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Cross-Region Transfer Multipliers in a Monetary Union: Evidence from Social Security and Stimulus Payments†

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US federal transfers to individuals are large, countercyclical, vary geographically, and are often credited with helping to stabilize regional economies. This paper estimates the short-run effects of these transfers using plausibly exogenous regional variation in temporary stimulus payments and permanent Social Security benefit increases. States that received larger transfers tended to grow faster contemporaneously, with a multiplier of around 1.5 for permanent transfers and 1/3 for temporary transfers. Results are broadly consistent with an open-economy New Keynesian model. At business cycle frequencies, cross-region transfer multipliers are not large, suggesting only modest gains in regional stabilization from US federal automatic stabilizers. (*JEL* E12, E32, E62, H23, H55, R12)

US federal expenditure increasingly involves making transfers to individuals rather than purchasing (or producing) output. In 2017, US federal transfers to individuals were around \$1.4–\$2.1 trillion (depending on the definition used), which is larger than government consumption. Transfers to individuals were the cornerstone of fiscal stimulus packages in 2001 and 2008 and accounted for the majority of the increase in expenditure around the 2007–2009 financial crisis in the United States and other countries (Oh and Reis 2012). More recently, transfers to households are a major part of the economic response to COVID-19 in more than two dozen countries, with the US federal government spending around \$250 billion on one-off payments in 2020:II (IMF 2020). In developing countries, conditional and unconditional cash transfers are increasingly popular antipoverty programs, reaching around 500 million beneficiaries (World Bank 2018).

Federal transfer policies naturally redistribute income across regions, with benefits often tilted to low-income areas or to regions receiving negative shocks. Automatic stabilizers mean individuals in regions entering recessions receive more federal benefits and pay lower federal taxes. A number of papers have found that

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these (and other) net federal transfers are about \$0.20–\$0.40 of every dollar fall in regional income in the United States, which are thought to help stabilize regional economies (Sala-i-Martin and Sachs 1991, Bayoumi and Masson 1995, Feyrer and Sacerdote 2013). As such, *The Economist* writes that "Indeed, 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."[1]

Despite their importance, little is known about the effect of transfers to individuals on short-run regional economic growth, which I call the size of the *cross-region transfer multiplier*. 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 US states in a way that is plausibly unrelated to regional business cycles. My findings are related to literatures on the marginal propensity to consume (MPC) transfers (e.g., Johnson, Parker, and Souleles 2006; Parker et al. 2013; Hausman 2016; Romer and Romer 2016) and the cross-sectional purchase multiplier (e.g., Chodorow-Reich et al. 2012, Nakamura and Steinsson 2014, Chodorow-Reich 2019). But the cross-region transfer multiplier is conceptually very different from both the MPC and cross-sectional purchase multiplier in New Keynesian and neoclassical models. Showing this analytically is my second contribution.[2]

The cross-region transfer multiplier differs from the MPC due to local general equilibrium effects. Local general equilibrium effects amplify the MPC in New Keynesian models with sticky prices/wages if incomes are mostly spent on locally produced goods, but they can dampen the MPC in more open economies. In neoclassical models, cross-region transfer multipliers are always negative due to wealth effects on labor supply in general equilibrium.

The cross-region transfer multiplier differs from the cross-region purchase multiplier due to a smaller boost to local demand from transfers, depending on the MPC and openness, and from stronger wealth effects. These factors mean the cross-region transfer multiplier is smaller than the purchase multiplier in both New Keynesian and neoclassical models and is more variable in sign and absolute size. The permanent income hypothesis suggests transfer multipliers are much more sensitive to the persistence of fiscal shocks, which I explore empirically by studying both temporary and permanent transfer policies. The wide variety of theoretically possible cross-region transfer multipliers—large, small, and negative—underscores the need for empirical evidence to discipline the choice of models and parameters. Providing this guidance is my third contribution.

There are two key challenges to identifying the cross-region transfer multiplier empirically. The first challenge is *reverse causality*, where changes in state-level growth can induce changes in countercyclical transfer policies to residents of the

[1] See "For Richer, for Poorer," *The Economist,* November 28, 2015. This perception has been around for more than a quarter century: Sala-i-Martin and Sachs (1991, p. 20) write that "Some economists … argue that this 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." Other benefits of fiscal unions in addition to automatic stabilizers, such as centralized deposit insurance and discretionary stimulus, are beyond the scope of this paper. See online Appendix Section 2 for background on the size and countercyclicality of transfers to individuals.

[2] Most of the recent theoretical literature has focused on determinants of the MPC (Kaplan and Violante 2014) or the (large) differences between cross-region and aggregate purchase multipliers (Nakamura and Steinsson 2014, Farhi and Werning 2016). Farhi and Werning (2017) focus on optimal transfers rather than on the size of multipliers.

affected states. 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 is constructed separately for each aggregate policy change.

For permanent transfers, the aggregate policy change is a series of ad hoc increases in the monthly stipend for Social Security (old-age pension) recipients over the period 1952–1974. 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, the aggregate policy changes are one-off stimulus payments in 2001 and 2008 (a "check in the mail") of \$300–\$600. While the 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 policies 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.

The second identification challenge is *omitted variable bias*, where other variables might affect both transfers and regional economic growth. I address this by controlling for state fixed effects (FEs) to remove state-level trends and time FEs to 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. The remaining omitted variable bias 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 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. But these impact multipliers also prove to be similar to cumulative multipliers over longer horizons.

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 \$1/3 (\$0.2–\$0.9, 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 (\$0.9–\$1.9, depending on the specification). Romer and Romer (2016) find that almost all of the permanent Social

Security increases were consumed, and Johnson, Parker, and Souleles (2006) and Parker et al. (2013) argue that a modest fraction of the temporary transfers was spent contemporaneously on nondurables—both of which are broadly consistent with my results (though those papers focus on the MPC and do not estimate cross-region multipliers).

These empirical findings have implications for the ability of the US federal fiscal system to smooth shocks to regional economic growth through automatic stabilizers, as that ability depends, in part, on the size of the cross-region transfer multiplier. Regional business cycles are temporary, and so the relevant multipliers are closer to those on temporary transfers and hence are modest in size. Combined with the countercyclicality of federal net transfers to the residents of US states estimated in the literature, back-of-the-envelope calculations suggest that the US federal system would only smooth about 10 percent of regional asymmetric shocks to output, which is perhaps less than in the popular perception.

My empirical estimates are also related to the size of the aggregate closed-economy transfer multiplier, albeit indirectly as an "identified moment" that guides the choice of theoretical models (Nakamura and Steinsson 2018). Aggregate fiscal multipliers are sensitive to monetary policy and tax responses and thus are very different, in general, to cross-sectional multipliers estimated here, which "difference out" these factors. My cross-sectional multiplier estimates are broadly consistent with a standard open-economy New Keynesian model that features home bias in consumption and a share of hand-to-mouth households, and they are less consistent with a canonical neoclassical model. That New Keynesian model produces aggregate *purchase* multipliers similar to those in Nakamura and Steinsson (2014): large (≥ 1) if monetary policy is accommodating and small (<1) otherwise. Closed-economy transfer multipliers at business cycle frequencies in this model are similar to purchase multipliers if transfers are targeted at hand-to-mouth households who spend them. But they are only one-third of the size if transfers are untargeted.

The rest of this paper is organized as follows. Section I outlines the empirical methodology, including describing the natural experiments, data construction, identification issues, and specification. Section II provides the main empirical results, robustness tests, and some extensions. Section III provides analytical expressions for cross-region transfer multipliers in New Keynesian and neoclassical models and compares them to the MPC and cross-region purchase multipliers. Section IV compares theoretical and empirical multiplier estimates quantitatively. Section V presents policy implications on the ability of the US federal fiscal system to smooth regional shocks and the size of the aggregate closed-economy transfer multiplier. Section VI concludes.

I. Empirical Methodology

A. *Transfer Policies and Variable Construction*

In this paper, I study the effect of permanent Social Security benefit increases (over the period 1952–1974) and one-off stimulus payments (in 2001 and 2008) on state-level nontransfer income growth. This subsection describes these policies, how the exogenous transfer variables are constructed, and how I address identification challenges. Though canonical New Keynesian and neoclassical models suggest persistence is the most important difference across these transfer policies, they naturally also differ in other ways, such as beneficiary demographics and payment size, which might be relevant in other models.[3]

Social Security Increases, 1952 to 1974.—Before 1975, Social Security payments (largely the old-age pension) were not indexed to inflation and were increased by Congress on an ad hoc basis (Wilcox 1989). These were mostly permanent increases in transfers—that is, a higher monthly stipend received by the elderly and their dependents—and so are likely to be spent, even by Ricardian consumers (by the permanent income hypothesis). As benefit increases varied in size and timing, they would not be subsumed into seasonal factors. Wilcox (1989) and Romer and Romer (2016) study these transfers at the aggregate level and find that increases in permanent Social Security payments significantly increase consumption. This is consistent with my finding of large income multipliers in the cross-section, though their results do not imply mine and vice versa. From 1975 onward, Social Security payments were indexed to the Consumer Price Index (CPI) and were adjusted annually, making them much more predictable, and so they are excluded from my analysis.[4]

One-Off Transfer Stimulus Payments, 2001 and 2008.—I consider two temporary stimulus payments, in 2001 and in 2008. My main results pool across these two temporary payments, though the payments are analyzed separately in Section IIE.

In 2001:III, the Bush Administration transferred $38 billion to households in a one-off stimulus 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. Exploiting the randomization of payment dates, Johnson, Parker, and Souleles (2006) find that 20 percent to 40 percent of the payment was spent on nondurables in the months that it was received, with a higher MPC for the poor and credit constrained, and no response of durables consumption.

In 2008:II–III, around $95 billion was transferred to households as one-time payments as part of the Bush Administration's Economic Stimulus Act. The vast majority (85 percent) of the payments were made during 2008:II (Parker et al. 2013). These stimulus payments had two main components: (i) $300 per capita payments made to those paying no net taxes but with at least $3,000 in eligible annual income (around $30 billion, which I call the *low-income rebate component*, which was refundable) and (ii) $600 per capita payments made to those paying net taxes with a phase-out for those earning over $75,000 (around $65 billion, which I call the *middle-income tax refund* component). Most of the effects of the 2008 stimulus

[3] The historical period might be also relevant, as the openness of regional economies may have changed between the 1950s–1970s and the 2000s (openness reduces multipliers in a New Keynesian model). While households likely had lower access to credit in the earlier period, this is less relevant for permanent transfers (that are spent even by Ricardian households) and for Social Security recipients (who are presumably drawing down their savings).

[4] 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.

payments turn out to be driven by the low-income component. Parker et al. (2013) exploit randomization in the timing of the stimulus payments and find that about 12 percent to 30 percent of the payments were spent on nondurable consumption in the months they were received (50 percent to 90 percent including durables).

