CROSS-REGION TRANSFERS IN A MONETARY UNION: EVIDENCE FROM THE US AND SOME IMPLICATIONS

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Abstract. US federal transfers to individuals are large, countercyclical, vary geographically, and are often credited for helping stabilize regional economies. This paper estimates the effects of these transfers using plausibly exogenous regional variation in recent temporary stimulus packages and earlier permanent Social Security increases. States which received larger transfers tended to grow faster contemporaneously, with larger multipliers for permanent than for temporary transfers. Results are broadly consistent with an open-economy New Keynesian model. At business-cycle frequencies, the multipliers on countercyclical cross-region transfers are closer to those for temporary transfers, suggesting only modest gains in regional stabilization from the US transfer union. JEL: E62, F45, F41

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1. Introduction

Transfers from the US federal government to individuals in different states are large, countercyclical, and vary geographically. In contrast with a traditional view of government expenditure, federal transfers are larger than government consumption, they account for the majority of the increase in expenditure around the 2009 financial crisis, and transfers were the bulk of earlier short-term fiscal stimulus in 2001 and 2008.[1] A number of papers have found that a $1 decrease in per capita income in a US state/region over time is associated with around a $0.20–0.40 net transfer from the federal government to the residents of that state/region through the tax-transfer system (Feyrer and Sacerdote 2013, Sala-i-Martin and Sachs 1991, Bayoumi and Masson 1995). Despite the size and countercyclicality of these federal transfers, to my knowledge there are no estimates as to whether changes in transfers to individuals have a large or small effect on growth in the short run in different states (the "cross-region transfer multiplier").[2] My main contribution is to fill this gap using several "natural experiments" in which permanent Social Security benefit increases and temporary stimulus payments allocate transfers across states in a way that is plausibly unrelated to regional business cycles (Section 2).

The cross-sectional effect of transfers on growth is conceptually different from the marginal propensity to consume (MPC) transfers (see Parker et al. 2013, Johnson et al. 2006, and Hausman 2016, among others) or cross-sectional multipliers for other types of spending (Nakamura and Steinsson 2014; henceforth NS, Clemens and Miron 2012, and Chodorow-Reich et al. 2012, among others) and varies drastically across models. My second contribution is to document these differences using analytical expressions for transfer and purchase multipliers (Section 3) and to use the empirical transfer multiplier estimates to distinguish between models (Section 4). In contrast, most of the recent theoretical literature has focused on determinants of the MPC (Kaplan and

[1]See Appendix 2 for background on the size of gross transfers, and countercyclical net transfers to different regions. Oh and Reis (2012) argue that 75% of the increase in US government spending over 2007-09 was due to transfers, with similar proportions in other countries.

[2]Hausman (2016) finds a high MPC out of the 1936 veterans' bonus (spent mostly on durables). In his Online Appendix F, he also correlates veterans per capita with the change in income or employment across states, but the estimates are imprecise and not conclusive.

Violante 2014) or the (large) differences between cross-region and aggregate purchase multipliers (Farhi and Werning (2016) and NS).[3]

The cross-region transfer multiplier differs from the MPC due to local general equilibrium (GE) effects. As transfers can be spent on goods produced in other regions, the transfer multiplier can be large or small depending on home bias in consumption (openness)—even if the transfers are spent. Local GE effects can be positive if prices/wages are sticky, but also make cross-region transfer multipliers negative in neoclassical models (due to wealth effects on labor supply). In contrast, regional purchase multipliers are always positive (and much larger) in a wide variety of models, due to an unequivocal boost to local demand and a lack of local wealth effects. Consequently, the transfer multiplier is more sensitive to shock persistence (which I exploit empirically), the share of financially constrained households, openness, and price/wage rigidities. The wide variety of theoretically possible transfer multipliers underscores the need for empirical evidence to discipline the choice of models and parameters.

The size of the cross-region transfer multiplier is also important for policy. In contrast with the US, variation in comparable cross-country transfers within the European Monetary Union (EMU) is negligible. For a quarter-century, commentators have argued that US-style countercyclical cross-region transfers might help Europe reduce the effects of negative shocks on the periphery—the shocks that recently threatened the viability of the EMU itself.[4] However, the strength of this argument depends on whether the US transfer union can actually smooth business cycle shocks affecting US states, which depends on the size of the cross-region transfer multiplier at business-cycle frequencies (which I address as my third contribution, Section 5).

Identification strategy My main empirical challenge is that transfers to individuals are typically endogenous to state-level output growth due to reverse causality and omitted variable bias. *Reverse causality* stems from the

[3]Farhi and Werning (2017) focus on optimal transfers rather than the size of multipliers.

[4]For example, *The Economist* writes "America's fiscal union is so good at absorbing [regional] shocks that it is often cited as a model for the more accident-prone euro zone" (Free exchange, 28 November 2015). For earlier evidence, see Sala-i-Martin and Sachs (1991): "Some economists...argue that [the] regional insurance scheme provided by the federal government is one of the key reasons why the system of fixed exchange rates within the United States has survived without major problems" (p. 20).

countercyclical nature of transfers to state residents. To address this problem, my identification strategy combines a transfer policy change at the aggregate level that affects all states—and so is unlikely to be driven by developments in a particular state—with a predetermined and slow-moving cross-state allocation of the transfer based on regulations that determine eligibility and their relative importance in different states. This is a similar idea to a Bartik (1991) instrument, though differs in implementation. *Omitted variable bias (OVB)* stems from other excluded variables that might affect both transfers and regional growth. State fixed effects (FEs) remove state-level trends, and time fixed effects remove all aggregate variation (e.g. the US business cycle, monetary policy, or international shocks), leaving only potential confounding factors that vary both across states and over time. Remaining OVB is reduced by (i) a research design that involves many policy changes (reducing the chance of a coincidental correlation), and (ii) a battery of robustness tests, such as including other time-and-state-varying controls, dropping states and quarters one at a time, and a number of placebo tests. I focus on the contemporaneous effect (impact multiplier) of cross-state transfers on quarterly state GDP, or non-transfer labor income ($W \times L$) which is available over a longer sample.

Transfer policies. For permanent transfers, I study a series of ad hoc increases in the monthly stipend of Social Security (old age pension) recipients over 1952–74. An increase in the monthly Social Security stipend naturally leads to a larger increase in transfers to states like Florida, relative to Alaska (which is less popular with retirees). Romer and Romer (2016) provide narrative evidence that this sample of Social Security increases was legislated mostly to compensate for past inflation, and never out of any desire to stimulate the economy. As such, these Social Security increases are likely even exogenous at the aggregate level, though I only require the weaker claim that they are exogenous in the cross-section.

For temporary transfers, I study one-off stimulus payments in 2001 and 2008 (a "check in the mail") of \$300–\$600. While the relatively fixed dollar value of payments means that these transfers are mechanically more important in poorer states, other eligibility criteria—such as having a tax liability in 2001—reduce the progressivity of the transfer and mean that the cross-state allocation varies across policies. It is difficult to argue that these programs were enacted to help specific states in particularly deep recessions given that the benefits were widely spread across states and eligibility rules were a simple function of prior-year individual taxable income.

Results Summary. My key empirical finding is that states receiving larger transfers tended to have faster short-run growth in non-transfer labor income or GDP, with a much larger cross-region transfer multiplier for permanent than for temporary transfers. Specifically, I find that a state that received an extra $1 temporary transfer experienced an increase in labor income or GDP in the quarter by around $0.3 ($0.2–0.4, depending on the specification). For permanent transfers, I find that states that received an extra $1 in Social Security payments increased their labor income by around $1.5 contemporaneously ($1.1–$1.9, depending on the specification). Romer and Romer (2016) found that almost all of the permanent Social Security increases were consumed, and Parker et al. (2013) and Johnson et al. (2006) argue that a modest fraction of the temporary transfers were consumed—both of which are broadly consistent with my results (though those papers do not investigate GE effects across regions, or look at effects on GDP or non-transfer income).

These empirical estimates of cross-region transfer multipliers turn out to be broadly consistent with a standard open-economy New Keynesian (NK) model that features home bias in consumption and a share of constrained households, and are less consistent with a canonical neoclassical model. If I calculated *aggregate* purchase and targeted-transfer closed-economy multipliers in this NK model, they would be large (≥ 1) if monetary policy is accommodating, and small (<1) otherwise, consistent with the results in NS.

Policy Implications for Transfer Unions. Using the NK model consistent with the data, I show that at business-cycle frequencies the relevant cross-region transfer multipliers are closer to small temporary transfer multipliers than large permanent transfer multipliers. I then combine these multipliers of around 0.42 (range 0.37–0.6) with estimates from the literature of the countercyclicality of federal transfers to the residents of US states. Results suggest that the US transfer union would only smooth about 10–15% of regional asymmetric shocks to output—less than commentators often assume.

2. Empirical Evidence

In this section, I estimate the causal effect of permanent Social Security transfers over 1952–74 (Section 2.2) and temporary transfers from the 2001 and 2008 US stimulus packages (Section 2.3) on quarterly economic growth at the state level. To my knowledge, these are the first estimates of cross-region transfer multipliers, which are conceptually very different from the estimates of purchase multipliers or the MPC in the literature (see Section 3). These estimates are broadly consistent with a quantitative NK model in Section 4, which is then used to produce implications for the efficacy of the US transfer union in smoothing asymmetric business cycle shocks (Section 5).

Specification In the main regressions, I investigate the effect of transfers on the contemporaneous growth rate of quarterly labor income per capita (equivalent to the wage bill) $\Delta(WL)^{pc}_{i,t} \equiv ((WL)^{pc}_{i,t} - (WL)^{pc}_{i,t-1})/(WL)^{pc}_{i,t-1}$, which is available for the whole sample; or quarterly GDP per capita from 2005 $\Delta Y^{pc}_{i,t} \equiv (Y^{pc}_{i,t} - Y^{pc}_{i,t-1})/Y^{pc}_{i,t-1}$, as in Equation 1 ($\boldsymbol{X}$ is vector of controls that varies across specifications).[5] The measure of transfers is scaled by lagged labor income or GDP to generate a "multiplier" interpretation of coefficients, as is standard in the literature $\Delta tr_{i,t} \equiv (tr_{i,t} - tr_{i,t-1})/(WL)_{i,t-1}$ or $\Delta tr_{i,t} \equiv (tr_{i,t} - tr_{i,t-1})/Y_{i,t-1}$ i.e., the dollar value of extra labor income or GDP produced by an extra dollar of transfers.[6]

$$\Delta(WL)^{pc}_{i,t} \text{ or } \Delta Y^{pc}_{i,t} = \beta \Delta tr_{i,t} + \boldsymbol{X}\boldsymbol{\delta} + \gamma_t + \mu_i + e_{it} \tag{1}$$

All variables are deflated by the national quarterly Personal Consumption Expenditures (PCE) Chain-type Price Index, though I also examine the effect on a constructed quasi-local price index in Section 2.4 and the Appendix.[7]

[5] I focus on the contemporaneous impact as (i) longer lags require stronger identification assumptions due to the greater range of potential omitted variables in the intervening period, and (ii) dynamics are generally insignificant (statistically or economically)—see Appendix Table 4. I focus on these dependent variables as other potential outcome variables are missing at the state level (consumption), not available quarterly (GDP before 2005), not seasonably adjusted, or not available for both temporary and permanent transfer samples.

