Excess Capacity and Heterogeneity in the Fiscal Multiplier: Evidence from the Obama Stimulus Package*

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Abstract

We estimate local multipliers from the ARRA (Obama stimulus) bill using cross-county variation in expenditure. We use within-state variation, and include other demographic controls as well as a predicted employment control using an industry shift-share measure. We estimate a peak annualized employment multiplier of 1 job-year per $100K spent per county resident and a 2 year average multiplier of 0.75. We do not find nonlinearity in the marginal treatment effect of a dollar in stimulus spending, but do find strong evidence of heterogeneous treatment effects: the employment response is much greater in counties with greater excess capacity. In above median excess capacity counties, the 2 year multiplier is 1.38. The level of spatial aggregation of the estimation has a large impact on estimation. Spatially aggregated multipliers rise to as high as 3.28 jobs per $100K spent per resident. Our smaller county-level estimates imply that ARRA created 1.97 million jobs. A spatially optimal stimulus, targeted to high excess capacity counties, would have created 83% more (3.60 million) jobs. Our cross-sectional results are consistent with macro-economic models with state-dependent fiscal multipliers. In addition, they also suggest that the state dependency arises from the presence of excess capacity in the economy during deep recessions, and not solely due to the zero lower bound on nominal interest rate. Our evidence is also inconsistent with equilibrium models where a positive multiplier arises from a drop in wealth resulting in an increase in the labor supply.

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1 Introduction

> "We do not have a good measure of the effects of fiscal policy in a recession because the methods that we use to estimate the effects of fiscal policy—both those using the observed outcomes following different policies in aggregate data and those studying counterfactuals in fitted model economies—almost entirely ignore the state of the economy and estimate "the" government multiplier, which is presumably a weighted average of the one we care about—the multiplier in a recession—and one we care less about—the multiplier in an expansion. Notable exceptions to this general claim suggest this difference is potentially large. Our lack of knowledge stems significantly from the focus on linear dynamics: vector autoregressions and linearized (or close-to-linear) dynamic stochastic general equilibrium (DSGE) models. Our lack of knowledge also reflects a lack of data: deep recessions are few and nonlinearities hard to measure." (Parker, 2011).

Empirical estimates of multipliers vary tremendously. Some are in the 0-0.5 range (Barro and Redlick 2011, Conley and Dupor 2013); others are near to 1 (Ramey 2011) and yet others are well above 1 (Blanchard and Perotti 2002). Theoretical estimates also vary from near zero (Baxter and King 1994) to well over 1 and sometimes even over 2 (Christiano, Eichenbaum and Rebelo 2011; Woodford 2011). Part of the reason for the disagreement, as Parker (2011) points out, may rely on the heterogeneity in the multiplier as a function of the degree of slack in the economy. However, what is of greatest interest for macroeconomic stabilization purposes is the multiplier during recession. Unfortunately, estimation of the multiplier as a function of excess capacity has been elusive. The reasons for this are three-fold. First, at a country level, the identification must exclusively rely on time series variation. However, the timing of expenditures is correlated with the business cycle itself and thus expenditures are confounded by the state of the economy. Second, there is a limited sample size from which to perform statistical inference. Some recent efforts have attempted to use a narrative approach in order to estimate fiscal multipliers (Romer and Romer 2010). However, this approach is plagued by small sample sizes, which is particularly problematic when investigating heterogeneous effects of fiscal stimulus by the amount of slack in the economy. Third, when excess capacity is high, interest rates tend to be low and fiscal expenditure more effective. Thus, the interest rate confounds the estimate of the heterogeneity in the multiplier. This is particularly important when excess capacity is quite large because the interest rate on government debt is likely to be close to the zero lower bound on nominal interest rates and a large recent literature argues that multipliers are much higher when policy interest rates are sticky downwards (Christiano, Eichenbaum and Rebelo 2011; Woodford 2011).

Beginning with Auerbach and Gorodnichenko (2012), a burgeoning literature has attempted to estimate the heterogeneity in the multiplier using time series variation (Auerbach and Gorodnichenko 2012, Auerbach and Gorodnichenko 2013, Baum *et al.* 2012, Clemens and Miron 2012, Fazzari *et al.* 2013, Ramey 2013, Mittnik and Semmler 2012, Semmler and Semmler 2013). However, it is focused on the difference in the multiplier at the zero lower bound in nominal interest rates. One recent working paper (Ramey and Zubairy, 2014) also uses time series variation and employs vector autoregression methods to estimate the differential multiplier in recessions versus booms. They, however, try to separate out the impact of being at the zero lower bound in interest rates versus the impact of having high excess capacity. They find no differential at the zero lower bound or at high degrees of excess capacity. Their multiplier estimates lie between 0.6 and 1.0 throughout during recessions and booms and during periods of high interest rates as well as low interest rates. However, the time series based estimation is not well identified. Moreover, estimates are not stable to timing mis-specification as well as method of construction of impulse response functions (Ramey and Zubairy, 2014).

An alternative approach to multiplier estimation using national time series is estimation using intranational variation in fiscal expenditures over time to estimate multipliers (Chodorow-Reich *et al* 2012, Conley and Dupor 2013, Feyrer and Sacerdote 2010, Moretti 2011, Nakamura and Steinsson 2013, Serrato and Wingender 2011, Shoag 2013). There is even a small set of papers estimating local multipliers using variation in spending across areas in the American Recovery and Reinvestment Act or 'Obama Stimulus Bill' (Chodorow-Reich *et al.* 2012, Conley and Dupor 2013, Feyrer and Sacerdote 2011).

Our paper uses cross-county variation in expenditure during the ARRA to estimate fiscal multipliers. We semi-parametrically estimate the multiplier as a non-parametric function of the degree of excess capacity. We show that our estimates of the fiscal multiplier satisfy time placebos, are robust to inclusion of Bartik controls for evolution of employment based upon the sectoral composition of local employment and national trends in employment by sector. Moreover, Boone, Dube and Kaplan (2014) show that allocation of funds in ARRA was not correlated with the unemployment rate[1]. Our paper is closest in topic to Ramey and Zubairy (2014) in that we estimate the heterogeneity in the multiplier as a function of excess capacity, partialing out interest rate effects. It is closest to Feyrer and Sacerdote (2011) in terms of methods. In contrast to Ramey and Zubairy (2014), we find a three-fold increase in the fiscal multiplier in the 2-4 percentage point excess capacity range relative to the 0-2 percentage point excess capacity range. One recent paper (Michaillat 2014) shows that when unemployment is higher, public sector employment crowds out fewer matches and thus the multiplier is countercyclical. Our methods are unable to distinguish whether the multiplier is higher in high excess capacity areas because in those areas there is more idle capital or whether there is less crowdout of matching efficiency from employment programs.

Of course, as with the rest of the local multiplier literature, our use of spatial variation comes at a cost. We do not directly estimate a national fiscal multiplier. Moreover, because expenditures are financed through federal rather than through local taxes, what we are estimating is more akin to a transfer multiplier in an open economy as opposed to a national fiscal multiplier. On the positive side, estimating locally allows us to say something about the channels through which the multiplier works (Farhi and Werning, 2013). One standard mechanism through which the multiplier is thought to work is that reduced wealth from the increased debt obligations needed to finance the fiscal expansion increases labor supply (Baxter and King, 1993). However, this channel should be shut off in our local estimates as long as those receiving more and less funds should not pay on average higher taxes from the increased expenditure. Since we show that our results are robust to controlling for deciles of average tax obligations per capita, we would expect that to the degree that public and private expenditures are substitutes, the counties with higher public expenditure should have lower employment. In fact, we find the opposite to be true.