Construction of Exogenous Transfer Variables.—The changes in exogenous transfers at the state level used in regressions ($\Delta tr_{i,t}$) are only publicly available for the 2008 low-income rebate component (see Bureau of Economic Analysis 2009) but otherwise have to be calculated based on the size of the aggregate transfer change $\Delta tr_{US,t}$ (across the whole United States) multiplied by a state-specific share $stateshare_{i,t-j}$:[5]

$$\Delta tr_{i,t} = \Delta tr_{US,t} \times stateshare_{i,t-4}, \tag{1}$$

where t denotes quarters, i denotes states, and Δ is the first difference operator. On the surface, this appears similar to a Bartik (1991) instrument, but they differ because the aggregate component $\Delta tr_{US,t}$ is based on individual policy changes (rather than general aggregate variation) and $stateshare_{i,t-4}$ is based on the eligibility rules for each specific policy change (rather than average transfer shares). Combined, this means that $\Delta tr_{i,t}$ reflects actual changes in transfers to different states—for example, in an instrumental variables (IV) specification, first-stage coefficients are close to 1 and first-stage F-statistics are above 500—rather than being only mildly correlated with transfer changes.

For the 2001 transfers and the 2008 middle-income tax refund, the aggregate size of the payments $\Delta tr_{US,t}$ in equation (1) are taken from Johnson, Parker, and Souleles (2006) and Parker et al. (2013), respectively (subtracting the size of the low-income rebate from BEA 2009 in the latter case). Each state's share ($stateshare_{i,t-4}$) is calculated using Internal Revenue Service (IRS) state-level data on individual tax returns from the previous tax year (IRS 2000 and 2007, respectively), using eligibility rules for each specific payment. The IRS also calculated eligibility for the stimulus payments using prior-year tax returns, as full-year income was not known in 2001:III or 2008:II–III when the payments were made, so the lag does not reduce accuracy. The cross-state distribution of one-off transfers is plotted in Figure 1.

For permanent Social Security benefit increases, the aggregate transfer change $\Delta tr_{US,t}$ is taken from Table 1 of Romer and Romer (2016) over the period 1952–1974.[6] I allocate the exogenous increase in aggregate Social Security payments across states in proportion to that state's share of Social Security payments a year

[5] The cross-state allocation of the low-income rebate component from BEA (2009) is primarily based on the geographic distribution of recipients of refundable earned income tax credits. According to BEA (2009), $28 billion of the low-income component was paid in 2008:II, with only $1.35 billion paid in 2008:III (the annualized figures in BEA 2009 divided by 4). Combined with Parker et al.'s (2013) quarterly profile, this suggests a middle-income payout of around $50 billion in 2008:II and $14 billion in 2008:III. All of the 2001 stimulus payments were made in 2001:III. Hence $\Delta tr_{i,t} > 0$ for 2001:III and 2008:II, $\Delta tr_{i,t} < 0$ for 2001:IV, 2008:III–IV (small in 2008:IV), and $\Delta tr_{i,t} = 0$ for all other quarters. The rest of the sample helps to estimate state fixed effects and controls.

[6] 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. For example, a permanent $1 increase on a $10 monthly payment starting on June 1 would be a 3.3 percent increase in Q2 and a ≈6.5 percent increase in Q3 (though my specification scales Social Security increases by labor income, not benefits).

before, $stateshare_{i,t-4}$ as in equation (1) (see online Appendix Section 1 for details). The lagging of eligibility criteria does not prevent an accurate cross-state allocation as demographic characteristics are largely predetermined and slow-moving over the medium term. For example, the size distribution across states of the 1972:IV increase in Social Security payments was almost exactly proportional to the 1970:II increase in Social Security payments (R^2 of 0.96) (panel A of online Appendix Figure 1, blue circles).

There are 20 exogenous Social Security increases during 1952–1974, which are spread over 27 quarters. There are no increases in the remaining 65 quarters ($\Delta tr_{it} = 0$), but these periods are included in the sample to estimate state fixed effects μ_i and other controls. The average size of the benefit increases is 0.2 percent of quarterly labor income, though they are highly heterogeneous across states and over time (see descriptive statistics in online Appendix Table 1). The largest permanent increases were in 1972:IV (a 20 percent increase in benefits) of around 1.45 percent of quarterly labor income in West Virginia, Arkansas, and Florida, which have a large number of retirees as a share of the population (panel A of online Appendix Figure 1). In contrast, the increase in Social Security benefits to residents of Alaska was only 0.2 percent of labor income in the same quarter.

B. *Identification Strategy*

As mentioned in the introduction, there are two key challenges in identifying the effect of transfers on cross-sectional growth rates: reverse causality and omitted variable bias.

Reverse causality is a serious problem for transfer multiplier estimates because transfers are often countercyclical. Indeed, simple regressions 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), with most coefficients being either insignificant or negative (see online Appendix Table 9).

My identification strategy addresses reverse causality by studying variation in transfers across US states (as a share of income) generated by an aggregate transfer change ($\Delta tr_{US,t}$) combined with rules determining who is eligible for the transfer ($stateshare_{i,t-4}$), as in equation (1). 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.[7] As the transfer policy change ($\Delta tr_{US,t}$) is at the national 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 that my sample of Social Security increases did not have countercyclical motivations. Stimulus payments were widely spread across states and had simple eligibility rules, making

[7] To be clear, the identification assumption (internal validity) requires that income growth in Florida would be similar to that in other states (after controls) if Florida did not receive such a large Social Security transfer. Identification does not require that the effect of actual transfers in Florida are the same as those in other states, which is about external, rather than internal, validity. In principle, the effects of transfers could be heterogeneous across states, but identifying heterogeneity is difficult with only 50 states.

it difficult to target them at specific states.[8] My allocation of transfers across states ($stateshare_{i,t-4}$) cannot be affected by contemporaneous state-level shocks because they are based on last year's eligibility characteristics.[9]

The second and perhaps more serious concern is omitted variable bias. The first way to address this is to include state fixed effects (μ_i) and quarter fixed effects (μ_t) in all specifications. State FEs 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, aggregate expectations, or international shocks). The inclusion of state and quarter fixed effects means that regressions only use 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. Including both fixed effects in all specifications drastically reduces the number of potentially confounding variables.

To further reduce the risk of omitted variable bias, I use a research design that involves many transfer policy changes with different sizes and timings and over a long period. Through this design, any omitted variable would need to take an unlikely time-varying pattern in high- versus low-transfer states, which is a much higher hurdle for potential confounding variables than in a standard difference-in-difference study with a one-off permanent policy change. For example, Social Security stipends increased by 10 percent in June 1971 and 20 percent 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 confounding variable not only has to spuriously boost growth in high-transfer states in the relevant quarter but also has to reduce growth in the following quarter when the transfer is withdrawn.[10]

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 37 percent of the variation in 2001 transfers (panel B of online Appendix Figure 1). 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 (1952–1972) led to a reallocation of Social Security benefits across many states such that the cross-state allocation in 1952 only explains 28 percent of the cross-state variation in 1972 (panel A of online Appendix Figure 1, green triangles).

Omitted variable bias is further reduced by adding other controls that vary by state and over time and also by conducting 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, differential

[8] Voting records (Govtrack.us 2001, 2008) 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 online Appendix Section 3.3).

[9] After removing trends and aggregate variation, state growth rates have little quarterly persistence, which removes the possibility of ex ante targeting states in recession.

[10] For example, in 2001 the confounding variable would have to boost growth in 2001:III in high-transfer states like West Virginia and then reduce growth in 2001:IV.

growth trends depending on industry composition, controls for state-specific quadratic trends, and the removal of influential years or states.

C. *Empirical Specification*

My main specification is the following regression for state i at quarter t:

$$\Delta Y_{i,t}/Y_{i,t-1} = \beta_0 \Delta tr_{i,t}/Y_{i,t-1} + \boldsymbol{\delta}'\mathbf{X}_{it} + \mu_t + \mu_i + e_{it}, \tag{2}$$

where $\Delta Y_{i,t}/Y_{i,t-1}$ is the quarterly growth rate of per capita income (excluding the transfer), $\Delta tr_{i,t}/Y_{i,t-1}$ is the contemporaneous change in my constructed exogenous transfer measure as a share of income, μ_i and μ_t are state and quarter fixed effects, respectively, and e_{it} is the error term. The term $\mathbf{X}_{it}$ is a vector of controls (and $\boldsymbol{\delta}$ is a vector of associated coefficients) that varies across specifications. The transfer measure is scaled by lagged income to generate a "multiplier" interpretation of coefficients (i.e., the dollar value of extra non-transfer income produced in a state when its residents receive an extra dollar of transfers). This specification of growth rates regressed on a scaled fiscal shock is standard in the multiplier literature.[11]

The parameter β_0 in equation (2) is known in the literature as the *impact multiplier* and is the main coefficient of interest in this paper (I also calculate cumulative multipliers below, which turn out to be similar). Relative to cumulative multipliers estimated over longer horizons, impact multipliers have two advantages: first, they require weaker identification assumptions, as there is only a short window between cause and effect that reduces the number of possible omitted variables; and second, they tend to be estimated more precisely (smaller standard errors).

In the spirit of Romer and Romer (2010, 2016), equation (2) is estimated as a reduced form (by ordinary least squares (OLS)) rather than by IV (I also produce IV estimates, which turn out to be similar). The reduced form is appropriate as my exogenous variation is the variable of economic interest (the state-level transfer shock induced by an aggregate policy change) rather than being merely correlated with the variable of interest, as is often the case.

I use two measures of non-transfer income as left-hand-side variables: labor income per capita $Y_{it} = (WL)^{pc}_{i,t}$ (equivalent to the wage bill), which is available for the whole sample, or quarterly GDP per capita $Y_{it} = GDP^{pc}_{i,t}$ from 2005.[12] All variables are deflated by the national quarterly Personal Consumption Expenditures Chain-Type Price Index, as there are no official state-level price data.[13] As state-level growth can be extremely volatile, especially for small states, I drop outliers from all

[11] For example, a specification of GDP growth rates regressed on the change in expenditure as share of GDP (or similar) is used in Barro and Redlick (2011); Kraay (2014); Nakamura and Steinsson (2014); and Miyamoto, Nguyen, and Sergeyev (2018), among others. $\Delta tr_{i,t}/Y_{i,t-1}$ is equivalent to the change in transfers per capita as a share of income per capita, with population measured at $t-1$.

[12] Labor income data come from the Bureau of Economic Analysis (BEA 2015) under the official title Earnings by Place of Work and mostly consist of wage and salary disbursements (70 percent). Labor income excludes income from transfers and is before income taxes. I focus on these dependent variables, as other potential outcome variables are not available at the state level at a quarterly frequency (consumption; GDP before 2005), are not seasonally adjusted, or are not available for both temporary and permanent transfer samples. State GDP data are also from BEA (2015). Population data are from the US Census Bureau (2011).