[6] This is equivalent to transfer per capita as a share of income income or GDP per capita.

[7] CPI data are not available by state. While the BEA does produce estimates of state-level GDP deflators, these use national prices for different industries, weighted using state-specific industry weights, which do not capture the price effects from local demand shocks.

Labor income data come from the Bureau of Economic Analysis (BEA) under the official title "Earnings by place of work" and mostly consist of Wage and Salary Disbursements (70%). The data exclude income from transfers and are before income taxes. As state-level quarterly labor income can be extremely volatile, especially for small states, I drop outliers from all specifications (unless otherwise noted) where the annualized growth rate of labor income growth or GDP is more than 20% in absolute value. Standard errors are cluster-robust (clustered at the state level), which allows for heteroskedasticity and arbitrary serial correlation. See Appendix 1 for descriptive statistics and additional information on data sources and construction.

2.1. **Identification.** There are two key challenges in identifying the effect of transfers on cross-sectional growth: reverse causality and omitted variable bias.

Reverse causality is a serious problem for transfer multiplier estimates because transfers are countercyclical. As such, a simple regression of growth on transfers will pick up a combination of the transfer multiplier and the countercyclical tax-transfer system, leading to downward-biased multiplier estimates (too negative).

My identification strategy addresses reverse causality by studying specific "natural experiments"—temporary stimulus packages and permanent Social Security changes—in which federal transfers across states change due to an aggregate change in federal policy. These aggregate policies generate variation in transfers across states through eligibility rules for the transfer that depend on demographic characteristics of states or the distribution of prior-year taxable income. For example, an increase in the monthly stipend of Social Security recipients generates a larger increase in transfers to states with many retirees, such as Florida. This is a similar idea to a Bartik (1991) instrument, though is based on aggregate policy changes (rather than general aggregate variation) and policy-specific weights which means that the state-level policy variable itself is exogenous.

This research design impedes feedback from state business cycles to the transfer in three ways. First, as the policy change is at the aggregate level (affecting all states), it is unlikely to be implemented to address a recession or boom in a particular state. Romer and Romer (2016) show this specifically for my sample of Social Security increases (discussed further below), and transfer stimulus payments were widely spread across states and had simple eligibility rules, making it difficult to target them at specific states. Voting records reveal that political support for one-off transfers was either mostly partisan (2001) or bipartisan (2008), with no evidence that legislators from slow-growing states were more likely to support the legislation (see Appendix 3.2).

Second, my allocation of transfers is based on last year's eligibility characteristics—such as the lagged number of Social Security recipients or the prior-year distribution of taxable income—which means that contemporaneous shocks in states can't affect the size of the transfer mechanically.[8]

Finally, demographic characteristics or the distribution of taxable income are essentially stocks rather than flows; they are predetermined and slow-moving over the medium term. For example, across states the size of the 1972Q4 increase in Social Security payments was almost exactly proportional to the 1970Q2 increase in Social Security payments (R-squared of 0.96) (blue circles on the left side of Appendix Figure 1.1). This limits feedback from short-run economic growth to the distribution of transfers across states.

Omitted variable bias (OVB). The second—and perhaps more serious—concern is that there might be other variables that affect both regional transfers and regional economic growth.

The first way I address OVB is by including state fixed effects (μ_i) and quarter fixed effects (γ_t) in all specifications. State fixed effects control for all state-level trends (like the faster growth of "Sun Belt" states relative to "Rust Belt" states), and quarter fixed effects remove aggregate variation (such as the US business cycle, monetary policy, expectations, or international shocks). This means that regressions only utilize variation in transfers and growth both across states and over time, and so any omitted variable would also have to vary across states and over time. This drastically reduced the number of potentially confounding variables.

[8]For one-off transfers, last year's taxable income was used to determine eligibility, as current-year income was not yet known. After removing trends and aggregate variation, state growth rates have little quarterly persistence, which removes the possibility of ex-ante targeting states in recession.

Second, the risk of OVB is reduced by a research design that involves many transfer policy changes with different sizes and timings, and over a long period. This means that any omitted variable would need to take an unlikely time-varying pattern in high vs low transfer states, which is a much higher hurdle for potential confounding variables than in a standard diff-in-diff study with a one-off permanent policy change. For example, Social Security rates increased by 10% in June 1971 and 20% in October 1972, but not at all in 1973, and so the confounding variable would have to follow a similar pattern in high-transfer states and only boost growth in those specific quarters. For temporary transfers, the specification in Equation 1 implies that the confounding variable not only has to spuriously boost growth in high transfer states in the relevant quarter, it also has to reduce growth in the following quarter as the transfer is withdrawn. For example, in 2001 the confounding variable would have to boost growth in 2001Q3 in high-transfer states like WV, MT, ME and then reduce growth in 2001Q4 in the same states.

Moreover, the identity of high-transfer states changes over time and across transfer policies, which means that the confounding variables would also have to change. For example, the cross-state allocation of transfers in 2008 and 2001 are quite different, with the 2008 allocation only explaining 12% of the variation in 2001 transfers (Appendix Figure 1.1 right side). This difference stems from the greater progressivity of the 2008 transfer—which was refundable with a high-income phase-out—relative to the 2001 transfers, which were not refundable with no phase-out. Demographic changes over a longer period (1952–72) led to a reallocation of Social Security benefits across many states, such that the allocation in 1952 only explains 28% of the allocation in 1972 (Appendix Figure 1.1 left side, green triangles).

Finally, OVB is reduced by adding other controls that vary by state and over time and a number of placebo tests in which the transfer is assumed to have occurred in a different period. Additional controls include state-specific sensitivities to the national business cycle or oil prices, state-specific quartic trends, as well as the removal of influential years or states.

I first present results for permanent transfers (next section) and then temporary transfers (Section 2.3).

2.2. **Permanent Transfers.**

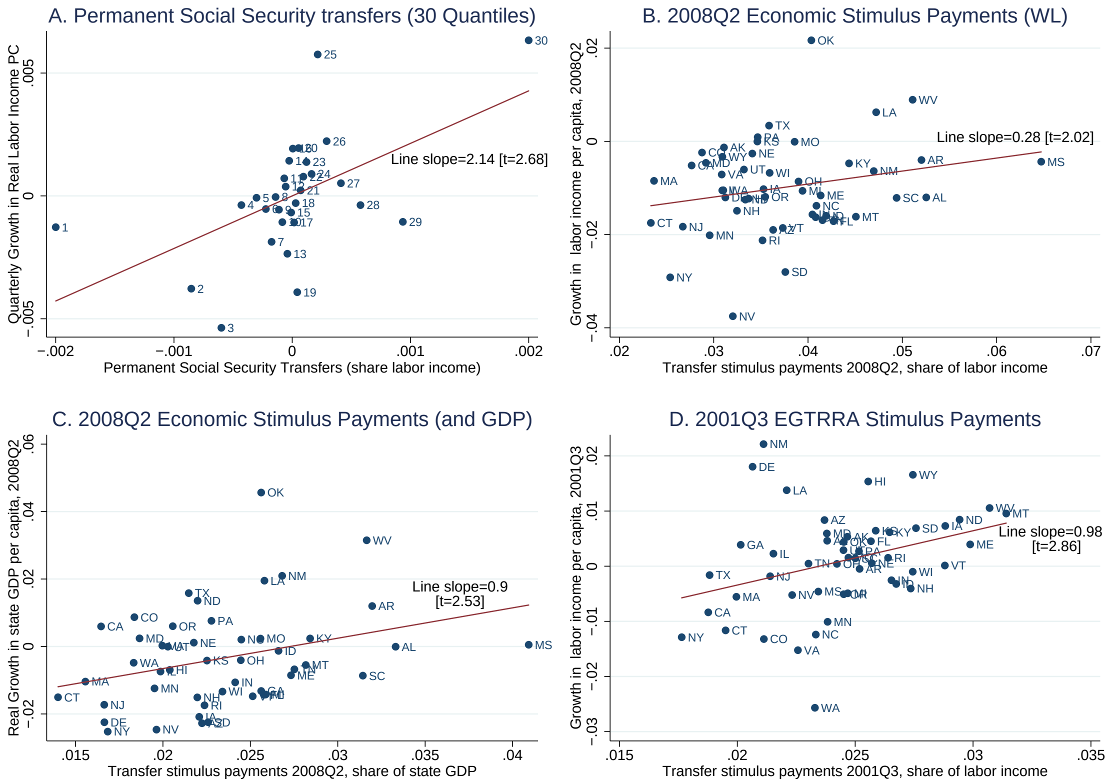
2.2.1. ***Description of Social Security Increases.*** Before 1975, Social Security payments (largely the old age pension) were not indexed to inflation and were increased by act of Congress on an ad hoc basis (Wilcox 1989). These were mostly *permanent* increases in transfers—i.e., a higher monthly stipend received by the elderly and their dependents—and so are more likely to be spent by unconstrained consumers (by the permanent income hypothesis). As transfer increases varied in size and timing they would not be subsumed into seasonal factors. From 1975 onward, Social Security payments were indexed to the CPI and generally adjusted annually, making them much more predictable and so are excluded from my analysis.[9]

The sample of Social Security payment increases at the aggregate level covers 1952–74 and is taken from Table 1 of Romer and Romer (2016). As Romer and Romer's data are monthly whereas mine are quarterly, I spread the adjustments over two quarters if the permanent increase in payments occurred mid-quarter.[10] There are exogenous 20 Social Security increases during 1952-74, which are spread over 27 quarters; there are no increases in the remaining 65 quarters ($\Delta tr_{i,t} = 0$). The latter are included in the sample to estimate state fixed effects μ_i and other controls.

I allocate the increase in aggregate Social Security payments across states in proportion to that state's share of Social Security payments a year before (see Appendix 1 for details). This depends on the stock of retirees in each state, which is both slow-moving and largely predetermined. The average size of the increases is 0.2% of quarterly labor income, though they are highly heterogeneous over states and time (see descriptive statistics in Appendix Table

[9]Romer and Romer (2016) and Wilcox (1989) find smaller or insignificant (respectively) responses of consumption from 1975 onward. An earlier version of this paper used Wilcox's (1989) shorter sample of pre-1975 Social Security increases, which produced broadly similar results.Romer and Romer's sample only covers benefit increases for existing Social Security recipients, and so excludes expansions in eligibility and other rule changes.

[10]For example, a permanent \$1 increase on a \$10 monthly payment starting on June 1st will become a 3.3% increase in Q2 and a ≈6.6% increase in Q3.



Notes: Panel A: Each point is the mean growth rate residual or mean Social Security increase residual (share of labor income) of 30 quantiles (controlling for state and time fixed effects). See Section 2.2.2. Panel B–D: each point is the state growth rate of labor income PC or GDPPC, or size of the transfer in the quarter (see Section 2.3.2). Sources: BEA, Romer and Romer (2016).

