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The Macroeconomic Effects of Cash Transfers: Evidence from Brazil*

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Abstract

We estimate the macroeconomic effects of Brazil's Bolsa Familia cash transfer program on state-level GDP and employment. Using a Bartik identification strategy over 2004–2019, we find a large and persistent relative multiplier: a state receiving extra transfers worth 1% of GDP grows 2–3 percentage points more than others, with effects concentrated in non-tradable sectors. Informal employment increases significantly while formal employment does not, leaving total employment little changed. The large GDP multiplier therefore reflects higher labor productivity. An open-economy New Keynesian model with no wealth effects on labor supply matches the GDP multiplier but misses the employment impacts.

JEL classification: E0, E32, E26, E60, E62, O54.

Keywords: cash transfers, Bolsa Familia, informality, local multiplier, developing countries.

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1 Introduction

Cash transfers are an important policy tool in both advanced and developing countries: more than 130 countries use direct cash payments for countercyclical stimulus, income stabilization, anti-poverty and social infrastructure programs (World Bank, 2015). The number of recipients increased from around 500 million before the COVID-19 pandemic to 1.4 billion in 2020–21 (Gentilini, 2022; World Bank, 2018). Therefore, understanding the aggregate impact of government transfers on the economy is a long-standing and important topic in both academic and policy spheres.

In theory, the macroeconomic effects of cash transfers on GDP depend on several mechanisms and are ambiguous in both sign and size. The effect of cash transfers on local output can be large if the transfer is spent on locally produced goods and the economy features sticky prices or wages. It might also be large if the economy has economic slack, due to a cyclical downturn or the chronic underemployment common in many developing countries. On the other hand, cash transfers may have zero or negative real effects if the transfer is saved or spent on goods produced in other regions, or if it leads to reduced labor supply due to wealth effects. The persistence of the cash transfer is important conceptually, as persistent shocks can have larger effects on consumption and labor supply decisions.

Despite a large literature on the microeconomic effects of cash transfers, there has been much less research on their macroeconomic effects. In the empirical macroeconomics literature, a large body of work focuses on the aggregate impact of fiscal policy in advanced countries, but much less is known about developing countries in general and transfers in particular.

This paper provides new evidence on the macroeconomic impact of cash transfers on GDP and employment in a developing country. We study the effect of Brazil's Bolsa Familia (BF) program, which is a large-scale anti-poverty cash transfer program, worth around 0.5% of Brazilian GDP in 2014 and up to 3.7% of GDP in the poorest states. Like most other cash transfer programs, BF provides persistent payments to eligible poor households, rather than a large one-off transfer. First, we directly estimate the relative GDP multiplier-the change in relative output when a state receives more transfers than others-using state-level output data, along with separate multipliers for tradable and non-tradable sectors. Second, we provide the first direct estimates of the effect of cash transfers on *informal* employment, alongside the more commonly studied formal employment. Third, we use these estimates to test whether the standard open-economy New Keynesian (NK) framework, which has rationalized transfer multipliers in advanced economies, can match the evidence in a developing

country. Our analysis identifies which features of the model are essential for reproducing the empirical patterns.

To this end, we construct a rich macro panel data set by harmonizing four data sets for over 16 years between 2004 and 2019 for 27 Brazilian states (including the Federal District). Our approach has several advantages. First, a state-level panel allows for the direct estimation of the effect of transfers on state-level GDP, the lowest aggregation level where GDP can be directly measured. Other approaches usually compute GDP multipliers indirectly, from employment effects via a production function. These implied GDP multipliers can be severely biased when transfers affect labor productivity or other unobserved inputs. This is a real concern in our setting: Bolsa Familia transfers raise state-level labor productivity substantially. Second, the state-level panel data allow us to estimate the effect of BF transfers on informal employment, since informal employment data are only representative at the state level in Brazil. As in most developing countries, informal employment accounts for a large fraction of total employment in Brazil. Therefore, capturing this margin gives a more complete picture of the labor-market response than existing work, which has largely focused on formal employment. Finally, the panel approach allows us to capture average effects across many BF rule changes over 15 years, rather than estimating the effect in a single year or of a specific policy change, enhancing external validity.

As in much of the fiscal multiplier literature, a key empirical challenge is that BF transfers may be endogenous due to reverse causality or omitted variable bias. Reverse causality is the main identification challenge as BF transfers are targeted at the poor: an adverse shock which reduces growth in a state may increase poverty, leading to an automatic increase in BF transfers. To address this problem, we use a Bartik identification strategy where states have systematically heterogeneous exposure to aggregate changes in BF transfers at the national level. For example, when national BF transfers increase by one percentage point (ppt) of GDP, BF transfers increase by around 6 ppts of state GDP in the poorest states in the Northeast but by less than 1 ppt of state GDP in richer South and Southeastern states. Our identification does not require national BF changes to be exogenous. The identification assumption is that, conditional on controls and fixed effects, there is no unobserved growth factor that is both correlated with changes in national BF transfers in the time series and that differentially affects the same states that are more sensitive to national BF transfers in the cross section.[1] We also include state fixed effects to control for state-level trends

[1] Or more precisely, conditional on controls and fixed effects, changes in Bolsa Familia spending are uncorrelated with an exposure-weighted average of unobservables. This identification assumption is similar to that in Nakamura and Steinsson (2014), Pennings (2021), and Guren et al. (2021).

and time fixed effects to control for all common aggregate shocks such as changes in aggregate fiscal and monetary policy. Furthermore, since Brazil's business cycles may affect both national BF transfer changes and differentially affect states that are more sensitive to national BF transfer, our benchmark specification also includes controls for state-specific sensitivity to the national GDP growth rate.

Results. Our main result is that the relative transfer multiplier is above 2, i.e., a state that receives an extra transfer worth 1% of its GDP relative to other states grows over 2 percentage points faster than other states. The magnitude depends on the specification: in the parsimonious case where we only control for time and state fixed effects, the point estimate of the relative cash transfer multiplier is 2, and in a benchmark case where we additionally control for possible state-specific effects of aggregate business cycles, the relative multiplier is 3.2. Both estimates are statistically significant at conventional levels. Cumulative multipliers are over one for up to 3–4 years after the transfer. Our estimated relative transfer impact multiplier is larger than those estimated using US state-level data in Pennings (2021), but similar to those estimated by Egger et al. (2022) in Kenya.[2] Furthermore, the impact of BF transfers is large and significant for non-tradable sectors, but insignificant for tradable sectors. This result is consistent with the mechanisms in a model where output is demand-determined in the short run due to sticky prices, and transfers operate through local demand for non-tradables, rather than for goods that can be sourced from other states.

Second, while the relative GDP multiplier is large, we estimate that BF transfers have close to zero impact on total formal-wage equivalent (FWE) employment, defined as an average of formal and informal employment weighted by their relative wages. The combination of a large GDP multiplier and a small employment impact implies that transfers work primarily through higher labor productivity. Specifically, we estimate that states that receive an additional transfer worth 1% of their GDP experience a 4.6% increase in labor productivity.

Third, while the total employment effect of transfers is about zero on average, we find that informal and formal employment respond differently. Specifically, a relative transfer worth 1% of GDP tends to *increase* relative informal employment by about 4% of total employment. In contrast, the effect on formal employment is close to zero or negative, depending on the specification. These results differ from several other studies, discussed below, suggesting formal employment impacts

[2] Pennings (2021) estimates a relative transfer multiplier of about 1.5 on permanent US Social Security transfer changes, but one-third on one-off stimulus transfer payments. Egger et al. (2022) estimate a village-level multiplier of 2.5 in Kenya in response to a temporary transfer.

may depend on the context or specification.

These estimates are robust to several extensions we include to address potential threats to identification. For example, Brazil is a large commodity exporter, and commodity price shocks can drive national fiscal policy as well as differentially affect states that receive a larger share of BF transfers. Nevertheless, the estimated multiplier is robust to adding controls for state-specific sensitivities to the growth rate of the commodity price index. Furthermore, we control for state-specific exposure to national variation in minimum wages, which is correlated with national BF levels. The estimated relative output multipliers remain robust and consistently above 2 even when we add controls for other types of federal transfers to states, tax revenues, lagged state output growth rate, lagged BF transfers, and poverty rates.

Finally, our analysis suggests that a standard open-economy New Keynesian (NK) model, calibrated to Brazilian data, is able to explain the large estimated GDP impact multiplier, but not the small employment impact. Our baseline NK model is similar to those in Nakamura and Steinsson (2014); Gali and Monacelli (2005). This class of models has been shown to be able to rationalize transfer multipliers in advanced economies, which makes it the natural benchmark for asking whether the same mechanisms apply in a developing country. The baseline calibrated model with no wealth effects on labor supply implies a multiplier of 2.5 on impact, within the estimated range of 2.0 to 3.2. The model-implied sectoral responses to transfers are also consistent with the empirical estimates, with non-tradable output rising more than tradable output. At longer horizons, the model-implied cumulative multipliers fall, and are smaller than their empirical counterparts. The predicted employment increase is also much larger than observed empirically. Motivated by this discrepancy, we augment the model with variable utilization, where firms can change the intensity of utilization after paying a cost. The calibrated model with variable utilization helps to generate a larger on-impact output multiplier of 3, similar to our benchmark specification. The variable utilization model also implies a smaller boost to employment, though one that is still larger than that in the data.

Related Literature. Our paper contributes to several strands of the literature. First, our paper adds to a large literature on the effects of fiscal policy on short-run macroeconomic performance. Traditionally, this literature has focused on the national level in advanced economies. However, identification challenges and differences in economic context—such as monetary policy and the state of the economy—have resulted in a wide range of multiplier estimates.[3] Since the

[3]See Ramey (2011, 2019), Auerbach and Gorodnichenko (2016); Ramey and Zubairy (2018); Miyamoto, Nguyen,

2008 financial crisis, a newer empirical literature has tried to overcome some of these challenges by exploiting cross-sectional variation across US states and municipalities such as Chodorow-Reich et al. (2012); Nakamura and Steinsson (2014); Serrato and Wingender (2016) among others. Except Pennings (2021), who studies transfers to individuals, most of this literature focuses on the effects of government spending on income and employment across US states, as surveyed in Chodorow-Reich (2019). Our paper contributes to this literature by studying a developing country context and a different fiscal instrument: cash transfers to poor households.

Second, we contribute to the literature studying the macroeconomic effects of fiscal policies in developing countries. Several papers such as Ilzetzki, Mendoza, and Végh (2013) and Kraay (2012, 2014) study the effects of government consumption and investment on short-run national growth at the national level in developing countries, and generally find small multipliers. Bracco et al. (2021) study the effect of social transfers on national growth in both developed and developing economies, and find larger effects in Latin American developing economies. A recent paper by Egger et al. (2022) finds a large multiplier on a one-off transfer across Kenyan villages. We study a very different policy: small, regular and almost-permanent transfers, rather than a large one-time payment, which can have substantially different aggregate implications. Our multiplier estimates are also about as large as those in Egger et al. (2022), but apply to state-level GDP rather than activity at the village level. Our paper also relates to Corbi, Papaioannou, and Surico (2019), who estimate local formal employment multipliers using municipal-level data in Brazil. However, they study intergovernmental transfers, not cash transfers to poor individuals, which are conceptually very different.[4]

The closest paper to ours is the contemporaneous work by Gerard, Naritomi, and Silva (2021), who estimate the effects of variation in the Bolsa Familia program in 2009 on municipal-level formal employment, and find a sizable positive effect. Our estimates of small or even negative impact on relative formal employment are different from their estimates of a large increase in formal employment in municipality data. There are important distinctions between our paper and theirs, which stem from our macroeconomic focus and may explain the differences. First, we apply a different identification strategy, which allows us to study the effects of many policy changes over 15 years, rather than studying a particular policy reform in 2009 as theirs does. Second, we study

and Sergeyev (2018); Barnichon, Debortoli, and Matthes (2021).

[4]In the development economics literature, much of the attention has been on individual behaviors such as consumption–savings decisions, or labor supply and earnings in response to experimental/quasi-experimental cash transfers, see Bastagli et al. (2016) for a review.

the effect of Bolsa Familia at the state level, rather than the municipal level. A state-level study results in a smaller sample size, but we obtain a clearer measurement of general equilibrium effects as GDP and informal employment data are only available at the state level.[5]

The rest of the paper is organized as follows. Section 2 presents the institutional background of the Bolsa Familia program relevant for our study. Section 3 discusses our empirical strategy and identification. Section 4 provides a summary of the Brazilian data used. Sections 5 and 6 present the main results and discuss several robustness tests and extensions, respectively. Section 7 interprets the empirical results using a New Keynesian model. Section 8 concludes.

2 Bolsa Familia Program Background

Bolsa Familia (BF) is a conditional cash-transfer program, created in late 2003 with the goal to provide a vast expansion of social protection benefits in Brazil. In its inaugural year, the BF program consolidated four other existing federal cash transfer programs, which we take account of in our measure of BF growth in 2004.[6] Soon after, the BF program became the largest social program in Brazil, reaching 14 million poor families–about one-fourth of the population–at the cost of about 0.5% of the Brazilian GDP in 2014. The scale and long duration of the program make it an attractive natural experiment to examine the macroeconomic effects of cash transfers in a developing country. We describe the program here as initially implemented, and note that eligibility rules have changed over time.

The program targets poor and extremely poor families in Brazil with two types of benefits: basic and variable. In 2004, a family was considered poor or extremely poor if the monthly per capita income, net of transfers, was equal to or less than R$100 and R$50, which is roughly US$PPP 100 and 50, respectively. In the same year, the basic benefit was R$50 per month and was unconditional, but only available to extremely poor households. The variable benefit is available for poor and extremely poor families, providing R$15 per child between 0 and 15 years, for up to three children, and R$30 per adolescent between 16 and 17 years, for up to two adolescents.[7]

[5] Other papers that operate at the municipal level (Gerard, Naritomi, and Silva (2021); Corbi, Papaioannou, and Surico (2019); Cunha et al. (2022)) focus on formal employment instead. GDP data are substantially interpolated at the municipal level.

[6] These four cash transfer programs are school allowance (*Bolsa Escola*), food allowance (*Bolsa Alimentação*), gas aid (*Auxílio Gás*), and food card (*Cartão Alimentação*). Therefore, to properly compute the changes in BF transfer in 2004, we construct a measure of pre-BF transfer in 2003 using the information from these prior cash transfer programs. We include more details in Appendix B.

[7] The BF benefit is conditional on health and education standards. The health conditionality requires that families keep all children and pregnant women on schedule with standard health check-ups and vaccines. The education

The BF program is funded by the federal government through the Ministry of Social Development (MDS), but key parts of the implementation, such as finding and registering the poor, are carried out at the municipal level. Quotas are assigned to states at the federal level and are adjusted infrequently, based on national household surveys (PNADs) and the Census (2000 and 2010). Municipal governments are then responsible for identifying eligible households and enrolling them with the Single Registry (*Cadastro Unico*), a rolling Census of the poor in Brazil.

The quota component of the BF transfer is arguably more exogenous to changes in current "local" economic conditions because quotas are defined at the federal level, and survey data are released with substantial lags. However, we do not exclusively exploit the exogeneity of quotas because they account for a small variation in transfers later in our sample, as shown in Appendix Figure D.3(a) which plots the history of quota changes at the national level. Instead, we pursue a Bartik-style identification strategy that exploits BF variations over time across states.

BF transfers have changed over time also due to the number of enrolled households relative to the state quota (the coverage ratio) and average benefits. From 2004 to 2008, the variation in BF transfers was dominated by the coverage component, as the municipal administration gradually filled the first quotas established in late 2003 (11 million). In 2009, there was a sizable increase in transfers driven by a quota reform, which was exploited in Gerard, Naritomi, and Silva (2021). After 2009, most of the variation in transfers was driven by changes in the average benefit.[8] We discuss this in more detail in Appendix A.

One particular empirical challenge, given our data are at an annual frequency, is that large changes in BF transfers tend to be implemented in the middle of the calendar year. For example, in 2004 when the program started, the largest increase in transfers happened in July. Over the 2005-2019 period, June, July and August recorded the highest average changes in BF transfers.[9] This timing means that a change in BF in year t can be highly correlated with BF changes in year $t+1$. Therefore, in addition to computing the first-year impact multiplier, we also estimate the cumulative multipliers over longer horizons which capture the combined effect of these changes.

conditionality requires that all children ages 6-15 be enrolled in school, with a minimum attendance.

[8] We plot in Appendix Figure D.1 the evolution of each component of transfers at the national level since the BF program conception in 2004 to the end of our sample in 2019.

[9] Appendix Figure D.2 plots the average BF transfer changes over the months.

3 Measurement of the Relative Multiplier

This section discusses how we estimate the relative multiplier. We first describe a general specification to estimate the effects of BF transfers on macroeconomic outcomes, followed by our identification of exogenous changes in relative BF transfers.

Specification. We estimate the effects of BF transfers on output and other macro variables using the variations in transfers across states and over time. The empirical specification is as follows:

$$\frac{y_{s,t} - y_{s,t-1}}{y_{s,t-1}} = \beta_0 \frac{b_{s,t} - b_{s,t-1}}{y_{s,t-1}} + X'_{s,t}\delta + \eta_s + \eta_t + e_{s,t} \tag{1}$$

where $y_{s,t}$ is real output in state s in year t, $b_{s,t}$ is the real BF transfer in state s in year t, $X_{s,t}$ is a vector of controls, and η_s and η_t are the state and year fixed effects, respectively. We estimate this specification using an annual panel data set between 2004 and 2019 for 27 Brazilian states. The coefficient β_0 is the relative transfer multiplier: when state s receives an extra 1% of its GDP in BF transfers relative to other states, output in state s grows β_0 percentage points faster than other states. Note that this relative transfer multiplier is different from the aggregate transfer multiplier, as aggregate shocks and aggregate policies such as monetary policy are removed by time fixed effects η_t. Since Brazil only has a small number of states (26 states and the Federal District), we report several standard errors for the estimates, including robust standard errors, state-clustered robust standard errors, and wild bootstrap standard errors clustered by state, and by state and year.

Identification and Instruments. There are two main challenges to the identification of the relative transfer multiplier, reverse causality and omitted variable bias (OVB). To address reverse causality, we use a Bartik-style instrumental variables strategy. This also reduces OVB, which we further address with time and state fixed effects and estimating a battery of other robustness tests.

Reverse causality. The typical identification challenge in the fiscal multiplier literature is that spending is pro-cyclical due to a greater availability of funds, or countercyclical due to policy makers' efforts to smooth the business cycle. However, as the BF program is federally funded, there should be no feedback from the local budgets to the size of BF transfer, and the national cycle is subsumed into time fixed effects. Instead, our clearest identification challenge is that the BF program targets the poor, so an adverse shock in state s that reduces its output ($e_{s,t} < 0$), could

lead to an increase in poverty, inducing larger BF transfers via higher coverage or greater average benefits. For these reasons, we expect ordinary least squares (OLS) estimates to be downward biased.

Instrument. Our IV approach is similar to a Bartik instrument as it is based on heterogeneous exposure of states to changes in national BF transfers. Following Goldsmith-Pinkham, Sorkin, and Swift (2020), we can decompose changes in state-level BF spending into an exposure γ_s to changes in national BF spending $b_{N,t}$, and a state-level idiosyncratic component $\Delta\tilde{b}^Y_{s,t}$:

$$\frac{b_{s,t} - b_{s,t-1}}{y_{s,t-1}} = \gamma_s \frac{b_{N,t} - b_{N,t-1}}{y_{N,t-1}} + \Delta\tilde{b}^Y_{s,t},$$

where the superscript Y indicates it is expressed as a share of GDP.

Our argument is that the most serious threats to identification, such as feedback from local economic shocks to poverty rates and BF transfers, affect the idiosyncratic component $\Delta\tilde{b}^Y_{s,t}$ and so the national exposure component $\gamma_s \frac{b_{N,t}-b_{N,t-1}}{y_{N,t-1}}$ can be used as an instrument.

The identifying assumption can be stated in several ways. Following the earlier macro literature, such as Nakamura and Steinsson (2014) and Guren et al. (2021), there is no unobserved factor that is both correlated with national BF transfers in the time series and that differentially affects growth in states that are more sensitive to national BF transfers in the cross section. As BF is a complex national program, we can rule out changes to program rules in response to shocks in particular states. Moreover, any rule changes tend to be permanent, so cannot be used to target year-to-year shocks in different states.

A more recent shift-share literature emphasizes two paths to identification (Borusyak, Hull, and Jaravel (2025)). The first path is through "exogeneity of the shares" (Goldsmith-Pinkham, Sorkin, and Swift (2020)) where BF exposure shares need to be uncorrelated with other growth shocks across locations. This is similar to a parallel trends assumption: absent BF, high and low BF exposure states would have otherwise seen similar growth trends. While BF exposure is non-random, it is not clear why growth residuals after fixed effects and controls (such as differential exposure to the national business cycle) would systematically differ.

The second path is through "exogeneity of the shocks" emphasized by Borusyak, Hull, and Jaravel (2021). This path suggests that national BF transfer shocks should not occur in years when high-exposure states have large growth shocks $e_{s,t}$, i.e., when the exposure-weighted residuals across locations are large. The requirement seems likely to be met as exposure weight is highest in

the poorer North/Northeastern states, and the national BF program does not respond to growth shocks in those states.

In addition to being exogenous, our national-exposure instrument needs to be relevant—that is, it has to be able to explain variation in state-level BF transfers. Figure 1 plots BF transfers as a percent of state output for the poorer Northern region and the richer Southern region as well as the national transfer as a percent of the national GDP. The transfer to the Northern states is an amplified version of trends in national transfers, whereas transfers to the richer Southern region are relatively flat. In general, changes in BF transfers in the Northern states (as a share of state output) respond more strongly to changes in national BF transfers than those in the Southern states do. This different sensitivity to changes in national BF transfers is used to identify the relative transfer multiplier.

Implementation. Our approach to compute state exposure to national BF shocks, γ_s, is based on the relative size of BF expenditures as a share of state GDP to national BF expenditures as a share of national GDP in a base period T, that is:

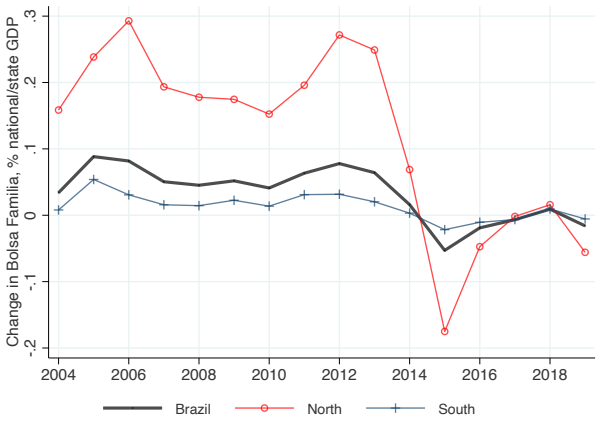
$$\hat{\gamma}_s = \frac{b_{s,T}/y_{s,T}}{b_{N,T}/y_{N,T}},$$

which means that if aggregate transfers increase by 1% of national output, transfers in state s increase by $\hat{\gamma}_s$% of state GDP. We chose our baseline period for the calibrated $\hat{\gamma}_s$ to be 2004 when the BF program started. Alternatively, we can also use other base years such as the last year of the sample in 2019, when the program was well-established, or an average exposure over a number of periods. The choice of the base period turns out not to be so important.[10]

Omitted Variable Bias (OVB). The second challenge is OVB, where "something else" can affect both BF transfers and output growth. In our first specification, "parsimonious", we include state fixed effects which allow for state-specific time trends in output growth and BF transfer growth, and time fixed effects to control for common aggregate shocks and aggregate policies such as national politics, global shocks, commodity prices, changes in distortionary taxes and monetary policy. Therefore, only confounding factors that vary across states and over time can be a threat to identification. Moreover, BF is a federal program with uniform national rules, which limits

[10] Another approach is to estimate γ_s in a first-stage regression using all available data, similar to Nakamura and Steinsson (2014). While the estimation approach chooses the γ_s to generate the best fit of national and state-level transfers, there is the concern that estimated γ_s may be more sensitive to measurement errors in state-level BF transfers, or shocks to state-level transfers. Therefore, we use the calibrated γ_s based on 2004 as our main instrument and present the alternative approach as a robustness exercise in Section D.1, which suggests that the results are similar across approaches.