[13] While the BEA does produce estimates of state-level GDP deflators, these variables use national prices for different industries weighted using state-specific industry weights, which will not capture the price effects from

specifications where the annualized growth rate of per capita labor income growth or GDP is more than 20 percent in absolute value. Standard errors are cluster-robust (clustered at the state level), which allows for heteroskedasticity and arbitrary serial correlation. See online Appendix Section 1 for descriptive statistics and additional information on data sources and data construction.

Cumulative Multipliers.—As an extension in Section IIF, I estimate dynamic cumulative multipliers over several quarters after the transfer is paid. The cumulative multipliers $\mathcal{C}_h$ are defined as the cumulative change in an income variable relative to the cumulative spending on transfers over a horizon of h quarters beyond the impact quarter ($h + 1$ quarters in total). I use two specifications, both of which collapse to equation (2) for $h = 0$.

For permanent transfers, I use a direct projections specification (Jordà 2005), which has been used by Miyamoto, Nguyen, and Sergeyev (2018) and others to estimate cumulative multipliers.[14] The estimated equation is

$$(3) \qquad \sum_{j=0}^{h} \frac{Y_{i,t+j} - Y_{i,t-1}}{Y_{i,t-1}} = \beta_h \sum_{j=0}^{h} \frac{tr_{i,t+j} - tr_{i,t-1}}{Y_{i,t-1}} + \boldsymbol{\delta}'\mathbf{X}_{it} + \mu_t + \mu_i + e_{it},$$

where $\sum_{j=0}^{h}(Y_{i,t+j} - Y_{i,t-1})/Y_{i,t-1}$ is the sum of $h + 1$ growth rates over horizons up to h periods (left side) and $\sum_{j=0}^{h}(tr_{i,t+j} - tr_{i,t-1})/Y_{i,t-1}$ (right side) is the sum of changes in scaled transfers over the same period. The cumulative multiplier can be estimated directly as $\mathcal{C}_h = \beta_h$.

For one-off transfers, I use a distributed lag specification, which adds extra lags of the transfer variable $\Delta tr_{i,t-j}/Y_{i,t-j-1}$ to the right-hand side of equation (2).[15] A distributed lag specification is standard in the MPC literature for one-off transfers, such as in Johnson, Parker, and Souleles (2006) and Parker et al. (2013).[16] Other specifications, including a specification in detrended levels, produce broadly similar results (see online Appendix Sections 3.4 and 3.5).

II. Empirical Results

A. *Graphical Evidence*

Figure 1 provides a visual representation of the size and direction of the relationship between changes in transfers on the x-axis (permanent and temporary) and contemporaneous growth in per capita labor income and GDP (y-axis). The slope of relationship is a simple estimate of the size of the cross-region transfer multiplier. For temporary transfers (panels B–D), each of the 50 dots represents the growth rate and transfer residuals in a different state in the quarter the transfer was paid. For

local demand shocks. The inflation data are taken from the St. Louis FRED (2017), as are oil price and US aggregate GDP data used in the construction of controls.

[14] Kraay (2014) uses a similar specification (in changes) but takes the sum after estimation. Ramey and Zubairy (2018) apply the same specification but estimate in detrended levels.

[15] Specifically, $\beta_0 \Delta tr_{i,t}/Y_{i,t-1}$ in equation (2) is replaced by $\sum_{j=0}^{h}\gamma_j \Delta tr_{i,t-j}/Y_{i,t-j-1}$, with the cumulative multiplier for a one-off transfer being the sum of the lags $\mathcal{C}_h = \sum_{j=0}^{h}\gamma_j$.

[16] Although the direct projections approach is more popular in the multiplier literature, regressions on simulated data reveal that it has difficulty isolating the lagged effects of one-off transfers over short horizons, whereas the distributed lag specification uncovers the correct multipliers exactly (see online Appendix Section 3.6).

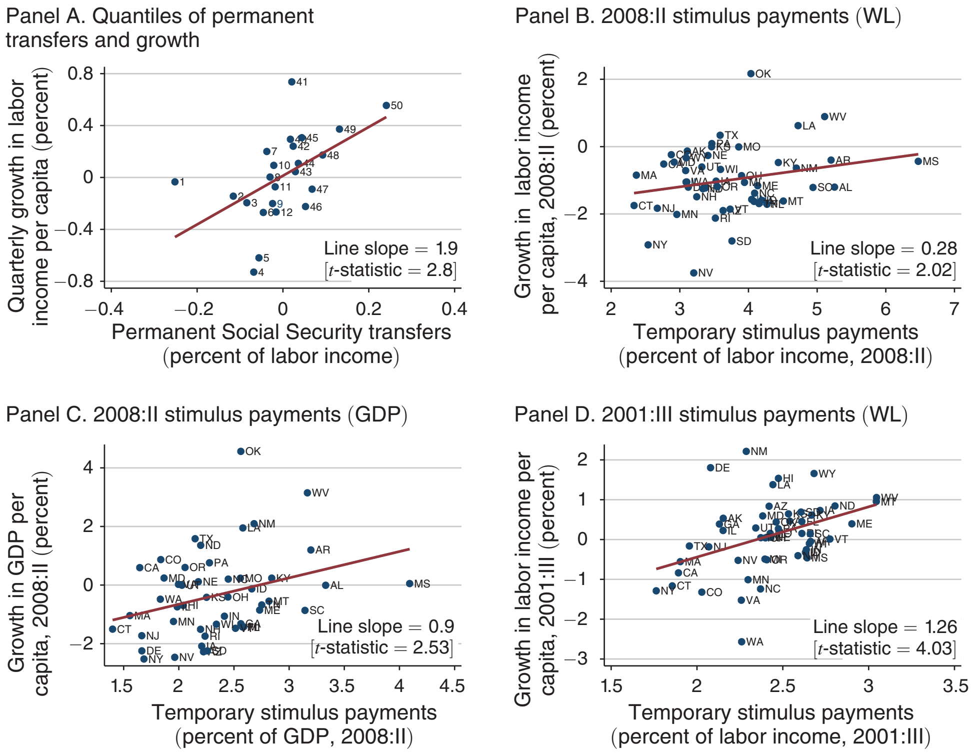

FIGURE 1. SCATTER PLOTS: TRANSFERS AND ECONOMIC GROWTH

Notes: Panel A: Each point is the mean growth rate residual or mean Social Security increase residual (share of labor income) of 50 quantiles (controlling for state and time fixed effects). Quantiles with transfer residuals close to zero are not shown. Panels B–D: Each point in the scatter plot is the growth in labor income per capita or GDP per capita (y-axis) in the quarter, plotted against the size of the contemporaneous transfer (x-axis).

Sources: BEA, Romer and Romer (2016)

permanent transfers (panel A), there are too many Social Security increases to be plotted in this way. So instead, I group permanent transfer change residuals into 50 bins based on their size (across different quarters and states) and plot the average transfer residuals 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.[17]

In all four subplots in Figure 1 there is a positive and significant relationship (line slope), indicating that states that received larger transfers tended to grow faster contemporaneously. Also note that the relationship between transfers and contemporaneous growth has a steeper slope for permanent than temporary transfers. For permanent transfers (panel A), the slope is about 1.9, significant at the 1 percent level. This relationship is not driven by a couple of bins: an outlier-robust regression generates a similar slope and t-statistic.

[17] Quarters without any Social Security increases are dropped in panel A of Figure 1. The figure also excludes bins where the Social Security residuals are approximately zero, though these are shown in online Appendix Figure 6 and do not affect the line slope.

For temporary transfers, the line slope ranges from 0.28–1.26, depending on the quarter and income variable, and the scatter plots help us to understand the drivers of cross-state variation in transfers. Panel B of Figure 1 plots 2008 transfers (share of labor income) against growth, which have a "relative multiplier" (line slope) of about 0.28 (t-statistic $=$ 2). This is remarkably similar to the pooled temporary transfer multiplier estimated later in regressions. The cross-state variation in transfers is striking: the 2008 stimulus payments ranges from around 2.3 percent of quarterly labor income in Connecticut (CT) to 6.4 percent 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 poorer states. Of the 4.1 percentage point gap in stimulus transfers in 2008:II between MS and CT, 3.4 percentage points are due to the cross-state allocation of the low-income component. Panel C of Figure 2 shows the same stimulus package and quarter but for quarterly growth in GDP per capita rather than labor income, which generates a steeper slope of 0.9 but one that is less precisely estimated (t-statistic $=$ 2.5). The final scatter plot (panel D) displays the relationship between the 2001 stimulus payments (as a share of labor income, x-axis) and contemporaneous per capita labor income growth in 2001:III (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 1.26 (t-statistic $=$ 4). However, I do not put too much quantitative weight on the multiplier estimates from the scatter plots, as they have no controls and discard evidence from the withdrawal of temporary transfers.

B. *Main Empirical Results*

The main empirical results are presented in panel A of Table 1, which pool across all transfer changes grouped by persistence and dependent variable. The first row presents a parsimonious specification, with no controls beyond state and quarter FEs. The second row presents a "benchmark" specification, which includes controls for US GDP growth $\times$ state fixed effects and population growth and is used for placebo tests (Figure 2) and to compare with the theoretical model in Section IV. The 50 US GDP growth $\times$ state fixed effects are used to control for differential state sensitivities to the aggregate business cycle. This is potentially important since the 2001 and 2008 transfers were explicitly countercyclical at the aggregate level (discussed further below). Population growth is important because Sun Belt states with more retirees tend to have faster growing populations than other states, which might affect per capita income growth and be correlated with transfer size (for example, if retirees claiming Social Security benefits migrate to warmer states).

