Decomposing the Government Transfer Multiplier*

Timothy G. Conley,† Bill Dupor,‡ Rong Li,§ and Yijiang Zhou¶

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Abstract

We estimate the local, spillover and aggregate causal effects of government transfers on personal income. We identify exogenous changes in federal transfers to residents at the state-level using legislated social security cost-of-living adjustments between 1952 and 1974. Each effect is measured as a multiplier: the change in personal income in response to a one unit change in transfers. The local multiplier, i.e., the effect of own-state transfers on own-state income holding fixed other state's income, at a four-quarter horizon is approximately 3.4. The cross-state spillover multiplier is about -0.7, but not statistically different from zero. The aggregate multiplier, i.e., the sum of its local and spillover components, equals 2.7. More generally, our paper provides a template for conducting inference that decomposes an aggregate effect into its local and spillover components.

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†Western University, tconley3@uwo.ca.

‡ Federal Reserve Bank of St. Louis, william.d.dupor@stls.frb.org.

§ School of Finance, Renmin University of China, lirong.sf@ruc.edu.cn.

¶ The Chinese University of Hong Kong, yijiangzhou@link.cuhk.edu.hk.

1 Introduction

Measured by expenditure dollars, transfers to individuals have become the largest component of federal US economic policy. Federal social benefit payments to person constituted 47 percent of all federal expenditures in 2022, up from 22 percent in 1950. By comparison, federal defense spending as a percentage of expenditures fell from 48 percent to 15 percent over the same period. There is great interest in the effects of transfers and quantifying them is a key objective of policy studies by, e.g., the US Congressional Budget Office, the US Council of Economic Advisers, and the International Monetary Fund.[1]

This paper attempts to answer two questions. First, do transfer payments have a greater than one-for-one "multiplier" effect on income? Second, do transfers in one state lead to spillover effects across states?

This paper uses state-level panel data on federal transfer payments and personal income to address these questions. Personal income equals funds from wages and salaries, government benefits, dividends, interest and other sources net of taxes paid for government social insurance.[2] We estimate the local (within region), spillover (across region) and aggregate (total national) multipliers of transfers on income.

We focus on three multipliers to measure effects. Each multiplier is the change in personal income in response to a one unit change in transfers. First, the local multiplier (the effect of own-state transfers on own-state income holding fixed average transfers across other states) at a four-quarter horizon is approximately 3.4. The cross-state spillover multiplier (the effect of other-state transfers on own-state income holding fixed own-state transfers) is about -0.7 but is not statistically different from zero. The aggregate multiplier, i.e. the sum of its local and spillover components, equals 2.7. In our empirical set-up, local and spillover multipliers are state-specific because they depend on states' income-measured sizes. The values reported above correspond to local and spillover multipliers for an average-sized state.

Our analysis addresses and overcomes three econometric complications: the endogeneity of aggregate transfers over time, the endogenity of how a given share of national transfers might be allocated across states at a point in time, and potential cross-state spillovers in the treatment effect of transfers.

First, at the aggregate level, potential endogeneity of transfers could bias a least-

[1]See for example Congressional Budget Office (2014), Council of Economic Advisers (2010) and International Monetary Fund (2014).

[2]We use the terms income and personal income interchangeably in this paper.

squares estimate of the income effect of transfers either positively or negatively. For example, government revenue surpluses, during times of strong economic growth, might lead legislators to increase transfers, causing an upward bias. Alternatively, weak economic growth might lead legislators to increase transfers as a countercyclical measure, causing a downward bias.

Second, even with aggregate-level exogeneity, endogeneity could arise in the cross-section if the federal government reallocated transfers across states in response to regional business cycle differentials. Suppose the federal government apportioned a greater share of an exogenous aggregate transfer increase to a relatively low income growth state. In that case, a cross-sectional least squares regression estimate of the effect of transfers would be downward biased.

Third, spillovers complicate interpretation of cross-sectional or panel regression results. In the presence of spillovers, if region A receives a treatment, region B may be affected by that treatment even if B receives no treatment on its own. A cross-region spillover constitutes a classic violation of the Stable Unit Treatment Value Assumption (SUTVA) which requires that potential outcomes be unaffected by the treatment status of other observational units. The local effect of a treatment need not equal the treatment's aggregate effect in the presence of spillovers. For example, if there are positive spillovers across regions, then a positive local effect will understate the aggregate effect of the treatment.

To address these endogeneity concerns, we construct exogenous state-level changes in transfers, i.e. adjustments, using data constructed by Pennings (2021), which builds upon data from Romer and Romer (RR, 2016). RR (2016) observe that between 1952 and 1975, social security cost-of-living adjustments (COLAs) were enacted through occasional federal legislation, instead of automatic indexation to inflation. They then estimate the consumption response to transfers by regressing aggregate consumption on a series constructed from these adjustments. We follow RR (2016) and Pennings (2021) and argue the adjustments' ad hoc timing due to political considerations is a source of exogenous variation. However, these adjustments also depend on the path of inflation. As the price level rises, the pressure to enact COLAs increases. COLAs became larger and more frequent as inflation picked up starting in the mid-1960s. Awareness of inflation and political pressures for COLA legislation likely increased during this period. To isolate the exogenous component due to ad hoc political considerations, we control for several lags of the inflation rate when utilizing the transfer adjustment series.

We follow Pennings (2021) in constructing a state-level adjustment series by partitioning every quarter's national adjustment to each state according to its lagged share of social security payments. Since slow moving demographics largely determine each state's share, we argue these lagged shares are reasonably treated as exogenous.

Our paper extends and advances the Pennings (2021) and RR (2016) analyses by connecting the local and aggregate effects of regional treatments in a unified estimation framework. RR (2016) find that aggregate consumption increases over the first-year following a positive aggregate transfer adjustment. While their analysis speaks to aggregate effects, it does not answer our second question–whether there are regional spillovers. Pennings (2021) estimates a two-way fixed effects model of own-state gross labor earnings on own-state transfers and refers to the coefficient on own-state transfers an open economy relative multiplier.[3] Such relative multipliers are of course related to our approach but they do not allow estimation of spillover or aggregate effects.

The following example illustrates the importance of accounting for spillovers. First suppose we partition the U.S. into two regions: North and South. Suppose the North's income depends directly on government transfers to the North and indirectly on government transfers to the South, and *vice versa* for the South's income. Suppose the government increases transfers to the North. Since national income is the sum of North output and South output, the national income effect of transfers to the North is the sum of the direct effect on the North's income plus the indirect (i.e., spillover) effect on the South's income. This is simply the sum rule of derivatives, i.e., the derivative of a sum equals the sum of its derivatives. The same logic applies to transfers made to the South.

Recall that the local multiplier measures the effect on own-state income of a one unit increase in own-state transfers holding fixed a weighted average of other states' transfers. The spillover multiplier measures the effect on own-state income of a one-unit increase in the weighted average of other states transfers holding fixed own state transfers. It follows that the aggregate effect of a uniform increase in transfers in both regions can be decomposed into the two direct effect and two indirect effects. Although our observation is simple, almost none of the existing research on spillovers connect the local and aggregate effects in a unified estimation framework.[4]

[3]Nakamura and Steinsson (2014) first introduce the "open economy relative multiplier" terminology. Other papers using this concept include Basso and Rachedi (2021), Berge, et.al. (2021) and Lu and Zhu (2021).