Figure 1: **Sensitivity of states to national variation in Bolsa Familia transfers**



Notes: The figure plots Bolsa Familia transfers as a % of state/national value-added output. *Data source*: Authors' calculations with data from IBGE and the MDS.

the ability of state-level officials to change transfers in response to local shocks. We also study variations generated by a wide variety of BF policy changes between 2004–19, which reduces the sensitivity of our results to a chance correlation with an omitted variable in one year.

In a second specification, called "benchmark", we also control for state-specific sensitivities to national business cycles. This alleviates a concern that there might be aggregate shocks that drive changes in national BF transfers and that differentially affect the high-BF-exposure states. In other words, our parsimonious specification can be problematic if states with large (or small) BF transfers are more cyclically sensitive than others, and the *national* BF program might respond to *national* economic shocks. To address this important concern, we add into the parsimonious specification 27 controls of state dummy variables interacted with contemporaneous national value-added output growth rate.[11]

As a final check against OVB, we perform a battery of robustness tests in Section 6. First, we show our results are generally robust to several additional controls that vary across states and over time, including state-specific sensitivities to international commodity cycles, state lagged GDP growth rates, other federal transfers to states, the poverty rate and population growth. Second, we show that our results are generally not driven by a particular region, state or year, as might be the case with some types of OVB. We also conduct other robustness tests to spillovers, anticipation effects and other dynamic specifications.

[11] We compute the national output growth rate for each state by removing output of that specific state from the national output level.

4 Data

We compile a comprehensive annual state-level panel data set from different sources covering several macro variables such as value-added output, BF transfers, formal and informal employment between 2003 and 2019.[12] This section describes the construction of the main data series used for our estimation, including data sources and summary statistics. More detailed information on data sources is provided in Appendix B. As the state-level informal employment is new to this literature, we discuss its construction in more detail in Section 4.3. Table D.3 provides summary statistics for key variables for Brazil in different regions over the 2004-2019 sample period. As expected, BF transfers are much larger as a share of GDP in the poorer Northern and Northeastern regions than the richer South and Southeast. Output growth is around 2%-3% in all regions, though informal employment tends to grow faster in the poorer North and Northeast.

4.1 Bolsa Familia Transfers

Our main explanatory variable is Bolsa Familia expenditure at the state level over 2003–2019, downloaded from the Brazilian Ministry of Social Development (MDS). Data on the four pre-existing transfers rolled into BF are available from other sources, detailed in Appendix B.

4.2 State GDP and Formal Employment

The state output data come from the Brazilian Statistics Bureau (*Instituto Brasileiro de Geografia e Estatística*, IBGE). Using IBGE data, we construct a panel on state output in Brazilian reais (B$) in constant 2010 prices, disaggregated to 15 sectors (ISIC3). The formal employment data are from RAIS (*Relação Anual de Informações Sociais*) collected by the Brazilian Ministry of Labor, which covers the universe of formal employment in Brazil from 2003 to 2019. This high-quality official formal employment from RAIS has been widely used in other studies on Brazil, including Corbi, Papaioannou, and Surico (2019); Cunha et al. (2022) and Gerard, Naritomi, and Silva (2021).

4.3 Measuring Informal Employment

Our measure of informal employment includes informal employees and informal self-employed, but not informal employers or unpaid workers.[13] Combined, these categories represent nearly 90% of

[12] Our sample excludes the COVID-19 pandemic and the emergency transfers in Brazil implemented in 2020-2021.

[13] Informal employers make up only 3% of informal employment. Unpaid employees are more common (14% of informal employment), but have an unknown labor contribution as they are unpaid.

informal employment, shown in the right panel of Appendix Table D.4. As in Ulyssea (2018), informal employees are defined as employees who do not hold a formal labor contract (*carteira de trabalho*), and make up around half of all informal employment. Informal self-employed are defined as self-employed workers who do not have a tax registration number (*Cadastro Nacional de Pessoas Jurídica*), and make up 34% of informal employment in Brazil.

Data on informal employment mostly come from the national household survey PNAD/PNAD-Continua, available from IBGE. Informal employment is representative at the state level. The surveys were not conducted in 2010, so we use the census in that year.[14] About half of total employment is informal, reaching two-thirds in the poorer North and Northeast and 85% in the state of Maranhão, one of the poorest states in Brazil (see Appendix Table D.4).[15]

4.4 Data Cleaning

State-level GDP and employment growth in Brazil are extremely volatile, even at an annual frequency.[16] To make sure that our estimated multipliers are reflective of a general relationship, rather than a handful of influential observations, we clean the data in two steps: (1) winsorize growth rates (rounding to a ±5% bound after removing time/state FEs), (2) drop the 5% most influential observations identified using the *DFBETAs* algorithm of Belsley, Kuh, and Welsch (1980). In our robustness check in Section 6.4.2, we show that alternative data cleaning methods do not alter our results qualitatively.

Winsorizing is conducted as follows: for each variable (GDP growth, formal employment, informal employment) and sector (agriculture, mining, Rest of Economy (ROE)), we first winsorize the growth rates such that the distribution of the growth rates for each variable is roughly between −5% and 5% (after removing year and state fixed effects). For ROE GDP growth, this involves truncating the top 2% and bottom 2% of observations, though more volatile the sector/variable

[14] The PNAD is available from the start of our sample period until 2009, and between 2011 and 2015. In 2016, the PNAD was replaced with PNAD-Continua, which provides a more comprehensive territorial coverage and quarterly information on employment nationwide. The PNAD-Continua is available from 2016 to 2019. We use the 2010 Census to supplement the two household surveys.

[15] We cross-check the accuracy of *formal* employment in survey-based PNAD using administrative data taken from RAIS. While the level of formal employment is similar across the different datasets, the growth rates of state-level formal employment are more volatile in PNAD. The correlation of formal employment growth over 2004-2019 between RAIS and PNAD is 0.58, after dropping the outlier Roraima state (see Appendix Figure D.4(a)). For this reason, employment multipliers are estimated using both datasets, which reach similar conclusions (see Appendix Table D.5).

[16] Developing countries are well known to have growth rates much more volatile than those in developed countries. Sub-national regions are even more volatile due to less-diversified regional economies, and exposure to volatile sectors like agriculture and mining.

use a wider winsor percentile.[17] Sectoral levels are reconstructed by accumulating the winsorized growth rates, and the aggregate levels are obtained by summing across sectoral levels.

Influential observations are calculated using the DFBETAs method as follows.[18] Specifically, for each observation i, we calculate how much the multiplier changes when we exclude that observation ($|\beta - \beta_{(i)}|$). We drop the 5% of observations with the largest $|\beta - \beta_{(i)}|$.[19]

5 Main Results

This section presents the main results of our paper: starting from the relative multiplier for output, then breaking down the effects into tradable and non-tradable goods. We then present the findings on the labor market including both formal and informal employment.

5.1 First Stage

As plotted in Figure 2(a), poorer areas have greater exposure to changes in national BF transfers with $\gamma_s > 2$ in Brazil's poor North and Northeast (except Amapá state, AP), and $\gamma_s < 1$ in the richer Southern states and the Midwest. The first-stage F-statistic is over 100, which is well above the rule-of-thumb cutoff of 10, indicating the instrument is highly relevant. Furthermore, as plotted in Figure 2(b), the *actual* changes in state-level BF transfers as a percent of state value added are highly correlated with the *predicted* change using the instrument. Also, we observe little idiosyncratic variation in state-level BF transfers, which is unsurprising given that the BF program is a federal program with uniform rules.

5.2 Output Multipliers

Table 1 presents the baseline estimates of the relative output multipliers.

First, columns (1) and (2) of Table 1 present the parsimonious estimates for the relative output multiplier: the effects of relative BF transfers on relative output using ordinary least squares (OLS) and two-stage least squares (2SLS). The OLS estimate of the relative output multiplier is

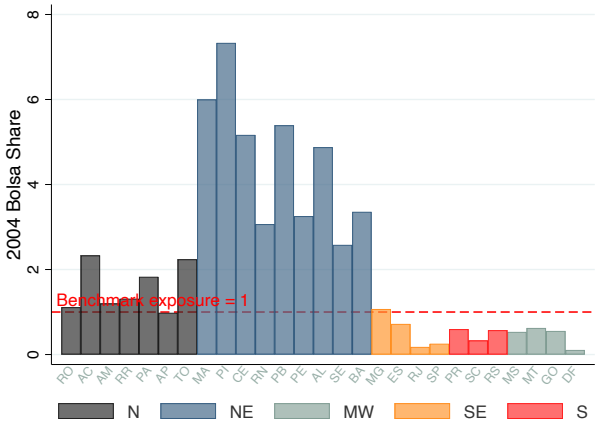
[17] For example, for agriculture and mining almost all specifications are winsorized at 25%. Here "mining" is the mining, quarrying, and petroleum sector. Formal employment is winsorized at 5% by default and informal employment at 15%.

[18] The influential observations are chosen based on the parsimonious specification, and for consistency, these are also used in the benchmark specification. Section 6.4.2 shows that calculating new outliers for the benchmark specification results in very similar estimates.

[19] This is equivalent to dropping if $\frac{|\beta - \beta_{(i)}|}{se_\beta} > 0.14$, where the cutoff 0.14 is chosen to exclude 5% of the sample. This is fairly similar to the standard cutoff of $2/\sqrt{n} = 0.1$ (with $n = 405$ in Belsley, Kuh, and Welsch (1980)).

Figure 2: **First stage: state transfers predicted by the national variation in BF**

(a) State exposure shares γ_s from 2004 data



(b) Actual versus predicted transfers,
% of state gross value added

Notes: γ_s are calculated from the benchmark specification. N, NE, MW, SE, S denote North, Northeast, Midwest, Southeast, and South regions, respectively.

2.37, statistically significant at the 10% significance level. The relative multiplier estimated using 2SLS, our preferred specification, is 2.00. The estimate is statistically significant at conventional levels across all our standard-error methods. This means that when a state receives extra BF transfers worth 1% of state GDP relative to other states, output on impact in that state tends to be 2 percentage points higher than the other states. The robust standard errors of these estimates are around 1.

Second, in the benchmark specification in which we additionally control for state-specific effects of national business cycles, Columns (3) and (4) of Table 1 report that both the OLS and 2SLS estimates of the relative output multipliers are larger: the 2SLS point estimate is 3.2. The 2SLS estimate is significant at the 5% significance level across different ways of computing the standard errors.

Our relative output multiplier estimate of between 2 and 3.2 is fairly large compared to the estimate of 1/3 for one-off transfer payments, and about 1.5 for permanent social security transfers across states in the United States in Pennings (2021). It is also larger than the relative government spending multiplier of 1.5 in the United States, as estimated in Nakamura and Steinsson (2014). Compared to the existing evidence in developing countries, our relative multiplier estimate of 2–3 is comparable to the village-level multiplier of 2.5 in Egger et al. (2022), who estimate the effects of a large one-off transfer in a randomized control trial in Kenyan villages. The implied relative

multiplier for transfers to municipal governments in Brazil in Corbi, Papaioannou, and Surico (2019) is also about 2, although transfers to governments are conceptually very different from those to individuals, and usually have a larger multiplier, as discussed in Pennings (2021). Taken together, these comparisons suggest that the size of the multiplier depends on the setting, so evidence from advanced economies may not extend to developing countries.

Table 1: **Relative output multipliers**

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Output growth				**B. Non-tradable output**		**C. Tradable output**	
	Parsimonious		**Benchmark**					
	OLS	**2SLS**	**OLS**	**2SLS**	**Parsimonious**	**Benchmark**	**Parsimonious**	**Benchmark**
GDP multiplier	2.37	2.00	3.06	3.22	4.03	3.03	-0.73	1.04
Default clustered SEs (state)	(1.25)*	(0.94)**	(1.68)*	(1.35)**	(0.94)***	(1.05)***	(0.43)	(0.57)*
Robust SEs	(1.03)**	(1.09)*	(1.24)**	(1.37)**	(0.89)***	(1.24)**	(0.51)	(0.67)
Wild bootstrap 90% CI								
Clustered by states	[.23, 4.3]	[.20, 3.6]	[.16, 5.8]	[.84, 5.5]	[2.3, 5.]	[1.0, 5.]	[-1.4, .01]	[.06, 2.0]
P-value	0.08	0.06	0.08	0.02	0	0.01	0.10	0.07
Clustered by states & years	[.10, 4.6]	[.43, 3.8]	[.25, 7.0]	[1.1, 5.0]	[2.0, 5.9]	[1.3, 4.7]	[-1.4, -.0]	[-.2, 2.5]
P-value	0.09	0.03	0.07	0.02	0.00	0.00	0.08	0.19
Control: Nat. Growth×State FE	-	-	Yes	Yes	-	Yes	-	Yes
State & Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	385	385	385	385	385	385	385	385
First-stage F-statistics		153		116	112	93.	126	120

Notes: The first four columns report the impact multiplier from a regression of state-level output growth on the Bolsa Familia transfer changes as a share of output. The last four columns are similar except the dependent variable is the change in non-traded output (columns 5–6) or tradable output (columns 7–8) as a share of total output. Columns (1) and (3) use an OLS specification whereas all other columns use 2SLS specification with a shift-share (Bartik) instrument. All specifications include state and year fixed effects, and the benchmark specification also includes controls for state-specific sensitivities to the national business cycle via the interaction between national GDP growth and a state fixed effect. The numbers in parentheses are standard errors calculated by different methods, whereas brackets represent 90% Confidence Intervals calculated by a Wild Bootstrap.

Tradable and Non-tradable Output. To investigate the mechanisms at play, we examine the effects of transfers on sectoral output. In theory, if the multiplier is working through standard Keynesian demand-side channels, we would expect growth in overall output to be driven by growth in non-tradable sectors, and output of tradable sectors would be largely unaffected, as those goods can be sourced from other states. To test this hypothesis, we use total value-added output in each state disaggregated into tradable and non-tradable sectors. The tradable sector consists of manufacturing, agriculture and mining output. The non-tradable sector consists of all other sectors. Our specification is now a modified version of that in Equation 1: the left-hand side variable is the change in tradable or non-tradable value-added output normalized by the lagged total output, and the right-hand side variable is the change in BF transfers as a share of total value-added output.[20] As reported in Columns (5) and (6) of Table 1, we find that an increase in BF transfers

[20]The multipliers in tradable or non-tradable specifications would normally *approximately* add to the relative multiplier for total output if the controls and sample were the same. However, we drop outliers in each regression

of 1% of the state output relative to other states increases the relative non-tradable output by 4% of total output in the parsimonious specification, and 3% of total output in the benchmark specification. The estimates are statistically significant at the 5% significance level by all different standard errors including the bootstrapped method. We note that in both specifications and from now on, we report all the estimates using the 2SLS approach and omit the OLS estimates. In all of these cases, instruments continue to be relevant with first-stage F-statistics above 90. In contrast, the effects of BF transfers on tradable output are small and even negative in the parsimonious specification, as shown in Columns (7) and (8). The estimates on tradable output are, however, mostly insignificant at standard significance levels. These results are consistent with the view that transfers affect output through an increase in demand for locally produced goods and services.

The increase in the non-tradable sector is consistent with the expenditure patterns of the poor in Brazil, as surveyed in the National Consumer Expenditure Survey "*Pesquisa de Orcamentos Familiares*" conducted prior to the implementation of Bolsa Familia in 2003. We show in Appendix Table D.6 that poor households from the bottom 25% of the national per capita household income distribution tend to spend more than 80% of their income on non-tradable goods and services such as housing, health, education and other services, and less than 20% on manufactured goods and other durable goods.

5.3 Cumulative Output Multipliers

To capture the dynamics in the relationship between relative BF transfers and local output, we estimate cumulative multipliers over longer horizons. The cumulative multiplier measures the cumulative output gain per each cumulative BF transfer during a given period. In our context, cumulative multipliers are helpful because they capture both the timing effects of BF transfer changes, since BF changes are often implemented mid-year, and the medium-run economic responses to transfers, which might be different to those in the short run.

The cumulative multiplier at each horizon h can be obtained through the following specification:

$$\sum_{j=0}^{h} \frac{y_{s,t+j} - y_{s,t-1}}{y_{s,t-1}} = \beta_h \sum_{j=0}^{h} \frac{b_{s,t+j} - b_{s,t-1}}{y_{s,t-1}} + X'_{s,t}\delta + \eta_s + \eta_t + e_{s,t}, \tag{2}$$

where $\sum_{j=0}^{h} \frac{y_{s,t+j}-y_{s,t-1}}{y_{s,t-1}}$ is the sum of $h+1$ cumulative output changes over horizons up to h

based on the DFBETA criteria, so the sum of the multipliers in tradable and non-tradable sectors is not the relative output multiplier.

periods and $\sum_{j=0}^{h} \frac{b_{s,t+j}-b_{s,t-1}}{y_{s,t-1}}$ is the sum of the corresponding changes in BF transfers over the same period. The cumulative multiplier, β_h, is the relative change in total output in a state that receives an additional R$1 of BF transfers relative to other states over the h periods.[21]

Table 2 reports the cumulative multiplier for horizons up to five years after the shock, with contemporaneous multiplier, $h = 0$, corresponding to our benchmark estimate. The estimated cumulative multipliers are quite large over different horizons: the 2SLS point estimates for the cumulative multiplier at the one-year horizon are 2.23 and 2.47, in the parsimonious and benchmark specifications, respectively. These estimates are statistically significant at the 10% significance level by three out of four of the reported standard errors. The first-stage F-statistics continue to be well above the rule-of-thumb cutoff of 10. The cumulative multipliers become slightly smaller, around 2 at the impact plus 2-year horizon, and continue to be statistically significant. At longer horizons, cumulative multipliers continue to decline slowly and start to lose significance. Combined, these estimates suggest that the increase in output is also highly persistent, as the BF increases are persistent.[22]

Table 2: **Cumulative multipliers over longer horizons**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	**Contemporaneous**		**1 year**		**2 years**		**3 years**		**4 years**	
	Parsi-monious	Bench-mark	Parsi-monious	Bench-mark	Parsi-monious	Bench-mark	Parsi-monious	Bench-mark	Parsi-monious	Bench-mark
GDP multiplier	2.00	3.22	2.23	2.47	2.18	1.85	1.85	1.91	1.35	1.40
Default clustered SEs (state)	(0.94)**	(1.35)**	(1.01)**	(1.21)**	(0.81)***	(0.92)**	(1.04)*	(1.49)	(0.97)	(1.23)
Robust SEs	(1.09)*	(1.37)**	(0.94)**	(1.34)*	(0.86)**	(1.28)	(0.82)**	(1.31)	(0.78)*	(1.13)
Wild bootstrap 90% CI										
Clustered by states	[.20, 3.6]	[.84, 5.5]	[.51, 4.0]	[.22, 4.7]	[.78, 3.]	[.33, 3.]	[-.27, 3.6]	[-.8, 4.5]	[-.9, 2.9]	[-.9, 3.6]
P-value	0.06	0.02	0.03	0.07	0.01	0.05	0.14	0.24	0.25	0.29
Clustered by states & years	[.43, 3.8]	[1.1, 5.0]	[-.2, 4.8]	[-.0, 5.0]	[.30, 3.8]	[.45, 3.3]	[-.37, 3.8]	[-.3, 3.8]	[-1., 2.7]	[-.0, 3.0]
P-value	0.03	0.02	0.15	0.11	0.06	0.05	0.14	0.12	0.36	0.11
Observations	385	385	358	358	331	331	304	304	277	277
First-stage F-statistics	153	116	108	78	121	95.	91	51	134	71

Notes: Columns (1) and (2) repeat the 2SLS specification in Columns (2) and (4) of Table 1. The other columns are analogous except that output growth and the scaled change in Bolsa Familia transfer are accumulated as Equation 2 over the additional h periods as indicated. See notes to Table 1 for additional details.

5.4 Employment and Labor Productivity

We next examine the effect of cash transfers on the labor market, including on formal employment, informal employment, total employment and formal wage-equivalent employment (FWE), as well as labor productivity. Because informal employment is associated with lower earnings and sometimes

[21] The instrument is then $\gamma_s \sum_{j=0}^{h} \frac{b_{N,j}-b_{N,t-1}}{y_{N,t-1}}$.

[22] Appendix Figure D.6 plots the impulse responses of BF transfers and GDP on impact up to 3 years after the shock. The responses are highly persistent, consistent with the cumulative multiplier reported here.

fewer hours, an informal job is not equivalent to a formal one. To properly account for the differences between informal and formal employment, we construct formal-wage equivalent (FWE) employment in state s and year t, $FWE_{s,t}$ as a weighted sum of all types of employment that discounts for lower productivity in informality. More specifically,

$$FWE_{s,t} = Emp^{formal}_{s,t} + \rho^{infemp}_{s,t} Emp^{infemp}_{s,t} + \rho^{infself}_{s,t} Emp^{infself}_{s,t}.$$

where $\rho^{infemp}_{s,t}$ is the relative wage of informal employees to that of formal employees in state s and year t. $\rho^{infself}_{s,t}$ is the analogous for informal self-employed.[23]

To estimate the effects of relative BF transfers on the labor market, we estimate a variation of Equation 1 as follows:

$$\frac{Emp^{j}_{s,t} - Emp^{j}_{s,t-1}}{Emp^{FWE}_{s,t-1}} = \beta^{j}_{0} \frac{(b_{s,t} - b_{s,t-1})}{y_{s,t-1}} + \eta_s + \eta_t + e_{s,t} \quad j \in \{\text{Formal, Informal, FWE, Total}\} \quad (3)$$

where the left-hand side variable is the change of formal (informal, FWE, or total) employment normalized by the lagged formal wage employment, and in the right-hand side, BF transfers are also normalized by lagged output.

Table 3: **Effects on the Labor Market**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Formal-wage Equiv.		**Total Employment**		**Formal Employment**		**Informal Employment**	
	Parsi-monious	Bench-mark	Parsi-monious	Bench-mark	Parsi-monious	Bench-mark	Parsi-monious	Bench-mark
Estimated effects	-0.73	0.57	1.91	3.17	-2.9	-1.3	4.73	4.16
Default clustered SEs (state)	(1.20)	(1.52)	(2.05)	(2.70)	(0.88)***	(1.40)	(1.58)***	(1.96)**
Robust SEs	[1.33]	[1.59]	[2.29]	[2.87]	[0.85]***	[1.31]	[2.25]**	[2.66]
Wild bootstrap 90% CI								
Clustered by states	[−2.8, 1.3]	[−2., 3.2]	[−1., 5.4]	[−1., 7.8]	[−4.3, −1.3]	[−3., 1.2]	[1.7, 7.]	[.41, 7.]
P-value	0.54	0.71	0.35	0.26	0.00	0.37	0.01	0.07
Clustered by states & years	[−3.4, 2.0]	[−2., 3.6]	[−2., 6.3]	[−2., 8.5]	[−4., −.2]	[−4., 2.1]	[.84, 9.3]	[−.9, 8.3]
P-value	0.59	0.73	0.45	0.33	0.07	0.48	0.05	0.15

Notes: This table reports 2SLS estimates of regressions of changes in employment (of different types), scaled by formal-wage-equivalent employment, on the change in Bolsa Familia transfers as a share of output, as in Equation 3. See notes to Table 1 for additional details.