Column 1 of Table 1 reports cross-region transfer multiplier estimates for permanent Social Security benefit increases. In general, I find that a \$1 increase in permanent Social Security transfers to the residents of a state increases that state's relative labor income by around \$1.5 contemporaneously, though the size of the multiplier can vary from 0.9–1.9 (about one standard error) across the specifications in Table 1. Using the parsimonious specification (row A1), the relative permanent transfer multiplier is almost exactly 1.5 (significant at the 1 percent level). For the

Table 1—Cross-Region Transfer Impact Multiplier Estimates

	Permanent transfers (Social Security)		Temporary transfers (2001 and 2008 stimulus payments)			
	Quarterly growth in real per capita:					
	Labor income, 1952–1974 (1)		Labor income, 2001–2008 (2)		GDP, 2005–2008 (3)	
Specification varies by row	Multiplier	Observations	Multiplier	Observations	Multiplier	Observations
Panel A. Impact multiplier estimates for main specifications						
A1. Parsimonious specification (no additional controls)	1.47 (0.49)	4,413	0.26 (0.08)	1,588	0.39 (0.16)	742
A2. Benchmark specification (controls for population growth and US GDP growth × state FEs)	1.29 (0.54)	4,413	0.31 (0.09)	1,588	0.41 (0.18)	742
Panel B. Impact multiplier estimates with additional economic controls (added one by one)						
B1. Controlling for US GDP growth × state FEs	1.14 (0.53)	4,413	0.31 (0.09)	1,588	0.42 (0.18)	742
B2. Controlling for oil prices (and lag) × state FEs	1.57 (0.50)	4,413	0.27 (0.08)	1,588	0.42 (0.15)	742
B3. Controlling for state-specific quadratic trends	1.71 (0.45)	4,413	0.25 (0.08)	1,588	0.41 (0.17)	742
B4. Controlling for state manufacturing GDP share × quarter FEs	0.92 (0.45)	4,413	0.25 (0.09)	1,588	0.41 (0.17)	742
B5. Controlling for Social Security back payments	1.51 (0.51)	4,413	–		–	
B6. Controlling for 2001–2008 recession depth × aggregate transfer size	–		0.26 (0.08)	1,588	0.39 (0.16)	742
Panel C. Impact multiplier estimates with additional dynamic controls (added one by one)						
C1. Controlling for lagged log income	1.53 (0.57)	4,413	0.26 (0.08)	1,588	0.47 (0.16)	742
C2. Controlling for lagged dependent variable	1.18 (0.51)	4,281	0.23 (0.08)	1,577	0.37 (0.15)	686
C3. Controlling for lagged transfers	1.36 (0.48)	4,413	0.21 (0.09)	1,588	0.45 (0.24)	742
Panel D. Impact multiplier estimates for other samples or IV specification						
D1. Excluding influential states or years	1.43 (0.69)	4,051	0.26 (0.10)	1,556	0.43 (0.21)	727
D2. Instrumenting Social Security transfers or other current transfers	1.88 (0.59)	4,413	0.20 (0.07)	1,588	0.36 (0.14)	742
IV first stage F-statistics	522		882		843	
State and quarter fixed effects	All specifications		All specifications		All specifications	

Notes: Each cell reports the impact multiplier of a regression of growth in per capita labor income (columns 1 and 2) or GDP (column 3) on the scaled change in permanent Social Security benefits (column 1) or temporary stimulus transfers (columns 2 and 3), as in equation (2). All specifications have state and quarter fixed effects. The main results are in panel A: row A1 is a parsimonious specification with no further controls, and row A2 is a benchmark specification used in figures (except scatter plots) with controls for US GDP growth × state fixed effects and population growth. Panels B and C add controls one by one to the parsimonious specification. Row B1 adds US GDP growth × state fixed effects; row B2 adds state fixed effects × $\log(OilPr_t)$, $\log(OilPr_{t-1})$; row B3 adds state fixed effects × t, t^2; row B4 adds the manufacturing share of state GDP × time fixed effects; row B5 controls for ad hoc temporary Social Security backpayments (t and $t-1$). Row B6 controls for the interaction between the depth of the 2001/2008 recessions in each state and the mean transfer variable each quarter. Row C1 controls for the lagged log level of per capita labor income (columns 1 and 2) or per capita GDP (column 3). Row C2 adds a control for the lagged dependent variable. Row C3 adds a control for the lagged transfer variable. Row D1 drops influential states or years from the parsimonious specification, as identified by leave-one-out regressions in online Appendix Section 3 and scatter plots; for permanent transfers: years 1952 and 1972; for temporary transfers: Mississippi. Row D2 reports estimates from an instrumental variables (IV) specification, where all BEA Social Security transfers (column 1) or "all other personal current transfer receipts" (columns 2 and 3) are instrumented with the constructed transfer measures used in the other rows. First-stage coefficients are close to 1 (across columns 1–3: 0.8, 1.1, and 1.1, respectively). Coefficients on controls are not reported. Outliers $|\text{growth}| > 20$ percent (annualized) are dropped. Robust standard errors are in parentheses (clustered by state).

benchmark specification (row A2), the relative multiplier falls slightly to 1.3 (also significant at 1 percent). This reflects partially offsetting effects of the two controls; if added individually, US GDP growth × state fixed effects tend to reduce the permanent transfer multiplier to around 1.14 (row B1, significant at the 1 percent level), whereas population growth tends to increase the multiplier to 1.61 (not reported, also significant at the 1 percent level).[18]

Columns 2 and 3 report cross-region transfer multiplier estimates for one-off stimulus transfers, using per capita labor income or GDP (respectively) as the income measure. Unlike the scatter plots in Figure 1, the regressions control for state and time FEs, impose that the withdrawal of transfers reverses any earlier increase in income, and for labor income, pools across the 2001 and 2008 stimulus payments (recall quarterly GDP data are only available from 2005, so those regressions exclude the 2001 transfer). In general, I find that a \$1 one-off transfer to the residents of a state increases that state's relative labor income or GDP by around \$1/3 contemporaneously, with multipliers around \$0.4 for GDP (range 0.36–0.47 depending on the specification) and \$0.25 for labor income (range 0.20–0.31). In the parsimonious specification (row A1), the temporary cross-region transfer labor income multiplier is around 0.26 (significant at the 1 percent level), rising slightly to 0.31 for the benchmark specification (also significant at 1 percent). When the dependent variable is per capita GDP growth, the multiplier for the parsimonious specification is larger at 0.39 (row A1), increasing marginally to 0.41 in the benchmark specification (row A2). Both GDP multipliers are significant at the 5 percent level, rather than the 1 percent level, due to wider standard errors.

C. *Robustness Tests* (*Controls*, *Samples*, *and Estimation Methods*)

To address potential omitted variable bias in the main results, I conduct a suite of robustness tests to different controls, dynamics, specifications, and estimation methods, which are presented in the remaining panels of Table 1.

Additional Controls.—Panel B of Table 1 controls for additional economic variables (added one by one) that might be correlated with both state-level growth and transfer size.

As mentioned above, the one-off transfer payments in 2001 and 2008 were explicitly countercyclical. While the aggregate business cycle is subsumed into quarter fixed effects, different states might have different sensitivities to the national cycle (e.g., if they specialized in cyclically sensitive industries like manufacturing). If these more sensitive states happened to receive relatively smaller transfers, that could drive a positive multiplier. I address this concern in three ways. First, for the regression in row B1 of Table 1, I include US GDP growth × state fixed effects (also included in the benchmark specification), which increases the estimated temporary transfer multipliers marginally but does not affect significance. Second, I interact time fixed effects with the average manufacturing share of the state's GDP, which allows manufacturing-intensive states to grow more slowly in

[18] The coefficient of population growth is negative (−0.37) and is statistically significant (not reported). Population growth has little effect on temporary transfer estimates and is often insignificant.

recessions. This also has little effect on temporary transfer multipliers (row B4). Finally, I construct a peak-to-trough measure of the depth of the 2001 and 2008 recessions in each state and interact it with the mean size of the transfer variable each quarter—again multipliers are unchanged (row B6).[19]

Although my sample of permanent Social Security benefit increases are not explicitly countercyclical (by Romer and Romer's narrative), I also control for industrial structure and differential state sensitivities to the aggregate business cycle for robustness. As mentioned above, adding 50 control variables for US GDP growth $\times$ state fixed effects (row B1 of Table 1, first column) reduces the estimated multiplier to 1.14 (significant at the 5 percent level). Controlling for each state's average manufacturing share of GDP $\times$ quarter fixed effects (92 variables, row B4, first column) reduces the permanent transfer multiplier to 0.92 (significant at the 5 percent level), though I cannot reject that the coefficient equals 1.5 (as in the parsimonious specification). The low coefficient is explained by the saturation of controls rather than by economic trends; interacting the manufacturing share with annual rather than quarterly fixed effects yields a multiplier of 1.57 (not reported).

Oil prices fell rapidly during the 2008 recession and were also volatile in the early 1970s. Oil prices are naturally subsumed into time fixed effects, but growth in oil-producing states might be stimulated (hurt) by higher (lower) oil prices, and those states might have coincidentally received higher or lower transfers. To assuage this concern, I add 100 controls for state-specific sensitivities to log real oil prices (50 contemporaneous variables and 50 first lags, which flexibly controls for price levels and changes). For both permanent and temporary transfers, the estimated multiplier is very similar to that in the parsimonious specification and is significant at the 1 percent level (row B2).

To cover any other general time-varying state-specific covariates, I also control for a quadratic state-specific trend (some terms of which are dropped due to collinearity). Doing this yields a permanent transfer multiplier of 1.71 (significant at the 1 percent level, row B3 first column), which is slightly higher than that in the parsimonious specification, but has little effect on multiplier estimates for temporary transfers.[20]

Finally, I control for an omitted variable specific to the permanent Social Security sample: three one-off back payments (in 1965, 1970, and 1971) in compensation for delayed increases in Social Security benefits (Romer and Romer 2016). These temporary payments coincided with the increases in permanent Social Security benefits in those years and had similar eligibility requirements. In row B5 of Table 1, I control for these temporary transfers (and their first lag), which has little effect on the permanent transfer multiplier.[21]

[19] The peak (trough) is the maximum (minimum) per capita income in the two years before (after) the NBER recession dates, 1999–2000 and 2006–2007 (2002–2003 and 2009–2010). These controls turn out to be insignificant at the 5 percent level.

[20] Online Appendix Table 4 presents several robustness tests with different standard errors (homoskedastic, robust without clustering and allowing for spatial error correlation). Permanent transfer multipliers are still significant at the 5 percent level, and temporary transfer multipliers significant at 1 percent or 5 percent, except for homoskedastic errors in the GDP specification (but in any case, homoskedasticity is an extreme assumption given diverse states).

[21] The temporary Social Security increases and first lag are insignificant coefficients of -0.58 ($t = -1.33$) and -0.16 ($t = -0.28$), respectively. I do not put much weight on these temporary transfer estimates, relative to those of the stimulus payments, because they are imprecise: standard errors are around five times larger.

Dynamic Controls.—Panel C of Table 1 addresses the concern that the parsimonious specification in equation (2) might miss important dynamics. The first row (C1) adds the lagged log level of income per capita to control for the possible effect of unconditional convergence on growth rates. Although this control is negative and highly significant (perhaps also reflecting mean reversion), it does not have much effect on the size or significance of transfer multiplier estimates. The second row (C2) allows for persistence in state-level growth rates by controlling for a lagged dependent variable, which has little effect on the size or significance of temporary transfer estimates. However, the lagged dependent variable reduces the estimated permanent transfer multiplier to 1.2 (still significant at 5 percent), though this is mostly due to sample selection (excluding the quarter after a removed outlier).