[4]The existing pertinent research typically defines a spillover treatment as the treatment of a nearby region or averaged over several nearby regions using some measure of closeness (e.g., trade flows and geographic proximity). Examples include Auerbach, Gorodnichenko, and Murphy (2019) and Alloza, et.al.

By comparison, one common approach in analyzing panel data involves using time fixed effects. In the presence of time fixed effects, macroeconomic causal effects are not identified because the relevant design matrix is not full rank. Despite the challenge including time fixed effects presents for identifying aggregate effects, their use has become commonplace in papers studying macro questions. See for example Auerbach and Gorodnichenko (2016), Auerbach, Gorodnichenko, and Murphy (2019), Bessho (2021), Clemens and Miran (2012), Cohen, Coval and Malloy (2011), Guren, et.al. (2020), Kraay (2012), Kraay (2014), Sheremirov and Spirovska (2022), Shoag (2013) and van Gemert, Lieb and Treibich (2022).

Our paper contributes to three literatures: (i) empirical macroeconomics using cross-sectional and panel data, (ii) estimating cross-region spillovers, and (iii) estimating the effects of government transfers on economic activity.

First, there is a burgeoning literature on using regional data to answer questions in macro. Chodorow-Reich (2020) cites 50 papers published between 2012 and 2018 in top economic journals that attempt to estimate the macro effects of shocks using cross-regional variation in treatments and regional outcomes. While instructive for understanding local or relative effects, several authors have described the potential disconnect between local and aggregate causal impacts when researchers use cross-sectional methods to do macroeconomic analyses. Cochrane (2012) writes "Showing that the government can move output around does not show that it can increase output overall."[5]

We can see this potential disconnect by returning to our North-South example. Assume North output is affected by a treatment to South, even if North receives no treatment on its own. This could result from regional trade in goods or movements in factors of production. This leads to a disconnect between local and aggregate effects. With positive spillovers across regions, an estimated positive local effect will understate the aggregate effect of the treatment. Macro papers using cross-sectional data typically make no or only passing reference to the complication that spillovers introduce.

Several authors address this local versus aggregate disconnect by bringing economic theory to the table. They first estimate local multipliers in cross-sectional or panel regressions using disaggregate data, and then, apply a quantitative economic model—–with assumptions on preferences, technology, etc.—to infer an aggregate multiplier (e.g., Dupor, et.al. (2022) and Nakamura and Steinsson (2014)). Our method improves on that ap-

(2019). The sole exception is Conley, et.al. (2022), which decomposes the aggregate effect of changes in defense spending into a local and spillover component, using the same technique as in our paper.

[5]See also Ramey (2011).

proach along the following dimension: We recover the aggregate, local as well as spillover multipliers, in a way that is robust to any assumptions on an economic model, in a unified causal inference framework.

One challenge in developing our decomposition is the requirement that our region-level regression *aggregates* to an equation that is compatible with macroeconometric analysis. From a macroeconomist's perspective, it is natural to think about an aggregate treatment and an aggregate outcome. From the perspective of a disaggregate analysis, in general knowing the aggregate treatment may not be sufficient to infer an aggregate causal impact. Returning to our North-South example, suppose first for simplicity that there is no spillover across the two regions. Next suppose a treatment in the North has a large effect on the North outcome but a treatment in the South has no effect on the South outcome. Then, the sum of treatments is *not* a sufficient statistic to calculate the nation-wide effect on the outcome. Empirical macro researchers typically address this issue by assuming the sum of the regional treatment is sufficient to infer aggregate causal effect.

In line with the existing research, we specify a regional regression in which a region's income depends on transfers only through its own-region transfers and the average of all regions' transfers. This assumption will help ensure that our regional regression equation can be summed up along the cross-sectional dimension to an aggregate estimation equation. Thus, our model features the capacity to study the effect on disaggregate and aggregate income from both disaggregate and aggregate treatments.

Second, our paper extends the literature on estimating spillovers. Several papers using cross-sectional or panel regressions estimate both local and spillover effects, in which case each region's spillover treatment is measured as a weighted sum of the treatments in other regions. Examples of weighting schemes include those depending on trade flows (e.g., Auerbach and Gorodnichenko (2016)) or geographic proximity (e.g., McCrory (2020)). None of these papers looks at the aggregate implications of their disaggregate regression.

Third, our paper contributes to the literature on the effects of government transfers. Our paper studies permanent changes in transfers. With regard to permanent changes, besides RR (2016), Gechert, et.al. (2021) and Parraga Rodriguez (2023) construct narrative data sets of social security shocks for Germany and Spain and find large GDP or consumption responses. Studies focusing on temporary transfers use mainly surveys or economic theory. Direct surveys of transfer recipients (e.g., Coibon, Gorodnichenko and Webber (2020) and Sahm, Shapiro and Slemrod (2012)) indicate how much of a transfer payment is spent or saved in the "first round"; however, understanding these initial first round effects does not answer either of our above questions. Surveys do not address potential general equilibrium effects which would inform the answers to our two questions. Studies investigating temporary transfer increases, usually as part of a stimulus package, typically find a large MPC from 0.25 to 0.9. Existing theoretical work on the effect of transfers and applications of these theories through calibration (e.g., McKay and Reis (2016), Kaplan and Violante (2014) and Oh and Reis (2012)) provide answers to the first question, but only through the lenses of the models these researchers employ.

2 Data

Let $Y_{i,t}$ denote seasonally adjusted personal income in state i during quarter t, constructed by the Bureau of Economic Analysis. Let $\Delta tr_{i,t}$ denote the one-quarter social security legislated change in transfers to state i, which we refer to as the transfer adjustment at time t. Both $Y_{i,t}$ and $\Delta tr_{i,t}$ are measured in real, per capita units. We restrict our sample to be 46 states, excluding Alaska, Hawaii, and the Dakotas.[6] One could partially aggregate the data into broader geographies (e.g., census regions or divisions). Aggregation would likely bias us against finding spillover. An underlying cross-state spillover would be subsumed by partial aggregation: The state-to-state spillover would become part of the "own effect" of transfers for the more coarse geographic region.

RR (2016) construct the national transfer adjustment using Congress-mandated cost-of-living adjustments which occurred intermediately during the years considered.[7] To map from national to state-level transfer adjustments, we follow Pennings (2021) and allocate the national series to the state level according to each state's one-year lag of the state's share of total social security payments.

We use annual state-level population estimates (linearly interpolated to be quarterly) from the US Census Bureau to construct per capita variables. We use the quarterly average of the monthly Personal Consumption Expenditure (PCE) price index to map transfer adjustments and personal income from nominal into real variables. We additionally use lags of the change in the log of this price index as conditioning variables in our regressions.

[6]These four states are relatively small and unusual in terms of their economies' connection to the rest of the country (Alaska and Hawaii) or are outliers in terms of income volatility (Dakotas).

[7]The RR (2016) series extends beyond 1975; however, we exclude this period because at that time the federal government was indexing COLAs to inflation after 1975.