[23] The discount factor is defined as $\rho^{i}_{s,t} = w^{i}_{s,t}/w^{formal}_{s,t}$ where $w^{j}_{s,t}$ is the average nominal wage of informal employment type j (employees or self-employed) in state s and year t. $w^{formal}_{s,t}$ is the analogous for formal employees. We compute nominal wages as the ratio between nominal earnings and total hours worked in state s and year t. Data on earnings and hours worked for informal employment is from PNAD and PNADC for 2004-2019, except 2010, which is interpolated. Data on formal earnings and hours worked are from RAIS. The average discount over 2004-2019 is $\rho^{infemp} = 0.4$ and $\rho^{infself} = 0.55$, so an informal job is equivalent to roughly 0.5 formal jobs. Consistent with that, in the back-of-the-envelope section, we apply an average $\rho = 0.5$ to calculate the FWE.

Formal-Wage Equivalent (FWE) & Total Employment Bolsa Familia only leads to at most a small increase in employment, and possibly none. Our preferred measure of the labor-market impact of BF transfers is FWE employment. Columns (1) and (2) of Table 3 show that a state receiving BF transfers worth 1% of its GDP more than other states has a small and statistically insignificant change in FWE employment in both parsimonious and benchmark specifications.[24] First-stage F statistics continue to be well above the common threshold level of 10 (not reported).

The weak response of employment to Bolsa Familia can also be seen in the regressions estimating the effect of BF transfers on *total employment*, which is the unweighted sum of formal and informal employment. As reported in Columns (3) and (4) of Table 3, the effect of BF on total employment is insignificant, though somewhat larger than the effect on FWE employment: a state that receives a relative 1% of GDP in transfers experiences an insignificant 2–3 percent increase in total employment.

Labor Productivity Since output increases without much change in FWE employment, BF transfers are associated with higher relative labor productivity, defined as the ratio of output to FWE employment. As plotted in the third panel of Figure 3, in a state that receives BF transfers worth 1% of its GDP more than other states, labor productivity in the benchmark specification increases by 4.6% relative to other states. The estimate for the effects of labor productivity is statistically significant at conventional levels.

Formal & Informal Employment. The last four columns of Table 3 disaggregate the employment impact of Bolsa Familia into a robust positive effect on informal employment, and a small or sometimes negative impact on formal employment. Specifically, a relative 1% GDP increase in BF transfers boosts informal employment by around 4.2%, with similar estimates across parsimonious and benchmark specifications, both significant at the 5% level as in Columns (7) and (8). In contrast, a relative 1% GDP increase in BF spending leads to a *fall* in formal employment by 1–3 %, though only the parsimonious specification estimates are statistically significant, as displayed in Columns (5) and (6).

We are careful not to over-interpret the estimated decline in formal employment, as this result is not robust across specifications. Specifically, in addition to the insignificance of the benchmark estimates, Appendix Table D.18 shows that if the dependent variable was the growth rate of formal

[24] A large informal jobs multiplier is not inconsistent with a small FWE multiplier. Informal jobs have lower earnings and lower hours than formal jobs, so their weight in FWE is much less than a formal job (which declines).

employment $\Delta FE_t/FE_{t-1}$—rather than $\Delta FE_t/FWE_{t-1}$ as in Equation 3— then the FE multiplier is now *positive* (but still insignificant).[25] Nonetheless, the absence of a positive impact puts our finding at odds with those of Gerard, Naritomi, and Silva (2021) (see Appendix C.1 for a discussion). As the Brazilian government observes whether a BF recipient is working in the formal sector, which will likely make workers ineligible for BF payments, a larger BF impact on informal employment is consistent with workers switching from formal to informal jobs in order to remain in the program. It also illustrates the importance of estimating the effect on informal employment, which can only be properly measured at the state level.

Sectoral Employment. Appendix Table D.7 decomposes the employment multipliers of different types of employment into the tradable and non-tradable sectors.[26] Consistent with the GDP multiplier, the relative multipliers for informal employment in the non-tradables sector are large and significant in most of the standard errors reported. In contrast, the effects of BF transfers on local employment in tradable sectors are either small, insignificant or negative. Nevertheless, within the non-tradable sectors, the effects of BF transfers on formal employment are still negative and are sometimes significant such as in the benchmark specification.

Overall, our results suggest that the effects of BF transfers on the labor market are estimated to be noisy, but there are clearly large positive effects on the non-tradable sector's informal employment.

5.5 Effects on Inflation and Wages

A demand channel may imply a rise in the inflation rate in response to a transfer shock, so we examine the effects of BF transfer on relative state inflation. Inflation is measured by the GDP deflator for each state. All the results are in Appendix Table D.8. In our benchmark specification, there is some evidence that states that receive more BF transfers tend to experience a higher inflation rate, although the standard errors are large. We further examine sectoral GDP deflator inflation rates and find that much of the increase in relative inflation is due to the non-tradable sectors such as construction. Finally, with data for CPI in 11 state capitals, we also find that BF

[25] This specification was presented in the first version of our working paper. In the employment growth rate specification, the sign or significance of informal, FWE or total employment multipliers does not change, although the impact on informal employment increases. We choose to normalize by FWE rather than a growth rate specification so that employment multipliers are additive.

[26] As in Section 5.2, the tradable sector includes agriculture, mining and manufacturing, and the non-tradable sector includes everything else, particularly construction and services.

transfers lead to a larger CPI inflation, and the estimates are imprecise given the smaller number of observations compared to GDP deflator inflation data.

We also examine the effects of BF transfers on real wages and earnings.[27] Appendix Table D.9 reports the results. We find that a relative increase in BF transfers leads to positive and statistically significant formal wage growth, and negative but insignificant informal wage growth. We also compute the responses of total earnings, as well as formal and informal earnings. The results for earnings are consistent with those of wages: total earnings increase in the state that receives more BF transfers than other states. The increase in total earnings is driven by an increase in formal earnings and small changes in informal earnings.

5.6 Economic Slack

We find that BF transfer multipliers for GDP and employment are larger when the local economy has higher economic slack. We classify each state in each period to be in a high (low) unemployment state if it is above (below) the panel median of 8.5%. We then estimate a state-dependent version of Equation 1 that accounts for a possible difference in the effects of BF transfers in the high unemployment and low unemployment states.[28]

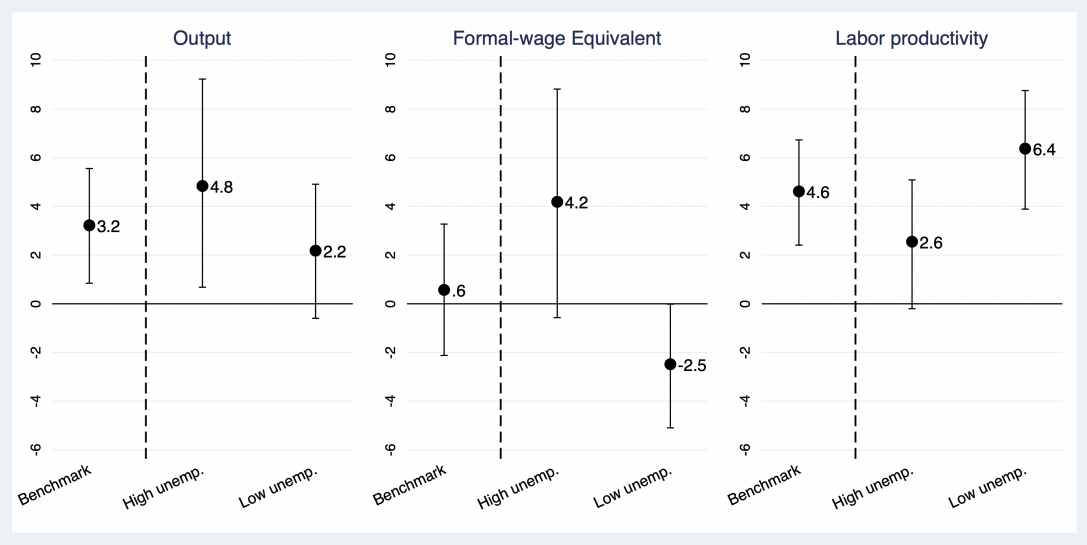
Figure 3 plots the results for the state-dependent relative multipliers for output, FWE employment and labor productivity. First, the relative output multiplier in higher slack (high unemployment) is larger, about 4.8, than that in the low unemployment state (2.2). The point estimate for the relative output multiplier is significant at the 10% significance level, but the estimate for the low unemployment state is insignificant at conventional levels. Second, BF has a larger positive impact on the labor market in a high unemployment state than in the low unemployment state. The effect of BF transfers on FWE employment is positive (4.2) in the high unemployment state, but statistically insignificant, while it is negative and significant (-2.5) in the low unemployment state.

Finally, the third panel of Figure 3 shows that the effects of BF transfers on labor productivity in the state-dependent specifications are all positive and significantly different from zero at the 10% significance level.

[27] We obtain real wages and earnings for formal and informal workers by deflating nominal wages and earnings by national CPI.

[28] The state-dependent specification includes an interaction of the change in BF transfers with an indicator for low and high unemployment states, i.e., $\frac{b_{s,t}-b_{s,t-1}}{y_{s,t-1}} \times I_{s,t}^{\text{slack}}$. The instruments are then our baseline IV interacted with an indicator for low and high unemployment states.

Figure 3: **BF effects and labor market slack**



Notes: All estimates are obtained using 2SLS. The first panel plots the relative output multipliers, the second panel plots FWE employment multiplier, and the third panel plots labor productivity multipliers as described in the text. Each point estimate is plotted with the 90% confidence band using bootstrapped method clustered by states. High or low unemployment is defined relative to a panel median of 8.5%.

5.7 Comparison with the literature

This section compares our result of formal employment with other papers. We further show that backing out output multipliers from employment multipliers can lead to biased conclusion about the effects of transfers on output, highlighting the importance of having a direct estimate of output multipliers.

Formal employment multiplier While Gerard, Naritomi, and Silva (2021) find that municipal-level formal employment *increases* in response to Bolsa Familia, we find that the impact of BF on formal employment is small or possibly negative. Appendix C.1 tries to reconcile these findings by estimating the effect of Bolsa Familia on formal employment at the municipal level using our Bartik instrument. While we *also* find a positive formal employment multiplier in the first half of our sample — which covers the period studied by Gerard, Naritomi, and Silva (2021) — the impact on formal employment turns negative in the second half of our sample. There are many differences in the types of BF policy changes and macroeconomic conditions throughout the sample. For example, earlier policy changes focused more on eligibility expansions. Macroeconomic conditions were more favorable in the first half of the sample (with high commodity prices), whereas the second half includes the deep recession starting in 2015. Intuitively, employers may be more likely to hire

hard-to-fire formal workers when the economy is stronger than in weaker times. Our estimates over a 15-year period cover both periods, improving external validity.

Implications for back-of-the-envelope (implied) multipliers. Many papers in the literature estimate employment multipliers, and then use a production function to calculate the implied back-of-the-envelope GDP multipliers.[29] However, implied back-of-the-envelope GDP multipliers will be severely downward biased when fiscal policy boosts labor productivity, as we demonstrate in this section.

To see this, start with a simple Cobb-Douglas production function $Y = AH^{1-\alpha}$, where H is total hours worked.[30] Using g to denote a growth rate, one can see that $g_Y = g_A + (1-\alpha)(1+\chi)g_E$, where g_E is the growth rate of employment, and $(1+\chi) \approx 1.12$ is the elasticity of total hours to employment from Chodorow-Reich (2019). Assume, as in the literature, that fiscal policy *only* affects GDP through employment. Then the GDP multiplier $\mathcal{M} = (1-\alpha)(1+\chi) \times \mathcal{M}_E$, where the labor share of GDP, $1-\alpha = 0.55$ in Brazil (PWT11). Using our benchmark estimates in Table 3, we have an insignificant Formal-Wage Equivalent Employment Multiplier of $\mathcal{M}_E = 0.57$. Hence, the back-of-the-envelope GDP multiplier should be around 0.35 ($= 0.57 \times 0.55 \times 1.12$), which is an order of magnitude smaller than our direct GDP multiplier estimates of 2–3.[31]

Moreover, in a Brazilian context, the back-of-the-envelope method is even less reliable when only the response of formal employment is observed. The reason is that we find that the largest employment response is in the informal sector. For example, if we assume that there is no change in informal employment in response to Bolsa Familia, then applying the benchmark formal employment multiplier of $\beta_E = -1.3$ in Table 3 yields an implied GDP multiplier of -0.8 ($= -1.3 \times 0.55 \times 1.12$), which has the wrong sign.[32]

6 Robustness Checks

This section discusses the robustness for our estimated relative transfer multipliers. We first estimate the output per capita multiplier, then consider additional controls, spillovers, and different

[29] Chodorow-Reich (2019), Gerard, Naritomi, and Silva (2021), Corbi, Papaioannou, and Surico (2019) and Cunha et al. (2022) all do this.

[30] The capital component K^α is assumed to be fixed in the short run.

[31] A similar alternative approach, as in Chodorow-Reich (2019), is to start with the same production function, but rearrange the expression in terms of the fiscal cost for each job generated.

[32] There are other ways to make this calculation, but they all yield substantial downward bias. In our case, they are even less reliable because formal employment multipliers are noisily estimated, and not robust across specifications.

samples to analyze the stability of our results. We also examine possible anticipation effects and estimates using alternative specifications. Our results are also robust to alternative ways of calculating our Bartik-style instrument; we relegate the details to Appendix D.1.

6.1 Output per capita

Many papers in the literature focus on explaining per capita output growth to remove possible confounding trends in population growth. Since state-level population data in Brazil are noisy, we estimate output per capita multipliers as a robustness check and report the results in Appendix Table D.10.[33] We replace GDP growth on the left-hand side of Equation 1 with GDP per capita growth rate, and the explanatory variable is the change in BF transfers per capita, as a share of lagged output per capita. We find that the estimate for output per capita multiplier in the benchmark specification is 2.96 and significant at the five percent significance level, suggesting that the relative output increase is not driven by changes in population.[34]

6.2 Additional Controls

We address potential OVB in the benchmark estimates of the relative transfer multiplier by adding several different controls to the benchmark specification. The results are displayed in Table 4. For ease of comparison, we also repeat the parsimonious and benchmark estimates in Columns (1) and (2).

First, we address a potential OVB coming from the fact that Brazil is a major exporter of many commodities, including iron ore, oil, soybean, coffee, and sugar. Brazil was affected by the high commodity prices in the 2000s. While commodity prices are subsumed into time fixed effects, they might still affect our results if they affect states differentially. To that end, we add state dummy variables interacted with contemporaneous Brazilian commodity price index growth rate (27 controls) into our benchmark specification. Column (3) of Table 4 displays the 2SLS estimate: the relative output multiplier remains large, at 2.56 and significant at the 10 percent level.

Second, we control for the state's exposure to minimum wage variations in the benchmark specification. There are several reasons to add this control. First, the detrended national minimum wage is correlated with national BF expenditure. Second, states that receive more BF (high γ_s) may

[33] Our measure of population growth is taken from PNAD for the 2005-2015 period, except 2010 which is interpolated, and PNADC for the 2016-2019 period.

[34] We also estimate the effects of BF changes on relative population growth rate and report the results in Appendix Table D.10. However, we are cautious about the results given the volatile population data.

have greater exposure to the minimum wage.[35] The control we add to the benchmark specification is the share of minimum wage employment in state s year t interacted with the national variation in minimum wage in year t. As displayed in column (4) in Table 4, the relative output multiplier is about 2.5 and remains statistically significant using both wild-bootstrap statistics clustered by states and by state-year and default clustered standard errors.

Third, we additionally control for other federal transfers to states, as they can be correlated with BF transfers and likely have an impact on state output. In particular, we sum six types of federal transfers: (i) the public pension to the poor ("*Beneficio de prestacao continuada*"), (ii) the largest programs of federal transfers to municipal and state governments, ("*Fundo de participacao dos municipios*" and "*Fundo de participacao dos estados*") as in Corbi, Papaioannou, and Surico (2019), (iii) funding to public education (FUNDEB), (iv) tax on manufacturing (IPI), and (v) royalties from natural resources extractions (mostly oil and gas to states in the Southeast).[36] We include the contemporaneous change of total other federal transfers between t and $t-1$ normalized by the lagged state-level value-added output $(t-1)$, i.e., $\frac{GFed_{s,t}-GFed_{s,t-1}}{Y_{s,t-1}}$, as well as one lag of this variable. Column (5) displays the BF multiplier for this case: the point estimate is about 3.4, and is now significant at the five percent level. The coefficient on the contemporaneous changes in the other federal transfer programs is small but statistically significant.

Fourth, column (6) adds state tax revenue change as a percent of lagged state GDP to the control set in the benchmark specification, as taxes may hit poor and rich states differently over time. Again, the result suggests that the relative output multiplier is still large (3.6 at the point estimate) and statistically significant.

Fifth, we add several controls to better capture dynamics in the data. To address potential mean reversion in state-level growth, we add one lag of the state-level value-added output growth rate as a control in the benchmark specification, which has no effect on multipliers (Column 7). The coefficient on the lag of state level value-added output is also insignificant (not reported). Since BF transfers are persistent, we also estimate a specification where we add a lag of the change in state BF transfers into the benchmark specification. As presented in column (8), the estimated relative output multiplier is 2.77 and statistically significant. Adding a lag for both the dependent

[35] Brotherhood, Iachan, and Parente (2024) also find that minimum wage increases informality.

[36] BF represents about 0.5% of national GDP, depending on the year, while the pension for the poor is about 0.3% of GDP, transfers to states and municipalities are 1.2% and 1.1% of GDP, respectively, and funding to public education is 1% of GDP. Tax on manufacturing and royalties are 0.1% and 0.2% of GDP, respectively. The *Beneficio de prestacao continuada* (BPC) program starts in 2004. In this specification, we assume that BPC does not change between 2003 and 2004.

variable (state output growth) and state BF transfer changes does not alter the result significantly: the point estimate for the relative output multiplier is 2.90 and significantly different from zero, as displayed in column (9). In another robustness check, we include two lags of the instrument, state output growth, and BF transfer changes, as suggested by Ramey (2021). The results in column (10) suggest that the relative output multiplier is still above 2, but it loses significance.

Sixth, as mentioned at the end of the previous section, we add controls for the log of state population and its first lag to flexibly control for any confounding variables related to population size or population growth. These additional controls may be necessary, as our main dependent variable is not in per capita terms. As shown in column (11), we find that controlling for population raises the multiplier slightly, but the significance levels remain the same as in the benchmark. Population controls themselves are not significant, and not reported. The results are similar if we control for population growth rate instead.

Seventh, as BF is a transfer targeted at the poor and changes in BF transfer may be due to trends in relative local economic conditions, we add the contemporaneous and one lag of the poverty rate in each state to control for the level or change in the state-level poverty rate. The estimate, displayed in column (12), does not differ much from the benchmark.

Finally, we include all these additional controls into the specification. The point estimate in column (13) is 3.25, similar to the benchmark specification, and is statistically significant at the 10% significance level in two of our four ways to compute standard errors.[37]

We run the same robustness exercises for formal and informal employment. The results are also similar to the baseline although less significant. Specifically, there is some evidence that relative formal employment falls and informal employment increases, although some estimates have large standard errors. The details are presented in Appendix Table D.16 and Appendix Table D.17.

The takeaway from these robustness checks is that our estimated multipliers are fairly robust to different controls.

6.3 Spillovers

One concern about the interpretation of the relative multiplier is the potential for large fiscal spillovers across states as their economies are integrated. To address this problem, we follow Auerbach, Gorodnichenko, and Murphy (2020) and add controls for the contemporaneous BF

[37] We also consider different price deflators for BF transfers (recall these are deflated by the National CPI as the default). The results in these exercises are fairly similar and are reported in Appendix Table D.13. Note that we always deflate state GDP with the GDP deflator.

Table 4: Relative output multiplier: robustness to additional controls

	(1)	(2)	(3)	(4)	(5)	(6)
			Benchmark + Controls			
Dependent variable:	**Parsimonious**	**Benchmark**	**Commodity price**	**Minimum wage**	**Other federal transfers**	**Tax revenues**
GDP multiplier	2.00	3.22	2.56	2.48	3.35	3.55
Default clustered SEs (state)	(0.94)**	(1.35)**	(1.41)*	(1.37)*	(1.35)**	(1.26)***
Robust SEs	[1.09]*	[1.37]**	[1.5]*	[1.71]	[1.38]**	[1.37]***
Wild bootstrap 90% CI						
Clustered by states	[.20, 3.6]	[.84, 5.5]	[.04, 4.]	[-.0, 4.9]	[.91, 5.7]	[1.3, 5.7]
P-value	0.06	0.02	0.09	0.11	0.01	0.00
Clustered by states & years	[.43, 3.8]	[1.1, 5.0]	[.29, 4.1]	[1.1, 3.6]	[.78, 5.6]	[1.6, 5.2]
P-value	0.03	0.02	0.08	0.02	0.04	0.01
Observations	385	385	385	385	385	385
First-stage F-statistics	153	116	95	107	114	116

	(7)	(8)	(9)	(10)	(11)	(12)	(13)
				Benchmark + Controls			
		Lagged state		**Ramey 2021**			
Dependent variable:	**Lagged LHS**	**change in BF**	**(7) + (8)**	**Lags**	**Population**	**Poverty**	**ALL**
GDP multiplier	3.44	2.77	2.90	2.25	3.40	2.78	3.25
Default clustered SEs (state)	(1.32)***	(1.37)**	(1.38)**	(1.65)	(1.01)***	(1.39)**	(1.82)*
Robust SEs	[1.33]***	[1.66]*	[1.61]*	[1.76]	[1.4]**	[1.45]*	[2.21]
Wild bootstrap 90% CI							
Clustered by states	[1.0, 5.]	[.41, 5.0]	[.55, 5.]	[-.6, 5.1]	[1.6, 5.1]	[.39, 5.2]	[-.0, 6.6]
P-value	0.01	0.05	0.04	0.18	0.00	0.05	0.10
Clustered by states & years	[1.4, 5.3]	[1.3, 4.2]	[1.3, 4.7]	[-1., 5.8]	[2.0, 4.7]	[.75, 4.6]	[-2., 8.2]
P-value	0.01	0.02	0.01	0.26	0.00	0.04	0.27
Observations	385	385	385	385	385	385	385
First-stage F-statistics	115	98.	98.	102	126	111	118

Notes: The first two columns (1) and (2) are the parsimonious and benchmark specification from Table 1 estimated using 2SLS, without additional controls. All of the robustness (3) to (13) are estimates that use benchmark specification with added controls. Column (3) displays the estimate when we control for contemporaneous commodity price index interacted with state FEs. Column (4) reports the estimate with additional controls for minimum wage. Columns (5) and (6) additionally control for other federal transfers and state tax revenue, respectively. Column (7) displays the estimates when one lag of the state value added growth is added, column (8) adds a lag of the BF transfer as a control, then column (9) presents the estimate when we add one lag of both the BF transfer and the state value added growth rate. Column (10) includes two lags of the dependent variable, the instrument and state BF transfers as suggested in Ramey (2021). Column (11) reports multipliers when we add log state population (and first lag) as a control. Column (12) controls for state poverty level. Finally, column (13) reports the results with all controls simultaneously.

transfer changes in neighboring states. We find that the relative multiplier in this case for the benchmark specification is 2.62. The estimate is statistically significant across different ways of computing standard errors. The coefficients on the neighboring states' BF transfers are small. In the benchmark specification, an R$1 increase in BF transfers in the neighboring states leads to about an 11-cent increase in home output. The details of the estimates are reported in Appendix Table D.11. This result suggests that the spillover effects are likely small.