A final concern regarding dynamics is that the specification in equation (2) imposes a symmetric "up-down" profile of the effect of temporary transfers: if a one-off transfer causes a *rise* in output by X percent output when paid, then withdrawing those transfers a quarter later should lead to a *fall* in output of X percent. If the restriction does not hold, then impact multiplier estimates could be biased. Here I add an extra lag of transfers as an additional control in row C3, which partially relaxes this restriction. Temporary transfer multipliers for labor income are similar. The multiplier for GDP is slightly larger but is less precisely estimated, causing the p-value to fall to 6 percent. Permanent Social Security increases are never withdrawn, and so are not affected by the symmetry assumption, but the extra lag provides a robustness test against misspecification of the timing of payment increases across quarters. The impact multiplier for permanent transfers is mostly unchanged (significant at the 5 percent level), and the first transfer lag is only significant at the 10 percent level (not reported).

Alternative Samples.—So far I have tested robustness to different controls, but it is also possible that the estimated multipliers are driven by specific states or years and might not be reflective of a general relationship. For permanent transfers, no individual states are influential when dropped one by one (see online Appendix Figure 4), but the multiplier does move by more than one standard error if either 1952 or 1972 are excluded. In Table 1, row D1, I report estimates excluding both these influential years, which has little effect on the multiplier, as their effects are mostly offsetting (standard errors are slightly larger, and significance is now at the 5 percent level). For temporary transfers, the scatter plots in Figure 2 suggest that Mississippi (MS) might be influential. In the right two columns of row D1, I drop MS and show that it has little effect on estimated multipliers, which are still significant at 5 percent. No other states are influential when dropped one by one (see online Appendix Figure 3).[22]

[22] The second to last column of online Appendix Table 4 adds back in previously dropped extreme outliers, where $|\text{growth}| > 20$ percent (annualized). For permanent transfers, that reduces significance to a p-value of 8 percent, and it also increases the estimated multiplier to two. For temporary transfers, there is little effect on the labor income specification, but these extreme observations make the GDP specification go to zero (and become insignificant). Dropping the two smallest states in 1980 (Alaska and Wyoming, last column of online Appendix Table 4), with extremely volatile growth rates and a combined population of $<$1m in 1980, increases the permanent transfer multiplier substantially (positive and significant at the 1 percent level) and results in multipliers for the GDP specification similar to those estimated in Table 1 (significant at 5 percent). See online Appendix Section 3.2 for further details.

Instrumental Variable Estimates.—As discussed in Section IC, the specification in equation (2) is a reduced form estimated by OLS, as the exogenous changes in transfers are essentially the variable of interest. However, it is worth estimating an IV specification (row D2) in case the constructed transfer variables are measured with error (which could attenuate multiplier estimates) or there are other correlated transfer payments that I am not capturing (leading to upward bias). For permanent transfers in the first column, I instrument all Social Security transfers, which are potentially endogenous, using my sample of exogenous permanent Social Security increases. The multiplier is around 1.9, significant at the 1 percent level (the first-stage F-statistic is above 500).[23] For one-off transfers in the second and third columns, I instrument changes in the potentially endogenous BEA category All Other Personal Current Transfer Receipts (as a share of labor income or GDP) using the pooled 2008 stimulus payments (as above) as instruments. Multipliers are similar to the parsimonious specification and are significant at the 5 percent level.[24]

D. *Placebo Tests*

Even if I cannot control for potential confounding variables directly, I can test for omitted variable bias by running a placebo regression using counterfactual growth rates when the confounding variable might be influential but no transfers were actually paid. A non-zero multiplier during these periods might indicate a spurious relationship. In the first set of placebo regressions, shown in panel A of Figure 2, I test whether states receiving large one-off transfers always grew faster during recessions by regressing growth rates during the three quarters of the 1990–1991 recession on the 2001 and 2008 transfers.[25] The estimated multipliers are always insignificant and are usually close to zero.

Alternatively, the potential confounding variable might be influential in the years around the time the transfer is actually paid. To test for this, I counterfactually move transfers backward or forward by up to six quarters (using the benchmark specification from Table 1). In addition to detecting spurious results, it can also pick up anticipation effects (for short leads, $t < 0$) or delayed effects (for short lags, $t > 0$). For permanent transfers, the results are shown in panel B of Figure 2. One can see that the largest t-statistic is at $t = 0$ (when the actual transfers occurred), and all

[23] The size of the IV coefficient is unsurprising, as a \$1 increase in my exogenous permanent Social Security transfers series increases all BEA Social Security payments by about \$0.80 (after controlling for time and state FEs). The strong first-stage relationship is plotted in panel A of online Appendix Figure 2 and provides an additional validation of the cross-state allocation of transfers and Romer and Romer's aggregate narrative.

[24] This endogenous BEA category includes all transfers that are not Social Security benefits, Medicare, or Medicaid payments or state unemployment compensation. Following a definitional change in 2015, the BEA classifies tax rebates as transfers if they are at least partially refundable, which includes all 2008 transfers but excludes the 2001 transfers, and so the latter are not included in the instrument. Multipliers are similar in the IV and reduced-form specifications because growth in All Other Personal Current Transfer Receipts in 2008:II is extremely well explained by growth in stimulus transfers as constructed above (see panel B of online Appendix Figure 2), with first-stage coefficients close to 1 and first-stage F-statistics over 800. This provides an external validation of my construction of the cross-state allocation of the 2008 stimulus payments.

[25] According to the NBER's definition, the 1990–1991 recession lasted through 1990:III, 1990:IV, and 1991:I, so I report placebo payouts during all of those dates. I thank an anonymous referee for suggesting this placebo test. The sample period for these regressions is 1990–1997, and they use the benchmark specification.

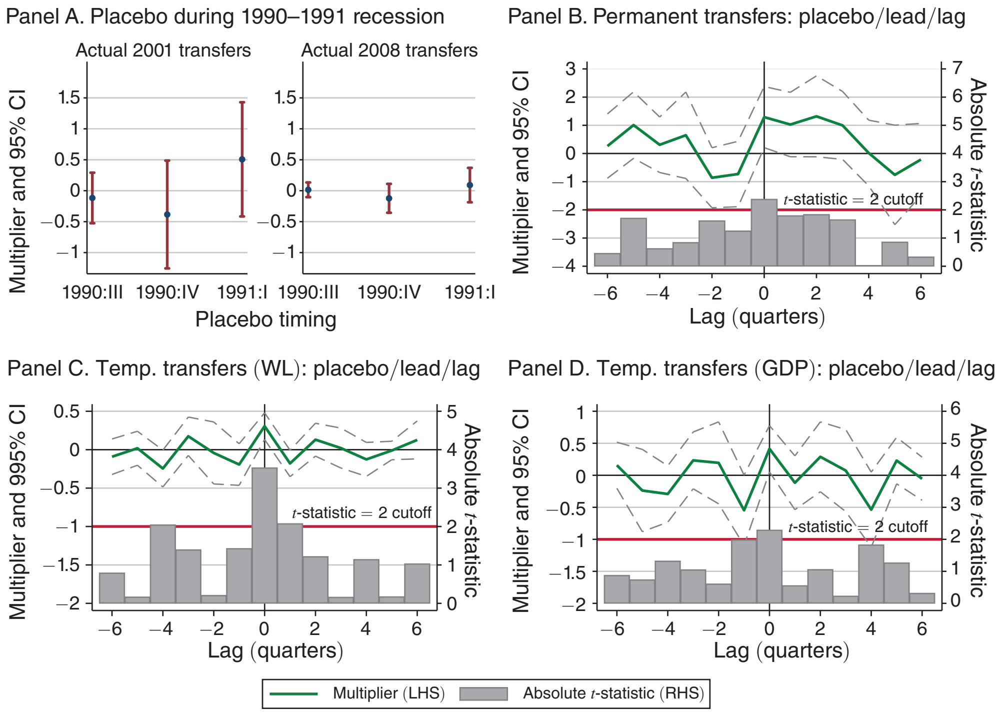

Figure 2. Placebo Tests

Notes: Panel A: Multiplier and 95 percent confidence intervals from regressions of real labor income growth during the 1990–1991 recession on actual state-level transfers from 2001 or 2008 using the benchmark specification; placebo timing (when first placebo transfer received). Panels B–D: Regressions of growth on transfers using the benchmark specification (permanent transfers, temporary transfers for labor income, and temporary transfers for GDP, respectively) but moving the growth variable backward/forward by up to six quarters.

Sources: BEA, Romer and Romer (2016), author's calculations

other leads and lags are insignificant at the 5 percent level (t-statistic less than the red cutoff of 2), which is what one would expect to see.[26]

For temporary transfers (Figure 2, panels C and D), the largest t-statistic is always at $t = 0$ (when the actual transfers were paid), and there is little evidence of consistently positive (or negative) coefficients at other times.[27] However, there are some marginally significant lags at $t = -4$ (growth a year before the transfer was paid) and $t = 1$ (growth the quarter after the transfer was paid) using the labor income specification and $t = -1$ (one quarter lead, GDP specification) and some other marginally insignificant leads and lags at the 10 percent level. The negative

[26] The estimated multiplier (green line) appears to be negative in the couple of quarters in anticipation of the Social Security rate increase and positive in the few quarters following the increase and is sometimes significant at the 10 percent level. 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. However, I do not want to emphasize those results given their insignificance and general lack of robustness (not reported).

[27] Both stimulus payments were legislated a quarter in advance, but there is little robust evidence of significant anticipation effects. In any case, Ricardian households that respond to news also save temporary transfers.

first lead in the GDP specification seems to be due to the overlap of the placebo withdrawal of stimulus in 2008:II and the effect of the actual stimulus. If I run the same specification and omit 2008:II from the sample (when most of the transfers were actually paid) but keep 2008:I and the other quarters, the coefficient on the lead halves and the p-value increases to 0.4.

Some marginally significant placebo coefficients are to be expected. Under a random allocation with no true effect, the $\approx$40 placebo regressions in Figure 2 would produce two false positives, on average, when testing at the 5 percent level. Moreover, the chance of no false positives is very low at around 12 percent ($= 0.95^{42}$) under a random allocation.

Having now established the robustness of the main results, I now consider two extensions involving heterogeneity and dynamics.

E. *Extension 1: Heterogeneity by Quarter and Transfer Policy*

So far, I have presented regression results that restrict the temporary transfer multiplier to be identical across payment and withdrawal quarters and across stimulus policies. In this subsection I relax those assumptions, with results presented in Table 2 (using the benchmark specification).[28] I first allow for heterogeneity by payment and withdrawal quarter and then by transfer component and finally discuss the results in the context of the large literature on the MPC for one-off transfer payments.

