|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Fiscal multiplier, d.CAPB, OLS estimate, booms v. slumps | | | | | |  | |
|  | (1) | (2) | (3) | (4) | (5) | |
|  | Year 1 | Year 2 | Year 3 | Year 4 | Year 5 | |
| Education expend. | -0.00068 | -0.06626 | -0.09523 | -0.08122 | -0.12006 | |
| *y > 0, boom* | -0.0244 | -1.1517 | -1.5184 | -1.5972 | -2.4228 | |
| Education expend. | -0.16025 | -0.19381 | -0.18283 | -0.17263 | -0.10299 | |
| *y <= 0, slump* | -1.5571 | -1.9545 | -2.5851 | -2.4419 | -1.8977 | |
| Health expend. | 0.014831 | -0.0245 | -0.09702 | -0.10928 | -0.08907 | |
| *y > 0, boom* | 0.811 | -0.596 | -1.6686 | -2.4582 | -2.6482 | |
| Health expend. | 0.013337 | -0.04511 | -0.11222 | -0.07219 | -0.03544 | |
| *y <= 0, slump* | 0.626 | -1.1984 | -1.803 | -1.9877 | -0.716 | |
| In-kind transfers | -0.00645 | -0.03477 | -0.06014 | -0.08009 | -0.05782 | |
| *y > 0, boom* | -0.3964 | -1.0507 | -1.8925 | -2.8288 | -1.6765 | |
| In-kind transfers | -0.01876 | -0.02095 | -0.02619 | -0.02562 | -0.00345 | |
| *y <= 0, slump* | -2.7656 | -1.844 | -1.8935 | -1.8619 | -0.1802 | |
| In-cash transfers | -0.00011 | -0.04022 | -0.11223 | -0.12571 | -0.09976 | |
| *y > 0, boom* | -0.0057 | -1.1591 | -2.11 | -3.7 | -2.3876 | |
| In-cash transfers | -0.01876 | -0.02095 | -0.02619 | -0.02562 | -0.00345 | |
| *y <= 0, slump* | -2.7656 | -1.844 | -1.8935 | -1.8619 | -0.1802 | |
|  |  |  |  |  |  | |
|  |  |  |  |  |  | |
| Property taxes | 0.000511 | 0.004564 | 0.007615 | 0.009323 | 0.004626 | |
| *y > 0, boom* | 0.2247 | 1.6089 | 1.61 | 1.9924 | 1.3275 | |
| Property taxes | 0.002791 | 0.00816 | 0.015106 | 0.017364 | 0.009144 | |
| *y <= 0, slump* | 1.0221 | 2.7583 | 2.435 | 2.6546 | 1.6161 | |
| Indirect taxes | 0.009601 | -0.0433 | -0.10832 | -0.10088 | -0.11933 | |
| *y > 0, boom* | 0.5257 | -1.6472 | -3.1783 | -4.145 | -4.5553 | |
| Indirect taxes | 0.026934 | -0.09497 | -0.13702 | -0.11448 | -0.1331 | |
| *y <= 0, slump* | 0.6502 | -2.3676 | -2.2557 | -3.2287 | -2.943 | |
|  |  |  |  |  |  | |
|  |  |  |  |  |  | |
| R&D + Savings | -0.01258 | -0.02167 | -0.03951 | -0.01222 | -0.02666 | |
| *y > 0, boom* | -1.7973 | -1.4753 | -2.7197 | -0.5573 | -1.0332 | |
| R&D + Savings | -0.0148 | -0.01888 | -0.04685 | -0.04098 | -0.05858 | |
| *y <= 0, slump* | -1.3767 | -0.9552 | -2.3976 | -2.1872 | -2.8262 | |