Figure 1. Scatter Plots: Transfers and Economic Growth

1). The largest permanent increases were in 1972Q4 (a 20% increase in benefits) of around 1.45% of quarterly labor income in West Virginia, Arkansas and Florida, which have a large number of retirees as a share of the population (Appendix Figure 1.1 left side). In contrast, the increase in transfers to residents of Alaska was only 0.2% of labor income in the same quarter.

2.2.2. ***Graphical Evidence.*** Figure 1 Panel A gives a visual representation of the strength of the relationship between Social Security increases and growth in per capita labor income. As there are many permanent Social Security changes, I group permanent transfer change residuals into 30 bins based on their size (across different quarters and states), and plot the average transfer size in each bin (x-axis) against the average growth rate residuals of per capita labor income for that bin (y-axis). Residuals are calculated after removing time and state fixed effects, as in the parsimonious specification (quarters without any Social Security increases are dropped). One can see that the relationship is strongly positive, with a slope of about 2, and also statistically significant at the 5% level. Moreover, this relationship isn't driven by one or two bins—in fact, an outlier-robust regression increases the slope and significance—and the vast majority of bins are in the NE and SW quadrants.

2.2.3. ***Main Regression Results.*** Table 1 shows that a permanent \$1 increase in transfers to the residents of a state tends to increase that state's relative labor income by around \$1.5 contemporaneously, though the size of multiplier varies slightly with the specification. Column 1 shows the most parsimonious specification: It includes time and state fixed effects, but no other controls, and yields a relative permanent transfer multiplier of 1.5, significant at the 1% level. Cluster-robust standard errors are around 0.5.

2.2.4. ***Robustness Tests and Threats to Identification.*** Romer and Romer (2016) note that during the 1952–74 period there were three temporary transfers to Social Security recipients (in 1965, 1970, and 1971), which reflect back payments for delayed increases in permanent Social Security benefits. As these temporary increases tended to coincide with the increases in permanent Social Security benefits—and have similar eligibility requirements—it is important to control for them. Column 2 includes these temporary transfers (and their first lag) as a control. The contemporaneous multiplier on permanent transfers is almost identical to those in Column 1, and the temporary Social Security increases and first lag are insignificant (coefficients of -0.58 (t=-1.33) and -0.15 (t=0.26), respectively). I do not put much weight on these these temporary transfer estimates—relative to those in Table 2—because they are imprecise: The standard errors are around five times larger than a similar specification using stimulus payments (see Table 2, Panel A, Column 1).

Another concern is that multipliers might be sensitive to specific years (or states), and are not reflective of a general relationship. In Appendix Figure

3.3, I test this by dropping individual states or years one by one. No individual states are influential, but the multiplier does move by more than one standard error if either 1952 or 1972 are excluded. Table 1 Column 3 thus reports the baseline specification excluding both these influential years, which has little effect on the multiplier as the effect of these years is mostly offsetting (standard errors are slightly larger, and significance is now at the 5% level).

Controlling for omitted variables. One reason that states might be growing faster than their peers is faster population growth—for example, retirees flock to Sun Belt states, which increases Social Security payments in those states. My main specification already has the dependent variable in per capita terms, but that imposes that labor income and population increase in proportion, so in Column 4 I control for population growth separately. The coefficient of population growth is negative (-0.37) and is highly statistically significant (p-value=0.000) (not reported). This is unsurprising, as standard growth models predict that higher population growth reduces per capita output growth. Controlling for population growth increases the size of the permanent transfer multiplier to 1.62 (significant at the 1% level).

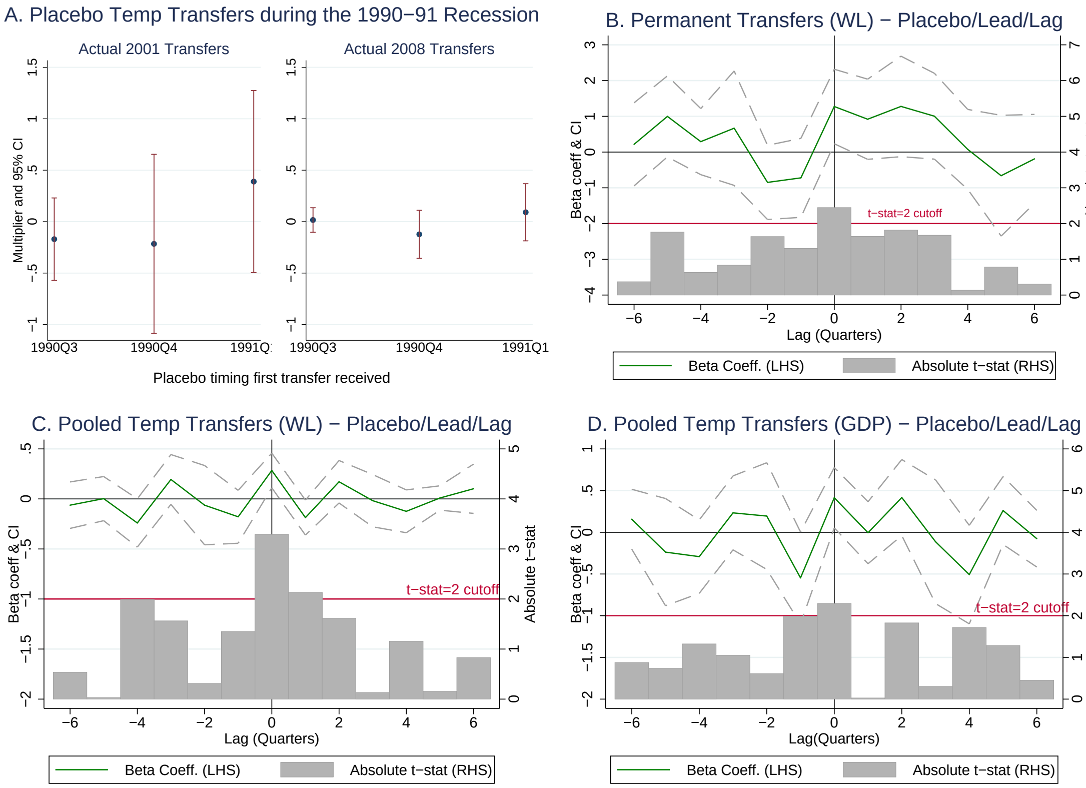
The early 1970s were a time of some of the largest increases in Social Security payments, but also the 1973 oil shock and two recessions. National recessions and oil prices are naturally subsumed into time fixed effects, but (i) states with cyclically sensitive industries might be more sensitive to national GDP growth, and (ii) oil-producing states might be stimulated by higher oil prices—and those states might have coincidentally received higher or lower transfers. To test this (i) Column 5 includes US GDP growth $\times$ state fixed effects, which reduces the multiplier to 1.27 (significant at the 5% level; I also keep the significant control for population growth). Given that both US GDP growth $\times$ state FEs and population growth are important controls, this is the "with controls" specification used for placebo tests (Figure 2) and used to compare with the theoretical model in Section 4. Column 6 adds controls for state-specific sensitivities to real log oil prices (50 contemporaneous variables and 50 first lags, which flexibly controls for price levels and changes), which result in a permanent transfer multiplier of 1.68 (significant at 1%). To cover any other general time-varying state-specific covariates, Column 7 instead adds quartic state-specific polynominal trends. This yields a multiplier of 1.65 (significant at the 1% level).

Alternative specifications. In Column 8 I estimate an instrumental variables (IV) specification, in which all Social Security transfers—which are potentially endogenous—are instrumented using my sample of exogenous Social Security increases (from Romer and Romer 2016). The multiplier is around 1.9, significant at the 1% level (the first-stage F stat is above 500).[11]

Placebo tests and leads/lags Another way to test for omitted variable bias is to run a placebo test in which the timing of the permanent transfers are counterfactually moved backward or forward by up to six quarters. This can help to detect spurious results if coefficients are more significant at other times, though it can also pick up anticipation effects (for short leads) or delayed effects (for short lags). For permanent transfers, the results are shown in Figure 2 Panel B (using the "with controls" specification from Table 1). One can see that the largest t-statistic is at t=0 (when the actual transfers occurred), and all other leads and lags are insignificant at the 5% level (t-statistics less than the red cutoff of 2), which is what one would expect to see.

Looking at the quarters immediately surrounding the transfer payment, the estimated multiplier (green line) appears to be negative in anticipation of the Social Security rate increase, and persistently positive in the few quarters following the increase. Romer and Romer (2016) find that Social Security increases were typically legislated roughly a quarter in advance of payment, though they find no evidence of an increase in aggregate consumption at that time. Anticipated demand shocks in New Keynesian models often reduce output as firms raise prices and markups in advance, a possible rationale for negative leads (the persistent growth in the following quarters is harder to explain). However, I do not want to emphasize those results given their insignificance and general lack of robustness (not reported).

[11] A similar, but slightly larger, IV coefficient is unsurprising as a $1 increase in my exogenous permanent Social Security transfers series increases all BEA Social Security payments by about 80c (after controlling for time and state fixed effects). The strong first-stage relationship is plotted in Panel B of Appendix Figure 3.1, and provides an additional validation of the cross-state allocation of transfers and Romer and Romer's aggregate narrative.



Notes: Panel A: Multipliers and 95% confidence intervals from regressing real labor income growth during 1990–91 recession on actual state transfer path from 2001 or 2008. "With controls" specification. Sample 1990–97. Panels B–D: Regressions of growth on transfers as in Table 1 (Col. 5), Table 2 (Col. 3, Panel A), and Table 2 (Col. 3, Panel B) respectively, but moving the growth variable backward/forward by up to 6 quarters. 2x standard error bounds shown (dotted lines).

FIGURE 2. Placebo/Lead/Lag Tests

Other robustness tests Appendix Table 3, Panel A presents several robustness tests with different standard errors (homoskedastic, robust without clustering and Conley's (1999) standard errors allowing for spatial error correlation)—with transfers always significant at the 5% level. The table also adds back in previously dropped extreme outliers, where |growth|>20% (annualized). While that reduces significance to a p-value of 8%, it also increases the estimated multiplier to 2. Dropping the two smallest states (Alaska and Wyoming), with a combined population of <1m in 1980 and with extremely volatile growth rates, restores significance to the 1% level and increases the multiplier. See Appendix 3.01 for further details.

Additional dynamics Appendix Table 4 (Panel A) adds extra dynamics to the specification in Equation 1. Column 1 shows that there is little persistence in state growth rates (after state and time fixed effects), such that a lagged dependent variable is small and insignificant. However, the estimated contemporaneous transfer multiplier does fall to 1.1, but is still significant at the 5% level. Two additional lags of permanent transfers (Column 2) are insignificant (though positive), with little effect on the estimated contemporaneous multiplier. The insignificant dynamic terms justify the contemporaneous specification in Equation 1.

