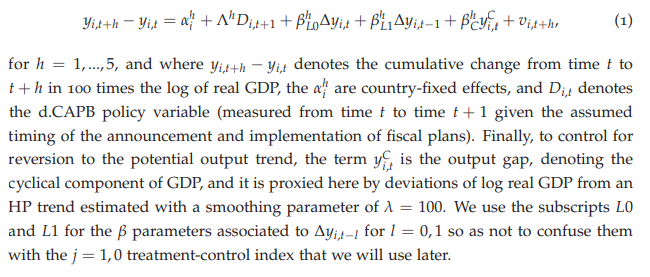
|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) | (5) |
|  | Year 1 | Year 2 | Year 3 | Year 4 | Sum |
| Education exp. | -0.0224 | -0.00112 | 0.00341 | 0.0147 | -0.0355\* |
|  | -1.34 | -0.07 | 0.16 | 1.06 | -1.63 |
| Health exp. | -0.00938 | 0.0132 | 0.00527 | -0.0140 | -0.00629 |
|  | -0.79 | 1.08 | 0.50 | -0.97 | -0.54 |
| Cash transfers | -0.0347\* | 0.0119 | 0.0351 | -0.00826 | -0.00790 |
|  | -1.70 | 1.09 | 1.24 | -0.56 | -0.72 |
| In-kind transfers | 0.00454\* | 0.00865\* | 0.00419 | 0.00504 | 0.00549 |
|  | 2.23 | 2.58 | 1.50 | 1.71 | 1.42 |
| Indirect tax | 0.0128 | 0.0203\* | 0.00656 | 0.0242\*\* | 0.0160\*\* |
|  | 1.00 | 1.79 | 0.94 | 2.14 | 2.23 |
| Property tax | 0.00632\*\* | 0.00331 | 0.000514 | -0.00311 | 0.00652 |
|  | 2.15 | 0.94 | 0.14 | -1.07 | 0.88 |
| Progressivity Inc. | 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** |

**Note:** Each row corresponds to an individual estimation along with its respective result. T-statistic (standard error 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 on Inequality: OLS Results**

Our first estimates use OLS estimation with the LP methodology, based on what is the traditional variable in the literature, the Gini net. The LP is done from year 1, when a policy change is assumed to be implemented or executed. Table 1 shows the LP output forecast until year 4, and the sum of these changes on Gini across all of those four years.

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 coefficient *Λh* from expression (1) is the parameter governing the impact of the continuous policy variable and corresponds to the constrained version of equation (6), where we have rearranged that expression to get a direct estimate of the average response to policy intervention *Λh* from the regression output.

Table 1 reports estimates based on equation (1). Estimated effect on Gini Net for each year are reported in columns 1 to 4, and the 4-year sum of the deviations in column 5. The data appear to support the notion that *Cash transfers* can alleviate (especially in the first year), although the cumulative effect over a four-year period is not significant. If we focus on taxation -*Indirect taxes* and *Property taxes*-, output has a negative effect impact on income equality, *Indirect taxes* has a longer effect. On the other hand, we found that Income Progressivity has the expected sign on the first years losing effect the following years.

**Table 2**

Table 2 presents an OLS estimated responses using expression (1), with a detailed breakdown analyzing the impact of fiscal policies based on whether the economy is in a boom or bust episode. The estimation was conducted on two subsamples to account for state-dependent responses. We categorized observations into "boom" and "bust" bins based on the sign of *yC*, the time-0 output gap (HP filtered), resulting in approximately 400 and 315 observations for each subsample after considering lag-induced observations lost. Here our objective is to offer a more detailed statistical overview of the primary effects of fiscal variables on inequality. It's important to note that we're not debating the likelihood of a boom or bust under a specific fiscal policy choice.