‘\*\*’ 0.01 ‘\*’ 0.05 ‘.’ 0.1

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Fiscal multiplier, XXX, OLS estimate, booms v. slumps | | | | | |  |  |
|  | (1) | (2) | (3) | (4) | (5) |
|  | Year 1 | Year 2 | Year 3 | Year 4 | Year 5 |
| Education expend. | -0.42031 | -0.45941 | -0.63161 | -0.03236 | 0.164519 |
| *y > 0, boom* | -1.9923 | -1.5181 | -2.1726 | -0.106 | 0.5548 |
| Education expend. | 0.04638 | 0.188081 | -0.28376 | 0.587261 | 0.56744 |
| *y <= 0, slump* | -0.1853 | 0.6061 | -0.7348 | 1.8534 | 1.2744 |
| Health expend. | -0.57195 | -1.06849 | -0.97031 | -0.34035 | -0.38077 |
| *y > 0, boom* | -1.6587 | -3.2663 | -3.1653 | -1.1746 | -1.4103 |
| Health expend. | -0.2157 | -0.50532 | -0.43922 | 0.295785 | 0.298864 |
| *y <= 0, slump* | -0.6347 | -1.277 | -1.5918 | 0.9963 | 0.564 |
| In-kind transfers | -0.58563 | -0.76126 | -0.47075 | -0.50763 | -0.18793 |
| *y > 0, boom* | -2.7591 | -3.5481 | -1.5901 | -2.5431 | -0.8339 |
| In-kind transfers | 0.146704 | -0.19295 | 0.169891 | -0.12906 | 0.396081 |
| *y <= 0, slump* | -0.5468 | -1.1876 | 1.3499 | -0.6827 | 1.2747 |
| In-cash transfers | -0.95255 | -1.25267 | -0.92266 | 0.830377 | -0.55081 |
| *y > 0, boom* | -6.1854 | -5.0232 | -4.5687 | -3.851 | -1.8234 |
| In-cash transfers | -0.71124 | -0.191 | 0.007326 | -0.03079 | -0.02675 |
| *y <= 0, slump* | -2.2879 | -0.5983 | 0.0261 | -0.0872 | -0.0261 |
|  |  |  |  |  |  |
|  |  |  |  |  |  |
| Property taxes | 0.012637 | 0.04311 | 0.048724 | 0.038707 | 0.033993 |
| *y > 0, boom* | 0.5584 | 1.433 | 1.6213 | 1.1414 | 1.006 |
| Property taxes | 0.007071 | 0.022379 | 0.044496 | 0.002223 | 0.026811 |
| *y <= 0, slump* | 0.2354 | 0.6374 | 1.0837 | 0.0514 | 0.4623 |
| Indirect taxes | -0.48486 | -0.54533 | -0.59608 | -0.47982 | -0.86514 |
| *y > 0, boom* | -2.2728 | -1.6573 | -1.8594 | -2.2728 | -3.575 |
| Indirect taxes | -0.41799 | -1.15687 | -1.18382 | -1.02618 | -1.30148 |
| *y <= 0, slump* | -1.0942 | -4.2043 | -3.4258 | -2.8469 | -2.4154 |
|  |  |  |  |  |  |
|  |  |  |  |  |  |
| R&D + Savings |  |  |  |  |  |
| *y > 0, boom* |  |  |  |  |  |
| R&D + Savings | 0.121633 | 0.080426 | -0.20576 | -0.08958 | -0.64455 |
| *y <= 0, slump* | 0.7816 | 0.5755 | -1.2557 | -0.2553 | -2.1624 |

Following a new and arguably more promising direction, we take a third fork on the road to identification based on the Rubin Causal Model. This approach has the attractive features of being semiparametric (and hence flexible with respect to the functional form), providing better control for observables, and offering a more reliable alternative when the putative instrumental variables for policy action are themselves possibly endogenous. Tests of instrument validity are well-known to have low power (see, e.g. Cameron and Trivedi 2005) but, more importantly, formal testing is not an option when we are in the case of exact identification.

We find that, on average, fiscal consolidations generate a drag on GDP growth. The effect is also state dependent: if a 1 percent of GDP fiscal consolidation is imposed in a slump then it results in a real GDP loss of around 4 percent over five years, rather than just 1 percent in a boom. We arrive at this conclusion by carefully constructing an encompassing framework that allows us to evaluate the type of approach followed by several recent papers in the literature (to be discussed in detail shortly) to improve comparability with the methods we introduce in this paper.

Using our estimates we compute how much of the slowdown could be attributed to the austerity program; we find it to be a very significant contribution (rising to 3.4% of GDP in 2013) and larger than official estimates. Thus, better models, with state-dependent features, could improve official fiscal policy analyses going forward.

