Should we trust cross sectional multiplier estimates?

Fabio Canova (Norwegian Business School, CAMP and CEPR)

August 18, 2022

Outline

Introduction

- Common in applied macro to compute average fiscal multipliers, monetary pass-through or elasticities with a **cross sectional** methodology (e.g. Nakamura and Steinsson, 2014, Chodorow, 2019; Peydro et al., 2014, Chakraborty et al., 2020).
- Shift from standard **time series** methods. Why? Two problems:

  Clean up the endogenous component

  - Need exogenous conditioning variables; but fiscal and monetary variables adjust to the trajectory of the economy. High frequency proxies (Gertler and Karadi, 2015) not typically available over a cross section of units.
  - Policy variables move together: if government expenditure increases, taxes, debt, or the term structure of nominal rates may simultaneously be affected. Estimate measure average dynamic **within** policy regimes.
- A time effect eliminates endogeneity and absorbs policy comovements.
- Meaningful estimates obtained by exploiting **random variations across units**, which are easier to justify than **random changes across time**.

Punchline

- There is no free lunch with cross-sectional methods.
  - If the conditional dynamic evolution of units is **homogeneous**, methodology delivers consistent estimates under a set of restrictive assumptions. Numerous odds-and-ends to be taken care of.
  - If the conditional dynamic evolution of units is **heterogeneous**, inconsistent estimates are obtained.
- Alternatives to measure average effects that work under both dynamic homogeneity and dynamic heterogeneity exist.
- Monte Carlo experiment shows the biases.
- Can use a diagnostic and an asymptotic test for the null of dynamic homogeneity.
- Application: Measure the average multiplier of US state spending.

Related the literature

- Stock and Watson, 2018: LP-IV results biased when conditioning variables improperly specified, unless instruments are exogenous. Here: without the proper conditioning variables, multiplier estimates inconsistent, unless the instruments are sufficiently lagged, even with dynamic homogeneity.
- Sun and Abraham, 2021: diff-in-diff estimates contaminated without time homogenous treatment effects. Here: when policy changes have heterogeneous dynamic effects, IV estimates of coefficients inconsistent.
- Sun and Abraham's point is developed in an intrinsically static model which does not allow for network or common effect, and it is potentially heterogeneous only in the timing of the treatment.
- Pesaran an Smith, 1996, Pesaran et al., 2006: (classical) dynamic heterogeneous panel approaches.
- Hsiao et al., 2019; Liu et al, 2020: shrinkage methods.

Outline

The cross section methodology I

- $i = 1, \ldots, N$ (states, sectors, banks). Sample size: T_i; $T = min_i T_i$.
- (Local) projection equation:

assuming homogeneity

$$Y_{i,t+h} - Y_{i,t} = \delta_{i,h} + \alpha_{t,h} + \beta_h P_{i,t} + \gamma_h X_{i,t,h} + e_{i,t+h} \tag{1}$$

  $P_{i,t}$ is the policy variable; $X_{i,t,h}$ a vector of controls (may change with h); $Y_{i,t}$ the relevant variable for the unit i at time t, $\alpha_{t,h}$ a time effect, $\delta_{i,h}$ the fixed effect, h the horizon of interest.

- β_h is the **cross sectional** multiplier (elasticity, pass-through) at horizon h: given $\alpha_{h,t}$, its identification comes from variations in $P_{i,t}$ across i, given t.
- Variations on (1):
  - Fixed effects $\delta_{i,h}$ and controls $X_{i,t,h}$ may be excluded.
  - $Y_{i,t}, P_{i,t}$ may be measured in per-capita terms.
  - $P_{i,t+h} - P_{i,t}$ may be used as policy variable (Nakamura and Steinsson, 2014).
  - $Y_{i,t+h} - Y_{i,t}$ and $P_{i,t}$ may be scaled by $Y_{i,t}$ (Dupour, 2017), or by its trend component (Ramey and Zubairy, 2018) (when fiscal multipliers are of interest).

The cross sectional methodology II

- When (1) is the DGP, $\hat{\beta}_h$ is consistent if, conditional on $X_{i,t,h}$, variations in $P_{i,t}$ at each t uncorrelated with the trajectory of $Y_{i,t+h} - Y_{i,t}$ across i.

- Because $P_{i,t}$ is not necessarily strictly exogenous, even with $\alpha_{h,t}$, need instruments $Z_{i,t,h}$ satisfying orthogonality ($E_t[Z_{i,t,h} e_{i,t+h} | X_{i,t}] = 0$) and relevance $[Z_{i,t,h} P_{i,t} | X_{i,t}] \neq 0$): IV-2SLS estimation.