In addition to providing an estimate of the multiplier as a function of excess capacity, our paper

[1] These results are at the Congressional District level but they hold at the county level as well.

also adds to the literature in a few other ways. First, we try to better bridge the gap between the local multiplier and national multiplier literature by showing the effect of spatial aggregation. We find much larger estimates when we estimate across larger spatial regions. This is in contrast to Nakamura and Steinsson (2012) who find the opposite when going from the state level to the national level. We interpret the difference as due to lowering the degree of spatial measurement error and estimating over a less open (more aggregated) economy. However, we find little evidence that the greater multipliers are driven by a greater degree of endogeneity of expenditure at a higher level of spatial aggregation.

Second, we provide more evidence on the dynamics of expenditure, computing non-parametric impulse responses similar to what is estimated in the Vector Autoregression literature (Blanchard and Perotti 2011, Ramey 2011, Ramey 2013). Allowing more non-parametric estimation of impulse responses to fiscal expenditure is beneficial because it allows for arbitrary non-linearities in the time path of the effect of expenditure. This is a particularly important contribution given Ramey's (2013) demonstration of the sensitivity of the multiplier to the method of constructing impulse response functions in non-linear models such as the VAR models pioneered by Auerbach and Gorodnichenko (2012) to estimate heterogeneity in the multiplier over the business cycle.

Finally, we point out that, given the heterogeneity which we find in the fiscal multiplier, our estimates reflect an average multiplier which is averaged over each dollar spent. We separate out the economic and political aspects of the multiplier by computing the multiplier for a politically unconstrained government which optimally targets federal dollars to the highest multiplier areas. In other words, in addition to estimating the actual multiplier, we compute what the multiplier would have been had it been optimally spent based upon the information that the government would have had access to at the time.

In section 2 of this paper, we present our empirical methodology. In section 3, we describe the data that we use in estimating our results. In section 4, we present our results and finally, in section 5, we conclude.

2 Empirical methodology

To estimate the effects of fiscal stimulus we regress county-level quarterly employment and earnings indicators on a quarterly measure of county-level stimulus and additional controls. We primarily use fixed effect methods. To explore heterogeneity of the multiplier in the extent of stimulus, we add a quadratic term in the amount of stimulus. To explore heterogeneity of the effects of stimulus across counties of varying excess capacity, we estimate over split samples as well as estimate using the semiparametric smooth coefficient estimator proposed by Li et al. (2002).

2.1 Primary specifications

Our primary regressions are county-level panel and county-level fixed effects regressions[2]. Letting i,s, and t denote, respectively, geographic region[3], state or division, and quarterly indices, the panel specification

$$Y_{it} = \alpha + \beta S_{it} + \gamma B_{it} + F_{st} + tD_{it}^{'}\Delta + \epsilon_{it} \tag{1}$$

regresses quarterly employment or earnings per capita outcomes Y_{it} on stimulus per capita S_{it}. Controls in this specification include a Bartik shift-share control for predicted employment based upon industrial shares of employment in county i in 2008 Quarter 1, state-time-specific fixed effects F_{st} and demographic controls which vary over time and across counties denoted byD_{it}. Demographic controls are Census 2000 estimates of percents black, Hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. These are all multiplied by linear time trends. In some specifications, the fixed effects are at the divisionXtime level or at just the pure time level: F_t.

[2]We also ran long difference regressions of the change in the employment to population ratio on stimulus from 2008Q3-2011Q3. These results look similar to the fixed effects results which use the full panel. Therefore, we report only the panel results. Long difference regression results are available from the authors upon request.

[3]For most specifications, the geographical region is county. However, in some specifications, regions are a higher level of spatial aggregation than county.

2.2 Time Aggregation

We also present dynamic estimates including lagged effects of stimulus. We present cumulated effects over time from our dynamic regressions. In particular, we estimate:

$$Y_{it} = \alpha + \sum_{k=0}^{8} \beta_k S_{it-k} + \gamma B_{it} + F_{st} + tD_{it}^{'}\Delta + \epsilon_{it} \tag{2}$$

We then report contemporaenous effects: β_0, impulse response incorporating contemporaneous effects and two quarter lagged effects $\sum_{k=0}^{2} \beta_k$, and other impulse responses cumulated up to two years of lagged effects: $\sum_{k=0}^{8} \beta_k$. Stimulus awards begin to flow in mid-2009. Our panel specification begins in 2008q1 and ends in 2011q3.

2.3 Heterogeneous effects

We also consider specifications where the effects of stimulus depend on the extent of stimulus. Denoting all right-hand side variables except for stimulus as the vector $\mathbf{Z}_{it}$, then we estimate

$$Y_{it} = \alpha + \beta_1 S_{it} + \beta_2 S_{it}^2 + \gamma B_{it} + F_{st} + tD_{it}^{'}\Delta + \epsilon_{it} \tag{3}$$

This differs from our main specification only by the addition of a quadratic term on the amount of stimulus spent in a quarter within a county.

We additionally examine how the extent of county-level excess capacity alters the effects of stimulus. As we discuss in section 3, our measure of excess capacity E_i is based on pre-period industry shares and is constant for each county. First we break down counties into above median excess capacity and below median excess capacity and estimate (1) separately for above-median and below-median excess capacity counties. We then consider a more flexible non-linear interaction or semi-parametric smooth coefficient model:

$$Y_{it} = g(E_i) + (S_{it}, \mathbf{Z}_{it})'\mathbf{h}(E_i) + \epsilon_{it} \tag{4}$$

where the scalar g and vector $\mathbf{h}$ are unspecified functions of excess capacity. We estimate equation 4 at each excess capacity percentile e^p by linear regressions of Y_{it} on S_{it} and $\mathbf{Z}_{it}$ for observations whose population-weighted kernel-based distance is near e^p, as suggested by Li et al. (2002). We county-cluster bootstrap these estimates to conduct inference.

2.4 Spatial Aggregation

In our final empirical specification, we aggregate spatially to estimate multipliers in economic geographic units with a lower degree of openness and a higher percentage of expenditures within the geographic area. For each county, we add employment, stimulus expenditure and demographics for all counties whose population centroid is within a given radius of an observation's base county. We consider 30, 60, 90 and 120 mile radii specifications. We then estimate equation (1) with each county's data replaced by the aggregated data. Of course, this induces mechanical serial correlation. We correct for this autocorrelation by using our knowledge of the spatial dependency under the maintained assumption that underlying county data is not inherently spatially correlated. In particular, we use Conley (1999) standard errors in a panel context, which allows arbitrary serial temporal and spatial autocorrelation of the error term for all counties whose centroids are within 2*D miles of each other, when we consider aggregation by D miles. Thus, the selector matrix is not block diagonal as it would be in the clustering case.

3 Data

The primary data sources are the Quarterly Census of Employment and Wages (QCEW) from the Bureau of Labor Statistics (BLS)[4] and the recipient-reported American Recovery and Reinvestment Act award data from recovery.gov (Stimulus).[5] Additionally we utilize a variety of other demographic and geographic data as control variables and tools to implement spatially-based regressions[6].

[4] `ftp://ftp.bls.gov/pub/special.requests/cew/`

[5] `http://www.recovery.gov/FAQ/Pages/DownloadCenter.aspx`

[6] Sensitivity of our findings to the starting and ending dates of our panel are available from the authors upon request.

3.1 Outcomes

The key outcomes on the left hand side are employment and earnings (wage bill) per capita. Quarterly earnings data are taken directly from the county-level BLS QCEW, and we calculate quarterly employment data as the average of monthly employment reported that quarter. These data are not seasonally adjusted. To calculate quarterly population levels we use the annual July 1 intercensal estimates published by the US Census Bureau [7] as our third quarter population estimates and interpolate estimates among quarters assuming a quarterly geometric growth rate.

3.2 Treatment

The recipient-reported stimulus award data from recovery.gov. These data are a panel of individual contracts, grants, and loans reported quarterly beginning in 2009q4 through the present. Award data is also reported for a single 2009q2-2009q3 period, which we we assign to 2009q3. We end the panel in 2011q3..

