DOD spending (instrumented by the DOD Bartik shock) and earnings (instrumented by the traditional Bartick 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	Disabled	Multi-family home	Homeowner	Married	divorced	single parent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Demographic Group								
All	-0.094***	-0.123***	-0.006	0.011	-0.030	0.003	-0.022	-0.027
	(0.023)	(0.030)	(0.012)	(0.020)	(0.025)	(0.028)	(0.017)	(0.026)
Education								
No Bachelors	-0.128***	-0.148***	-0.005	-0.000	0.001	0.019	-0.021	-0.027
	(0.029)	(0.032)	(0.014)	(0.022)	(0.030)	(0.035)	(0.018)	(0.031)
Bachelors	0.030	-0.024	-0.002	0.049	-0.140***	-0.094**	-0.030	-0.024
	(0.031)	(0.043)	(0.020)	(0.040)	(0.041)	(0.044)	(0.023)	(0.015)
Age								
20-40	-0.135***	-0.170***	-0.010	0.019	-0.037	0.009	-0.033	-0.031
	(0.044)	(0.036)	(0.020)	(0.037)	(0.036)	(0.049)	(0.029)	(0.059)
41-61	-0.086***	-0.098**	-0.018	0.001	-0.033	0.007	-0.028	-0.023
	(0.022)	(0.037)	(0.019)	(0.034)	(0.029)	(0.031)	(0.020)	(0.016)
62-70	0.033	-0.030	0.045	0.055*	-0.015	-0.052	0.059	-0.038
	(0.042)	(0.052)	(0.049)	(0.030)	(0.041)	(0.065)	(0.050)	(0.035)
Race								
White	-0.030	-0.095**	-0.022	-0.006	-0.038	-0.012	-0.020	-0.023
	(0.031)	(0.037)	(0.021)	(0.020)	(0.030)	(0.043)	(0.026)	(0.024)
Black	-0.083	-0.196**	-0.011	-0.040	0.135*	0.179**	-0.103*	0.115*
	(0.092)	(0.077)	(0.084)	(0.043)	(0.078)	(0.084)	(0.059)	(0.064)
Hispanic	-0.301***	-0.294***	-0.008	0.120*	-0.098	0.043	-0.010	-0.139**
	(0.074)	(0.083)	(0.024)	(0.069)	(0.119)	(0.050)	(0.023)	(0.053)
Sex								
Male	-0.094***	-0.125***	-0.023*	0.024	-0.032	-0.018	-0.024	-0.008
	(0.022)	(0.040)	(0.014)	(0.024)	(0.033)	(0.033)	(0.031)	(0.018)
Female	-0.091***	-0.121***	0.013	-0.001	-0.029	0.022	-0.019	-0.047
	(0.029)	(0.028)	(0.019)	(0.023)	(0.033)	(0.029)	(0.018)	(0.043)
N	2542	2542	2542	2542	2542	2542	2542	2542
First-Stage F statistic	132.315	132.315	132.315	132.315	132.315	132.315	132.315	132.315

Note: This table reports the effect of a percent increase 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. 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 11. Social Outcomes by Demographic Group, General Demand Shock