Stratifying the results by the state of the cycle at time 1 reveals a novel insight. It becomes apparent that the impacts of In-kind transfers and Progressive tax income are entirely influenced by boom periods, and they exhibit the expected sign on inequality. Additionally, it's observed that the effect of Progressivity diminishes over time. Conversely, during bust episodes, we observe a significant negative response of the Gini coefficient to Cash transfers. On average, this coefficient is nearly 0.04 in years 1 and 3. However, over a 4-year period, the cumulative effect of these responses is negligible and statistically insignificant, mirroring the findings from Table 1.

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| ***Fiscal effects, yc > 0, boom*** | |  |  |  |  |
|  | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
|  | **Year 1** | **Year 2** | **Year 3** | **Year 4** | **Sum** |
| Cash transfers | -0.0108 | -0.019 | -0.0326 | -0.00289 | -0.00061 |
|  | -0.57 | -0.82 | -1.77 | -0.16 | -0.03 |
| In-kind transfers | -0.00015 | 0.0221\*\* | 0.0160\* | 0.0224\*\*\* | 0.0112 |
|  | -0.04 | 3.05 | 2.56 | 4.81 | -1.14 |
| Indirect tax | -0.0151 | -0.0547 | -0.011 | -0.0195 | -0.0149 |
|  | -0.49 | -2.06 | -0.85 | -0.92 | -1.16 |
| Progressivity Inc. | -0.019 | -0.0245\*\* | -0.0259\*\* | -0.0137\*\* | -0.0239\*\* |
|  | -1.75 | -3.28 | -3.16 | -2.85 | -3.21 |
|  |  |  |  |  |  |
| ***Fiscal effects, yc < 0, slump*** | |  |  |  |  |
|  | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
|  | **Year 1** | **Year 2** | **Year 3** | **Year 4** | **Sum** |
| Cash transfers | -0.0460\*\* | -0.0142 | -0.0378\* | 0.0251 | -0.02 |
|  | -2.95 | -1.11 | -2.42 | -0.88 | -1.02 |
| In-kind transfers | 0.0214\*\*\* | -0.0204 | 0.0076 | 0.00399 | 0.0117 |
|  | -4.75 | -2 | -0.86 | -1.04 | -2.04 |
| Indirect tax | -0.0131 | -0.00274 | -0.00423 | -0.0133 | -0.024 |
|  | -1.95 | -0.29 | -0.21 | -0.89 | -1.89 |
| Progressivity Inc. | -0.0236 | -0.0084 | -0.0055 | -0.0178 | -0.00081 |
|  | -1.34 | -0.64 | -0.31 | -1.45 | -0.06 |

**Note:** Each row corresponds to an individual estimation along with its respective result. T-statistic (standard error 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.

**Table 5**

We've summarized the current findings from the literature, which suggest that the impact of fiscal policies varies significantly depending on the state of growth. Specifically, the response of inequality to government transfers tends to be more positive when the economy is weaker, whereas the opposite is observed for income taxes.

Moving forward, we aim to assess whether these fiscal variables could serve as valid instruments. For instance, we'll test if fiscal variables can be predicted by certain excluded controls, and if those controls might be correlated with the Gini net index. If such correlations exist, these excluded controls should be included in the regression analysis. At worst, if predictability is observed, it indicates that we have failed to address allocation bias in our estimates and that our results might simply reflect an endogenous response.

To tackle this issue, we'll present two diagnostic tests in this section, outlined in Tables 5 and 7. These tests will help us evaluate the robustness of our findings and ensure that our estimates are not biased by omitted variables.

In an ideal randomized controlled trial, where treatment and control units are assigned randomly, the probability density function of each control would be identical for every subpopulation, this would lead to a complete overlap between the densities of the two subpopulations. To assess this balance condition, we conduct a test comparing the means across subpopulations.

The balance condition is foundational to the simple assumption that one can estimate the LP by constraining the coefficients of the controls to be equal for both the treatment and control groups. This concept is thoroughly explored in Section 5.