3 The Local-Spillover Decomposition

Our outcome variable is the cumulative change in state i income over an h quarter horizon relative to $t-1$, scaled by own-state income at $t-1$:

$$y^h_{i,t+h} = \sum_{k=0}^{h} \frac{Y_{i,t+k} - Y_{i,t-1}}{Y_{i,t-1}} \tag{1}$$

It follows the horizon zero ($h = 0$) variable $y^0_{i,t}$ corresponds to the one quarter growth rate in income.

We construct a parsimonious model where state income depends upon the own-state transfer adjustment and the weighted average transfer adjustment of all states. The mapping of coefficients for each variable into the local and spillover multipliers is described below.

Each state's own treatment is the cumulative transfer adjustment over an h quarter horizon relative to $t-1$, scaled by own-state income at $t-1$

$$x^h_{i,t+h} = \sum_{k=0}^{h} \frac{\Delta tr_{i,t+k}}{Y_{i,t-1}} \tag{2}$$

The aggregate treatment is

$$x^h_{t+h} = \sum_{j=1}^{N} s^Y_j x^h_{j,t+h} \tag{3}$$

where s^Y_j is the share of personal income in state j. The use of share-weights in calculating the cross-sectional sums in equation (3) arises because $x^h_{j,t+h}$ is scaled by own-state (rather than national) lagged income. This construction implicitly assumes income shares are approximately stable over time so we measure s^Y_j as the share of personal income in state j in the first quarter of our sample.

We scale by lagged income in both the outcome and the treatment in order that the regression below delivers estimated coefficients that can be interpreted as multipliers (i.e., either local, spillover or aggregate): the dollar quantity change in income in response to a one dollar increase in transfers.

Our spillover definition takes an expansive form, including all other states. It is related more closely to common monetary and fiscal policy, rather than trade linkages or geographic closeness that might suggests using state-specific weights to construct the spillover variable. For example, RR (2016) present evidence that national monetary policy responded countercyclically to cost-of-living adjustments during the sample. Any contractionary effect of tighter monetary policy would manifest itself nationally, which is consistent with an expansive spillover channel.

We measure the effects of transfers using a cumulative multiplier, which is defined as the cumulative change in income in response to a cumulative change in transfers over a horizon of h quarters. This is because both local and spillover effects on income may occur with delay. Also, economic activity stimulated in the short run, e.g., durable goods purchases, might be offset if demand is pulled forward from the future (see for example Mian and Sufi (2012)).

Our local spillover decomposition is implemented using the following regression:

$$y^h_{i,t+h} = \alpha_{i,h} + \gamma_h x^h_{i,t+h} + \phi_h x^h_{t+h} + \Lambda'_h H_{i,t-1} + e^h_{i,t+h} \tag{4}$$

Equation (4) states that the scaled-cumulative change in state income depends upon the scaled-cumulative change in own-state transfers and the scaled-cumulative change in the weighted average of all states' transfers. The regression also includes a state fixed effect and a set of lagged conditioning variables. Our conditioning vector $H_{i,t-1}$ contains four lags each of quarterly PCE inflation rate, $y^0_{i,t}$, $y^0_t \equiv \sum_i s_i y^0_{i,t}$, $x^0_{i,t}$, and x^0_t.

By having own-state income depend on transfers only through own-state transfers and economy-wide average transfers, we achieve a parsimonious model. Moreover, as shown below, this state-level estimation equation "aggregates" to an equation that macro researchers typically employ. That is, the aggregate outcome depends only on the aggregate treatment rather than the entire vector of regional treatments.

In equation (4), for any i, the own-state transfer adjustment appears twice: once multiplying γ_h and once multiplying ϕ_h as part of the sum of scaled cumulative transfer changes across states. Bearing this in mind, we compute the horizon h local multiplier of state i as $\gamma_h + s^Y_i \phi_h$ and the spillover multiplier is $\left(1 - s^Y_i\right) \phi_h$. Note that the magnitude of each multiplier depends on the size of the state. In the empirical results below, we report the "average local multiplier" as $\gamma_h + \phi_h / N$. Similarly, we report the "average spillover multiplier" as $(N-1)\,\phi_h / N$. If N is large, the average local and average spillover multipliers approximately equal γ_h and ϕ_h, respectively.

Our specification's aggregate multiplier can be expressed using local and spillover components. Take the s^Y_i weighted sum of both sides over i applied to equation (4) which results in

$$y^h_{t+h} = \alpha_h + (\gamma_h + \phi_h)\, x^h_{t+h} + \Gamma'_h H_{t-1} + e_t \tag{5}$$

where $y^h_{t+h} \equiv \sum_{j=1}^N s^Y_j y^h_{j,t+h}$. H_t contains the share-weighted averages of their state-level analogues. We call $\gamma_h + \phi_h$ the aggregate multiplier. It represents the response of aggregate income to a one unit increase in aggregate transfers, in which both are measured as scaled cumulative changes in the respective variables. Note that our weighted average treatment x^h_{t+h} is approximately equal to the aggregate transfer change, provided that each state's share of income is sufficiently stable over time.

Direct estimation of (5) provides $(\gamma_h + \phi_h)$ as an alternate estimate of our aggregate multiplier. We compare this to the typical macroeconometric approach which estimates the analog of (5) using aggregate growth rates in place of our share-weighted averages of state growth rates, y^h_{t+h} and x^h_{t+h}. This comparison allows us to investigate the quality of the approximation underlying our interpretation of $(\gamma_h + \phi_h)$ as an aggregate multiplier, that aggregate treatment and outcomes are sufficiently close to the share-weighted sums of their state-level analogues.[8] Previewing results presented below, estimates of (5) and its analog with true aggregates are very close.

We are concerned with potential endogeneity of the RR (2016) transfer adjustment series because these adjustments are plausibly related to inflation. Panel (a) of Figure 1 contains the national RR (2016) transfer adjustment series (dashed line), expressed as a percent of national personal income, and the annualized quarterly PCE inflation rate (solid line). Transfer adjustments vary in size and are spread intermittently over the sample. RR (2016) use this series as a source of random variation to identify the effect of transfers on consumption. However, from the figure we observe that, as inflation picks up starting in the mid 1960s, transfer adjustments become larger and more frequent. This indicates a potential endogeneity problem.