Recipient-reported data is available for prime awardee recipients and sub-recipients who receive more than $25,000. Prime awardees report the overall award amount and sub-recipients report subawards. We construct dataset P using prime awards and their award amount. We additionally construct dataset $\tilde{P}$ using subrecipients and their subawards, as well as prime recipients and their residual award after subtracting all subawards. Prime awards also report a project status variable (not started, less-than-half complete, more-than-half complete, and complete) which we use to adjust award amounts with the intention of more accurately measuring actual stimulus spent (0%, 25%, 75%, or 100%-complete). Subawards do not report project status, so we assign prime award status to subrecipients.

In the raw recovery data project status can sometimes behave nonmonotonically. Awards may regress in status, or awards fail to be reported at all during some periods. Reported status-unadjusted award amounts may furthermore change from quarter-to-quarter, presumably due either to reporting error or to total award amounts legitimately changing over time. For example, it is not necessarily the case that the status-unadjusted total award amount for a prime recipient reported in 2009q3 will be

[7] `http://www.census.gov/popest/`

the same amount reported in 2011q3. One reasonable restriction is to create a dataset R excluding awards with award status regressions and reporting breaks, and using only the most recent (2011q3) reported unadjusted award amount and recipient geography. We also create a dataset $\tilde{R}$ that does not impose these time restrictions: it does discriminate on status or reporting breaks, and it uses the contemporaneous status-unadjusted award amount and geography reported during that quarter.

Since not all award, geographic, and status data is reported accurately, the most appropriate dataset is unclear: using prime-only status-adjusted awards may be more accurate in terms of status-assignment and geographic information for some spending, but such data ignore subaward flows to other counties. For this paper we mostly report results using prime-only status-adjusted stimulus and dataset PR, but estimates using the dataset including subawards without time restrictions $\tilde{P}\tilde{R}$ are available from the authors upon request.

For a given award in the recipient-reported data, monies awarded and place of performance zip code are available for prime recipients and subrecipients. We assign zip codes to counties using the MABLE/Geocorr2K Census 2000 zip code-to-county crosswalk. [8]

3.3 Excess Capacity

We also create an excess capacity variable so that we can estimate the differential impact of stimulus in high versus low excess capacity areas. We compute excess capacity using the employment to population ratio in a county. In particular, we take the negative of the maximum difference in the employment to population ratio for a county over the period between the first quarter of 2009 and the third quarter of 2011 and between the first quarter of 2008 and the first quarter of 2009. Intuitively, this gives us the difference between employment to population ratio after the onset of the Great Recession relative to before the Great Recession.

[8]When a zip code in multiple counties, we allocate awards based on population shares using MABLE/Geocorr2K Census 2000 population allocation factors.

3.4 Controls

Because our identification using panel data relies on county fixed effects, we do not use lagged outcomes as controls for fear of biasing our OLS estimates. Instead we use as a control predicted employment and earnings, using pre-period county-level industry shares and contemporaneous national level employment to predict actual employment in the manner of Bartik (1991). Specifically, we first calculate county-level average overs the years 2006 and 2007 employment (earnings) shares of national employment (earnings) at the three-digit NAICS level. Then we multiply these county-NAICS shares by contemporaneous national three-digit NAICS employment. We sum the resulting county-NAICS series over NAICS categories to form a single, time-varying predicted employment (earnings) series for each county.

In addition to Bartik-predicted outcomes and geographic and time dummies, we also employ a variety of pre-period demographic controls in the hope of increasing the precision of estimates and also to account for some of the selection bias remaining in stimulus assignment. We use US 2000 Census county-level estimates of percents black, Hispanic, urban, and under poverty, as well as county-level median income.[9] Because of the central role of housing wealth in the most recent recession, we also use two county-level housing variables derived from the loan origination reported under the Home Mortgage Disclosure Act (HMDA): the 2006 average value of home purchase loans, and the 2006 total of all HMDA loans divided by county population.[10]

All of the county-level demographic and housing controls are time-invariant and are used in the long-difference regressions. In the regressions with panel data and county fixed effects, we simply interact these demographic controls by time dummies. For spatially-based regressions we calculate neighbors' and neighbor distance bins by defining the distance between two counties as the distance between their population-weighted centroids.[11] The population-weighted centroids are taken from the Missouri Census Data Center MABLE/Geocorr2K database [12], which uses Census 2000 geography and population data.

[9] `http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/13402/ascii`

[10] `http://www.ffiec.gov/hmda/hmdaproducts.htm`

[11] We implement the distance calculation in Stata using Kenneth L. Simons' `circnum` command: `http://homepages.rpi.edu/~simonk/technical.html`.

[12] `http://mcdc2.missouri.edu/websas/geocorr2k.html`

4 Results

First, however, we show that money was spent continuously throughout time. The percent of status-adjustment stimulus money spent is roughly linear in time up to 2013 as is the average dollar amount per capita spent. Moreover, we see in Figure 2 that the amount spent was relatively randomly distributed across the United States. Looking at the shaded map of amount spent by county, we see no obvious spatial patterns of expenditure. To effectively use the variation in expenditure over both space and time, we use a panel approach at the county and quarterXyear level.

4.1 Own-county Multipliers

We present our baseline estimates of the contemporaneous own-county impact of stimulus in Table 1. These estimates are broken down into two super-columns. The left super-column contains estimates of the impact of stimulus on own-county employment and the right super-column contains estimates of stimulus on the own-county wage bill. In the first row, we show static estimates of current stimulus expenditures on employment in the following quarter. We show five separate specifications in each super column. In our first column, we include only time fixed effects at the quarterXyear level. In column 2, we replace the time fixed effects with divisionXtime fixed effects. In column 3, we replace the divisionXtime fixed effects with state fixed effects. In column 4, we add Bartik predicted employment to population controls. Finally, in column five, we show our preferred specification where with stateXtime fixed effects, Bartik predicted employment to population controls and demographic controls. Our estimates range in magnitude from 0.11 and 0.25. The three estimates that are equal to or above 0.20 are statistically significant at the 10% level or lower. The highest of these estimates is the specification with stateXtime effects as well as controls for the predicted employment to population ratio and demographics. It is 0.25 and significant at below the 1% level. A 0.25 coefficient can be interpreted as saying that an increase in stimulus of 100K per resident increases employment by 0.25 more jobs per resident in the initial quarter; annualized, this corresponds to one job per year. Aggregated over time by summing up lagged effects of stimulus, the completed number of extra employees per resident of a county from an extra $100K per person is 0.479 over 1.5 years and 0.755 over 2 years. Both estimates are statistically significant at below a 1% of significance. The contemporaneous wage bill estimates are substantially smaller and are, with the exception of the full covariate specification in column five, statistically insignificant at conventional levels. The time-aggregated wage bill multipliers are substantially larger. In quarter six, the estimates peak at 0.42 for the full covariates specification. Note that if the average worker hired in the local economy from an injection of stimulus makes $40K in compensation, then a $100K expenditure will yield .42*40,000*1.5=.24 extra dollars over 1.5 years.

We note that our own-county estimates are small but are larger than the estimates in Feyrer and Sacerdote (2011). This is mostly due to status adjustment and restriction to prime awards. Without these adjustments, our estimates are quite similar to those by Feyrer and Sacerdote (2011). We thus attribute Feyrer and Sacerdote's small estimates to measurement error in the timing of stimulus and to spatial measurement error in the sub-prime awards. Feyrer and Sacerdote (2011) find significantly larger estimates at the state level, consistent with a significant impact of spatial measurement error on estimation. We discuss this more in section (4.6) when we discuss spatial aggregation.

The sign of our estimates is informative and useful in better understanding the channels through which stimulus worked. It is important to note that we are estimating the impact of expenditures holding fixed tax payments as long as tax payments are not correlated with stimulus expenditures. We empirically estimate but do not report[13] estimates controlling for deciles of tax payments per capita. Thus, we show that our estimates do not reflect differences in future potential tax burden across counties but rather differences in expenditures. Different from traditional national estimates, our local estimates thus effectively hold expected future tax payments constant. Therefore, net wealth increases are at least weakly larger in the areas which receive stimulus. Since, in the baseline macro model, an increase in wealth should reduce rather than increase labor supply, the fact that we estimate positive coefficients indicates that the multiplier is not likely through the wealth-labor supply channel. The leading alternative channel is the Keynesian demand-side channel.