Social Outcomes (rates):	Occupational Prestige	Transportation time to work	Health Insurance	Medicaid
	(1)	(2)	(3)	(4)
Demographic Group				
All	0.433	3.644*	0.118**	0.018
	(0.849)	(2.039)	(0.047)	(0.032)
Education				
No Bachelors	1.327**	3.002	0.137***	0.011
	(0.638)	(2.122)	(0.051)	(0.038)
Bachelors	-1.890*	5.451**	-0.006	0.077***
	(1.046)	(2.537)	(0.044)	(0.022)
Age				
20-40	1.522	3.120	0.206***	0.013
	(1.538)	(2.893)	(0.056)	(0.046)
41-61	-0.640	4.248	0.046	0.039
	(0.937)	(2.943)	(0.055)	(0.035)
62-70	-0.475	0.072	0.105*	-0.051
	(3.071)	(6.862)	(0.062)	(0.042)
Race				
White	-0.117	3.703*	0.156***	0.049
	(0.977)	(2.206)	(0.034)	(0.034)
Black	0.559	-0.329	0.111	-0.163**
	(3.317)	(3.750)	(0.133)	(0.075)
Hispanic	3.846*	3.175	0.099	0.012
	(1.940)	(4.941)	(0.067)	(0.083)
Sex				
Male	0.470	4.787	0.117**	0.019
	(1.552)	(3.054)	(0.046)	(0.032)
Female	0.384	1.863	0.115**	0.017
	(0.883)	(1.782)	(0.054)	(0.035)
N	2542	2542	1756	1756
First-Stage F statistic	132.315	132.315	127.806	127.806

Note: 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. The sample is limited to CBSA-years with at least 100 respondents for the given category. 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 12. Child Poverty and Health Insurance, General Demand Shock

Social Outcomes (rates):	Poor	Health Insurance	Medicaid
	(1)	(2)	(3)
Demographic Group			
Age 6 to 10	-0.199**	-0.051	-0.212
	(0.075)	(0.096)	(0.142)
Age 10 to 15	-0.176**	0.109	0.028
	(0.073)	(0.097)	(0.111)
Age 16 to 20	-0.176**	0.140	-0.064
	(0.078)	(0.098)	(0.117)
N	2081	1430	1430
First-Stage F statistic	109.188	313.127	313.127

Note: 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. 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 13. Mortality by Cause of Death, General Demand Shock

Cause of Death	All	Suicide	Drug/Alcohol	Assault	Internal	Accident
	(1)	(2)	(3)	(4)	(5)	(6)
All CBSAs						
Mortality Rates	109.521**	-3.240	12.689**	0.163	83.212**	25.892**
	(41.462)	(4.783)	(5.609)	(1.059)	(38.140)	(12.745)
N	14055	14055	14055	14055	14055	14055
First-Stage F statistic	187.206	187.206	187.206	187.206	187.206	187.206
ACS cities						
Mortality Rates	156.814***	-0.919	13.407	2.248	114.837***	36.987**
	(37.586)	(4.593)	(13.310)	(3.976)	(37.957)	(14.590)
N	3114	3114	3114	3114	3114	3114
First-Stage F statistic	175.559	175.559	175.559	175.559	175.559	175.559

Note: This table reports the effect of a percent increase in CBSA earnings (instrumented by a general demand shock) on death rates by cause 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 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 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.

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Table 14. Mortality by Age, General Demand Shock

Age Category	All	0-14	15-24	25-44	45-65	65-99
	(1)	(2)	(3)	(4)	(5)	(6)
All CBSAs						
Mortality Rates	109.521**	20.844	1.031	38.263	91.635*	531.819**
	(59.789)	(12.960)	(23.873)	(40.606)	(47.831)	(214.436)
N	14055	14055	14055	14055	14055	14055
First-Stage F statistic	187.206	187.206	187.206	187.206	187.206	187.206
ACS cities						
Mortality Rates	156.814***	16.861	88.682***	94.645*	137.701***	657.396**
	(37.586)	(25.354)	(29.536)	(52.085)	(49.878)	(265.464)
N	3114	3114	3114	3114	3114	3114
First-Stage F statistic	175.559	175.559	175.559	175.559	175.559	175.559

Note: This table reports the effect of a percent increase in CBSA earnings (instrumented by the general demand shock) 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 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. The middle panel presents regressions that are weighted by a CBSAs population as of 2000. 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 15. Crime rates, General Demand Shock.

