The Macroeconomic Effects of Cash Transfers: Evidence from Brazil*

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Abstract

This paper provides new evidence on the macroeconomic impact of cash transfers in developing countries. Using a Bartik-style identification strategy, the paper documents that Brazil's Bolsa Familia transfer program leads to a large and persistent increase in relative state-level GDP, formal employment, and informal employment. A state receiving 1% of GDP in extra transfers grows 2.2ppts faster in the first year, with R$100,000 of extra transfers generating five formal-equivalent jobs, half of which are informal. Consistent with a demand-side mechanism, the effects are concentrated in non-tradable sectors. However, an open-economy New Keynesian model only partially captures the high multipliers estimated.

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. The evidence on the aggregate effects of cash transfers in developing countries is relatively small, such as recent works by Egger et al. (2022); Gerard, Naritomi, and Silva (2021); Bracco et al. (2021); Cunha et al. (2022).[1]

This paper provides new evidence on the macroeconomic impact of cash transfers on GDP and employment in a developing country, to improve our understanding about fiscal policy broadly and government cash transfers specifically. We study the effect of Brazil's Bolsa Familia (BF) program, which is a large-scale anti-poverty cash transfer program, worth around 0.6% of Brazilian GDP in 2014, though 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.

[1]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. Corbi, Papaioannou, and Surico (2019) estimate the effects of inter-governmental transfers, not cash transfers to individuals, as we do here.

We estimate the effect that an increase in transfers received in one state relative to another state has on relative output—the *relative* transfer multiplier—and provide the first direct estimates of the effect of cash transfers on sub-national GDP and *informal* employment, in addition to formal employment, which is more commonly studied.

To this end, we construct a rich panel data set by harmonizing four large data sets for over 16 years between 2004 and 2019 for 26 Brazilian states, plus the Federal District. Our approach has two advantages. First, the state-level data allow us to estimate the effect of BF transfer on informal employment, since informal employment data are only representative at the state level. Like in most developing countries, informal employment is pervasive in Brazil, representing about half of total employment and up to 75% of employment in some lower-income states. Therefore, our estimated impacts of cash transfers on both formal and informal employment give a more complete picture of the effect on the labor market in developing countries. Second, we employ a panel data estimation to capture average effects across many BF rule changes over 15 years, rather than estimating the effect in a single year or specific policy changes, providing external validity. We then use the empirical estimates to shed light on whether an open-economy New Keynesian model, which has been shown to replicate well the evidence in an advanced country such as the United States, can be consistent with the evidence in a developing country.

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. Reserve 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-style 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 1 percentage point (ppt) of GDP, BF transfers increase by around 6ppts of state GDP in the poorest states in the Northeast but by less than 1ppt 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 there is no unobserved factor that is both correlated with national BF transfers in the time series and that differentially affect the same states that are more sensitive to national BF transfers in the cross section.[2] We also include state fixed effects to control for state-level trends, and time fixed effects to control for all common

[2]This identification assumption is similar to that in Nakamura and Steinsson (2014), Pennings (2021), and Guren et al. (2021).

aggregate shocks such as changes in aggregate fiscal and monetary policy.

We estimate a relative cash transfer multiplier of 2.2—a state that receives extra transfer worth 1% of its GDP relative to other states, grows 2.2 percentage points faster than other states—with cumulative multipliers that are large for up to 5 years after the transfer. This relative transfer multiplier is larger than those estimated using US state-level data in Pennings (2021), who finds a relative transfer multiplier of about 1.5 with permanent Social Security transfer changes, and 1/3 with one-off stimulus transfer payments. Furthermore, the impact of an expansion of transfers is large and significant for non-traded sectors, but insignificant for tradable sectors, consistent with a demand-side mechanism.

Second, we find that a state that receives R$100,000 in additional transfers relative to other states generates about three formal sector jobs (relative to other states). This result implies that the cost per formal sector job is about R$33,000. This cost-per-job is slightly above the estimates in Corbi, Papaioannou, and Surico (2019) who study intergovernmental transfers, but lower than the cost-per-job of COVID-19 Emergency Aid transfers in Cunha et al. (2022), and substantially lower than the estimates for the US economy in Chodorow-Reich et al. (2012) and Chodorow-Reich (2019).