Comparison with literature Wilcox (1989) and Romer and Romer (2016) find (among other things) that increases in permanent Social Security benefits significantly increase consumption at the aggregate level. This is consistent with my finding of large income multipliers in the cross-section, though their results do not imply mine and vice versa. A positive MPC for a transfer is consistent with a positive, negative, or zero cross-region transfer multiplier (depending on openness and price/wage stickiness; see Section 3), and likewise a sizable cross-region transfer multiplier is consistent with a large or small aggregate consumption response (depending on monetary policy).

2.3. **Temporary Transfers.**

2.3.1. ***Description of Transfer Packages.*** I consider two temporary stimulus payments, in 2001 and 2008.

2001 EGTRRA stimulus payments In 2001Q3, the Bush administration transferred $38bn to households in a one-off payment as part of the broader Economic Growth and Tax Relief Reconciliation Act (EGTRRA). Individuals paying net taxes mostly received $300 per capita, though unlike the 2008 transfers there was no payment for those with no tax liability, and no phase out for those on high incomes.

I allocate the $38bn in transfers across states using IRS state-level data on individual tax returns for the 2000 tax year (see Appendix 1 for details). Exploiting the randomization of payment dates, Johnson et al. (2006) found

Table 1. Permanent Cross-region Transfer Multipliers

Dependent Variable: Δ(WLpc)	(1) Parsi-monious	(2) Control Temp SS Tr	(3) Excl infl-uential years	(4) Incl pop growth	(5) With Controls (US GDP gr X State FE & Pop)	(6) Incl Oil Prices	(7) State quartic trends	(8) IV
Perm.SS Transfers	1.48	1.44	1.44	1.62	1.27	1.68	1.65	1.89
	(0.48)	(0.50)	(0.69)	(0.48)	(0.52)	(0.50)	(0.47)	(0.58)
Observations	4,407	4,363	4,045	4,407	4,407	4,363	4,407	4,407
State & Qtr FE	YES	YES	YES	YES	YES	YES	YES	YES
Temp SS transfers		YES						
Excl 1952 & 1972			YES					
Incl. State Pop Growth				YES	YES			
US GDP gr. X state FE					YES			
log(Oil) X state dummies						YES		
State FE X Quartic Trend							YES	
First Stage F-Stat								523

Notes: Dependent variable is the quarterly growth rate of real per capita labor income. Regressor is the change in permanent Social Security transfers, as a share of lagged labor income (Columns 1–7). All specifications include quarter and state FEs, based on Eq 1. Columns: 1) no other controls; 2) controls for contemporaneous temp Social Security (SS) transfers and first lag; 3) removes influential years (1952 & 1972); 4) adds for population growth; 5) adds US GDP growth $\times$ state FEs (and population growth); 6) adds State FE$\times$ $log(OilPr_t)$, $log(OilPr_{t-1})$; 7) adds state FE $\times t$, t^2, t^3, t^4; 8) Instrument change in BEA SS transfers by permanent SS increases. Robust std errors in parentheses (clustered by state). Outliers $>$20% (annualized) dropped.

that 20–40% of the payment was spent on non-durables in the months that it was received, with a higher MPC for the poor and credit constrained, and no response for durables.

2008 Economic Stimulus Act Around \$95bn was transferred to households as one-time payments in 2008Q2–Q3 as part of the Bush administration's Economic Stimulus Payments (ESP) (Parker et al. 2013). This package had two main components: (i) \$300 per capita payments made to those paying no net taxes but with at least \$3000 in eligible annual income (around \$30bn, which I call the *low-income rebate component*) and (ii) \$600 per capita payments made to those paying net taxes with a phase out for those earning over \$75,000 (around \$65bn, which I call the *middle-income tax refund* component). Most of the effects of the 2008 ESP turn out to be driven by the low-income component. Parker et al. (2013) exploited randomization in the timing of the ESPs and found that about 12–30% of the payments were spent on non-durable consumption in the months they were received (50–90% including durables).

I allocate the payments for the low-income rebate component using the cross-state allocation from BEA (2009), which is primarily based on the geographic distribution of recipients of refundable Earned Income Tax Credits (EITCs).

According to the BEA's allocation, about 95% of the refundable 2008 low-income ESP were paid out in 2008Q2 (as against 85% of the whole package).[12] Eligibility for the 2008 stimulus payment was made based on 2007 income, as full-year 2008 income was not known in 2008Q2. I allocate the middle-income tax rebate payments across states using IRS data on 2007 income tax returns by state and income combined with eligibility rules for the tax rebate (see Appendix 1 for details).

2.3.2. ***Graphical Evidence.*** Figure 1 Panel B shows the relation between total transfers in 2008Q2 as a share of labor income on the x-axis, and the growth of labor income on the y-axis. States that receive larger transfers (as a share of labor income) tend to grow faster contemporaneously, with a "relative multiplier" (line slope) of about 0.28 (t-stat=2.0), which is remarkably similar to the pooled temporary transfer multiplier estimated in Table 2. The cross-state variation in transfers is striking: The 2008 ESP ranges from around 2.3% of quarterly labor income in Connecticut (CT) to 6.4% of quarterly labor income in Mississippi (MS). Two factors are at play here: First, the level of per capita labor income is much lower in MS than CT, and so fixed dollar payments are mechanically more important. Second, much of the variation is driven by the low-income rebate (those paying no net taxes), which is focused toward poor states. Of the 4.1 percentage point gap in stimulus transfers in 2008Q2 between MS and CT, 3.4 percentage points are due to the cross-state allocation of the low-income component. Figure 1 Panel C shows the same stimulus package and quarter, but for quarterly growth in GDP per capita rather than labor income on the y-axis (with transfers as a share of quarterly state GDP on the x-axis). As before, there is a positive relationship between

[12]According to the BEA (2009), for the low-income component, $28bn was paid in in 2008Q2 and $1.35bn in 2008Q3. This is consistent with a high percentage of recipients receiving their ESPs by direct deposit, which is faster than by check. IRS data show that around 80% of those with refundable EITCs filed electronically. Direct deposit payments were made in the first half of May 2008, leaving plenty of time for output to be affected in 2008Q2. Combined with Parker et al.'s (2013) quarterly profile, this suggests a middle-income payout of around $50bn in 2008Q2 and $14bn in 2008Q3. All of the 2001 stimulus payments were made in 2001Q3. Hence $\Delta tr_{i,t} > 0$ for 2001Q3 and 2008Q2, $\Delta tr_{i,t} < 0$ for 20081Q4, 2008Q3-Q4 (small in 2008Q4), and $\Delta tr_{i,t} = 0$ for all other quarters. The rest of the 2001-08 or 2005-08 sample helps to estimate state FEs and controls.

the size of the transfer received and quarterly GDP growth, though the relative multiplier is now larger at around 0.9 (t-stat=2.5).

Figure 1 Panel D displays the relationship between the 2001 EGTRRA stimulus payments (as a share of labor income, x-axis) and contemporaneous per capita labor income growth in 2001Q3 (y-axis). Again, states that received a larger payment (as a share of labor income) tended to grow faster contemporaneously, though here the multiplier is large at 0.98 (t-stat=2.9).

2.3.3. ***Main Regression Results.*** Table 2 presents regressions of growth in per capita labor income (Panel A) or per capita GDP growth (Panel B) on the size of the transfer at the state level over 2001–08 or 2005–08 (respectively). Unlike the scatter plots, the regressions control for state and time effects, and impose that the withdrawal of transfers the following quarter reverses any earlier increase in labor income or GDP growth.

Column 1 reports a parsimonious specification with only time and state fixed effects, and pools across the 2001 and 2008 temporary transfers. It suggests that a relative $1 increase in temporary transfers to a state increases relative per capita labor income in that state by $0.24, significant at the 1% level (Panel A). When the dependent variable is per capita GDP growth, the multiplier is larger at 0.39, significant at the 5% level. I fail to reject the equality of multipliers (at the 5% level).

2.3.4. ***Robustness and Threats to Identification.*** One of the first concerns from looking at Panels B and C of Figure 1 is that results might be driven by Mississippi (MS) as an outlier. In Column 2 of Table 2, I drop MS and show that it has little effect on estimated multipliers, which are still significant at 5%. In Appendix Figure 3.2, I drop all states one by one and do not find any substantially influential states.

Controlling for omitted variables. Unlike the permanent Social Security increases, the 2001 and 2008 transfers were explicit countercyclical policies implemented during aggregate downturns. While the aggregate business cycle is subsumed into the quarter fixed effects, different states might have different sensitivities to the national cycle; for example, if they specialized in cyclically sensitive industries like manufacturing. If those states happened to receive relatively small transfers, that could drive a positive multiplier. I address this concern in two ways. First, the regression in Table 2 Column 3 includes US GDP growth × state fixed effects, which increases the estimated multiplier marginally but doesn't affect significance.[13] This is the "with controls" specification used for placebo tests and comparison with the theoretical models. Second, in Column 4 I interact time FEs with the average manufacturing share of GDP in that state over 2001–08, which allows manufacturing-intensive states to grow more slowly in recessions.[14] The estimated multipliers are shown in Column 4 and are almost unchanged (significant at the 5% level). Recessions also reduce oil prices, which might hit oil-producing states relatively harder. As above, I interact state FEs with the contemporaneous and first lag of log real oil prices in Column 5 of Table 3. Estimated multipliers are almost unchanged, and statistical significance is unchanged or stronger. To cover any other general time-varying state-specific covariates, Column 6 instead adds quartic state-specific polynominal trends (though some are collinear and dropped). Transfer multipliers (and their significance) are almost unchanged.

Placebo tests. A placebo test is an alternative way to test the concern that high-transfers states always grow faster during recessions, and hence the estimated relationship between transfers and growth estimated is spurious. In Panel A of Figure 2, I regress growth rates during the 1990–91 recession on the 2001 and 2008 transfers. According to the NBER's definition, that recession lasted three quarters, so there are three ways to implement the placebo, with placebo payouts during 1990Q3, 1990Q4, or 1991Q1. In all three cases the estimated multiplier is insignificant, and usually is close to zero.[15]

We can also counterfactually move the transfer forward or backward for a few quarters (as estimated above), which can help to detect spurious results, or pick up anticipation effects (for short leads) or delayed effects (for short lags). Results are shown in the bottom panels of Figure 2. One can see that

[13]That specification controls for population growth, which is always insignificant in the 2000s sample and has little effect on estimated multipliers.

[14]Using the share of durable-goods manufacturing instead produces similar results (not shown).

[15]I thank an anonymous referee for suggesting this placebo test. The sample period for these regressions is 1990–97, and I include time and state fixed effects, as well as controls for sensitivity to national GDP growth and population growth.