6.4 Influential Observations, States, Regions or Years

This section presents the robustness of our results with respect to the data of individual states and years, as well as the data cleaning process.

6.4.1 Individual States, years or regions

We next examine whether our results are driven by specific influential years, states or regions, which we drop one-by-one. All the results are presented in Appendix Table D.14.

First, dropping each year from our sample does not alter our benchmark results in a meaningful way. In particular, the relative output multipliers in the benchmark specification are all around 3 and statistically significant. The estimates for formal jobs multipliers remain negative with large standard errors, and those for the informal jobs multipliers are between 3.6 and 5.9, and significant, similar to the benchmark of 4.16.

Second, as reported in Appendix Table D.14 Panel B, our results are also generally robust to dropping states one-by-one. The estimated relative output multipliers remain significant and range between 2.7 and 4, and BF transfers have large and positive effects on relative informal employment but negative and insignificant on relative formal employment. Dropping the North, South, and Midwest regions one-by-one (Panel C) generally yields similar GDP or formal/informal employment to those in the baseline. However, dropping the Northeast increases the output multiplier, and dropping the Southeast reduces it, with the effects being mostly offsetting in our full-sample results.[38]

6.4.2 Alternative Data Cleaning

In this subsection, we examine the robustness of our estimated multiplier with alternative combinations of winsorizing and dropping outliers. Appendix Table D.15 shows that across different data cleaning methods, the benchmark multipliers continue to be generally large and significant, varying between 3 and 5. In addition, multiplier estimates are not systematically larger or smaller, but standard errors tend to be wider with outliers/influential observations included. Recall that our default method includes winsorizing data by sector so that the growth rate has a range ±5% (after time/state FEs), and dropping the 5% most influential observations, as identified by DFBETAs applied to the parsimonious specification (1st Column). Robustness 1 instead estimates using

[38] Excluding the Northeast also increases the formal employment multiplier and reduces the informal employment multiplier.

the raw data (no winsorizing or outliers dropped). This increases the benchmark multiplier to 3.8 (significant at the 5% level), although the parsimonious multiplier is smaller and insignificant.

The second and third robustness tests start with the raw data/full sample, and winsor the data and drop outliers one at a time. First, applying our default winsor but not dropping outliers (Robustness 2) results in even larger multipliers which are significant at the 1% level for both parsimonious and benchmark specifications. Not winsoring the data (Robustness 3) does not affect the benchmark multiplier substantially, relative to the default, but standard errors are much wider. Part of the loss of the significance for benchmark is because influential observations are chosen based on the parsimonious specification: if instead they are chosen based on the benchmark, it regains significance at the 10% level (Robustness 4).[39]

In the final robustness test, winsorizing at a uniform 5% level across sectors rather than higher/lower depending on volatility as in our baseline results in a larger benchmark multiplier. The benchmark multiplier continues to be significant at the 5% level, but the parsimonious multiplier becomes smaller and insignificant.

As our baseline drops 5% of the sample, which is 20 outliers, based on DFBETAs scores, we further examine how dropping different numbers of outliers ranked in descending order by their DFBETAs scores affects our estimates. We find that multipliers are always large (> 1) and significant, even after dropping 10% of the sample. In addition, locally, our multipliers are quite stable. Finally, dropping outliers generally reduces multiplier estimates, suggesting our results are relatively conservative. Detailed results are summarized in Appendix Figure D.5.

6.5 Anticipation Effects

Households and firms in standard economic models are forward-looking, so they react to expected policy changes. While BF recipients are mostly poor households who arguably may be less able to respond to news, the firms that employ and sell to them may be more able to do so. To examine whether there are significant anticipation effects, we estimate a version of the benchmark specification (Equation 1) as follows:

$$\frac{y_{s,t+h} - y_{s,t-1}}{y_{s,t-1}} = \beta_h \frac{b_{s,t} - b_{s,t-1}}{y_{s,t-1}} + X'_{s,t}\delta + \eta_s + \eta_t + e_{s,t} \qquad \text{with} \qquad h = \{-3, -2\}. \tag{4}$$

[39] Note that in general, choosing outliers from the benchmark specification results in a slightly larger benchmark multiplier, 3.4 and significant at 1%, but a substantially lower and insignificant parsimonious specification multiplier.

The anticipation effect, β_h for $h = -3, -2$, captures the response of output to changes in BF transfers that happen at time t. We find that the estimates of β_h one to three years before the shock are small, negative, and insignificant at conventional levels, i.e., there is no statistically significant anticipation effect on output. The results are plotted in Appendix Figure D.8.

6.6 Alternative Dynamic Specifications

As noted in Section 2, changes in Bolsa Familia are often implemented mid-year, which leads to changes in spending that are split across two years. While we estimate cumulative multipliers in our main results, alternative approaches could be (1) estimating a dynamic specification where we include one lag of BF transfer changes, and the effect of BF transfers is the sum of the coefficients on BF transfer changes, or (2) regressing two-year changes in output on two-year changes in BF transfers as in Nakamura and Steinsson (2014). Appendix Table D.12 shows that the estimates from the two-year change specification, 3.69, are roughly in line with our benchmark and statistically significant at 5% significance level. In the dynamic specification, the sum of the coefficients is 4.34, which is larger than our benchmark and has larger standard errors, so it is not statistically distinguishable from the benchmark. Employment multipliers are also broadly similar to our main results using these alternative specifications, as detailed in Appendix Table D.12. These analyses suggest that our baseline contemporaneous and cumulative multipliers broadly capture the effects of BF transfers on the local economy.

7 Interpretation of Empirical Estimates in a New Keynesian Model

Given the empirical estimates, we investigate whether a standard open-economy New Keynesian (NK) model, which has been shown to explain the effects of transfers in advanced countries, can rationalize the findings in a developing country like Brazil. We find that a standard NK model with low wealth effects on labor supply can generate large relative output multipliers, but the model additionally needs variable utilization in order to better match the muted response of employment.

We provide a summary of the model here and refer the reader to Appendix E for further details. The model is an extension of a standard open-economy NK model, as in Nakamura and Steinsson (2014). The economy is a monetary union consisting of a small home region, size n, representing a state in Brazil whose residents receive a net BF transfer, and a large foreign region with the rest of the population, size $1 - n$, which pays for the BF transfer via lump sum taxes.

A share of households ω are hand-to-mouth, i.e., they consume all their income each period, and the remaining fraction $1-\omega$ are Ricardian and can trade non-state-contingent one-period bonds. Households maximize their lifetime expected utility, $E_0 \sum_{t=0}^{\infty} \beta^t U\left(C_t^j, L_t^j\right)$, where $j = \{R, N\}$ denotes Ricardian and non-Ricardian (hand-to-mouth) households, and C_t and L_t denote consumption and labor, respectively.[40]

Each region produces both tradable (T) and non-tradable (S, "services") goods. Both goods are used for consumption, i.e., consumption for Ricardian households is given by

$$C_t^R = \left[\alpha_C^{\frac{1}{\theta_C}} \left(C_t^{R,T}\right)^{\frac{\theta_C-1}{\theta_C}} + (1-\alpha_C)^{\frac{1}{\theta_C}} \left(C_t^{R,S}\right)^{\frac{\theta_C-1}{\theta_C}}\right]^{\frac{\theta_C}{\theta_C-1}}, \tag{5}$$

where $C_t^{R,T}, C_t^{R,S}$ are tradable and non-tradable consumption for Ricardian households, respectively, α_C is the share of tradable goods in final consumption and θ_C is the elasticity of substitution between tradable and non-tradable goods (the hand-to-mouth specification is analogous). Tradable goods are, in turn, a CES aggregate of imperfectly substitutable tradable goods across regions with an elasticity of substitution θ_T.

A final good firm in each sector in each region aggregates intermediate goods into final goods. We denote Y_t^T and Y_t^S to be tradable and non-tradable final aggregate output, respectively. Total output is defined as the sum of sectoral output evaluated at steady-state prices. There is a continuum of intermediate goods firms in each sector $i \in [0,1]$. The production function of differentiated product by firm i in the tradable sector is given by:

$$Y_t^T(i) = A^T U_t^T(i)^{\alpha_U} L_t^T(i)^{\alpha_L}, \tag{6}$$

where A^T denotes the level of total factor productivity, $U_t^T(i)$ denotes capacity utilization, which can be interpreted as capital utilization, labor utilization and technology utilization, and α_L is the labor share. We introduce variable utilization in the model to match both the large GDP multiplier and small FWE employment response in the data. This variable utilization feature is commonly modeled in macroeconomics, as it captures changes in other inputs such as longer operating hours,

[40] All variables for households in the foreign region are denoted with an asterisk. As in Bilbiie (2009) and Furlanetto and Seneca (2014), we do not specify the functional form for the utility function. Instead, we directly impose the values for parameters for the Frisch elasticity of labor supply, the intertemporal elasticity of substitution, and the wealth effects on labor supply in the log-linearized model. This approach gives us the flexibility to examine the importance of these parameters in explaining the observed relative transfer multipliers. In fact, we can calibrate these parameters so as to replicate separable utility function, or Greenwood, Hercowitz, and Huffman (1988) utility function. The details of this general preference are in Appendix E.6.

increased equipment usage, and technology adoption, and it helps the model to match salient features of the data, see for example the recent work by Klein and Linnermann (2024).

Intermediate firm i pays the utilization cost associated with changes in capacity utilization of the following quadratic functional form:

$$CU^T\left(U_t^T(i)\right) = \phi_1^{CU,T}\left(U_t^T(i) - 1\right) + \frac{\phi_2^{CU,T}}{2}\left(U_t^T(i) - 1\right)^2, \tag{7}$$

where $\phi_1^{CU,T}, \phi_2^{CU,T} > 0$, and a larger value of $\phi_2^{CU,T}/\phi_1^{CU,T}$ increases the curvature of utilization costs, making capacity utilization less responsive.

We assume nominal price rigidities *a la* Calvo with parameters θ_p^T in the tradable sector. The setup for intermediate firms in the non-tradable sector is analogous. These assumptions lead to standard NK Phillips curves for each sector. We assume that monetary policy at the national level follows a standard Taylor rule.

The national government transfers cash to the home region, NTR_t^{reg}, by imposing a size-adjusted lump-sum tax on the foreign region.[41] Cash transfers are targeted at hand-to-mouth households in the poorer home region, but funded by lump-sum taxes on Ricardian HHs in the foreign region, i.e.,[42]

$$NTR_t^{reg} = -\frac{1-n}{n} NTR_t^{reg\star}.$$

Mechanisms. In the NK model, a transfer from the federal government to individuals in the small home region boosts demand for home goods to the extent (i) that the transfer is spent rather than saved and (ii) that spending is on locally produced goods. In our model, the transfers are almost permanent, so they will be spent in the home region even by Ricardian households. However, whether they are spent on local goods depends on home bias in consumption. Because prices are sticky, output is partially demand-determined in the short run, so the increase in local demand raises output. As prices adjust in the longer horizon, higher local demand feeds into higher prices and wages rather than higher output, so the multiplier declines.

Calibration. We briefly discuss our calibration of the model; details are in Appendix Table E.20. We calibrate the model at a quarterly frequency, and to match the empirical estimates, we aggregate simulated data from the model into annual frequency. The home region has a size

[41] This is modeled as an AR(1) process for tr_t, the change in real transfer, defined as nominal transfer in the home region, NTR_t^{reg}, deflated by CPI (P_t) for the change in real transfers. $tr_t = \rho_{tr^1} tr_{t-1} + e_t^{tr}$.

[42] Specifically, we assume a fraction $\omega_T \in [0,1]$ of the transfer goes to non-Ricardian households. In the baseline, as BF is a targeted program, $\omega_T = 1$. In the foreign region, $\omega_T^\star = 0$.

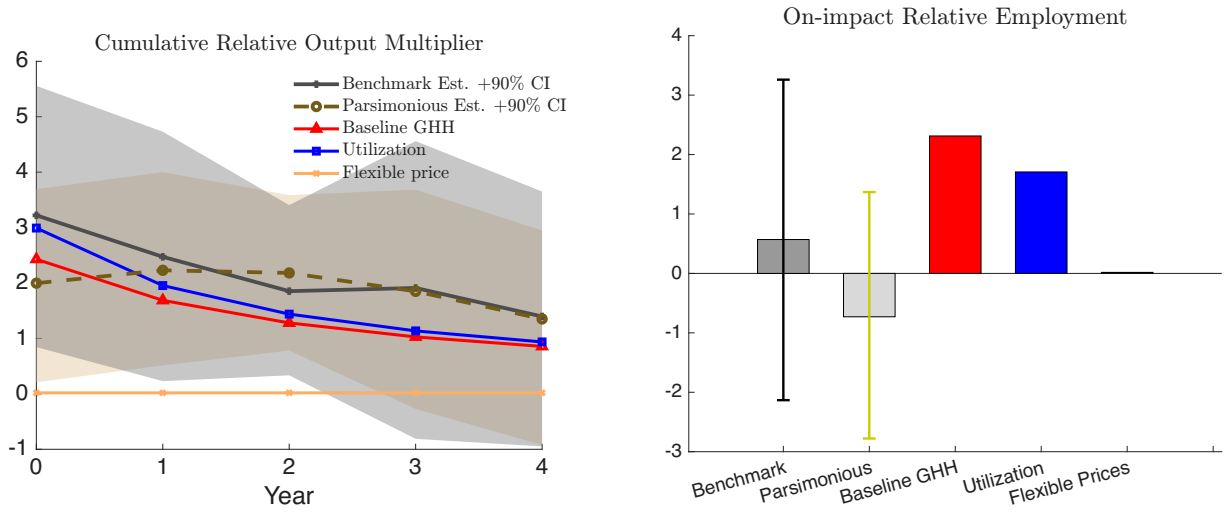
$n = 1/27$, as there are 27 states in Brazil including the Federal District. The share of tradables in consumption goods α_C is set to be 0.243 according to our calculation in Table D.2, where services account for 75.7% of consumption at the national level. We also calibrate the elasticity of substitution between tradable and non-tradable goods in consumption, θ_C, to be 1, i.e., home consumption is a Cobb-Douglas function of tradable and non-tradable goods. To simplify the model with little effect on the results, we set a large elasticity of substitution between home and foreign regions for tradable goods, i.e., θ_T is 10. We set the share of hand-to-mouth households, ω, to 0.54, the fraction of Brazilian households unable to raise emergency funds equal to 1/20 of GNI per capita (Global Findex 2017, as reported in Bracco et al. (2021)). For comparison, Pennings (2021) uses 1/3 for US states. The sticky price parameters for both tradable and non-tradable sectors are $\theta_p^T = \theta_p^S = 0.75$, as is standard in the literature. In the production function, the labor elasticity α_L is 0.58, so that the implied labor share is 0.55, consistent with Penn World Table data for Brazil. In our baseline, we assume GHH preferences with no wealth effects on labor supply, but shut down variable utilization.[43]

Results. The left panel of Figure 4 plots the model-implied cumulative multipliers over several horizons along with their empirical counterparts. The baseline GHH model (red) generates a large impact multiplier of around 2.5, which is between our estimates of the benchmark (3.2) and parsimonious (2.0) specifications. Moreover, the baseline GHH model is also consistent with a large impact multiplier on non-tradable output in Figure 5 and an almost-zero multiplier on tradable-sector production. As such, the benchmark GHH model does a reasonable job of matching the evidence on the size of GDP impact multipliers. Moreover, it also performs much better than a flexible price model, which predicts a multiplier of close to zero (orange lines).

However, the baseline GHH model does not perform as well over longer horizons, or for employment multipliers. After the first year, the baseline GHH model-implied multiplier is less than the benchmark and parsimonious model estimates by around 1 unit. At the longest horizons, the model-implied cumulative multipliers fall below unity, while the estimated counterparts are above 1. Moreover, the mechanism in the baseline GHH model involves a sizable increase in employment, whereas the FWE empirical estimates fail to find evidence of such an increase (the right panel of Figure 4).

[43] There is little evidence in the micro-development literature in favor of negative wealth effects from cash transfers targeted at poor households (see Baird, McKenzie, and Özler (2018) for a survey of the micro evidence). Banerjee et al. (2020) goes further and, using a field experiment in Ghana, argues that transfers can *increase* labor supply as higher incomes boost health, allowing people to work longer and harder (the *physiology* channel) and they can also encourage better decision-making (the *psychology* channel).

Figure 4: **Rationalizing estimated BF multipliers using a NK model**



Notes: In the left panel, the black line with the light gray shaded 90% confidence band area plots the benchmark cumulative multipliers as reported in Table 2. The dotted light gray with circles is the parsimonious estimates with the 90% confidence band in light beige shaded area. The red line with triangle signs plots the multipliers in the baseline calibration with GHH preferences. The blue line with square signs plots the multipliers in the baseline model with GHH and variable utilization. The yellow line with x signs plots the multipliers in the model with GHH preferences and flexible prices. The right figure plots the on-impact responses of employment: the first two bars are the benchmark and parsimonious FWE employment estimates with the 90% confidence bands, the next three bars are the model-implied hours responses in the baseline GHH model, GHH model with utilization, and GHH model with flexible prices, respectively.

To match this feature of the data, we allow for variable utilization in the baseline GHH model (blue lines). Specifically, we set the utilization cost to $\frac{\phi_2^{CU,T}}{\phi_1^{CU,T}} = 1.17$, following Smets and Wouters (2007), and $\alpha_U = 1 - \alpha_L$ (Utilization case), keeping everything else unchanged. The model with variable utilization generates a smaller employment response than the baseline model, bringing the response closer to the empirical estimates, as plotted in the right panel of Figure 4. Furthermore, the model-implied output multipliers also match the data better. As plotted in the left panel of Figure 4, the relative output multiplier is 3 on impact, which is closer to the empirical estimate in the benchmark specification. The sectoral multipliers also improve: Figure 5 shows that the non-tradable output multiplier is closer to its empirical counterpart, while the tradable multiplier remains small, consistent with the data.

We conclude that a relatively standard NK model with GHH preferences can partially rationalize the empirical multipliers. While the calibrated baseline GHH model is able to rationalize a large impact multiplier similar to that estimated in the data and its sectoral decomposition, multipliers at longer horizons are a little too small, and it misses the lack of an employment response seen in the data. An extension with variable utilization improves the fit for both employment and output,

although the response of hours worked is still larger than the empirical point estimate, and the model-implied multipliers at longer horizons are still smaller than the empirical counterparts.[44]

To identify the key determinants of the multiplier, we also consider several alternative model specifications, including a high HtM share, greater trade openness of the home region, separable preferences, and sticky wages (see Appendix Figure E.9). First, while the hand-to-mouth share is larger in developing countries than in advanced countries, it is not an important determinant of the size of the multiplier in the case of a permanent transfer shock. The reason is that the BF transfers are close to permanent, so even Ricardian households spend most of the transfers. Therefore, increasing the HtM share (or making transfers untargeted) has little effect on the multiplier size. Second, a less open region would have a larger relative multiplier, as demand is concentrated more on home-produced goods. Conversely, greater trade openness reduces the relative multiplier by increasing import leakages, highlighting the importance of trade openness as a key determinant of the relative multiplier. Third, wealth effects on labor supply are an important determinant of the multiplier. Under standard separable preferences, households have less incentive to work following the transfer because the positive wealth effect reduces labor supply, resulting in a smaller output multiplier. Finally, adding sticky wages has little effect on the multipliers because GHH preferences eliminate wealth effects on labor supply, limiting the role of wage rigidities.

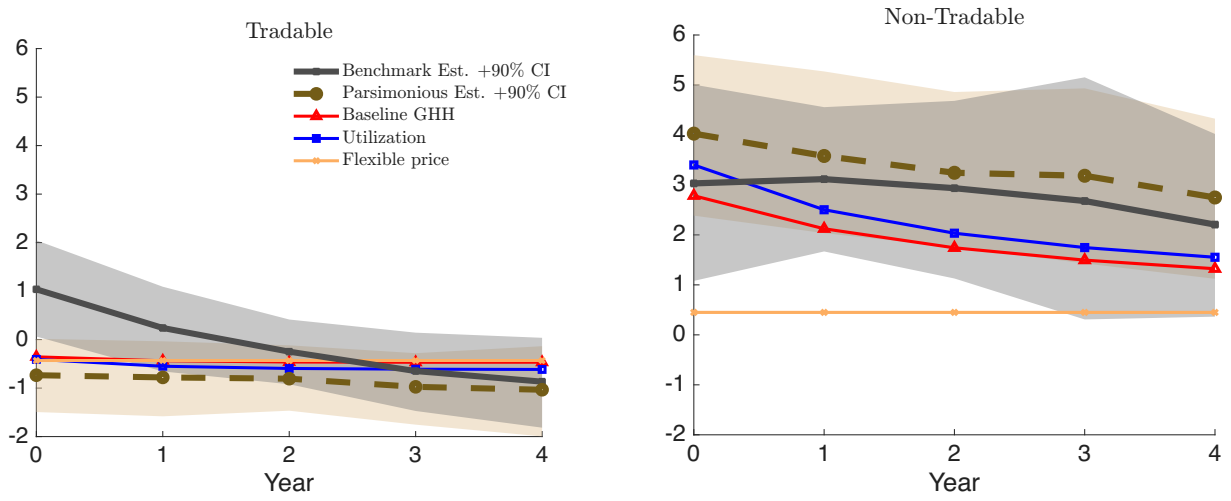
8 Conclusion

This paper provides new estimates of the effect of cash transfers on output and employment growth in a developing country using the data from Brazil's Bolsa Familia program. Our macroeconomic focus at the state level with many years of policy changes allows us to directly estimate GDP multipliers and the impact on informal employment, both of which have been missing from the literature.

We find a large macroeconomic effect of cash transfers, driven by local demand effects. Specifically, states that receive R$1 more in BF transfers, relative to other states, experience a relative R$2–3 increase in output driven by the non-tradable sector, and the effect persists for several years. We estimate that informal employment increases substantially, but formal employment stagnates or even falls, leaving the total effect of BF on formal-wage employment close to zero. An open-economy New Keynesian model with no wealth effects can explain the large effects of BF transfers

[44] Our model also has implications for the national output multiplier, which is presented in Appendix E.7.

Figure 5: **Sectoral Output Multipliers**



Notes: In both figures, the black line with the light gray shaded 90% confidence band area plots the benchmark cumulative multipliers. The dotted light gray with circles is the parsimonious estimates with the 90% confidence band in light beige shaded area. The red line with triangle signs plots the multipliers calculated using the baseline calibration with GHH preferences. The blue line with square signs plots the multipliers in the baseline model with GHH and variable utilization. The yellow line with x signs plots the model-implied multipliers with GHH preferences and flexible prices.

on output but generates employment responses that are too large. Incorporating variable utilization into the model helps to come closer to the data but still falls short in explaining the muted employment response.

