

# Missing Intercept, Biased Slope: Identification in General Equilibrium\*

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## Abstract

Many studies use cross-sectional data to identify the “direct” (or “partial equilibrium”) effects of macroeconomic shocks. I consider identification of these parameters via “shift-share” panel regressions within a general environment that features agent-heterogeneity, dynamics and general equilibrium effects. I first demonstrate that identification via “exogenous shocks” is confounded by the presence of agents’ heterogeneous responses to general equilibrium effects. I instead propose a robust and simple approach to identification by adding control variables that capture the targeting rule of the exposure-shares across units. In an application, I find that adjustments to regression specifications in line with my theoretical results substantially alter the conclusions from previous studies regarding the size of the local military-spending multiplier.

**Key Words:** Identification; General Equilibrium; Spillovers

**JEL Codes:** C21, C23, E10, E20, E60.

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# 1 Introduction

An increasingly popular approach in empirical macroeconomics uses cross-sectional data to identify the direct (or “partial equilibrium”) effects of macroeconomic shocks. This includes classic studies estimating households’ marginal propensities to consume (Parker et al., 2013), regional fiscal multipliers (Nakamura and Steinsson, 2014) or firms’ responses to tax cuts (Zwick and Mahon, 2017). It is well-understood that this approach in general suffers from the so-called “missing intercept” problem: cross-sectional data typically does not contain sufficient information to recover the total (or “general equilibrium”) effect of the shock under study. However, the approach remains popular since the direct response of individual units to macroeconomic shocks can provide information on key parameters in macroeconomic models. Estimates of these ‘direct’ effects thus serve as a powerful source of indirect evidence on questions of central interest such as the effectiveness of fiscal stimulus (see e.g. Nakamura and Steinsson, 2018; Chodorow-Reich, 2020).

In this paper, I take as given the usefulness of these objects and study methods for reliably achieving identification. I do so in a comprehensive setup that incorporates key features of modern macroeconomic models: dynamics, forward-looking expectations, heterogeneous-agents and general equilibrium effects. I focus discussion on identification via panel “shift-share” regressions of the form:

$$y_{i,t+h} = \alpha_i + \delta_t + \beta(z_i \cdot \varepsilon_t) + \text{controls} + u_{i,t} \quad (1)$$

where  $y_{i,t+h}$  is the outcome variable of interest for unit- $i$  (such as household consumption or regional output)  $h$ -periods from time- $t$ ,  $\varepsilon_t$  is a time- $t$  macroeconomic shock, and  $z_i$  is a measure of the “exposure” of unit- $i$  to the shock under study, and  $\alpha_i$  and  $\delta_t$  denote unit- and time-fixed effects respectively.<sup>1</sup>

To fix ideas, I focus on identification of the direct effects of government stimulus – e.g. households’ marginal propensities to consume (MPCs) out of government transfers or regional government-spending multipliers. My key contribution is to provide conditions on the exposure-shares  $z_i$  and the shock-variable  $\varepsilon_t$  such that, alongside an appropriate choice of control variables,  $\beta$  captures the (average) direct response of individual units to fiscal stimulus shocks. Although shift-share identification has been well-studied in the microeconomics literature (see e.g. Borusyak et al., 2025, for a review), I depart from this previous work along a number of important dimensions. First, I depart from the “Stable Unit Treatment Value Assumption” (SUTVA) – an assumption that underpins classic results on identification across applied microeconometrics but which explicitly rules out the possibility of general equilibrium (GE) effects arising from the intervention under study. I show that depart-

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<sup>1</sup>Such specifications are common in macroeconomics. See, for example: Nakamura and Steinsson (2014); Penning (2021); Chodorow-Reich et al. (2021).

tures from this assumption – alongside incorporating other key features of my environment – has material consequences for identification. Second, my framework maps directly to (a class of) heterogeneous-agent dynamic stochastic general equilibrium (DSGE) models, exactly the macroeconomic models which cross-sectionally identified moments are typically intended to be informative about. Third, my framework jointly specifies processes for macroeconomic aggregates alongside unit-level outcomes, and is cast within the Slutsky-Frisch paradigm commonly used to study dynamic causal effects in macroeconomics (e.g. [Stock and Watson, 2018](#)). This allows me to present a unified discussion of identification in macroeconomics using either time-series or cross-sectional data.

**Identification Results** I present a series of results on identification via panel regressions of the form (1) within my environment. Previous literature highlights two alternate routes to identification: either via exogeneity of the shocks  $\varepsilon_t$ , or via exogeneity of the shares  $z_i$ . My initial results are negative and relate to identification via “exogenous shocks”. I show that even if  $\varepsilon_t$  captures a series of essentially random adjustments in government stimulus at the national level over time, the shift-share regression (1) generally fails to identify the direct effects of the stimulus since  $\beta$  is contaminated by the differential response of treatment and control groups to the GE effects of the shock. The problem arises since, in GE, changes in government spending impact individuals via various channels – not just via the stimulus directly but also via changes in e.g. interest rates, taxes, wages – and so (1) generally picks-up the heterogeneous responses of units to *all* of these transmission channels simultaneously. Crucially, heterogeneity across units implies that inclusion of time fixed-effects in (1) does not serve to ensure these GE responses simply “wash-out” in the cross-section. These negative results echo some previous discussion on identification in macroeconomic settings which I then extend along numerous dimensions. I stress that various approaches to correct for this issue through additional control variables that seek to capture directly these GE spillovers do not overcome the problem – either because they are not robust to the presence of *dynamic* channels of GE transmission, or because they are not robust to the presence of heterogeneity in units’ responses to their *own* treatment.

My main contribution is to demonstrate an approach to identification that is immune to these concerns. My approach views the exposure shares  $z_i$  as being implicitly targeted by the policymaker based on some set of underlying unit-level characteristics. For example, government social security transfers are naturally targeted to households with lower levels of income and wealth; and likewise national military spending is naturally targeted to regions with larger high-tech manufacturing sectors, and thus with a larger proportion of employment in the tradable sector. I then show that identification can be achieved by controlling appropriately for any unit-level characteristics that feature in the underlying “targeting rule”. In the shift-share regression (1), this requires adding as controls the interaction of these unit-level variables with

the time-series shock of interest. Intuitively, this approach ensures that  $\beta$  from regression (1) compares only across units that (on average) respond the same to any GE effects arising from the aggregate shock  $\varepsilon_t$ . I allow for the direct effects of the stimulus to be heterogeneous across agents, and so my key result demonstrates that this approach successfully recovers a (convex-) weighted average of the underlying direct effects (i.e. the typical target of applied work in settings with heterogeneous treatment effects). I stress that this approach remains robust in the face of various features that typically pose significant challenges for identification e.g. heterogeneity, dynamics, forward-looking expectations, GE effects, etc. Further, the control-based approach is simple to implement and does not require the researcher to have any knowledge of the underlying GE effects of the stimulus shock under study – e.g. by how much interest rates or taxes adjusted to the shock, or how different units responded to such changes. Nevertheless, the informational requirements are generally strong, and crucially rely on the researcher having access to control variables that effectively span the various dimensions of unit-level heterogeneity that simultaneously determine treatment and drive heterogeneous responses to the GE effects of the policy intervention. This requirement is trivially satisfied in settings where stimulus was randomised across units (as in e.g. [Parker et al., 2013](#)), although it is generally harder to satisfy in empirical settings where variation in stimulus received by each unit lacks a clear “targeting rule” based on underlying unit-characteristics (as in e.g. [Nakamura and Steinsson, 2014](#)).

I further show that this approach remains robust even when the researcher is unable to correctly identify a series of government spending shocks at the national level. This is crucial since this is typically the motivation for employing panel (rather than time-series) data to achieve identification.<sup>2</sup> I demonstrate that, so long as the researcher controls for the “targeting rule” for the exposure-shares  $z_i$ , then identification of the direct effects of government stimulus on each unit can proceed by interacting  $z_i$  with *any* combination of (time- $t$ ) macroeconomic shocks. This materially weakens the time-series identification challenge, e.g. allowing the researcher to recover an estimate of (intertemporal) household MPCs out of government stimulus, even when the total effects on aggregate consumption are not identified.

My main results focus on identification via panel shift-share regressions. However, I demonstrate that my proposed approach can also be used to achieve identification with purely cross-sectional data. Again, the key requirement for identification is that the researcher controls for any unit-characteristics that determine treatment-exposure and responses to any GE variables that adjust to the aggregate shock under study. This requirement differs from (and is generally stronger than) the canonical “parallel trends” assumption typically employed in the difference-in-difference literature. Further, while my baseline environment considers an infinite population of agents who respond heterogeneously to aggregate variables that adjust in GE, I extend

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<sup>2</sup>Note that if the researcher in fact had access to a series of government spending shocks at the national level then the aggregate fiscal multiplier would be straightforward to recover from a simple time-series local projection.

this to consider settings with a finite number of agents who respond directly to treatment of each other individual unit. This speaks to identification within a class of (linearised) network models. I show that a similar bias arises due to heterogeneous spillover effects from aggregate shocks in these settings. The bias can be large when unit-level shocks are “clustered”, such that units who receive large stimulus shocks simultaneously experience larger spillover effects arising from stimulus shocks to other units. I demonstrate that an appropriate correction is again to control for the underlying “targeting rule” that generates this clustering, where crucially this does not require any knowledge of the underlying network structure that generates spillover effects across units.

**Quantitative Example** I demonstrate my results within a simple calibration of a Two-Agent New Keynesian Model. I start by considering a setting where the researcher has direct access to a series of government transfer shocks, and seeks to estimate household MPCs out of the transfer. I assume the probability of a household receiving the transfer depends on their hand-to-mouth status, and consider results for various targeting rules. I show that a shift-share regression of the form (1) that interacts the transfer shock  $\varepsilon_t$  with an indicator for whether the household receives the transfer  $z_i$  can be heavily biased for MPCs. For example, when the transfers are targeted entirely to Ricardian households, the shift-share regression estimates an on-impact MPC out of the transfer of around 1.0, even though the true MPC is around 0.01. In this example, since the transfers are financed by lump-sum taxes on all units, the shock serves to transfer resources to Ricardian households, leading to a contraction in aggregate demand in the near-term, depressing wages and interest rates. The hand-to-mouth households cut their consumption in response to these GE effects (since they face higher taxes and lower wages), while the Ricardian households increase their consumption in the near-term in response to these adjustments (since the intertemporal smoothing motive from the lower interest rates dominate the increase in taxes and fall in wages). Thus an econometrician running (1) in this setting may mistakenly conclude that the transfer policy was highly effective at stimulating consumption (at least directly), when in fact the stimulus was primarily saved (not spent) and in fact led to a significant *contraction* in aggregate consumption.

Within this model, the bias is avoided by simply controlling for the transfer shock interacted with an indicator for whether households are Ricardian or hand-to-mouth. Then the resulting estimate retains the desired interpretation as capturing a weighted average of MPCs of recipient households for essentially any assignment of transfers across the population. For example, when the probability of receiving the transfer is zero for Ricardian households, and lies strictly between zero and one for hand-to-mouth households, the shift share coefficient with this additional control recovers exactly the MPC of hand-to-mouth agents.

I then demonstrate that, when aggregate transfers follow an endogenous policy rule (e.g. responding to adjustments in aggregate inflation), the researcher can recover an estimate of

household (intertemporal) MPCs by projecting on any combination of time- $t$  shocks in the model. In this case, a shift-share regression which includes a technology shock interacted with the stimulus shares (alongside the appropriate controls) retains its interpretation as capturing household intertemporal MPCs, even though the shifter in this case captures variation in transfers that are endogenous to business cycle developments. The intuition is simply that the shift-share regression in this case compares across households who are differentially affected by the technology shock only via their differential exposure to changes in government transfers.

**Empirical Applications** I then consider the practical importance of my econometric results through various empirical applications. I first revisit the empirical application of [Nakamura and Steinsson \(2014\)](#) who estimate local fiscal multipliers out of military spending using a shift-share research design, interacting a measure of national military spending with state-level exposure-shares. My results imply that identification fails even when adjustments in national military spending occur essentially randomly over time. I then show that the exposure-shares in the shift-share regression from [Nakamura and Steinsson \(2014\)](#) are highly predictable with just a handful of variables that macroeconomic theory suggests should be important determinants of heterogeneity in regional responses to GE objects – e.g. measures of regional openness and MPCs ([Bellifemine et al., 2023](#)). Including combinations of these variables as additional controls (interacted with the shock of interest) greatly reduces the estimated local fiscal multiplier in the shift-share regression – from 2.5 in the original study to 0.5-0.9 depending on the specification, where the latter are tightly estimated (e.g. rejecting multipliers above 1.5 at conventional levels of significance). This essentially reverses the headline results of the paper that regional multiplier estimates are more in line with those from a “New Keynesian” model than a “Neoclassical” model, and underscores the practical significance of my econometric results.

I also revisit [Chodorow-Reich \(2019\)](#) who combines three studies estimating local fiscal multipliers out of the 2009 stimulus package in the US - [Chodorow-Reich et al. \(2012\)](#), [Wilson \(2012\)](#), [Dupor and Mehkari \(2016\)](#). I show all three instruments exhibit significant regional clustering. I demonstrate explicitly with a simple quantitative model that such clustering tends to overstate local fiscal multipliers when spending stimulates external demand and trade is governed by a gravity equation. I use discussion from the original studies regarding the underlying targeting rule to propose various control variables in order to clean each instrument of the clustering. With these additional control variables I find weaker evidence of above-1 fiscal multipliers than in [Chodorow-Reich \(2019\)](#), although, taken together, the three studies continue to jointly estimate a multiplier out of the 2009 stimulus package of around 2.0.

**Literature** This paper relates to various strands of literature.

First, and most directly, I relate to a literature on (panel) shift-share research designs. The

majority of this literature considers identification in settings without dynamics or GE effects (Adao et al., 2019; Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022b; Arkhangelsky and Korovkin, 2020; Majerovitz and Sastry, 2023). I show that incorporating these features has important implications for identification – and demonstrate a route to identification within a comprehensive framework that incorporates both of these features, alongside unit-heterogeneity. My work is complementary to this previous literature in that I highlight that the key to identification in this environment is to compare across cross-sectional units that are appropriately “balanced” (as in e.g. Goldsmith-Pinkham et al., 2020; Arkhangelsky and Korovkin, 2020) rather than solely by relying on exogeneity of the time-series shocks (as in e.g. Adao et al., 2019; Borusyak et al., 2022b; Majerovitz and Sastry, 2023). My environment of interest is closely related to Almuzara and Sancibrián (2024), and, like them, I begin with a general characterisation of the estimand from shift-share regressions. I then employ this characterisation to answer a different set of questions from Almuzara and Sancibrián (2024) – they are concerned with inference (not identification), and consider cases where the researcher is interested only in recovering an estimate of unit-level heterogeneity in impulse responses (rather than the “direct” effect of shocks per se).<sup>3</sup> Thus, while they provide a general framework for inference when employing microeconomic data in macroeconomic settings, I work within the same environment to provide a general framework for identification.

In recent work, Donaldson (2026) also considers identification via shift-share panel regressions in a heterogeneous-agent DSGE setting. As in this paper, she notes that typical shift-share specifications are not robust when agents respond heterogeneously to GE objects. The two works are complementary and jointly offer applied researchers a menu of options for both assessing and correcting for biases stemming from heterogeneous responses to GE objects. Donaldson (2026) focuses on a setting where agents respond homogeneously to their own treatment but are heterogeneously exposed to changes in a single GE variable (the interest rate), and shows that this bias can be stripped out by using time-series evidence on the effects of monetary policy shocks (following the method in McKay and Wolf (2023)). In contrast, I consider a general environment where agents respond heterogeneously to their own treatment as well as to multiple GE objects and develop an approach to identification that is robust to this possibility. This is crucial since macroeconomic shocks typically exhibit multiple GE channels of transmission – e.g. not just via interest rates, but also prices, wages, taxes, etc. – and the forces that drive heterogeneous responses to any one of these channels – e.g. heterogeneity in units’ discount factors – simultaneously drives heterogeneous responses to the other channels. I demonstrate a route to identification that is robust to this concern and which succeeds even when the researcher is unable to credibly identify any individual macroeconomic

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<sup>3</sup>Specifically, they are primarily concerned with studies such as Ottonello and Winberry (2020) who estimate the heterogeneous effects of monetary policy shocks across firm-types, while I am concerned with studies such as Nakamura and Steinsson (2014) who seek to recover an estimate of local fiscal multipliers.

shock (typically the motivation for employing cross-sectional data to achieve identification). Various other work also considers identification via panel data in macroeconomic settings – although their focus is on achieving identification of the aggregate effects of shocks e.g. via SVAR-based identification schemes (Sarto, 2025; Matthes et al., 2025).

My discussion of biases that can arise for shift-share regressions when agents respond heterogeneously to GE effects also closely relates to recent discussion of identification in the presence of spatial spillovers in Adão et al. (2019) and Borusyak et al. (2022a), who focus on trade and migration models respectively. Both papers propose to achieve identification by first writing down a fully-specified GE model and then estimating a model-consistent reduced-form equation. While motivated by similar concerns to this paper, the setting of interest and proposed solutions are very different – since my solution is “design-based” (i.e. relying only on the researcher having knowledge of the treatment assignment across units), it: (a) does not rely on knowledge of the underlying GE model that generates the spillovers; and so (b) allows researchers to recover “identified moments” that are valid across a broad class of models.

Further, I relate to a literature in macroeconomics on the appropriate interpretation of cross-sectional regressions from the perspective of macroeconomic models (Nakamura and Steinsson, 2014, 2018; Chodorow-Reich, 2019; Koby and Wolf, 2020; Chodorow-Reich, 2020; Guren et al., 2021; Wolf, 2023). Traditional discussion of this issue considers settings where agents are homogeneous – see e.g. Nakamura and Steinsson (2014); Chodorow-Reich (2020); Guren et al. (2021) – in which case the intercept term in cross-sectional regressions (or likewise the time fixed effect in panel regressions) captures all relevant GE effects of policy interventions (and so the issues for identification discussed in this paper do not arise).<sup>4</sup> The fact that identification of key model parameters is complicated by the presence of heterogeneous agents and GE effects appears to have first been noted in Koby and Wolf (2020) who employ a calibrated structural model to circumvent the problem. Wolf (2023) also considers identification in a setting with heterogeneous agents but assumes that the researcher has access to a series of well-identified aggregate stimulus shocks and that the exposure-shares across agents is by construction randomly assigned – where my key contribution is to demonstrate a route to identification when both these assumptions fail.

I also relate to a literature analysing the estimands of cross-sectional research designs in settings with heterogeneous treatment effects – e.g. classic discussion of OLS and IV (Imbens and Angrist, 1994; Angrist, 1998), as well as more recent discussion on DiD (see e.g. de Chaisemartin and D’Haultfoeuille (2023) for a review). I depart from this literature by considering identification in a (semi-parametric) framework that nests a class of heterogeneous-agent DSGE models. My key result leverages results from Borusyak and Hull (2024) who

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<sup>4</sup>In these settings, cross-sectional regressions identify direct effects of interventions, and the intercept term soaks up the GE effects alongside the average effect of any other shocks, which generates the classic “missing intercept” problem.

show that design-based (but not model-based) regression-control specifications identify a convex weighted-average of treatment effects – where I show that this insight can still be used constructively to achieve identification of objects of interest in this environment.

Throughout, the discussion is focused on the macroeconomics literature (and on identifying the effects of fiscal stimulus in particular). However, my results are also informative for applications across other areas of economics – e.g. in finance (e.g. [Chodorow-Reich, 2014](#)), labour (e.g. [Acemoglu and Restrepo, 2020](#)), trade (e.g. [Autor et al., 2013](#)) and development (e.g. [Gerard et al., 2021](#); [Egger et al., 2022](#); [Galego Mendes et al., 2023](#)), – that employ cross-sectional identification in settings with potentially sizable spillover effects via GE forces, and seek to interpret resulting estimates as capturing the “direct”, “local”, or “partial equilibrium” effect.

**Outline** The rest of the paper is outlined as follows. Section 2 presents the key issues discussed in the paper through the lens of a simple setting. Section 3 sets out my general environment and contains my key results on identification. Section 4 provides an illustration of my results in a Two-Agent New Keynesian model. Sections 5 and 6 contain empirical applications. Section 7 concludes.

## 2 Simple Setting

I begin with a simple setting to demonstrate how bias can arise in settings with heterogeneous-agents and GE effects and to describe my proposed solution. I consider an econometrician interested in studying the effects of government transfer shocks on household consumption. I first describe a simple stylised model, then discuss issues related to identification.