- $Z_{i,t,h}$ may be change with h.

- Cumulative multipliers (setting $X_{i,t,h} = X_{i,t}$);

$$\sum_{h=1}^{H} (Y_{i,t+h} - Y_{i,t}) = \alpha_t + \beta P_{i,t} + \gamma X_{i,t} + \sum_{h=1}^{H} e_{i,t+h} \tag{2}$$

  where $\alpha_t = \sum_{h=1}^{H} \alpha_{h,t}, \beta = \sum_{h=1}^{H} \beta_h, \gamma = \sum_{h=1}^{H} \gamma_h$.

- β is the **cumulative effect** of changes in $P_{i,t}$ up to horizon H.

Cross sectional contributions (short and incomplete list)

- Moretti, 2010 (AER), Bruckner and Taladhar, 2014 (EJ), Dupour and Guerrero, 2017 (JME) Leduc et al.,2017 (AEJ:PP) , Guren et al, 2020 (NBER MacAnn): average effect of fiscal expenditure.
- Carpinelli and Crosignani, 2021, (JFE), Peydro et al, 2021 (JFE) Chodorow, 2014 (Brookings): average effect of monetary policy.
- Criscuolo et al, 2019 (AER): average effect of industrial policy.
- Mian and Sufi, 2014 (Econometrica): average employment consequences of net worth decline.
- McLeay and Terneyro, 2020 (NBER MacAnn): average Phillips curves.

A spatial dynamic DGP I

- (1) is misspecified as DGP for spatial macroeconomic data.
  - Neglects interdependencies across units (network effects, contagion);
  - Disregards general equilibrium constraints within a unit;
  - Omits common effects;
  - Restricts the response of $Y_{i,t+h} - Y_{i,t}$ to a policy change to be the same across units.
- Alternative DGP:

may be a panel VAR, where each y is a vector

$$y_t = A + B(L)y_{t-1} + C(L)w_t + u_t$$

$y_t = [y'_{1t}, \ldots, y'_{Nt}]'$; y_t is a $(G+1)N \times 1$ vector; w_t is a $M \times 1$ vector of aggregate variables, u_t $iid \sim (0, \Sigma)$, L lag operator.

- Can be given a DSGE or VAR justification.

A spatial dynamic DGP II

- Representation for unit i (B(L)=B):

$$y_{i,t} = a_i + b_i y_{i,t-1} + c_i p_{i,t} + d_i x_{i,t} + u_{i,t} \tag{3}$$

$p_{i,t}$ is the policy variable, $x_{i,t} = [y_{j,t-1}, j \neq i; w_t, w_{t-1}, \ldots]$. $\mathrm{cov}(u_{i,t}) = \Sigma_i$.

- Lag (state) dependent variable: Anderson and Hsiao (1982), Arellano and Bond (1991), Ahn and Schmidt (1993) , etc.
- Social networks (spatial interactions, peer effects, contagion): Anselin (1988), Manski (1993)-(1995), Rigobon (2003) .
- Common factors: Favero et al (2005), Heckman (2006), Sarafidis and Wennsbeck (2021).
- Coefficients in (3) generally unit specific.
- **If (3) is the DGP, when is (1) correctly specified? If misspecification occurs, what are the (inconsistency) consequences?**

Restrictions for correct specification

- Set $b_i = b, c_i = c, \ \forall i$ (dynamic homogeneity).

Results 1: Projection model (1) misspecified unless:

i) domestic interactions do not matter $(G = 1)$;

ii) there are no network effects $(y_{jt-1} = 0, \forall j)$; no feedback across units

iii) effect of controls is identical across i $(d_i = d)$;

iv) the DGP is non-stationary $(b_i = I)$;

vi) the policy variable is an impulse, i.e. $p_{i,t+1} = 1$, and $p_{i,t+h} = 0$, for $h > 1$, fixed t.

Inconsistencies in β_h estimates

- If i)-ii) do not hold: omitted variables; require no within unit GE effects; units behave like islands. OLS and IV potentially inconsistent.
- If iii) fails: there is correlation between the regressors and error term in (1) which is hard to break, even with IV.
- ii) and iii) imply $\alpha_{h,t} = d \sum_{m=0}^{h-1} b^m x_{t+h-m}$. Time effect must be a MA of exogeneous variables. Change with t and horizon. It implies that β_h identified not at a point in time but around a (discounted) average changing with h.
- If iv) fails: overdifferencing. Both OLS and IV are inconsistent.