Table 2. Temporary Transfer Multipliers (Pooled)

	(1) Parsi-monious	(2) Excl MS	(3) With Controls (US GDP gr X State FE & Pop)	(4) Incl Manf GDP X time FE	(5) Incl Oil Prices	(6) Incl Quartic State trend	(7) IV
Panel A: Dependent Variable: Quarterly Growth Real Labor Income per capita (2001-08)							
Temp Tr	0.24	0.23	0.28	0.22	0.25	0.23	0.21
Multipliers	(0.08)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.07)
Obs	1,588	1,556	1,588	1,588	1,588	1,588	1,588
Panel B: Dependent Variable:Quarterly Growth Real GDP per capita (2005-08)							
Temp Tr	0.39	0.43	0.41	0.41	0.42	0.41	0.41
Multipliers	(0.16)	(0.21)	(0.18)	(0.17)	(0.15)	(0.17)	(0.14)
Obs	742	727	742	742	742	742	742
State & Qtr FE	YES	YES	YES	YES	YES	YES	YES
US GDP Gr × state dummy			YES				
State Population Growth			YES				
Manf GDP Share × state dummy				YES			
log(OilPR) X state dummies					YES		
State FE X Quartic Trend						YES	
First Stage F-Stat (Labor income / GDP)							322 / 299

Notes: Each cell represents the regression of growth rate of per capita labor income (Panel A) or GDP (Panel B) on the change in temporary transfers with quarter and state FEs and a variety of other controls, indicated at the bottom of the table. Columns: 1) no extra controls; 2) excluding Mississippi; 3) adds US GDP growth × state FEs (and population growth); 4) adds manufacturing share of state GDP × time FE; 5) adds state FE× $log(OilPr_t)$, $log(OilPr_{t-1})$; 6) adds state FE $\times t$, t^2, t^3, t^4; 7) Instrumenting "All other BEA transfers" with constructed stimulus transfer. Robust std errors in parentheses (clustered by state). Outliers dropped.

the largest t-statistic is always at t=0 (when the actual transfers occurred), and there is little evidence of consistently positive (or negative) coefficients at other times. However, there are some marginally significant lags at t=-4 and t=1 (WL specification), and t=-1 (lead, GDP specification). The negative first lag in the GDP specification seems to be due to the overlap of the placebo withdrawal of stimulus in 2008Q2 and the effect of the actual stimulus. If I run the same specification and omit 2008Q2 from the sample (when most of the transfers were actually paid) but keep 2008Q1 and the other quarters, the coefficient on the lead halves, and p-value increases to 0.4. Consequently, although both stimulus payments were legislated a quarter in advance, there's little robust evidence of significant negative anticipation effects (including from cross-sectional regressions, not reported).[16] The other marginally significant coefficients are to be expected. Under random allocation with no true effect, the ≈40 placebo regressions in Figure 2 would produce two false positives on

[16]In any case, Ricardian households who respond to news also save temporary transfers. Forward-looking firms would be reluctant to change prices in advance of a temporary shock if there is any cost to changing them back.

average when testing at the 5% level. Moreover, the chance of no false positives is also very low at around 12% (= 0.95^{42} under random allocation).

Alternative Specifications One concern is that my measure of transfer payments could have classical measurement error (resulting in attenuation bias), or that there are similar transfer payments I am not capturing (leading to upward bias). To address this concern, in Column 7 of Table 3 I instrument changes in the endogenous BEA category "All other personal current transfer receipts" (as a share of labor income or GDP) using the low- and middle-income 2008 ESP transfers (as above) as instruments. Multipliers are similar to the parsimonious specification and significant at the 1% level.[17]

Other robustness tests Appendix Table 3 Panels B and C present extra robustness tests with different standard errors (homoskedastic, no clustering, and Conley's (1999) standard errors allowing for spatial error correlation). Most multipliers are still significant at the 5% or 1% levels, except for homoskedastic errors in the GDP specification (but homoskedasticity is a extreme assumption in any case). The table also adds back in previously dropped extreme outliers, where |growth|>20% (annualized). This has little effect on the labor income specification, but makes the GDP specification go to zero (and become insignificant). Dropping the two smallest states (Alaska and Wyoming), with a combined population of <1 million in 1980 and with extremely volatile growth rates, results in multipliers similar to those estimated above, significant at the 5% level (see Appendix 3.01).

Additional dynamics Appendix Table 4, Panel B (Labor income) and Panel C (GDP) adds extra dynamics to the specification in Equation 1. While a lagged dependent variable is significant (Columns 4, 6, 7, and 9), it has very little effect on multiplier estimates, and the coefficient is small and negative (-0.1). Two additional lags of temporary transfers are insignificant, though they raise the estimated coefficient GDP impact multiplier substantially (significant

[17] The BEA classifies payments as transfers if they are at least partially refundable, which includes all 2008 transfers. The 2001 EGTRRA payments were non-refundable and so are classified as tax cuts. Multipliers are similar in the IV specification because growth in "All other personal current transfer receipts" in 2008Q2 is extremely well explained by growth in ESP transfers as constructed above (see Appendix Figure 3.1 Panel B), with first-stage coefficients close to one, and first stage F-stats around 300. This provides an external validation of my construction of the cross-state allocation of the 2008 ESPs.

at 5%), mostly because the extra lags offset the fall in GDP growth in 2008Q3 as transfers are withdrawn (discussed further in Section 2.3.5)

Parallel trends Diff-in-diff studies often include a graph demonstrating that the outcome variable exhibits a similar trend in high vs low treatment states pre-treatment, with a gap in the outcome variable opening up post-treatment. Appendix Figure 3.4 shows exactly this pattern for 2001 transfers and 2008 transfers (for GDP). However, for 2008 transfers (labor income) the results are not so clear: The gap is small and temporary—lasting only a quarter—and there is some difference in labor income trends in 2007. However, confidence intervals are wide and typically overlap (except for 2001 transfers post-treatment, not reported). This is unsurprising given the small number of states in each group and substantial within-group variation in transfers that is discarded. Moreover, a temporary gap for temporary transfers is more consistent with theory, as discussed in the next section.

2.3.5. *Heterogeneity by transfer policy and quarter.* So far I have presented regression results that restricted the temporary transfer multiplier to be identical across quarters where transfers are paid or withdrawn and across different individual transfer policies. In this subsection I relax those assumptions, with results presented in Table 3 that also help to shed light on the mechanisms driving the pooled results.

Heterogeneity by quarter Motivated by theory, the specification in Table 2 and Equation 1 imposes a symmetric effect of transfers: If increasing temporary transfers leads to a certain increase in output, then withdrawing those transfers should lead to the same-size fall in output. Columns 1 and 6 in Table 3 test this by allowing the pooled transfer multiplier to vary quarter by quarter, but keeping the same pooled specification and sample. Despite substantial heterogeneity in estimated coefficients, I fail to reject the restriction that all payment and withdrawal multipliers are equal, which justifies the pooled specification used above. Moreover, 2008 (labor income specification) multipliers are around 0.25 for both payment and withdrawal quarters—and are remarkably similar to pooled estimates above.

Although not statistically different, point estimates are sometimes larger when transfers are paid than when they are withdrawn. This is the case for

2008Q2 (GDP) and 2001Q3 (labor income), where the multipliers upon payment are around 0.8, but multipliers on withdrawal are close to zero. This means states receiving a larger transfer continued to have higher relative income, even after that transfer was withdrawn (as in the parallel trends plots in Appendix Figure 3.4). It is not obvious which mechanism would explain such a result, which is another reason I prefer pooled specification.

Simple quarter-by-quarter cross-sections (Columns 2–5 and 7–8)—the same as the scatter plots in Figure 1 in the payment quarter—generally produce multipliers similar to the panel regressions in Columns 1 and 6 which allow the multiplier to vary quarter-by-quarter. However, the cross-sections are more prone to omitted variable bias because they do not control for state specific trends (such as Sun Belt states growing faster than Rust Belt states), or any other covariates—and should be treated with caution. For instance, larger multipliers in the cross-section in 2001Q3, smaller multipliers in 2008Q3 (GDP), and differences in significance could be due to these factors.

Heterogeneity by transfer component In Columns 9 and 10, I break down pooled transfers into three components described in Section 2.3.1—2008 refundable (low-income) rebates, 2008 (middle-income) tax refunds, and 2001 transfers—using the same sample period and specification as the pooled results. Despite substantial heterogeneity in estimated multipliers for different policies in Columns 9 and 10 of Table 2, I fail to reject the restriction that coefficients are equal, justifying the pooled specification above.

With labor income as a dependent variable (Column 9), the pooled results above seem to be driven by the 2008 low-income rebate, with a relative multiplier of 0.33 (significant at the 1% level)—similar to the pooled multiplier from Table 2. The 2001 EGTRRA stimulus payments have the exact same multiplier of 0.33, but have much wider standard errors (leading to a loss of significance). The 2008 middle-income tax refund seemed to have little effect on labor income growth. For GDP growth as a dependent variable (Column 10), the results are consistent but even starker: The coefficient on the low-income rebates is even larger at around 0.77 (significant at the 1% level), which is almost double the pooled coefficient, but the multiplier on the middle-income tax refund is negative and insignificant.

Why might this be the case? First, it is not all that surprising that the low-income rebate has the largest multiplier, as low-income households are usually thought to be more likely to be financially constrained. Parker et al. (2013) found that low-income households had a large and statistically significant MPC out of the same 2008 transfers. This is consistent with Johnson et al.'s (2006) earlier results that the 2001 transfers were more likely to be spent by low-income households. The small and insignificant multiplier on the middle-income transfers might be because a larger fraction of this payment was saved, as higher-income households were less likely to be financially constrained

Another important difference between the 2008 middle-income tax rebate and the 2001 and 2008 low-income rebate was size: the 2008 mid-income rebate was around $600 per recipient, double the other transfers of $300 per recipient. Other papers have found two effects of transfer size. First, there is some mixed evidence that larger payments are more likely to be saved.[18] But more importantly, larger transfers can be spent on "big ticket" durable goods like cars—both Parker et al. (2013) and Hausman (2016) report a large effect on sales of autos and related goods. Crucially, these durable goods are likely to be produced outside the state, which means that they provide a limited boost to local demand, and hence generate a small relative transfer multiplier—even if the transfer were consumed.

2.4. **Real or Nominal Multipliers?** Although I deflate all variables by national inflation (PCE)—and so are real in a time-series sense—the estimated multipliers are nominal in the cross-section. As GDP and labor income are not deflated by *local* prices, we do not know whether an increase in the value of GDP in a state (relative to the national average), reflects an increase in local prices or local quantities. This is a common problem: most papers that estimate cross-region multipliers deflate using a measure of national prices (e.g., Acconia et al. 2014, among others). Ideally, one would like to test whether local producer prices increase in response to a transfer, but unfortunately there are no official state-level price data to test this hypothesis.

[18]Hseih (2003) finds that households that saved large Alaska permanent fund dividends tended to spend small tax refunds, which is consistent with evidence from Kaplan and Violante (2014) using a model with fixed costs of reallocating liquidity. However, Hausman (2016) finds that very large veterans' dividends were mostly spent, albeit on durables.