**Model** The economy is populated by a unit-continuum of households:  $i \in [0, 1]$ . The deviation of consumption from its steady-state level for each household- $i$  is determined by a simple Keynesian consumption function:

$$\hat{c}_i = \beta_i(\hat{y}_i + \hat{\mathcal{T}}_i - \hat{\tau}_i) + u_i \quad (2)$$

where  $\beta_i$  captures household  $i$ 's MPC out of disposable income,  $\mathcal{T}_i$  and  $\tau_i$  denote government transfers and taxes respectively,  $y_i$  denotes other income, and  $u_i$  is a mean-zero random variable capturing idiosyncratic shocks to consumption, and hats denote deviations of each variable from their steady-state. Market clearing implies:

$$\int_i \hat{c}_i di = \int_i \hat{y}_i di \quad (3)$$

I assume output is entirely demand-determined, with income allocated across units such that:

$$\hat{y}_i = \alpha_i^y \int_i \hat{y}_i di \quad (4)$$

The government's budget constraint is:

$$\int_i \hat{\mathcal{T}}_i di = \int_i \hat{\tau}_i di \quad (5)$$

where unit-level taxes are set such that:

$$\hat{\tau}_i = \alpha_i^\tau \int_i \hat{\tau}_i di \quad (6)$$

In this case, each units' consumption can be written as a function of changes in their *own* transfer  $\hat{\mathcal{T}}_i$ , GE responses to changes in *aggregate* transfers, as well as idiosyncratic shocks:

$$\hat{c}_i = \underbrace{\beta_i \hat{\mathcal{T}}_i}_{\text{Direct Effect}} + \underbrace{\zeta_i}_{\text{GE Effect}} + \underbrace{u_i}_{\text{Other Shocks}} \quad (7)$$

where the GE effect can be decomposed as:<sup>5</sup>

$$\zeta_i = \beta_i \alpha_{y,i} \frac{\left( E[\beta_i \hat{\mathcal{T}}_i] - E[\hat{\mathcal{T}}_i] E[\beta_i \alpha_i^\tau] \right)}{1 - E[\beta_i \alpha_i^y]} - \beta_i \alpha_i^\tau E[\hat{\mathcal{T}}_i] \quad (8)$$

The first term captures unit-*i*'s response to GE feedback from changes in aggregate transfers via changes in income – the standard Keynesian multiplier channel, which here also depends on the covariance between transfers and MPCs as well as the covariance of MPCs with income- and tax-allocations – and the second term captures their response to GE feedback via changes in taxes.

**Object of Interest** In this setting the effect of transfer shocks on aggregate consumption  $\hat{c} = \int_i \hat{c}_i di$  is given by:

$$\int_i \left( \beta_i \hat{\mathcal{T}}_i + \zeta_i \right) di$$

Although this is often the *ultimate* object of interest in macroeconomics, the purpose of this paper is to discuss specifically how cross-sectional methods can be effectively employed to recover an estimate of the “direct” effects of the intervention on individual units. As has been argued elsewhere, these objects serve as “identified moments” across a broad class of models and thereby provide powerful evidence on key questions in macroeconomics by providing effective indirect evidence on the overall effects of policy interventions (Nakamura and Steinsson, 2018). When agents are heterogeneous, the goal of typical regression analysis is to learn about some *average* measure of these direct effects:

<sup>5</sup>This follows from plugging in equations (3)-(6) into the consumption function (2) and rearranging.

$$\int_i \tilde{\omega}_i \beta_i di \quad (9)$$

for  $\int_i \tilde{\omega}_i = 1$  and  $\forall i : \tilde{\omega}_i \geq 0$ . Note that the requirement that the  $\tilde{\omega}_i$ 's sum to be one and be non-negative ensures that the object is indeed interpretable as a weighted average of underlying treatment effects.<sup>6</sup>

**Identification** As a starting point, consider cross-sectional regressions of the form:

$$\hat{c}_i = \alpha + \beta^{ols} \hat{\mathcal{T}}_i + e_i \quad (10)$$

Under what assumptions does this regression recover the object of interest? For simplicity, consider the case with binary shocks:  $\hat{\mathcal{T}}_i \in \{0, 1\}$ . The standard "parallel trends" assumption that typically permits identification is:

$$E[u_i | \hat{\mathcal{T}}_i = 1] = E[u_i | \hat{\mathcal{T}}_i = 0] \quad (11)$$

This assumption ensures that there were no other confounding shocks that might explain differences in consumption between treated and control groups. Note in this setting however, this assumption does not suffice for identification of the object of interest. In particular, (11) implies:

$$\beta^{ols} = \underbrace{E[\beta_i | \hat{\mathcal{T}}_i = 1]}_{\text{ATT}} + \underbrace{E[\zeta_i | \hat{\mathcal{T}}_i = 1] - E[\zeta_i | \hat{\mathcal{T}}_i = 0]}_{\text{Differential responses to G.E. effects of shock}} \quad (12)$$

The first term is exactly the object of interest and so the second term can be interpreted as generating bias. This bias term does not arise either when there are no spillovers arising from the treatment ( $\zeta_i = 0$ ) – when the Stable Unit Treatment Value Assumption (SUTVA) holds – or when these spillovers affect all units exactly the same ( $\zeta_i = \alpha$ ). While SUTVA may be a reasonable assumption in some settings – as with 'small-scale interventions' such as those studied in [Fagereng et al. \(2021\)](#) and [Boehm et al. \(2025\)](#) – it is clearly inappropriate in settings where researchers claim that the intervention under study generated potentially sizable GE effects.

Why would this bias term be problematic in practice? Consider a case where a researcher estimates a high- $\beta^{ols}$  coefficient. It is tempting to conclude that this implies the transfer shock was effective at directly stimulating household consumption (i.e. that recipient households had high-MPCs). But another interpretation of this finding is that the the transfer shock failed to directly stimulate consumption, but treatment and control groups responded very differ-

<sup>6</sup>In particular, requiring  $\tilde{\omega}_i \geq 0$  ensures that the resulting estimate does not suffer from a risk of 'sign reversals', an issue that has been discussed extensively in the microeconometrics literature on identification with heterogeneous treatment effects – see e.g. [de Chaisemartin and D'Haultfoeuille \(2023\)](#).

ently to the GE effects of the shock – e.g. via the income- or tax-channel.<sup>7</sup> A high- $\beta^{ols}$  could even be consistent with an alternate “neoclassical” model in which households consume only out of permanent (not temporary) changes in income – so that MPCs are all near-zero – and transfers are financed via distortionary (not lump-sum) taxes, which differentially affect treatment and control groups. This could occur, if for example, the control group were more sensitive to higher labour taxation (e.g. had a higher Frisch elasticity of labour supply).

How might researchers ensure that their empirical specification is robust to this concern? Suppose the transfers were targeted to individual households based on some vector of underlying characteristics  $a_i$  (e.g. household income, wealth etc.), where the “targeting rule” is such that:

$$E[\hat{\mathcal{T}}_i | a_i, \beta_i, \alpha_i^y, \alpha_i^T, u_i] = \gamma + \delta' a_i \quad (13)$$

And suppose the econometrician adds as controls to regression (10) the variables that appear in the “targeting rule”:

$$\hat{c}_i = \alpha^L + \beta^L \hat{\mathcal{T}}_i + \lambda' a_i + e_i^L \quad (14)$$

In this case, it can be shown that the OLS-coefficient captures a measure of average MPCs:<sup>8</sup>

$$\beta^L = E[\omega_i \beta_i] / E[\omega_i] \quad \forall i : \omega_i \geq 0 \quad (15)$$

Intuitively, the  $\beta^L$ -coefficient in the “long” regression (14) avoids the bias term in (12), since it compares across units who (on average) are equally responsive to the GE effects from the shock. In particular, under (13), after residualising with respect to  $x_i$ , the treatment variable  $\mathcal{T}_i$  is uncorrelated with agents’ responses to GE effects both via the income channel – captured by  $\alpha_i^y \beta_i$  – and via the tax channel –  $\alpha_i^T \beta_i$ . Appealingly, this does not require direct knowledge of the propagation of the transfer shock via GE channels – i.e. how much aggregate taxes and income actually adjusted to the shock, or how different agents responded to those changes.

**Discussion** Although this section focuses on a specific stylised model, the discussion around identification is somewhat general. The key takeaway is simply that in (linearised) settings with GE effects and heterogeneous agents, observed unit-level outcomes can typically be decomposed as in (7) – in which case bias can arise either if units’ treatment is correlated with the effects of some other extraneous shocks, or if it is correlated with their response to other units’ treatment. In either case, controlling for the targeting rule as in (14) suffices for iden-

<sup>7</sup>The discussion here is deliberately stylised. I present a formal example in Section 4 where I demonstrate that standard cross-sectional identification methods mistakenly imply that transfers to Ricardian households in a Two-Agent New Keynesian model have very large stimulative effects on household consumption.

<sup>8</sup>I omit this proof since this is essentially a simplified version of Proposition 5 in the following section.

tification. The next section is devoted to illustrating these issues for identification in a more general dynamic environment that relates directly to heterogeneous-agent DSGE models.

### 3 Identification Results

The section presents my key results on cross-sectional identification in macroeconomic settings. Section 3.1 introduces the environment, Section 3.2 discusses the object of interest and Section 3.3 contains the identification results. I relegate all proofs from this section to Appendix A.

#### 3.1 Environment

I begin with a general characterisation of a (linearised) DSGE model with agents who are heterogeneous across a range of (non-time-varying) characteristics. I consider an economy with a unit-continuum of agents  $i \in [0, 1]$  and an infinite horizon  $t = 0, 1, 2, \dots$ . Agents are heterogeneous across some  $n_x$ -dimensional vector of (non-time-varying) variables, denoted by  $x_i = [x_{1,i}, x_{2,i}, \dots, x_{n_x,i}]'$ . The economy is subject to  $n_\varepsilon$  independent, mean-zero, unit-variance stochastic shocks denoted by  $\varepsilon_t = [\varepsilon_{1,t}, \dots, \varepsilon_{n_\varepsilon,t}]'$ . I treat the first shock as a particular shock of interest which I refer to as a government stimulus shock – I label this as  $\varepsilon_{1,t} \equiv \varepsilon_t^g$  and collect remaining shocks in the  $(n_\varepsilon - 1)$ -dimensional vector  $\varepsilon_t^{-g}$ .

I assume there are  $n_Y$  aggregate (potentially observable) variables which I partition as  $Y_t = [y_t, g_t, w_t]'$  where  $y_t$  is the outcome variable of interest,  $g_t$  denotes the aggregate ‘treatment’ variable – some form of government stimulus such as government spending or transfers – and  $w_t$  is a  $(n_Y - 2)$ -dimensional vector collecting all other aggregate variables. I denote corresponding unit-level variables:  $Y_{i,t} = [y_{i,t}, g_{i,t}, w_{i,t}]'$ . I assume the unit-level outcome variable maps to the aggregate outcome variable in the standard way:  $y_t = \int_i y_{i,t} di$ .

I assume the treatment of interest is distributed arbitrarily across agents, with  $z_i$  denoting the “share”<sup>9</sup> of government stimulus going to each agent, with their own treatment denoted  $g_{i,t} = z_i g_t$ . Aggregate and unit-level variables are linear functions of all current and past shocks (as well as steady-state values):

$$Y_t = \sum_{l=0}^{\infty} \Theta_l \varepsilon_{t-l} + \bar{Y} \quad (16)$$

$$Y_{i,t} = \sum_{l=0}^{\infty} \Theta_{i,l} \varepsilon_{t-l} + \bar{Y}_i \quad (17)$$

where  $\Theta_l$  and  $\Theta_{i,l}$  are  $n_Y \times n_\varepsilon$ -dimensional matrices that map shocks to aggregate and

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<sup>9</sup>I do not restrict  $z_i$ 's to be literal shares i.e. being positive and integrating to one over the population.

unit-level outcomes respectively, and  $\bar{Y}$  and  $\bar{Y}_i$  are  $n_Y$ -dimensional vectors denote steady-state outcomes. It is well-known that the Structural Vector-Moving Average (SVMA) model (16) encompasses all discrete-time, linearized DSGE models, where (17) then extends this structure to the unit-level outcomes (as in [Almuzara and Sancibrián \(2024\)](#)). I focus on the dynamic causal effect of government stimulus shocks on each units' outcome  $y_{i,t}$ , which I assume admits the following “sequence-space” decomposition:

$$\Theta_{i,1}^y = \mathcal{M}_i^{yg} \Theta_1^g z_i + \mathcal{M}_i^{yw} \Theta_1^w \quad (18)$$

where  $\Theta_{i,1}^y = \{\Theta_{i,h,1}^y\}_{h=0}^\infty$ ,  $\Theta_1^g = \{\Theta_{h,1}^g\}_{h=0}^\infty$  and  $\Theta_1^w = \{\Theta_{h,1}^w\}_{h=0}^\infty$  are sequences collecting the dynamic causal effects of  $\varepsilon_t^g$  on  $y_{i,t}$ ,  $g_t$  and  $w_t$ , and  $\mathcal{M}_i^{yg}$  and  $\mathcal{M}_i^{yw}$  are conformable and map bounded sequences into bounded sequences, and depend only on underlying sources of agent-heterogeneity  $x_i$ .<sup>10</sup>

Decompositions of this form are common within macroeconomic models cast in the sequence-space to distinguish between the notion of “direct” (or “partial equilibrium”) and “indirect” (or “general equilibrium”) effects of shocks. In my setting, the first term can be understood as capturing the “direct effect” of the shock – since it captures unit  $i$ 's responses to the path of their own stimulus  $g_{i,t}$ , and the second term as the “GE effect” since it captures units' responses to other variables and is equal to the effect of the shock for units who have no direct exposure ( $z_i = 0$ ). Since decompositions of the form (18) arise naturally in linear settings, this imposes relatively minor additional structure on the transmission of the shock to each-unit – allowing agents to respond heterogeneously to a range of transmission channels in a forward-looking way.

As well as the standard assumption that shocks are mutually independent and i.i.d. over time, I assume they are independent of unit-level variables:

$$\forall i \forall t : \varepsilon_t \perp (z_i, x_i, \bar{y}_i) \quad (19)$$

where this is a natural assumption given the unit-level variables are non-time-varying.

### 3.2 Object of Interest

The goal is to study the direct effect of government stimulus on each agent (i.e. holding fixed treatment for all other agents). To ease notation, I focus on the effect at the  $h$ 'th horizon,  $[\Theta_{i,h,1}^y]$ , and following (18), decompose this as:

<sup>10</sup>Formally, I impose that the mappings are measurable with respect to  $x_i$  in that, for any bounded sequence  $v$ ,  $E[\mathcal{M}_i v | x_i] = \mathcal{M}_i v$ .

$$\Theta_{i,h,1}^y = \underbrace{\beta_{i,h}^y}_{\text{Direct Effect}} z_i + \underbrace{\zeta_{i,h}^y}_{\text{GE Effect}} \quad (20)$$

where  $\beta_{i,h}^y$  and  $\zeta_{i,h}^y$  correspond to the  $h$ 'th term of the sequences  $\mathcal{M}_i^{yg} \Theta_1^g$  and  $\mathcal{M}_i^{yw} \Theta_1^w$  respectively. My object of interest is then simply some weighted average of my notion of direct effects – i.e.  $E[\omega_i \beta_{i,h}^y] / E[\omega_i]$  for some non-negative weights  $\omega_i$ . To tighten the connection between  $\beta_{i,h}$  and typical objects of interest in macroeconomic models, it is helpful to consider various examples. <sup>11</sup>

**Transfers to Households** Consider government transfers  $g_{i,t}$  to households in a setting where household consumption is determined according to:  $c_{i,t} = c_i(\mathbf{g}_{i,t}; \mathbf{w}_t)$  where  $w_t$  collects aggregate variables that the household treats as given (e.g. the real interest rate, taxes etc.) and boldface denotes sequences:  $\mathbf{x}_{i,t} = \{x_{i,s}\}_{s=t}^{\infty}$  for all variables- $x$ . To first-order, given  $g_{i,t} = z_i g_t$ , the response of household consumption to a shock to transfers  $\varepsilon_t^g$  is given by:

$$\Theta_{i,1}^y = \underbrace{\left[ \frac{\partial c_i}{\partial \mathbf{g}_{i,t}} \tilde{\mathbf{g}}_t \right]}_{\text{Direct Effect}} z_i + \underbrace{\frac{\partial c_i}{\partial \mathbf{w}_t} \tilde{\mathbf{w}}_t}_{\text{GE Effect}}$$

where  $\tilde{\mathbf{w}}_t$  denotes the effect of the shock on paths for aggregate variables  $\mathbf{x}_t$ . In models with atomistic households, the second-term does not depend on  $z_i$ , and so the first term captures my object of interest. This corresponds exactly to the notion of households' intertemporal marginal propensities to consume (Auclert et al., 2024): how households adjust their dynamic consumption path over time in response to news about a dynamic transfer path (holding fixed any general-equilibrium objects).

**Government Spending in Regions** Consider government purchases  $g_{i,t}$  across regions in settings where output in each region is determined according to:  $\mathbf{y}_{i,t} = \mathbf{y}_i(\mathbf{g}_{i,t}, \mathbf{p}_{i,t}; \mathbf{w}_t)$  where  $\mathbf{p}_{i,t}$  captures the price of local output in region- $i$ , where  $w_t$  collects aggregate variables (e.g. interest rates and taxes) and boldface again denotes sequences. In this case, the response of regional output to a shock to government purchases  $\varepsilon_t^g$  is given by:

$$\Theta_{i,1}^y = \left[ \frac{\partial \mathbf{y}_i}{\partial \mathbf{g}_{i,t}} \tilde{\mathbf{g}}_t \right] z_i + \frac{\partial \mathbf{y}_i}{\partial \mathbf{p}_{i,t}} \tilde{\mathbf{p}}_{i,t} + \frac{\partial \mathbf{y}_i}{\partial \mathbf{w}_t} \tilde{\mathbf{w}}_t$$

As before, with atomistic regions, the final term typically does not depend on  $z_i$ . But in

<sup>11</sup>Note that my notion of “direct effects” differs from Wolf (2023) who uses this terminology exclusively for the partial equilibrium effects of interventions holding *all* prices fixed. In this paper, I’m only concerned with recovering an estimate that is interpretable as the effect of *own*-treatment holding fixed treatment for all other units. As I discuss below, this corresponds directly to typical objects of interest in studies that employ cross-sectional data – e.g. household marginal propensities to consume, firm-level responses to tax cuts/subsidies, regional fiscal multipliers.

regional models the response of prices in each region typically depends on region-specific shocks. Assuming this term can be further decomposed into the adjustment that occurs as a result of the increase in spending in region- $i$  vs spending in “foreign” regions  $g_{i,t}^F$ , we have:

$$\Theta_{i,1}^y = \underbrace{\left[ \frac{\partial y_i}{\partial g_{i,t}} \tilde{g}_t + \frac{\partial y_i}{\partial p_{i,t}} \frac{\partial p_{i,t}}{\partial g_{i,t}} \tilde{g}_t \right]}_{\text{Direct Effect}} z_i + \underbrace{\left[ \frac{\partial y_i}{\partial p_{i,t}} \frac{\partial p_{i,t}}{\partial g_{i,t}^F} \tilde{g}_{i,t}^F + \frac{\partial y_i}{\partial w_t} \tilde{w}_t \right]}_{\text{GE Effect}}$$

And so, in this case, my notion of the direct effect includes the adjustment of local prices that occurs as a result of the shock – i.e. the conventional notion of a local fiscal multiplier.<sup>12</sup>

**Subsidies in a Production Network** Consider government production subsidies  $g_{i,t}$  across firms in settings with  $k$  industries that form a production network, where the output of each firm is determined according to:  $y_{i,t} = \mathbf{y}_i(\mathbf{g}_{i,t}; \mathbf{p}_t)$  where  $p_t = [p_{1,t}, \dots, p_{k,t}]$  is a vector of prices such that  $p_{l,t}$  denotes the price of the the good from industry- $l$ , where this is constructed as an appropriate aggregator of prices from each individual firm in that industry. The response of each firm’s output to a subsidy shock  $\varepsilon_t^g$  is given by:

$$\Theta_{i,1}^y = \underbrace{\left[ \frac{\partial y_i}{\partial g_{i,t}} \tilde{g}_t \right]}_{\text{Direct Effect}} z_i + \underbrace{\left[ \frac{\partial y_i}{\partial p_{l,t}} \tilde{p}_{l,t} \right]}_{\text{GE Effect}}$$

With atomistic firms, the second term again does not depend on  $z_i$  and so the first term captures my notion of direct effects – the response of output for each firm to a government subsidy (holding industry prices fixed).

### 3.3 Identification

My focus is on identification via shift-share panel regressions.