We hypothesize that incorporating some of the supply-side benefits of cash transfers, such as labor, capital (human and physical) and TFP (via reduced misallocation), as documented in the microeconomics literature, may be important. For example, Sadoulet, de Janvry, and Davis (2001) find cash transfers boost incomes of liquidity constrained farmers in Mexico, de Mel, McKenzie, and Woodruff (2008) find large returns to a one-time cash transfer to micro enterprises in Sri Lanka, Best, Lobel, and Pinho Neto (2026) find positive health impacts of Bolsa Familia, and Paul Schultz (2004) finds a 10% increase in schooling from a cash transfer program in Mexico. All of these channels, which can affect productivity, are missing from our NK model, but form an important future research agenda.

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Appendix

A Additional details on Bolsa Familia

To better understand the variation in state-level BF transfers, it is useful to decompose state-level BF expenditure into three components: (i) state enrollment quotas, (ii) state coverage rate (beneficiaries per quota), and (iii) average benefit per beneficiary.

Quotas are updated *infrequently* with an information lag of two to three years after the release of a new PNAD or Census, as shown in Appendix Figure D.3(a).[45] The initial quotas were established in late 2003 using information from the 2001 PNAD. In 2006, the quotas were revised with the 2004 PNAD, leading to a redistribution of quotas across municipalities, but without substantial aggregate change (see Feler (2015)). In 2009, there was a sizable increase in quotas due to a change in the methodology of poverty measurement and new data from the 2006 PNAD (see Gerard, Naritomi, and Silva (2021)).

The coverage rate was only about 40% in 2004, as the program was new, but it slowly increased to about 90% in 2006, as it took time to implement quotas, which we plot in Appendix Figure D.3(b). Rules for coverage also change over time: with the rise in income, the initial eligibility income threshold in 2004 was revised in 2006, then the MDS passed a new "rule of permanence" in 2010, which allows a family's income to fluctuate above the income ceiling for as long as two years before becoming ineligible. In 2011, the coverage rate approached 100% and remained at that level, except for a small drop in 2017 when the MDS canceled about 8% of the existing beneficiaries due to fraud and irregularities.

Average benefits per household are the main source of variation in transfers, particularly after 2009. As a result of several changes in the program design, the average benefit rose 45% in real terms between 2010 and 2014. One of the most important reforms was a (roughly) 50% increase in the children's variable benefit at both intensive and extensive margins in 2011.[46] Another major change was the Benefit to Overcome Poverty (BOP), a "poverty gap" payment of the difference between income and the poverty line, introduced in 2012.

[45] The 2023 quota update was based on the 2010 Census, but is not in our sample.

[46] In 2011, the variable benefit per child increased 45% in real terms and the maximum number of eligible children per household was upgraded from 3 to 5, as plotted in Appendix Figure D.3(c).

B Data sources

In this section, we provide more details on the data sources:

Bolsa Familia expenditure. Administrative data on monthly Bolsa Familia expenditure at the municipal level were downloaded from the MDS (https://aplicacoes.cidadania.gov.br/vis/data3) on 18 June 2021 and aggregated to state-level annual expenditure. We deflate Bolsa Familia transfers by the National CPI, all measured in 2010 prices, because we do not want shocks to state-level *producer* prices to drive changes in real BF transfers.

National Consumer Price Index (CPI). The CPI is produced by IBGE and is known as IPCA (Índice de Preços ao Consumidor Ampliado). The IPCA was downloaded from the IPEA website (http://www.ipeadata.gov.br/ExibeSerie.aspx?serid=38391) on 5 April 2023.

Other federal transfers in 2003. In 2003, four existing federal programs were consolidated into the Bolsa Familia. We use data on these federal transfers to properly account for the *change* in transfers in 2004 (the first year of Bolsa Familia and of our sample). As with the BF, these programs are also deflated by the National CPI. The details for each program are provided below:

- Bolsa Escola (School Allowance) provided conditional transfers to boost preschool enrollments of children of poor families. The number of children enrolled in the program for each state was taken from Schwartzman (2006) (Table 12) downloaded from http://www.schwartzman.org.br/simon/2006_Bolsaesc.pdf on 26 October 2021. State-level annual expenditure was calculated as R$180 multiplied by the number of children. R$180 was the annual benefit per child (https://pt.wikipedia.org/wiki/Bolsa-escola).

- Bolsa Alimentação (Food Allowance) was a health and nutrition program. State level expenditure was calculated as the national expenditure multiplied by the state share of beneficiaries (state beneficiaries over total beneficiaries). Both variables were taken from the Brazilian Ministry of Health https://bvsms.saude.gov.br/bvs/publicacoes/alimenta_saudavel.pdf on 26 October 2021.

- Auxílio Gás (Gas aid) provided subsidies for cooking gas. This program was linked to Bolsa Escola, providing R$90 per year for families enrolled in that program. Due to lack of data on state-level expenditure, we multiply the annual expenditure per family (R$90) by the number of beneficiaries of the Bolsa Escola in that state.

- Cartão Alimentação (Food Card) was designed to eradicate extreme hunger. Total expen-

diture (mostly in the North and Northeast regions) was taken from Balsadi et al. (2004) – download here. The number of beneficiaries per state, used to calculate state-level expenditure, was taken from Pasquim (2006) (Gráfico 02) downloaded from the UnB website on 10 November 2023.

State Gross Domestic Product All variables related to state GDP are taken from the System of Regional Accounts of the Brazilian Statistics Bureau (Instituto Brasileiro de Geografia e Estatística, IBGE). To compute GDP and Gross Value Added (VA) at different levels of aggregation, we used several tables provided by IBGE. The tables were downloaded from the IBGE website under "Tabelas/PIB pela Ótica da Produção (2002-2020)/Especiais" on 26 October 2021. Below we provide a brief summary of the data sources and the manipulations needed to compute the main output variables from 2004 to 2019:

1. State GDP
   - Nominal GDP in state s and year t: $NGDP_{st}$
     - Source: IBGE spreadsheet "Tabela 1"
   - Volume of state GDP Index: I_{st}^{GDP}
     - Source: IBGE spreadsheet "Tabela 3"
   - Real GDP in state s and year t: $RGDP_{st}$
     - Computed iterating back and forwards $NGDP_{s,2010}$ with the growth rate of I_{st}^{GDP}
2. State VA
   - Nominal VA in state s and year t: NVA_{st}
     - Source: IBGE spreadsheet "Tabela 4"
   - Volume of state VA Index: I_{st}^{VA}
     - Source: IBGE spreadsheet "Tabela 5"
   - Real VA in state s and year t: RVA_{st}
     - Computed iterating back and forwards $NVA_{s,2010}$ with the growth rate of I_{st}^{VA}
3. Sectoral state VA
   - Share of state s in VA of sector i: $\eta_{st}^{i} \equiv \frac{NVA_{s,t}^{i}}{NVA_{BR,t}^{i}}$

  - Source: IBGE spreadsheet "Tabela 6"

- Share of sector i in the national VA: $\gamma_t^i \equiv \frac{NVA_{BR,t}^i}{NVA_{BR,t}}$
  - Source: IBGE spreadsheet "Tabela 10.4" downloaded on 26 October 2021.
- Nominal sectoral state VA
  - Computed as: $NVA_{st}^i = NVA_{BR,t} \times \eta_{st}^i \times \gamma_t^i$
- Volume of sectoral state VA Index: $I_{st}^{VA,i}$
  - Source: IBGE spreadsheet "Tabela 5"
- Real sectoral state RVA_{st}^i
  - Computed iterating back and forwards $NVA_{s,2010}^i$ with the growth rate of $I_{st}^{VA,i}$

Formal employment. Formal employment data are taken from the Brazilian Matched Employer-Employee (Ministry of Labor – RAIS). The state-level aggregates from RAIS were downloaded from the Brazilian data repository *Base de Dados* on 12 September 2022.

Population and Poverty. State-level population and poverty rates were taken from the National Household Sample Survey (PNAD). The PNAD was carried out annually by the Brazilian Bureau of Statistics (IBGE) until 2016 and is representative at the state level. The PNAD was replaced, with updated methodology, with the Continuous National Household Sample Survey - PNADC. Both PNAD and PNADC were downloaded from the IBGE website under "Microdados" on 12 September 2022.

Informal employment in 2010. Informal employment and informal income in 2010 were taken from the Brazilian 2010 Census, downloaded from the SIDRA portal `https://sidra.ibge.gov.br/tabela/3586` on 27 September 2022.

C Comparison with Literature

C.1 Formal Employment and comparison with Gerard, Naritomi, and Silva (2021)

The closest paper to ours is contemporaneous work by Gerard, Naritomi, and Silva (2021), who study the effect of an expansion in Bolsa Familia coverage around 2009–11, which they find *boosted* formal employment at the municipal (AMC) level. This might be surprising as we find Bolsa Familia

had a small effect on employment overall, and a zero-to-negative effect on formal employment. In this section, we try to reconcile these results.

To better compare results, we re-estimate the formal jobs multiplier (inverse of cost per job), but at the municipal level using our 2SLS Bartik-style instrument over our whole sample.[47] Results are in the first column of Appendix Table C.1, and we find that a transfer of R$100,000 larger than other states generates 0.5 more formal sector jobs, which is insignificantly different from zero and similar to our main estimates.

However, one of the main differences is the time period studied. Gerard, Naritomi, and Silva (2021) focus on a natural experiment over 2009–11, whereas we focus on a longer period from the start of Bolsa Familia up to COVID-19. The macroeconomic environment in Brazil changed substantially over this period. To test if this is important, we estimated the formal jobs multiplier, splitting the sample in the middle. For the earlier period, in Column 2 of Appendix Table C.1, we find that an extra R$100,000 generates 11 formal-sector jobs, significant at the 5% level. This is similar to the estimate of 10 in Gerard, Naritomi, and Silva (2021). However, in the second half of the sample (Column 3 of Appendix Table C.1), the sign of the relationship flips, with an extra R$100,000 of transfers causing a cut of 2 formal jobs.[48]

One explanation is that in the first half of the sample the macroeconomic environment was generally good – Brazil benefited from the commodity boom and unemployment was generally falling. As a result, the job market was generally stronger, and so employers were willing to hire formal-sector workers (which are difficult to fire). However, in the second half of the sample, Brazil experienced a severe recession due to the end of the commodities boom (among other factors), so employers were less confident to add formal-sector workers. Indeed, Gerard, Naritomi, and Silva (2021) find the extra formal sector workers were of low skill, who are likely similar to informal sector workers we find were hired in Table 3. This highlights the importance of using a longer sample to boost external validity.[49]

[47] Specifically, we regress the growth rate of formal employment on the change in Bolsa Familia spending (in R$100,000) per formal worker (at the AMC level). We include AMC and year fixed effects. All estimates are 2SLS with a Bartik-style instrument constructed at the AMC level: the Bartik weight is BF spending per AMC formal worker in 2004, relative to national BF spending per formal worker, which is then multiplied by the change in national Bolsa Familia spending per formal worker. The data are cleaned as follows: dropping the top and bottom 1% municipalities by population (similar to Gerard, Naritomi, and Silva (2021)), dropping the top and bottom 5% of the FE growth distribution and removing very high Bartik weights > 50.

[48] The results, especially the sign flip in the second half, are generally robust to other specifications. For example, adding state-by-year FEs (as in Gerard, Naritomi, and Silva (2021)) increases the size of the first-half FE impact but also makes it less precisely estimated (though still significant), with the second-half estimates almost unchanged. Results are also broadly robust to alternative more aggressive data cleaning of large Bartik weights, or estimating FEs over the whole sample rather than by subsample (not reported).

[49] Nonetheless, there are many other differences across the studies. For example, our Bartik-style instrument best

We also find that the estimated impacts of Bolsa Familia on formal sector employment are sensitive to the estimation method and the way that states are weighted.[50] In a past version of this paper, we found that BF transfers tended to increase formal sector employment. However, further investigation suggested that this result was sensitive to (1) estimating Bartik weights, rather than the more common method of fixing them at the start of the sample, and (2) normalizing by FE rather than FWE employment (in the denominator). Normalizing by FE dramatically increases the weight on states with very low formal employment, i.e. the poorest states, whereas normalizing by FWE Employment generates a more balanced measure of employment growth.

Table C.1: Formal Jobs per R$100,000 of Bolsa Familia at Municipal Level

	2005–19 (Full)	2005–11 (1st Half)	2012–19 (2nd Half)
Formal jobs per R$100,000	0.5	11.45***	-1.99***
	(0.37)	(2.16)	(0.4)
Obs	46,590	21,734	24,839
First stage F stat	719	48	808

Notes: Standard errors are clustered at the AMC level. 2SLS Bartik-style specification with Bartik weight of BF spending per AMC formal worker in 2004, relative to national BF spending per formal worker, which is then multiplied by the change in national Bolsa Familia spending per formal worker.

D Additional Robustness Tests

D.1 Alternative Instruments

Our results are computed using the change in national BF transfers as a share of national GDP interacted with state dummy variables as instruments for changes in state-level BF transfers as a share of state GDP. In this framework, the heterogeneous exposures, γ_s, are the fixed share at the beginning of the sample in 2004.

An alternative approach, as discussed in Section 3, is to estimate this exposure based on each state's BF transfers as a share of state GDP in some base year, relative to the national BF share of GDP, similar to Nakamura and Steinsson (2014). Another possibility is to use the exposure share calibrated to the 2019 level, or an average between 2004 and 2019. We consider these alternative IVs in this robustness check.

picks up changes in BF that boost expenditure in proportion to national trends, such as increases in benefits or broad expansion of eligibility. In contrast, the natural experiment in Gerard, Naritomi, and Silva (2021) isolates increased enrollment due to changes in the way that poverty is measured, which is likely mostly orthogonal to our Bartik-style variation.

[50] This echoes Ramey (2019)'s criticism of the impacts of the ARRA, which are much smaller when US states are weighted by population.

Specifically, our estimated instrument is the estimated coefficients, $\hat{\gamma}_s$, on the interaction of state dummy variables $\mathbf{1_s}$ with national BF transfers in the first-stage regression:

$$\frac{b_{s,t} - b_{s,t-1}}{y_{s,t-1}} = \gamma_s \mathbf{1_s} \frac{b_{N,t} - b_{N,t-1}}{y_{N,t-1}} + X'_{s,t}\delta_s + \eta_s + \eta_t + \varepsilon_{s,t}, \tag{8}$$

Similar to the calibrated IV, $\hat{\gamma}_s$ is the estimated heterogeneous exposure of state s to aggregate BF changes.

Table D.1 presents estimates of the impact relative multiplier using the alternative instrument for different base years, including an average over the whole sample. The results suggest that estimated multipliers are large, between 3 and 5, and are statistically significant. Specifically, using the estimated exposure as an instrument, we find that the relative multipliers are 1.5 and 4.1 in the parsimonious and benchmark specifications, respectively. As reported in column (4), the benchmark estimate is statistically significant at the 1% significance level. When we use the calibrated exposure in 2019 as a base year, the estimates as shown in columns (5) and (6) go up to 3.6 and 5.5 and are statistically significant in both parsimonious and benchmark specifications. The estimates are slightly lower (2.7 and 4.6) if we use the calibrated share as the average between 2004 and 2019. We note that the calculated exposure shares are all strong instruments, with large first-stage F-statistics.

The reason the main (calibrated) and alternative (estimated) instruments generate similar impact multipliers is that state BF expenditure shares are very good proxies for the estimated γ_s, with a correlation of 0.94. Appendix Figures D.7(a) and D.7(b) show that the baseline calibrated exposure shares line up close to the 45-degree line for both estimated share and the average share between 2004 and 2019.

Table D.1: **Relative Output Multiplier with Alternative IVs**

Dependent variable:	(1) **Default (2004 BF shares)** **Pars**	(2) **Bench**	(3) **Estimated Gammas** **Pars**	(4) **Bench**	(5) **2019 BF shares** **Pars**	(6) **Bench**	(7) **2004-2019 BF shares** **Pars**	(8) **Bench**
GDP multiplier	2.00	3.22	1.48	4.14	3.61	5.51	2.71	4.57
Default clustered SEs (state)	(0.94)**	(1.35)**	(1.32)	(1.51)***	(1.21)***	(1.69)***	(1.16)**	(1.41)***
Robust SEs	[1.09]*	[1.37]**	[1.18]	[1.46]***	[1.2]***	[1.53]***	[1.12]**	[1.41]***
Wild bootstrap 90% CI								
Clustered by states	[.20, 3.6]	[.84, 5.5]	[-.8, 4.0]	[1.3, 7.0]	[1.4, 5.]	[2.5, 8.]	[.609, 4.6]	[2.1, 7.1]
P-value	0.06	0.02	0.31	0.01	0.00	0.00	0.03	0.00
Clustered by states & years	[.43, 3.8]	[1.1, 5.0]	[-1., 4.8]	[1.9, 6.4]	[1.1, 6.4]	[2.6, 8.4]	[.697, 4.8]	[2.7, 6.5]
P-value	0.03	0.02	0.42	0.01	0.04	0.00	0.05	0.00
Observations	385	385	385	385	385	385	385	385
First stage F-statistics	153	116	72	33	155	193	644	459

Notes: The first stage equation is: $\frac{b_{s,t}-b_{s,t-1}}{y_{s,t-1}} = \phi(\gamma_s' \frac{b_{N,t}-b_{N,t-1}}{y_{N,t-1}}) + X_{s,t}'\delta + \eta_s + \eta_t + \varepsilon_{s,t}$, where the alternative exposure variable γ_s' is calibrated directly using relative BF expenditure $\gamma_s' = \frac{b_{s,T}}{y_{s,T}} / \frac{b_{N,T}}{y_{N,T}}$ over some base period T, rather than being estimated in the first stage (as in the main estimates).

D.2 Additional Figures and Tables

Figure D.1: **Bolsa Familia Variation over time**, % of value-added output

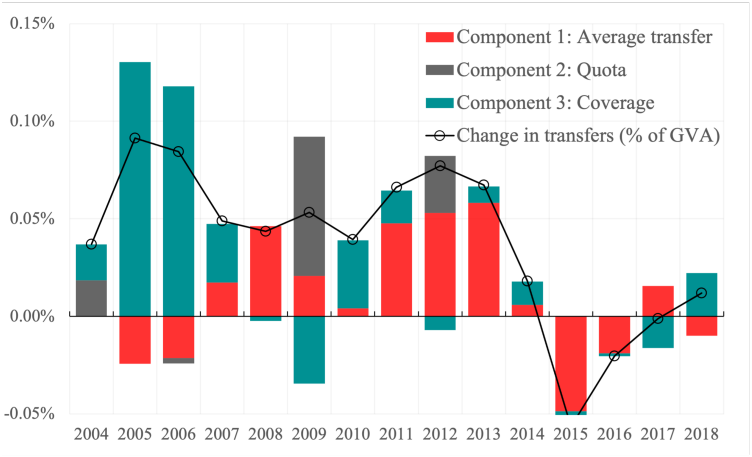


Figure D.2: **Timing of Bolsa Familia**: Average change in spending across states for each month

(a) 2004 **(b) Average 2005-2019,**

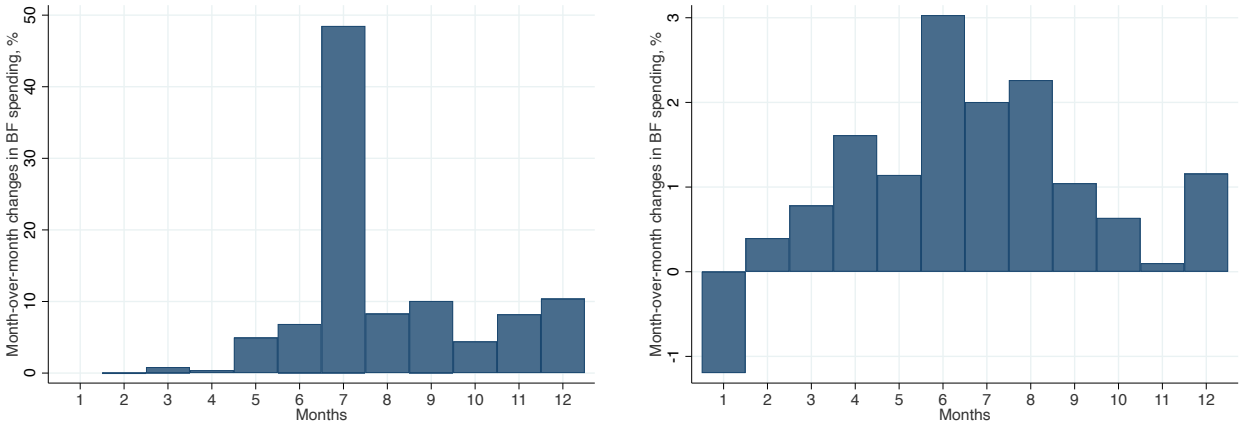
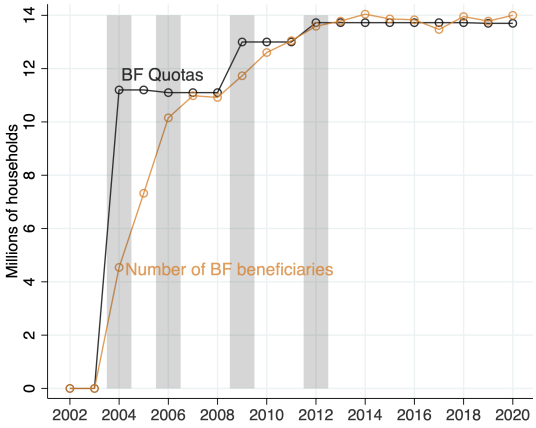
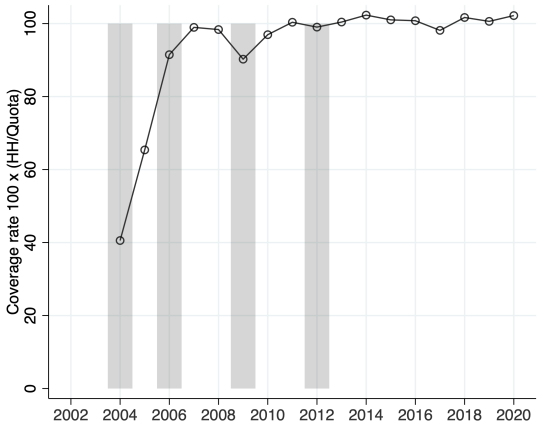


Figure D.3: **Decomposition of the National BF: Quota, Coverage, and Average benefits**

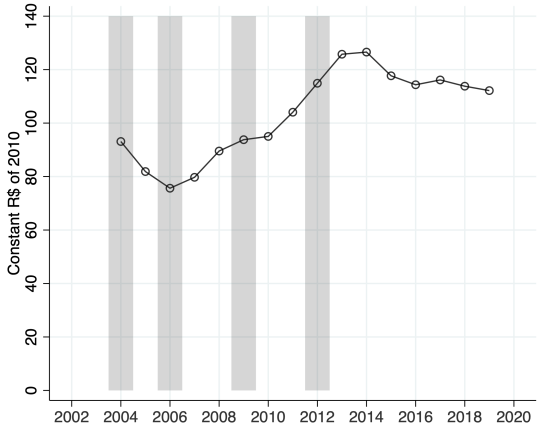
(a) Quotas



(b) Coverage rate



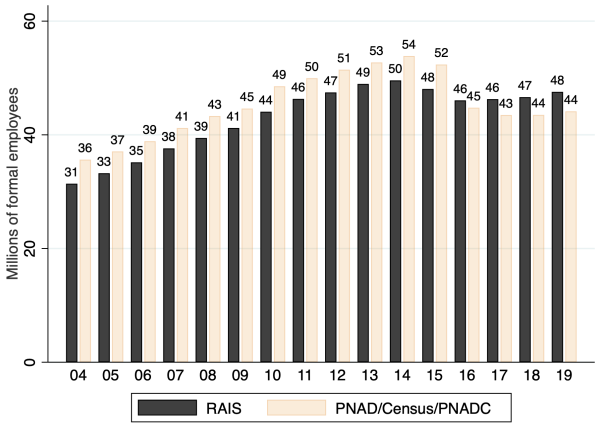
(c) Average monthly benefit



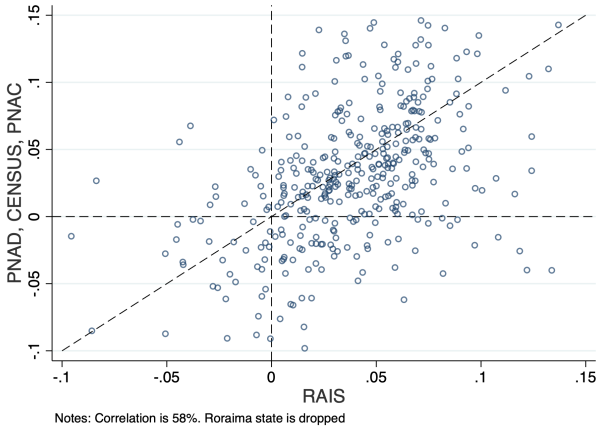
Notes: Figure D.3(a) plots the evolution of national quotas, Figure D.3(b) the coverage rate, and Figure D.3(c) the average benefit per household. *Data source*: Authors' calculations based on data from the Ministry of Social Development.