Additional issues

- If v) fails: β_h captures the average effect of (discounted) cumulative changes in $p_{i,t}$: $P_{i,t} = (1 + b_i L + b_i^2 L^2 + \ldots + b_i^{h-1} L^{h-1}) p_{i,t+h}$.
  - Need a policy impulse, i.e. $p_{i,t+1} = 1$, for h=1, and $p_{i,t+h} = 0$, for $h > 1$; otherwise overestimation of the true β_h if $p_{i,t}$ is positively serially correlated.
  - Using $P_{i,t+h} - P_{i,t}$ in (1) does not help.
- Error term $e_{t+h} = (1 + b_i L + b_i^2 L^2 + \ldots b_i^{h-1} L^{h-1}) u_{i,t+h}$: a HAC correction is needed for the standard errors of the $\hat{\beta}_h$.
- IV estimation generally problematic:
  - If $Z_{i,t,h}$ uncorrelated with $u_{i,t+h}$ but not be necessarily with $e_{i,t+h}$. Need $Z_{i,t-\tau,h}, \tau > h$.
  - If network effects disregarded ($y_{j,t-1}$ omitted from $x_{i,t}$), $Z_{i,t}$ invalid as it may be correlated with $e_{i,t+h}$ due to variable omission.

A more important problem

- Cross sectional methods require $b_i = b, c_i = c \ \forall i$ for identification.
- Need to construct homogeneous groups of units to use them, see Canova (2004), Bonhomme and Manresa (2015).
- Clustering based on standard features mail fail, Altavilla et al. (2020).
- Fixed effects can not account for dynamic heterogeneity.
- **What happens to cross sectional estimates of β_h with dynamic heterogeneity?**

$$b_i = b + v_i \quad v_i \ iid(0, \Sigma_b) \tag{4}$$

$$c_i = c + \nu_i \quad \nu_i \ iid(0, \Sigma_c) \tag{5}$$

b, c are the common components; v_i, ν_i the unit specific components, uncorrelated with all the leads and lags of $p_{i,t}$ and $x_{i,t}$.

- Σ_b, Σ_c capture the degree of cross-sectional dynamic heterogeneity; $\Sigma_b = \Sigma_c = 0$ homogeneity; $\Sigma_b = \infty$ or $\Sigma_c = \infty$, completely heterogeneity.

Cross sectional estimation with dynamic heterogeneity

Result 2. Let $G = 1, y_{jt-1} = 0, d_i = d$, $p_{i,t+1} = 1$, and $p_{i,t+h} = 0$, for $h > 1$.

i) If $c_i = c$, cross-sectional average OLS and IV estimators of β_h are inconsistent, each h.

ii) If $b_i = b$, cross-sectional average OLS and IV estimators of β_h are inconsistent, each h.

iii) If $b_i = b_k$ (or $c_i = c_k$), $k = 1, \ldots K < N$, i) (ii)) still holds.

iv) If v_i *iid* $\sim (fH_i, \Sigma_b)$, (ν_i *iid* $\sim (gH_i, \Sigma_c)$) where H_i are time invariant non-observables, i) (ii)) still holds.

Intuition

$$y_{i,t+h} - b^h y_{i,t} = a_i \sum_{m=0}^{h-1} b^m + c \sum_{m=0}^{h-1} b^m p_{i,t+h-m} + d \sum_{m=0}^{h-1} b^m x_{i,t+h-m} + \zeta_{i,t+h} \quad (6)$$

where

$$\zeta_{i,t+h} = v_i^h y_{i,t} + a_i \sum_{m=0}^{h-1} v_i^m + c \sum_{m=0}^{h-1} v_i^m p_{i,t+h-m} + d \sum_{m=0}^{h-1} v_i^m x_{i,t+h-m} + e_{i,t+h} \quad (7)$$

Let $p_{i,t+1} = 1, p_{i,t+h} = 0, h > 1$.

- OLS applied to (6) inconsistent for cb^{h-1} because the regressors of (6) and the composite error $\zeta_{i,t+h}$ are correlated.
- IV does not work. Proper instruments will be hard to find because $x_{i,t+h}$ and $p_{i,t+h}$ appear as regressors and in the composite error.
- Same conclusion holds if $b_i = b$ but $c_i \neq c$ (heterogeneous impact effect), see also Sun and Abraham, 2021.
- Same conclusion holds if there is residual within group heterogeneity or if there are unobservable covariates determining parameter heterogeneity.