**Proposition 1** Consider shift-share regressions of the form:

$$y_{i,t+h} = \beta^{ss}(s_i \cdot d_t) + e_{i,t} \quad (21)$$

where  $\forall i \forall t : s_i \perp \varepsilon_t$  and  $d_t = \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l}$  for some  $1 \times n_\varepsilon$ -length vector  $\Psi_l$ . Then:

$$\beta^{ss} = \frac{E \left[ \left( \sum_{l=0}^{\infty} \Theta_{i,h+l}^y \eta_l \right) s_i \right]}{E[s_i^2]}$$

<sup>12</sup>The fact that regional regressions typically include the effect of any adjustment in local prices is discussed extensively in e.g. Guren et al. (2021) and Wolf (2019). If researchers are concerned with recovering a “pure” partial equilibrium effect in settings with regional data then additional steps are typically required and discussed in both Guren et al. (2021) and Wolf (2019). My results are still useful in this case since they clarify how to achieve identification in the initial regression (i.e. the first step for the application of the methods proposed in these other papers).

where  $\eta_l$  is a  $n_\varepsilon \times 1$ -length vector with  $k$ 'th element equal to:  $\eta_{l,k} = \frac{\psi_{l,k}}{\sum_{k=1}^{n_\varepsilon} \sum_{l=0}^{\infty} \psi_{l,k}^2}$ .

**Proof** See Appendix A.1

The proposition provides a formula for the shift-share coefficient for an arbitrary choice of “shares”  $s_i$  and arbitrary “shifter”  $d_t$ . This result follows closely the characterisation of the population estimand from shift-share regressions in a similar environment in [Almuzara and Sancibrián \(2024\)](#) but extended to allow for an arbitrary choice of shifter. The only requirement on the shifter  $d_t$  is that it reflect a linear combination of contemporaneous and past shocks – equivalent in this environment to allowing the shifter to be any observable aggregate variable.<sup>13</sup>

The result serves to demonstrate that the shift-share coefficient can be constructed via a two-step procedure. First, one recovers the unit-level impulse responses to the underlying structural shocks that underpin the shifter  $d_t$  and “weights” these up according to the contribution of each shock in  $d_t$ . Note this combination of impulse responses functions reflect the same linear combination of IRFs that one would recover from (time-series) projections of any of the aggregate or unit-level outcome variables on  $d_t$  – see Corollary 1, Appendix A.2. The shift-share coefficient is then constructed by regressing these weighted unit-level impulse responses on the chosen shares  $s_i$ . Note that for binary shares,  $s_i \in \{0, 1\}$ , and a single structural shock,  $d_t = \varepsilon_{k,t}$ , then so long as the shift-share regression includes time fixed effects, the resulting estimate simply takes the difference of impulse responses between treatment and control groups:

$$\beta^{ss} = E[\Theta_{i,h,k}^y | z_i = 1] - E[\Theta_{i,h,k}^y | z_i = 0]$$

I now consider routes to identification. The previous literature has stressed two distinct routes for shift-share identification – either via exogenous shares ([Goldsmith-Pinkham et al., 2020](#)) or exogenous shocks ([Adao et al., 2019](#); [Borusyak et al., 2022b](#)). Within my environment, these two distinct routes can be summarised as follows:

**Condition 1 (Identification From Shocks)**  $d_t = \varepsilon_t^g$

**Condition 2 (Identification From Shares)**  $z_i \perp x_i$

The first condition assumes the econometrician has direct access to a series of government stimulus shocks. Since these are independent of all other structural shocks, this mimics an experimental ideal where the researcher is able to directly observe a series of essentially random

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<sup>13</sup>One small caveat to this is that I restrict  $d_t$  to be zero-mean in Proposition 1. I do so for expositional purposes although it is immediate that a version of Proposition 1 goes through allowing  $d_t$  to be non-zero mean so long as unit fixed effects are included.

adjustments in government spending at the national level over time. The second condition instead assumes that the exposure of each unit to changes in government stimulus  $z_i$  is independent of any unit-characteristics  $x_i$ . This mimics an alternate experimental ideal where the exposures  $z_i$  are essentially randomly assigned across the population. I now consider the validity of each of these routes within my environment.

### 3.3.1 Identification From Shocks

I start by considering identification when the researcher has direct access to a series of government spending shocks (i.e. Condition 1 holds). The following Proposition clarifies that this is generally not sufficient to achieve identification of the object of interest:

**Proposition 2** Consider a setting where  $z_i \in \{0, 1\}$ . Consider shift-share regressions of the form:

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + e_{i,t} \quad (22)$$

Then the shift-share coefficient  $\beta^{ss}$  can be expressed as:

$$\beta^{ss} = \underbrace{E[\beta_{i,h}^y | z_i = 1]}_{\text{ATT}} + \underbrace{E[\zeta_{i,h}^y | z_i = 1] - E[\zeta_{i,h}^y | z_i = 0]}_{\text{Bias from G.E. responses}}$$

**Proof** See Appendix [A.3](#)

Following the discussion of Proposition 1, in this case the shift-share coefficient is constructed by differencing impulse responses for treatment and control groups. However, absent any restriction on  $z_i$ , the resulting coefficient is contaminated by the differential responses of treatment and control groups to the GE effects of the shock. Previous macroeconomics literature has derived a similar bias term for shift-share regressions with identified shocks in particular settings with heterogeneous agents (see e.g. [Koby and Wolf, 2020](#); [Donaldson, 2026](#)) – where the above result extends this to my general environment. As I come on to demonstrate in the next section, the bias in macroeconomic settings can be severe, dramatically altering conclusions around the effectiveness of particular government stimulus policies.

The bias from Proposition 2 arises because agents respond heterogeneously to aggregate variables  $w_t$  that adjust in response to the shock. A natural correction might therefore be to allow agents to load differentially on these aggregate variables  $w_t$  by including additional interaction terms in the regression.<sup>14</sup> The following clarifies that this is not in general sufficient for identification:

<sup>14</sup>This approach to dealing with this concern is common in applied work – see e.g. [Nakamura and Steinsson \(2014\)](#), [Pennings \(2021\)](#), [Chodorow-Reich et al. \(2021\)](#) and [Fukui et al. \(2025\)](#). [Donaldson \(2026\)](#) refers to regressions of the form (23) as the “control function” approach and similarly demonstrates bias within their environment.

**Proposition 3** Consider a setting where  $z_i \in \{0, 1\}$ . Consider shift-share regressions of the form:

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + \lambda'(z_i \cdot w_t) + e_{i,t} \quad (23)$$

Then the shift-share coefficient  $\beta^{ss}$  has the form:

$$\begin{aligned} \beta^{ss} = & \underbrace{\lambda_{0,1} E[\beta_{i,h}^y | z_i = 1]}_{\text{ATT}} + \underbrace{\lambda_{0,1} \left( E[\zeta_{i,h}^y | z_i = 1] - E[\zeta_{i,h}^y | z_i = 0] \right)}_{\text{Bias from G.E. responses}} \\ & + \underbrace{\sum_{k=2}^{n_\varepsilon} \lambda_{0,k} \left( E[\Theta_{i,0,k}^y | z_i = 1] - E[\Theta_{i,0,k}^y | z_i = 0] \right)}_{\text{Bias from other contemp. shocks}} + \underbrace{\sum_{l=0}^{\infty} \sum_{k=1}^{n_\varepsilon} \lambda_{l,k} \left( E[\Theta_{i,h,k}^y | z_i = 1] - E[\Theta_{i,h,k}^y | z_i = 0] \right)}_{\text{Bias from other lagged shocks}} \end{aligned}$$

where the  $\lambda$ -loadings are given by:

$$\lambda_{k,l} = \frac{\psi_{k,l}}{\sum_{k=1}^{n_\varepsilon} \sum_{l=0}^{\infty} \psi_{k,l}^2}, \quad \psi_{0,1} = 1 - b' \Theta_{0,1}^w, \quad \forall (l,k) \neq (0,1) : \psi_{l,k} = b' \Theta_{l,k}^w$$

and:

$$b' = -(\Theta_{0,1}^w)' \Sigma_w^{-1}, \quad \Sigma_w = \sum_{l=0}^{\infty} \Theta_l^w (\Theta_l^w)'$$

**Proof** See Appendix A.4

Proposition 3 characterises the bias in this case purely in terms of the (macro) and (micro) structural impulse responses  $\Theta_l^w$  and  $\Theta_{i,l}^y$ . By Frisch-Waugh-Lovell, the shift-share coefficient in this case is constructed by first orthogonalising the shock with respect to  $w_t$ , and then using this orthogonalised shock  $(\varepsilon_t^g)^\perp w_t$  as the shifter. Since  $w_t$  generally reflects an outcome variable of the shock  $\varepsilon_t^g$ , the additional bias terms can be understood as stemming from the “bad control” problem, where conditioning on outcome variables introduces bias (even when the treatment is randomly assigned). In particular, orthogonalising  $\varepsilon_t^g$  with respect to  $w_t$  recovers a linear combination of all shocks up to time- $t$ , and so the resulting shift-share coefficient now additionally captures the differential response of treatment and control groups to all (past and contemporaneous) shocks, where the  $\lambda$ -loading on each term depends on the particular combination of shocks captured by  $(\varepsilon_t^g)^\perp w_t$ . It is of course possible that these additional bias terms offset each other, although there is no reason for this to hold in general. I demonstrate this with a quantitative example in later sections where I show the bias from this approach can be severe.

One way to understand the bias here is that the controls fail to appropriately “hold fixed” the relevant GE variables that drive heterogeneous responses across units. Returning to the sequence-space decomposition (26), note that the responses to GE variables in this environ-

ment are captured by units' responses to dynamic *paths* for aggregate variables triggered by the shock – i.e. by units' responses to  $\Theta_k^w$  – which in general is not captured by the additional control variables in (23). Indeed, it follows from Proposition 1 that researchers can test directly whether their controls successfully recover a linear combination of shocks that serves to “hold fixed” these GE variables via (time-series) regressions of the form:

$$w_{t+h} = \beta_h^w(\varepsilon_t^g) + \delta'w_t + u_{t+h}$$

where generically, although  $\beta_h^w = 0$  by construction, we have:  $\beta_h^w \neq 0$  for  $h > 0$ , and even  $\beta_h^w \neq 0$  for  $h < 0$  – i.e. the researcher will additionally now be capturing differential responses to GE variables that occur both after *and* before time- $t$ . One way to remove the bias term from Proposition 2 is to follow the logic of the solution proposed in Donaldson (2026) and instead project on a linear combination of time- $t$  shocks that produce opposite paths for GE variables than  $\varepsilon_t^g$  – i.e. projecting on a set of time- $t$  shocks that effectively hold fixed the entire dynamic path for  $w_t$ . Clearly, orthogonalising  $\varepsilon_t^g$  with respect to  $w_t$  does not in general recover the necessary combination of time- $t$  shocks to implement this strategy.

I now consider an alternate strategy to directly control for units' differential responses to GE variables. To investigate this, I consider a situation where the researcher has direct access to the differential GE responses of agents to the shock – i.e. to  $\zeta_{i,h}^y$  in equation (20). The informational requirements in this case are exceptionally high – since  $\zeta_{i,h}^y$  is the  $h$ 'th element of the sequence  $\mathcal{M}_i^{y,w} \Theta_k^w$  from equation (26), it requires the researcher to have knowledge both of how GE variables adjusted to the shock (captured by  $\Theta_k^w$ ), and how units responded differentially to such changes (captured by  $\mathcal{M}_i^{y,w}$ ). Nevertheless, one might imagine a situation where these heterogeneous spillover terms take on a simple form – e.g. with spillovers only operating between “groups” or “regions” of units or via some known “network” structure – such that it is possible to construct a plausible set of controls that capture this. This speaks to a large literature that seeks to identify the direct effect of shocks on each unit alongside identifying heterogeneous spillover effects (see e.g. Huber, 2022). Somewhat surprisingly, I now show that even with this knowledge, controlling for  $\zeta_{i,h}^y$  does not recover the object of interest:

**Proposition 4** Consider shift-share regressions of the form:

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + \lambda(\zeta_{i,h}^y \cdot \varepsilon_t^g) + e_{i,t} \quad (24)$$

Then the shift-share coefficient  $\beta^{ss}$  has the form:

$$\beta^{ss} = \frac{E[\beta_{i,h}^y \omega_i]}{E[\omega_i]}$$

where  $\omega_i = \tilde{z}_i z_i$  and  $\tilde{z}_i$  is the (population)-residual from a regression of  $z_i$  on a constant and  $\zeta_{i,h}^y$ .

**Proof** See Appendix A.5

In this case the bias term is removed, but the weights are given by:  $\omega_i = \tilde{z}_i z_i$  – which can in general take on negative values. Hence the shift-share coefficient does not have an interpretation as capturing some (properly) weighted average of the underlying direct effects. In particular, the estimate has the potential to suffer from “sign reversals” – a negative value for  $\beta_{ss}$  even when the underlying direct effects of the shock are all positive. The result follows from Proposition 1, combined with results in [Borusyak and Hull \(2024\)](#) around negative weights with “model-based” controls. Problems associated with negative weights have been discussed extensively within the difference-in-differences literature in the context of two-way fixed-effect estimation – the above result then highlights that this problem also arises in settings where researchers attempt to control for heterogeneous spillover effects (while leaving unmodeled any heterogeneity in units’ responses to their *own* treatment). While it is possible that the issue of negative weights from regressions of the form (24) may be avoided by using alternate (non-OLS) estimators, since this route to identification necessarily requires exceptionally high informational requirements (requiring knowledge of  $\zeta_{i,h}$ ), I instead go on to consider alternate routes to identification.

### 3.3.2 Identification From Shares

I now consider whether identification can be achieved via the exogeneity of shares  $z_i$ . It is immediate that, with exogenous shares (i.e. assuming Condition 2 holds), then regression (22) no longer suffers from bias since, when the shares are orthogonal to unit characteristics, we have:

$$E[\zeta_{i,h}^y | z_i = 1] - E[\zeta_{i,h}^y | z_i = 0] = 0$$

Condition 2 is strong however and so I consider a weakening that permits identification via control variables even when  $z_i$  is correlated with  $x_i$ . Letting  $a_i$  denote some observable  $n_a$ -vector of control variables that is independent of the shocks  $\varepsilon_t$  (jointly with the other unit-level variables),<sup>15</sup> consider:

**Condition 3 (Identification From Shares With Controls)**  $E[z_i | x_i, a_i] = \alpha + \delta' a_i$

In this case, controlling for  $a_i$  interacted with the shock of interest is sufficient for identification:

**Proposition 5** *Suppose Condition 3 holds. Consider shift-share regressions of the form:*

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + \lambda'(a_i \cdot \varepsilon_t^g) + e_{i,t} \quad (25)$$

<sup>15</sup>Formally, I assume:  $\forall i \forall t : \varepsilon_t \perp (z_i, x_i, y_i, a_i)$ . This ensures that Proposition 1 still applies.

Then:

$$\beta^{ss} = \frac{E[\beta_{i,h}^y \omega_i]}{E[\omega_i]}$$

where  $\omega_i = E[\tilde{z}_i^2 | x_i, a_i]$  and  $\tilde{z}_i$  is the (population)-residual from a regression of  $z_i$  on a constant and  $a_i$ .

**Proof** See Appendix A.6

The controls here serve to remove the bias term while ensuring the weights  $\omega_i$  are guaranteed to be non-negative. As above, the result follows from Proposition 1, combined with results in [Borusyak and Hull \(2024\)](#) around the avoidance of negative weights with “design-based” controls. While it is well-known in general that exogeneity of the shares is a possible route to identification for shift-share regressions ([Goldsmith-Pinkham et al., 2020](#)), Proposition 5 demonstrates that this approach remains robust even in an environment with heterogeneous and dynamic treatment effects, forward-looking agents and numerous (and unknown) GE channels of transmission. The identifying assumptions are strong in that it requires researchers to model correctly the assignment of the shares  $z_i$  and to include controls that capture this. Specifically the controls must span any dimensions of unit-heterogeneity  $x_i$  that determine  $z_i$ . Nevertheless, identification can be achieved in this case without any knowledge of the underlying GE channels underpinning the transmission of the shock, and the resulting regression is simple to implement. In practice, in order to implement this procedure, researchers must first take a stance on the unit-level characteristics that are likely to drive heterogeneous responses to GE effects in their setting –  $x_i$  in my framework – and then, to the extent that the exposure-shares  $z_i$  are likely to be correlated with  $x_i$ , to control directly for  $x_i$  in the shift-share regression (or argue that they have some control variables  $a_i$  that effectively ‘span’  $x_i$ ). I demonstrate how to implement this procedure explicitly with examples via a quantitative model and an empirical application in later sections.

Proposition 5 continues to rely on the researcher having access to a single well-identified shock  $\varepsilon_t^g$  (i.e. that Condition 1 holds). This is a strong assumption in practice since it requires the researcher to have direct access to a series of essentially random adjustments in government stimulus at the national level over time. I now show this assumption can be meaningfully weakened. To that end first consider a weaker version of Condition 1:

**Condition 4 (No Anticipation)**  $d_t = q' \varepsilon_t$  for  $q \in R^{n_\varepsilon \times 1}$

Note that this condition ensures that the shifter  $d_t$  is a function of only contemporaneous shocks. Within this environment, that allows the shifter to be essentially any (endogenous) variable so long as it is not predictable prior to time- $t$  – hence “no anticipation”. I now show that this alternate condition is sufficient to achieve identification of an object closely related to the object of interest discussed thus far:

**Proposition 6** Assume Condition 3 and 4 hold. Suppose in addition to (18), the effect of all shocks can be decomposed as:

$$\forall k \in (1, \dots, n_\varepsilon) : \Theta_{i,k}^y = \mathcal{M}_i^{yg} \Theta_k^g z_i + \mathcal{M}_i^{yw} \Theta_k^w \quad (26)$$

Consider the pair of shift-share regressions of the form:

$$y_{i,t+h} = \alpha_{i,y} + \delta_{t,y} + \beta_{y,h}^{ss} (z_i \cdot d_t) + \lambda'(a_i \cdot d_t) + e_{i,t} \quad (27)$$

$$g_{i,t+h} = \alpha_{i,g} + \delta_{t,g} + \beta_{g,h}^{ss} (z_i \cdot d_t) + \lambda'(a_i \cdot d_t) + u_{i,t} \quad (28)$$

Then:

$$\beta_y^{ss} = \frac{E[\mathcal{M}_i^{yg} \beta_g^{ss} \omega_i]}{E[\omega_i]}$$

for  $\beta_y^{ss} = \{\beta_{y,h}^{ss}\}_{h=0}^\infty$  and  $\beta_g^{ss} = \{\beta_{g,h}^{ss}\}_{h=0}^\infty$ , where  $\omega_i = E[\tilde{z}_i^2 | x_i, a_i]$  and  $\tilde{z}_i$  is the (population)-residual from a regression of  $z_i$  on a constant and  $a_i$ .

**Proof** See Appendix A.7

This result demonstrates that the path traced out by the shift-share coefficient  $\beta_{y,h}^{ss}$  can be interpreted as a convex-weighted average of units' responses to a dynamic treatment path for  $g_t$  (holding fixed the path for any GE-objects  $w_t$ ), where that dynamic treatment path is traced out by the shift-share coefficient  $\beta_g^{ss}$ . Returning to a previous example, if  $y_{i,t+h}$  denotes household consumption, and  $g_{i,t+h}$  denotes government transfers, then the proposition demonstrates that the shift-share coefficient captures some weighted average of household intertemporal MPCs (iMPCs) – i.e. how consumption responds dynamically to a dynamic transfer path traced out by  $\beta_g^{ss}$ .

The result relies on extending the sequence-space decomposition (18) to *all* shocks. The substantive assumption is that the causal mappings  $\mathcal{M}_i$  do not depend on the source of the shocks (i.e. they do not vary by  $k$ ). This is closely related to the notion of “instrument sufficiency” in the macroeconomics literature on the mapping between policy shocks and policy rules (Bar-nichon and Mesters, 2023; McKay and Wolf, 2023). As discussed in McKay and Wolf (2023), a broad class of macroeconomic models satisfy instrument sufficiency since agent optimisation problems depend only on (expected) paths for policy variables (and not on the underlying source of the shock). Similarly, equation (26) imposes that individual units respond to changes in expected paths for  $g_{i,t}$  and  $w_t$  in the same way regardless of the underlying source of the shock (i.e. whether it was a government stimulus shock per se, or an endogenous response of government stimulus to some other shock).