TABLE 3. Temporary Transfer Multipliers—Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Heterogenity type:				Panel A: By Quarter Paid/Withdrawn					B: By Transfer Policy	
Dependant Variable:		Growth Qtrly Labor Income PC (WL)				Growth Qtrly GDP PC			WL	GDP
Sample:	2001-08	2008Q2	2008Q3	2001Q3	2001Q4	2005-08	2008Q2	2008Q3	2001-08	2005-08
Ch Transfer 2008Q2	0.26	0.28				0.89	0.9			
(paid)	(0.13)	(0.14)				(0.37)	(0.36)			
Ch Transfer 2008Q3	0.24		0.22			-0.07		0.25		
(withdrawn)	(0.12)		(0.12)			(0.26)		(0.29)		
Ch Transfer 2001Q3	0.75			0.98						
(paid)	(0.35)			(0.34)						
Ch Transfer 2001Q4	-0.11				-0.33					
(withdrawn)	(0.38)				(0.35)					
2008 Rebates									0.33	0.77
(low-income)									(0.09)	(0.27)
2008 Tax Refunds									-0.07	-0.56
(middle-income)									(0.29)	(0.54)
2001 Transfers									0.33	
									(0.24)	
State & Qtr FE	YES					YES			YES	YES
P-value Equal Coeffs.	46.1%					10.4%			40.4%	5.5%
Observations	1,588	50	50	50	50	742	48	50	1,588	742

Notes: Dependent variable is growth rates of real labor income per capita (Columns 1–5, 9) or real GDP per capita (Columns 6–8, 10). Regressor is the change in temporary transfers, as a share of labor income or GDP (as above), by quarter (Panel A, Columns 1–8), or type of transfer policy (Panel B, Columns 9–10; see Section 2.3.5). Columns 1, 6, and 9–10 are panel estimates with quarter and state FEs, based on Eq 1; others are simple cross-sections only. Robust std errors in parentheses (clustered by state). Outliers > 20% (annualized abs value) dropped.

Instead, I construct my own proxies of quarterly state-level prices from a variety of sources. In sum, I find little evidence that the transfers studied above increase local prices (see Appendix 3.1 for details). Regressions of the quarterly state inflation proxy on transfers are always insignificant in the 2000s, and over 1952–74 are always negative and insignificant once I control for the state-specific effects of oil price shocks. New Keynesian models predict little movement in prices in the short term (as they are sticky), and neoclassical models predict (if anything) that high demand increases local prices. Nonetheless, I do not put too much weight on these results, given that I am trying to explain only a proxy for state inflation—which requires many assumptions and approximations—rather than an official measure.

3. Understanding Cross-region Transfer Multipliers in an Analytical Model

This section illustrates the analytical determinants of the cross-region transfer multiplier estimated in Section 2: when it will be large, small, or negative, and how it differs from other cross-region purchase multipliers in the literature and the MPC. To do this, I use two simplified open-economy models: (i) an "Ultra Keynesian" model with fixed prices/wages and a fraction of hand-to-mouth agents, and (ii) a canonical neoclassical model. Both simple models are special cases of a more general full New Keynesian (NK) model—which is used in Section 4 to quantitatively interpret the empirical estimates. This section also relates transfer multipliers to the difference between federally financed spending (like in Nakamura and Steinsson 2014 (NS)) and locally financed spending (like Clemens and Miron 2012 (CM)). As with the empirics, I focus exclusively on the impact multiplier $\mathcal{M}$—the real increase in GDP generated by a \$1 transfer or purchase in the first quarter.

Model overview. The full model is similar to NS's separable preferences, incomplete markets New Keynesian model (Section IV, D of their paper), with two main differences: (i) a share ω of households that can't borrow/save and consume their income hand-to-mouth (the remaining fraction $1-\omega$ are Ricardian and borrow/save through a risk-free bond) and (ii) wages are sticky and can not be changed each quarter with probability θ_w, as in Erceg et al. (2000) (θ_p is the analogous Calvo stickiness of prices).[19] Hand-to-mouth households are needed to match the multiplier on temporary transfers, and sticky wages are needed to get close to the multiplier on permanent transfers.[20] The model is also similar to Gali and Monacelli (2005) but with sticky wages, hand-to-mouth households, and incomplete markets (for Ricardian households).

[19] As is common in models with hand-to-mouth households and sticky wages, (i) forward-looking nominal wage setting only takes into account of the Ricardian household's labor-leisure condition, and (ii) the labor supply of the two households move proportionately. This shuts down the wealth effects for hand-to-mouth households receiving transfers in Giambattista and Pennings (2017), leaving the effect of transfers on demand as the dominant force.

[20] I also abstract from capital, and so assume output is produced using only labor with constant returns (equivalent to $a = 1$ in Nakamura and Steinsson 2014).

As in NS, the monetary union consists of a small home region (representing a small US state with population n) and a large foreign region with population $1-n$, each of which produces their own variety of imperfectly substitutable goods that can be consumed locally or abroad, or used for local government purchases (which are not valued by households). Given that the model is relatively standard, further details are presented in Appendix 4.

Fiscal policy. I study cross-region untargeted lump-sum transfers from households in the rest of the monetary union to members of the small home region in proportion to their population share (i.e., hand-to-mouth agents receive a fraction ω). These transfers represent those from the federal government, as in the empirical analysis. I also discuss self-financed (locally financed) untargeted lump-sum transfers from home Ricardian agents to all home households in proportion to their population share.[21] Due to Ricardian equivalence, this is equivalent to a debt-funded transfer with future lump sum taxes falling on home Ricardian agents—which forms a natural benchmark. Both transfers and purchases are expressed in deviations from steady state as a share of GDP, and both fiscal shocks follow an AR(1) process with persistence ρ.

3.1. Analytical Impact Multipliers in a Simple "Ultra Keynesian" Model.

The simple "Ultra Keynesian" model is the limit of the full NK model when (i) prices and wages become perfectly sticky ($\theta_w, \theta_p \to 1$) and (ii) the home region becomes infinitesimal ($n \to 0$). Transfer impact multipliers—the response of output in the first quarter, denoted $\mathcal{M}_{Tr}$—are presented in Proposition 1, and are decomposed into self-financed (SF) and pure cross-region components. Here α is home bias in consumption and β is the household's discount factor. All proofs are in Appendix 7.

Proposition 1. *"Ultra Keynesian" model transfer impact multipliers are:*

(i) self-financed: $\mathcal{M}_{Tr}^{SF} = [\underbrace{\omega \times 1}_{MPC(transfer)} - \underbrace{\omega \times (1-\beta)/(1-\beta\rho)}_{MPC(tax)}] \times \underbrace{\alpha/(1-\alpha\omega)}_{Local\,GE}$

[21]As the home region is small, results are almost identical when the federal government levies taxes proportionately on the whole monetary union. Ricardian households represent higher-income households (that pay more tax) and are less likely to be liquidity constrained. The large foreign region is almost unaffected by the shock, and so acts as a control group that makes this exercise equivalent to that in the empirical regression in Section 2.

(ii) cross-region: $\mathcal{M}_{Tr}^{CR} = \mathcal{M}_{Tr}^{SF} + \underbrace{(1-\beta)/(1-\beta\rho)}_{MPC(transfer)} \times \underbrace{\alpha/(1-\alpha)}_{Local\,GE}$

The self-financed transfer multiplier, $\mathcal{M}_{Tr}^{SF}$, is the initial increase in home output due to an untargeted transfer, funded by lump-sum taxes on home Ricardian agents. It has two components. The first term is the increase consumption in the absence of general equilibrium effects, which is the difference between the MPC of hand-to-mouth households (one) times their population share ω, less the fall in consumption of Ricardian households paying the tax. Being Ricardian, the latter is just the annuity value of the lump sum taxes ($(1-\beta)/(1-\rho\beta)$), scaled by the fraction of the transfer received by hand-to-mouth agents ω ($1-\omega$ of the transfer is from Ricardian agents to themselves and so drops out).[22] This difference is largest for once off payments ($\rho = 0$) and approaches zero as the transfer becomes perfectly persistent ($\rho \to 1$). If $\omega = 0$, then $\mathcal{M}_{Tr}^{SF} = 0$. The micro literature on the MPC (e.g. Johnson et al. 2006) only identifies $MPC(transfer) = \omega$, which is only one part of the overall transfer multiplier.

Second, this increase in demand is then amplified (or dampened) by local GE effects $\alpha/(1-\alpha\omega)$. Only a fraction α of any increase in consumption will be spent on local goods. In the absence of home bias in consumption ($\alpha = n = 0$), the increase in aggregate demand will be outside the home region, and so $\mathcal{M}_{Tr}^{SF} = 0$, even if the MPC is large.[23] This is a realistic concern for some transfer policies, given evidence by Parker et al. (2013) and Hausman (2016) of large transfers being spent on autos, which are likely not locally produced.

The cross-region transfer multiplier $\mathcal{M}_{Tr}^{CR}$ (as estimated in the data) is equal to the self-financed transfer multiplier plus the "pure" cross-region transfer multiplier $[(1-\beta)/(1-\beta\rho)]\,[\alpha/(1-\alpha)]$. The latter is a measure of how much greater the transfer is when federally financed rather than locally financed. Specifically, it is the boost to home output from a \$1 transfer from

[22] $(1-\beta\rho)^{-1}$ is the present value of the transfer and $(1-\beta)$ is the interest on that sum.

[23] $\alpha\omega$ is the MPC on local goods. With fixed prices, output is demand determined. An additional unit of income generates demand for $\alpha\omega$ local goods, which generates extra income and so forth, such that $1 + \alpha\omega + \alpha^2\omega^2 + .. = 1/(1-\alpha\omega)$, which is the traditional Keynesian multiplier.

Table 4. "Ultra Keynesian" Analytical Impact Multipliers

NK Impact Multipliers	A. Purchases	B. Transfers	Diff (A-B)
1. Cross-Region (federal financed)	$1 + \mathcal{M}^{SF}_{Tr} + \frac{\alpha}{1-\alpha}\frac{1-\beta}{1-\beta\rho}$	$\mathcal{M}^{SF}_{Tr} + \frac{\alpha}{1-\alpha}\frac{1-\beta}{1-\beta\rho}$	
	(as in NS)	(this paper)	1
2. Self Financed	$1 + \mathcal{M}^{SF}_{Tr}$	$\mathcal{M}^{SF}_{Tr}$	
	(as in CM)		
Difference (1 - 2)	$\frac{\alpha}{1-\alpha}\frac{1-\beta}{1-\beta\rho}$ ("pure CR transfer multiplier")		

Notes: This table explains the relationship between regional purchase (Column A) and transfer (Column B) impact multipliers, depending on whether those transfers or purchases are financed by the federal government (Row 1) or local lump-sum taxes (Row 2). These are calculated in a simple "Ultra Keynesian" model with fixed prices/wages. $\mathcal{M}^{SF}_{Tr}$ is the self-financed transfer impact multiplier, defined in Proposition 1. β is the household's discount factor, α is consumption home bias, and ρ is fiscal shock persistence.

the rest of the monetary union to home Ricardian agents, which undoes the effects of local lump-sum taxation for a self-financed transfer. Ricardian agents will spend the annuity value of this transfer (it never has to be repaid, unlike locally financed transfers or a loan), with a MPC(transfer) of $(1-\beta)/(1-\rho\beta)$. The local GE effects depend on the fraction α of that is spent on local goods.[24] As such, the pure cross-region transfer multiplier will be close to zero when the economy is very open ($\alpha \to 0$) or for temporary transfers ($\rho \approx 0$), but becomes large as transfers become persistent (see Figure 3 Panel B).