Panel A of Table 2 relaxes the symmetry restriction in equation (2) by allowing multipliers to vary across payment and withdrawal quarters. Despite substantial heterogeneity in estimated coefficients, I fail to reject the restriction that all payment and withdrawal multipliers are equal, justifying the pooled specification used in the main results in Table 1.[29] For labor income in 2008, multipliers are around 0.3 for both payment and withdrawal quarters and are remarkably similar to the pooled estimates in Table 1.[30] For the 2001 transfers (labor income) and 2008 transfers (GDP), the point estimates are larger when transfers are paid than when they are withdrawn, though these differences are not statistically significant. Specifically, the 2001:III (labor income) and 2008:II (GDP) payment-quarter multipliers are around 0.9, but multipliers on withdrawal are close to zero. This means states receiving a larger transfer may continue to have higher relative income even after that transfer was withdrawn, though the evidence is not strong enough to be definitive.[31]

Panel B of Table 2 breaks down pooled temporary transfers into three components described in Section IA—i.e., 2008 refundable (low-income) rebates, 2008 (middle-income) tax refunds, and 2001 transfers—using the same sample period and specification as the main results in Table 1 (which also imposes “up-down” symmetry). Despite substantial heterogeneity in estimated multipliers for different policies, I

[28] Results using the parsimonious specification are shown in online Appendix Table 3 and are generally similar.

[29] That is, the restriction $\beta_{2008:\mathrm{II}} = \beta_{2008:\mathrm{III}} = \beta_{2001:\mathrm{III}} = \beta_{2001:\mathrm{IV}}$ has a p-value of 0.4 for the labor income specification, and the restriction $\beta_{2008:\mathrm{II}} = \beta_{2008:\mathrm{III}}$ has a p-value of 0.1 for the GDP specification.

[30] When pooling across 2001 and 2008 transfers, the estimates are always similar to the 2008 multipliers due to their greater leverage (not reported).

[31] Online Appendix Figure 5 shows that when split into two groups, high-transfer states and low-transfer states exhibit similar trends before the transfer, but high-transfer states grow faster the quarter the transfer is paid, leaving a gap in normalized incomes. However, dividing states into only two groups removes much of the important variation in transfer size, and so the regression estimates are preferred.

Table 2—Heterogeneity by Quarter and Transfer Policy

Panel A. Heterogeneity by quarter			*Panel B. Heterogeneity by transfer component*		
	Labor income	GDP		Labor income	GDP
2008:II Δ transfer (paid)	0.27	0.90	2008 Δ tax rebates	0.36	0.64
	(0.14)	(0.38)	(low-income)	(0.10)	(0.25)
2008:III Δ transfer (withdrawn)	0.31	−0.19	2008 Δ tax refunds	0.07	−0.18
	(0.12)	(0.33)	(middle income)	(0.31)	(0.57)
2001:III Δ transfer (paid)	0.84		2001 Δ tax refund	0.44	
	(0.34)			(0.24)	
2001:IV Δ transfer (withdrawn)	0.03				
	(0.41)				
p-value (equal coeff.)	0.41	0.10	*p*-value (equal coeff.)	0.59	0.24
State and quarter fixed effects	Yes	Yes	State and quarter FEs	Yes	Yes
Benchmark controls	Yes	Yes	Benchmark controls	Yes	Yes
Observations	1,588	742	Observations	1,588	742

Notes: Each column represents a regression of the growth rate of real labor income per capita or real GDP per capita on the change in scaled temporary transfers. The regressor is disaggregated by the quarter of payment/withdrawal (panel A) or by the transfer policy (panel B). All specifications have state and quarter fixed effects, with "benchmark" controls for population growth and US GDP growth × state fixed effects. Robust standard errors are in parentheses (clustered by state). Growth outliers greater than 20 percent (annualized, absolute value) are dropped.

fail to reject that all coefficients are equal, with *p*-values 0.59 (labor income) or 0.24 (GDP), justifying the pooled specification.

Overall, the pooled results seem to be driven by the 2008 low-income rebate, which is consistent with Parker et al.'s (2013) finding that low-income households had a large and statistically significant MPC the same 2008 transfers.[32] With labor income as a dependent variable, the 2008 low-income rebates have a relative multiplier of 0.36 (significant at the 1 percent level) which is similar to the pooled multiplier from Table 1. The 2001 stimulus payments have a similar multiplier of 0.44 but have much wider standard errors (leading to significance only at the 10 percent level). For GDP growth as a dependent variable, the coefficient on the low-income rebates is even larger at around 0.64 (significant at the 1 percent level), which also drives the pooled results.

The 2008 middle-income tax refund seems to have little effect on state-level growth. Specifically, the point estimates are close to zero for the labor income specification and negative for the GDP specification but are insignificant in both cases. This might be because a larger fraction of this payment was saved, as middle-income households receiving the bulk of this transfer were not financially constrained. Alternatively, it could be due to the transfer being spent on durable goods produced in other regions, which in turn depends on the transfer size (recall the 2008 mid-income rebate was around $600 per taxpayer, double the size of the other one-off transfers). Larger transfers can be spent on "big ticket" durable goods

[32] Johnson, Parker, and Souleles (2006) also find that the 2001 transfers were more likely to be spent by low-income households, though the 2001 transfer was nonrefundable and so cannot be disaggregated as easily in my sample. This could be because low-income households are more likely to be financially constrained. However, Misra and Surico (2014) argue that both low- and high-income earners had the highest MPC, with the latter being the "wealth hand-to-mouth" of Kaplan and Violante (2014).

with less local content, and indeed Parker et al. (2013) find a larger fraction of the 2008 rebate coefficient was spent on durables.[33]

F. *Extension 2: Cumulative Multipliers with Flexible Dynamics*

In this section, I estimate cumulative multipliers $\mathcal{C}_h$ over the first few quarters following each transfer shock, which allow for dynamics beyond the first quarter (Figure 3 in black circles, with 95 percent confidence interval bars). I use the benchmark specification with state and time fixed effects, and so $\mathcal{C}_0 (h = 0)$ represents the impact multipliers in the second row of Table 1.[34] The specifications estimated are presented in Section IC. As permanent transfers are likely to have longer-lasting effects than one-off transfers, I estimate over a longer horizon (as do others in the literature). For permanent transfers, I follow Romer and Romer (2016) and estimate over a horizon of a year ($h + 1 = 4$ quarters), and for one-off transfers I follow Parker et al. (2013) and estimate over a horizon of half a year ($h + 1 = 2$ quarters).[35]

For permanent transfers (panel A of Figure 3), the cumulative multipliers are roughly constant at about 1.5 and are similar to impact multipliers presented in panel A of Table 1 (within the 95 percent confidence interval). While the multiplier rises to $\mathcal{C}_3 = 1.7$ after a year, this is less than half of a standard error from 1.5. For temporary transfer shocks and labor income (panel B(ii)), $\mathcal{C}_1 = 0.26$ is very similar to the impact multiplier estimate of 0.31 (and insignificantly different). For temporary transfers and GDP (panel B(iii)), the point estimate $\mathcal{C}_1 = 0.8$ is larger than the impact multiplier estimate of 0.4, but they are insignificantly different. In part this is because $\mathcal{C}_1$ is imprecisely estimated: the standard errors triple in width, even leading to a loss of significance.[36]

In sum, my main finding in this subsection is that the impact multipliers reported in Table 1 are a good summary statistic of cumulative multipliers over subsequent quarters; point estimates are often similar, and any differences are not statistically significant. Ramey (2019) also finds that cumulative spending multipliers do not vary greatly by horizon.

[33] Hausman (2016) also reports that a sizable fraction of the 1936 veterans' bonus was spent on durables, including cars, though that payment was much larger as a share of recipient income. Other evidence on the effect of transfer size on the MPC is mixed. Hseih (2003) finds that households saved large Alaska permanent fund dividends but tended to spend small tax refunds. But Kueng (2018) finds that those permanent fund dividends were spent, mostly by high-income households, with a MPC of 0.25.

[34] For $h > 0$, I drop the top and bottom 1 percent observations of $\sum_{j=0}^{h} (Y_{i,t+j} - Y_{i,t-1})/Y_{i,t-1}$ for permanent transfers using the projection method. Due to mean reversion, these influential observations are not well captured when dropping extreme quarterly growth rates.

[35] Longer horizons introduce other confounding variables, like the 2009 fiscal stimulus.

[36] Of the 0.4 increase in the GDP multiplier from $\mathcal{C}_0$ to $\mathcal{C}_1$ in Figure 3, about two-thirds is due to a larger impact multiplier in a specification that includes a lagged transfer variable. However, the larger estimates of $\mathcal{C}_1$ are not robust, as they only occur in the benchmark specification and not in the parsimonious specification (see online Appendix Table 6, panel C). Additionally, lagged transfers have little effect on the impact multiplier in the parsimonious specification (Table 1, row C3 and online Appendix Table 6, panel C).

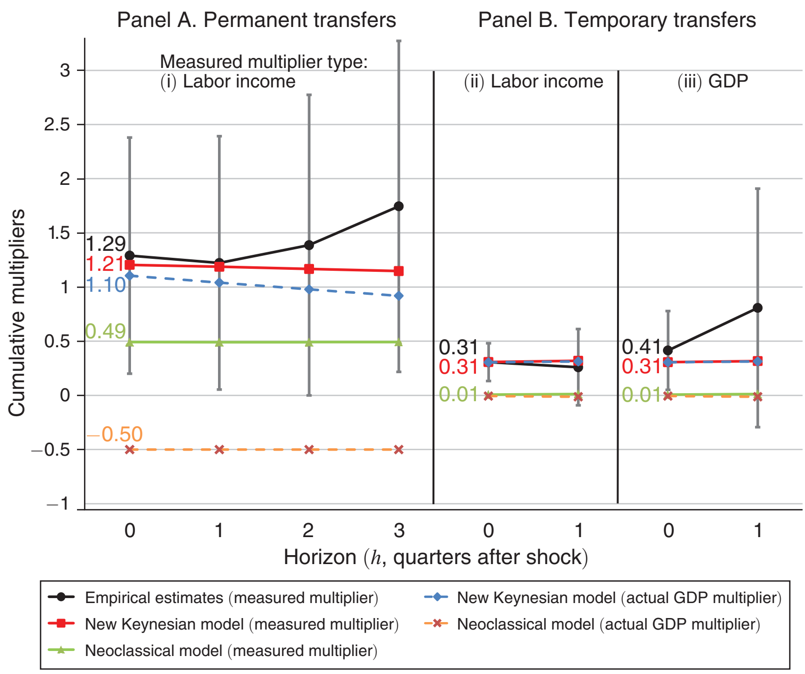

Figure 3. Cumulative Cross-Region Transfer Multipliers (Data and Models)

Notes: This figure compares cumulative cross-region transfer multipliers (y-axis) estimated in the data and theoretical models over different horizons. Panel A reports cumulative multipliers for permanent transfers, with temporary transfers in panel B. The black lines (circles) are estimated cumulative multipliers using projection methods for permanent transfers and a distributed lag model for one-off transfers (see Section IC). Error bars are 95 percent confidence intervals. The first quarter is the impact multiplier, as in Table 1 (benchmark specification). Red lines (squares) and green lines (triangles) report cumulative "measured multipliers" (deflated using national prices) in the New Keynesian model and neoclassical models, respectively. Measured multipliers have a dependent variable of per capita labor income in subpanels (i) and (ii) and per capita GDP in subpanel (iii). Dashed lines are actual (not measured) GDP multipliers over the same horizon in the New Keynesian model (blue diamonds) and the neoclassical model (orange crosses).