Second, we use (Jorda` 2005) local projections (LPs), rather than structural vector auto regressions (SVARs). The reason is that, among other advantages that we will discuss momentarily, LPs are a convenient pedestal on which all extensions of existing estimation methods can rest. The unified framework provides the reader a way to compare the results across a set of nested estimation strategies. LPs provide a flexible semi-parametric regression control strategy to estimate dynamic multipliers and include, as a special case, impulse responses calculated with an SVAR. LPs accommodate possibly nonlinear, or state-dependent responses easily, and indeed we find that the effects of fiscal policy can be very different in the boom and the slump, as emphasized by Keynes in the 1930s.

Fourth, we show that the proposed IMF narrative instrumental variable has a significant forecastable element driven by plausible state variables, such as the debt-to-GDP level, the cyclical level or rate of growth of real GDP, and the lagged treatment indicator itself (since austerity programs are typically persistent, multi-year affairs).

The local projection is done from year 0, when a policy change is assumed to be announced, with the fiscal impacts first felt in year 1, consistent with the timing in GLP. The LP output forecast path is constructed out to year 5, and deviations from year 0 levels are shown, and also the sum of these deviations, or “lost output” across all of those five years

. Small consolidation packages have a small effect on output, but the estimates are imprecise.

If the IMF approach is correct and has found truly exogenous shocks to fiscal policy, then it would be a valid instrument for d.CAPB. It would also be a potentially strong instrument: the raw correlation between d.CAPB (year 1 versus year 0) and Treatment (in year 1) is 0.31, and a bivariate regression has an F-statistic of over 50; the same applies when Treatment is replaced by Total (in year 1).

The IV-based responses suggest that austerity is contractionary since the only statistically significant coefficients here have a negative sign. However, stratification by the state of the cycle shows that this result is now driven by what happens in slumps. It is only in the slump bin that we find a significant negative response of real GDP to fiscal tightening. In Table 4 we find a coefficient or “multiplier” of between -0.25 and -0.95 in years 1 to 5. Over five years the sum of these effects is -3.35∗∗, so the average loss for a 1% of GDP fiscal consolidation is to depress the output level by about -0.67% per year over this horizon.

4. Endogenous Austerity: Is the Narrative Instrument Valid? So far we have briefly replicated the current state of the literature, but this is not entirely pointless. It serves to show that the LP framework can capture different sides of the debate in a uniform empirical design, on a consistent data sample, allowing us to focus on how differences in estimation and identification assumptions lead to different results. It also shows how the LP estimation method makes it very easy to allow for nonlinearity and do a stratification of results; here we found significant variations in responses across bins designed to capture variations in the state of the economy from boom to slump.

Statistical design

The previous section raises concerns that the narrative IMF variable could be an invalid instrument using three different checks. The empirical strategy that we propose is based on taking triple insurance against this potential endogeneity. F

In order to facilitate the exposition we momentarily drop the cross-sectional country index in the panel. Denote, as before, yt the outcome variable of interest, the log of real GDP. In other applications yt could be a ky-dimensional vector. Let Dt denote the fiscal policy variable. Dt will now be a discrete random variable Dt ∈ {0, 1} based on the IMF narrative indicator of exogenous fiscal consolidations, although earlier it was the continuous d.CAPB variable.

This is a good place to make a connection with structural VAR identification. When h = 1, the LP is equivalent to the corresponding equation in a VAR. A specification that includes all contemporaneous variables as controls (in addition to their lags) could be seen as equivalent to imposing the Choleski ordering in which the policy variable is ordered last. However, unlike a VAR, there is no need to impose exclusion restrictions on the remaining variables in the system if the focus shifts to a different response/intervention pair. Practically speaking it would be advisable not to impose such constraints but rather include all available observable controls again and let the data choose which variables are appropriate conditioning information. The larger principle is to ensure that fluctuations in the shocked variable cannot be explained by any observable information. In that respect, it is perhaps useful to remember that the true square-root of the reduced-form residual covariance matrix need not be upper-triangular or even have any zero entries for that matter.