Figure D.4: **Formal Employment Data Comparison: RAIS versus PNAD**

(a) Total Brazilian formal employment



(b) State-level growth rates of formal employment



Notes: This figure plots formal employment at the state level in RAIS and PNAD (panel (a)) and the growth rates of state-level formal employment in these two data sources (panel (b)).

Figure D.5: **Robustness of output multipliers to different outliers**

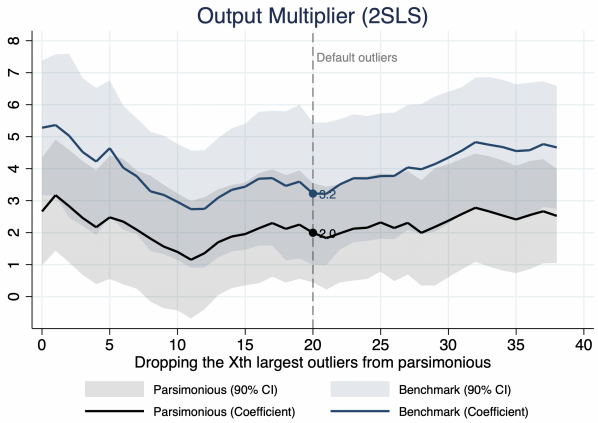
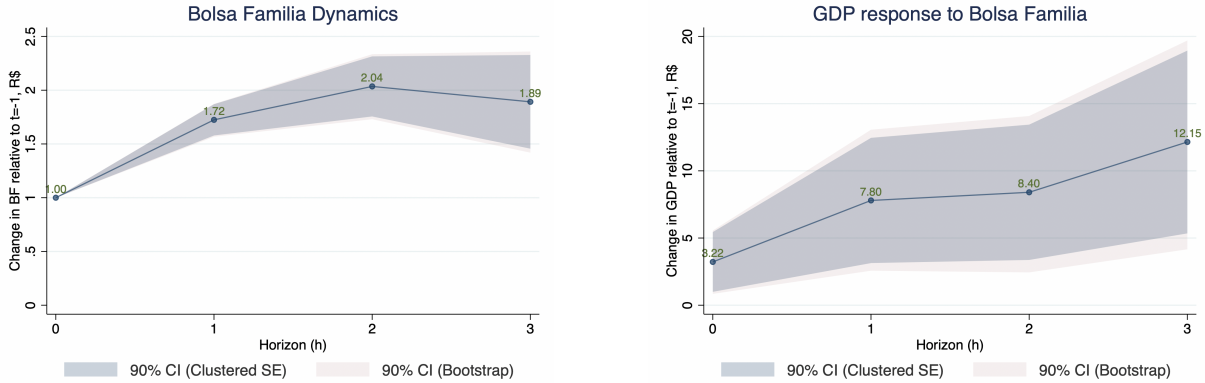


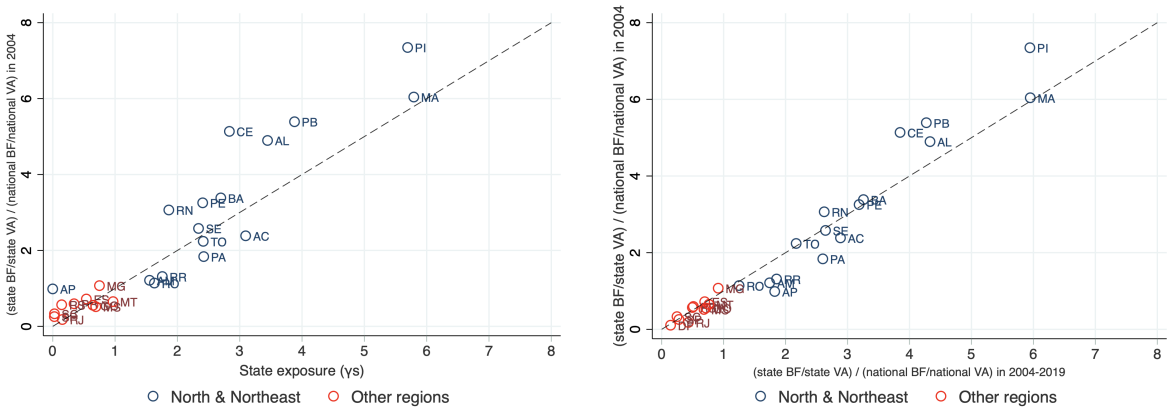
Figure D.6: **BF transfers and GDP dynamics**



Notes: This figure plots the impulse responses of BF transfers and GDP to a transfer shock happening in period 0. We obtain the estimate with the same benchmark specification for the LHS to be $\frac{y_{t+h}-y_{t-1}}{y_{t-1}}$ for $h = 0, ..., 3$.

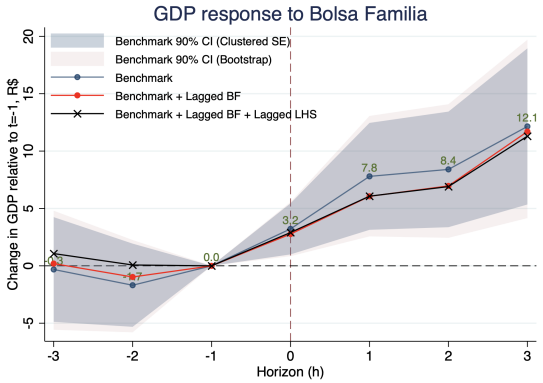
Figure D.7: **Alternative IVs**:
Estimated state exposure (γ_s) (X-axis) versus calibrated Bolsa Familia shares (Y-axis) in Panel A and 2004-2019 average shares (Y-axis) in Panel B

Panel A. Estimated shares **Panel B. 2004-2019 shares**



Notes: Correlation is 0.94 in Panel A and 0.96 in Panel B.

Figure D.8: **BF transfers and GDP dynamics**



Notes: This figure plots the impulse responses of GDP to a transfer shock with anticipation effects in horizons $-3, -2$.

Table D.2: **Sectoral decomposition of Gross Value Added**

	Agriculture	Mining/quarrying	Manufacturing	Construction	Services
National	7.8%	3.2%	10.2%	3.1%	75.7%
Region:					
Midwest	12.7%	0.6%	9.0%	3.3%	74.5%
North	8.5%	3.7%	7.8%	2.6%	77.4%
Northeast	7.1%	2.6%	7.9%	3.3%	79.1%
Southeast	8.1%	0.2%	19.3%	3.5%	68.8%
South	3.0%	8.8%	13.6%	2.8%	71.8%

Notes: Percent of VA, average 2003-2019.

Table D.3: **Summary Statistics for Brazil between 2004 and 2019**

	Output growth	BF transfer growth rate	BF, % of output	Formal empl growth rate	Informal emp growth rate	Poverty rate (% of pop)	Population growth rate
National	2.2	10.1	0.45	3.1	1.0	30.5	1.1
	(2.8)	(12.5)	(0.0)	(3.4)	(5.1)	(1.9)	(0.6)
North	3.1	14.3	1.0	4.2	2.9	44.3	1.6
	(3.6)	(14.9)	(0.3)	(4.6)	(6.6)	(1.4)	(0.2)
Northeast	2.4	9.7	1.6	3.5	1.2	49.2	0.8
	(2.9)	(11.2)	(0.2)	(3.9)	(5.0)	(3.7)	(0.2)
Southeast	2.0	10.8	0.1	2.8	0.7	21.6	0.8
	(3.0)	(16.0)	(0.0)	(3.5)	(5.5)	(1.2)	(0.1)
South	1.9	6.3	0.1	3.1	-0.0	17.1	0.8
	(3.2)	(15.5)	(0.0)	(2.9)	(6.1)	(1.5)	(0.1)
Midwest	3.2	10.8	0.2	3.4	1.3	21.8	1.6
	(2.5)	(21.4)	(0.0)	(3.4)	(7.7)	(3.2)	(0.2)

Notes: We report the average in the national and regional levels between 2004 and 2019. The numbers in parentheses are standard deviations. Following de Souza et al. (2019), a household is considered poor if its monthly per capita income, net of transfers, was less than half the national minimum wage.

Table D.4: **Descriptive Statistics: Informal Employment in 2010**

	A. The size of informality			**B. Decomposition of Informality**			
	Millions*	**% of employment**		(Type of informal emp. as % of total informality)			
		Total	**Formal**	**Employees**	**Employers**	**Self-employed**	**Unpaid**
National:	40.5	48%	92%	53%	1%	34%	12%
North	4.1	63%	168%	46%	1%	37%	16%
Northeast	13.5	63%	169%	51%	1%	30%	18%
Southeast	14.4	39%	64%	58%	1%	35%	6%
South	5.4	42%	71%	48%	1%	38%	13%
Midwest	3.2	47%	88%	57%	1%	34%	8%
Sectors:							
Agriculture	9.6	87%	654%	25%	0%	33%	41%
Mining and quarrying	0.1	27%	38%	64%	1%	33%	1%
Manufacturing	2.7	26%	35%	48%	2%	48%	3%
Construction	3.6	59%	142%	45%	1%	54%	1%
Services	22.3	41%	70%	68%	1%	29%	2%
Exc. agric/mining/quarrying	30.9	42%	73%	61%	1%	34%	3%
Distribution (state-year):							
p5	0.2	34%	52%	44%	1%	28%	4%
p25	0.5	47%	87%	48%	1%	30%	10%
p50	0.9	57%	133%	51%	1%	34%	15%
p75	2.1	60%	149%	56%	1%	36%	18%
p95	4.1	70%	229%	58%	2%	39%	22%

Notes: (*) includes all types of informality: employees, employers, self-employed and unpaid.

Table D.5: **Formal Employment Results: RAIS data vs. PNAD data.**

	(1)	(2)	(3)	(4)
	RAIS (Default)		**PNAD**	
Dependent variable:	**Parsimonious**	**Benchmark**	**Parsimonious**	**Benchmark**
GDP multiplier	-2.9	-1.3	-1.2	0.63
Default clustered SEs (state)	(0.88)***	(1.40)	(1.47)	(1.70)
Robust SEs	[0.85]***	[1.31]	[1.43]	[2.16]
Wild bootstrap 90% CI				
Clustered by states	[−4., −1.]	[−3., 1.2]	[−4., 1.4]	[−2., 3.7]
P-value	0.00	0.37	0.46	0.72
Clustered by states & years	[−4., −.2]	[−4., 2.1]	[−3., 2.5]	[−1., 2.4]
P-value	0.07	0.48	0.46	0.52
Observations	385	385	385	385
First stage F-statistics	99	98	136	127

Table D.6: **Spending Patterns of Poor Households in Brazil**

	Share of Household Expenditure on...		
	Housing	Manuf. and Durable Goods	Food, Health, Educ., and Services
Poor Dummy	-0.057***	-0.038***	0.095***
	(0.006)	(0.003)	(0.006)
Constant	0.370***	0.224***	0.406***
	(0.004)	(0.003)	(0.005)
Number of households	48,470	48,470	48,470

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Standard errors, clustered at the state- by-strata level (for which there are 443 clusters), are reported in parentheses. Data come from Brazil's Pesquisa de Orcamentos Familiares (POF) from 2002-03. Poor households are those among the bottom 25% of the national per capita household income distribution, whereas non-poor households are those among the top 75%.

Table D.7: **BF effects on the Labor Market by Sector**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Formal-wage Equiv.		**Total Employment**		**Formal Employment**		**Informal Employment**	
	Pars	Bench	Pars	Bench	Pars	Bench	Pars	Bench
Non-Tradable Sector	-0.04	0.83	2.62	4.02	-1.7	-1.7	4.76	5.62
Default clustered SEs (state)	(1.35)	(1.58)	(1.91)	(2.50)	(0.92)*	(1.23)	(1.54)***	(2.34)**
Robust SEs	[1.31]	[1.58]	[2.07]	[2.61]	[0.83]**	[1.24]	[1.92]**	[2.65]**
Wild bootstrap 90% CI								
Clustered by states	[−2.3, 2.3]	[−2., 3.6]	[−.7, 6.1]	[−.7, 8.6]	[−3., −.0]	[−3., .62]	[1.9, 7.]	[.95, 10]
P-value	0.97	0.63	0.21	0.15	0.08	0.20	0.01	0.05
Clustered by states & years	[−2.3, 2.4]	[−1., 3.4]	[−.1, 5.1]	[.50, 7.1]	[−3., .36]	[−4., 1.4]	[1.2, 8.9]	[−.7, 12.]
P-value	0.98	0.56	0.11	0.06	0.14	0.31	0.04	0.17
Tradable Sector	-0.69	-0.2	-0.3	-0.3	-1.1	0.37	-0.0	-1.4
Default clustered SEs (state)	(0.56)	(0.79)	(0.93)	(1.84)	(0.40)***	(0.42)	(0.92)	(2.26)
Robust SEs	[0.91]	[1.06]	[1.72]	[2.13]	[0.34]***	[0.39]	[1.7]	[2.25]
Wild bootstrap 90% CI								
Clustered by states	[−1.8, .42]	[−1., 1.2]	[−2., 1.5]	[−3., 3.2]	[−1., −.4]	[−.3, 1.1]	[−1., 1.]	[−6., 3.]
P-value	0.26	0.76	0.77	0.87	0.01	0.40	0.98	0.55
Clustered by states & years	[−2.4, .96]	[−3., 2.5]	[−3., 2.8]	[−6., 5.5]	[−2., .47]	[−.8, 1.5]	[−3., 4.1]	[−6., 4.5]
P-value	0.49	0.87	0.87	0.95	0.25	0.58	0.98	0.64

Notes: All estimates are obtained using 2SLS estimation. The table reports the percent change in employment in states that receive more BF transfers by 1% of their GDP relative to other states.

Table D.8: **BF effects on Inflation**

	(1)	(2)	(3)	(4)	(5)
	GDP Deflator				
Dependent variable:	**Total**	**Services**	**Construction**	**Agriculture**	**Municipal CPI (11 state capitals)**
Coefficient	5.32	2.38	2.16	23.2	3.04*
Default clustered SEs (state)	(1.90)***	(1.83)	(5.79)	(9.42)**	(1.46)**
Robust SEs	[2.58]**	[1.94]	[7.55]	[7.05]***	[1.59]*
Wild bootstrap 90% CI					
Clustered by states	[1.8, 8.9]	[−1., 5.8]	[−8., 13.]	[6.2, 40.]	[−.1, 6.4]
P-value	0.02	0.25	0.69	0.03	0.11
Clustered by states & years	[1.1, 9.1]	[−.9, 5.9]	[−4., 7.6]	[2.4, 45.]	[−.0, 7.8]
P-value	0.05	0.22	0.46	0.06	0.10
Observations	385	385	385	385	155

Table D.9: **BF effects on Wages and Earnings**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	**Formal Wage Growth***		**Informal Wages Growth***		**Total Earnings****		**Formal Earnings****		**Informal Earnings****	
	Pars	**Bench**	**Pars**	**Bench**	**Pars**	**Bench**	**Pars**	**Bench**	**Pars**	**Bench**
Coefficient	3.37	3.95	-1.84	-1.70	2.17	1.33	1.29	0.98	1.01	0.60
Default clustered SEs (state)	(0.97)***	(1.52)***	(2.37)	(3.13)	(0.66)***	(0.87)	(0.53)**	(0.70)	(0.40)**	(0.52)
Robust SEs	[0.95]***	[1.45]***	[2.68]	[3.50]	[0.54]***	[0.86]	[0.47]***	[0.70]	[0.40]**	[0.55]
Wild bootstrap 90% CI										
Clustered by states	[1.83, 5.16]	[1.14, 6.66]	[-6.09, 2.33]	[-7.38, 3.71]	[0.95, 3.39]	[-0.30, 2.99]	[0.33, 2.21]	[-0.29, 2.27]	[0.29, 1.72]	[-0.37, 1.55]
P-value	0.00	0.02	0.46	0.62	0.00	0.17	0.03	0.21	0.02	0.30
Clustered by states & years	[1.23, 6.17]	[-0.32, 8.30]	[-5.96, 1.98]	[-7.11, 3.48]	[0.53, 3.75]	[0.01, 3.15]	[-0.14, 2.39]	[-0.96, 2.85]	[0.24, 1.89]	[-0.57, 1.59]
P-value	0.01	0.14	0.39	0.50	0.07	0.10	0.13	0.39	0.03	0.32
Observations	385	385	385	385	385	385	385	385	385	385
First stage F-statistics	112	65	119	85	125	111	114	85	93	59

Table D.10: **Output per capita and Population**

Dependent variable:	(1)	(2)	(3)	(4)
	Output Per Capita		**Population**	
	Parsimonious	**Benchmark**	**Parsimonious**	**Benchmark**
Estimated effects	1.16	2.96	0.53	0.55
Default clustered SEs (state)	(1.06)	(1.35)**	(0.13)***	(0.13)***
Robust SEs	(1.1)	(1.45)**	(0.1)***	(0.13)***
Wild bootstrap 90% CI				
Clustered by states	[-.7, 3.0]	[.45, 5.4]	[.31, .75]	[.32, .78]
P-value	0.30	0.04	0	0
Clustered by states & years	[-.9, 3.6]	[1.5, 4.4]	[.25, .83]	[.28, .81]
P-value	0.36	0.03	0.00	0.00
Observations	385	385	385	385
First-stage F-statistics	157	116	156	103

Notes: The first two columns report the impact multiplier from a regression of state-level output growth per capita on the change in per capita Bolsa Familia transfers as a share of per capita output. The last two columns are similar except the dependent variable is the population growth rate, and the independent variable is not in per capita terms. All columns are 2SLS specifications with a shift-share (Bartik) instrument. See notes to Table 1 for additional details.

Table D.11: **Robustness: Relative Multipliers controlling for Spillovers**

	(1)	(2)	(3)	(4)
	A. Baseline		**B. Control for Spillovers**	
Dependent variable:	**Parsimonious**	**Benchmark**	**Parsimonious**	**Benchmark**
Output multiplier	2.00	3.22	1.11	2.62
Default clustered SEs (state)	(0.94)**	(1.35)**	(1.24)	(1.04)**
Robust SEs	[1.09]*	[1.37]**	[1.27]	[1.47]*
Wild bootstrap 90% CI				
Clustered by states	[.20, 3.6]	[.84, 5.5]	[−1., 3.4]	[.70, 4.5]
P-value	0.06	0.02	0.42	0.03
Clustered by states & years	[.43, 3.8]	[1.1, 5.0]	[−1., 4.0]	[2.4, 2.7]
P-value	0.03	0.02	0.41	0
Spillover effect			0.06	0.11
Default clustered SEs (state)			(0.07)	(0.06)*
Robust SEs			[0.1]	[0.11]
Wild bootstrap 90% CI				
Clustered by states			[-.1, .24]	[-.0, .27]
P-value			0.56	0.19
Clustered by states & years			[-.1, .24]	[-.1, .39]
P-value			0.53	0.38
Observations	385	385	385	385
First stage F-statistics	153	116	100	77

Table D.12: **Robustness: Timing of BF changes**

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline		**Contemp + Lag**		**Two-year changes**	
	Pars	**Bench**	**Pars**	**Bench**	**Pars**	**Bench**
GDP multiplier	2.00	3.22	3.08	4.34	2.34	3.69
Default clustered SEs (state)	(0.94)**	(1.35)**	(0.93)***	(1.42)***	(1.00)**	(1.13)***
Robust SEs	[1.09]*	[1.37]**	[1.27]*	[1.37]***	[0.88]***	[1.05]***
Wild bootstrap 90% CI						
Clustered by states	[.20, 3.6]	[.84, 5.5]	[1.1, 4.8]	[1.7, 6.9]	[.58, 4.]	[1.6, 5.]
P-value	0.06	0.02	0.01	0.01	0.03	0.00
Clustered by states & years	[.43, 3.8]	[1.1, 5.0]	[2.1, 4.3]	[2.2, 6.0]	[.50, 4.3]	[1.5, 6.1]
P-value	0.03	0.02	0.00	0.01	0.03	0.00
Formal employment	-2.9	-1.3	-2.82	-1.29	-4.0	-2.8
Default clustered SEs (state)	(0.88)***	(1.40)	(0.82)***	(1.41)	(0.64)***	(0.62)***
Robust SEs	[0.85]***	[1.31]	[0.88]***	[1.36]	[0.56]***	[0.78]***
Wild bootstrap 90% CI	0	0	0	0	0	0
Clustered by states	$[-4.3, -1.3]$	$[-3., 1.2]$	$[-4., -1.]$	$[-3., 1.3]$	$[-5., -2]$	$[-3., -1]$
P-value	0.00	0.37	0.00	0.41	0	0
Clustered by states & years	$[-4., -.2]$	$[-4., 2.1]$	$[-4., -.3]$	$[-4., 2.2]$	$[-5., -2.]$	$[-4., -1.]$
P-value	0.07	0.48	0.06	0.47	0	0.00
Informal employment	4.73	4.16	6.67	6.23	5.69	5.99
Default clustered SEs (state)	(1.58)***	(1.96)**	(1.65)***	(2.41)**	(1.36)***	(2.01)***
Robust SEs	[2.25]**	[2.66]	[2.58]**	[2.95]**	[1.54]***	[1.85]***
Wild bootstrap 90% CI						
Clustered by states	[1.7, 7.]	[.41, 7.]	[3.6, 9.7]	[1.4, 10.]	[3.0, 8.]	[2.0, 9.]
P-value	0.01	0.07	0.00	0.04	0	0.01
Clustered by states & years	[.84, 9.3]	$[-.9, 8.3]$	[2.3, 10.]	$[-.6, 12.]$	[2.3, 8.6]	[1.9, 9.8]
P-value	0.05	0.15	0.04	0.11	0.01	0.04
FWE employment	-0.73	0.57	-0.7	0.67	-1.2	-0.2
Default clustered SEs (state)	(1.20)	(1.52)	(1.34)	(1.41)	(1.28)	(1.10)
Robust SEs	[1.33]	[1.59]	[1.57]	[1.65]	[0.98]	[1.15]
Wild bootstrap 90% CI	0	0	0	0	0	0
Clustered by states	$[-2.8, 1.3]$	$[-2., 3.2]$	$[-3., 1.5]$	$[-1., 3.0]$	$[-3., 1.]$	$[-2., 1.]$
P-value	0.54	0.71	0.58	0.66	0.40	0.87
Clustered by states & years	$[-3.4, 2.0]$	$[-2., 3.6]$	$[-3., 2.7]$	$[-1., 3.1]$	$[-3., 1.5]$	$[-2., 1.8]$
P-value	0.59	0.73	0.71	0.60	0.38	0.88
Total employment	1.91	3.17	1.68	3.18	1.40	1.62
Default clustered SEs (state)	(2.05)	(2.70)	(1.65)	(2.41)	(1.68)	(2.18)
Robust SEs	[2.29]	[2.87]	[2.58]	[2.95]	[1.57]	[1.86]
Wild bootstrap 90% CI						
Clustered by states	$[-1., 5.4]$	$[-1., 7.8]$	$[-2., 5.6]$	$[-1., 8.0]$	$[-1., 4.]$	$[-2., 5.]$
P-value	0.35	0.26	0.46	0.26	0.45	0.50
Clustered by states & years	$[-2., 6.3]$	$[-2., 8.5]$	$[-1., 5.3]$	$[-1., 7.4]$	$[-1., 4.2]$	$[-2., 5.8]$
P-value	0.45	0.33	0.43	0.21	0.38	0.46

Notes: Columns (1) to (2) display the baseline parsimonious and benchmark 2SLS estimation. Columns (3) and (4) display the estimate for the multiplier when the specification includes a lag of BF transfers – the numbers reported are the sum (point estimate and standard errors) for the sum of the coefficient on the contemporaneous and lag BF transfers. Columns (5) and (6) present the estimate when we use two-year changes of BF as dependent variables. The table shows the results for output, formal, informal, and FWE employment.