[13] Estimates are available from the authors upon request.

4.2 Heterogeneity by Excess Capacity

In Table 2, we present our first estimates of heterogeneity by the degree of excess capacity. There is substantial heterogeneity in the multiplier by degree of excess capacity. The average stimulus effects reported in section 4.1 are entirely due to the effect in above median excess capacity areas. We base our estimation off of our most saturated specification from Table 1, with stateXtime fixed effects, Bartik-predicted employment to population and demographic controls. In below median excess capacity regions, the estimated effects are statistically insignificant and very close to zero at all levels of time aggregation. In the high excess capacity areas, the contemporaneous flow multiplier is 0.45 and rises, mostly monotonically in the number of quarters over which we aggregate the multiplier, peaking at 1.38 job-years over 2 years per $100K spent. The large differences between the 6 quarter and 8 quarter specifications are likely due to the fact that the 8th lag is identified off of only one quarter of data and moreover only a small set of counties had a non-zero stimulus expenditures in 2009Q3.

The wage bill multipliers are more volatile. From flow estimates to six quarter time-aggregated estimates, the effects are positive and significant ranging from a 0.05 flow effect to 0.54 cumulated over 6 quarters. By contrast, the wage bill multipliers are generally statistically insignificant and below 0.05. However, at 2 years time aggregation, the low excess capacity wage bill multipliers rise to a statistically significant 0.13 and in the high excess capacity counties, the estimates drop substantially to -0.20 though statistically insignificant.

In Figure 3, we present semi-pararmetric estimates of the effect of stimulus on employment and the wage bill by excess capacity. Both the employment and wage bill estimates are close to weakly monotonic in excess capacity. The employment effects are stable around zero until reaching an excess capacity of one. Then, the multiplier rises to approximately 0.1 at an excess capacity of 0.02. The multiplier rises more quickly between excess capacities of 2 and 3 percentage points, peaking at 0.5 just after reaching an excess capacity of four. The shape of the semi-parametric plot of the wage bill multiplier is quite similar, peaking at an excess capacity of approximately 0.08 with an excess capacity. The standard errors are bootstrapped and are much larger at high levels of excess capacity due to the smaller number of counties with very high unemployment, even during the Great Recession.

4.3 Heterogeneity in extent of stimulus and across excess capacity

The main identification concern we have is that places which got more stimulus were more likely to experience recovery for other reasons and that they received money precisely when they were about to recover. However, from Table 1, we see that adding predicted employment to population ratio controls actually increases rather than decreases the size of the estimates, suggesting that, if anything, more money was spent in lower unemployment areas than in higher unemployment areas. In Table 3, we furthermore show that the stimulus effects were concentrated in periods of time when money was being spent and in the high excess capacity areas. We replace contemporaneous stimulus in our baseline specification presented in Table 1 with a set of complete time dummies multiplied by the amount of total stimulus allocated to a county. We find that with the exception of a small effect which is significant at only the 10% level in 2001Q1, the statistically insignificant effects are 2009Q3 to 2011Q3 exclusive of 2010Q2 and 2010Q3. However, the coefficient on 2010Q3 is actually higher than many of our statistically significant results. In contrast, in the pre-ARRA period there is no statistical significance, not even after the beginning of the crisis. We do not see any effects at all in the low excess capacity period. Unfortunately the wage bill effects are more mixed.

4.4 Non-Linear Impacts of Stimulus Funds

One possible explanation for the heterogeneity in the multiplier between high excess capacity regions and low excess capacity regions is that more money was spent in high excess capacity regions and the multiplier increases with the amount spent. In Table 4, we show that this does not explain our heterogeneity results. In particular, we regress the employment to population ratio on the amount spent in county c at time t on stateXtime fixed effects, Bartik predicted county employment to population, demographic controls, the amount of stimulus spent in the county at time t and the amount of stimulus in the county at that time squared. We do this first for employment and then for the wage bill.

The coefficient on the linear term is 0.32 and is significant at below a 1% level of confidence. The coefficient on the quadratic term is -0.90 but the standard error is -0.75 and thus the coefficient is not significant even at a 10% level of confidence. The quadratic term is difficult to statistically identify due to the skewed distribution of awards. A small number of counties received very large awards and those awards lead to very high standard errors on the quadratic term. The estimated decreasing returns to fiscal expenditure is quite small beyond being statistically significant. It implies that the multiplier declines to zero at $35.6 thousand of expenditure per resident. This is roughly equivalent to a stimulus size of 70% of GDP. However, we do not find much evidence that stimulus was more effective per dollar the larger the amount spent.

We then break down our non-linear estimates of the impact of stimulus into effects in below median excess capacity and above median excess capacity counties.The results for the low excess capacity counties echo the linear estimates. Both the linear and quadratic terms for the impact of stimulus in below median excess capacity regions are small and statistically insignificant. The linear coefficient for the high excess capacity counties is approximately double that in the full sample. It is 0.62. The coefficient on the quadratic term is -3.11. Again, the estimated degree of decreasing returns is small. The multiplier is driven to zero after $20K of expenditures per capita or a stimulus size of roughly 40% of GDP. However, in the high excess capacity counties, the standard error is more than 10 times that for the linear term and so the coefficient is far from statistically significant.

In all three cases, the linear term is not substantially different with a quadratic term compared to without a quadratic term. Moreover, since the quadratic term is always negative, we see no evidence that spending more raises the multiplier.

The wage bill multipliers numbers have uniformly statistically significant positive linear coefficients without a quadratic term. Adding a quadratic term increases the standard errors on the linear term by a factor of 2 to 3 depending upon the sample. However, the coefficients on the linear terms stay relatively constant. None of the quadratic terms are statistically significant at a conventional level and only one of the linear term coefficients is statistically significant and just at a 10% level. Interestingly, the coefficient which is statistically significant is for the low excess capacity counties and the coefficient is substantially lower than for either the full sample or the high excess capacity counties. However, the standard error is also much lower, thus explaining it's statistical significance. Overall, we see no evidence of increasing returns to fiscal expenditures and weak evidence of decreasing returns to fiscal expenditures.

4.5 Sector Specific Multipliers

As evidence that our local multipliers are picking up demand-side effects, we show how fiscal expenditure impacts on employment and wages vary by sectors of the local economy. First, we break down impacts into effects on public versus private employment and wage bill. Then, we further break down impacts on the private sector into services, goods production and construction. Both in our employment estimates and our wage bill estimates, we find that the large majority of the increase is in the private sector. In fact, in our baseline estimates, the point estimate of the impact on public sector employment is negative, small and statistically insignificant at conventional levels.

Approximately 80% of the private sector employment increases are concentrated in services and construction. This is consistent with demand side channels for fiscal stimulus since consumer demand in an area should only impact non-tradables in that area. There is a small, positive and statistically significant impact on employment in the goods production sectors. One hundred thousand more dollars of expenditures in a county is associated with 0.056 more job-years in goods industries. This probably is due to direct employment effects of contracts. Money spent on automobile employment programs went, in part, to hiring individuals in the automobile construction industry in the county that received money. As we see, however, a very small fraction of the increase from local expenditures was in the tradable industries.

The results on the wage bill are mostly qualitatively similar though lower in magnitude and with a lower number of statistically significant coefficients. The impact on the private sector wage bill is positive and statistically significant at a 10% level or lower even in the low excess capacity counties. In the high excess capacity regions, a $100K increase in fiscal expenditures per person is associated with an increase in the wage bill of $4,400. These results are statistically significant from zero at a 5% level of confidence. Wage bill coefficients are positive and statistically significant at a 10% level or lower in every category (private, public, construction, services, goods production) except goods production. However, the public sector coefficients are generally an order of magnitude below the private sector ones. The construction sector wage bill results are decently strong with 5% significance or lower overall as well as in high excess capacity counties.