Third, we find that the impact of cash transfers on informal employment is nearly three times as large as that on formal employment and is statistically significant: an extra R$100,000 in transfers generates almost nine extra informal jobs, albeit with a large standard error. This result could be due to the rigidity of the formal labor market, but also raises the possibility that households may hide their income to remain eligible for the BF program. While these are large jobs multipliers, many of the informal jobs are part-time or poorly paid. Consequently, our preferred measure of total formal-wage equivalent employment—a sum of the employment estimates above weighted by wages—increases by around five jobs per R$100,000 of transfers.[3] As such, we estimate that the total labor market effects of transfers are almost double the effect on formal employment alone, suggesting that other studies with only formal-sector data may be missing a large part of the impact of transfers in a developing country. Given the employment multiplier estimates, we can compute an implied GDP multiplier, as in Chodorow-Reich (2019) and Corbi, Papaioannou, and Surico (2019), among others. We find a wide range of implied multipliers, depending on whether the informal employment is ignored or interpolated, and how it is interpolated. Our analysis highlights

[3] This estimate does not imply that nine informal jobs are equivalent to two formal jobs. The formal-wage equivalent job multiplier is estimated independently and accounts for the–potentially–negative correlation between formal and informal employment.

that it is important to have a direct estimate of the output multiplier, as estimated here.

These baseline estimates are robust to several extensions we include to address potential threads to identification. For example, Brazil's business cycles may affect both national BF transfer changes and differentially affect states that are more sensitive to national BF transfer. We, therefore, add controls for state-specific sensitivity to the national GDP growth rate in one exercise, and controls for state-specific sensitivity to the commodity price growth rate in another exercise. In both cases, the relative output multipliers are larger than the baseline estimates, and continue to be statistically significant at conventional significance levels. Furthermore, we run a placebo regression when the confounding factors might be present but no transfers were actually paid. In particular, we counterfactually move the outcome variable 20 years earlier when no actual BF transfers took place. We find that there is no statistically significant effect on either relative output or formal employment.

Finally, our analysis suggests that an open-economy New Keynesian (NK) model can only partially explain the large and persistent local transfer multiplier in Brazil. In particular, we consider a version of the NK model similar to that in Nakamura and Steinsson (2014); Pennings (2021); Gali and Monacelli (2005), which has been used to rationalize the local transfer multipliers for permanent US Social Security transfers in Pennings (2021). We calibrate some key parameters to Brazilian data, such as the fraction of hand-to-mouth households. The calibration also reflects the fact that Bolsa Familia transfers are federally financed, almost permanent and targeted at poor households who are likely financially constrained. The NK model generates a multiplier of around 1.2 in the first year, a unit lower than our point estimate of 2.2. Over longer horizons, both model-implied and empirical cumulative multipliers trend down, but the multipliers in the model remain uniformly smaller (less than 1) than those estimated in the data (greater than 1). These results suggest that the NK model, while able to rationalize the local multipliers across US states, can only partially capture the larger local multipliers observed in Brazil. Motivated by the microeconomic literature on cash transfers, we discuss additional supply-side mechanisms—such as physical and human capital accumulation, increased labor supply due to improved health and enhanced entrepreneurship or better labor-market matches—which may have the potential to increase the local multiplier but are missing in a standard open-economy NK model. These areas reflect possible future research directions for the literature on the macroeconomics of developing countries.

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 been at the national level in advanced economies, as in Ramey (2011, 2019), Auerbach and Gorodnichenko (2016); Ramey and Zubairy (2018); Miyamoto, Nguyen, and Sergeyev (2018); Barnichon, Debortoli, and Matthes (2021). 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. Since the 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); Pennings (2021) among others. Except Pennings (2021), who studies transfers to individuals, most of this literature focus 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: regular cash transfers to poor households. Our estimates of the relative multiplier allow a comparison with multiplier estimates in an advanced country such as the United States. Our analyses help to understand deeper questions such as whether developing countries are more or less sensitive to demand shocks than developed countries, or if a cash transfer program that provides long-term and persistent social benefits can have a negative or positive short-run macro impact.