III. Understanding Cross-Region Transfer Multipliers in an Analytical Model

This section presents analytical expressions for the cross-region transfer impact multiplier estimated in Section II—when it will be large, small, or negative—and shows how it differs from the MPC and cross-region purchase multiplier. To do this, I use two simplified open-economy models that I can solve analytically: (i) the limit of a full New Keynesian model as prices/wages become rigid (an "ultra Keynesian" model) and (ii) a canonical neoclassical model. The neoclassical model and full New Keynesian model are used in Section IV to quantitatively interpret the empirical estimates. So long as fiscal shocks are not too persistent, the analytical impact multipliers presented here are informative about those presented in the empirics and in the full quantitative models.[37] As the models are relatively standard, I provide an

[37] For highly persistent shocks, some differences arise as relative prices move. Specifically, persistent shocks allow for substantial movement in relative prices in the full New Keynesian model in the long run, which result in lower multipliers than predicted by the simple New Keynesian model with rigid prices and wages. In the neoclassical

overview here, with the complete model (and list of equations) presented in online Appendix Section 4.

The full New Keynesian model is similar to Nakamura and Steinsson's (2014) separable preferences, incomplete markets New Keynesian model (see their Section IVD) but with two main differences. First, a share ω of households consume their income hand-to-mouth as they cannot borrow/save (the remaining fraction $1-\omega$ are Ricardian and borrow/save through a risk-free bond), and second, wages are sticky and cannot be changed each quarter with probability θ_w, as in Erceg, Henderson, and Levin (2000) (θ_p is the analogous Calvo stickiness of prices).[38] Hand-to-mouth households are needed to match the multiplier on temporary transfers, and sticky wages are needed to match the impact multiplier for permanent transfers.[39] The New Keynesian model is also similar to Galí and Monacelli (2005) but with sticky wages, hand-to-mouth households, and incomplete markets for Ricardian households. The neoclassical model is the New Keynesian model with flexible prices and wages ($\theta_w, \theta_p \rightarrow 0$) and no hand-to-mouth households ($\omega = 0$).

As in Nakamura and Steinsson (2014), the economy is a monetary union consisting of a small home region (representing a small US state with population n) and a large foreign region with population $1-n$ (representing the rest of the United States), each of which produces their own variety of imperfectly substitutable goods that can be consumed locally, consumed abroad, or used for local government purchases. In the analytical models, $n \rightarrow 0$, so the small region becomes infinitesimal.

While the model is relatively standard, the application to cross-region transfer multipliers (rather than purchase multipliers) is mostly new. The closest paper is Farhi and Werning (2016). As part of its analysis of the financing of government purchases, that paper has related multiplier expressions that overlap with mine when there are no hand-to-mouth households ($\omega = 0$).

A. *Analytical Cross-Region Transfer Impact Multipliers*

Proposition 1 presents cross-region transfer impact multipliers (denoted $\mathcal{M}_{Tr}$) in the simple rigid price/wage New Keynesian model and simple neoclassical model. $\mathcal{M}_{Tr}$ is the dollar increase in real output in the first quarter in a small home region whose households receive a \$1 lump-sum transfer from the "federal government." The "federal government" here funds the \$1 transfer by levying lump-sum taxes on the residents of the rest of the monetary union. The transfer is untargeted, so hand-to-mouth agents receive a fraction ω of the transfer, equal to their population share. As long as the transfer is financed federally, further financing details (deficit financing, distortionary taxation, etc.) do not affect $\mathcal{M}$, as the small home region is infinitesimal in

model with persistent shocks, regional prices adjust, so empirical "measured multipliers" using national price deflators are higher than actual multipliers (using regional deflators): see Section IV.

[38] As is common in models with hand-to-mouth households and sticky wages, (i) forward-looking nominal wage setting only takes into account the Ricardian household's labor-leisure first-order 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 studied in Giambattista and Pennings (2017).

[39] 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). The simple New Keynesian model is the limit of the full New Keynesian model when prices and wages become perfectly sticky ($\theta_w, \theta_p \rightarrow 1$).

size.[40] Here α is home bias in consumption, β (close to 1) is the household's quarterly discount factor, and ρ is the quarterly persistence of the transfer (which follows an AR(1) process). Proofs are available in online Appendix Section 7.

PROPOSITION 1: *Cross-region transfer impact multipliers in the rigid price/wage New Keynesian (NK) model and neoclassical (NC) models are given by*

$$(a)\quad \mathcal{M}_{Tr}^{NK} = \underbrace{\frac{\alpha}{1-\alpha}}_{LocalGE(perm)} \times \underbrace{1 \times \frac{1-\beta}{1-\beta\rho}}_{MPC(perm)} + \underbrace{\frac{\alpha}{1-\alpha\omega}}_{LocalGE(temp)} \times \underbrace{\omega \times \left[1 - \frac{1-\beta}{1-\beta\rho}\right]}_{MPC(temp)},$$

$$(b)\quad \mathcal{M}_{Tr}^{NC} = \underbrace{-\frac{1}{1+\varphi}}_{LocalGE(perm)} \times \underbrace{1 \times \frac{1-\beta}{1-\beta\rho}}_{MPC(perm)} + 0.$$

Proposition 1 decomposes the cross-region transfer multipliers in each model into two terms relating to the permanent and temporary components of the transfer. The permanent component of a \$1 cross-region transfer payment is given by the annuity value of the transfer $\$(1-\beta)/(1-\beta\rho)$. The temporary component is the excess of the initial \$1 payment over its annuity value: $\$(1-(1-\beta)/(1-\beta\rho))$. As the transfer shock becomes perfectly persistent ($\rho \to 1$), the permanent component approaches unity and the temporary component approaches zero (and vice versa when $\rho = 0$ for one-off payments). The cross-region transfer multipliers depend on the fraction of each component that is spent, as well as the local general equilibrium effects, which I discuss in turn.

The permanent component is spent by all households (Ricardian and hand-to-mouth) according to the permanent income hypothesis, yielding a MPC contribution of $1 \times (1-\beta)/(1-\beta\rho)$. This is the same in simple New Keynesian and neoclassical models. The temporary component is spent only by hand-to-mouth households. These households receive a fraction ω of the total temporary component of the transfer $\$(1-(1-\beta)/(1-\beta\rho))$, yielding a MPC contribution of $1 \times \omega[1-(1-\beta)/(1-\beta\rho)]$. The micro literature on the MPC (e.g., Johnson, Parker, and Souleles 2006) focuses on the consumption response to one-off transfers ($\rho = 0$) and so only identifies $MPC(temp) \approx \omega$, which is only one part of the expression for the cross-region transfer multiplier. In neoclassical models, $\omega = 0$, yielding $MPC(temp) \approx 0$, and a cross-region transfer multiplier that is trivially close to 0 for one-off shocks.

Local general equilibrium effects amplify or dampen the MPC in the expressions in Proposition 1. In New Keynesian models, output is demand-determined in the short run, so local demand and output initially increase by the fraction α that is spent on locally produced goods (α is consumption home bias for an atomistic region, in the numerator in the expression for local general equilibrium effects). This increase is then amplified by later-round effects. For the permanent component, an extra α of local demand/income generates α^2 local demand/income (and so forth), amplifying

[40] $\mathcal{M}_{Tr}$ can also be interpreted as the response of output in the small region *relative* to the rest of the monetary union (like in the empirics).

the initial effect by the traditional Keynesian multiplier $1/(1-\alpha)$. For the temporary component, the amplification mechanism is similar, but in each round the extra income is only spent by hand-to-mouth households, yielding a smaller increase in local demand of $\alpha\omega$, $(\alpha\omega)^2, \ldots$ and hence a smaller traditional Keynesian multiplier of $1/(1-\alpha\omega)$.

In the neoclassical model, local general equilibrium effects are negative. Any increase in consumption demand for local goods (from a transfer) will result in an increase in the relative price of the home good and will cause a shift in expenditure toward foreign goods until the extra demand is exhausted. Output then falls as households spend some of their extra income on leisure. With the preferences in my model, the size of wealth effects on labor supply depend on the size of the Frisch elasticity φ^{-1}. The larger the Frisch elasticity, the stronger the wealth effects and the more negative the local general equilibrium effects $-1/(1+\varphi)$. As such, in the neoclassical model, cross-region transfer multipliers range from close to 0 (for one-off transfers) to $-1/(1+\varphi)$ for permanent transfers.

B. *Relation to Cross-Region Purchase Multipliers*

Cross-region purchase multipliers estimated in the literature are very different from cross-region transfer multipliers, with purchase multipliers being larger in both the New Keynesian and neoclassical models.

In the simple New Keynesian model, federal purchases of local goods have multipliers *exactly one unit greater* than those of the cross-region transfer multipliers above (see Proposition 2a in online Appendix Section 7). The reason for this is that for a purchase, the initial \$1 of extra income is generated as payment for an extra unit of local output rather than as a windfall. In a very open small region ($\alpha \approx 0$), the cross-region transfer multiplier would be close to 0, but the purchase multiplier would be around 1. In proportional terms, cross-region transfer multipliers are also much more sensitive than purchase multipliers to model parameters such as shock persistence ρ, consumption home bias α, and the hand-to-mouth household share ω.

In the neoclassical model, cross-region government purchase multipliers are always positive (see Proposition 2b in the online Appendix) because they increase demand for local goods, and hence prices and wages, without any wealth effects on labor supply (I assume government purchases are not valued by households). Again, this is very different from the cross-region transfer multiplier, which is negative in neoclassical models.

IV. Quantitative Cross-Region Transfer Multipliers

In this section, I investigate the extent to which standard New Keynesian or neoclassical models can rationalize the size of the multipliers estimated in the data. The full models used here are similar to the analytical models presented at the start of Section III, but the full New Keynesian model has Calvo sticky prices/wages rather than rigid prices/wages. The population share of the home region is calibrated to represent a typical US state ($n = 1/50$ rather than $n \rightarrow 0$ as in the analytical model). Other parameters are taken from Nakamura and Steinsson (2014), who present a very similar model except for parameters relating to two features not in their model: the fraction of hand-to-mouth households ($\omega = 1/3$, based on evidence from Kaplan, Violante, and Weidner 2014) and sticky wages that adjust once a year, on average ($\theta_w = 0.75$). See online Appendix Table 10 for a full list of parameters.