Interaction dummies

- Typical to use interaction dummies to capture heterogeneity.
- Approach reduces the inconsistency problem only if the interaction terms capture the cross correlation of *both* $(y_{i,t}, x_{i,t+h})$ and $p_{i,t+h}$ with v_i.
- With $K << N$ it is unlikely that the problem will disappear.
- Specification errors will still be present.
- Inconsistencies will still be produced.

Relationship with Sun and Abraham, 2021

$$Y_{i,t} = \delta_i + \alpha_t + \sum_{g \in G} \beta_g \mathbf{1}[t - E_i \in g] + e_{i,t} \tag{8}$$

- G collects disjoint sets of relative periods $\ell = [-T, T]$; E_i = time for unit i to receive the treatment effect
- Static model with no common factors, nor interdependencies.
- Differences in the time when units are treated.
- No intrinsic dynamic (one δ_i, one α_t)
- $\beta_{g,OLS}$ does not coverge as $N, T \to \infty$ to true β_g Contamination from different time periods.
- Suggest "group" OLS estimator: group unit treated at the same time.

Outline

Alternative I: Average time series estimator

You need to group (people) by their dynamic properties (?)

- Estimate

$$Y_{i,t+h} = \delta_{i,h} + \beta_{i,h} P_{i,t} + \gamma_{i,h} X_{i,t,h} + \lambda_{i,h} Y_{i,t-1} + e_{i,t+h} \quad (9)$$

Compute $\hat{\beta}_h = \sum_{i=1}^{N} \hat{\beta}_{i,h}$. Consistent for $T, N \to \infty$.

- Alternative: run a VAR on $(Y_{i,t}, P_{i,t})$. Identify a policy shock. Compute responses and average.
- Under homogeneity, $\hat{\beta}_h$ the same as cross sectional estimator but less efficient.
- Could use ω_i to get a weighted mean (to trim outliers) or to weight different units with different policy variability.
- **Produce the cross-sectional distribution. Can cut it various ways to see sources of asymmetries. Allow economic analyses.**
- Similar to Pesaran and Smith (1996). Used by Imbs et al. (2005), De Graeve et al. (2007), Ilzeztki (2020).

Alternative II: Partial pooling estimator I

Bayesian pooling / Suami (?) estimator [Suami est.=nothing but a penalized estimator]

- Estimate (9), unit by unit using (4)-(5) as an exchangeable prior.
- Regularization of individual estimates via shrinkage.
- Compute $\tilde{\beta}_h = \sum_{i=1}^{N} \tilde{\beta}_{i,h}$.
- If N is large, small changes in estimates of β_h.
- Let ϕ_i be the parameters of the projection model for unit i. Assume:

$$\phi_i = \phi_0 + v_i \quad v_i \; iid \; N(0, \Sigma_v) \tag{10}$$

- Let $Q_{i,t}$ the vector of right hand side variables and $R_{i,t}$ the dependent variable of (9) and $e_{it+h} \sim N(0, \sigma_i^2)$. Let (3) be the DGP.

Alternative II: Partial pooling estimator II

works pretty well when T=12 (with 50 countries) (based on Monte Carlo in some Canova paper)

Result 3.

i) Conditional on $(\sigma_i^2, \phi_0, \Sigma_v)$, ϕ_i is normal with mean $\tilde{\phi}_i = (\frac{1}{\sigma_i^2} Q_i' Q_i + \Sigma_v^{-1})^{-1} (\frac{1}{\sigma_i^2} Q_i' Q_i \hat{\phi}_i + \Sigma_v^{-1} \phi_0)$, and variance $\tilde{\Sigma}_{\phi_i} = (\frac{1}{\sigma_i^2} Q_i' Q_i + \Sigma_v^{-1})^{-1}$, where $\hat{\phi}_i$ is the estimator of ϕ_i with unit i data.

ii) If σ_i^2 has an inverted gamma distribution, with scale parameter σ_0 and degrees of freedom ν_0, conditional on (ϕ_0, Σ_v) ϕ_i is t-distributed with mean $\tilde{\phi}_i$, variance $\frac{\sigma_{i,T}}{\nu_T} \tilde{\Sigma}_i^2$, and degrees of freedom $2\nu_0 + T$, where $\sigma_{i,T} = \sigma_0 + \frac{1}{2}(\phi_0' \Sigma_v^{-1} \phi_0 + R'R - \tilde{\phi}_i' \tilde{\Sigma}_\phi^{-1} \tilde{\phi}_i); \nu_T = \nu_0 + \frac{T}{2}$

iii) For T large, $\tilde{\phi} \to \hat{\phi}_T = \sum_{i=1}^N \kappa_i \hat{\phi}_i$ a (weighted) average time series estimator, for $0 < \kappa_i < 1, \sum_i \kappa_i = 1$.