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) studies the effect of a large one-off transfer across Kenyan villages, and translates the impact to a local multiplier of 2.5. 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 almost 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.

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 effect. Although our findings are qualitatively consistent, there are three important distinctions, which stem from our macroeconomic focus. First, we directly estimate the impact of Bolsa Familia spending on state-level GDP as well as formal and informal employment. GDP data allow us to calculate fiscal multipliers directly, and informal employment is the majority of employment in the poorer states where Bolsa Familia spending as a share of state output is large. Second, we study the effect Bolsa Familia at the state level, rather than the municipal level. Although this results in a smaller sample size, we obtain a clearer measurement of general equilibrium effects as GDP and informal employment data are only available at the state level.[4] In addition, GDP data are substantially interpolated at the municipal level, and the national household survey (PNAD) with informal employment is not representative below the state level. Finally, we apply a different identification strategy, which allows us to study the effects of many policy changes over 15 years, rather studying a particular policy reform in 2009. We also compare our dynamic multipliers with those predicted by a NK model.

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, which was created in late 2003 with the goal to provide a vast expansion of social protection benefits in Brazil. In its inaugural year, the BF consolidated four other existing federal cash transfer programs, which we take account of in our measure of BF growth in 2004.[5] Shortly, the BF became the largest social program in Brazil,

[4] 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.

[5] 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.

reaching 14 million poor families–about one-fourth of the population–at the cost of about 0.5% of the Brazilian GDP. 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.

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. The benefit sizes and eligibility rules have changed over time. 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.[6]

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. To better understand the variation in expenditure, 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 assigned to states at the federal level to track the most recent estimates of poor households, based on national household surveys (PNADs) and the Census (2000 and 2010). In contrast with other cash transfers, the BF is not an automatic entitlement. Enrollment is limited to the state quotas, which are adjusted infrequently based on new vintages of PNAD and the Census. 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 is 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 A3(a) which plots for the history of quota changes at the national level. Instead, we pursue a Bartik-style identification strategy that exploits BF variation across all three components.

The coverage rate is the ratio of households enrolled in the BF relative to the state quota. Municipal governments are responsible for identifying eligible households and enroll them with the Single Registry (*Cadastro Unico*), a rolling Census of the poor in Brazil. As finding and registering the poor is no trivial task, there is often a lag between the assignment of quotas at the federal level

[6]The BF benefit is conditional to 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 conditionality requires all children ages 6-15 be enrolled in school, with a minimum attendance.

and enrollment by municipal administrations, so during this time, the coverage rate remains below one. The coverage rate could be endogenous if local authorities tend to enroll people faster during local recessions.

Finally, the average benefit is simply the total benefit received by each state divided by the number of households in the program and mostly reflects the generosity of the monthly transfer, as well as other program rules. Although the size of the benefits was originally fixed, the average benefit can be endogenous to local economic conditions as more people become eligible for the basic benefit for extremely poor households during local downturns. Moreover, the introduction of the Benefit to Overcome Poverty (BOP) in 2012, a poverty-gap payment, is directly tied to payment size to the incomes of the poor, greatly increasing the potential endogeneity of the payments.

The importance of the different components changed over time. From 2004 to 2008, the variation in BF transfers was dominated by the coverage component, as the municipal administration gradually fills the first quotas established in late 2003 (11 million). In 2009, there was a sizable increase in transfers driven by a quota reform–exploited in Gerard, Naritomi, and Silva (2021). After 2009, most of the variation in transfer was driven by changes in the average benefit, partly in response to inflation shocks.[7]

One particular empirical challenge, given our data are at an annual frequency, is that large changes in BF transfers tended 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.[8] 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.

3 Measurement of the Relative Multiplier

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

[7]We plot in Appendix Figure A1 the evolution of each component of transfers at the national level since the Bolsa Familia conception in 2004 to the end of our sample in 2019. We also discuss changes in each component in more detail in Appendix A.

[8]Appendix Figure A2 plots the average BF transfer changes over the months.