This result then formalises exactly the usefulness of employing cross-sectional data in

macroeconomic settings. In particular, it clarifies how identification of the ‘direct’ effects of some path for government stimulus can be achieved, even when the researcher lacks a well-identified measure of a stimulus shock – i.e. when identification via time-series methods with aggregate data is not possible. Note further that, in this case, the researcher can recover IRFs for the outcome variable of interest to different treatment paths for  $g_t$  by varying the aggregate shifter  $d_t$  (so long as it continues to satisfy the “no anticipation” assumption). This is potentially a fruitful avenue of exploration for applied work. As [Auclert et al. \(2024\)](#) demonstrate, the full iMPC matrix  $\mathcal{M}_i^{yg}$  is a crucial object for heterogeneous-agent models. Applied work is typically only informative about particular entries however – e.g. household responses over time to an unanticipated and contemporaneous rise in transfers (i.e. the first column of the iMPC matrix). Proposition 6 clarifies that, if the researcher is able to find units who are differentially exposed to shifts in government transfers for exogenous reasons (conditional on controls  $a_i$ ), then their responses to different macroeconomic shocks could in theory be used to identify the full iMPC matrix  $\mathcal{M}_i^{yg}$ . I provide a simple illustration of this point via a Two-Agent New Keynesian model in the next section.

### 3.3.3 Extensions

I now discuss extensions to the baseline framework. I relegate all details to the Appendix and provide only a high-level overview of my key results.

**Cross-sectional data** My main results focus on identification via panel shift-share regressions. Many studies in macroeconomics employ purely cross-sectional data, and seek to identify the direct effects of a single, one-off shock ([Mian and Sufi, 2012](#); [Parker et al., 2013](#); [Zwick and Mahon, 2017](#)). In Appendix B, I show that my results extend naturally to this setting, where I focus on cross-sectional difference-in-differences regressions. I demonstrate that identification can be achieved so long as unit-level exposure to the shock under study is uncorrelated with any characteristics that determine heterogeneous responses to GE variables. As discussed in the simple setting from Section 2, the exogeneity requirements are conceptually distinct, and generally somewhat stronger, than the canonical parallel trends assumptions employed in the difference-in-differences literature.

**Finite-n** I also consider settings with a finite-n population of agents, where spillovers occur directly between units. This speaks to a general class of (linearised) “network” models, which do not directly map to the main environment from Section 3.1. In Appendix C, I show that, in this environment, a similar bias arises as in Proposition 1, and that a similar correction can be employed, following Proposition 5. I supplement these results in Appendix E with an additional quantitative model, in which a collection of finite units (e.g. US states) trade directly

with each other and are subject to a set of government spending shocks. In this model, estimates of the local fiscal multiplier are biased when government spending shocks are regionally clustered and trade follows a “gravity” equation. The bias arises since units with high levels of government spending are simultaneously more exposed to higher government spending in neighbouring regions via increased demand for exports than the control group. A simple correction in this case is to control for the underlying treatment assignment that generated such clustering, where this can be as simple as including region fixed-effects. This discussion relates to my empirical application in Section 6.

## 4 Quantitative Example

To demonstrate my key results in a familiar setting, I consider the use of shift-share regressions to recover an estimate of household consumption responses to transfers in a Two-Agent New Keynesian (TANK) model. I relegate all details of the model to Appendix D – I provide a high-level overview of the model in Section 4.1 and then discuss identification in Section 4.2.

### 4.1 Setting

**Households** The model is populated by a unit mass of households  $i \in [0, 1]$  with preferences over consumption  $c_{i,t}$  and labour  $n_{i,t}$ . Households receive income from their labour via the real wage  $w_t$ , alongside real dividend income  $d_t$ , real taxes  $\tau_t$  and transfers  $\mathcal{T}_{i,t} = z_i \bar{\mathcal{T}}_t$ . A fraction  $\alpha \in [0, 1]$  of households are unconstrained ( $U$ ) and can choose to hold bonds  $b_t$  with (ex-post) real return  $r_t$ , while the remaining fraction are hand-to-mouth ( $H$ ), unable to borrow or save. Each unconstrained household  $i \in [0, \alpha]$  solves a familiar dynamic consumption-savings problem:

$$\begin{aligned} \max_{c_{i,t}, n_{i,t}, b_{i,t}} E_0 \left[ \sum_{t=0}^{\infty} \beta^t \frac{(c_{i,t})^{1-\sigma}}{1-\sigma} - \theta \frac{(n_{i,t})^{1+\chi}}{1+\chi} \right] \\ \text{s.t. } c_{i,t} + b_{i,t} \leq w_t n_{i,t} + d_t - \tau_t + r_t b_{u,t-1} + \mathcal{T}_{i,t} \end{aligned} \quad (29)$$

Each hand-to-mouth household  $i \in [\alpha, 1]$  solves the static problem:

$$\begin{aligned} \max_{c_{i,t}, n_{i,t}} \left[ \frac{(c_{i,t})^{1-\sigma}}{1-\sigma} - \theta \frac{(n_{i,t})^{1+\chi}}{1+\chi} \right] \\ \text{s.t. } c_{i,t} \leq w_t n_{i,t} - \tau_t + \mathcal{T}_{i,t} \end{aligned} \quad (30)$$

**Firms** Production is identical to the standard New-Keynesian model with a representative competitive final goods firm alongside monopolistically competitive intermediate goods producers, the latter facing sticky prices.

**Policy** Monetary policy is set according to a Taylor Rule. The government’s budget constraint is:

$$G_t + \mathcal{T}_t \int_i z_i di = \tau_t \quad (31)$$

In the baseline model, the level of government spending  $G_t$  and aggregate transfers  $\mathcal{T}_t$  follow exogenous AR(1) processes and so taxes  $\tau_t$  are set to balance the budget each period.

**Shocks** Alongside transfer shocks  $\varepsilon_t^{\mathcal{T}}$ , the economy is also subject to shocks to government spending  $\varepsilon_t^G$ , monetary policy  $\varepsilon_t^m$  and technology  $\varepsilon_t^A$ .

**Shares** I define the binary variable:

$$HTM_i = \mathcal{I}(i \in [\alpha, 1])$$

where  $\mathcal{I}(\cdot)$  denotes the indicator function. I consider binary exposures to the transfer  $z_i \in \{0, 1\}$  where the probability of treatment varies with hand-to-mouth status:

$$Pr(z_i = 1 | HTM_i = 0) = \delta_1 \text{ and } Pr(z_i = 1 | HTM_i = 1) = \delta_2$$

For simplicity, I restrict  $\delta_1$  to be binary:  $\delta_1 \in \{0, 1\}$ , and allow  $\delta_2$  to take on values in the range  $[0, 1]$ . In this case the model essentially reduces to a “Three”-Agent model consisting of: Ricardian households (who either receive the transfer or not); (ii) “Treated” hand-to-mouth households (who receive the transfer); (iii) “Control” hand-to-mouth households (who do not receive the transfer), where the relative size of groups (ii) and (iii) depends on the choice of  $\delta_2$ . This keeps the model tractable while allowing me to demonstrate the full range of results from the previous section.

## 4.2 Identification

I consider an econometrician who seeks to recover an estimate of households’ marginal propensities to consume out of government transfers. Given the household problems (29) and (30), individual household consumption functions – that determines each household’s consumption choices over time – can be written as a function of sequences in:  $r_t, w_t, d_t, \tau_t, \mathcal{T}_{i,t}$ . That is, for all  $i, t$ , we have:

$$c_{i,t} = c_i(\mathcal{T}_{i,t}, w_t, d_t, \tau_t)$$

And so, to first-order, the effect of government transfer shocks on household consumption can be decomposed as:

$$\frac{d\mathbf{c}_{i,t}}{d\varepsilon_t^g} = \left[ \mathcal{M}_i^{cT} \tilde{\mathcal{T}}_t \right] z_i + \mathcal{M}_i^{cr} \tilde{\mathbf{r}}_t + \mathcal{M}_i^{cd} \tilde{\mathbf{d}}_t + \mathcal{M}_i^{c\tau} \tilde{\boldsymbol{\tau}}_t + \mathcal{M}_i^{cw} \tilde{\boldsymbol{w}}_t$$

where  $\mathcal{M}_i^{cx}$  denotes  $\frac{\partial \mathbf{c}_i}{\partial \mathbf{x}_t}$  and  $\tilde{\mathbf{x}}_t$  denotes  $\frac{\partial \mathbf{x}_t}{\partial \varepsilon_t}$ . My interest is in the first term – which here captures household intertemporal marginal propensities to consume (iMPC) – which I label:

$$\boldsymbol{\beta}_i = \mathcal{M}_i^{cT} \tilde{\mathcal{T}}_t$$

Since this varies only depending on whether households are hand-to-mouth or not, I define:

$$\mathcal{M}_U^{cT} = \forall i \in [0, \alpha] : \mathcal{M}_i^{cT} \text{ and } \mathcal{M}_{HTM}^{cT} = \forall i \in [\alpha, 1] : \mathcal{M}_i^{cT} \quad (32)$$

$$\boldsymbol{\beta}_U = \{\forall i \in [0, \alpha] : \beta_{i,s}\}_{s=0}^\infty \text{ and } \boldsymbol{\beta}_{HTM} = \{\forall i \in [\alpha, 1] : \beta_{i,s}\}_{s=0}^\infty \quad (33)$$

as the values of  $\mathcal{M}_i^{cT}$  and  $\boldsymbol{\beta}_i$  for Ricardian and hand-to-mouth households respectively. As in the previous section I consider different routes to identification.

#### 4.2.1 Identification With Exogenous Shocks

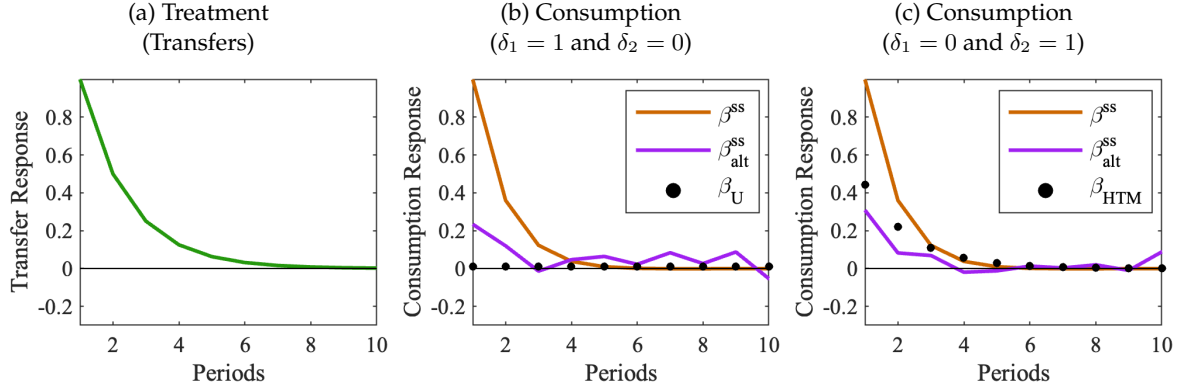
I assume the econometrician has access to the true transfer shocks  $\varepsilon_t^g$ , and begin by considering shift-share regressions of the form:

$$c_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + e_{i,t} \quad (34)$$

Figure 1 shows my results for different values of  $\delta_1$  and  $\delta_2$ . Figure 1a plots the path for transfers following the shock. Figures 1b and 1c then compare the shift share coefficients, shown in the orange lines, to the true iMPC of the treated group (i.e. the ATT), shown in the black dots, for the extreme cases where transfers are targeted entirely to Ricardian households ( $\delta_1 = 1, \delta_2 = 0$ ) or entirely to hand-to-mouth households ( $\delta_1 = 0, \delta_2 = 1$ ). These plots serve to highlight the potential severity of the bias term from Proposition 2. For example, when transfers are targeted to the Ricardian households (Figure 1b), the true iMPC is close to zero ( $< 0.01$ ) and flat, while the shift-share coefficient estimates an on-impact MPC of around 1 – mistakenly suggesting the policy intervention was highly effective at stimulating consumption at the household-level.

Next, I consider a correction in line with Proposition 3. Specifically, I construct estimates of  $\beta_{alt}^{ss}$  from a regression that includes as additional controls (interacted with the shares  $z_i$ ):

Figure 1: Targeted transfers in TANK (Exogenous Shocks, Endogenous Shares)



Notes: Green line in (a) plots the impulse response of transfers following the shock  $\varepsilon_t^g$  – i.e.  $\tilde{T}_t$ . Orange (Purple) lines in (b) and (c) denote  $\beta_{ss}$  ( $\beta_{alt}^{ss}$ ) from (34) for the case with:  $\delta_1 = 1, \delta_2 = 0$  and  $\delta_1 = 0, \delta_2 = 1$  respectively. Black dots in (b) and (c) denote the ‘direct’ response of unconstrained and hand-to-mouth households to the shock respectively, defined by (32). All responses are scaled to the contemporaneous rise in transfers i.e. to  $\tilde{T}_0$ .

$$c_{i,t+h} = \alpha_i + \delta_t + \beta_{alt}^{ss}(z_i \cdot \varepsilon_t^g) + \gamma'(z_i \cdot m_t) + e_{i,t} \quad (35)$$

where  $m_t = [r_t, d_t, \tau_t, w_t]$  – i.e. exactly the aggregate GE variables that determine differential responses across treated and control groups in this setting. Figure 1 shows my results, where I additionally report estimates of  $\beta_{alt}^{ss}$  from (35) in the purple line. This alternate estimator generally performs poorly – delivering similar estimates regardless of whether the transfers are targeted to Ricardians (with low iMPCs) or hand-to-mouth agents (with high iMPCs).

#### 4.2.2 Identification With Exogenous Shares

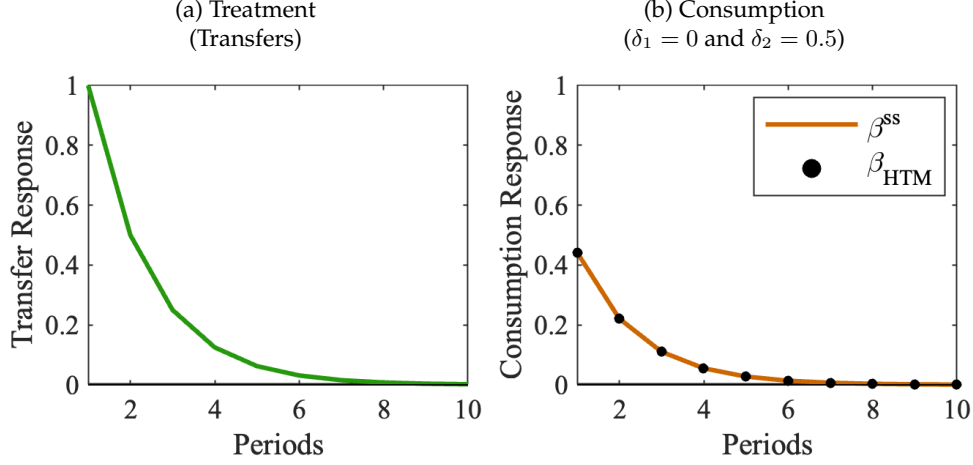
Now I demonstrate that identification can instead proceed via exogenous shares. Following Proposition 5, I do not assume that the shares  $z_i$  are directly exogenous, but rather that the econometrician is aware that treatment status varied depending on whether households were hand-to-mouth, and includes appropriate control variables to account for this. Specifically, I consider regressions of the form:

$$c_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + \lambda(HTM_i \cdot \varepsilon_t^g) + e_{i,t} \quad (36)$$

I consider the case where:  $\delta_1 = 0$  and  $\delta_2 = 0.5$ . In this case, using Proposition 1, the shift-share coefficient is essentially recovered from a ‘‘saturated’’ regression of each units’ impulse response to the shock on  $z_i$  controlling for a constant and  $HTM_i$ . Hence the coefficient on  $z_i$  simply captures the difference in impulse responses for hand-to-mouth agents in ‘‘treatment’’ and ‘‘control’’ groups. Figure 2 displays my results. As before, Figure 2a plots the path for transfers following the shock. Figure 2b then demonstrates that the shift share coef-

ficient, shown in the orange lines, recovers exactly an estimate of hand-to-mouth households' marginal propensities to consume, shown in the black dots.

Figure 2: Targeted transfers in TANK (Exogenous Shocks, Exogenous Shares)



Notes: Green line in (a) plots weighted impulse responses of transfers to shocks to  $\varepsilon_{\mathcal{T}}$  and  $\varepsilon_{\mathcal{A}}$ — i.e.  $\tilde{\mathcal{T}}_t$ . Orange (Purple) lines in (b) and (c) denote  $\beta_{ss}$  ( $\beta_{alt}^{ss}$ ) from (34) for the case with:  $\delta_1 = 1, \delta_2 = 0$  and  $\delta_1 = 0, \delta_2 = 1$  respectively. Black dots in (b) and (c) denote the 'direct' response of unconstrained and hand-to-mouth households to the shock respectively, defined by (32). All responses are scaled to the contemporaneous rise in transfers i.e. to  $\tilde{\mathcal{T}}_0$ .

Finally, I consider an application of Proposition 6. To do so I tweak slightly the baseline model such that transfers additionally respond to other macroeconomic shocks.<sup>16</sup> Specifically I assume that transfers also follow a Taylor-style rule of the form:

$$\mathcal{T}_t = (1 - \rho_{\mathcal{T}})\mathcal{T} + \rho_{\mathcal{T}}\mathcal{T}_{t-1} + (1 - \rho_{\mathcal{T}})\phi_{\mathcal{T}}(\log \Pi_t - \log \Pi) + s_{\mathcal{T}}\varepsilon_t^{\mathcal{T}}. \quad (37)$$

where  $\Pi_t$  denotes aggregate inflation and  $\phi_{\mathcal{T}} > 0$  such that the transfers rise in response to higher inflation. I now assume the econometrician does not have access to the transfer shock  $\varepsilon_{\mathcal{T}}$ , but does have access to the technology shock  $\varepsilon_{\mathcal{A}}$ .<sup>17</sup>

$$d_t = \varepsilon_t^{\mathcal{A}} \quad (38)$$

I then assume they run pairs of regressions of the form

$$c_{i,t+h} = \alpha_{i,c} + \delta_{t,c} + \beta_c^{ss}(z_i \cdot d_t) + \lambda_c(HTM_i \cdot d_t) + e_{i,t} \quad (39)$$

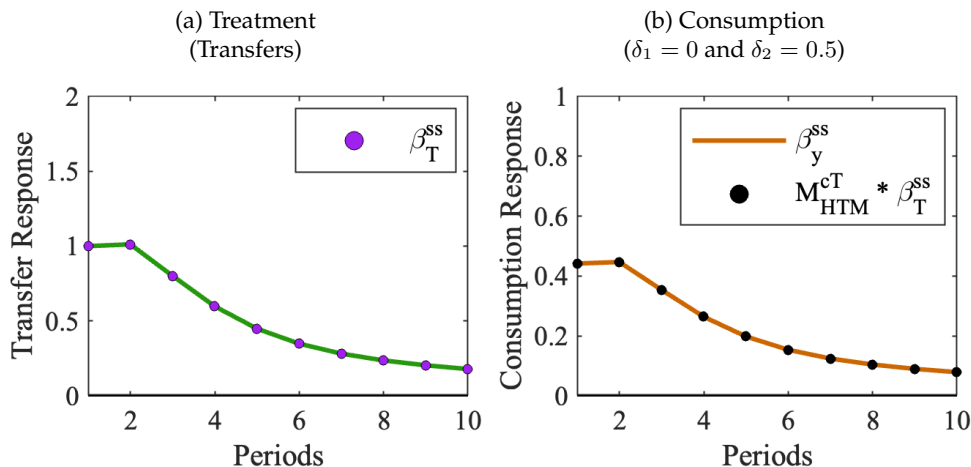
$$\mathcal{T}_{i,t+h} = \alpha_{i,\mathcal{T}} + \delta_{t,\mathcal{T}} + \beta_{\mathcal{T}}^{ss}(z_i \cdot d_t) + \lambda_{\mathcal{T}}(HTM_i \cdot d_t) + u_{i,t} \quad (40)$$

<sup>16</sup>Note that this adjustment is not necessary to apply Proposition 6, although it serves to demonstrate how projecting on different macroeconomic shocks can produce different estimates of consumption responses to different paths for transfers.

<sup>17</sup>This example is only illustrative. Proposition 6 clarifies that the econometrician does not require *any* of the individual shocks from the model to achieve identification in this case, but can instead use any linear combination of time- $t$  shocks – including say, reduced-form Wold innovations from a correctly-specified VAR.

Figure 3 below shows my results. The green line in Figure 3a traces out the path for transfers following the technology shock. As in Proposition 6, the shift-share coefficient  $\beta_T^{ss}$ , shown in the purple dots, traces out exactly this path. Figure 3b then shows the path for  $\beta_y^{ss}$  traced out by the shift-share regression in the orange line, where this corresponds to HTM households' iMPCs out of a transfer shock that produces the dynamic path for transfers from Figure 3a.