Crime Type:	Violent Crime Rate	Murder Rate	Aggravated Assault Rate	Rape Rate	Property Crime Rate	Robbery Rate	Burglary Rate	Larceny Rate	Vehicle Theft Rate	Arson Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
All CBSAs	128.272*	2.275	115.828*	1.782	37.941	7.475	-139.985	-317.088	107.938**	7.006
	(69.454)	(2.078)	(59.821)	(11.735)	(353.491)	(11.446)	(104.374)	(227.913)	(44.326)	(8.357)
N	13610	13610	13610	13610	13610	13610	13610	13610	13610	13610
First-Stage F statistic	328.324	328.324	328.324	328.324	328.324	328.324	328.324	328.324	328.324	328.324
CBSAs in ACS	111.81	4.922	61.261	-10.593	60.974	49.587	-140.664	-495.062	186.866**	2.94
	(97.857)	(3.02)	(61.676)	(11.325)	(455.729)	(48.047)	(243.135)	(314.945)	(75.072)	(8.724)
N	3045	3045	3045	3045	3045	3045	3045	3045	3045	3045
First-Stage F statistic	265.704	265.704	265.704	265.704	265.704	265.704	265.704	265.704	265.704	265.704

Note: This table reports the effect of a percent increase in CBSA earnings (instrumented by a Bartik shock) on crime rates over a two-year time span. Earnings growth is instrumented with a traditional Bartik shock. All variables except the Bartik shock are winsorized at the 1% and 99% levels. 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 16. Employment Margin among Households without a Bachelors 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)
	-0.051***	-0.031***	-0.055***	0.001	0.010	0.014**	-0.006	0.779***
	(0.016)	(0.011)	(0.018)	(0.003)	(0.007)	(0.007)	(0.004)	(0.248)
N	2541	2541	2541	2541	2541	2541	2541	2541
First-Stage F statistic	28.576	28.576	28.576	28.576	28.576	28.576	28.576	28.576

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.

Table 17. Predicted Earnings and Employment of no-bachelor's based on Industry and City Composition of Demand Shocks

Prediction based on:	Industry composition		City Composition		Occupation composition		Total Effect	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Predicted Employment								
DOD shock	0.185***		0.251**		0.257***		0.459***	
	(0.061)		(0.094)		(0.081)		(0.132)	
General demand shock		0.126***		0.143***		0.162***		0.250***
		(0.029)		(0.039)		(0.035)		(0.056)
Panel B: Predicted Earnings								
DOD shock	0.320**		0.367*		0.380**		0.548***	
	(0.150)		(0.195)		(0.161)		(0.161)	
General demand shock		0.362***		0.392***		0.392***		0.460***
		(0.050)		(0.060)		(0.060)		(0.060)

Note: This table reports the effect DOD shocks and general demand shocks on prediceted 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 .Actual outcomes for no-bachelor's workers are in columns 7 and 8. All variables except the traditional 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.

Table 18: Industries and Occupations with strongest differential employment effect of DOD shocks among those with no bachelor's degree

Prediction based on:	Industries				Occupations			
	Construction		Manufacturing		Military		Production and Maintenance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Employment (no bachelor's)								
DOD shock	0.132**		0.124***		0.166**		0.108***	
	(0.062)		(0.040)		(0.076)		(0.037)	
General demand shock		0.067***		0.060***		-0.020		0.088***
		(0.020)		(0.022)		(0.028)		(0.026)
Panel B: Predicted Earnings (no bachelor's)								
DOD shock	0.154		0.248***		0.253		0.176**	
	(0.092)		(0.086)		(0.164)		(0.074)	
General demand shock		0.130***		0.063*		-0.009		0.138***
		(0.040)		(0.037)		(0.049)		(0.028)
N	1406	1406	1837	1837	284	284	2303	2304
First-Stage F statistic	8.68	130.94	19.93	97.53	44.63	42.33	97.24	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.

Table 19: 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	0.031	-0.126
Employment rate	0.152	0.081
Average home value	0.209	0.052
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 Table 1: Correlations between Demand Shocks and CBSA Characteristics

	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.