Before comparing model-based and empirical multipliers, it is important to ensure that these multipliers are calculated in the same way. As there are no official state-level price data, empirical estimates in Section II are produced by deflating nominal labor income or nominal GDP by *national* inflation.[41] I call these "measured multipliers" to distinguish them from the actual multipliers in Proposition 1 (actual multipliers are quantities, which are nominal home GDP deflated by *local* (state-level) producer prices). Measured and actual impact multipliers are very similar in New Keynesian models (due to sticky prices), but in neoclassical models with persistent shocks, demand shocks increase local prices, leading to measured multipliers that are substantially larger than actual multipliers.[42]

A. *Comparing Measured Multipliers in the Models and in the Data*

Figure 3 compares measured cumulative multipliers in the models and data over the first few quarters following the transfer shock. In sum, I find the New Keynesian model is more consistent with the empirical evidence than the neoclassical model, which is why I use it in Section V for a discussion of policy implications. Also note that cumulative multipliers over the first few quarters are very similar to impact multipliers in both canonical New Keynesian and neoclassical models, suggesting that the impact multiplier is a good summary statistic of later dynamics.

Panel A of Figure 3 compares permanent cross-region transfer multipliers in the model and data. The empirical impact multiplier of 1.29 (black circles, same as the benchmark specification in Table 1) is almost the same as the impact measured multiplier in the New Keynesian model of 1.21 (red squares). Despite missing the rise in point estimates in the final quarter, cumulative New Keynesian multipliers are close to the center of the 95 percent confidence interval, even over longer horizons. In contrast, the *measured* cumulative multiplier in the neoclassical model is 0.5 over all horizons, which is on the border of the 95 percent confidence interval.

Panels B(i) and B(ii) of Figure 3 compare empirical and theoretical cross-region temporary transfer multipliers for labor income and GDP dependent variables, respectively. For labor income, the cumulative measured multipliers in the data are close to 0.3 and are close to the multipliers in the New Keynesian model. For

[41] Using my own rough proxy of quarterly state-level prices, I find little robust evidence that the transfers studied above increased local inflation. Regressions of my quarterly state inflation proxy on transfers are always insignificant in the 2000s, and over 1952–1974 transfers are always negative and insignificant once I control for the state-specific effects of oil price shocks (not reported). Nakamura and Steinsson (2014) find 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 annual CPIs.

[42] In the analytical models, measured GDP and labor income in the home region can be rewritten as $\hat{Y}_{meas.} \approx (\hat{Y}_h + \hat{P}_h)$ and $(\widehat{WL})_{meas.} \approx (\hat{w}_h + \hat{Y}_h + \hat{P}_h)$, as $n \approx 0$, $\hat{Y}_h = \hat{L}_h$, and there are no aggregate or foreign shocks (hats denote deviations from steady state, $\hat{P}_h$ is the producer price of the home good). In the neoclassical model, $\hat{Y}_{meas.} = (\widehat{WL})_{meas.}$, as $\hat{w}_h = 0$. Lower values of the Armington elasticity θ_T generate larger movements in $\hat{P}_h$ in the neoclassical model and hence generate potentially larger increases in measured labor income or measured GDP in response to a permanent transfer. 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.

GDP, the multiplier of 0.3 in the New Keynesian model is a little too low but is always toward the center of the 95 percent confidence interval. In contrast, the lack of a response to a temporary transfer shock in the neoclassical model is outside the 95 percent 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.

B. *Cross-Region Transfer Multipliers (Quantities) in the Full Models*

The dashed lines in Figure 3 report actual GDP multipliers (not measured multipliers) in the New Keynesian and neoclassical models for comparison. For temporary transfers, actual multipliers are almost identical to measured multipliers in both New Keynesian and neoclassical models (as prices and wages do not move much): about 0.3 in the New Keynesian model and 0 in the neoclassical model. For permanent transfers, actual GDP multipliers in the New Keynesian model (blue dashed lines, impact multiplier of 1.1) are marginally smaller than measured labor income multipliers, as wages and prices increase slightly in response to a permanent transfer. However, in the neoclassical model, a 1 percent of GDP permanent transfer generates an actual multiplier of -0.5 $(= -(1+\varphi)^{-1}$ with $\varphi^{-1} = 1)$, which is much lower than the measured multiplier of $+0.5$ because home producer prices increase by 1 percent. This illustrates the importance of measuring empirical and theoretical multipliers in a consistent way.

V. Some Policy Implications

In this section I first discuss the implications of my empirical estimates for the ability of federal automatic stabilizers to dampen regional shocks in a monetary union and then relate my findings to the size of the aggregate transfer multiplier in a closed economy.

The primary policy implication of my cross-region transfer estimates is that the automatic stabilizers built into the US federal fiscal system can only provide modest stabilization of regional output at business cycle frequencies.[43] The ability of the US federal system to smooth regional shocks has become prominent of late, as eurozone policymakers have looked across the Atlantic for an alternative fiscal structure following the deep recessions in a number eurozone countries (see *The Economist's* quote in the introduction). The literature cited in the introduction (and summarized in online Appendix Section 2.2) suggests that when a US region enters a recession, the residents of that region receive extra federal social benefits and pay fewer federal taxes, generating a cross-region transfer of around \$0.30 for every \$1 fall in income (known as the normalized tax change, NTC).[44] This is effectively a temporary cross-region transfer from the rest of the monetary union to the residents of the affected region, and so one can apply my estimates of a temporary cross-region transfer multiplier of around $\mathcal{M}_{Tr} = 1/3$. As such, the automatic stabilizers from a

[43] See Pennings (2020) for a comparison between federally financed multipliers and locally financed multipliers.

[44] Automatic stabilizers can also change marginal tax rates, the effects of which are not considered here (see Auerbach and Feenberg 2000).

federal tax-transfer system would only stabilize around 10 percent $(= 0.3 \times 1/3)$ of any fall in output in a short-lived regional recession. A more complete calculation adjusts for (i) the effect of smoothing on the size of the cross-region transfer generated and (ii) asymmetric regional recessions being more persistent than a one-off transfer, which raises the relevant multiplier to around $\mathcal{M}_{Tr} = 0.42$ (calculated using the New Keynesian model with $\rho = 0.935$). Combined, these only increase the fraction smoothed to 11 percent. Simulated recessions in the New Keynesian model produce similar results (see online Appendix Section 5.2). The fraction smoothed ranges from 6 percent to 18 percent with alternative assumptions.[45]

My cross-region transfer multiplier estimates are also related to the size of the aggregate closed-economy transfer multiplier, albeit indirectly. Aggregate and cross-region multipliers are very different in general, as monetary policy and tax responses are differenced out in the cross-section but are relevant for the aggregate multiplier (and there are no demand leakages in a closed economy). Nonetheless, my empirical estimates can be used as an "identified moment" (Nakamura and Steinsson 2018) to distinguish between models, with the favored model being used to calculate aggregate closed-economy transfer multipliers. This is the approach taken by Nakamura and Steinsson (2014) but for purchases rather than transfers.

As mentioned in Section IVA, my empirical estimates can be rationalized by a New Keynesian model with sticky wages and a share of hand-to-mouth households. In that New Keynesian model, the aggregate present-value *transfer* multiplier $\mathcal{M}_{Tr}^{AggPV}$ at business cycle frequencies 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 $\mathcal{M}_{Tr}^{AggPV} = \omega_T \mathcal{M}_{G}^{AggPV}$.[46] Consistent with the findings of Nakamura and Steinsson (2014), this type of model produces aggregate *purchase* multipliers $\mathcal{M}_{G}^{AggPV}$ at business cycle frequencies that are small (<1) when monetary policy "leans against the wind" but are large (≥1) when monetary policy is more accommodating of inflation (using a constant real interest rate here for simplicity).[47] My empirical estimates of one-off cross-region transfer multipliers are consistent with a modest share of hand-to-mouth households $\omega = 1/3$, which also pins down the scaling factor for untargeted transfer multipliers (where $\omega_T = \omega$). Hence my New Keynesian model produces large aggregate transfer multipliers $(\mathcal{M}_{Tr}^{AggPV} \geq 1)$ at business cycle frequencies if both monetary policy is accommodating *and* transfers are well targeted at hand-to-mouth households $(\omega_T \geq 2\omega)$, modest multipliers $\mathcal{M}_{Tr}^{AggPV} \approx 1/2$ for

[45] The smoothing fraction is $\mathcal{S} = NTC \times \mathcal{M}_{Tr}/(1 + NTC \times \mathcal{M}_{Tr})$, with $\mathcal{M}_{Tr} = 0.42$ and $NTC = 0.3$. Auerbach and Feenberg (2000) estimate a lower NTC for the United States as a whole. With the same transfer multiplier, but Auerbach and Feenberg's $NTC = 0.25$ (applied to adjusted gross income), the smoothing fraction falls to 10 percent, and with their $NTC = 0.15$ (applied to GDP), the smoothing fraction falls to 6 percent. On the other hand, with a hand-to-mouth share of 50 percent and home bias of 75 percent, the New Keynesian model produces cross-region transfer multipliers of 0.61 for temporary transfers and 1.54 for permanent transfers. At asymmetric business cycle frequencies, this implies $\mathcal{M}_{Tr} = 0.73$, which produces a smoothing fraction of 18 percent (with $NTC = 0.3$).

[46] 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 simplifying assumptions on the sticky wage setting process abstract from the second channel, leaving only the effects through demand.

[47] Multipliers can be substantially larger when monetary policy is more accommodating, for example, when the zero lower bound on nominal interest rates binds for an extended period (see Giambattista and Pennings 2017).

untargeted transfers with constant real interest rates, and small transfer multipliers $\mathcal{M}_{Tr}^{AggPV} < 1/2$ otherwise (see online Appendix Section 6 for details).[48]

VI. Conclusion

In this paper, I investigate the size of the cross-region transfer multiplier in the United States. This is novel, as the cross-region transfer multiplier is conceptually different from the MPC and cross-region purchase multipliers estimated in the literature, and is relevant, as transfers are countercyclical and 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 1/3 for one-off transfers as part of stimulus packages (\$0.2–\$0.9, depending on the specification) and around 1.5 for permanent Social Security transfers (\$0.9–\$1.9, depending on the specification). The size of the multipliers can be roughly rationalized by a standard New Keynesian model with sticky wages and a fraction of hand-to-mouth households.

My estimates of modest temporary cross-region transfer multipliers, in turn, imply that the automatic stabilizers built into the US federal tax-transfer system only have a limited ability to smooth output in standard regional recessions, perhaps less than in the popular perception. However, this is not to say that other federal policies, such as discretionary federal purchases in regions in recessions, or intergovernmental transfers, might not be more effective (see Chodorow-Reich 2019 for a survey). Moreover, transfers might help to smooth regional consumption, which would enhance welfare even if output smoothing were modest.

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