Specification. We estimate the effects of Bolsa Familia 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. All the standard errors are robust to serial correlation and heteroskedasticity.[9]

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 an instrumental variables strategy. We deal with OVB by controlling for fixed effects and a battery of other robustness tests. We discuss these two issues in detail below.

Reverse causality. The typical concern 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 BF transfer, and the national cycle is subsumed into time fixed effects. Therefore, reverse causality due to the procyclicality of fiscal policy is not the main concern in our case. Nevertheless, given that the BF program targets the poor, reverse causality from states' business cycles to BF transfers can still be an important threat to identification. For example, an adverse shock in state s that reduced its output ($e_{s,t} < 0$), could lead to an increase in poverty, inducing larger BF transfers via higher coverage or greater average benefits. A shock to state-level business cycles could also lead to larger future BF transfers via the quotas component, violating the exogeneity assumption. For these reasons, we expect ordinary least squares (OLS) estimates to be downward biased, requiring an instrumental variable (IV) approach.

Instrument. Our IV approach is similar to a Bartik instrument as it is based on heterogeneous

[9] We do not cluster standard errors at the state level because Brazil only has a small number of states: 26 states and the Federal district.

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 $\Delta\tilde{b}^Y_{s,t}$ (where the superscript Y denotes as a share of GDP):

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

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 we should exploit variation in the national exposure component $\gamma_s \frac{b_{N,t}-b_{N,t-1}}{y_{N,t-1}}$ as our instrument. The identifying assumption in this case is that national BF transfers do not change in response to economic shocks in particular states, especially those receiving larger BF transfers, i.e. higher exposure. Putting it differently, there is no unobserved factor that is both correlated with national BF transfers in the time series and that differentially affect the same states that are more sensitive to national BF transfers in the cross section. This identifying assumption is similar to that in Nakamura and Steinsson (2014), and is related to that in other papers in the Bartik-shock literature including Guren et al. (2021).[10] As BF is a complex national program, it is difficult to change program rules in response to shocks in particular states. Moreover, these rule changes tend to be permanent, so cannot be used to target year-to-year shocks in different 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. To see the heterogeneous exposure in the data, Figure 1(a) plots the BF transfer as a percent of state output for Piauí, a poor Northeast state, and São Paulo, a high-income Southeast state, as well as the national transfer as a percent of the national GDP. One can see the transfer to Piauí is an amplified version of trends in national transfers, whereas transfers to São Paulo are relatively flat. In general, as shown in Figure 1(b), BF transfer changes as a percent of state value-added output in northern states tend to be more sensitive to national BF transfer changes than those of southern states. This different sensitivity to changes in national BF transfers is used to identify the relative transfer multiplier.

[10]Specifically, consider the second version of our IV where the exposure shares γ_s are based on the relative size of BF transfers as a share of state GDP in some base period. Then Goldsmith-Pinkham, Sorkin, and Swift (2020) argue that these exposure shares need to be uncorrelated with other growth shocks, the errors $e_{s,t}$ in Equation 1, across locations. Borusyak, Hull, and Jaravel (2021) restate the same requirement that national BF transfer shocks should not be in years when high-exposure states have large growth shocks $e_{s,t}$, i.e. when the exposure-weighted residuals across locations is large. The requirement seems likely to be met as exposure weight are highest in the poorer North/Northeastern states, and the national BF program does not respond to growth shocks in those states.

Implementation. Following Nakamura and Steinsson (2014), we use two approaches to calculate state exposure to national BF shocks (γ_s): estimation of γ_s in a first stage regression and calibration of γ_s based on the relative size of BF expenditures as share of state GDP in a base period, which is closer to the traditional Bartik approach. As the results are very similar across approaches, we focus on the estimation approach as our main instrument and present the calibration approach as a robustness exercise in Section 6.3.