Intuitively, higher inflation likely raised awareness of the declining value of the dollar and increased political pressure to enact cost of living adjustments. Political pressures for adjustments may have also been stronger when inflation occurred alongside slowing economic activity during the stagflation over part of the sample. It is thus inappropriate

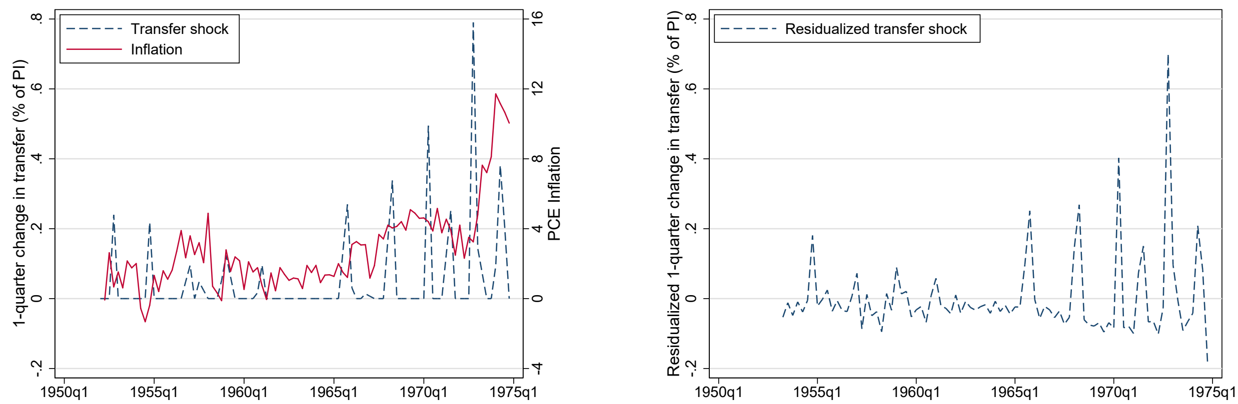
[8]The output change approximation is

$$y^h_{t+h} \approx \sum_{k=0}^{h} \left(\sum_{j=1}^{N} \left(Y_{j,t+k} - Y_{j,t-1} \right) / \sum_{j=1}^{N} Y_{j,t-1} \right)$$

and the transfer adjustment approximation is defined analogously.

Figure 1: Inflation and national RR (2016) transfer adjustments, 1952Q1-1974Q4

(a) Original Romer-Romer series (b) Residualized Romer-Romer series



Note: PI=personal income; PCE=personal consumption expenditure. PCE inflation is quarterly and annualized.

to treat the RR (2016) series—unadjusted—as exogenous in the regressions we consider.[9]

The aggregate transfer change series is positively correlated with past inflation, and past inflation itself is negatively correlated with income growth. Thus the estimated coefficient on x^h_{t+h} in equation (4) could be biased downward if past inflation is omitted.

Our response to this concern is to control for four lags of quarterly inflation in each of our specifications. We contend that the variation in the RR (2016) series that is orthogonal to this conditioning information is plausibly exogenous. The dashed line on panel (b) of Figure 1 plots a residual series, i.e, an adjusted RR (2016) series, from a regression of the original RR (2016) series on four lags of inflation.

For comparison with Pennings (2021), we also estimate a modified version of (4) in which we drop the weighted average of transfer adjustments and add a time fixed effect. We call this the time fixed effect regression, given by:

$$y^h_{i,t+h} = \theta_{i,h} + \mu_{h,t} + \psi_h x^h_{i,t+h} + \Phi'_h H_{i,t-1} + \epsilon^h_{i,t+h} \tag{6}$$

Pennings (2021) estimates an equation similar to (6) and refers to his analogue of ψ_h as an open economy relative multiplier. Unlike our local spillover decomposition, one cannot recover the aggregate multiplier from this specification. In this specification, location-

[9] The correlation between one quarter lagged inflation and the RR (2016) series is 0.26.

invariant inflation controls are omitted because they are absorbed by the time fixed effect.

Why would one include time fixed effects in the first place? Pennings (2021) motivates inclusion of time fixed effects to limit potential omitted variable bias. Time fixed effects remove aggregate variation from, for example, monetary policy shocks and international shocks. We observe that, while these shocks might affect state and national business cycles, they are plausibly uncorrelated with the state-level change in transfers (after controlling for inflation) as well as the aggregate change in transfers. This is—at its core—the identification assumption used in RR (2016).[10]

We estimate the model using data from 1952Q1 to 1974Q4. In benchmark specifications, we weight regressions with the weights given by s_i^Y. For panel-based regressions, we report Driscoll-Kraay standard errors using a Bartlett window with four leads and lags. For the aggregate based estimates, we use a Bartlett window with four leads and lags.

4 Income Multipliers

Table 1 presents the local-spillover decomposition of the impact multiplier ($h = 0$). Column (1) reports the estimates from equation (4) and constitutes our benchmark specification. It uses the panel of 46 states and weights the regression by state (initial) income share s_i^Y. Using weights in our panel regressions reflects the size-dependency inherent in aggregate analysis. In the row labeled Local, we report estimates of an average local multiplier,$\gamma_h + \phi_h/N$, with a point estimate of 2.61 (SE=0.97). This corresponds to a unit increase in own-state transfers—holding fixed the average of other state transfers—raising income by 2.61 units. Thus the point estimate implies that there is a greater than one-for-one local effect of transfers on income, but the effect is not statistically different from one at a 5% level. In the row labeled spillover, we report estimates of the average spillover multiplier, $(N-1)\phi_h/N$. Our point estimate is -0.34 (SE=1.04). Thus, there is no statistically significant cross-state effect of transfers. In the row labeled Aggregate, we report our estimates of the aggregate multiplier (which equals the sum of the spillover and local multipliers). Our point estimate is 2.27 (SE=0.52). This is close to the point estimate of the local multiplier, because the spillover multiplier is estimated to be close to zero. Importantly, our estimate of the aggregate multiplier is much more precise than either the local or spillover multipliers individually. Our aggregate estimates provide evidence that

[10]Recall that we additionally condition on lagged inflation rates, relative to RR (2016).

the aggregate multiplier is greater than one, with a 90% confidence interval of [1.40,3.14].

In the last row of column (1), we report our estimate of γ for comparison with two-way fixed effects (TWFE) estimates, reported in column (2) which presents estimates of equation (6) with a state level treatment coefficient analogous to γ. Intuitively, the coefficient on the state-level treatment in our benchmark specification is identified by variation that is orthogonal to the (weighted) aggregate treatment and other regressors. This variation is also orthogonal to time period indicator variables so the slope estimate is unchanged by the addition of (weighted) time fixed effects (and dropping the average treatment).

Table 1: The fiscal multiplier on personal income from an increase in government transfers, one-quarter impact multiplier

	(1)	(2)	(3)	(4)	(5)	(6)
Local	2.605***				2.320***	
	(0.973)				(0.888)	
Spillover	-0.336				-0.081	
	(1.037)				(1.149)	
Aggregate	2.269***		2.269***	2.285***	2.239***	
	(0.528)		(0.528)	(0.517)	(0.647)	
γ/TWFE	2.613***	2.613***			2.322**	2.322**
	(0.993)	(0.993)			(0.909)	(0.909)
Weights	✓	✓				
State FE	✓	✓			✓	✓
Time FE		✓				✓
Num of periods	92	92	92	92	92	92
Num of divisions	46	46	1	1	46	46

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses.

Dependent variable is one-quarter growth in per capita personal income. Column 3 uses approximate RHS and LHS variables and estimates aggregate model. Column 4 uses exact RHS and LHS variables and estimates aggregate model. All estimates include full sets of (appropriately-scaled) controls. Column 3 and 4 report Bartlett SE and remainder report Driscoll Kraay SE, all of which use a Bartlett window with four leads and lags.

Column (3) estimates the analog of equation (5) using the aggregate data for comparison. Because we are summing along the cross sectional dimension, we have only one national division. Thus, we rely on time series variation for identification. The aggregate multiplier is estimated to be 2.27 (SE=0.53). It is identical to the aggregate multiplier implied by the local-spillover decomposition in columns (1).