4.6 Geographical Aggregation

We next explore the estimated multiplier at lower and higher degrees of geographical aggregation. This is important for two reasons. First, we wish to relate our local multipliers to national multipliers. Second, we want to figure out what an appropriate level for geographic aggregation.

Cross-sectional variation inherently omits the net impact of fiscal expenditure, taking into account the endogenous response of monetary policy. As pointed out in Nakamura and Steinsson (2014), this means that estimations are done with a fixed interest rate, which replicates the impact of fiscal expenditure at the zero lower bound where monetary policy is largely absent. Of course, if we aggregate up to the national level, then we rely upon pure time-series variation in which the effect of monetary policy is present.

In choosing a level of spatial aggregation, we risk greater endogeneity at higher levels of spatial aggregation.We have lesser ability to reject differential trends in state level differences in expenditures than at the county level. On the other hand, in the presence of spatial measurement error where the degree of error is positively correlated with geographic proximity, aggregation can reduce attenuation bias. Finally, as shown in Farhi and Werning (2014), multipliers are larger when a larger share of goods is non-tradable since increased expenditures are more likely to be spent within the economy. Empirically, more spatially aggregated units are more likely to be closed.

Spatial aggregation has a significant impact on the magnitude of our estimates. Consistent with the county-level results from Feyrer and Sacerdote (2011) who find no significant effect of stimulus on employment at the county level but do find moderately large sized effects at the state level, we find that spatial aggregation monotonically increases the estimated multiplier from the county level on up. We consider 4 levels of aggregation. First, we consider geographical units incorporating all counties whose population centroids lie within a 30 mile radius of the population centroid of the given observation's base county. We then further aggregate to 60, 90 and finally 120 mile radii. We estimate equation (1) with aggregated units and correct, as noted earlier, for the serial correlation across observations due, at least in part, to county data being included in more than one region's observations. We show static estimates as well as dynamic impulse responses to stimulus.

All estimates are statistically significant at the 10% level or lower and a majority are statistically significant at lower than a 1% level of confidence. Estimates are strictly monotonically increasing in the level of spatial aggregation independent of the number of quarters of the impulse response we consider. The employment multiplier increases roughly at the rate of 0.2-0.3 jobs per unit of time for every 30 mile radius increase. The numbers are somewhat larger for cumulative impulse responses of 2 years than for shorter time window impulse responses. In unreported regressions, we looked at leads of the effect of stimulus at higher levels of spatial aggregation and found no systematic relationship between stimulus expenditures and pre-stimulus employment. Thus, we believe that the effects of spatial aggregation are due either to reduced measurement error or to the fact that demand effects from stimulus are more likely to accrue to a larger geographical area than a smaller one.

The employment effects at 8 quarters are substantially larger, particularly for the 2 year time aggregation. The employment multiplier for a 120 miles catchment area more than doubles from 1.33 at 6 quarters to 3.28 at 8 quarters. The 8 quarter lag estimate at 120 miles aggregation is quite similar to the instrumental variables estimates in Chodorow-Reich *et al.* (2013) as well as the Shoag (2012) estimates during periods of high unemployment. However, the fact that our estimates at 8 quarters are much larger than at 6 quarters time aggregation gives us pause. In fact, at all levels of spatial aggregation beyond the 30 mile catchment area, the multipliers more than double going from 6 to 8 quarters. As discussed earlier, this is largely due to the 8th lag being noisily measured given that it is based upon only one quarter of data. The standard errors almost double for the aggregation incorporating up to the 8th lag compared to the time aggregation incorporating up to the 6th lag. This is quite surprising given that the additional lags being added to the cumulative stimulative effect are 1/5 of the coefficients being added up.

4.7 Effectiveness of an optimally allocated stimulus

In this section we compute the number of job-years created from the contracts, grants and loans portion of the Obama stimulus bill. The contracts, grants and loans totaled approximately 1/3 of total stimulus expenditures. We also compute the number of job-years that would have been created had the money been spent solely in above median excess capacity counties.

Our baseline estimates suggest a multiplier of 0.76 jobs per $100,000 spent. Since $261 billion were spent on the contracts, grants and loans portion of the ARRA bill, that leaves us with 1.97 million jobs created. As shown in Boone, Dube and Kaplan (2014, Brookings), the amount of stimulus provided to an area is very weakly correlated with the unemployment rate in the area. Under the assumption that the multiplier is the same in all regions, this allocation would maximize the efficacy of the stimulus. However, in this paper, we have shown that the multiplier in high excess capacity regions is many times greater than the multiplier in low excess capacity areas. This poor targeting of high unemployment areas lowers the average multiplier that we estimate.

We now compute the number of jobs created if the stimulus had been optimally spatially targeted towards high excess capacity counties. We first note that even if stimulus had been allocated to only above median excess capacities, excess capacity in the above median counties would still have remained above the median. Since in those counties the average jobs multiplier was 1.38, $261 billion would have translated into 3.6 million jobs. The multiplier, as a result, would have been 83% higher.

5 Conclusion

In this paper, we estimate local fiscal multipliers. We show how to aggregate multipliers from local multipliers to regional multipliers. We also semi-parametrically estimate the multipliers as a function of excess capacity. We find large differentials between low and high excess capacity regions. Even in the Great Recession, we find no statistically significant, contemporaneous impact of public expenditures on employment in counties below median excess capacity did not significantly increase local employment. However, we find a substantially larger contemporaneous employment multiplier of 0.5 jobs per person for every $100,000 spent per person. Over two years, the $100,000 spent per person cumulates to 1.38 job-years. Spatial aggregation substantially increases the size of the multiplier. The average static multiplier is 0.98 job-years when we estimate using catchment areas including counties within 120 miles of a given county. That rises to 3.28 over two years. Moreover, we find that a spatially optimally targeted stimulus would have raised the multiplier by 83 percent.

Our estimates are useful for understanding when and where public funds are effective at increasing employment and output. We hope that future work will improve upon what we have done by better reconciling local multiplier estimates with national estimates. This reconciliation could be improved in three ways. First, we have focused solely upon labor market impacts of stimulus. However, stimulus could impact capital income as well. Second, we have primarily focused on non-tradable sector employment because that is what is easily identifiable using cross-county variation. However, the magnitude of the multiplier would presumably be greater if we could incorporate effects on the tradable sector. There could also be qualitative differences between tradable and non-tradable sector multipliers. If tradable sectors are more likely to use increasing returns to scale technologies, then the multiplier in tradable sectors might display non-linearity in amount spent in contrast to what we have found in predominantly non-tradable sectors. Finally, we have estimated our effects during the Great Recession when the nominal interest rate on government debt was at zero. This presumably makes monetary policy relatively ineffective but fiscal policy quite effective. Our estimates would be improved if they could be generalized outside of the ZLB interest rate zone and also if they could incorporate endogenous responses of monetary policy. We hope future empirical work will make progress in these three ways.

Finally, we also would like to see better theoretical explanations for why the fiscal multiplier is increasing in excess capacity. Michaillat (2014) has a model where the multiplier is decreasing in the degree of labor market tightness. Our empirical results cannot differentiate between a labor market tightness effect from an excess capacity effect. Both theoretical work on the potential impact of excess capacity on the multiplier as well as empirical work differentiating between different theories of the countercyclical multiplier for reasons unrelated to the zero lower bound in nominal interest rates would be highly welcome.