Figure 3: Targeted transfers in TANK (No Anticipation, Exogenous Shares)



Notes: Green line in (a) plots impulse response of transfers to technology shock  $\varepsilon_A$ . Purple dots in (a) trace out the shift-share coefficient  $\beta_T^{ss}$  from (40). Orange line in (b) denotes  $\beta_{ss}$  from (40) for the case with:  $\delta_1 = 0$  and  $\delta_2 = 0.5$ . Black dots in (b) and (c) denote the HTM iMPC matrix multiplied by  $\beta_T^{ss}$ . All responses are scaled to the contemporaneous rise in transfers i.e. to  $\tilde{T}_0$ .

In this case, the variation in aggregate transfers underpinning the regressions stems from an endogenous response of transfers to business cycle developments. In particular, note that regressing *aggregate* consumption  $C_{t+h}$  on the shifter  $d_t$  would not recover a valid estimate of the causal effect of transfers on the macroeconomy. Nevertheless, identification of MPCs can still proceed via regressions (39) and (40) – where this information can still be informative about the effect of particular transfer policies at stimulating consumption. This serves to demonstrate exactly the usefulness of employing cross-sectional methods to achieve identification in macroeconomic settings.

## 5 Empirical Application 1: Nakamura and Steinsson (2014)

I now illustrate the practical implications of my econometric framework. In line with the framing in the previous sections, I focus on studies that seek to estimate the 'direct' effects of fiscal stimulus. In this section, I revisit Nakamura and Steinsson (2014), who estimate local (state-level) fiscal multipliers using a shift-share research design with a national shifter. I describe the empirical setup, then discuss issues related identification.

## 5.1 Setting

Nakamura and Steinsson (2014) seek to identify local fiscal multipliers through a shift-share research design. They use annual panel data on US states for 1966–2006, and estimate regressions of the form:<sup>18</sup>

$$\frac{y_{i,t} - y_{i,t-2}}{y_{i,t-2}} = \alpha_{y,i} + \delta_{y,t} + \beta_y^{ss}(z_i \cdot \varepsilon_t^g) + u_{y,i,t} \quad (41)$$

$$\frac{g_{i,t} - g_{i,t-2}}{g_{i,t-2}} = \alpha_{g,i} + \delta_{g,t} + \beta_g^{ss}(z_i \cdot \varepsilon_t^g) + u_{g,i,t} \quad (42)$$

where  $y_{i,t}$  and  $g_{i,t}$  denote per capita output and per capita military procurement spending respectively in state- $i$  in year- $t$ ,  $\varepsilon_t^g$  denotes national military spending in year  $t$ , and  $z_i$  denotes the average level of military spending in state- $i$  relative to state output in the first five years of the sample. The “local multiplier” is then constructed as the ratio of the estimated coefficients:

$$\beta_m = \frac{\beta_y^{ss}}{\beta_g^{ss}} \quad (43)$$

Nakamura and Steinsson (2014) argue that identification in this case comes from exogeneity of the shocks  $\varepsilon_t^g$ :

*“Our identifying assumption is that the United States does not embark on military buildups—such as those associated with the Vietnam War and the Soviet invasion of Afghanistan—because states that receive a disproportionate amount of military spending are doing poorly relative to other states.”*

This is a claim about the implicit mechanism that lies behind the assignment of the shocks  $\varepsilon_t^g$  over time (i.e. the motivation for national military buildups), and not about the assignment of the (time-invariant) shares  $z_i$  across states. In particular, nothing in the above description suggests that states with high-exposure shares would be ‘similar’ to states with low-exposure shares across a range of characteristics.<sup>19</sup> Rather, the authors implicitly argue that any such differences do not pose an issue for identification, so long as the military spending shocks are appropriately exogenous. This claim is correct but only if there were no GE effects from military spending shocks (i.e. SUTVA holds), or if all agents respond the same to any GE effects. The authors implicitly assume the latter through the following claim:

*“By including time fixed effects, we control for aggregate shocks and policy that affect all states at a particular point in time – such as changes in distortionary taxes and aggregate monetary policy.”*

<sup>18</sup>The original study additionally considers a specification where state military spending is instrumented using total national military spending interacted with a state dummy. I focus discussion on the shift-share specification but also consider the sensitivity of results from this additional specification in Appendix F.

<sup>19</sup>The authors themselves suggest that it is implausible that military spending shares are assigned randomly across states, writing: “Military spending is notoriously political and thus likely to be endogenous to regional economic conditions”. As an example, and as I go on to show explicitly, one would expect military spending shares to correlate highly with states’ industry composition (e.g. the balance of tradable vs non-tradable goods production).

Proposition 2 from Section 3 highlights that these claims about identification break down when agents respond heterogeneously to GE effects. Even if national military buildups occurred randomly over time, regressions of the form (41) in general fail to identify local fiscal multipliers since they are contaminated by differential effects of treatment and control groups to any GE effects of the shock.

Proposition 5 clarifies that one solution is to include additional control variables (interacted with  $\varepsilon_t^g$ ) that capture the (implicit) assignment mechanism of the shares  $z_i$  across states. Specifically, the controls must span exactly the variables that determine heterogeneous responses to GE objects that move in response to the shock. What control variables would be appropriate in this case? I take as a starting point [Bellifemine et al. \(2023\)](#) who construct a multi-region Heterogeneous-Agent New Keynesian model of US counties and show that the response of regions to aggregate shocks are shaped by two key dimensions of heterogeneity: MPCs and trade openness. In their model, these two variables are “sufficient statistics” for regional heterogeneity, capturing differences in regional responses to shocks regardless of whether the underlying differences across states are driven by e.g. heterogeneity in discount factors, income risk or borrowing limits.<sup>20</sup>

## 5.2 Share-Assignment

I begin by demonstrating that the shares  $z_i$  indeed correlate highly with variables that plausibly drive heterogeneity in response to GE variables. States that receive high levels of national military spending tend to specialise in high-tech manufacturing sectors (e.g. California) - and as such are likely to be richer (i.e high-income, low MPCs) and more open (a higher fraction of employment in tradable goods) than the average state. To test this, I run regressions of the form:

$$z_i = \alpha + \lambda' a_i + u_i \quad (44)$$

where I include in  $a_i$  various state-characteristics. Specifically I include measures of states' MPCs ( $mpc_i$ ) and openness ( $open_i$ ), which I take directly from [Bellifemine et al. \(2023\)](#), as well as states' income per capita ( $inc_i$ ), which I take from the The Correlates of State Policy database ([Grossmann et al., 2021](#)).<sup>21</sup> The first two variables link directly to the discussion above, while income per capita can be viewed as an additional variable that plausibly captures differences in military spending across states while also being an important determinant of regional heterogeneity in response to shocks.

<sup>20</sup>One key message of this model is that MPCs and openness shape states' responses to GE variables in highly non-linear ways. These nonlinearities are not directly relevant to the solution proposed in Proposition 5: so long as the *assignment rule* is linear then controlling linearly for appropriate variables is sufficient for identification.

<sup>21</sup>The openness and MPC measures vary over time so I use the first year from which both are available (2011). The appropriate controls would capture drivers of  $z_i$  and so ideally would capture these measures from the early-sample period when the shares are constructed (1966-1971). I find there is relatively little variation in these two measures over time so expect this to be of limited importance for my results. I use a measure of states' income per

Table 1: Predictability of Shares

Dependent Variable: Shares ( $z_i$ )	
MPC	-10.927*** (1.882)
Openness	2.061* (1.223)
Income per capita	0.220*** (0.079)
Observations	51
R <sup>2</sup>	0.431
Adjusted R <sup>2</sup>	0.395

*Note:* Table displays coefficient estimates alongside (heteroskedasticity-robust) standard errors and p-values from regression (44). Significance at the 10%, 5%, and 1% levels are denoted by \*, \*\*, and \*\*\* respectively.

I find that these three variables jointly explain over 40% of the variation in the shares  $z_i$ , with each variable significant at least at the 10% level (Table 1). The signs of the coefficients are in line with previous discussion: states with high shares of military spending tend on average to be more open, have higher incomes and lower MPCs. To further validate the use of these three control variables specifically I consider a purely data-driven exercise where I include 13 variables in  $a_i$  that capture various dimensions of heterogeneity across economic (e.g. income and income inequality), fiscal (e.g. tax and welfare policies) and demographic characteristics (e.g. the fraction of Black or Urban workers).<sup>22</sup> I estimate (44) via LASSO and, using 10-fold cross-validation with a  $\lambda_{1se}$  penalty, I find that the regression selects two variables:  $mpc_i$  and  $inc_i$ .

### 5.3 Shift-Share Identification

Guided by my theoretical results I then estimate the following pair of regressions:

$$\frac{y_{i,t} - y_{i,t-2}}{y_{i,t-2}} = \alpha_{y,i} + \delta_{y,t} + \beta_y^{ss}(z_i \cdot \varepsilon_t^g) + \lambda'_y(a_i \cdot \varepsilon_t^g) + u_{y,i,t} \quad (45)$$

$$\frac{g_{i,t} - g_{i,t-2}}{g_{i,t-2}} = \alpha_{g,i} + \delta_{g,t} + \beta_g^{ss}(z_i \cdot \varepsilon_t^g) + \lambda'_g(a_i \cdot \varepsilon_t^g) + u_{g,i,t} \quad (46)$$

including variously  $mpc_i$ ,  $open_i$  and  $inc_i$  in  $a_i$ , with the multiplier then defined appropriately from 1975.

<sup>22</sup>I take additional variables from the Correlates of State Policy database, where I look specifically for variables that are available in the earlier part of the sample (i.e. pre-1975) when the "shares"  $z_i$  were determined.

ately as  $\beta_y^{ss}/\beta_g^{ss}$ . Table 2 displays my results. To maintain direct comparability with the original study I compute standard errors by clustering at the state-level. The first column replicates the baseline results from Nakamura and Steinsson (2014) with an estimated multiplier of 2.48 (significant at the 1% level). The second column adds controls for  $mpc_i$  and  $open_i$  in which case the multiplier drops to 1.41 (significant at the 5% level). The third column adds controls for  $mpc_i$  and  $inc_i$  – the variables selected by LASSO as the key determinants of  $z_i$  – in which case the multiplier drops to 0.9 (significant at the 10% level). The final column adds all three control variables in which case the multiplier drops to 0.5 and is no longer significantly different from zero at conventional levels.

Although the significance levels drop as more control variables are added, the coefficients become more precisely estimated. In the final column for example, one can reject a multiplier above 1.3 at the 5% level. In Appendix F, I additionally show results for an alternate specification employed by Nakamura and Steinsson (2014) where I instrument for state military procurement using total national procurement interacted with a state dummy. Including these same additional control variables (interacted with the shock as above) produces qualitatively similar, but quantitatively less stark, results: in that case the multiplier is lower in all specifications than in the original study, but falls from a baseline of 1.43 to 1.02 with all three additional control variables.

Table 2: Replication and Extension of Nakamura and Steinsson (2014)

	Dependent Variable: Output			
	(1)	(2)	(3)	(4)
Prime military contracts	2.48*** (0.97)	1.41** (0.81)	0.70** (0.38)	0.52 (0.40)
Additional Controls	None	$mpc_i, open_i$	$mpc_i, inc_i$	$mpc_i, open_i, inc_i$
Observations	1989	1989	1989	1989

*Note:* Table displays coefficient estimates alongside standard errors and p-values (clustered at the state level) from the IV regressions described in the main text. Significance at the 10%, 5%, and 1% levels are denoted by \*, \*\*, and \*\*\* respectively.

## 6 Empirical Application 2: Chodorow-Reich (2019)

I next replicate and extend the analysis in Chodorow-Reich (2019), which itself draws on three studies estimating local employment multipliers from the 2009 American Recovery and Reinvestment Act (ARRA). Specifically, following Chodorow-Reich (2019), I first estimate pairs of

regressions of the form:

$$y_i = \alpha_y + \beta_y \varepsilon_i^g + \delta_y' x_i + e_{y,i} \quad (47)$$

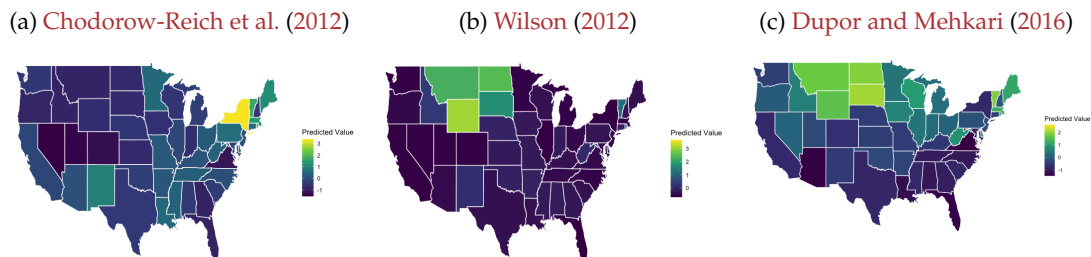
$$g_i = \alpha_g + \beta_g \varepsilon_i^g + \delta_g' x_i + e_{g,i} \quad (48)$$

where  $y_i$  is the growth in annualised employment (normalised by the adult population) in state  $i$  between 2008:12 and 2010:12,  $g_i$  is the total ARRA outlays over 2008:12 and 2010:12,  $x_i$  collects lagged state-employment (in changes and levels) and lagged growth in Gross State Production as controls for pre-treatment economic conditions, and  $\varepsilon_i^g$  is an instrument that captures exogenous shocks to ARRA spending. Local fiscal multipliers are then computed by taking the ratios:  $\beta_m = \beta_y/\beta_g$ . The instruments  $\varepsilon_i^g$  come from previous studies, exploiting ARRA spending across states that was determined by various pre-recession related formulas – from components of Medicaid spending as determined by the Federal Medical Assistance Percentages (FMAP)(Chodorow-Reich et al., 2012), Department of Transport (DOT) spending (Wilson, 2012) as well as other elements of the package (Dupor and Mehkari, 2016).

Since all instruments are constructed using pre-recession formulae, it is plausible that the resulting shocks are uncorrelated with any other shocks that drove states' outcomes over the same period. Nevertheless, bias can still arise if government spending shocks  $\varepsilon_i^g$  are distributed across units such that the spillover effects from other units' shocks differentially affected treatment and control groups. I demonstrate this explicitly with a simple quantitative model in Appendix E. In the model, each state is subject to spillovers from government spending in other states via trade channels. In this case, when demand for imports is governed by a gravity equation, bias can arise when government spending shocks are geographically clustered since the treatment and control groups are heterogeneously exposed to changes in external demand from shocks to other units.

**Shock-Assignment** To consider the potential for this bias to arise, I begin by visualising the shocks  $\varepsilon^g = [\varepsilon_1^g, \dots, \varepsilon_n^g]$ . Figure 4 displays heatmaps for each measure, which all display significant regional clustering.

Figure 4: Heatmaps of Instruments Across Studies



To assess this more formally I run regressions of the form:

$$\varepsilon_i^g = \alpha + \gamma \left( \sum_{j \neq i} w_{i,j} \varepsilon_j^g \right) + \theta' x_i + e_i \quad (49)$$

where, I set  $w_{i,j}$  as an appropriate proxy for the relative spillover effects of the shock in state- $j$  on state- $i$ . I proxy for these spillover effects using geographical distance between states, consistent with various evidence of sizable local spillovers from the ARRA package proportional to geographical distance (Dupor and McCrory, 2018; Auerbach et al., 2020).<sup>23</sup> Specifically I set:

$$w_{i,j} = \left( \frac{d_{i,j}}{\sum_{k \neq i} d_{i,k}} \right)^{-\delta} \quad (50)$$

where  $d_{i,j}$  is the geographical distance between state- $i$  and state- $j$  and  $\delta$  controls how local spillovers decay with distance. I set  $\delta = 5$  – broadly consistent with the relationship between distance and trade from the gravity literature (see Adão et al. (2019)). Panel A of Table 3 displays my results. Each of the columns represents estimates for the instruments – from Chodorow-Reich et al. (2012) ('FMAP'), Wilson (2012) ('DOT') and Dupor and Mehkari (2016) ('DM') – where  $\gamma$  is positive and highly significant in each case.

Following the logic of the previous section, a way to adjust for this bias is to directly control for the implicit assignment mechanism that generated the clustering. To this end, I then estimate regressions of the form:

$$\varepsilon_i^g = \alpha + \lambda' a_i + \theta' x_i + \eta_i \quad (51)$$

where I include in  $a_i$  additional variables that plausibly generate the spatial clustering of shocks. The original studies provide useful information on this point. For example, the instrument from Chodorow-Reich et al. (2012) is based on pre-recession generosity of Medicaid spending, where the authors note this tended to be larger in more liberal coastal and Midwestern states – and so I include regional fixed effects to capture this. The instrument from Wilson (2012) captures differences in department of transport spending – where the original study notes that this in part depends on geographic features of states that in turn determine population density and lane-miles per capita<sup>24</sup> – and so I include controls for population density (and population density squared). Finally, since the instrument from Dupor and Mehkari (2016) collects together various similar sources of variation across different elements of the ARRA

<sup>23</sup>Note that my identification strategy does not rely on these capturing the 'true' spillovers - I use them here only as a way of conducting a plausibility test to establish the potential for bias in this setting.

<sup>24</sup>This is directly visible from Figure 4b, where the largest shocks all occur in four very sparsely populated neighbouring Western states.

Table 3: Predictability of Shocks

	Dependent Variable: Shocks ( $\varepsilon_i^g$ )		
	(1)	(2)	(3)
<i>A: Model 1</i>			
Distance-Weighted Foreign Shocks	0.68*** (0.19)	0.58* (0.23)	0.50** (0.20)
Adjusted $R^2$	0.35	0.51	0.33
<i>B: Model 2</i>			
Region 2	-5.12*** (1.38)	-	-0.04 (0.11)
Region 3	-6.81*** (1.29)	-	-0.26*** (0.10)
Region 4	-7.37*** (1.45)	-	-0.23* (0.13)
Pop. Density	-	-0.79*** (0.22)	-0.16 (0.11)
Pop. Density Sq.	-	0.18*** (0.04)	0.04** (0.02)
Adjusted $R^2$	0.47	0.63	0.43
Instrument	FMAP	DOT	DM
Observations	50	50	50

*Note:* Table displays coefficient estimates alongside (heteroskedasticity-robust) standard errors and p-values from regressions described in the main text. Panel A displays estimates of  $\gamma$  from (49) and Panel B displays estimates of  $\lambda$  from (51). Each column refers to estimates from the FMAP, DOT and DM instrument respectively. Significance at the 10%, 5%, and 1% levels are denoted by \*, \*\*, and \*\*\* respectively.

package, I include both regional fixed effects and measures of population density.<sup>25</sup> Panel B of Table 3 shows my results, where I find that these variables are effective at capturing variation in the shocks  $\varepsilon_i^g$  – coefficients on my selected variables tend to be highly significant and (adjusted) R-squareds are around 0.4-0.6. Finally, I confirm that these residualised instruments are plausibly successful at removing the bias by re-estimating equation (49) using the residualised shocks  $\eta = [\eta_1, \dots, \eta_m]$  in place of  $\varepsilon_i^g = [\varepsilon_1^g, \dots, \varepsilon_n^g]$ . I find  $\gamma$  becomes small and statistically

<sup>25</sup>Dupor and Mehkari (2016) note that population density directly determined variation in their instrument: “the Capital Transit Assistance program allocated roughly 6 billion to fund public transit capital improvements to urbanized areas (UZAs). The apportionment for medium sized UZAs was determined by **population density** and population, and the apportionment for large UZAs (populations greater than 200,000) was determined by factors such as bus revenue vehicle miles, bus passenger miles, fixed guideway route (such as rail) miles and **population density**.” Other criteria for the allocation of funds for their instrument include “flood risk” and “the presence of inland and costal navigation” which motivates the use of additional regional fixed effects.

Table 4: Replication and Extension of Chodorow-Reich (2019)

	Dependent Variable: Job-Years per 100k			
	(1)	(2)	(3)	(4)
<i>A: Original</i>				
ARRA Spending	2.29*** (0.75)	2.22*** (1.28)	1.82** (0.91)	2.01*** (0.91)
F-stat.	26.07	10.75	49.21	36.29
Adj. R <sup>2</sup>	0.44	0.44	0.45	0.45
<i>B: + Assignment Controls</i>				
ARRA Spending	1.46 (1.36)	3.77 (2.36)	2.40* (1.22)	2.01* (1.01)
F-stat.	12.74	4.74	25.96	17.61
Adj. R <sup>2</sup>	0.47	0.35	0.46	0.47
Instrument	FMAP	DOT	DM	All
Observations	50	50	50	50

*Note:* Table displays coefficient estimates alongside (heteroskedasticity-robust) standard errors p-values, F-statistics and R-squareds from IV regressions with reduced-form (??) with different control variables  $a_i$ . Panel A displays estimates with no additional controls, and Panel B displays estimates with the controls from Panel B of Table 3. Each column refers to estimates from the FMAP, DOT and DM instrument respectively, with the final column reporting 2SLS estimates employing all instruments simultaneously. Significance at the 10%, 5%, and 1% levels are denoted by \*, \*\*, and \*\*\* respectively.

insignificant in each case (Appendix Table 8). I additionally present plots of these residualised shocks in Appendix Figure 6.