Table D.13: **Robustness: Different deflators for BF transfers**

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Default (National CPI)		**State GDP deflator**		**National GDP deflator**	
	Parsimonious	**Benchmark**	**Parsimonious**	**Benchmark**	**Parsimonious**	**Benchmark**
GDP multiplier	2.00	3.22	3.23	4.43	3.51	3.25
Default clustered SEs (state)	(0.94)**	(1.35)**	(0.97)***	(1.17)***	(1.12)***	(0.96)***
Robust SEs	[1.09]*	[1.37]**	[0.97]***	[1.49]***	[1.1]***	[1.39]**
Wild bootstrap 90% CI						
Clustered by states	[.20, 3.6]	[.84, 5.5]	[1.4, 5.0]	[2.1, 6.5]	[1.5, 5.5]	[1.4, 4.9]
P-value	0.06	0.02	0.00	0.00	0.00	0.00
Clustered by states & years	[.43, 3.8]	[1.1, 5.0]	[.61, 5.2]	[1.8, 6.9]	[.83, 5.8]	[1.1, 5.4]
P-value	0.03	0.02	0.04	0.01	0.02	0.02
Observations	385	385	385	385	385	385
First stage F-statistics	153	116	139	130	162	96

Notes: Columns (1) and (2) display the baseline parsimonious and benchmark 2SLS estimation where nominal BF transfers are deflated by national CPI. Columns (3) and (4) display the estimate for the multipliers when BF transfers are deflated by state GDP deflator. Then, Columns (5) and (6) display the estimate for the multipliers when BF transfers are deflated by national GDP deflator.

Table D.14: **Multipliers dropping years, states, and regions one-by-one (2SLS).**

	(1)	(2)	(3)		(4)	(5)	(6)		(7)	(8)	(9)
	A. Dropping years one-by-one				**B. Dropping states one-by-one**				**C. Dropping regions one-by-one**		
	GDP Multiplier	Formal emp	Informal emp		GDP Multiplier	Formal emp	Informal emp		GDP Multiplier	Formal emp	Informal emp
Benchmark	3.22**	-1.37	4.16**	Benchmark	3.22**	-1.37	4.16**	Benchmark	3.22**	-1.37	4.16**
	(1.35)	(1.40)	(1.96)		(1.35)	(1.40)	(1.96)		(1.35)	(1.40)	(1.96)
Drop:				**Drop:**				**Drop:**			
2005	3.04**	-1.67	3.65*	RO	3.08**	-1.25	3.73*	Northeast	4.52	-2.09	6.09*
	(1.24)	(1.26)	(1.92)		(1.37)	(1.39)	(1.95)		(3.84)	(1.68)	(3.34)
2006	3.38***	-1.72	3.60*	AC	3.06**	-1.31	4.17**	North	3.60***	-1.39	4.89**
	(1.30)	(1.48)	(2.00)		(1.32)	(1.44)	(1.96)		(1.29)	(1.47)	(1.92)
2007	3.39**	-1.87	4.33**	AM	2.98**	-1.49	4.57**	South	2.16	-1.75	4.56*
	(1.42)	(1.36)	(2.00)		(1.37)	(1.41)	(1.94)		(1.60)	(1.52)	(2.34)
2008	3.53***	-1.30	4.19**	PR	2.87**	-1.36	4.44**	Southeast	0.71	-2.20	2.76
	(1.34)	(1.48)	(2.05)		(1.41)	(1.44)	(2.03)		(1.31)	(1.49)	(2.53)
2009	3.14**	-0.84	5.07**	PA	3.22**	-1.39	4.15**	Midwest	4.34***	-1.17	2.80
	(1.25)	(1.25)	(2.18)		(1.39)	(1.41)	(2.00)		(1.42)	(1.38)	(1.99)
2010	2.68	-1.05	3.97*	AP	3.79***	-1.30	4.72**	Northeast	4.01	-2.62	4.54
	(1.88)	(1.76)	(2.15)		(1.28)	(1.43)	(1.98)	and Southeast	(2.79)	(1.76)	(3.48)
2011	3.16**	-1.19	3.60*	TO	3.21**	-1.35	4.24**				
	(1.43)	(1.36)	(2.10)		(1.34)	(1.41)	(1.95)				
2012	3.76***	-0.29	2.64	MA	3.56**	-0.12	5.09**				
	(1.30)	(1.95)	(2.32)		(1.54)	(1.38)	(2.16)				
2013	3.30*	-1.70	5.95***	PI	2.91**	-0.86	3.42*				
	(1.76)	(1.55)	(1.98)		(1.46)	(1.50)	(1.97)				
2014	3.04**	-1.51	4.49**	CE	3.56***	-1.34	4.33**				
	(1.35)	(1.40)	(1.95)		(1.32)	(1.51)	(2.03)				
2015	3.71**	-1.16	5.42***	RN	3.25**	-1.35	4.36**				
	(1.54)	(1.58)	(2.08)		(1.32)	(1.41)	(1.92)				
2016	3.03**	-1.37	4.12**	RO	3.08**	-1.25	3.73*				
	(1.34)	(1.40)	(1.95)		(1.37)	(1.39)	(1.95)				
2017	2.93**	-1.41	2.89	PE	3.05**	-1.59	3.96**				
	(1.32)	(1.44)	(2.12)		(1.30)	(1.37)	(1.96)				
2018	2.72**	-0.68	3.35	AL	3.43**	-1.81	4.77**				
	(1.35)	(1.31)	(2.56)		(1.37)	(1.39)	(2.05)				
2019	3.49**	-2.46*	5.89***	SE	3.19**	-1.60	4.29**				
	(1.41)	(1.31)	(2.21)		(1.34)	(1.35)	(1.95)				
2012 & 2015	3.22**	-1.37	4.16**	BA	3.39**	-1.41	3.60*				
	(1.35)	(1.40)	(1.96)		(1.34)	(1.43)	(1.85)				
				MG	3.12**	-1.52	4.33**				
					(1.40)	(1.40)	(2.00)				
				ES	2.74**	-1.19	3.99*				
					(1.35)	(1.44)	(2.05)				
				RJ	2.97**	-1.44	3.46*				
					(1.49)	(1.49)	(2.04)				
				SP	3.31**	-1.73	4.24*				
					(1.57)	(1.41)	(2.16)				
				PR	2.87**	-1.36	4.44**				
					(1.41)	(1.44)	(2.03)				
				SC	2.89**	-1.42	4.13**				
					(1.44)	(1.46)	(2.09)				
				RS	3.05**	-1.64	4.23**				
					(1.43)	(1.40)	(2.08)				
				MS	3.34**	-1.54	4.53**				
					(1.42)	(1.42)	(2.01)				
				MT	3.32**	-1.64	3.32*				
					(1.39)	(1.39)	(1.84)				
				GO	3.14**	-1.37	3.45*				
					(1.41)	(1.45)	(1.91)				
				DF	4.02***	-0.74	4.32**				
					(1.21)	(1.36)	(2.14)				

Notes: We report the default clustered standard errors. The estimates are from the 2SLS specification.

Table D.15: **Robustness to different winsor levels/outliers**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Default	**Robustness 1**	**Robustness 2**	**Robustness 3**	**Robustness 4**	**Robustness 5**	**Robustness 6**
Winsor level:	Winsor by sectors	No winsor	Default winsor	No winsor	No winsor	Default winsor	5% winsor all sectors
Influential Obs Dropped:	Outliers from pars (5%)	Full sample	Full sample	Outliers from pars (5%)	Outliers from bench (5%)	Outliers from bench	Outliers from pars (5%)
GDP multiplier (Benchmark)	3.22**	3.81**	5.28***	3.06	2.90*	3.42***	4.07**
Default clustered SE (state, bench)	(1.35)	(1.70)	(1.27)	(1.92)	(1.73)	(1.24)	(2.07)
GDP multiplier (Parsimonious)	2.00**	1.34	2.66***	0.32	1.42	1.56	1.34
Default clustered SE (state, pars)	(0.94)	(1.38)	(1.02)	(1.68)	(1.76)	(1.28)	(1.45)
Observations	385	405	405	385	385	385	385

Notes: We report the default standard errors clustered by state. The estimates are from the 2SLS specification. The first stage is always very relevant (first-stage F-statistics around 80 or higher, not reported). In all regressions, the influential observations—if dropped—are recalculated based on specification in that column.

Table D.16: Robustness: Formal Employment

	(1)	(2)	(3)	(4)	(5)	(6)
			Benchmark + Controls			
Dependent variable:	**Parsimonious**	**Benchmark**	**Commodity price**	**Minimum wage**	**Other federal transfers**	**Tax revenues**
Formal employment	-2.9	-1.3	-0.5	-1.8	-1.3	-1.16
Default clustered SEs (state)	(0.88)***	(1.40)	(1.31)	(1.47)	(1.41)	(1.33)
Robust SEs	[0.85]***	[1.31]	[1.32]	[1.49]	[1.32]	[1.32]
Wild bootstrap 90% CI						
Clustered by states	[-4.3, -1.3]	[-3., 1.2]	[-2., 1.8]	[-4., .90]	[-3., 1.2]	[-3., 1.3]
P-value	0.00	0.37	0.73	0.26	0.37	0.42
Clustered by states & years	[-4., -.2]	[-4., 2.1]	[-3., 3.3]	[-4., 1.3]	[-4., 2.0]	[-4., 2.2]
P-value	0.07	0.48	0.81	0.25	0.46	0.53
Observations	385	385	385	385	385	385
First stage F-statistics	99.3	98.9	109	90	97	100

	(7)	(8)	(9)	(10)	(11)	(12)	(13)
			Benchmark + Controls				
Dependent variable:	**Lagged LHS**	**Lagged state change in BF**	**(7) + (8)**	**Ramey 2021 Lags**	**Population**	**Poverty**	**ALL**
Formal employment	-1.70	-3.2	-3.6	-1.8	-0.07	-1.78	-0.74
Default clustered SEs (state)	(1.37)	(1.20)***	(1.23)***	(1.65)	(1.43)	(1.48)	(1.94)
Robust SEs	[1.33]	[1.56]**	[1.58]**	[1.72]	[1.26]	[1.39]	[2.08]
Wild bootstrap 90% CI							
Clustered by states	[-4., .9]	[-5., -1.]	[-5., -1.]	[-4., 1.1]	[-2., 2.7]	[-4., 1.0]	[-4., 2.9]
P-value	0.26	0.01	0.01	0.32	0.96	0.28	0.74
Clustered by states & years	[-4., 1.8]	[-6., .06]	[-7., -.0]	[-5., 2.1]	[-3., 3.8]	[-4., 2]	[-3., 2.6]
P-value	0.39	0.10	0.09	0.36	0.97	0.37	0.66
Observations	385	385	385	385	385	385	385
First stage F-statistics	94	73	70	121	108	97	138

Notes: The first two columns (1) and (2) are the parsimonious and benchmark specification estimated using 2SLS, without additional controls. All of the rest of the columns are estimates that use benchmark specification with added controls. Column (3) displays the estimate when we control for contemporaneous commodity price index interacted with state dummies. Column (4) reports the estimate with additional controls for minimum wage. Columns (5) and (6) additionally control for other federal transfers and state tax revenue, respectively. Column (7) displays the estimates when one lag of the LHS is added, column (8) adds a lag of the BF transfer as a control, then column (9) presents the estimate when we add a lag of both the BF transfer and the state value added growth rate. Column (10) includes two lags of the dependent variable, the instrument and state BF transfers as suggested in Ramey (2021). Column (11) reports the estimates when we add log state population (and first lag) as a control. Column (12) controls for state poverty level. Finally, column (13) reports the results when we include all controls simultaneously.

Table D.17: Robustness: Informal Employment

	(1)	(2)	(3)	(4)	(5)	(6)
				Benchmark + Controls		
Dependent variable:	**Parsimonious**	**Benchmark**	**Commodity price**	**Minimum wage**	**Other federal transfers**	**Tax revenues**
Informal employment	4.73	4.16	3.63	3.77	4.23	4.41
Default clustered SEs (state)	(1.58)***	(1.96)**	(2.70)	(3.26)	(1.98)**	(1.98)**
Robust SEs	[2.25]**	[2.66]	[2.9]	[3.43]	[2.68]	[2.67]*
Wild bootstrap 90% CI		0				
Clustered by states	[1.7, 7.]	[.41, 7.]	[-2., 1.8]	[-4., .90]	[-3., 1.2]	[-3., 1.3]
P-value	0.01	0.07	0.73	0.26	0.37	0.42
Clustered by states & years	[.84, 9.3]	[-.9, 8.3]	[-3., 3.3]	[-4., 1.3]	[-4., 2.0]	[-4., 2.2]
P-value	0.05	0.15	0.81	0.25	0.46	0.53
Observations	385	385	385	385	385	385
First stage F-statistics	126	67.5	53	47	66	69

	(7)	(8)	(9)	(10)	(11)	(12)	(13)
				Benchmark + Controls			
Dependent variable:	**Lagged LHS**	**Lagged state change in BF**	**(7) + (8)**	**Ramey 2021 Lags**	**Population**	**Poverty**	**ALL**
Informal employment	5.68	4.08	5.88	3.37	3.68*	3.64*	6.78*
Default clustered SEs (state)	(2.00)***	(3.10)	(3.06)*	(2.78)	(2.04)*	(1.84)**	(3.53)*
Robust SEs	[2.58]**	[3.38]	[3.33]*	[4.15]	[2.75]	[2.77]	[4.53]
Wild bootstrap 90% CI							
Clustered by states	[-4., .9]	[-1., 10.]	[.05, 11.]	[-4., 1.1]	[-3., 2.0]	[-3., 1.2]	[-5., 2.0]
P-value	0.26	0.25	0.09	0.32	0.70	0.37	0.42
Clustered by states & years	[-4., 1.8]	[-4., 10.]	[-1., 13.]	[-5., 2.1]	[-3., 2.8]	[-4., 2.2]	[-5., 1.9]
P-value	0.39	0.31	0.17	0.36	0.74	0.47	0.33
Observations	385	385	385	385	385	385	385
First stage F-statistics	71	84	89	62	71	62	91

Notes: The first two columns (1) and (2) are the parsimonious and benchmark specification estimated using 2SLS, without additional controls. All of the rest of the columns are estimates that use benchmark specification with added controls. Column (3) displays the estimate when we control for contemporaneous commodity price index interacted with state dummies. Column (4) reports the estimate with additional controls for minimum wage. Columns (5) and (6) additionally control for other federal transfers and state tax revenue, respectively. Column (7) displays the estimates when one lag of the LHS is added, column (8) adds a lag of the BF transfer as a control, then column (9) presents the estimate when we add a lag of both the BF transfer and the state value added growth rate. Column (10) includes two lags of the dependent variable, the instrument and state BF transfers as suggested in Ramey (2021). Column (11) reports the estimates when we add log state population (and first lag) as a control. Column (12) controls for state poverty level. Finally, column (13) reports the results when we include all controls simultaneously.

Table D.18: **Employment Robustness: Growth Specification (scaled by its own lag).**

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Formal		**Informal**		**Total**	
	Pars	**Bench**	**Pars**	**Bench**	**Pars**	**Bench**
Changes in employment	0.98	0.19	12.6	12.5	0.86	2.41
Default clustered SEs (state)	(1.02)	(1.86)	(2.59)***	(3.22)***	(1.41)	(1.79)
Robust SEs	[1.36]	[1.96]	[3.38]***	[3.82]***	[1.68]	[2.01]
Wild bootstrap 90% CI						
Clustered by states	[-0.79, 2.78]	[-3., 3.6]	[7.9, 17]	[6.8, 18]	[-1., 3.4]	[-7., 5.5]
P-value	0.34	0.92	0	0	0.58	0.20
Clustered by states & years	[-2., 4.0]	[-4., 4.6]	[6.9, 19.]	[5.7, 19.]	[-1., 3.8]	[-1., 5.9]
P-value	0.55	0.94	0.00	0.01	0.60	0.25
Observations	385	385	385	385	385	385
First stage F-statistics	107.	82.0	154	97.9	135	97.5

Notes: This table presents similar results to Table 3, but where we scale the change in employment on the LHS by its own lag to form a growth rate rather than by FWE as in Table 3. Note in Table 3, the change in FWE employment is already scaled by its own lag, and so omitted from this table. Specifically, the LHS in each specification is the percent change of formal employment (columns (1) and (2)), informal employment (columns (3) and (4)), and total employment (columns (5) and (6)). In all specifications, the RHS is changes in BF as a share of lagged GDP.

Intensive and extensive margins

Changes in BF transfers to a state's residents can be either at the intensive or extensive margin. Intensive margin changes involve increasing the size of benefits to existing BF households, whereas extensive-margin changes involve increasing the number of households in the program at a constant benefit rate. In terms of the decomposition, extensive margin changes are a combination of changes in quotas and coverage, whereas the intensive margin changes reflect changes in the average benefits. To investigate whether our results are driven by intensive or extensive margin changes, we disaggregate the right-hand side variable of equation 1 into extensive versus intensive margin changes, where extensive margin changes are a combination of changes in quotas and coverage (and intensive margin are changes in average benefits). Results, in Table D.19, suggest that multipliers on the extensive margin are not significantly different from zero, whereas intensive margin changes are responsible for the large relative output multipliers.

Table D.19: **Decomposing the multiplier: Intensive and Extensive Margins**

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	A. Output growth		**B. Extensive Margin**		**C. Intensive Margin**	
	Parsimonious	Benchmark	Parsimonious	Benchmark	Parsimonious	Benchmark
GDP multiplier	2.00	3.22	0.67	2.36	3.67*	3.96
Default clustered SEs (state)	(0.94)**	(1.35)**	(1.23)	(1.23)*	(1.12)***	(1.02)***
Robust SEs	[1.09]*	[1.37]**	[1.29]	[1.37]*	[1.48]**	[1.43]***
Wild bootstrap 90% CI						
Clustered by states	[.20, 3.6]	[.84, 5.5]	[−1., 2.]	[.11, 5.]	[1.72, 5.6]	[2.1, 5.8]
P-value	0.06	0.02	0.59	0.07	0.00	0.00
Clustered by states & years	[.43, 3.8]	[1.1, 5.0]	[−0.7, 4.1]	[−0.6, 7.8]	[1.53, 6.3]	[2.9, 5.1]
P-value	0.03	0.02	0.41	0.13	0.01	0.00
Observations	385	385	385	385	385	385
First stage F-statistics	153	116	522	358	620	711

E Model Details

We describe the model details in this section.

E.1 Setup

The economy is a monetary union consisting of a small home region, size n, representing a state in Brazil whose residents receive a net BF transfer, and a large foreign region with the rest of the population, size $1-n$, which pays for the BF transfer via lump-sum taxes.

A share of households ω are hand-to-mouth, i.e. they consume all their income each period, and the remaining fraction $1-\omega$ are Ricardian and can trade non-state contingent one-period bonds.

Variables with hats denote (log) deviations from their steady-state values, and the subscript ss denotes the steady-state values .

E.1.1 Ricardian Households

Ricardian households in the home region solve the following utility maximization problem.

$$E_0 \sum_{t=0}^{\infty} \beta^t U\left(C_t^R, L_t^R\right) \tag{9}$$

subject to

$$P_t C_t^R + \frac{B_{t+1}}{R_t^*} = B_t + W_t^L L_t^R + NTR_t^R + \Pi_t^R, \tag{10}$$

where C_t and L_t denote consumption and labor, respectively, the subscript R denotes Ricardian households, B_t is the non-state contingent one-period bond, R_t^* is the gross nominal interest rate on the one-period bond, P_t is the consumption price index (CPI), W_t is the nominal wage, NTR_t denotes the nominal transfer, and Π_t denotes nominal profit from firms.

Consumption for Ricardian households is an aggregator of tradable and non-tradable goods, as follows:

$$C_t^R = \left[\alpha_C^{\frac{1}{\theta_C}} \left(C_t^{R,T}\right)^{\frac{\theta_C-1}{\theta_C}} + (1-\alpha_C)^{\frac{1}{\theta_C}} \left(C_t^{R,S}\right)^{\frac{\theta_C-1}{\theta_C}}\right]^{\frac{\theta_C}{\theta_C-1}}, \tag{11}$$

where $C_t^{R,T}, C_t^{R,S}$ are tradable and services (non-tradable) consumption for Ricardian households, respectively, α_C is the share of tradable goods in final consumption and θ_C is the elasticity of substitution between tradable and non-tradable goods. For generality, we assume that tradable and services are aggregates of home h and foreign f (rest of the country) goods with the elasticities

θ_T and θ_S, respectively, as follows:

$$C_t^{R,T} = \left[\alpha_T^{\frac{1}{\theta_T}} \left(C_{h,t}^{R,T} \right)^{\frac{\theta_T - 1}{\theta_T}} + (1 - \alpha_T)^{\frac{1}{\theta_T}} \left(C_{f,t}^{R,T} \right)^{\frac{\theta_T - 1}{\theta_T}} \right]^{\frac{\theta_T}{\theta_T - 1}} \tag{12}$$

$$C_t^{R,S} = \left[\alpha_S^{\frac{1}{\theta_S}} \left(C_{h,t}^{R,S} \right)^{\frac{\theta_S - 1}{\theta_S}} + (1 - \alpha_S)^{\frac{1}{\theta_S}} \left(C_{f,t}^{R,S} \right)^{\frac{\theta_S - 1}{\theta_S}} \right]^{\frac{\theta_S}{\theta_S - 1}}. \tag{13}$$

Note that we can shut down cross-regional trade for the service/nontradable sector by setting $\alpha_S = 1$. The cost minimization problem leads to the following price indices:

$$P_t = \left[\alpha_C \left(P_t^T \right)^{1-\theta_C} + (1 - \alpha_C) \left(P_t^S \right)^{1-\theta_C} \right]^{\frac{1}{1-\theta_C}} \tag{14}$$

$$P_t^T = \left[\alpha_T \left(P_{h,t}^T \right)^{1-\theta_T} + (1 - \alpha_T) \left(P_{f,t}^T \right)^{1-\theta_T} \right]^{\frac{1}{1-\theta_T}} \tag{15}$$

$$P_t^S = \left[\alpha_S \left(P_{h,t}^S \right)^{1-\theta_S} + (1 - \alpha_S) \left(P_{f,t}^S \right)^{1-\theta_S} \right]^{\frac{1}{1-\theta_S}}, \tag{16}$$

where P_t is the price index for aggregate consumption as above, P_t^T and P_t^S are the price indices for consumption of tradable goods and nontradable goods, respectively, and $P_{h,t}^T$ is the price index for tradable goods produced in home region Y_t^T. The law of one price implies $P_{h,t}^T = P_{h,t}^{T*}$. In addition, $P_{f,t}^T$ is the price index for tradable goods produced in foreign region Y_t^{T*}, and the law of one price implies that $P_{f,t}^T = P_{f,t}^{T*}$. The problem for Ricardian households in the foreign region is analogous, and we add a star to the variables to denote foreign variables.