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Figure 1: Cumulative flow of stimulus awards

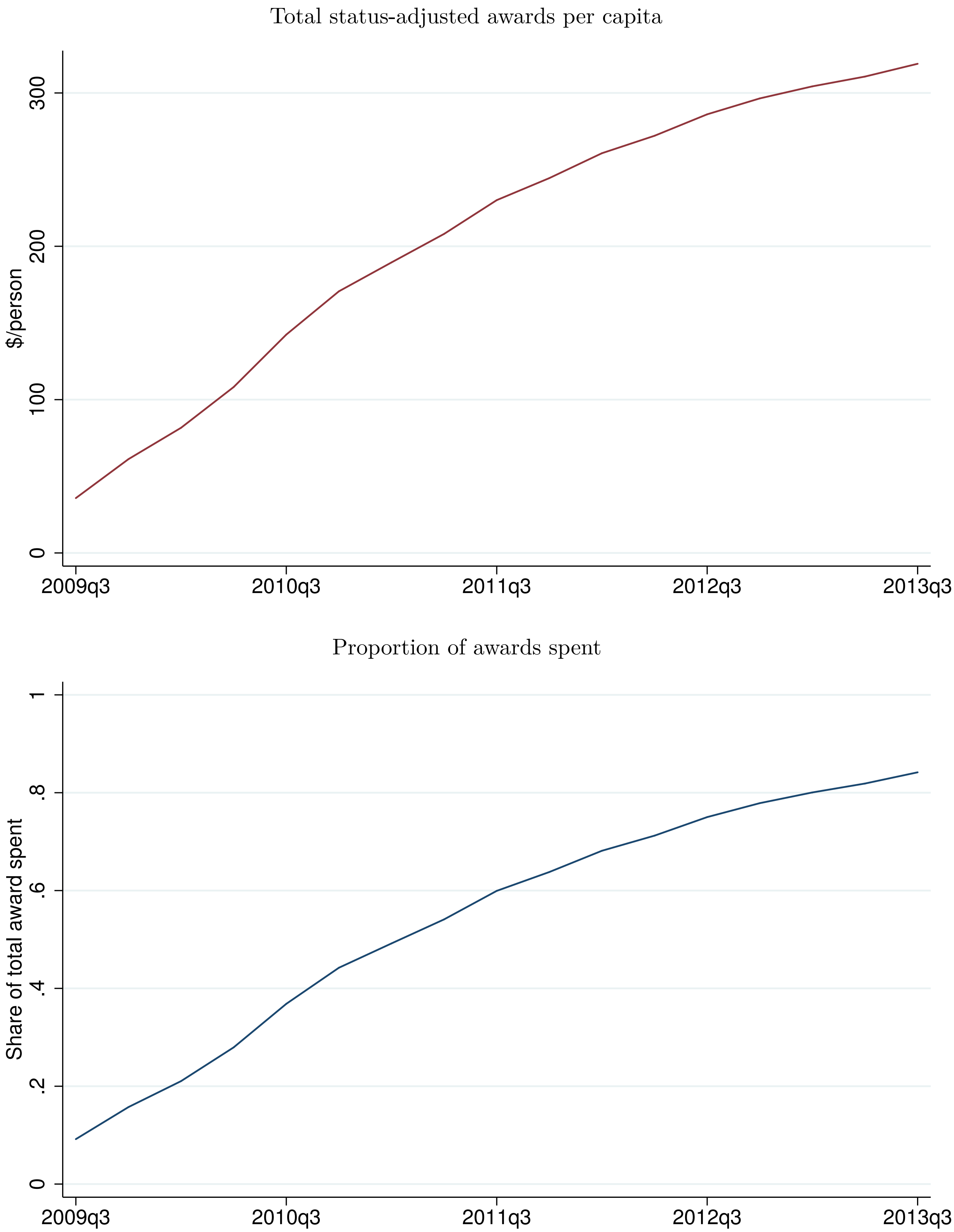

Figure 2: Status adjusted awards per capita in 2011q3

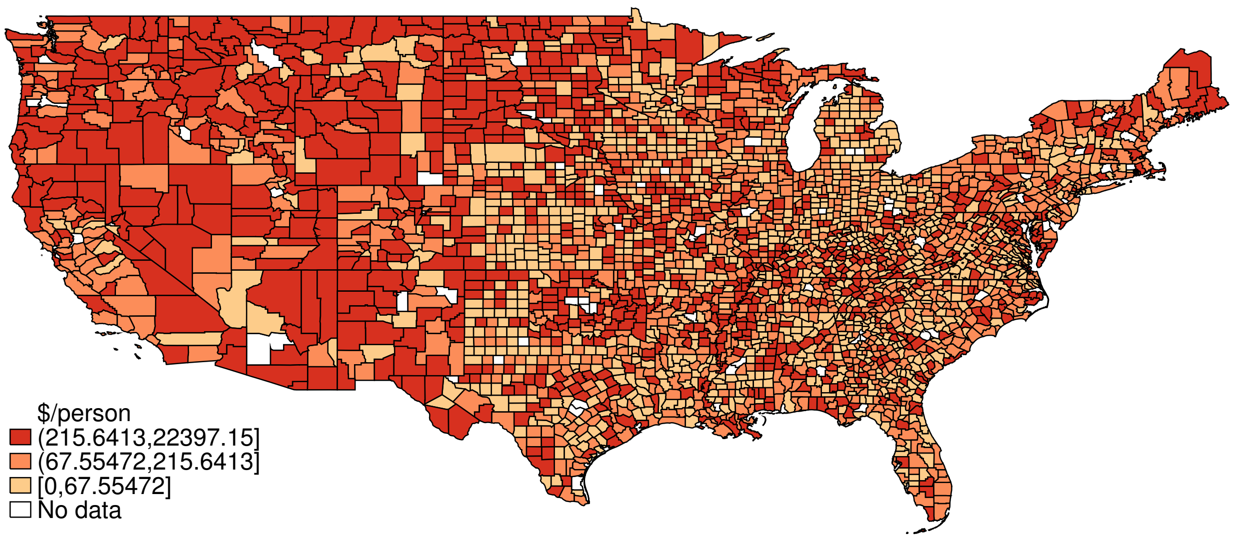

Figure 3: Effects of stimulus, by excess capacity

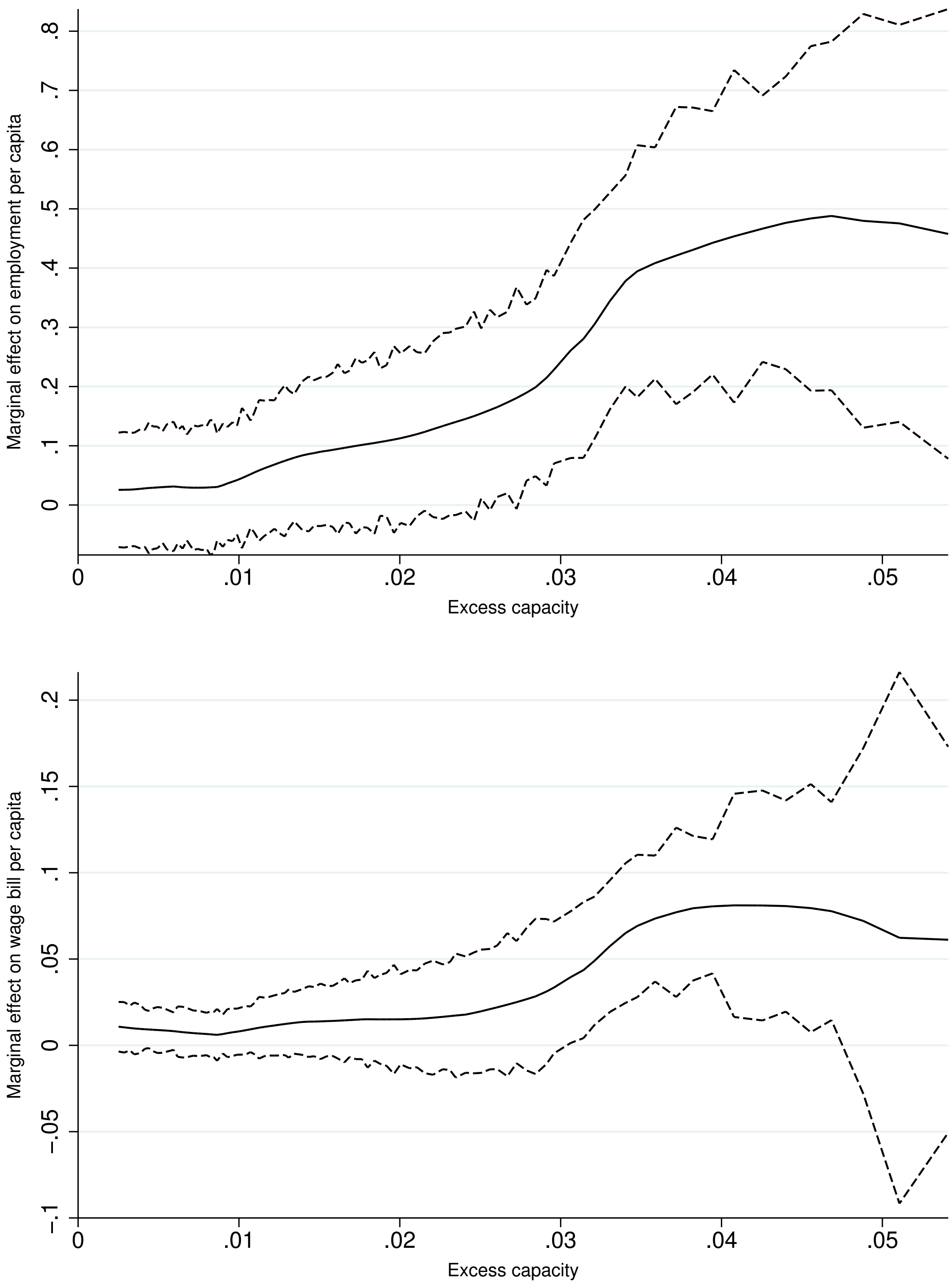

Table 1: Main table

	Employment per capita					Wage bill per capita				
Static specification	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	0.117	0.207*	0.114	0.198**	0.251***	0.037	0.044	0.023	0.016	0.033*
	(0.105)	(0.116)	(0.128)	(0.086)	(0.082)	(0.033)	(0.034)	(0.036)	(0.027)	(0.020)
Time aggregated										
Two quarters	0.009	0.089	0.052	0.086	0.119	–0.017	0.007	–0.013	0.163	0.202**
	(0.107)	(0.118)	(0.126)	(0.083)	(0.080)	(0.149)	(0.154)	(0.148)	(0.100)	(0.097)
Four quarters	0.108	0.269	0.163	0.216	0.274**	–0.098	–0.029	–0.102	0.347**	0.414**
	(0.171)	(0.187)	(0.201)	(0.137)	(0.121)	(0.370)	(0.361)	(0.364)	(0.169)	(0.162)
Six quarters	0.171	0.355*	0.212	0.389**	0.479***	2.101	2.154	2.086	0.311**	0.415***
	(0.196)	(0.200)	(0.233)	(0.163)	(0.149)	(1.468)	(1.386)	(1.373)	(0.153)	(0.111)
Eight quarters	0.402*	0.525**	0.259	0.589***	0.755***	–0.084	–0.051	–0.368	–0.312	–0.100
	(0.219)	(0.231)	(0.245)	(0.206)	(0.219)	(0.762)	(0.753)	(0.746)	(0.481)	(0.429)
Common time FE	Y					Y				
Division X time FE		Y					Y			
State X time FE			Y	Y	Y			Y	Y	Y
Predicted EPOP				Y	Y					
Predicted wage									Y	Y
Demo. controls					Y					Y

Notes: Estimates are of own-county employment and wage bill on own-county stimulus expenditures. Stimulus expenditures are in $100,000 per resident. The sample is from 2008Q1 to 2011Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports available at www.recovery.gov. Regressions are at the quarterly level. The outcome variable is employment per capita in the five columns on the left and wage bill per capita in the five columns on the right. Each column shows coefficients or sums of coefficients from a single regression. The first row shows the coefficient on contemporaneous stimulus expenditure. Remaining rows show the sum of contemporaneous results plus subsequent lags. Predicted employment to population ratio and predicted wage are Bartik predicted measures. Predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are multiplied by linear time trends. Standard errors are clustered at the county level.

* p<10%, ** p<5%, *** p<1%

Table 2: Excess capacity, split sample

	Employment per capita			Wage bill per capita		
	All counties	Low excess	High excess	All counties	Low excess	High excess
Static specification	0.251***	0.009	0.447***	0.033*	0.010*	0.057***
	(0.082)	(0.040)	(0.081)	(0.020)	(0.005)	(0.020)
Time aggregated						
Two quarters	0.119	–0.003	0.318***	0.202**	0.026	0.397**
	(0.080)	(0.040)	(0.107)	(0.097)	(0.023)	(0.180)
Four quarters	0.274**	0.006	0.489***	0.414**	0.037	0.731***
	(0.121)	(0.064)	(0.140)	(0.162)	(0.037)	(0.269)
Six quarters	0.479***	0.031	0.751***	0.415***	0.083	0.537***
	(0.149)	(0.106)	(0.161)	(0.111)	(0.056)	(0.127)
Eight quarters	0.755***	0.013	1.381***	–0.100	0.127*	–0.196
	(0.219)	(0.148)	(0.284)	(0.429)	(0.073)	(0.616)
Observations	45,570	22,785	22,785	45,570	22,785	22,785