In Table 5, we evaluate the balance condition for several significant economic control variables outlined in expression (1). The results indicate that the null hypothesis of balance is rejected for all variables, indicating that fiscal policies may not be entirely exogenous events.

|  |  |  |
| --- | --- | --- |
|  | Difference |  |
| Treatment Education exp. | -0.12\*\*\* | -0.04 |
| Treatment Health exp. | 0.19\*\*\* | -0.04 |
| Treatment Payable | 0.28\*\*\* | -0.04 |
| Treatment In-kind | 0.21\*\*\* | -0.04 |
| Treatment Ind. taxes | 0.15\*\*\* | -0.04 |
| Treatment Prop. taxes | 0.07\* | -0.04 |
| Treatment PIT | 0.17\*\*\* | -0.04 |
| **Note:** Each row corresponds to an individual estimation along with its respective result. T-statistic (standard error 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. | | |

**Table 7**

Probit model of fiscal treatment at time t+1 (fiscal and expenditure event)

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Treatment Variable* | *Education exp.* | *Health exp.* | *Cash transfers* | *In-kind transfers* | *Property tax* | *Indirect tax* | *Progressivity Inc.* |
| Output growth rate | -0.100\*\*\* | -0.043\* | -0.073\*\*\* | -0.060\*\* | -0.019 | 0.063\*\* | -0.02 |
|  | (-0.032) | (-0.023) | (-0.027) | (-0.026) | (-0.023) | -0.027 | (-0.022) |
| Public debt to GDP ratio | -0.029 | -0.056\* | 0.023 | 0.003 | -0.108\*\*\* | -0.068 | -0.017 |
|  | (-0.026) | (-0.03) | (-0.031) | (-0.029) | (-0.036) | -0.051 | (-0.028) |
| Treatment Variable | 1.433\*\*\* | 1.219\*\*\* | 1.252\*\*\* | 0.061\* | 0.291\*\*\* | 0.186\* | 0.919\*\*\* |
|  | (-0.236) | (-0.202) | (-0.187) | (-0.036) | (-0.068) | -0.099 | (-0.225) |
| Observations | 759 | 759 | 759 | 759 | 759 | 759 | 693 |
| Model AUC | 0.621 | 0.605 | 0.608 | 0.571 | 0.585 | 0.585 | 0.579 |
| s.e. | 0.0202 | 0.0205 | 0.0203 | 0.0208 | 0.0206 | 0.0206 | 0.0216 |

To delve deeper into this issue, we conduct an additional examination to determine if omitted controls have predictive power regarding fiscal policy events. Table 7 explores whether changes in the fiscal binary treatment variables are anticipatable. The findings suggest that our apprehension is warranted. By examining a series of treatment equations, where we employ a pooled probit estimator to forecast the fiscal event variable in year 1, anticipated a year prior.

Columns 1 to 7 depict the estimated likelihood of fiscal treatment, with column 1 representing the results for Education expenditure, column 2 for Health expenditure, and so forth. As anticipated, during periods of economic growth, there is a heightened probability of increasing expenditure on education, health, and social transfers. Conversely, there is insufficient significant evidence to support an increase in taxes, except for indirect taxes.

Conversely, when public debt to GDP is elevated, the coefficients are negative only for Health expenditure and Property taxes. This suggests that governments tend to adopt austerity measures when debt levels are high. Additionally, by including the lag of the dependent treatment variable, all variables exhibit highly significant coefficients. This implies that once a fiscal program is initiated, it tends to persist for several years (X and Y). Consequently, we can infer that being under treatment today serves as a reliable predictor of being under treatment tomorrow.

Further confirmation of the predictive capability of above regressions is provided by the AUC statistic, which is commonly used in machine learning to assess classification performance[[1]](#footnote-1). Table 7 evaluates the classification ability of each variable specification. The AUC statistics reveal that the probits demonstrate excellent predictive ability, with AUC values around 0.6. Importantly, all AUC values surpass the 0.5 threshold, indicating statistically significant predictive power. The key takeaway from Table 7 is that treatment variables possess a considerable forecastable component. However, a lingering question pertains to addressing the issue of endogenous instruments. The remainder of this paper offers one potential solution.