**Results** Finally, I estimate regressions of the form:

$$y_i = \alpha_y + \beta_y \varepsilon_i^g + \gamma_y' a_i + \delta_y' x_i + e_i \quad (52)$$

$$g_i = \alpha_g + \beta_g \varepsilon_i^g + \gamma_g' a_i + \delta_g' x_i + u_i \quad (53)$$

with multipliers defined as the ratio:  $\beta_y/\beta_g$ . Table 4 displays my results. The first panel replicates Chodorow-Reich (2019), where the first three columns report results for each of the instruments, and the final column uses 2SLS using all three instruments simultaneously. As in Chodorow-Reich (2019) I find that, without additional controls, the estimated multiplier is around 1.8-2.3 and highly significant for each study. With additional controls, the multiplier estimate falls for the FMAP instrument (and becomes insignificantly different from zero), and

risers but becomes very imprecisely estimated for the DOT instrument (with the instrument no longer 'strong' at conventional levels). The DM instrument, which combines variation from various sources remains significant and positive (2.40), and using all three instrument simultaneously (alongside all control variables) yields an unchanged (albeit less precise) estimate for the multiplier of 2.01. Overall, the evidence for large local fiscal multipliers from ARRA appears somewhat weaker than the discussion in [Chodorow-Reich \(2019\)](#), although jointly the three studies continue to provide significant evidence of sizable jobs multipliers (around 2).

## 7 Conclusion

The use of cross-sectional data to answer key questions of interest in macroeconomics has risen sharply in recent years. It is well-understood that identification in these settings generally faces the so-called "missing intercept" problem: there is typically insufficient information in cross-sectional data to recover the "total" (or "general equilibrium") effects of policy interventions. In this paper, I consider a less well-studied problem: if policy interventions indeed propagate via GE forces, then otherwise credible research designs from the applied microeconometrics literature generally fail to recover *either* the "total" *or* the "direct" effects of these interventions. That is, the presence of GE effects implies regression coefficients in these settings generically suffer from both a "missing intercept" as well as a "biased slope".

The main contribution of this paper is to demonstrate a robust and simple approach to achieve identification of the direct effects of policy interventions using cross-sectional variation. The key informational requirement for this approach is knowledge of the underlying "targeting" of the policy intervention across units. Crucially, this approach does not require the econometrician to have knowledge of the underlying GE transmission mechanisms or to have access to any well-identified time-series shocks – thus respecting the typical motivation for employing cross-sectional variation to achieve identification. Taken together, my results highlight both the promise and the pitfalls of employing cross-sectional data in macroeconomics.

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## Appendix

### A Section 2 Proofs

#### A.1 Proof of Proposition 1

**Proposition 1** Consider shift-share regressions of the form:

$$y_{i,t+h} = \beta^{ss}(s_i \cdot d_t) + e_{i,t}$$

where  $s_i \perp \varepsilon_t$  and  $d_t = \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l}$  for  $(1 \times n_\varepsilon)$  length (row-)vector  $\Psi_l$ .

Then:

$$\beta^{ss} = \frac{E \left[ \left( \sum_{l=0}^{\infty} \Theta_{i,h+l}^y w_l \right) s_i \right]}{E[s_i^2]}$$

where  $\eta_{kl}$  is  $n_\varepsilon$ -length vector of ‘weights’ with  $k$ ’th element equal to:  $\eta_{k,l} = \frac{\psi_{k,l}}{\sum_{k=1}^{n_\varepsilon} \sum_{l=0}^{\infty} \psi_{k,l}^2}$ .

**Proof:** The shift-share coefficient is equivalent to:

$$\beta^{ss} = \frac{E[y_{i,t+h} s_i d_t]}{E[(s_i d_t)^2]}$$

Plugging in equation (17):

$$\beta^{ss} = \frac{E \left[ \left( \sum_{l=0}^{\infty} \Theta_{i,h+l}^y \varepsilon_{t-l} + \bar{y}_i \right) s_i \sum_{m=0}^{\infty} \Psi_m \varepsilon_{t-m} \right]}{E \left[ \left( s_i \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l} \right)^2 \right]}$$

Since  $\forall i \forall t : \bar{y}_i, s_i \perp \varepsilon_t$  and shocks are mean-zero:

$$\beta^{ss} = \frac{E \left[ s_i \left( \sum_{l=0}^{\infty} \Theta_{i,h+l}^y \varepsilon_{t-l} \right) \left( \sum_{m=0}^{\infty} \Psi_m \varepsilon_{t-m} \right) \right]}{E \left[ \left( s_i \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l} \right)^2 \right]}$$

Since  $\forall l \neq m : \varepsilon_{t-l} \perp \varepsilon_{t-m}$  and shocks are mean-zero:

$$\beta^{ss} = \frac{E \left[ \left( s_i \sum_{l=0}^{\infty} \left( \Theta_{i,h+l}^y \varepsilon_{t-l} \right) \left( \Psi_l \varepsilon_{t-l} \right) \right) \right]}{E \left[ \left( s_i \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l} \right)^2 \right]}$$

Switching terms:

$$\beta^{ss} = \frac{\sum_{l=0}^{\infty} E \left[ \left( s_i \left( \Theta_{i,h+l}^y \right) \varepsilon_{t-l} \left( \Psi_l \varepsilon_{t-l} \right) \right) \right]}{E \left[ \left( s_i \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l} \right)^2 \right]}$$

Note that  $\forall i, \forall t : x_i, z_i \perp \varepsilon_t \implies \forall i, \forall t, \forall h : \Theta_{i,h+l}^y \perp \varepsilon_t$ . And also  $\forall i, \forall t : s_i \perp \varepsilon_t$ , so:

$$\beta^{ss} = \frac{\sum_{l=0}^{\infty} E \left[ s_i \Theta_{i,h+l}^y \right] E \left[ \varepsilon_{t-l} \left( \Psi_l \varepsilon_{t-l} \right) \right]}{E \left[ s_i^2 \right] E \left[ \left( \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l} \right)^2 \right]}$$

And so defining  $\eta_l$  as the  $n_\varepsilon \times 1$  vector :

$$\eta_l = \frac{E \left[ \varepsilon_{t-l} \left( \Psi_l \varepsilon_{t-l} \right) \right]}{E \left[ \left( \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l} \right)^2 \right]}$$

We have the desired expression:

$$\beta^{ss} = \frac{E \left[ \sum_{l=0}^{\infty} \eta_l \Theta_{i,h+l}^y s_i \right]}{E \left[ s_i^2 \right]}$$

Note that since the shocks are mutually independent and unit-variance we have:

$$\eta_{k,l} = \frac{\psi_{k,l}}{\sum_{k=1}^{n_\varepsilon} \sum_{l=0}^{\infty} \psi_{k,l}^2}$$

## A.2 Proof of Corollary 1

**Corollary 1** Consider regressions of the form:

$$Y_{t+h} = \beta^{ols} d_t + e_t$$

$$\forall i \in [0, 1] : Y_{i,t+h} = \beta_i^{ols} d_t + e_{i,t}$$

for  $d_t = \sum_{l=0}^{\infty} \Psi_l \varepsilon_{t-l}$ . Then:

$$\beta^{ols} = \sum_{l=0}^{\infty} \Theta_{h+l} \eta_l$$

$$\forall i \in [0, 1] : \beta_i^{ols} = \sum_{l=0}^{\infty} \Theta_{i,h+l} \eta_l$$

where  $\eta_l$  has the same form as Proposition 1.

**Proof:** The proof follows the same steps as Proposition 1 setting  $s_i = 1$ . For completeness, for  $\beta^{ols}$  we have:

$$\beta^{ols} = \frac{E \left[ \left( \sum_{l=0}^{\infty} (\Theta_{h+l} \varepsilon_{t-l} + \bar{Y}) \right) \left( \sum_{m=0}^{\infty} \Psi_m \varepsilon_{t-m} \right) \right]}{E \left[ d_t^2 \right]}.$$

Since shocks are mean-zero and serially uncorrelated, the numerator reduces to:

$$\sum_{l=0}^{\infty} E[(\Theta_{h+l} \varepsilon_{t-l})(\Psi_l \varepsilon_{t-l})]$$

And using unit-variance:

$$E[(\Theta_{h+l} \varepsilon_{t-l})(\Psi_l \varepsilon_{t-l})] = \Theta_{h+l} E[\varepsilon_{t-l} \varepsilon'_{t-l}] \Psi_l' = \Theta_{h+l} \Psi_l'.$$

Therefore

$$\beta^{ols} = \frac{\sum_{l=0}^{\infty} \Theta_{h+l} \Psi_l'}{E \left[ d_t^2 \right]} = \sum_{l=0}^{\infty} \Theta_{h+l} \eta_l,$$

where the (vector) weights are

$$\eta_l := \frac{\Psi_l'}{E \left[ d_t^2 \right]} = \frac{E[\varepsilon_{t-l}(\Psi_l \varepsilon_{t-l})]}{E \left[ d_t^2 \right]}.$$

Noting again  $E \left[ d_t^2 \right] = \sum_{m=0}^{\infty} \Psi_m \Psi_m' = \sum_{m=0}^{\infty} \sum_{k=1}^{n_\varepsilon} \psi_{k,m}^2$ , the  $k$ th element is

$$\eta_{k,l} = \frac{\psi_{k,l}}{\sum_{m=0}^{\infty} \sum_{j=1}^{n_\varepsilon} \psi_{j,m}^2},$$

which this matches  $\eta_l$  from Proposition 1. It is straightforward to repeat this argument for  $Y_{i,t+h}$ .  $\square$

### A.3 Proof of Proposition 2

**Proposition 2** Consider a setting where  $z_i \in \{0, 1\}$ . Consider shift-share regressions of the form:

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss} (z_i \cdot \varepsilon_t^g) + e_{i,t}$$

Then the shift-share coefficient  $\beta^{ss}$  can be expressed as:

$$\beta^{ss} = \underbrace{E[\beta_{i,h}^y | z_i = 1]}_{\text{ATT}} + \underbrace{E[\zeta_{i,h}^y | z_i = 1] - E[\zeta_{i,h}^y | z_i = 0]}_{\text{Bias from G.E. responses}}$$

**Proof:** By FWL, the shift-share coefficient is equivalent to:

$$\beta^{ss} = \frac{E[y_{i,t+h}\ddot{g}_{i,t}]}{E[\ddot{g}_{i,t}^2]}$$

where  $\ddot{g}_{i,t}$  denotes the residual from a (population) regression of  $g_{i,t} \equiv z_i \varepsilon_t^g$  on unit- and time-fixed effects. Given shocks are mean-zero we have:

$$\ddot{g}_{i,t} = (z_i - E[z_i])(\varepsilon_t^g - E[\varepsilon_t^g]) = \tilde{z}_i \varepsilon_t^g$$

where  $\tilde{z}_i$  denotes de-meanned  $z_i$ . By Proposition 1:

$$\beta^{ss} = \frac{E[\theta_{i,h}^{yg} \tilde{z}_i]}{E[(\tilde{z}_i)^2]}$$

where note this can be interpreted as the slope coefficient in a population regression of  $\theta_{i,h}^{yg}$  on  $z_i$  and a constant. For binary exposures  $z_i \in \{0, 1\}$ , this coefficient captures the difference in conditional expectations across groups:

$$\beta^{ss} = E[\theta_{i,h}^{yg} | z_i = 1] - E[\theta_{i,h}^{yg} | z_i = 0]$$

The desired conclusion then follows by plugging in (20):

$$\beta^{ss} = E[\beta_{i,h}^y | z_i = 1] + E[\zeta_{i,h}^y | z_i = 1] - E[\zeta_{i,h}^y | z_i = 0] \quad \square$$

#### A.4 Proof of Proposition 3

**Proposition 3** Consider a setting where  $z_i \in \{0, 1\}$ . Consider shift-share regressions of the form:

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + \lambda'(z_i \cdot w_t) + e_{i,t}$$

Then the shift-share coefficient  $\beta^{ss}$  has the form:

$$\begin{aligned} \beta^{ss} = & \underbrace{\lambda_0^g E[\beta_{i,h}^y | z_i = 1]}_{\text{ATT}} + \underbrace{\lambda_0^g \left( E[\zeta_{i,h}^y | z_i = 1] - E[\zeta_{i,h}^y | z_i = 0] \right)}_{\text{Bias from G.E. responses}} \\ & + \underbrace{\sum_{k=2}^{n_\varepsilon} \lambda_{0,k} \left( E[\Theta_{i,0,k}^y | z_i = 1] - E[\Theta_{i,0,k}^y | z_i = 0] \right)}_{\text{Bias from other contemp. shocks}} + \underbrace{\sum_{l=0}^{\infty} \sum_{k=1}^{n_\varepsilon} \lambda_{l,k} \left( E[\Theta_{i,h,k}^y | z_i = 1] - E[\Theta_{i,h,k}^y | z_i = 0] \right)}_{\text{Bias from other lagged shocks}} \end{aligned}$$

**Proof:** By FWL, the shift-share coefficient is equivalent to:

$$\beta^{ss} = \frac{E[y_{i,t+h}\tilde{g}_{i,t}]}{E[\tilde{g}_{i,t}^2]}$$

where  $\tilde{g}_{i,t}$  denotes the residual from a (population) regression of  $g_{i,t} \equiv z_i \varepsilon_t^g$  on unit- and time-fixed effects and  $z_i w_t$ . We then have:

$$\tilde{g}_{i,t} = (z_i - E[z_i])(\varepsilon_t^g)^{\perp w_t} = \tilde{z}_i (\varepsilon_t^g)^{\perp w_t}$$

where  $(\varepsilon_t^g)^{\perp w_t}$  denotes the residual from a regression of  $\varepsilon_t^g$  on  $w_t$ . Regressing  $\varepsilon_t^g$  on  $w_t$  recovers a linear combination of shocks up to time- $t$ , specifically:

$$(\varepsilon_t^g)^{\perp w_t} = \underbrace{(1 - b' \Theta_{0,1}^w)}_{\equiv \psi_{0,1}} \varepsilon_t^g + \sum_{k=2}^{n_\varepsilon} \underbrace{b' \Theta_{0,k}^w}_{\equiv \psi_{0,k}} \varepsilon_{k,t} + \sum_{l \geq 1} \underbrace{b' \Theta_l^w}_{\equiv \psi_l} \varepsilon_{t-l}$$

where:

$$b' = -(\Theta_{0,1}^w)' \Sigma_w^{-1}, \quad \Sigma_w = \sum_{l=0}^{\infty} \Theta_l^w (\Theta_l^w)', \quad \psi_l = [\psi_{l,1} \dots \psi_{l,n_\varepsilon}]$$

By Proposition 1:

$$\beta^{ss} = \frac{E[\sum_{l=0}^{\infty} \Theta_l^y \lambda_l \tilde{z}_i]}{E[\tilde{z}_i^2]}$$

where  $\lambda_l$  is  $n_\varepsilon$ -length vector with  $k$ 'th element equal to:  $\lambda_{k,l} = \frac{\psi_{k,l}}{\sum_{k=1}^{n_\varepsilon} \sum_{l=0}^{\infty} \psi_{k,l}^2}$ . Note that for binary, de-meant  $z_i$  the shift-share coefficient captures differences in conditional means:

$$\beta^{ss} = \sum_{l=0}^{\infty} \sum_{k=1}^{n_\varepsilon} \lambda_{k,l} E[\Theta_{l,k}^y | z_i = 1] - E[\Theta_{l,k}^y | z_i = 0]$$

The finally applying the decomposition (20):

$$\begin{aligned} \beta^{ss} &= \lambda_{0,1} E[\beta_{i,h}^y | z_i = 1] + \lambda_{0,1} \left( E[\zeta_{i,h}^y | z_i = 1] - E[\zeta_{i,h}^y | z_i = 0] \right) \\ &+ \sum_{k=2}^{n_\varepsilon} \lambda_{0,k} \left( E[\Theta_{i,0,k}^y | z_i = 1] - E[\Theta_{i,0,k}^y | z_i = 0] \right) + \sum_{l=0}^{\infty} \sum_{k=1}^{n_\varepsilon} \lambda_{l,k} \left( E[\Theta_{i,h,k}^y | z_i = 1] - E[\Theta_{i,h,k}^y | z_i = 0] \right) \quad \square \end{aligned}$$

## A.5 Proof of Proposition 4

**Proposition 4** Consider shift-share regressions of the form:

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss} (z_i \cdot \varepsilon_t^g) + \lambda (\zeta_{i,h}^y \cdot \varepsilon_t^g) + e_{i,t}$$

Then the shift-share coefficient  $\beta^{ss}$  has the form:

$$\beta^{ss} = \frac{E[\beta_{i,h}^y \omega_i]}{E[\omega_i]}$$

where  $\omega_i = E[\tilde{z}_i z_i]$  and  $\tilde{z}_i$  is the (population)-residual from a regression of  $z_i$  on a constant and  $\zeta_{i,h}^y$ .

**Proof** By FWL, the shift-share coefficient is equivalent to:

$$\beta^{ss} = \frac{E[y_{i,t+h}\tilde{g}_{i,t}]}{E[\tilde{g}_{i,t}^2]}$$

where  $\tilde{g}_{i,t}$  denotes the residual from a (population) regression of  $g_{i,t} \equiv z_i \varepsilon_t^g$  on unit- and time-fixed effects and  $\zeta_{i,h}^y \varepsilon_t^g$ . We then have:

$$\tilde{g}_{i,t} = \tilde{z}_i(\varepsilon_t^g)$$

where  $\tilde{z}_i$  denotes the residual from a regression of  $z_i$  on  $\zeta_{i,h}^y$  and a constant. Using Proposition 1:

$$\beta^{ss} = \frac{E[\theta_{i,h}^{yg} \tilde{z}_i]}{E[(\tilde{z}_i)^2]}$$

Plugging in (20):

$$\beta^{ss} = \frac{E[\beta_{i,h}^y z_i \tilde{z}_i]}{E[(\tilde{z}_i)^2]} + \frac{E[\zeta_{i,h}^y \tilde{z}_i]}{E[(\tilde{z}_i)^2]}$$

Since  $\tilde{z}_i$  is an OLS-population residual,  $E[\zeta_{i,h}^y \tilde{z}_i] = 0$ . In addition:

$$\begin{aligned} E[(\tilde{z}_i)^2] &= E[(z_i - a - b\zeta_{i,h}^y)\tilde{z}_i] \\ &= E[z_i \tilde{z}_i] \end{aligned}$$

where  $a$  and  $b$  denote OLS coefficients from the regression of  $z_i$  on a constant and  $\zeta_{i,h}^y$ , and the final line follows from the fact OLS-residuals are uncorrelated with regressors. And so:

$$\frac{E[\beta_{i,h}^y z_i \tilde{z}_i]}{E[z_i \tilde{z}_i]} \quad \square$$

## A.6 Proof of Proposition 5

**Proposition 5** *Suppose Condition 3 holds. Consider shift-share regressions of the form:*

$$y_{i,t+h} = \alpha_i + \delta_t + \beta^{ss}(z_i \cdot \varepsilon_t^g) + \lambda'(a_i \cdot \varepsilon_t^g) + e_{i,t}$$

Then:

$$\beta^{ss} = \frac{E[\beta_{i,h}^y \omega_i]}{E[\omega_i]}$$

where  $\omega_i = E[\tilde{z}_i^2 | x_i, a_i]$  and  $\tilde{z}_i$  is the (population)-residual from a regression of  $z_i$  on  $a_i$ .