Notes: Estimates are of own-county employment and wage bill on own-county stimulus expenditures. Stimulus expenditures are in $100,000 per resident. The sample is from 2008Q1 to 2011Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports available at www.recovery.gov. Regressions are at the quarterly level. The outcome variable is employment per capita in the five columns on the left and wage bill per capita in the five columns on the right. Regressions control for Bartik predicted employment to population ratio or Bartik predicted wage bill. stateXtime fixed effects, county fixed effects and demographic controls. Predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are multiplied by linear time trends. Excess capacity is the difference between the current employment to population ratio and the maximum excess capacity in the 3 years before 2009Q1. Low excess capacity is below the 50th percentile of mean county excess capacity averaged over the sample and high excess capacity is above the 50th percentile of mean excess capacity averaged over the sample. Standard errors are clustered at the county level.

* p<10%, ** p<5%, *** p<1%

Table 3: Time-based effects of stimulus, using time fixed effects interacted with total award per capita

	Employment per capita			Wage bill per capita		
Time	All counties	Low excess	High excess	All counties	Low excess	High excess
2008q2	–0.024	0.001	–0.092	–0.016	0.001	–0.036
	(0.035)	(0.004)	(0.093)	(0.011)	(0.001)	(0.022)
2008q3	0.005	0.004	–0.006	–0.018	0.000	–0.038
	(0.038)	(0.005)	(0.111)	(0.016)	(0.001)	(0.036)
2008q4	0.024	0.000	0.056	–0.040	0.002	–0.112
	(0.016)	(0.005)	(0.041)	(0.040)	(0.002)	(0.093)
2009q1	0.021	0.006	0.071*	0.002	0.000	0.022
	(0.016)	(0.006)	(0.038)	(0.005)	(0.001)	(0.014)
2009q2	–0.008	0.009	–0.039	0.001	0.001	0.022
	(0.050)	(0.006)	(0.136)	(0.009)	(0.002)	(0.027)
2009q3	0.025	0.011	0.066	–0.000	0.001	0.024
	(0.053)	(0.007)	(0.145)	(0.007)	(0.001)	(0.021)
2009q4	0.044	0.006	0.133*	–0.033	0.002*	–0.085
	(0.034)	(0.009)	(0.079)	(0.034)	(0.001)	(0.080)
2010q1	0.052	0.007	0.176***	0.023	0.002**	0.079**
	(0.035)	(0.010)	(0.067)	(0.017)	(0.001)	(0.040)
2010q2	0.017	0.005	0.063	0.003	0.001	0.028
	(0.053)	(0.010)	(0.141)	(0.008)	(0.002)	(0.022)
2010q3	0.069	0.016	0.206	0.004	0.001	0.036
	(0.056)	(0.011)	(0.138)	(0.009)	(0.001)	(0.022)
2010q4	0.100**	0.017	0.291***	–0.034	0.007***	–0.095
	(0.047)	(0.013)	(0.073)	(0.044)	(0.002)	(0.107)
2011q1	0.096**	0.015	0.310***	0.028	0.002	0.092**
	(0.049)	(0.013)	(0.069)	(0.018)	(0.002)	(0.038)
2011q2	0.063	0.012	0.203*	0.000	0.003	0.018
	(0.053)	(0.016)	(0.123)	(0.010)	(0.003)	(0.023)
2011q3	0.100	0.013	0.314**	–0.010	0.003	–0.011
	(0.062)	(0.015)	(0.136)	(0.020)	(0.002)	(0.047)
Observations	45,570	22,785	22,785	45,570	22,785	22,785

Notes: See notes for Table 2 for detailed description of regressions. Each column presents coefficients from a separate regression. Rows within a column present coefficients of a time dummy interacted with the total amount of stimulus allocated to the county interacted with a time dummy. Time dummy interactions with total of stimulus money allocated replace contemporaneous stimulus amount.

