|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) | (5) |
|  | gini\_net0 | gini\_net1 | gini\_net2 | gini\_net3 | gini\_net4 |
|  |  |  |  |  |  |
| education\_exp | 0.00989 | -0.0117 | -0.0149 | -0.0284 | 0.0165 |
|  | (0.40) | (-0.56) | (-0.93) | (-1.18) | (0.86) |
| L.education\_exp | -0.0224 | -0.00112 | 0.00341 | 0.0147 | -0.0355\* |
|  | (-1.34) | (-0.07) | (0.16) | (1.06) | (-1.63) |
| health\_exp | -0.00726 | -0.0168 | -0.00202 | 0.00945 | -0.000214 |
|  | (-0.43) | (-1.58) | (-0.13) | (1.35) | (-0.02) |
| L.health\_exp | -0.00938 | 0.0132 | 0.00527 | -0.0140 | -0.00629 |
|  | (-0.79) | (1.08) | (0.50) | (-0.97) | (-0.54) |
| soc\_payable | 0.0359\* | 0.00499 | -0.0516 | 0.00174 | 0.00862 |
|  | (1.57) | (0.33) | (-1.52) | (0.21) | (1.12) |
| L.soc\_payable | -0.0347\* | 0.0119 | 0.0351 | -0.00826 | -0.00790 |
|  | (-1.70) | (1.09) | (1.24) | (-0.56) | (-0.72) |
| soc\_kind | 0.00454\* | 0.00865\* | 0.00419 | 0.00504 | 0.00549 |
|  | (2.23) | (2.58) | (1.50) | (1.71) | (1.42) |
| ind\_tx | -0.0107 | -0.00464 | 0.00253 | -0.00782 | 0.00986 |
|  | (-1.06) | (-0.43) | (0.26) | (-0.93) | (1.10) |
| L.ind\_tx | 0.0128 | 0.0203\* | 0.00656 | 0.0242\*\* | 0.0160\*\* |
|  | (1.00) | (1.79) | (0.94) | (2.14) | (2.23) |
| property\_taxes | 0.00632\*\* | 0.00331 | 0.000514 | -0.00311 | 0.00652 |
|  | (2.15) | (0.94) | (0.14) | (-1.07) | (0.88) |
| L.property\_taxes | -0.00410 | -0.00137 | -0.00320 | 0.000714 | -0.0129 |
|  | (-1.11) | (-0.47) | (-1.01) | (0.18) | (-1.61) |
| pit | -0.0339\*\* | 0.00907 | -0.0311\* | -0.0295\*\* | 0.00272 |
|  | (-3.82) | (1.10) | (-2.50) | (-3.34) | (0.31) |
| L.pit | 0.0103 | -0.0283\*\* | 0.00642 | 0.0217\*\* | -0.00145 |
|  | (1.00) | (-3.84) | (0.49) | (2.46) | (-0.11) |
| ***N*** | **713** | **690** | **667** | **644** | **621** |
| adj. *R*2 | 0.063 | 0.037 | 0.093 | 0.057 | 0.060 |
| adj. R2 | 0.068 | 0.039 | 0.092 | 0.054 | 0.055 |
| adj. R2 | 0.033 | 0.040 | 0.069 | 0.054 | 0.055 |
| adj. R2 | 0.026 | 0.051 | 0.037 | 0.057 | 0.058 |
| adj. R2 | 0.016 | 0.042 | 0.036 | 0.061 | 0.067 |
| adj. R2 | 0.018 | 0.037 | 0.036 | 0.055 | 0.070 |
| adj. R2 | 0.031 | 0.050 | 0.058 | 0.071 | 0.064 |

**Notes:** Standard errors (clustered by country) in parentheses. ∗∗∗/∗∗/∗ indicate p < 0.01/0.05/0.10. Additional controls: cyclical component of y, 2 lags of change in y, country fixed effects.

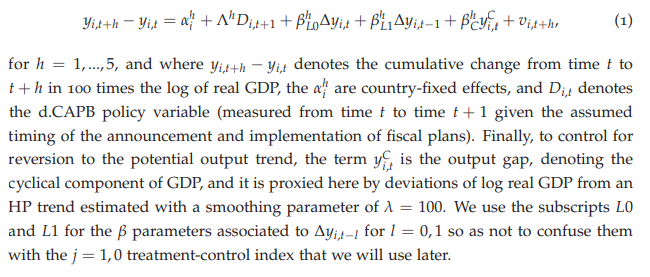
**Replicating Fiscal policies and taxation on Inequality: OLS Results**

Our first estimates use OLS estimation with the Local Projection (LP) method, based on what is the traditional variable in the literature, the Gini index (in our case Gini net). 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. The LP output forecast path is constructed out to year 4, and deviations from year 1 levels are shown, and also the sum of these deviations, or “lost output” across all of those four years.

To create a benchmark estimating equation that mimics the standard setup in the literature, the typical LP equation that we estimate has the form:

(1)

For h=1…4, and where are country-fixed effects, and denotes the fiscal policy variable. Finally, to control for reversion to the potential output trend, the term is the output gap, denoting the cyclical component of GDP, estimated by deviations of log real GDP from an HP trend with a smoothing parameter of 100.



The specification (1) nests the main elements in AA and GLP to facilitate comparisons of our results with theirs. The coefficient Λh from expression (1) is the parameter governing the impact of the continuous policy treatment measured by d.CAPB and corresponds to the constrained version of expression (7) below in Section 5, where we have rearranged that expression to get a direct estimate of the average response to policy intervention Λh from the regression output, but it is otherwise specified the same way.

Table 1 reports estimates based on expression (1). Estimated log real GDP impacts (× 100) for each year are reported in columns 1 to 5, and the 5-year sum of the deviations in final column 6. In parallel with the main result in AA, although the effects are economically modest, the data appear to support the notion that fiscal consolidation can be expansionary (especially in the first two years), although the cumulative effect over a five-year period is small and negative. If we focus on multiplier estimates based on large consolidations (i.e., changes in CAPB larger than 1.5 percent of GDP using the Alesina and Perotti (1995) and AA cutoff value), then the results are almost identical. Small consolidation packages have a small effect on output, but the estimates are imprecise.

Would the picture change much if we broke down the analysis of the impact of consolidation as a function of whether the economy is experiencing a boom or a slump? Estimation was next carried out on two bins of the data to allow responses to be state dependent. We sort on the sign of y C, the time-0 output gap (HP filtered) into “boom” and “slump” bins, to capture conditions at time 0 varying across the cycle. This partition places just over 200 observations in each of the “boom” and “slump” bins, given the AA-GLP combined dataset with about 450 observations in total, after allowing for observations lost due to lags. Note that this partition is meant to provide a more granular statistical summary of the main features of the data. We are not arguing whether or not a boom or a slump is more likely under a particular choice of fiscal policy or another.