Alternative II: Partial pooling estimator III

- Used in Canova (2005), Canova and Pappa (2007).
- A weighted average of the information in unit i data and in ϕ_0, with weights given by the precision of the two types of information.
- If Σ_v is large; $\tilde{\phi}_i \to \hat{\phi}_i$; if $\Sigma_v \to 0$, $\tilde{\phi}_i \to \phi_0$. For ϕ_0 is appropriately chosen, this is the same as cross sectional estimator.
- Normality only used to obtain closed form solutions. Same estimator if use GLS with uncertain linear restrictions.
- With t-errors the estimator form is unchanged.
- Get ϕ_0 and Σ_v (and σ_i^2) from a presample; different cross sections; aggregate data, etc.
- Compare with composite likelihood estimator, Canova and Matthes (2021).

Outline

Test for homogeneity

- Hausman test:

$$H = (\phi_{cs} - \phi_{ts})'(var(\phi_{ts}) - var(\phi_{cs}))^{-1}(\phi_{cs} - \phi_{ts}) \tag{11}$$

  where ϕ_{cs} is a cross-sectional estimator, ϕ_{ts} is the average time series estimator and $var(.)$ indicate the asymptotic variances.

- Reject homogeneity if $H > \chi^2(\nu)$; $\nu = rank[(var(\phi_{cs}) - var(\phi_{ts})]$
- Asymptotic test; would not use it with short macro data.
- Sensitive to misspecification. Scaling matrix may not be positive definite.

Diagnostic for homogeneity

- Coefficient of Variation (CV) of multipliers (or impact coefficients) should have degenerate distribution under the null of homogeneity. Under heterogeneity it should be spread out.
- CV robustly estimated by taking interquartile range/ mid point of interquartile range.
- Construct small sample critical values in two relevant sistuations.

$$Y_{1,i,t} = \delta_{i,2} + \gamma_{1,i} Y_{1,i,t-1} + \gamma_{2,i} Y_{2,i,t-1} + \beta_i e_{3,i,t} + \psi_i x_t + \nu_{i,t} \tag{12}$$

where $e_{3,i,t}$ is the shock in the third variable of the DGP and

$$Y_{1,i,t} = \delta_{i,2} + \gamma_{1,i} Y_{1,i,t-1} + \gamma_{2,i} Y_{2,i,t-1} + \beta_i (Y_{3,i,t} - Y_{3,i,t-1}) + \psi_i x_t + \nu_{i,t} \tag{13}$$

where $Y_{3,i,t} - Y_{3,i,t-1}$, the change in the third variable of the DGP, is instrumented with $z_{i,t}$. The first equation is estimated by OLS; the second by 2SLS. For each $i = 1, \ldots, N$, the multipliers at horizon h are are computed using $\hat{\beta}_i * \hat{\gamma}_{1,i}^h$.

Outline

Experimental evidence I

- N=50, T=40, and Q=1000 replications. H=2.
- For each $i = 1, \dots, N$ DGP is:

$$
\begin{aligned}
y_{1,i,t} &= a_{1,i} + b_{1,i} y_{1,i,t-1} + b_{2,i} y_{2,i,t-1} + d_{1,i} x_t + e_{1,i,t} + 0.5 e_{2,i,t} + c_{1,i} e_{3,i,t} \\
y_{2,i,t} &= a_{2,i} + b_{2,i} y_{1,i,t-1} + d_{2,i} x_t + e_{2,i,t} + 0.5 e_{1,i,t} + c_{2,i} e_{3,i,t} \\
p_{i,t} &= a_{3,i} + b_{3,i} p_{i,t-1} + c_{3,i} e_{1,i,t} + e_{3,i,t} \\
x_t &= 0.95\ x_{t-1} + 0.1\ u_{i,t} \\
z_{i,t} &= b_{4,i} e_{3,i,t} + v_{i,t}
\end{aligned}
\tag{14}
$$

  $e_{i,t}, u_{i,t}, v_{i,t}$ iid across t and i,zero mean, unit variance, zero covariances.