Relation to purchase multipliers. Table 4 compares the cross-region transfer multiplier estimated in this paper (top RHS) with federally financed purchase multipliers (as in NS) or locally financed purchase multipliers as in Clemons and Miron (2012) (CM). With fixed prices, government purchases of home goods have impact multipliers *exactly one unit greater* than those of transfer multipliers, given the same financing (see Proposition 3 in Appendix 7). The reason is that for a purchase, the initial $1 of extra income is generated as payment for an extra unit of output, rather than as a windfall.

[24]The traditional Keynesian multiplier here is $1/(1-\alpha)$. Graphically $\alpha/(1-\alpha)$ is the slope of the income expansion path in Appendix Figure 4.1.

Some theoretical implications. The first implication is that the size of transfer multipliers are very different from those of purchase multipliers, and almost always much smaller. For example, in regions without consumption home bias ($\alpha \approx 0$), the cross-region transfer multiplier could be close to zero, but a comparably financed purchase multiplier would be around one.

Second, in proportional terms, transfer multipliers are more sensitive to changes in persistence ρ, consumption home bias α, and the hand-to-mouth household share ω than purchase multipliers. We see the former in the difference between temporary and permanent transfer multipliers.

Third, a number of papers have found large *federally funded* government purchase multipliers at the regional level (e.g., NS estimate $\approx$1.5), but smaller *self-financed* government purchase multipliers (Clemens and Miron (2012) estimate $\ll 1$). Could financing explain the difference? Table 4 shows that the pure cross-region transfer multiplier is also the difference between federally and locally financed purchase multipliers. With business cycle persistence $\rho = 0.935$ and consumption home bias $\alpha = 0.69$ (see calibration in Section 4.1) the pure cross-region transfer impact multiplier is only around 0.30—even if prices/wages are fixed— and so will not be able to explain large federally financed purchase multipliers.[25]

Related literature. Farhi and Werning (2016) analyze the determinants of regional government purchase multipliers in a similar New Keynesian model. They do not focus on transfer multipliers or produce the cross-region and self-financed transfer multiplier expressions in Proposition 1 (especially with $\omega > 0$). However, they do show that the same pure cross-region transfer multiplier (which they call the "net-present-value transfer" multiplier) is the difference between locally and federally financed purchase multipliers, and show this is one unit less than the outside-financed purchase impact multiplier (Section 8, pp. 2458–59 of their paper). Chodorow-Reich (2018) reports similar expressions for the difference between locally and federally financed purchase multipliers.

[25]These results are consistent with NS (and Farhi and Werning (2016) and Chodorow-Reich (2018)), who make a similar argument numerically.

3.2. **Analytical Impact Multipliers in a Neoclassical Model.** The simple neoclassical model is the limit of the full model when (i) prices and wages become perfectly flexible ($\theta_w, \theta_p \to 0$); (ii) the home region becomes infinitesimal ($n \to 0$); and (iii) there are only Ricardian agents ($\omega \to 0$). The absence of multiple agents means the self-financed transfer multiplier is trivially zero.

Proposition 2. *In the simple neoclassical model, the cross-region transfer multiplier is* $\underbrace{-1/(1+\varphi)}_{LocalGE} \times \underbrace{(1-\beta)/(1-\beta\rho)}_{MPC(transfer)} < 0$.

In a neoclassical model, cross-region transfer multipliers are now negative. The consumption response (MPC) the same as that for pure-cross region transfers in the Ultra Keynesian model: Ricardian agents will spend only the annuity value of the transfer, and so almost all temporary transfers are saved. However, it is the local GE effects that determine the sign, and are very different from the GE effects in the Ultra Keynesian model. Any increase in consumption demand for local goods will result in an increase in their prices, and cause a shift in expenditure toward foreign goods until the extra demand is exhausted. Output will fall as households spend a fraction of their extra income on greater leisure. With my preferences, the size of these wealth effects on labor supply depends on the size of the Frisch elasticity φ^{-1}.

In contrast with transfers, cross-region government purchase multipliers are positive in the neoclassical model (see Proposition 4 in Appendix 7), because they increase demand for local goods without any wealth effects on labor supply (recall that I assume government purchases are not valued by households). Self-financed purchase multipliers are even larger because of wealth effects: Self-financing makes households poorer, which induces them to work more.

4. Quantitative Cross-Region Transfer Multipliers

In this section I show that my full New Keynesian model (with Calvo-sticky prices and wages) is broadly consistent with the empirical impact multipliers estimated in Section 2—more so than with a canonical neoclassical model. I then produce quantitative estimates of self-financed and cross-region transfer multipliers using the full NK model, which are similar to those in the analytical model so long as fiscal shocks are not too persistent.

4.1. **Calibration.** My goal is to investigate the extent to which a reasonably standard NK or neoclassical model can rationalize the multipliers estimated in the data. As such, I calibrate key parameters from the literature. Most parameter are taken from NS, with the neoclassical model being a restricted version of the NK model with flexible prices, flexible wages and no hand-to-mouth households. See Appendix Table 7 for a full list of parameters. I calibrate the population share of the home region $n = 1/50$ to represent a typical US state.

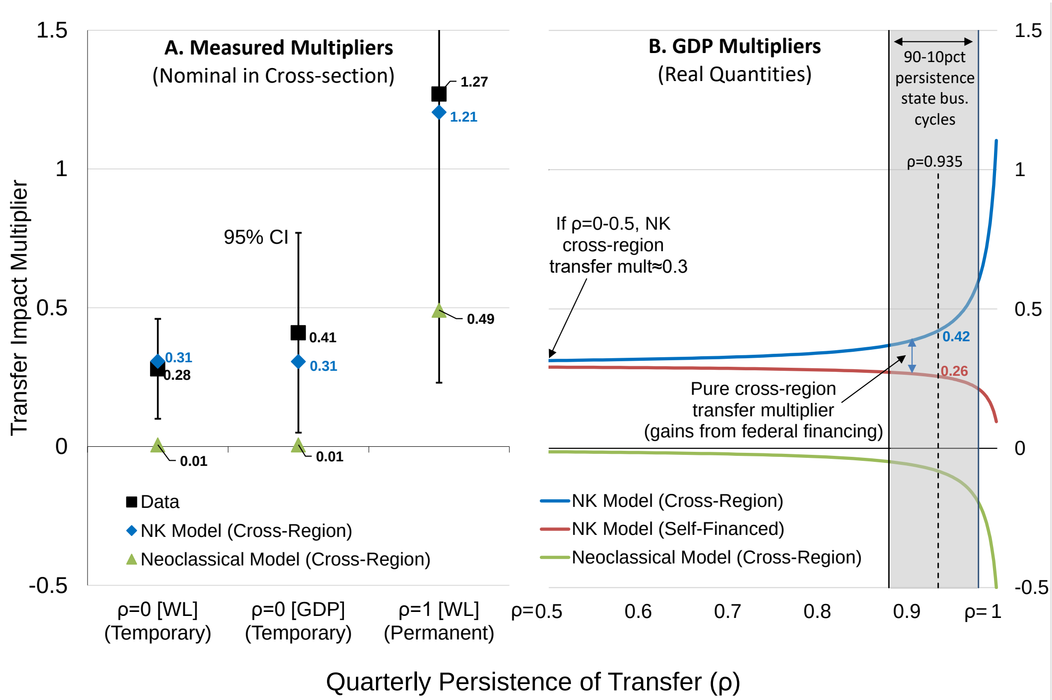
However, there are two main departures from NS. First, my model includes a fraction of hand-to-mouth households, which I calibrate to a third ($\omega = 1/3$) based on evidence from Kaplan et al. (2014); this is in the middle of the range of estimates in the literature. Second, I assume that wages are sticky and adjusted once a year on average, as is standard in the literature ($\theta_w = 0.75$).

4.2. **Comparing Multipliers in the Model and in the Data.** Panel A Figure 3 compares impact multipliers from the empirics with analogous measures from the NK and neoclassical models, measured in a comparable way (discussed below). For temporary transfers and labor income, the cross-region transfer multiplier in the data is around 0.3 (from Table 2, Column 3), which is almost exactly the same as the full NK model. Empirical estimates using GDP (0.4) are only marginally greater than those in the NK model. In contrast, the lack of a response to a temporary transfer shock in the neoclassical model is outside the 95% confidence interval for labor income and GDP multipliers. This is largely because the neoclassical model lacks hand-to-mouth households, and as a result temporary transfers are saved and have little effect on the regional economy. The response of measured labor income to a permanent transfer in the NK model is 1.21, which is extremely close to the 1.27 estimated in the data (from Table 1 Column 5). In the neoclassical model, the measured labor income multiplier for permanent transfers is +0.5%. Although this is within the 95% confidence interval, it a long way (1.5 standard errors) from the point estimate. As such, the data are much more supportive of the NK model, and so I use it for the subsequent analysis.

While the choice of deflator does not matter much for impact multipliers in NK models in a monetary union—prices don't change much in the short term—deflators are important for open-economy neoclassical models with flexible prices. In the empirical section, the measured multiplier is nominal in the cross-section: The difference between the change in nominal GDP (or labor income) for the home region and the change in nominal GDP (or labor income) for the US as a whole which is captured by the time fixed effect (the national CPI used as a deflator cancels out). In the neoclassical model, a permanent 1% of GDP cross-region transfer lowers the quantity of home GDP ($\hat{Y}_h$) by 0.5% due to wealth effects on labor supply ($-(1+\varphi)^{-1} = 0.5$ with $\varphi = 1$ Figure 3 Panel B). However, the local GDP deflator for home goods ($\hat{P}_h$) increase by almost 1%, which is why *measured* home GDP ($\hat{P}_h + \hat{Y}_h$) increases by 0.5% in response to a permanent transfer.[26]

4.3. **Cross-region Transfer Multipliers (Quantities) in the Full Model.** Panel B of Figure 3 presents impact transfer multipliers (quantities) in the full NK model and neoclassical model. As foreshadowed in the results above, the New Keynesian model predicts an increase in output initially by around 30c in the dollar in response to a temporary transfer and increasing to around \$1.1 in response to a permanent transfer. In contrast, the neoclassical model predicts a small fall in output in response to a temporary transfer (most of which is saved), and a sizable fall in output by 50c in response to a \$1 permanent transfer (as households choose to "spend" some of their extra income on extra leisure). Multipliers in the simple analytical model are quite similar, so long as transfers are not too persistent (not shown). For low persistence transfers, the size of the cross-region transfer multiplier is similar to the self-financed component, and so the gains from federal financing (the pure cross-region transfer

[26]Measured labor income and measured GDP can be rewritten as $\hat{Y}_{meas.} \approx (\hat{Y}_h + \hat{P}_h)$ and $(\hat{WL})_{meas.} \approx (\hat{w}_h + \hat{Y}_h + \hat{P}_h)$ using (i) the fact that the home region is small ($n \approx 0$) and in the absence of aggregate shocks, foreign variables are almost constant; and (ii) the production function is $\hat{Y}_h = \hat{L}_h$ (hats denote deviations from steady state). In the neoclassical model, home real product wages $\hat{w}_h$ are constant, and hence $(\hat{WL})_{meas.} = \hat{Y}_{meas.} \approx \hat{Y}_h + \hat{P}_h$. Lower values of the Armington elasticity θ_T generate larger movements in $\hat{P}_h$ in the neoclassical model, and hence potentially larger increases in *measured* labor income or GDP in response to a transfer shock. However, the fact that I am calibrating to small regions within a monetary union, rather than large countries, suggests a higher value of θ_T is appropriate. NS argue that there is very little movement in the local CPI in response to local military purchases, and (consequently) get similar results when they deflate by either national or state CPIs.