**Proof** By FWL, the shift-share coefficient is equivalent to:

$$\beta^{ss} = \frac{E[y_{i,t+h}\tilde{g}_{i,t}]}{E[\tilde{g}_{i,t}^2]}$$

where  $\tilde{g}_{i,t}$  denotes the residual from a (population) regression of  $g_{i,t} \equiv z_i \varepsilon_t^g$  on unit- and time-fixed effects and  $a_i \varepsilon_t^g$ . We then have:

$$\tilde{g}_{i,t} = \tilde{z}_i \varepsilon_t^g$$

where  $\tilde{z}_i$  denotes the population residual from a regression of  $z_i$  on a constant and  $a_i$ . By Proposition 1:

$$\beta^{ss} = \frac{E[\theta_{i,h}^{yg} \tilde{z}_i]}{E[(\tilde{z}_i)^2]}$$

Plugging in (20):

$$\beta^{ss} = \frac{E[\beta_{i,h}^y z_i \tilde{z}_i] + E[\zeta_{i,h}^y \tilde{z}_i]}{E[(\tilde{z}_i)^2]}$$

Note that under Condition 3, since the conditional expectation function is linear:

$$\tilde{z}_i = z_i - c - \delta' a_i$$

which implies:  $E[\tilde{z}_i | x_i, a_i] = E[\tilde{z}_i | a_i] = 0$ . And so:

$$\begin{aligned} E[\beta_{i,h}^y z_i \tilde{z}_i] + E[\zeta_{i,h}^y \tilde{z}_i] &= E[\beta_{i,h}^y E[\tilde{z}_i z_i | x_i, a_i]] + E[\zeta_{i,h}^y E[\tilde{z}_i | x_i, a_i]] \\ &= E[\beta_{i,h}^y E[\tilde{z}_i z_i | x_i, a_i]] \end{aligned}$$

Finally since:

$$\begin{aligned} E[\tilde{z}_i z_i | x_i, a_i] &= E[\tilde{z}_i (\tilde{z}_i + c + \delta' a_i) | x_i, a_i] \\ &= E[\tilde{z}_i^2 | x_i, a_i] \end{aligned}$$

it then follows that:

$$\beta^{ss} = \frac{E[\beta_{i,h}^y E[\tilde{z}_i^2 | x_i, a_i]]}{E[E[\tilde{z}_i^2 | x_i, a_i]]} \quad \square$$

## A.7 Proof of Proposition 6

**Proposition 6** *Assume Condition 3 and 4 hold. Consider the pair of shift-share regressions of the form:*

$$y_{i,t+h} = \alpha_{i,y} + \delta_{t,y} + \beta_{y,h}^{ss}(z_i \cdot d_t) + \lambda'(a_i \cdot d_t) + e_{i,t}$$

$$g_{i,t+h} = \alpha_{i,g} + \delta_{t,g} + \beta_{g,h}^{ss}(z_i \cdot d_t) + \lambda'(a_i \cdot d_t) + u_{i,t}$$

Then:

$$\beta_y^{ss} = \frac{E[\mathcal{M}_i^{yg} \beta_g^{ss} \omega_i]}{E[\omega_i]}$$

for  $\beta_y^{ss} = \{\beta_{y,h}^{ss}\}_{h=0}^\infty$  and  $\beta_g^{ss} = \{\beta_{g,h}^{ss}\}_{h=0}^\infty$ , where  $\omega_i = E[\tilde{z}_i^2 | x_i, a_i]$

**Proof:** Start with  $\beta_y^{ss}$ . By Proposition 1:

$$\beta_y^{ss} = \frac{\sum_k w_k E[\Theta_{i,k}^y \tilde{z}_i]}{E[\tilde{z}_i^2]}$$

where  $\tilde{z}_i$  denotes the population residual from a regression of  $z_i$  on a constant and  $a_i$ , and  $w_k = \frac{q_k}{\sum_{k=1}^{n_\varepsilon} (q_k)^2}$ . Next recall the decomposition (26):

$$\Theta_{i,k}^y = \mathcal{M}_i^{yg} z_i \Theta_k^g + \mathcal{M}_i^{yw} \Theta_k^w$$

Then by Condition 3:

$$\forall k : E[\mathcal{M}_i^{yw} \Theta_k^w \tilde{z}_i] = E[\mathcal{M}_i^{yg} \Theta_k^g E[\tilde{z}_i | x_i, a_i]] = 0$$

where the first equality uses LIE and that  $\mathcal{M}_i^{yw}$  is measurable with respect to  $x_i$ , and the second line uses that  $E[\tilde{z}_i | x_i, a_i] = 0$ . And so returning to the expression for  $\beta_y^{ss}$ :

$$\beta_y^{ss} = \frac{\sum_k w_k E[\mathcal{M}_i^{yg} \Theta_k^g z_i \tilde{z}_i]}{E[\tilde{z}_i^2]}$$

Next:

$$E[\mathcal{M}_i^{yg} \Theta_k^g z_i \tilde{z}_i] = E[\mathcal{M}_i^{yg} \Theta_k^g E[z_i \tilde{z}_i | x_i, a_i]] = E[\mathcal{M}_i^{yg} \Theta_k^g E[\tilde{z}_i^2 | x_i, a_i]]$$

where again this uses LIE alongside the fact that  $\mathcal{M}_i^{yg}$  is measurable with respect to  $x_i$ , then (as in the proof of Proposition 5):  $E[z_i \tilde{z}_i | x_i, a_i] = E[\tilde{z}_i^2 | x_i, a_i]$ . Then letting  $\omega_i = E[\tilde{z}_i^2 | x_i, a_i]$ :

$$\begin{aligned} \beta_y^{ss} &= \frac{\sum_k w_k E[\mathcal{M}_i^{yg} \Theta_k^g \omega_i]}{E[\omega_i]} \\ &= \frac{E[\mathcal{M}_i^{yg} \sum_k w_k \Theta_k^g \omega_i]}{E[\omega_i]} \end{aligned}$$

Now turn to  $\beta_g^{ss}$ . By FWL, letting  $x_{i,t} = z_i d_t$ :

$$\beta_{g,h}^{ss} = \frac{E[\tilde{g}_{i,t+h} \tilde{x}_{i,t}]}{E[\tilde{x}_{i,t}^2]}$$

where  $\tilde{g}_{i,t+h}$  and  $\tilde{x}_{i,t}$  denote  $g_{i,t+h}$  and  $x_{i,t}$  orthogonalised with respect to unit- and time-fixed effects and  $a_i * d_t$ . Given  $g_{i,t+h} = z_i g_t$ , we have:

$$\tilde{g}_{i,t+h} = \tilde{z}_i(g_{t+h} - E[g_{t+h}]), \quad \tilde{x}_{i,t} = \tilde{z}_i(d_t - E[d_t]),$$

Then since the OLS estimand satisfies scaling invariance,  $\beta_{g,h}^{ss}$  is equivalent to a population regression of  $(g_{t+h} - E[g_{t+h}])$  on  $(d_t - E[d_t])$ , which by Corollary 1 gives:

$$\beta_g^{ss} = \sum_k w_k \Theta_k^g$$

And so finally:

$$\beta_y^{ss} = \frac{E[\mathcal{M}_i^{yg} \omega_i \beta_g^{ss}]}{E[\omega_i]} \quad \square$$

## B Extension 1: Cross-sectional data

I extend the results in Section 3 to cover settings with cross-sectional data. I keep the data generating process the same but treat the shocks as fixed (i.e. non-stochastic) in this case. Specifically I suppose the econometrician has access to only a single government spending shock that occurs at some fixed point in time  $s$  and uses cross-sectional local-projections to estimate the direct (dynamic) causal effect of the shock. Consider the following two conditions in this setting:

**Condition 1a** :  $\{\varepsilon_t^g\}_{t \neq s} = 0$  and  $\forall k \neq 1 : \Theta_k^g = 0$

**Condition 2a** :  $E[z_i | x_i, a_i] = \alpha + \delta' a_i$

Condition 1a ensures that the researcher observes some one-off stimulus shock  $\varepsilon_s^g$ , where the aggregate path for the stimulus is pinned down purely by that one-off shock. This is a natural condition and merely ensures that the estimand of interest is interpretable as recovering the effect of some specific (unanticipated) stimulus policy.<sup>26</sup> Condition 2a is then familiar and is the key exogeneity requirement, ensuring that exposure to the stimulus is (mean) independent of underlying agent characteristics, conditional on controls.

**Proposition A1** *Assume (26) holds alongside Conditions 1a and 2a. Consider a cross-sectional regression for some single time-series observation ( $t = s$ ) of the form:*

$$y_{i,s+h} - y_{i,s-1} = \alpha + \beta^{ols}(z_i \cdot \varepsilon_s^g) + \lambda' a_i + e_i$$

---

<sup>26</sup>Note that  $g$  can be defined narrowly in this case – e.g. not as aggregate e.g. transfers or government spending, but just the portion of transfers or government spending that was adjusted at time  $s$  and which the researcher is interested in studying. This is in line with the typical interpretation in applied work which seeks to recover an estimate of the effect of some specific government stimulus shock – e.g. the ARRA stimulus package.

Then:

$$\beta^{ols} = \frac{E[\beta_{i,h}^y \omega_i]}{E[\omega_i]}$$

where  $\omega_i = E[(z_i)^2]$ .

**Proof:** First note that the path for the outcome variable can be written as:

$$y_{i,s+h} = \beta_{i,h} z_i \varepsilon_s^g + \zeta_{i,h} \varepsilon_s^g + \bar{y}_i + u_{i,s+h}$$

where the first term is the 'direct' effect, the second term is the 'GE' effect and  $u_{i,s+h}$  collects the effect of all other shocks. And so taking differences:

$$y_{i,s+h} - y_{i,s-1} = \beta_{i,h} z_i \varepsilon_s^g + \zeta_{i,h} \varepsilon_s^g + \tilde{u}_{i,s+h}$$

where  $\tilde{u}_{i,s+h}$  can again be written as a linear combination of all shocks other than  $\varepsilon_s^g$ . Given Condition 1a,  $\tilde{u}_{i,s+h}$  depends only on  $x_i$  (not on  $z_i$ ), since no other shocks operate via the  $g$ -channel. Turning then to the OLS estimand, and letting  $\tilde{z}_i$  denote the (population) residual from a regression of  $z_i$  on a constant and  $a_i$ :

$$\begin{aligned} \beta^{ols} &= \frac{E[(\beta_{i,h} z_i \varepsilon_s^g + \zeta_{i,h} \varepsilon_s^g + \tilde{u}_{i,s+h}) (\tilde{z}_i \varepsilon_s^g)]}{E[(\tilde{z}_i \varepsilon_s^g)^2]} \\ &= \frac{E[\beta_{i,h} z_i \tilde{z}_i]}{E[(\tilde{z}_i)^2]} + \frac{E[\zeta_{i,h} \tilde{z}_i]}{E[(\tilde{z}_i)^2]} + \frac{\varepsilon_s^g E[\tilde{u}_{i,s+h} \tilde{z}_i]}{(\varepsilon_s^g)^2 E[(\tilde{z}_i)^2]} \\ &= \frac{E[\beta_{i,h} z_i \tilde{z}_i]}{E[(\tilde{z}_i)^2]} \end{aligned}$$

where the final line follows uses:  $E[\zeta_{i,h} \tilde{z}_i] = E[\zeta_{i,h} E[\tilde{z}_i | x_i, a_i]] = 0$  and  $E[\tilde{u}_{i,s+h} \tilde{z}_i] = E[\tilde{u}_{i,s+h} E[\tilde{z}_i | x_i, a_i]] = 0$ . Following the same steps as the proof to Proposition 5, we then have:

$$\beta^{ols} = \frac{E[\beta_{i,h}^y \omega_i]}{E[\omega_i]}$$

where  $\omega_i = E[\tilde{z}_i^2 | x_i, a_i]$

□

## C Extension 2: Finite-n

I extend the results in Section 3 to cover settings with a finite population. This requires adjusting the environment. I discuss the adjustments to the environment in Sections C.1 and C.2, and then discuss identification in Section C.3.

### C.1 Environment

I now consider a setting with a finite population  $i \in \{1, 2, \dots, n\}$ , observed for periods  $t = \{1, 2, \dots\}$ . I keep the rest of the environment broadly the same, but now define  $\varepsilon_t^g$  as an  $n$ -length

vector of random variables:  $\varepsilon_t^g = [\varepsilon_{1t}^g, \dots, \varepsilon_{nt}^g]'$ , and likewise for other shocks so that  $\varepsilon_t^{-g}$  is  $(n_\varepsilon - 1) * n$  vector. I continue to assume shocks are (vector-wise) mutually independent – so e.g.  $\varepsilon_t^g \perp \varepsilon_t^{-g}$  – and *i.i.d.* over time but allow them to be non mean-zero. The unit-level outcome of interest  $y_t = [y_{1t}, \dots, y_{nt}]'$  evolve as before according to:<sup>27</sup>

$$y_t = \sum_{l=0}^{\infty} \Theta_l^y \varepsilon_{t-l} \quad (54)$$

where  $\varepsilon_t$  now denotes a  $(n_\varepsilon * n)$ - dimensional vector of shocks and  $\Theta_l^y$  is a  $n \times (n_\varepsilon * n)$  dimensional matrix.

## C.2 Object of Interest

Mirroring the previous section, to focus on the object of interest, the effect of an aggregate government stimulus shock  $\varepsilon_t^g$  on each unit at some horizon  $h$  can be decomposed as:

$$\Theta_{i,h,1}^y \varepsilon_t^g = \underbrace{\beta_{i,h}}_{\text{Direct Effect}} \varepsilon_{i,t}^g + \underbrace{\sum_{j \neq i} \zeta_{i,j,h}}_{\text{Spillover Effect}} \varepsilon_{j,t}^g \quad (55)$$

where  $\Theta_{i,h,1}^y$  is a  $1 \times n_\varepsilon$  length (row) vector that picks the relevant entries of  $\Theta_l^y$ , capturing the effect of government stimulus shocks  $\varepsilon_t^g$  on unit- $i$ . As before the object of interest is the direct effect  $\beta_{i,h}$ . In regional models this captures how the outcome of interest (e.g. income) in region- $i$  responds to higher government spending in region- $i$  (holding fixed spending in all other regions). Models of this form are fairly common in the analysis of fiscal spending – see e.g. [Flynn et al. \(2021\)](#).

## C.3 Identification

I proceed analogously to Section 3, first highlighting the specific challenges for identification that arise in this setting and then proceeding to my proposed solution.

**Proposition A2** Consider a setting where outcomes can be represented by (54) and government-spending shocks for each unit  $\varepsilon_{i,t}^g$  are mean-zero. Consider regressions of the form:

$$y_{i,t} = \beta^{ols} \varepsilon_{i,t}^g + e_{i,t} \quad (56)$$

Then:

$$\beta^{ols} = \underbrace{\frac{\sum_i \beta_{i,h} E[w_i]}{\sum_i E[w_i]}}_{\text{ATT}} + \underbrace{\frac{\sum_i \sum_{j \neq i} \zeta_{i,j,h} E[\varepsilon_{i,t}^g \varepsilon_{j,t}^g]}{\sum_i E[w_i]}}_{\text{Bias from GE spillovers}}$$

<sup>27</sup>Since I now allow non mean-zero shocks I abstract from any additional steady-state term

where  $w_i = (\varepsilon_{i,t}^g)^2 > 0$ .

**Proof:** The OLS estimand in this case is:

$$\begin{aligned}\beta^{ols} &= \frac{E[\sum_i y_{i,t} \varepsilon_{i,t}^g]}{E[\sum_i (\varepsilon_{i,t}^g)^2]} \\ &= \frac{E[\sum_i (\beta_{i,h} \varepsilon_{i,t}^g + \sum_{j \neq i} \zeta_{i,j,h} \varepsilon_{j,t}^g + u_{i,t+h}) \varepsilon_{i,t}^g]}{E[\sum_i (\varepsilon_{i,t}^g)^2]}\end{aligned}$$

where  $u_{i,t+h}$  collects the effect of all other shocks other than  $\varepsilon_{i,t}^g$ . Since shocks are mutually independent and i.i.d,  $E[u_{i,t+h} \varepsilon_{i,t}^g] = 0$ . And so:

$$\beta^{ols} = \frac{\sum_i \beta_{i,h} E[(\varepsilon_{i,t}^g)^2]}{\sum_i E[(\varepsilon_{i,t}^g)^2]} + \frac{\sum_i \sum_{j \neq i} \zeta_{i,j,h} E[\varepsilon_{i,t}^g \varepsilon_{j,t}^g]}{\sum_i E[(\varepsilon_{i,t}^g)^2]} \quad \square$$

The bias arises since, even though  $\varepsilon_t^g$  is orthogonal to *other* shocks, government-spending shocks may be correlated across units. In a regional setting, for example, the bias would be large when government spending shocks are distributed across regions in such a way that regions with high- $\varepsilon_{i,t}^g$  simultaneously experienced higher (or lower) spending in regions with particularly close linkages, such that they were simultaneously more (or less) exposed to spillovers arising from the shock. Identification can alternatively proceed as in the previous section, by appropriately including controls that capture the *assignment* of government spending shocks across units (without knowledge of the true "linkages" between units):

**Proposition A3** Consider a setting where the shocks are assigned across units based on unit-characteristics according to the following 'rule':

$$\varepsilon_{i,t}^g = \delta' a_i + \eta_{i,t}$$

where  $a_i$  is a  $n_a$ -length vector of observables and  $\eta_t^g = [\eta_{1,t}, \dots, \eta_{m,t}]$  denotes a mean-zero, i.i.d vector of random variables where:

$$\forall j \neq i : \eta_{i,t} \perp \eta_{j,t}$$

$$\forall l : \eta_t \perp \varepsilon_{t-l}$$

Consider the following regression controlling for  $a_i$ :

$$y_{i,t+h} = \beta^{ols} \varepsilon_{i,t}^g + \lambda' a_i + e_{i,t}$$

Then:

$$\beta^{ols} = \frac{\sum_i \beta_{i,h} E[\omega_i]}{\sum_i E[\omega_i]}$$

where  $\omega_i = (\eta_{i,t})^2$ .

**Proof:** Consider first the OLS estimand from a regression of  $\varepsilon_{i,t}^g$  on  $a_i$ :

$$\begin{aligned} \delta^{ols} &= E \left[ \sum_i a_i a_i' \right]^{-1} E \left[ \sum_i a_i \varepsilon_{i,t}^g \right] \\ &= \left[ \sum_i a_i a_i' \right]^{-1} \sum_i a_i E \left[ \varepsilon_{i,t}^g \right] \\ &= \left[ \sum_i a_i a_i' \right]^{-1} \sum_i a_i (\delta' a_i + E[\eta_{i,t}]) \\ &= \delta \end{aligned}$$

And so by FWL:

$$\begin{aligned} \beta^{ols} &= \frac{E[\sum_i y_{i,t} \eta_{i,t}]}{E[\sum_i (\eta_{i,t})^2]} \\ &= \frac{E[\sum_i (\beta_{i,h} \varepsilon_{i,t}^g + \sum_{j \neq i} \zeta_{i,j,h} \varepsilon_{j,t}^g + u_{i,t+h}) \eta_{i,t}]}{E[\sum_i (\eta_{i,t})^2]} \\ &= \frac{E[\sum_i (\beta_{i,h} (\delta' a_i + \eta_{i,t}) + \sum_{j \neq i} \zeta_{i,j,h} (\delta' a_j + \eta_{j,t}) + u_{i,t+h}) \eta_{i,t}]}{E[\sum_i (\eta_{i,t})^2]} \end{aligned}$$

where the first line substitutes in the process for  $y_{i,t+h}$  from (54), collecting the effect of all other shocks other than  $\varepsilon_t^g$  in  $u_{i,t+h}$  and the second line uses the targeting rule. Since  $\eta_t$  are mutually independent and mean-zero  $\forall j \neq i : E[\eta_{j,t} \eta_{i,t}] = 0$ . And since  $\eta_t$  are independent of other shocks:  $\forall l : E[\eta_t u_{i,t+h}] = 0$ . So:

$$\beta^{ols} = \frac{\sum_i \beta_{i,h} E[(\eta_{i,t})^2]}{\sum_i E[(\eta_{i,t})^2]} \quad \square$$

Similar in spirit to Proposition 5, the implication of this result is that, when bias arises due to 'clustering' of government spending shocks across regions, this can be removed by controlling for the underlying unit-observables that feature in the targeting rule that drives the clustering. I demonstrate this with a quantitative model of fiscal spending in US states in Appendix E.

## D Two-Agent New Keynesian (TANK) Model

This appendix describes the Two-Agent New Keynesian (TANK) model used as an illustrative laboratory in the paper. The model comes directly from a graduate course taught by Eric

Sims, with full details helpfully provided online: [https://sites.nd.edu/esims/files/2024/04/notes\\_tank\\_sp2024.pdf](https://sites.nd.edu/esims/files/2024/04/notes_tank_sp2024.pdf). I reproduce the full model here for completeness and to make explicit the key modifications relative to the baseline framework.

## D.1 Overview

The economy is a standard New Keynesian model augmented with household heterogeneity. There is a unit mass of households. A fraction  $\alpha \in (0, 1)$  are unconstrained (“Ricardian”) households with full access to financial markets, while the remaining fraction  $1 - \alpha$  are constrained or hand-to-mouth households who consume their current disposable income each period. In order to implement the exercise from Section 4, I further decompose the hand-to-mouth agents such that a fraction  $\delta_2 \in (0, 1)$  receive some government cash transfer, while  $1 - \delta_2$  do not. Firms operate under monopolistic competition with Calvo-style price rigidity, monetary policy follows a Taylor rule, and fiscal policy consists of government spending financed through lump-sum taxes and transfers.