- $\phi_i = (a_i = (a_{1,i}, a_{2,t}, a_{3,t}), b_i = (b_{1,i}, b_{2,i}, b_{3,i}, b_{4,i}), d_i = (d_{1,i}, d_{2,i}), c_i = (c_{1,i}, c_{2,i}, c_{3,i})$ normally distributed; mean $\phi_0 = ((0,0,0), (0.6, 0.3, 0.9, 0.2), (1.6, 0.05), (1.0, 0.8, 0.7))$; covariance matrix $\omega * I$, where $\omega = (\omega_1, \omega_2)$. ω_1 applies to all the components of ϕ_i except to $c_{1,i}$ and $d_{1,i}$ for which ω_2 is used instead.

Experimental evidence II

- ω regulates the amount of dynamic heterogeneity:
  - $\omega_1 = \omega_2 = 0$: homogeneity;
  - $\omega_1 = 0, \omega_2 = 2.5$: only instantaneous heterogeneity
  - $\omega_1 = 0.1, \omega_2 = 0$ only dynamic heterogeneity;
  - $\omega_1 = 0.1, \omega_2 = 1.5$ instantaneous and dynamic heterogeneity.
- Robustness exercises:
  - $b_{1,0}$ from 0.6 to 0.2 (less state dynamics)
  - $b_{4,0}$ from 0.2 to 0.8 (stronger instruments)
  - $b_{2,0}$ from 0.3 to 0.15 (less general equilibrium effects within a unit).
  - Corrupted instruments

$$z_{i,t} = b_{4,i} p_{i,t} + v_{i,t} \tag{15}$$

Experimental evidence III

- Basic model: (Chorodow-Reich, 2019)

$$Y_{1,i,t+2} - Y_{1,i,t} = \delta_{i,2} + \alpha_{2,t} + \beta_2 P_{i,t} + e_{i,t+2} \tag{16}$$

  Estimated by (pooled) IV methods using $S_t = (\mathbf{1}, t, z_t)$ as instruments, where $\mathbf{1}$ and t are $T \times 1$ vectors.

- NS model (Nakamura and Steinsson, 2014): in (16) policy variable is $(P_{i,t+2} - P_{i,t})$; instrument vector is $S_t = (\mathbf{1}, t, z_{i,t+2} - z_{i,t})$.

- LagDep model:

$$\begin{aligned} Y_{1,i,t+2} - Y_{1,i,t} &= \delta_{i,2} + \gamma_1 (Y_{1,i,t+1} - Y_{1,i,t-1}) + \gamma_2 (Y_{2,i,t+1} - Y_{2,i,t-1}) \\ &+ \beta_2 (P_{i,t+2} - P_{i,t}) + e_{i,t+2} \ \boxed{\text{<- will be serially correlated}} \end{aligned} \tag{17}$$

  Estimated by (pooled) IV methods with
  $S_t = (\mathbf{1}, Y_{1,t+2} - Y_{1,t}, Y_{2,t+2} - Y_{2,t}, z_{t+2} - z_t)$ as instruments.

- LagIV model: model as in (17), but
  $S_t = (\mathbf{1}, Y_{1,t+2} - Y_{1,t}, Y_{2,t+2} - Y_{2,t}, z_{t+1} - z_{t-1})$.

Experimental evidence IV

- ATS model:

$$
\begin{aligned}
Y_{i,t+2} - Y_{i,t} &= \delta_{i,2} + \gamma_{i,1}(Y_{1,i,t+1} - Y_{1,i,t-1}) + \gamma_{i,2}(Y_{2,i,t+1} - Y_{2,i,t-1}) \\
&+ \beta_{i,2}(P_{i,t+2} - P_{i,t}) + e_{i,t+2} \qquad (18)
\end{aligned}
$$

  Estimated unit by unit using as instruments $S_{it} = (1, Y_{1,i,t+2} - Y_{1,i,t}, Y_{2,i,t+2} - Y_{2,i,t}, z_{i,t+1} - z_{i,t-1})$. Unit specific multipliers are $\hat{m}_i = \hat{\beta}_{i,2} \sum_{h=1}^{2} \hat{\gamma}_{i,1}^2$ and $\hat{m} = \frac{1}{N} \sum \hat{m}_i$ is the time series estimate of the average effect.

- PP model: use (18) with (ϕ_0, Σ_v) obtained from estimation of additional 20 units. Same instruments as ATS.