Notes: This figure shows measures of transfer impact multipliers in model and data (y-axis) against transfer policies with different rates of persistence (x axis). Panel A compares impact multiplier estimates from the data (specification "with controls" from Table 2 (Temporary) and Table 1 (Permanent), black squares) with cross-region transfer multipliers in the New Keynesian (NK) model (blue diamonds) and neoclassical model (green triangles), measured in a comparable way. The latter refers to the fact that these "measured multipliers" in the data are deflated by national rather than local prices, and so model estimates have to be constructed in the same way. Error bars are 95% confidence intervals (CIs). Panel B plots transfer impact multipliers from the model in terms of quantities: NK model cross-region (blue), NK model self-financed (red) and neoclassical model cross-region (green). The shaded gray area is the persistence of asymmetric US state-level business cycles (from Section 5.1), with the mean $\rho = 0.935$ marked with a dotted line. The gap between self-financed and cross-region multipliers is the pure cross-region transfer multiplier, which is the increase in multipliers when transfers are federally rather than locally financed.

FIGURE 3. Transfer Impact Multipliers (Data vs Models)

multiplier) are small. As the transfer becomes more persistent, cross-region transfer multipliers more than triple in size due to the larger pure cross-region component (self-financed multipliers go to zero).

5. Some Implications

5.1. **Implications for Regional Stabilization.** For more than 25 years, commentators have argued that the US transfer union has helped to smooth asymmetric business cycle shocks to GDP in different US states—and suggested that such a fiscal regime might be needed in the European Monetary Union. However, the strength of this argument depends on the size of the cross-region transfer multipliers, which is why I am in a unique position to shed light on this important policy question. To be clear, I am evaluating the extent to which countercyclical net transfers can help stimulate GDP growth in US states in recessions, which is distinct from the extent to which transfers can help to provide regional consumption insurance (as in Asdrubali et al. 1996 and related literature).[27]

Countercyclicality. The smoothing potential of transfers naturally first depends on how countercyclical they are. The regional literature surveyed in the introduction and Appendix 2.2 suggests that the US tax-transfer system generates around a 20- to 40-cents-on-the-dollar countercyclical net transfer to state residents through automatic stabilizers (mostly on the tax side). That is, when incomes in a state fall by \$1, transfers increase or tax receipts fall by roughly 30c. Hence I calibrate the Normalized Tax Change (using Auerbach and Feenberg's 2000 terminology) to be $NTC = 0.3$.

Cross-region transfer multipliers at business cycle frequencies US state-level business cycles are neither perfectly temporary nor permanent, so I use my calibrated model to convert the temporary and permanent estimated cross-region multipliers to business cycle frequencies. The analysis in Appendix 5 suggests that the average persistence of asymmetric state-level business cycles is $\rho = 0.935$ (a half life of around 2.5 years), with the 90th–10th percentile across states being ρ =0.88–0.98 (half-life 1.4-8.5 years). This region is shaded in Panel B of Figure 3. One can see that cross-region transfer multipliers only become large when the transfer is extremely close to permanent,

[27]Farhi and Werning (2017) find that it is optimal for transfers to be used for regional countercyclical stimulus, even when there is complete consumption risk sharing. Relative to their approach, my conclusions are disciplined by the size of estimated transfer multipliers, and also by the size of countercyclical net transfers in the US tax-transfer system, which might explain why I find smaller benefits of a transfer union.

and so at business-cycle frequencies they must be closer to those of temporary transfers. At mean persistence $\rho = 0.935$, the cross-region transfer multiplier is $\mathcal{M}_{Tr}^{CR}$ =0.42 (marked with a dotted line in Figure 3). Even at the 90th percentile of state-level persistence, the cross-region transfer multiplier is only 0.6 (0.37 and the 10% percentile).

The fraction smoothed $\mathcal{S}$ is the percentage reduction in the depth of a regional idiosyncratic recession (or boom) due to countercyclical net transfers. This can be approximated by $\mathcal{S} = NTC \times \mathcal{M}_{Tr}/(1 + NTC \times \mathcal{M}_{Tr})$, where the numerator is the size of the countercylical net transfer $(NTC)\times$ the relevant transfer multiplier $(\mathcal{M}_{Tr})$, and the denominator adjusts for the fact that as GDP is smoothed, the size of the countercyclical transfer is reduced. Applying $NTC = 0.3$ and $\mathcal{M}_{Tr} = 0.42$, the fraction smoothed is $\mathcal{S} = 11\%$. Using alternative values of persistence (from 10th and 90th percentiles across states), the fraction smoothed ranges from $\mathcal{S} = 10\%$ to 15%. Hence, contrary to popular perceptions, the US transfer union may only provide modest benefits in terms of regional business cycle stabilization. These results are consistent with the literature simulating modest smoothing benefits from automatic stabilizers through disposable income at the national level (Auerbach and Feenberg 2000, McKay and Reis 2016).[28]

Moreover, as the self-financed transfer multiplier is around two-thirds of the cross-region transfer multiplier at the relevant frequencies, a large share of these benefits is available to states if they are able to conduct a similar debt-financed countercyclical fiscal policy themselves.[29] The modest boost to multipliers from federal financing at business cycle frequencies is because only Ricardian households respond differently to cross-region and self-financed transfers (hand-to-mouth households spend both). But Ricardian households save transfers at business cycle frequencies.

[28]In simulations using the full NK model, the amount of short-term smoothing $\mathcal{S}$ is very similar to the approximation used here (see Appendix 6.1). If anything, these smoothing fractions are likely to be too optimistic, due to the measurement of countercyclicality in the literature which often (i) includes intergovernmental transfers and (ii) is measured in terms of personal income (which is smaller than GDP). See Appendix 2.2.

[29]While balanced budget rules do restrict the ability of most US states to run countercyclical fiscal policy, in practice they are often less restrictive (Poterba 1994). Unlike US states, Canadian provinces "have considerable fiscal freedom, which they...use to operate their own counter-cyclical policy" (Bayoumi and Mason 1995).

5.2. **Implications for Aggregate Closed-economy Multipliers.** This paper focuses on transfer (and purchase) multipliers that affect a small state or country in a monetary union, which are very different from aggregate closed-economy transfer or purchase multipliers. In part, this difference is because cross-region multipliers "difference out" the monetary policy and tax responses that are key determinants of aggregate multipliers (Nakamura and Steinsson 2014). But cross-region transfer multipliers in a New Keynesian model are also strongly increasing in consumption home bias (α) and fiscal persistence (ρ), whereas these variables either do not affect aggregate transfer multipliers or affect it with the opposite sign (respectively).[30] Moreover, the share of hand-to-mouth households ω (for untargeted transfers) is always important for aggregate transfer multipliers, but only important for less-persistent cross-region transfer multipliers. As such, even conditioning on a set of aggregate monetary and tax policies, the mapping from empirical cross-region transfer multipliers to aggregate transfer or purchase multipliers is far from one-to-one.

Nonetheless, one can use the empirical evidence presented in this paper to distinguish between models, and then use the favored model to calculate aggregate multipliers. The discussion in Section 4.2 suggests that my empirical evidence is best captured by a NK model with sticky wages and a share of hand-to-mouth households. In that NK model, the aggregate present-value *transfer* multiplier $\mathcal{M}_{Tr}^{AggPV}$ is simply proportional to the aggregate present-value *purchase* multiplier $\mathcal{M}_{G}^{AggPV}$, scaled by the fraction of transfers targeted at hand-to-mouth households ω_T such that $\mathcal{M}_{Tr}^{AggPV} = \omega_T \mathcal{M}_{G}^{AggPV}$.[31] Consistent with the findings of NS, this type of model produces aggregate purchase multipliers $\mathcal{M}_{G}^{AggPV}$ at business cycle frequencies that are small ($\ll 1$) when monetary policy "leans against the wind," but are large (> 1) when monetary policy is more accommodating (in this case, with a constant real interest rate). My empirical estimates for one-off cross-region transfer multipliers are consistent with a modest share of hand-to-mouth households ω, which also pins down

[30]Aggregate transfer multipliers are closer to self-financed transfer multipliers: increasing in the share of hand-to-mouth households (ω), and falling in persistence (ρ).

[31]Aggregate transfers boost demand if they are spent, as well as possibly reducing the labor supply of households receiving the transfer (see Giambattista and Pennings 2017). In this paper, my labor market assumptions for sticky wage setting remove the second channel, allowing me to focus on the effect of transfers on demand.

the scaling factor for untargeted transfer multipliers ($\omega_T = \omega$). Hence my NK model produces large aggregate transfer multipliers ($\mathcal{M}_{Tr}^{AggPV} > 1$) when monetary policy is both accommodating *and* transfers are well targeted ($\omega_T \gg \omega$), modest multipliers $\mathcal{M}_{Tr}^{AggPV} \approx 1/2$ for untargeted transfers when monetary policy is accommodating, and small transfer multipliers $\mathcal{M}_{Tr}^{AggPV} < 1/2$ otherwise (see Appendix 6.2 for details). However, given the differences between cross-region and aggregate transfer multipliers, these implications are more suggestive than definitive, with more research being needed.

6. Conclusions

In this paper I investigate the size of the cross-region transfer multiplier in the US, which is novel as this multiplier is conceptually different from the MPC and other multipliers estimated in the literature, and relevant as transfers are the largest component of federal spending. I find that cross-region transfers significantly boost short-run growth in the states receiving them, with impact multipliers around 0.3 for one-off transfers and 1.5 for permanent transfers. The size of the multipliers can be roughly rationalized by a standard New Keynesian model. That model, in turn, produces large aggregate closed-economy purchase and targeted transfer multipliers when monetary policy is accommodating, and small multipliers when monetary policy "leans against the wind."

My estimates of cross-region transfer multipliers inform the debate on the efficacy of the US federal transfer union in smoothing regional shocks, and the macroeconomic benefits of fiscal centralization more broadly. Contrary to commonly held perceptions, I find that federal tax-transfer systems like those in the US only smooth a modest fraction of asymmetric shocks to regional GDP at business cycle frequencies, in part because the relevant multipliers are not large. However, this is not to say there might not be other cyclical benefits of federal fiscal systems. For example, they might help to smooth consumption or intergovernmental transfers might help prevent pro-cyclical regional fiscal policy. Chodorow-Reich et al. (2012) (among others) have found substantial regional multipliers on such intergovernmental transfers during the

Great Recession, though there are also papers finding smaller multipliers (see Chodorow-Reich 2018 for a survey).

An online appendix with supplementary material is available here

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