As explained above, equation (5) uses share-weighted state-level changes. Therefore, these changes are not precisely the same as those changes applied to data that are first cross-sectionally aggregated. Column (4) contains estimates in which we instead directly use the corresponding aggregate variables. The aggregate multiplier equals 2.29 (SE=0.52), close to that from column (3). This confirms the quality—for our purposes—of the weighted-sum approximation.

To investigate the impact of weighting, Columns (5) and (6) report the analogues of columns (1) and (2) with uniform weighting for either the construction of aggregates or the estimation procedure. In other words, we replace income share s_j^Y in equation (3) by $1/N$ and estimate via OLS. Overall, our estimates are smaller in magnitude. Our result of no evidence of spillovers is robust to weighting. However, our evidence of the aggregate multiplier being greater than one is a bit weaker in column (5) with a 90% confidence interval of [1.17,3.40].

Table 2 provides the four-quarter (i.e., $h = 3$) cumulative local, spillover and aggregate transfer multipliers on income. Tables 1 and 2 share the same format. In column (1), relative to the one-quarter impact we find a substantially larger local multiplier estimate 3.36 (SE=0.98) with approximately the same precision, providing evidence of a local multiplier greater than one. However, we still find no evidence of a spillover multiplier with our point estimate -0.66 (SE=1.23). Our one-year aggregate multiplier is also a bit larger than our one-quarter results, estimated as 2.70 (SE=0.80). Thus, the estimated local and aggregate cumulative multipliers are somewhat larger than the corresponding one-quarter multipliers. Our approximation of changes in aggregates with share-weighted sums of state changes still appears good contrasting columns (3) and (4). Finally, the last two columns investigate the impact of weighting; we do find that our uniformly weighted local multiplier is substantially lower in column (5) versus our preferred share-weighted estimates in column (1).

Next we graphically examine our data for both panel and aggregate time series. Panel (a) of Figure 2 contains a scatter plot using our state-level panel. For both the income change and transfer adjustment, we residualize with respect to the controls described above as well as the average transfer change. Given the panel's large size, we plot the data after first clustering into 92 bins.[11] We also plot the weighted-least-squares best fit

[11] Panel (a) is called a binscatter plot. It is generated by dividing the x-axis into equal-sized bins, computing the mean of both the x and y variables within each bin and constructing a scatter plot from these group means. The number 92 comes from the number of time periods in our sample, so that panel (a) is visually

line, which has slope equal to 2.61 (SE=0.97). This corresponds to our local multiplier point estimate in column (1) estimate from Table 1.

Table 2: The fiscal multiplier on personal income from an increase in government transfers, one-year cumulative multiplier

	(1)	(2)	(3)	(4)	(5)	(6)
Local	3.355***				2.576**	
	(0.982)				(1.076)	
Spillover	-0.655				0.033	
	(1.225)				(1.413)	
Aggregate	2.700***		2.700***	2.643***	2.609***	
	(0.803)		(0.803)	(0.811)	(0.852)	
γ/TWFE	3.369***	3.369***			2.576**	2.576**
	(1.002)	(1.002)			(1.101)	(1.101)
Weights	✓	✓				
State FE	✓	✓			✓	✓
Time FE		✓				✓
Num of periods	92	92	92	92	92	92
Num of divisions	46	46	1	1	46	46

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses.

Dependent variable is one-year cumulative growth in per capita personal income. Column 3 uses approximate RHS and LHS variables and estimates aggregate model. Column 4 uses exact RHS and LHS variables and estimates aggregate model. All estimates include full sets of (appropriately-scaled) controls. Column 3 and 4 report Bartlett SE and remainder report Driscoll Kraay SE, all of which use a Bartlett window with four leads and lags.

In the absence of spillovers the local multiplier in panel (a) is equal to the aggregate multiplier, depicted in panel (b) of Figure 2. Panel (b) contains a scatter plot of the transfer adjustment (horizontal axis) and the change in income (vertical axis), in which both are measured as one quarter changes at the national level and residualized with respect to the benchmark controls. Each observation is marked by its year-quarter. The figure also contains the line of best fit, with slope equal to 2.29 (SE = 0.52), corresponding to the specification in column (4) of Table 1. The similarity of the slopes in panels (a) and (b) reflects our estimates of spillovers being near zero.

comparable to panel (b).

Figure 2 allows an investigation of the influence of outliers upon our results. Most notably, in panel (b) there are clearly two unusually large increases in aggregate transfers. The points marked 1970Q2 and 1972Q4 in the northeast direction represent the two largest increases in transfer during our sample period. Our benchmark (full sample) slope estimate is 2.29 (SE=0.52). The slope of the best-fit line after removing 1970Q2, 1972Q4 and both equals to 2.36 (SE=0.61), 1.56 (SE=0.66) and 1.11 (SE=0.96), respectively. This clearly demonstrates the importance of these two quarters for the precision of our estimates. Furthermore, it makes clear that our (full sample) finding of an aggregate multiplier greater than one with aggregate series is largely driven by data from 1972Q4.

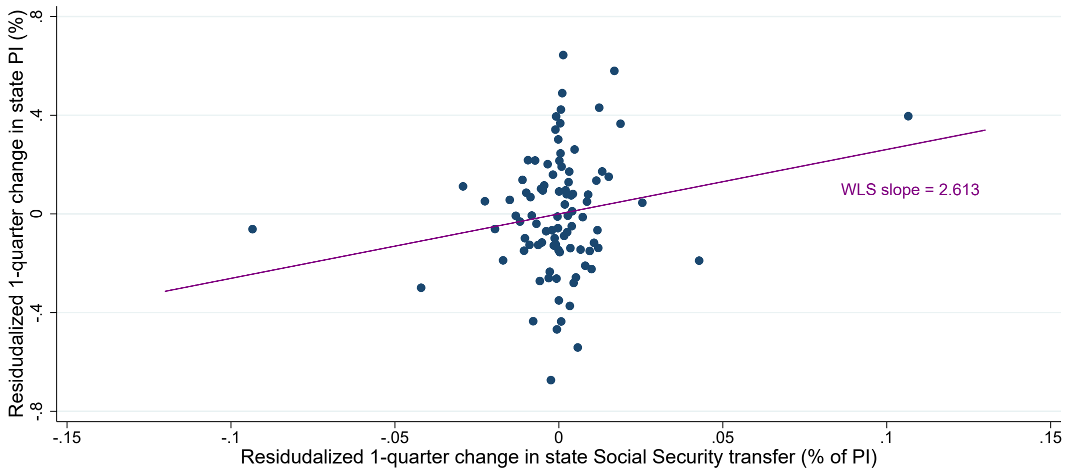
The situation with aggregate time series is similar but a bit less severe at a horizon of four quarters, presented in Figure B1 in the Appendix. At a four quarter horizon there are still two unusually large treatment values: 1972Q3 and 1972Q4. These quarters are very influential in our aggregate results. When dropping 1972Q3, 1972Q4, and both our aggregate multipliers change from the full sample 2.64 (SE=0.81) to 2.86 (SE=1.36), 3.32 (SE=1.07) and 3.09 (SE=1.28) respectively. These two influential quarters again impact our estimation precision considerably. Our estimated four quarter aggregate multipliers remain statistically different from zero without these two quarters, unlike the one quarter horizon estimates absent 1972Q4 and 1970Q2. However, it is clear our full sample finding of an aggregate four quarter multiplier greater than one is largely due to data from 1972Q3.