* p<10%, ** p<5%, *** p<1%

Table 4: Nonlinear effects of the extent of stimulus, split sample

	Employment per capita			Wage bill per capita		
	All counties	Low excess	High excess	All counties	Low excess	High excess
Linear specification	0.251***	0.009	0.447***	0.033*	0.010*	0.057***
	(0.082)	(0.040)	(0.081)	(0.020)	(0.005)	(0.020)
Quadratic specification						
Linear term	0.321***	0.002	0.624***	0.034	0.017*	0.048
	(0.120)	(0.082)	(0.179)	(0.035)	(0.010)	(0.060)
Quardratic term	–0.900	0.056	–3.114	–0.009	–0.061	0.158
	(0.747)	(0.356)	(2.252)	(0.244)	(0.045)	(0.728)
Observations	45,570	22,785	22,785	45,570	22,785	22,785

Notes: Estimates are of own-county employment and wage bill on own-county stimulus expenditures. Stimulus expenditures are in $100,000 per resident. The sample is from 2008Q1 to 2011Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports available at www.recovery.gov. Regressions are at the quarterly level. The outcome variable is employment per capita in the three columns on the left and wage bill per capita in the three columns on the right. The first row reports coefficients on stimulus expenditures in a model with only a linear term for stimulus expenditures. The second and third rows report linear and quadratic coefficients respectively from a single regression with a quadratic polynomial in stimulus expenditures. Regressions control for Bartik predicted employment to population ratio or Bartik predicted wage bill. stateXtime fixed effects, county fixed effects and demographic controls. Predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are multiplied by linear time trends. Excess capacity is the difference between the current employment to population ratio and the maximum excess capacity in the 3 years before 2009Q1. Low excess capacity is below the 50th percentile of mean county excess capacity averaged over the sample and high excess capacity is above the 50th percentile of mean excess capacity averaged over the sample. Standard errors are clustered at the county level.

* $p<10\%$, ** $p<5\%$, *** $p<1\%$

Table 5: Effects of stimulus by industrial sector, split sample

	Employment per capita			Wage bill per capita		
	All counties	Low excess	High excess	All counties	Low excess	High excess
Overall	0.251***	0.009	0.447***	0.033*	0.010*	0.057***
	(0.082)	(0.040)	(0.081)	(0.020)	(0.005)	(0.020)
	45,570	22,785	22,785	45,570	22,785	22,785
Private sector	0.263***	0.008	0.442***	0.022	0.002	0.044**
	(0.082)	(0.039)	(0.084)	(0.022)	(0.005)	(0.022)
	45,120	22,560	22,560	45,120	22,560	22,560
Public sector	–0.031	–0.007	–0.016	0.004**	0.003*	0.003*
	(0.021)	(0.023)	(0.017)	(0.002)	(0.002)	(0.002)
	38,460	19,230	19,230	38,460	19,230	19,230
Services	0.153**	0.015	0.292***	0.018	0.002	0.036*
	(0.070)	(0.029)	(0.070)	(0.019)	(0.004)	(0.019)
	44,685	22,350	22,335	44,685	22,350	22,335
Goods	0.054**	–0.017	0.065*	–0.002	–0.000	–0.002
	(0.027)	(0.033)	(0.036)	(0.005)	(0.003)	(0.007)
	40,305	20,160	20,145	40,305	20,160	20,145
Construction	0.059***	0.020	0.076***	0.007**	0.002	0.011***
	(0.020)	(0.019)	(0.026)	(0.003)	(0.003)	(0.004)
	35,880	17,940	17,940	35,880	17,940	17,940

Notes: Estimates are of own-county employment and wage bill on own-county stimulus expenditures. Stimulus expenditures are in $100,000 per resident. The sample is from 2008Q1 to 2011Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports available at www.recovery.gov. Regressions are at the quarterly level. The outcome variable is employment per capita in the three columns on the left and wage bill per capita in the three columns on the right. Regressions control for Bartik predicted employment to population ratio or Bartik predicted wage bill. stateXtime fixed effects, county fixed effects and demographic controls. Predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are multiplied by linear time trends. Excess capacity is the difference between the current employment to population ratio and the maximum excess capacity in the 3 years before 2009Q1. Low excess capacity is below the 50th percentile of mean county excess capacity averaged over the sample and high excess capacity is above the 50th percentile of mean excess capacity averaged over the sample. Industries come from NAICS classifications in the QCEW. Standard errors are clustered at the county level.

* p<10%, ** p<5%, *** p<1%

Table 6: Geographically aggregated effects of stimulus, by catchment size

	Employment per capita				Wage bill per capita			
	30 miles	60 miles	90 miles	120 miles	30 miles	60 miles	90 miles	120 miles
Static specification	0.350***	0.522***	0.757***	0.982***	0.086***	0.129***	0.181***	0.200***
	(0.089)	(0.117)	(0.159)	(0.272)	(0.016)	(0.031)	(0.048)	(0.064)
Time aggregated								
Two quarters	0.243**	0.400**	0.621***	0.843***	0.308***	0.463***	0.736***	0.841***
	(0.108)	(0.161)	(0.167)	(0.263)	(0.091)	(0.177)	(0.217)	(0.294)
Four quarters	0.392**	0.524**	0.734***	1.009***	0.544***	0.663***	1.010***	1.190**
	(0.158)	(0.207)	(0.252)	(0.378)	(0.111)	(0.216)	(0.363)	(0.487)
Six quarters	0.637***	0.854***	1.079***	1.330**	0.816***	1.045***	1.307***	1.345***
	(0.127)	(0.229)	(0.370)	(0.539)	(0.108)	(0.213)	(0.301)	(0.399)
Eight quarters	0.966***	1.711***	2.605***	3.279***	0.484*	1.159***	1.701**	1.816*
	(0.194)	(0.365)	(0.577)	(0.944)	(0.280)	(0.413)	(0.711)	(1.083)
Observations	45,570	45,570	45,570	45,570	45,570	45,570	45,570	45,570

Notes: Estimates are of regional employment and wage bill on own-region stimulus expenditures. Stimulus expenditures are in $100,000 per resident. Regions are groups of counties whose population centroids are within 30, 60, 90 or 120 miles respectively of a base county. An observation is a region constructed from a base county in a quarterXyear. Observations spatially overlap with other observations with nearby base counties. The sample is from 2008Q1 to 2011Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports available at www.recovery.gov. The outcome variable is employment per capita in the three columns on the left and wage bill per capita in the three columns on the right. Regressions control for Bartik predicted employment to population ratio or Bartik predicted wage bill. stateXtime fixed effects, county fixed effects and demographic controls. Predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are multiplied by linear time trends. Standard errors allow for arbitrary forms of correlation for counties with population centroids within 2X miles where X miles is the radius of spatial aggregation.

* p<10%, ** p<5%, *** p<1%