E.1.2 Hand to mouth Households

Hand-to-mouth households in the home region solve the following utility maximization problem:

$$E_0 \sum_{t=0}^{\infty} \beta^t U \left(C_t^N, L_t^N \right) \tag{17}$$

subject to

$$P_t C_t^N = NTR_t^N + W_t^L L_t^N + \Pi_t^N, \tag{18}$$

where subscript N denotes hand-to-mouth/non-Ricardian households' variables. Similar to the Ricardian households, non-Ricardian household consumption is a combination of tradable and

non-tradable goods,

$$C_t^N = \left[\alpha_C^{\frac{1}{\theta_C}} \left(C_t^{N,T} \right)^{\frac{\theta_C - 1}{\theta_C}} + (1 - \alpha_C)^{\frac{1}{\theta_C}} \left(C_t^{N,S} \right)^{\frac{\theta_C - 1}{\theta_C}} \right]^{\frac{\theta_C}{\theta_C - 1}} \tag{19}$$

$$C_t^{N,T} = \left[\alpha_T^{\frac{1}{\theta_T}} \left(C_{h,t}^{N,T} \right)^{\frac{\theta_T - 1}{\theta_T}} + (1 - \alpha_T)^{\frac{1}{\theta_T}} \left(C_{f,t}^{N,T} \right)^{\frac{\theta_T - 1}{\theta_T}} \right]^{\frac{\theta_T}{\theta_T - 1}} \tag{20}$$

$$C_t^{N,S} = \left[\alpha_S^{\frac{1}{\theta_S}} \left(C_{h,t}^{N,S} \right)^{\frac{\theta_S - 1}{\theta_S}} + (1 - \alpha_S)^{\frac{1}{\theta_S}} \left(C_{f,t}^{N,S} \right)^{\frac{\theta_S - 1}{\theta_S}} \right]^{\frac{\theta_S}{\theta_S - 1}}. \tag{21}$$

E.1.3 Final Goods Firms

A final goods firm in each sector aggregates intermediate goods, as follows:

$$Y_t^T = \left[\int Y_t^T (i)^{\frac{\epsilon_Y - 1}{\epsilon_Y}} di \right]^{\frac{\epsilon_Y}{\epsilon_Y - 1}} \tag{22}$$

$$Y_t^S = \left[\int Y_t^S (i)^{\frac{\epsilon_Y - 1}{\epsilon_Y}} di \right]^{\frac{\epsilon_Y}{\epsilon_Y - 1}}, \tag{23}$$

where Y_t^T and Y_t^S are tradable sector and nontradable sector aggregate outputs, respectively. Demand for product i in each sector can be derived from profit maximization of the final goods firms, as follows:

$$Y_t^T (i) = \left(\frac{P_{h,t}^T (i)}{P_{h,t}^T} \right)^{-\epsilon_Y} Y_t^T \tag{24}$$

$$Y_t^S (i) = \left(\frac{P_{h,t}^S (i)}{P_{h,t}^S} \right)^{-\epsilon_Y} Y_t^S, \tag{25}$$

where $P_{h,t}^T$ and $P_{h,t}^S$ are the price indices for consumption of tradable and non-tradable outputs as above. Then, price indices for sectoral output are given by

$$P_{h,t}^T = \left[\int P_{h,t}^T (i)^{1-\epsilon_Y} di \right]^{\frac{1}{1-\epsilon_Y}} \tag{26}$$

$$P_{h,t}^S = \left[\int P_{h,t}^S (i)^{1-\epsilon_Y} di \right]^{\frac{1}{1-\epsilon_Y}}. \tag{27}$$

We define total output as the sum of sectoral output evaluated at the steady-state prices, i.e.

$$Y_t = P_{h,ss}^T Y_t^T + P_{h,ss}^S Y_t^S = Y_t^T + Y_t^S. \tag{28}$$

The steady-state prices are assumed to be 1.

Market clearing conditions for output in each sector are given by

$$nY_t^T = nC_{h,t}^T + (1-n)\, C_{h,t}^{T*} + nCU\left(U_t^T\right) \tag{29}$$

$$=n\left((1-\omega)\, C_{h,t}^{R,T} + \omega C_{h,t}^{N,T}\right) + (1-n)\left((1-\omega)\, C_{h,t}^{R,T*} + \omega C_{h,t}^{N,T*}\right) + nCU\left(U_t^T\right), \tag{30}$$

and

$$nY_t^S = nC_{h,t}^S + (1-n)\, C_{h,t}^{S*} + nCU\left(U_t^S\right) \tag{31}$$

$$=n\left((1-\omega)\, C_{h,t}^{R,S} + \omega C_{h,t}^{N,S}\right) + (1-n)\left((1-\omega)\, C_{h,t}^{R,S*} + \omega C_{h,t}^{N,S*}\right) + nCU\left(U_t^S\right). \tag{32}$$

E.1.4 Intermediate Goods Firms

There is a continuum of firms in each sector, $i \in [0,1]$. The production function of differentiated product by firm i in each sector is given by:

$$Y_t^T(i) = A^T U_t^T(i)^{\alpha_U} L_t^{A,T}(i)^{\alpha_L} \tag{33}$$

$$Y_t^S(i) = A^S U_t^S(i)^{\alpha_U} L_t^{A,S}(i)^{\alpha_L}, \tag{34}$$

where A^T and A^S denote sector-specific productivity levels, and $U_t^T(i)$ and $U_t^S(i)$ are capacity utilization, which can be interpreted as capital utilization, labor utilization, and technology utilization. Firms pay the cost for utilization in the unit of sectoral output described as:

$$CU^T\left(U_t^T(i)\right) = \phi_1^{CU,T}\left(U_t^T(i) - 1\right) + \frac{\phi_2^{CU,T}}{2}\left(U_t^T(i) - 1\right)^2. \tag{35}$$

In the steady state, utilization is assumed to be 1 and the cost at the steady state level of utilization is 0.

We assume Calvo-type price rigidities with parameters θ_p^T and θ_p^S. These assumptions lead to standard New Keynesian Phillips curves in each sector.

E.2 Monetary Policy

Monetary policy follows the following Taylor rule

$$\widehat{R}_t^* = \rho_R \widehat{R}_{t-1}^* + (1-\rho_R)\left(\phi_\pi \widehat{\pi}_t^{agg} + \phi_Y \widehat{Y}_t^{agg}\right), \tag{36}$$

where hats denote (log) deviations from the steady state, and $\widehat{\pi}_t^{agg}$ and $\widehat{Y}_t^{agg}$ denote aggregate inflation and output, computed as weighted averages of regional inflation rates and output levels, respectively. We assume the central bank targets the nominal interest rate on the one-period bond faced by households, R_t^*.

E.3 Fiscal Policy

The government transfers cash to the home region with the following shock process:

$$tr_t = \rho_{tr} tr_{t-1} + e_t^{tr}, \tag{37}$$

$$\left(\frac{NTR_t^{reg}}{P_t}\right) = tr_t, \tag{38}$$

where tr_t denotes the real government transfer, e_t^{tr} is the transfer shock, and NTR_t^{reg} denotes the nominal transfer to the home region. We assume that the budget is balanced, so the budget constraint at the national level is given by:

$$n\frac{NTR_t^{reg}}{P_t} + (1-n)\frac{NTR_t^{reg*}}{P_t} = 0. \tag{39}$$

Transfers are allocated between Ricardian and non-Ricardian households as follows:

$$\omega\frac{NTR_t^{N}}{P_t} = \omega_T\frac{NTR_t^{reg}}{P_t} \tag{40}$$

$$(1-\omega)\frac{NTR_t^{R}}{P_t} = (1-\omega_T)\frac{NTR_t^{reg}}{P_t}, \tag{41}$$

where NTR_t^N and NTR_t^R are the nominal transfer for non-Ricardian and Ricardian households, respectively. When $\omega_T = 1$, all transfers go to non-Ricardian households in the home region. When $\omega_T = 0$, all transfers go to Ricardian households.

Similarly, in the foreign region,

$$\omega\frac{NTR_t^{N}}{P_t} = \omega_T^*\frac{NTR_t^{reg,*}}{P_t} \tag{42}$$

$$(1-\omega)\frac{NTR_t^{R*}}{P_t} = (1-\omega_T^*)\frac{NTR_t^{reg*}}{P_t}. \tag{43}$$

E.3.1 The rest of the model

Profits from firms are allocated as follows.

$$\frac{\Pi_t}{P_t} = \frac{\Pi_{h,t}^T}{P_t} + \frac{\Pi_{h,t}^S}{P_t} = (1-\omega)\frac{\Pi_t^R}{P_t} + \omega\frac{\Pi_t^N}{P_t} \tag{44}$$

where $\frac{\Pi_t}{P_t}$ is the real total profits, $\frac{\Pi_{h,t}^T}{P_t}$ is the real profits from tradable sector firms, $\frac{\Pi_{h,t}^S}{P_t}$ is the real profits from non-tradable sector firms, and $\frac{\Pi_t^R}{P_t}$ and $\frac{\Pi_t^N}{P_t}$ are the real profits for Ricardian and non-Ricardian households.

The allocation of profits is as follows. For non-Ricardian households,

$$\omega\frac{\Pi_t^N}{P_t} = \omega_\Pi\frac{\Pi_t}{P_t}, \tag{45}$$

where ω_Π is the parameter to determine the allocation of profits. We assume $\omega_\Pi = \omega$. Analogously, profits for Ricardian households are given by

$$(1-\omega)\frac{\Pi_t^R}{P_t} = (1-\omega_\Pi)\frac{\Pi_t}{P_t} \tag{46}$$

E.4 Detailed Calibration

The calibrated parameters for the baseline model are detailed in Table E.20. We set the discount factor to be 0.99 for a quarterly model. The home size is set to be $n = 1/27$ to reflect the number of states in Brazil.

The parameters for household preferences in the baseline model are as follows. The inverse of the elasticity of intertemporal substitution is set to be 1, which is a standard value in the literature. The inverse Frisch elasticity of labor supply parameter ψ is set to 1, following Furlanetto and Seneca (2014) and Bilbiie (2009). The parameter governing wealth effect on labor supply is 0, as the baseline is GHH. We note that as in the section below, E.6, this parameter is 1 if the utility function is the standard separable utility function, and 0 in the case of GHH preferences.

The share of tradable in consumption goods α_C is set to be 0.243 according to our calculation in Table D.2: services account for 75.7% at the national level. The share of tradable goods in total tradable consumption in the steady state α_T is set to be the same as n, which implies that there is no home bias for home goods. This assumption reduces the data requirements, since it avoids the need for interstate trade data in Brazil. We also assume that home and foreign tradable goods within a

country are highly substitutable, so the elasticity of substitution is large, i.e. $\theta_T = 10$. This value is larger than the value commonly set in the international business cycle literature, which ranges from below 1 to 2, but not far off from the international trade literature. In the regional spending literature, Nakamura and Steinsson (2014) and Pennings (2021) set this parameter to be 2 for the United States, although their models do not have tradable and non-tradable sectors like ours. We note that setting θ_T to a lower value like 2 does not change our model's prediction. Naturally, the share of the home region's non-tradable goods in non-tradable consumption, α_S is set to be 1. Since there is no consensus on the elasticity of substitution between tradable and non-tradable goods, we set θ_C to be 1 for simplicity, implying a Cobb-Douglas consumption function of tradable and non-tradable goods.

For the production function, we assume α_L, the labor elasticity, to be 0.58, so that the labor share is 0.55 as in Brazilian data from Penn World Tables 10. Then the production function utilization elasticity, α_U, is assumed to be $1 - \alpha_L$. The sticky price parameters for both tradable and non-tradable goods firms are set to be 0.75, which means the expected duration of prices is one year. The elasticity of differentiated goods, ϵ_Y, is set to be 21, which implies a steady-state markup ($\frac{\epsilon_Y}{\epsilon_Y - 1}$) of 1.05.

In the baseline, transfers are allocated to only home non-Ricardian households, so $\omega_T = 1$ and foreign Ricardian households finance it, i.e. $\omega_T^* = 0$. The fraction of profits allocated to Ricardian households in both home and foreign regions is assumed to be the same as the fraction of hand-to-mouth households.

E.5 Other calibrated parameter cases

We use the following calibration to examine the importance of different features of the model. Utilization case is associated with $\frac{\phi_2^{CU,T}}{\phi_1^{CU,T}} = 1.17$, taken from Smets and Wouters (2007). To examine the importance of the price stickiness, we assume $\theta_p^T = \theta_p^S = 0.001$ to shut down price stickiness. The temporary transfer case assumes that $\rho_{tr} = 0$. The no targeting case is when both types of households receive the same amount of transfers in the home region and finance it by the same amount in the foreign region, i.e., $\omega_T = \omega_T^\star = 0.54$. To examine the role of trade openness, we consider two cases: a less open economy with a home bias in tradable goods of $\alpha_T = 0.5$, and a more open economy with $\alpha_T = n/3$. We also consider a high wealth effect case by setting $\gamma_u = 1$, which corresponds to a separable utility specification. A high HtM share case assumes $\omega = 0.75$, while the low HtM share case is associated with $\omega = 0.25$. We also consider several alternative

Table E.20: **Model Baseline Calibration**

Parameter	Value	Description
β	0.99	Discount factor
σ	1	Inverse elasticity of intertemporal substitution
γ_u	0	Parameter that governs the wealth effect on labor supply
ψ	1	Inverse of Frisch elasticity of labor supply parameter
n	1/27	Home size
α_C	0.243	Share for tradables
α_T	1/27	Home bias for tradables
α_S	1	Home bias for non-tradables
θ_C	1	Elasticity between tradables and non-tradables
θ_T	10	Elasticity between home and foreign goods for tradables
ϵ_Y	21	Elasticity of differentiated goods
θ_p^T	0.75	Sticky price parameter for tradable goods
θ_p^S	0.75	Sticky price parameter for non-tradable goods
α_L	0.58	Production function labor elasticity
α_U	$1-\alpha_L$	Production function utilization elasticity
$\frac{\phi_2^{CU,T}}{\phi_1^{CU,T}}$	10000	Utilization cost parameter (tradable goods)
$\frac{\phi_2^{CU,S}}{\phi_1^{CU,S}}$	10000	Utilization cost parameter (non-tradable goods)
ω	0.54	Hand-to-mouth households share
ω_T	1	Transfers given to home non-Ricardian households
ω_T^*	0	Transfers given to foreign Ricardian households
ω_Π, ω_Π^*	ω	Allocation of profits in home and foreign regions
ρ_R	0.8	Interest rate persistence
ϕ_π	1.5	Taylor rule coefficient on inflation
ϕ_Y	0.5	Taylor rule coefficient on output
ρ_{tr}	0.99	Transfer persistence

calibrations, including a higher Frisch elasticity of labor supply ($\psi = 0.5$), a larger home region ($n = 0.5$), and Calvo-type sticky wages.

E.6 General Preferences

- Consider the first-order conditions for Ricardian households.

$$U_{C^R}^R\left(C_t^R, L_t^R\right) = \lambda_t^R$$

$$-U_{L^R}^R\left(C_t^R, L_t^R\right) = \lambda_t^R \frac{W_t^L}{P_t}$$

- λ_t^R is Lagrange multiplier for the budget constraint.

- For simplicity, we suppress the superscript R. Log-linearizing these conditions yields

$$\frac{U_{CC}C}{U_C}\widehat{C}_t + \frac{U_{CL}L}{U_C}\widehat{L}_t = \widehat{\lambda}_t$$

$$\frac{U_{CL}C}{U_L}\widehat{C}_t + \frac{U_{LL}L}{U_L}\widehat{L}_t = \widehat{\lambda}_t + \widehat{\left(\frac{W_t^L}{P_t}\right)}$$

- Define εs as follows.

$$\begin{aligned} \varepsilon_1 &\equiv -\frac{U_{CC}C}{U_C}, \ \varepsilon_2 \equiv -\frac{U_{CL}L}{U_C} \\ \varepsilon_3 &\equiv -\frac{U_{CL}C}{U_L}, \ \varepsilon_4 \equiv -\frac{U_{LL}L}{U_L} \end{aligned}$$

- Rather than calibrating the εs directly, we calibrate three preference parameters that uniquely determine them: the inverse elasticity of intertemporal substitution (σ), the inverse Frisch elasticity of labor supply (ψ), and the parameter governing the wealth effect on labor supply (γ_u). Specifically,

- $\sigma \equiv -\frac{U_{CC}C}{U_C}\sigma \, (= -\varepsilon_1)$
- $\psi \equiv \frac{U_{LL}L}{U_L} - \frac{(U_{CL})^2 L}{U_L U_{CC}} = \varepsilon_4 - \varepsilon_2 \frac{\varepsilon_3}{\varepsilon_1}$
- $\gamma_u \equiv \frac{U_{CL}C}{U_L} - \frac{U_{CC}C}{U_C} = \varepsilon_3 - \varepsilon_1$
- Notice that the two first-order conditions can be written as

$$\widehat{L}_t = \frac{1}{\psi}\left[\left(\frac{\gamma_u}{\sigma}\right)\widehat{\lambda}_t + \widehat{\left(\frac{W_t^L}{P_t}\right)}\right]$$

  - Separable utility function implies $\gamma_u = \sigma$.
  - GHH utility function implies $\gamma_u = 0$.

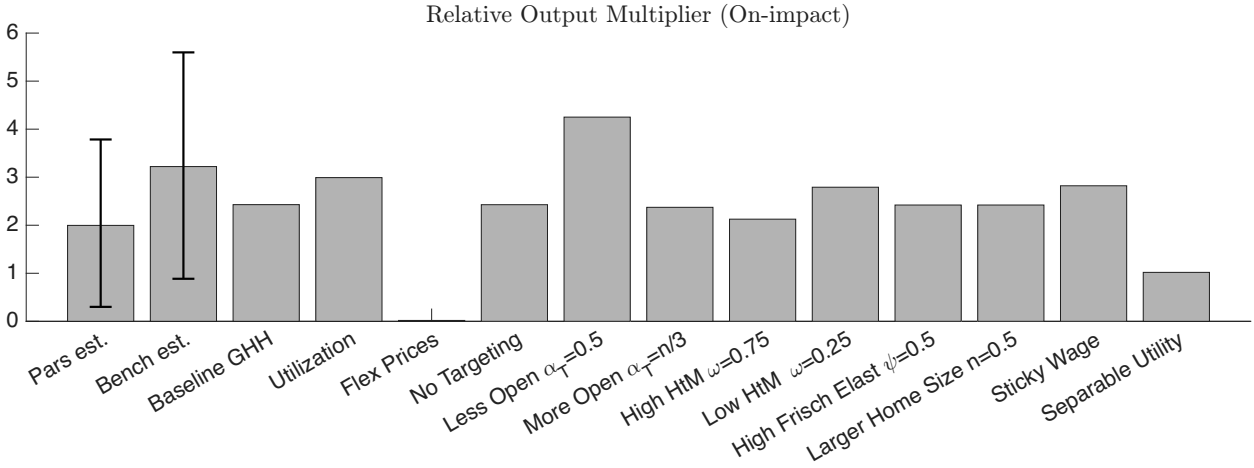
- Additionally, we have the following steady-state restriction.

$$\frac{\left(\frac{W^L}{P}\right)_{ss} L_{ss}}{C_{ss}} = -\frac{\varepsilon_2}{\varepsilon_3}$$

- These four conditions pin down the values of εs.

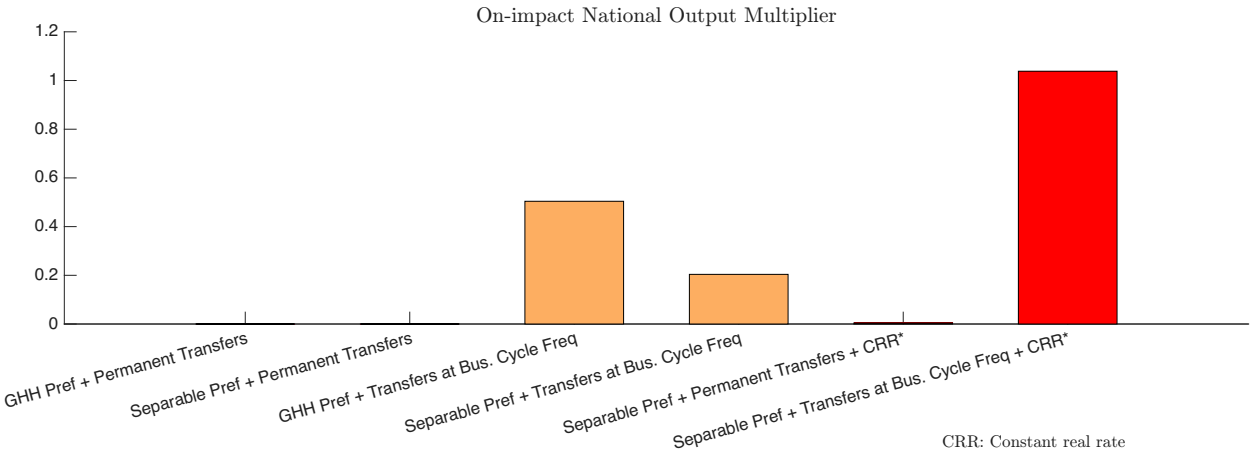
E.7 Additional model results

Figure E.9: **Impact local multipliers in different calibrations**



Notes: This figure plots on-impact multipliers implied in the model with different calibrations.

Figure E.10: **Model-implied National Output Multiplier**



Notes: The first two bars plot the first-year national output multiplier in the model with GHH and separable utility function in the baseline calibration of permanent transfer and active monetary policy rule (GHH Perm, Sep Perm), the next two bars plot national output multipliers in models with GHH and separable utility functions and transfer shocks' persistence in business cycle frequency $\rho = 0.935$ (GHH BC Freq, Sep BC Freq), and the last two bars plot national output multipliers in the model with separable utility function and two schemes of transfers and constant real rate monetary policy (Sep Perm CRR, Sep BC Freq CRR).