Appendix Tables B1 and B2 collect results from examining the sensitivity our panel estimates to the same unusually large treatment time periods as above. For our one quarter multipliers in the Table B1, benchmark estimates are compared to those dropping 1970Q2, 1972Q4 and both. Likewise in Table B2 benchmark four-quarter multiplier estimates are compared to those dropping 1972Q3, 1972Q4, and both.

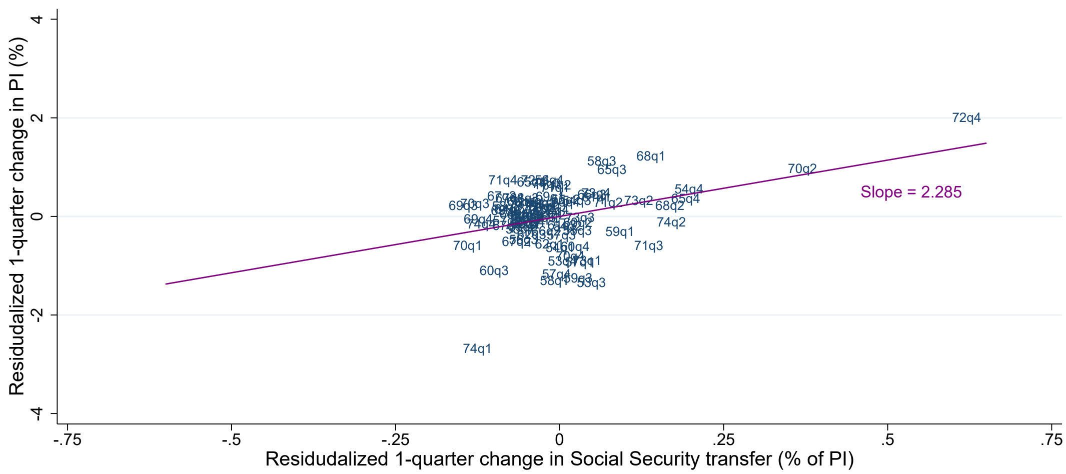
The sensitivity of our panel results mirror those with aggregate data: There is substantial influence of these time periods for our results. Our local multipliers are of course identified from cross sectional variation but they are also substantially influenced by data from these unusual quarters, though the impact varies with horizon. One-quarter local multipliers lose precision when we drop unusual time periods and are not significantly different from zero when 1972Q4 is dropped. In contrast, while local multipliers at a four-quarter horizon also lose precision when the unusual time periods are dropped, they maintain statistical significance at the 5% level.

Figure 2: Scatter plot of changes in income versus transfer adjustments

(a) State-level data, controlling for average transfer adjustment



(b) Aggregate data



Note: The weighted-least-squares (WLS) slopes report the percentage increase in (aggregate or state) income in response to an (aggregate or state) transfer adjustment as a percent of income. Changes in income and transfer are residualized with respect to relevant controls. Each point in panel (a) corresponds to an average within a binned cluster of the underlying state-level variable.

Our panel estimates of aggregate multipliers have analogous sensitivity to these unusual time periods. One quarter horizon estimates are not statistically different from zero without the two large treatment periods. Four quarter horizon aggregate multipliers remain statistically different from zero but our (full sample) finding of an aggregate multiplier greater than one does not survive the loss of 1972Q3.

Our panel results appear robust to at least small variation in the included states; details are in the Appendix A.

5 Labor Earnings Multipliers

In this section, we estimate multipliers as above with gross labor income rather than total personal income used in our benchmark specifications. This allows us both a more complete picture of the impact of transfers and a more direct comparison with the existing work of Pennings (2021).

We replicate the multiplier estimates in Table 1 and 2, using earnings by place of work (i.e., labor earnings) reported by Bureau of Economic Analysis to construct outcomes and weights. Following Pennings (2021), we do not net out employer and employee contributions for social insurance. Specifically, we re-define $Y_{i,t}$ to be the seasonally adjusted gross labor earnings in state i during quarter t, measured in real and per capita units. We then follow equation (1), (2) and (3) to calculate variables. Our weighting s_i^Y uses initial labor income shares in this section; otherwise specifications mirror those in Section 4.

Table 3 presents the 1-quarter impact multiplier on labor earnings, the analog of Table 1. Column (1) presents estimates of local and aggregate multipliers of 1.67 (SE=0.87) and 1.32 (SE=0.61), respectively. The local and aggregate multipliers are smaller than their counterparts in Table 1, mainly because Social Security transfers (along with income from other sources such as dividends and interest) are included in personal income but not in labor earnings. The spillover effect is estimated to be close to zero and not statistically significant. We find evidence of a positive aggregate multiplier, driven by the local multiplier. A comparison of column (5) with column (1) reveals that weighting matters for these results.[12] In column (5) without weighting (weighting uniformly) we get very imprecise local estimates and aggregate estimates with only a borderline significant aggregate effect. However, our (relatively noisy) column (5) aggregate estimates are still compatible with results under weighting in column (1).

The specification in column (6), the unweighted least squares with state and time fixed effects, is very close to the main specification in Pennings (2021). In column (6), we estimate the local multiplier to be 1.15 (SE=0.82), close to his benchmark estimates of 1.29

[12] Throughout this section, the weight we use in the regressions refers to the state (initial) shares of labor earnings. The states' personal income shares and labor earnings shares are highly similar. The correlation coefficient of the two series is above 0.99.

(SE=0.54). Pennings (2021) finds smaller standard errors than we do. He uses state clustered errors whereas we report Driscoll-Kraay standard errors. The latter allows for cross state correlation in disturbances whereas the former does not. In our view, the latter is more appropriate in the presence of cross-sectional correlation in the error term, a feature of an economy in which different regions have similar exposures to business cycle shocks. Accordingly, Pennings (2021) may overstate the precision of his estimates because he does not account for cross-state correlation in the errors. For reference, if we switch from Driscoll-Kraay errors to state clustered errors, the SE for local multiplier in column (6) shrinks to 0.55.

Table 3: The fiscal multiplier on labor earnings from an increase in government transfers, one-quarter impact multiplier

	(1)	(2)	(3)	(4)	(5)	(6)
Local	1.674*				1.149	
	(0.872)				(0.797)	
Spillover	-0.352				0.143	
	(0.927)				(1.145)	
Aggregate	1.322**		1.322**	1.345**	1.291*	
	(0.615)		(0.615)	(0.605)	(0.740)	
γ/TWFE	1.681*	1.681*			1.145	1.145
	(0.888)	(0.888)			(0.817)	(0.817)
Weights	✓	✓				
State FE	✓	✓			✓	✓
Time FE		✓				✓
Num of periods	92	92	92	92	92	92
Num of divisions	46	46	1	1	46	46

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses.