Experimental evidence V

Table 1: Two years ahead Mean Square Bias

DGP	Basic	NS	LagDep	LagIV	ATS	PP
Baseline exercises						
$\omega_1 = 0.1, \omega_2 = 1.5$	0.341	0.372	0.341	0.333	0.233	**0.228**
	(0.135)	(0.424)	(0.132)	(0.126)	(0.135)	(0.119)
$\omega_1 = 0.0, \omega_2 = 2.5$	0.608	0.828	1.476	0.252	0.151	**0.141**
	(0.695)	(0.675)	(1.062)	(0.325)	(0.166)	(0.131)
$\omega_1 = 0.1, \omega_2 = 0.0$	0.334	0.362	0.335	0.331	**0.280**	0.282
	(0.041)	(0.361)	(0.046)	(0.043)	(0.049)	(0.042)
$\omega_1 = 0.0, \omega_2 = 0.0$	0.521	0.706	1.199	**0.029**	0.235	0.237
	(0.473)	(0.174)	(0.366)	(0.043)	(0.032)	(0.024)

Notes: In parenthesis is the dispersion of estimates across replications.

Experimental evidence VI

Table 2: Two years ahead Mean Square Bias						
DGP	Basic	NS	LagDep	LagIV	ATS	PP
Additional exercises						
$\omega_1 = 0.1, \omega_2 = 1.5$	0.027	0.029	0.029	0.027	0.018	**0.012**
$b_{10} = 0.2$	(0.007)	(0.016)	(0.018)	(0.007)	(0.027)	(0.013)
$\omega_1 = 0.0, \omega_2 = 0.0$	1.212	2.696	3.036	0.121	0.010	**0.004**
$b_{10} = 0.2$	(0.750)	(0.585)	(0.753)	(0.108)	(0.014)	(0.006)
$\omega_1 = 0.1, \omega_2 = 1.5$	0.341	0.397	0.341	0.334	0.236	**0.234**
$b_{40} = 0.8$	(0.138)	(0.868)	(0.139)	(0.131)	(0.196)	(0.160)
$\omega_1 = 0.0, \omega_2 = 0.0$	0.384	0.699	1.377	**0.005**	0.225	0.230
$b_{40} = 0.8$	(0.116)	(0.055)	(0.126)	(0.006)	(0.076)	(0.044)
$\omega 1 = 0.1, \omega_2 = 1.5$	0.260	0.265	0.266	0.259	**0.190**	0.195
$b_{20} = 0.15$	(0.108)	(0.115)	(0.117)	(0.109)	(0.110)	(0.100)
$\omega_1 = 0.0, \omega_2 = 0.0$	0.617	1.072	1.376	**0.030**	0.178	0.179
$b_{20} = 0.15$	(0.522)	(0.233)	(0.409)	(0.049)	(0.027)	(0.020)
$\omega 1 = 0.1, \omega_2 = 1.5$	0.337	0.337	0.332	0.334	**0.230**	0.231
IV invalid	(0.127)	(0.129)	(0.122)	(0.122)	(0.136)	(0.116)
$\omega_1 = 0.0, \omega_2 = 0.0$	0.165	0.359	0.118	0.683	**0.236**	0.237
IV invalid	(0.029)	(0.072)	(0.046)	(0.589)	(0.034)	(0.024)

Critical values diagnostic

Table 3: Critical values for the CV statistics, OLS estimates

	N=20			N=50			N=100		
	0.99	0.95	0.90	0.99	0.95	0.90	0.99	0.95	0.90
One year head multipliers									
T=30	1.774	2.130	2.670	1.607	1.838	2.153	1.512	1.677	1.925
T=40	1.212	1.335	1.520	1.115	1.168	1.251	1.077	1.119	1.170
T=50	0.998	1.059	1.136	0.960	0.999	1.024	0.921	0.948	0.976
T=100	0.532	0.556	0.590	0.486	0.504	0.522	0.468	0.486	0.503
T=1000	0.127	0.135	0.142	0.114	0.118	0.123	0.107	0.110	0.113
Two years head multipliers									
T=30	0.989	0.994	0.998	0.982	0.986	0.989	0.974	0.979	0.983
T=40	0.961	0.969	0.979	0.955	0.963	0.969	0.944	0.948	0.953
T=50	0.949	0.961	0.970	0.928	0.939	0.951	0.920	0.929	0.936
T=100	0.773	0.799	0.832	0.726	0.748	0.769	0.708	0.722	0.739
T=1000	0.215	0.228	0.242	0.193	0.201	0.209	0.183	0.187	0.192
Four years head multipliers									
T=30	1.000	1.000	1.000	1.000	1.000	1.000	0.999	0.999	0.999
T=40	0.999	0.999	0.999	0.998	0.999	0.999	0.997	0.998	0.998
T=50	0.998	0.998	0.999	0.996	0.997	0.998	0.995	0.996	0.997
T=100	0.960	0.969	0.978	0.943	0.952	0.959	0.930	0.937	0.945
T=1000	0.389	0.407	0.431	0.351	0.366	0.378	0.336	0.343	0.352