## D.2 Households

### D.2.1 Unconstrained households

A representative unconstrained household maximizes expected lifetime utility

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left( \frac{C_{u,t}^{1-\sigma}}{1-\sigma} - \theta \frac{N_{u,t}^{1+\chi}}{1+\chi} \right), \quad (57)$$

where  $C_{u,t}$  denotes consumption,  $N_{u,t}$  labor supply,  $\beta$  is the discount factor,  $\sigma$  is the inverse elasticity of intertemporal substitution, and  $\chi$  is the inverse Frisch elasticity.

The budget constraint (in real terms) is

$$C_{u,t} + b_{u,t} = w_t N_{u,t} + d_t + \delta_1 \mathcal{T}_t + (1 + i_{t-1}) \Pi_t^{-1} b_{u,t-1} - \tau_t, \quad (58)$$

where  $b_{u,t}$  denotes real bond holdings,  $w_t$  the real wage,  $d_t$  firm profits rebated lump-sum,  $g_t$  denotes lump-sum transfers,  $\tau_t$  taxes,  $i_t$  the nominal interest rate, and  $\Pi_t$  gross inflation. The additional parameter  $\delta_1 \in \{0, 1\}$  determines whether or not the unconstrained households receive the government transfer  $\mathcal{T}_t$ .

The optimality conditions are:

$$\theta N_{u,t}^\chi = C_{u,t}^{-\sigma} w_t, \quad (59)$$

$$1 = \mathbb{E}_t \left[ (1 + i_t) \Lambda_{u,t,t+1} \Pi_{t+1}^{-1} \right], \quad (60)$$

$$\Lambda_{u,t,t+1} = \beta \left( \frac{C_{u,t+1}}{C_{u,t}} \right)^{-\sigma}, \quad (61)$$

where  $\Lambda_{u,t,t+1}$  denotes the stochastic discount factor.

## D.2.2 Constrained households

Constrained households have identical preferences but cannot borrow or save and do not own firms. I distinguish between ‘treated’ and ‘control’ constrained households – where the former receive a government transfer while the latter do not. The representative ‘treated’ household solves:

$$\max_{C_{h,t}^d, N_{h,t}^d} \frac{(C_{h,t}^d)^{1-\sigma}}{1-\sigma} - \theta \frac{(N_{h,t}^d)^{1+\chi}}{1+\chi} \quad (62)$$

subject to

$$C_{h,t}^d = w_t N_{h,t}^d + \mathcal{T}_t - \tau_t \quad (63)$$

And the representative ‘control’ household solves:

$$\max_{C_{h,t}^c, N_{h,t}^c} \frac{(C_{h,t}^c)^{1-\sigma}}{1-\sigma} - \theta \frac{(N_{h,t}^c)^{1+\chi}}{1+\chi} \quad (64)$$

subject to

$$C_{h,t}^c = w_t N_{h,t}^c - \tau_t \quad (65)$$

Aggregate consumption and labor for constrained households are then

$$C_{h,t} = \delta_2 C_{h,t}^d + (1 - \delta_2) C_{h,t}^c, \quad (66)$$

$$N_{h,t} = \delta_2 N_{h,t}^d + (1 - \delta_2) N_{h,t}^c. \quad (67)$$

## D.3 Firms and Price Setting

The model features a representative competitive final goods firm alongside monopolistically competitive intermediate goods producers. A fraction  $1 - \phi$  of intermediate goods producers can reset prices each period. Optimal price setting for intermediate producers yields:

$$p_t^\# = \frac{\varepsilon}{\varepsilon - 1} \frac{x_{1,t}}{x_{2,t}}, \quad (68)$$

$$x_{1,t} = mc_t Y_t + \phi \mathbb{E}_t [\Lambda_{u,t,t+1} \Pi_{t+1}^\varepsilon x_{1,t+1}], \quad (69)$$

$$x_{2,t} = Y_t + \phi \mathbb{E}_t [\Lambda_{u,t,t+1} \Pi_{t+1}^{\varepsilon-1} x_{2,t+1}], \quad (70)$$

where  $p_t^\#$  denotes the optimal relative reset price,  $Y_t$  denotes aggregate production and  $mc_t$  denotes real marginal cost:

$$mc_t = \frac{w_t}{A_t}. \quad (71)$$

where  $A_t$  denotes aggregate productivity. Price dispersion  $v_t$  evolves according to:

$$v_t = (1 - \phi)(p_t^\#)^{-\varepsilon} + \phi \Pi_t^\varepsilon v_{t-1}, \quad (72)$$

$$1 = (1 - \phi)(p_t^\#)^{(1-\varepsilon)} + \phi \Pi_t^{(\varepsilon-1)}; \quad (73)$$

Finally, aggregate production satisfies:

$$Y_t v_t = A_t N_t. \quad (74)$$

## D.4 Monetary and Fiscal Policy

Monetary policy follows a Taylor rule with interest rate smoothing:

$$i_t = (1 - \rho_i)i + \rho_i i_{t-1} + (1 - \rho_i)\phi_\pi(\log \Pi_t - \log \Pi) + s_i \varepsilon_t^i. \quad (75)$$

There is no borrowing and so the government budget constraint is:

$$G_t + \alpha \delta_1 T_t + (1 - \alpha)\delta_2 T_t = \tau_t \quad (76)$$

where  $G_t$  denotes government spending on the production good.

## D.5 Aggregation and Market Clearing

Aggregate labor and consumption are:

$$N_t = \alpha N_{u,t} + (1 - \alpha)N_{h,t}, \quad (77)$$

$$C_t = \alpha C_{u,t} + (1 - \alpha)C_{h,t}. \quad (78)$$

The resource constraint is:

$$Y_t = C_t + G_t. \quad (79)$$

## D.6 Exogenous Processes

Productivity, government spending and transfers follow AR(1) processes:

$$\log A_t = \rho_A \log A_{t-1} + s_A \varepsilon_t^A, \quad (80)$$

$$\log G_t = (1 - \rho_G) \log G + \rho_G \log G_{t-1} + s_G \varepsilon_t^G, \quad (81)$$

$$\mathcal{T}_t = (1 - \rho_h)\mathcal{T} + \rho_h \mathcal{T}_{t-1} + s_{\mathcal{T}} \varepsilon_t^{\mathcal{T}} \quad (82)$$

## D.7 Steady State

The model is approximated about a zero-inflation steady-state:  $\Pi = 1$ . Government spending is a fixed-fraction of output in steady-state:  $G = \psi_G Y$ . And there are no government transfers in steady-state:  $\mathcal{T} = 0$ .

## D.8 Calibration

Table 5 reports the parameter values used in the simulations.<sup>28</sup> I keep these parameter values constant and then display results for different values of  $\delta_1$  and  $\delta_2$  in the main text. I solve for impulse responses via first-order perturbation using Dynare.

## D.9 Extension

I consider an extension to the model where transfers instead follow a Taylor rule of the form:

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<sup>28</sup>I follow closely the "standard" values for parameters employed in the illustrative simulations from Eric Sims - see [https://sites.nd.edu/esims/files/2024/04/notes\\_tank\\_sp2024.pdf](https://sites.nd.edu/esims/files/2024/04/notes_tank_sp2024.pdf)

Table 5: Calibration: TANK Model Parameters

Parameter	Value	Description
$\beta$	0.99	Discount factor
$\sigma$	1	Inverse EIS
$\chi$	1	Inverse Frisch elasticity
$\theta$	1	Utility weight on labor
$\alpha$	0.6	Share of unconstrained households
$\phi$	0.75	Calvo price stickiness
$\varepsilon/(\varepsilon - 1)$	1.1	Desired markup of price over marginal cost
$\psi_G$	0.2	Govt. spending share of steady-state output
$\phi_\pi$	1.5	Taylor rule inflation coefficient
$\rho_A$	0.9	Persistence of productivity
$\rho_G$	0.8	Persistence of government spending
$\rho_i$	0.8	Interest rate smoothing
$\rho_T$	0.5	Persistence of transfers
$s_A$	0.01	Std. dev. productivity shock
$s_G$	0.01	Std. dev. government spending shock
$s_i$	0.0025	Std. dev. monetary shock
$s_T$	0.01	Std. dev. transfer shock

$$\mathcal{T}_t = (1 - \rho_T)\mathcal{T} + \rho_T\mathcal{T}_{t-1} + (1 - \rho_T)\phi_T(\log \Pi_t - \log \Pi) + s_T\varepsilon_t^T. \quad (83)$$

For this exercise, I keep parameter values the same as above and set  $\phi_T = 2.0$ .

## E Fiscal Spending in Network Model

This Appendix describes a simple quantitative model to illustrate the results from Appendix C. The discussion links closely to the empirical application from Section 6.

**Setting** Each region  $i = 1, \dots, n$  is endowed with  $q_i$  units of a distinct good. All agents are able to save via a bond available at price  $p_s$  and in fixed next supply:  $\bar{s}$ . There is a representative agent in each region with Cobb-Douglas preferences:

$$u_i(c_{1i}, \dots, c_{ni}, s_i) = \alpha_{1i}\log(c_{1i}) + \dots + \alpha_{ni}\log(c_{ni}) + \alpha_{s,i}\log(s_i)$$

where  $c_{ji}$  denotes the consumption of region  $i$  of the  $j$ th good and  $s_i$  denotes savings of region  $i$ . The government spends  $g_i$  on each region's good and raises taxes  $\tau_i$  in each region subject to the budget constraint:

$$\sum_i g_i = \sum_i \tau_i$$

**Competitive Equilibrium** Define (nominal) income for each region as:  $y_i = p_i q_i$ . Consumer demand, budget constraints and market clearing implies that for each region  $i$ :

$$y_i = \sum_{j=1}^n [\alpha_{ij} \underbrace{(1 - \alpha_{sj})}_{\equiv mpc_j} (y_j - \tau_j)] + g_i$$

And so the relationship between income, government spending, and taxes in all regions can be summarised as:

$$\underbrace{y}_{n \times 1} = (I - A)^{-1} \cdot \underbrace{g}_{n \times 1} - (I - A)^{-1} A \cdot \underbrace{\tau}_{n \times 1}$$

where  $A_{ij} = \alpha_{ij} * mpc_j$ . Note the effects of government spending in this simple model exhibit a Keynesian-cross-style logic: higher government spending in region 1 directly increases incomes in region 1, which increases their spending on all goods, thereby increasing incomes in other regions, which further increases spending on all goods (and so on). The effect of higher government spending in region  $j$  on region  $i$  therefore depends on the direct linkage between  $i$  and  $j$  (governed by  $\alpha_{ij}$ ), as well as all indirect linkages (e.g.  $\alpha_{ik} * \alpha_{kj}$  etc.) - where the sum of all these linkages is captured by the appropriate entry in the Leontief-inverse multiplier matrix  $(I - A)^{-1}$ . Note that given taxes are constrained to be equal across regions, the model can be written:

$$y = \mathcal{M}^g g$$

where  $\mathcal{M}^g = (I - A)^{-1} - (1/n)(I - A)^{-1} A J_n$ . I assume the econometrician has access to repeated observations  $t = [1, 2, \dots, T]$  from this static model, and further assume that *observed* incomes  $\tilde{y}_t$  are subject to some idiosyncratic (stochastic) measurement error in each state which I denote  $u_t = [u_{1,t}, \dots, u_{n,t}] \sim i.i.d. N(0, \sigma_u I_n)$ , such that:

$$\tilde{y}_t = \mathcal{M}^g g_t + u_t$$

**Calibration** I consider a simple parametrization of the model that satisfies rotational symmetry. I assume there are 50 regions, with each spending 60% of their income on their own good ( $\forall i : \alpha_{ii} = 0.6$ ) and MPCs of 50% ( $\forall i : \alpha_{si} = 0.5$ ). The proportions spent on other regions are inversely proportional to the 'distance' between each region, as if the regions were evenly spaced around a unit-circle i.e. -

$$\forall i \neq j : \alpha_{ij} = (1 - \alpha_{ii}) \cdot \frac{w_{ij}}{\sum_{k \neq i} w_{ik}}$$

where  $w_{ij}$  captures the 'distance' between each region:

$$w_{ij} = \frac{1}{\sqrt{2(1 - \cos(\theta_i - \theta_j))}}$$

and  $\theta_i$  captures the 'location' of each unit on the circle:

$$\theta_i = \frac{2\pi(i-1)}{N}$$

In this calibration, regions satisfy rotational symmetry ensuring all diagonal elements of the  $\mathcal{M}^g$  matrix are the same:

$$\beta_{i,h} = [\mathcal{M}^g]_{ii} = \beta^*$$

where, for this calibration,  $\beta^* \approx 1.4$  (broadly consistent with local fiscal multipliers estimated in the literature). I consider a simple assignment rule for spending shocks which targets spending towards regions implicitly 'located' in one-half of the unit-circle - i.e.:

$$\varepsilon_{i,t}^g = \delta D_i + \eta_{i,t} = \delta \mathcal{I}\{i \in [1, \dots, 25]\} + \eta_{i,t} \quad (84)$$

where  $\eta_t = [\eta_{1,t}, \dots, \eta_{n,t}] \sim i.i.d. N(0, \sigma_\eta I_n)$ .

**Identification** I consider cross-sectional regressions of the form:

$$y_{i,t} = \beta_s \varepsilon_{i,t}^g + e_{i,t} \quad (85)$$

$$y_{i,t} = \beta_l \varepsilon_{i,t}^g + \gamma D_i + u_{i,t} \quad (86)$$

And consider consider the large-sample bias of the estimators across simulations i.e.  $E[\hat{\beta} - \beta^*]$  for  $T = 5000$ .

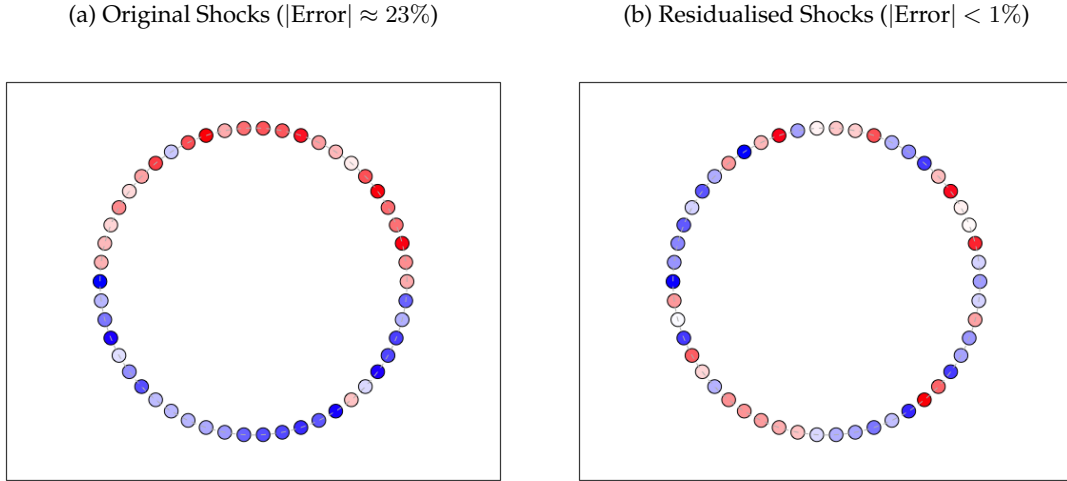
**Results** Figure 5 shows intuition for the bias that can arise in this case and the correction. Figure 5a shows the distribution of shocks across regions for a single draw of  $[\varepsilon_t^g, u_t]$ . In this case,  $\hat{\beta}_s$  is much larger than the true local multiplier since regions who experience high- $\varepsilon_i^g$  simultaneously experience a greater rise in external demand for their exports since their close-trading partners also tend to be experience large shocks. Figure 5b shows the distribution of the residualised shocks (i.e. residualised with respect to  $D_i$ ). Since the residualisation removes the regional clustering,  $\hat{\beta}_l$  is much closer to the true local multiplier.

Table 6 shows the bias of different estimators in simulations. The 'short' regression overestimates the local multiplier by over 20%, while the 'long' regression that controls for  $D_i$  has negligible bias.

Estimator	Bias (%)
$\beta_s$	22.55%
$\beta_l$	-0.21%

Table 6: Mean bias of estimators in Monte Carlo simulation.

Figure 5: Bias-correction with regionally clustered shocks



Notes: Blue denotes lower values of the shock while red denotes higher values. Left hand-side chart plots  $\varepsilon_{i,t}^g$  for a single draw of the shocks and computes the error as:  $\hat{\beta}_s - \beta^*$ . Right hand-side chart plots  $\varepsilon_{i,t}^g$  residualised with respect to  $D_i$  and computes the error as:  $\hat{\beta}_l - \beta^*$ .

## F Additional Empirical Results

This Appendix collects additional results from the empirical application in Sections 5.

### F.1 Nakamura and Steinsson (2014)

I show additional empirical results for the empirical application from Nakamura and Steinsson (2014). Specifically, following Nakamura and Steinsson (2014), I estimate IV-regressions of the form:

$$\frac{y_{i,t} - y_{i,t-2}}{y_{i,t-2}} = \alpha_i + \delta_t + \beta \frac{g_{i,t} - g_{i,t-2}}{g_{i,t-2}} + u_{i,t} \quad (87)$$

where I instrument the right-hand side variable using total national procurement,  $\varepsilon_t^g$  interacted with a state dummy. I then additionally estimate IV-specifications of the form:

$$\frac{y_{i,t} - y_{i,t-2}}{y_{i,t-2}} = \alpha_i + \delta_t + \beta \frac{g_{i,t} - g_{i,t-2}}{g_{i,t-2}} + \lambda'(a_i \cdot \varepsilon_t^g) + u_{i,t} \quad (88)$$

where the control variables are intended to additionally purge any variation in the instrument of regional heterogeneity in response to objects that adjust in GE in response to the shock  $\varepsilon_t^g$ . Table 7 displays my results for different control variables  $a_i$ .

### F.2 Chodorow-Reich (2019)

I show additional empirical results for the empirical application from Chodorow-Reich (2019).

Table 7: Replication and Extension of Nakamura and Steinsson (2014) - Alternate Specification

	Dependent Variable: Output			
	(1)	(2)	(3)	(4)
Prime military contracts	1.43*** (0.36)	1.22*** (0.37)	1.22*** (0.33)	1.02*** (0.34)
Additional Controls	None	$mpc_i, open_i$	$mpc_i, inc_i$	$mpc_i, open_i, inc_i$
Observations	1989	1989	1989	1989

### F.2.1 Clustering of residualised shocks

Table 8 reports results estimating regional clustering of the residualised shocks employed in the main text. Figure 6 plots heatmap for these residualised shocks.

Table 8: Predictability of Residualised Shocks

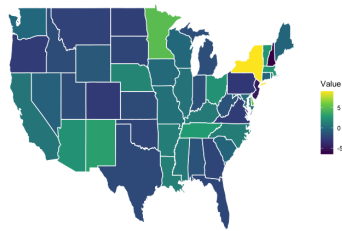
	Dependent Variable: Residualised Shocks ( $\eta_i^g$ )		
	(1)	(2)	(3)
Distance-Weighted Residualised-Shocks	-0.34 (0.20)	0.12 (0.19)	-0.02 (0.19)
Adjusted $R^2$	-0.02	-0.01	-0.02
Instrument	FMAP	DOT	DM
Observations	50	50	50

*Note:* Table displays coefficient estimates alongside (heteroskedasticity-robust) standard errors and p-values for estimates of  $\gamma$  from (49) with the original shocks  $\varepsilon^g = [\varepsilon_1^g, \dots, \varepsilon_n^g]$  replaced with the residualised shocks from (51),  $\eta = [\eta_1, \dots, \eta_n]$ . Each column refers to estimates from the FMAP, DOT and DM instrument respectively. Significance at the 10%, 5%, and 1% levels are denoted by \*, \*\*, and \*\*\* respectively.

Figure 6: Heatmaps of Residualised Instruments Across Studies

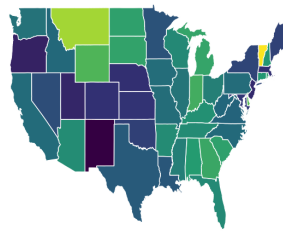
(a) Chodorow-Reich et al. (2012)

Distribution of Residualised FMAP instrument



(b) Wilson (2012)

Distribution of Residualised DOT instrument



(c) Dupor and Mehkari (2016)

Distribution of Residualised DM instrument