Dependent variable is one-quarter growth in per capita earnings by place of work (labor earnings). Column 3 uses approximate RHS and LHS variables and estimates aggregate model. Column 4 uses exact RHS and LHS variables and estimates aggregate model. All estimates include full sets of (appropriately-scaled) controls. Column 3 and 4 report Bartlett SE and remainder report Driscoll Kraay SE, all of which use a Bartlett window with four leads and lags.

Table 4 provides the four-quarter cumulative multipliers on labor earnings, the analog of Table 2. Compared to Table 2, we find a smaller local multiplier of 2.53 (SE=0.95). Notably, our estimated aggregate gross earnings multiplier of 2.84 (SE=0.93) is larger than its counterpart net income multiplier of 2.70 (SE=0.80). This is despite the former not including the "mechanical" positive (equal to one) component of transfers. Thus our results for labor income indicate a substantially larger aggregate multiplier than for total income. This occurs because our net personal income specification subtracts contributions for government social insurance whereas the gross earnings specification does not. In results available on request, we find that a one dollar increase in transfers is associated with an $0.86 increase in social insurance contributions over a four-quarter horizon.

Again, column (6) employs a comparable specification to that in Pennings (2021). Our estimate of four-quarter cumulative local multiplier equals 1.64 (SE=0.91), close to 1.74 (SE=0.76), the benchmark result of Pennings (2021). Also as above, weighting matters for this similarity, our local multiplier estimate with weighting in column (1) is substantially larger. However, weighting does not matter much for our aggregate multiplier estimates as we obtain very similar results across columns (1) and (5).[13]

Our above findings of large local and aggregate transfer multipliers along with small spillover multipliers is generally consistent with existing research. As explained above, our local multiplier is close to the TWFE multiplier Pennings (2021) estimates. Pennings (2021) does not estimate an aggregate multiplier, but rather explains how his estimated multiplier along with an economic model can used to infer the aggregate multiplier. He shows analytically how different model aspects (e.g., the stance of monetary policy and the share of hand-to-mouth consumers) can influence the mapping from the TWFE multiplier an aggregate one. He explains how varying those aspects can result in a large or small aggregate multiplier. Gechert, et.al. (2021) studies legislated transfer shocks in Germany and finds that the benefits multiplier on GDP is about 1.1 and persistent, which is consistent with our finding of a large labor earnings multiplier.

[13]The same is true in terms of weighted state changes in growth rates versus changes in aggregates reflected in columns (3) and (4).

Table 4: The fiscal multiplier on labor earnings from an increase in government transfers, one-year cumulative multiplier

	(1)	(2)	(3)	(4)	(5)	(6)
Local	2.53***				1.66*	
	(0.95)				(0.90)	
Spillover	0.30				1.02	
	(1.25)				(1.24)	
Aggregate	2.84***		2.84***	2.79***	2.69***	
	(0.93)		(0.93)	(0.94)	(0.91)	
γ/TWFE	2.53***	2.53***			1.64*	1.64*
	(0.97)	(0.97)			(0.91)	(0.91)
Weights	✓	✓				
State FE	✓	✓			✓	✓
Time FE		✓				✓
Num of periods	92	92	92	92	92	92
Num of divisions	46	46	1	1	46	46

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses.

Dependent variable is one-year cumulative growth in per capita earnings by place of work (labor earnings). Column 3 uses approximate RHS and LHS variables and estimates aggregate model. Column 4 uses exact RHS and LHS variables and estimates aggregate model. All estimates include full sets of (appropriately-scaled) controls. Column 3 and 4 report Bartlett SE and remainder report Driscoll Kraay SE, all of which use a Bartlett window with four leads and lags.

Our results for labor income multipliers display sensitivity to unusual treatments similar to the sensitivity of multipliers for personal income. For the most part, the unusually large treatment periods provide much of the precision of our full sample estimates. Our four quarter horizon estimates display less sensitivity to these periods relative to those at one quarter, with aggregates at the longer horizon remaining statistically different from zero across all ’dropping highest treatment period(s)’ exercises.

6 Conclusion

This paper uses (conditionally) exogenous state level variation in federal transfer payments along with the sum rule of derivatives to estimate the aggregate transfer multiplier and decompose it into a local and spillover multiplier. Our point estimates indicate that the local four-quarter multiplier is approximately 3.4 and the spillover multiplier is about -0.7. The aggregate multiplier, the sum of its two components, equals 2.7. Our estimates indicate, that at conventional confidence levels, (i) one can reject a less than one-for-one effect of aggregate transfers on aggregate income; (ii) one cannot reject a zero spillover effect. Across specifications, the precision of our findings for local and aggregate multipliers is in large part due to a small number of unusually informative transfer changes in the early 1970s.

Finding (i) answers our paper's first question, concerning knock-on multiplier effects. Specifically, increasing aggregate transfers may provide an efficacious way to stimulate macroeconomic activity because of a greater than one-for-one effect on aggregate income.

Finding (ii) provides a less precise answer to the paper's second question, concerning cross-region spillovers. While we cannot reject zero spillovers, the spillover estimate's standard error is large. We cannot reject either substantial positive or negative spillovers. The imprecision arises because there is strong covariance between the local and spillover treatments in our sample. To see this, recall that because most states are small, the spillover treatment is approximately equal to the average treatment. Next, each local treatment is calculated as a share of the average treatment in that quarter, where that share is measured from the lagged share of social security benefits to a state. These shares move little over the 20 year time span we study. As such, the observed comovement between the local and spillover variables makes it difficult to disentangle the local versus spillover channel of the aggregate multiplier.

Methodologically, we illustrate how to simultaneously estimate local and aggregate causal impacts in a framework where aggregate treatments are the sum of state-level treatments. This allows a decomposition of aggregate effect estimates (comparable to those estimated from aggregate time series) into local and spillover components. The method contrasts with many applied macro papers employing panel data which use time fixed effects.[14] Including time fixed effects precludes the kind of analysis in our paper because aggregate effects become no longer identified. In this situation, researchers might alternatively appeal to additional economic structure to identify aggregate effects from disaggregate data.

[14] See for example all of the papers in footnote 9.

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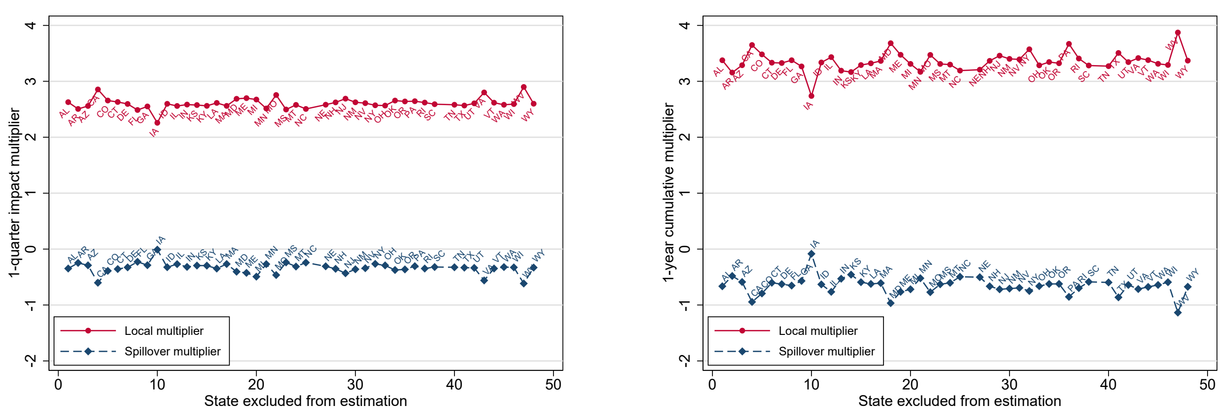
Appendix A Sensitivity of Particular States to Results

We investigate the sensitivity of our results to omission or any individual state. We weight our panel-based regressions by state size s_i^Y to account for size heterogeneity in estimation and to make the disaggregate panel-based results comparable with aggregate ones. However, it is still possible that an individual state is particularly influential in estimation. To study the potential influence of outliers, we re-estimate the one-year cumulative and one-quarter impact multipliers in Figure A1, excluding one state at a time.