Table 8

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | *Year 1* | *Year 2* | *Year 3* | *Year 4* | *Year 5* |
| Education exp. | -0.0067 | -0.0057 | -0.0094\*\* | -0.0104\*\* | -0.0092\*\* |
|  | (-0.0044) | (-0.0043) | (-0.0046) | (-0.0045) | (-0.0044) |
| Health exp. | -0.0065 | -0.0062 | -0.0076\* | -0.0077\* | -0.006 |
|  | (-0.0044) | (-0.0044) | (-0.0044) | (-0.0044) | (-0.0043) |
| Cash transfers | 0.0013 | -0.003 | -0.0087\*\* | -0.0102\*\*\* | -0.0095\*\*\* |
|  | (-0.0037) | (-0.0034) | (-0.0037) | (-0.0038) | (-0.0035) |
| In-kind transfers | -0.0086\*\* | -0.0071\* | -0.0070\* | -0.0064\* | -0.0058 |
|  | (-0.0037) | (-0.0037) | (-0.0037) | (-0.0036) | (-0.0036) |
| Property taxes | 0.0066 | 0.0069\* | 0.0065\* | 0.0067\* | 0.0056 |
|  | (0.004) | (0.0038) | (0.0038) | (0.0038) | (0.0038) |
| Indirect taxes | -0.005 | -0.0023 | -0.002 | 0.0002 | 0.0009 |
|  | (-0.0039) | (-0.0041) | (-0.004) | (-0.004) | (-0.0039) |
| Progressivity Inc tax | 0.0031 | 0.0013 | -0.0015 | -0.0025 | -0.0017 |
|  | (-0.0042) | (-0.0043) | (-0.0041) | (-0.004) | (-0.004) |

We begin by discussing Table 8, which is the direct counterpart to the OLS and IV result presentations in Tables 1 and 3. Here we show the ATE of fiscal consolidation using the AIPW estimator (18), for the full sample (i.e., no use of boom and slump bins, yet) and using the propensity score estimates based on the saturated probit. Both the treatment-equation probit model and the outcome-equation AIPW model include country-fixed effects. Table 8 is organized into two rows. The first row reports the results based on imposing the restriction θh 1 = θh 0, the usual implicit restriction used without hesitation in the macro-VAR empirical literature and the same restriction we imposed in reporting the results of Tables 1 and 3. The second row reports the results that do not impose the θh 1 = θh 0 restriction. The results are qualitatively similar to those reported in Table 3 in that we still find that austerity is contractionary. However, the estimated impacts of fiscal consolidations on output are now even bigger. Recall that according to the IV estimates, the accumulated loss over five years was-2.94∗∗∗. This would imply an average annual real GDP loss of about 0.59% of GDP per 1% of fiscal consolidation over each of the 5 years. Here our AIPW estimate with unrestricted coefficients has a sum effect of-3.61∗∗∗ over 5 years. This would imply an average annual real GDP loss of about 0.74% of GDP per 1% of fiscal consolidation over each of the 5 years (using a 1/0.97 rescaling factor). Thus, the implied output losses due to austerity are about 20% larger under our AIPW estimation than with IV estimation. Next, we once again explore the same partition of the data into booms and slumps, allocating to the bins according to whether output is above or below trend as in earlier sections to provide a more granular view of these results, and Table 9 presents these AIPW estimates based on the same saturated policy propensity score probit model described earlier. These results show that in a boom a fiscal consolidation has on average a small, negative, but imprecisely estimated effect. The first row of the table indicates that the accumulated loss over five years is-1.80 percent of GDP. In a slump, the results are about three times as strong and highly statistically significant: over five years, the accumulated loss is-3.54∗∗ percent of GDP, as shown in the second row of the table. Scaling these effects for the average treatment size (0.97 percent of GDP) the average loss per 1% fiscal consolidation is 0.37% of GDP per year over the five-year window in booms, and 0.73% of GDP per year in slumps. Summing up our LP results, we always find more adverse paths when austerity is imposed in slumps rather than in booms, but there are sometimes big differences across specifications. OLS suggests that austerity might have a small and imprecisely estimated expansionary effect, although a more granular view indicates that even then, this result holds only in booms. Using the “narrative” instrument we would walk away believing more firmly that austerity is contractionary. The estimated effect with IV is relatively small and imprecisely estimated for the boom, but stronger and significant in the slump, adding up to a loss of-3.3% of output over 5 years for the typical consolidation. Finally, using the AIPW estimator we find even larger contractionary effects of austerity, still not statistically significant in booms, and amounting to-3.5% over 5 years in slumps. One may quibble that the size of the consolidation should also be taken into consideration.