Critical values diagnostic

Table 4: Critical values for the CV statistics, IV estimates									
	N=20			N=50			N=100		
	0.99	0.95	0.90	0.90	0.95	0.99	0.90	0.95	0.99
One year head multipliers									
T=30	4.541	6.426	10.51	6.890	9.731	13.013	6.561	8.373	14.454
T=40	3.568	4.908	7.298	4.071	5.378	8.420	3.594	4.430	6.061
T=50	2.940	3.855	5.971	2.592	3.140	4.133	2.153	2.551	3.235
T=100	1.429	1.689	2.007	1.165	1.234	1.358	1.100	1.132	1.191
T=1000	0.354	0.371	0.392	0.318	0.330	0.342	0.299	0.305	0.316
Two years head multipliers									
T=30	2.161	2.897	4.127	2.162	2.587	3.334	2.073	2.501	3.174
T=40	1.666	2.075	3.037	1.648	2.135	2.707	1.490	1.623	1.909
T=50	1.544	1.857	2.620	1.425	1.576	1.903	1.296	1.428	1.615
T=100	1.212	1.333	1.590	1.049	1.097	1.175	1.015	1.035	1.061
T=1000	0.408	0.431	0.454	0.361	0.373	0.385	0.344	0.354	0.365
Four years head multipliers									
T=30	1.535	1.970	2.628	1.551	1.965	2.392	1.442	1.714	2.171
T=40	1.317	1.608	2.174	1.284	1.500	1.791	1.145	1.236	1.363
T=50	1.279	1.502	1.998	1.118	1.203	1.365	1.078	1.131	1.204
T=100	1.074	1.160	1.316	1.008	1.024	1.049	1.001	1.004	1.013
T=1000	0.539	0.561	0.586	0.478	0.489	0.507	0.460	0.471	0.482

Outline

Fiscal multipliers in US states I

- Use BEA data plus integration from Canova and Pappa (2007).

- Compute average state expenditure multipliers at h=1,2. Models:
  - Model Basic.
  - Model NS.
  - Model Lagdep: as in model NS, but use lag dependent variable and no time effects.
  - Model LagIV: as in model Lagdep, but use lagged taxes, lagged oil growth, US real rate in the $x_{i,t}$. Lagged Z_{t-q} instruments.
  - Model ATS.
  - Model PP: (ϕ_0, Σ_v) obtained with aggregate US data.

- Use federal transfers (ex-Medicare) as instruments for state expenditure (or Bartik shares for cross sectional estimation).

Fiscal multipliers in US states II

Table 5: Average US state government expenditure multipliers						
Horizon	Basic Model	NS Model	LagDep Model	LagIV Model	ATS Model	PP Model
1 year	0.08	-0.28	-0.25	-0.49	0.53	0.81
	(0.20)	(12.49)	(12.11)	(12.47)	(2.12)	(0.76)
2 years	0.22	-0.28	-0.09	-1.48	0.11	0.93
	(0.52)	(9.51)	(7.75)	(19.03)	(10.11)	(0.44)
Bartik share instruments						
1 year	0.09	1.20	0.93	0.85		
	(0.20)	(12.89)	(12.56)	(12.73)		
2 years	0.26	1.29	0.56	0.47		
	(0.51)	(10.34)	(8.70)	(8.73)		

Notes: In parenthesis are the standard errors of the estimates.

- **Why are cross sectional multiplier estimates zero or negative?**

Heteroscedasticity even with the US states

Fiscal multipliers in US states III

- The CV of the estimated distribution of multipliers at the two year horizon is 13.28, which exceed the 99% critical value present in table 3 for T=40, N=50.
- Despite the small sample available, when comparing the Basic and the PP model, a Hausman test rejects the null at the 10% significance at the 1 year horizon.

Cross-sectional average estimates should not be trusted.

Fiscal multipliers in US states IV

- Conclusions depend on the methodology used.
- Cross sectional estimates of dynamic effects should not be trusted unless dynamic homogeneous groups are created.

!!!

- Otherwise, identification of the effects of interest is flawed.

Plot the distributions of your data, to see if they're homogenous.