The empirical specifications in panel (a) and (b) correspond to that in column (1) of Table 1 and Table 2, respectively. Each point denotes a local or spillover multiplier when a specific state, whose name is marked alongside the point, is excluded from the estimation. It is clear from Figure A1 that no state is particularly important in driving the results, and all of the alternative-sample-based multipliers are well within the 95% confidence interval of their counterparts in Table 1 and Table 2.

Figure A1: Multiplier estimates excluding specific states

(a) One-quarter impact multipliers (b) One-year cumulative multipliers

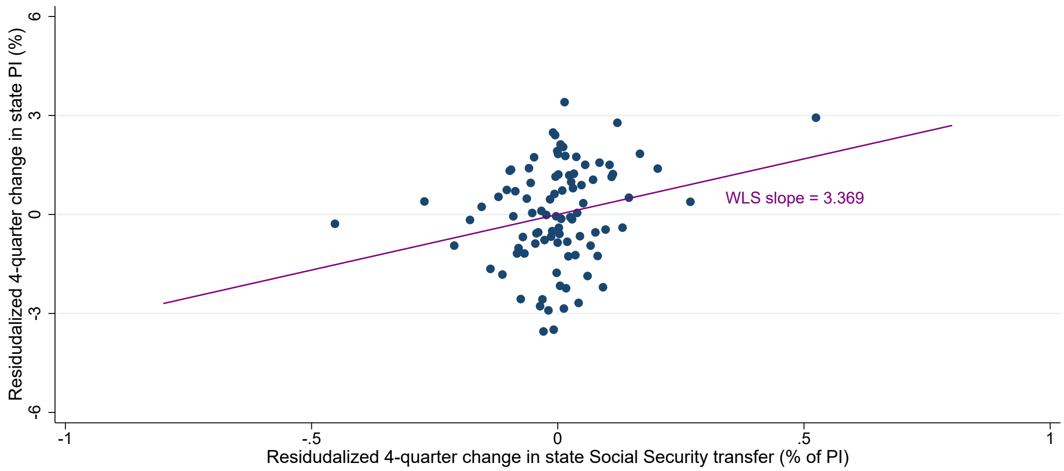


Note: Each point corresponds to an estimate of local multiplier or spillover multiplier with a particular state excluded from estimation. All estimates include full sets of (appropriately-scaled) controls.

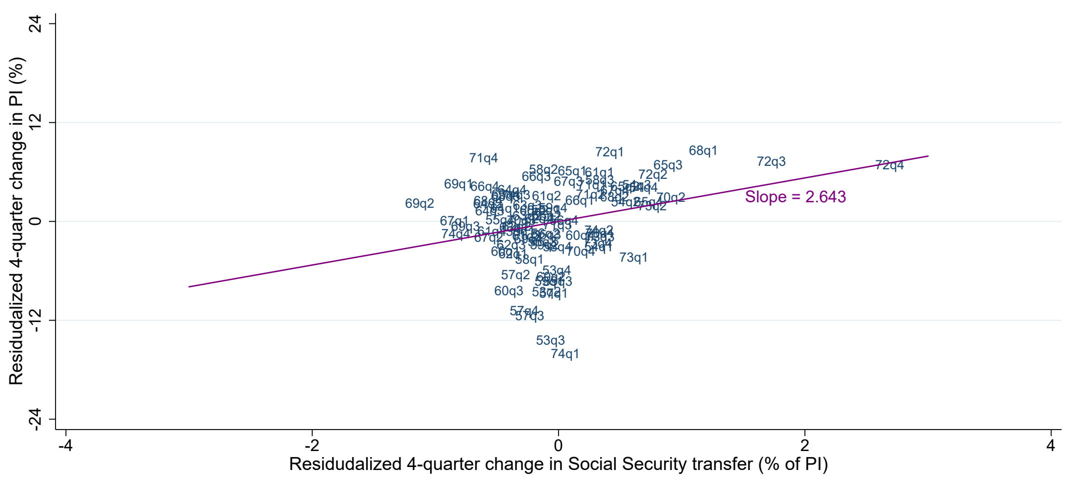
Appendix B Sensitivity of Particular Quarters to Results

Figure B1: Scatter plot of changes in income versus transfer adjustments at 4-quarter horizon

(a) State-level data, controlling for average transfer adjustment



(b) Aggregate data



Note: The weighted-least-squares (WLS) slopes report the percentage increase in (aggregate or state) income in response to an (aggregate or state) transfer adjustment as a percent of income. Changes in income and transfer are residualized with respect to relevant controls. Each point in panel (a) corresponds to an average within a binned cluster of the underlying state-level variable.

Table B1: One-quarter impact multiplier without unusual treatments

	(1) Benchmark	(2) Drop 2Q1970	(3) Drop 4Q1972	(4) Drop Both
Local	2.605***	3.109***	2.250	3.532*
	(0.973)	(1.017)	(1.577)	(2.089)
Spillover	-0.336	-0.746	-0.696	-2.431
	(1.037)	(1.223)	(1.828)	(2.289)
Aggregate	2.269***	2.363***	1.555**	1.101
	(0.528)	(0.610)	(0.664)	(0.956)
Weights	✓	✓	✓	✓
State FE	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses.

Dependent variable is one-quarter growth in per capita personal income. All estimates include full sets of (appropriately-scaled) controls. All columns report Driscoll Kraay SE, using a Bartlett window with four leads and lags.

Table B2: Four-quarter multipliers without unusual treatments

	(1) Benchmark	(2) Drop 3Q1972	(3) Drop 4Q1972	(4) Drop Both
Local	3.355***	2.551**	2.668**	2.623**
	(0.982)	(1.236)	(1.092)	(1.282)
Spillover	-0.655	0.354	0.678	0.503
	(1.225)	(1.657)	(1.459)	(1.631)
Aggregate	2.700***	2.905**	3.346***	3.126**
	(0.803)	(1.336)	(1.063)	(1.267)
Weights	✓	✓	✓	✓
State FE	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses.

Dependent variable is one-year cumulative growth in per capita personal income. All estimates include full sets of (appropriately-scaled) controls. All columns report Driscoll Kraay SE, using a Bartlett window with four leads and lags.