In principle, this is a valid concern. However, in practice this would mean extending the space of discrete interventions. Given the data, there would be too few observations to obtain robust results (and in some cases, insufficient data to estimate the desired effects). Fortunately, as we have discussed earlier, fiscal consolidations typically average about one percent relative to GDP with a tight range of variation, which greatly facilitates the interpretability of our findings. Figure 3 displays the coefficients reported in Table 9, with appropriate rescaling in the case of AIPW to allow for the average treatment size, to show the dynamic ATE impacts of fiscal consolidations in graphical form and compares them with the responses obtained using the IV coefficient estimates which were reported earlier in Table 4. Our results underscore that austerity tends to be painful, but that timing matters: the least painful fiscal consolidations, from a growth and hence budgetary perspective, will tend to be those launched from a position of strength, that is, in the boom not the slump. This would seem to require moderately wise policymaking and/or fiscal regimes (councils, rules, etc.), not to mention an ability to stay below any debt limit so as to maintain capital market access to permit smoothing. The next section puts our new results to work in the context of the austerity program launched in UK by the Coalition administration in 2010, to show how our analysis can be used in practice. Moreover, by putting our results in a realistic situation outside the sample used for estimation, we obtain a feel for how well calibrated our findings are to the recent macroeconomic experience of a representative economy from our sample.

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | *Year 1* | *Year 2* | *Year 3* | *Year 4* | *Year 5* |
| Cash transfers | 0.0039 | 0.0025 | 0.0006 | -0.001 | -0.0009 |
|  | (-0.0063) | (-0.0059) | (-0.0064) | (-0.0058) | (-0.0066) |
| In-kind transfers | -0.0075 | -0.0074 | -0.007 | -0.0061 | -0.0057 |
|  | (-0.0051) | (-0.0051) | (-0.0051) | (-0.0051) | (-0.0051) |
| Property taxes | 0.0072 | 0.0096\* | 0.0096\* | 0.0101\* | 0.0091\* |
|  | (0.0053) | (0.0053) | (0.0055) | (0.0054) | (0.0055) |
| Indirect taxes | 0.0034 | 0.0062 | 0.0073 | 0.0074 | 0.0082 |
|  | (-0.005) | (-0.0055) | (-0.0057) | (-0.0055) | (-0.0056) |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | *Year 1* | *Year 2* | *Year 3* | *Year 4* | *Year 5* |
| Cash transfers | 0.0093\* | 0.0090\* | 0.0065 | 0.0061 | 0.0062 |
|  | (-0.0052) | (-0.0052) | (-0.0052) | (-0.0051) | (-0.005) |
| In-kind transfers | -0.0113\*\* | -0.008 | -0.008 | -0.0077 | -0.0074 |
|  | (-0.0053) | (-0.0054) | (-0.0052) | (-0.0052) | (-0.0051) |
| Property taxes | 0.0079 | 0.0128 | 0.0085 | 0.0033 | 0.0022 |
|  | (0.0069) | (0.0083) | (0.0064) | (0.0069) | (0.0072) |
| Indirect taxes | -0.0179\* | -0.0223\* | -0.0366\*\*\* | -0.0139 | -0.0164 |
|  | (-0.0107) | (-0.0125) | (-0.0135) | (-0.0125) | (-0.0117) |

1. Under the null that the covariates have no classification ability, AUC = 0.5. Perfect classification ability corresponds to AUC = 1. The AUC has an approximate Gaussian distribution in large samples. [↑](#footnote-ref-1)