Specifically, our main instrument is the interaction of state dummy variables $\mathbf{1_s}$ with national BF transfers, such that the first stage becomes:

$$\frac{b_{s,t} - b_{s,t-1}}{y_{s,t-1}} = \gamma_s \mathbf{1_s} \frac{b_{N,t} - b_{N,t-1}}{y_{N,t-1}} + X'_{s,t}\delta_s + \eta_s + \eta_t + \varepsilon_{s,t}, \tag{2}$$

In this specification, γ_s is the exposure of state s to aggregate BF changes: if aggregate transfers increase by 1% of national output, transfers in state s increase by γ_s% of state GDP.[11]

For the second type of instrument, we calibrate γ_s based on the relative size of state-level and national BF transfers $\gamma'_s = \frac{b_{s,T}}{y_{s,T}} / \frac{b_{N,T}}{y_{N,T}}$ over some base period T.[12] The correlation between the calibrated γ'_s in base year 2003 or 2019, and the estimated γ_s in our main approach is around 0.95, as plotted in Appendix Figure A6(a) and Appendix Figure A6(b) (respectively). The multipliers using the calibrated exposure instruments are discussed in Section 6.3.

Omitted Variable Bias (OVB). The second challenge is OVB, where "something else" can affect both BF transfers and output growth. In our specification, 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, which removes much local discretion to its operations. 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.[13]

As a final check against OVB, we perform a battery of robustness tests in Section 6. First, we

[11] Equation 2 can be written in a more complete fashion where $\gamma_s \mathbf{1_s} \frac{b_{N,t} - b_{N,t-1}}{y_{N,t-1}}$ is replaced by $\sum_{j=1..s..27} \gamma_j \mathbf{1_j} \frac{b_{N,t} - b_{N,t-1}}{y_{N,t-1}}$. Note that these are equivalent as $\mathbf{1_j} = 0$ for $j \neq s$.

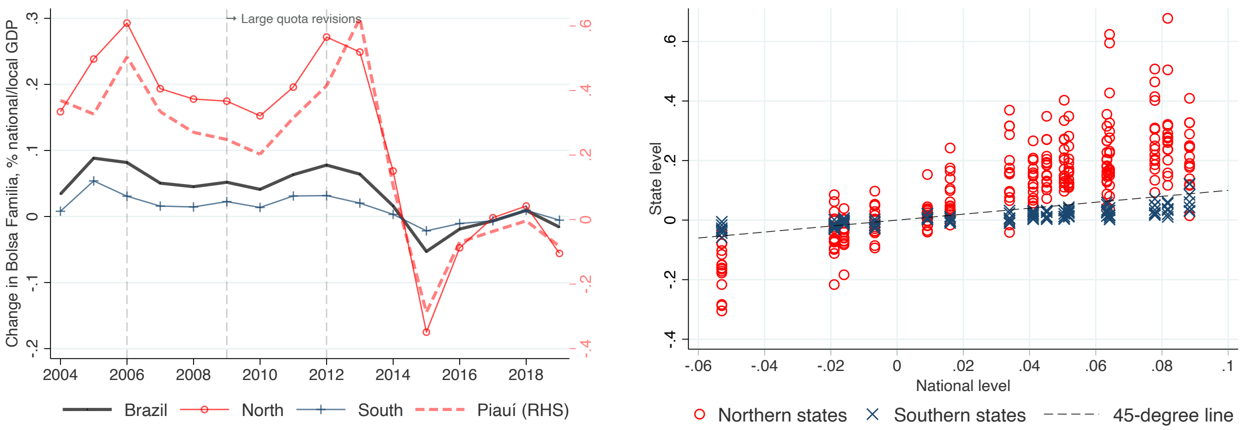
[12] The choice of the base period turns out not to be so important. We also consider average exposure over a number of periods.

[13] Uniform national program rules reduce the ability of state-level officials to change BF in response to a local shock.

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

(a) Bolsa Familia transfers, % of state/national value-added output

(b) Annual changes in BF transfers, % of state/national value-added output



Notes: Gross value added excludes agriculture, mining and quarrying sectors. In Figure 1(a), BF changes for Piaui is on the right-hand size axis. *Data source*: Authors' calculations with data from IBGE and the MDS.

show our results are generally robust to several additional controls that vary across states and over time, including state-specific sensitives to the national business and the international commodity cycles, other federal transfers to states, the poverty rate and population growth. Second, we perform placebo tests where the timing of transfers is moved 20 years earlier to test for potential omitted variables correlating with output growth in the years prior to the start of the program. We find no relationship between the placebo transfers and growth. Finally, we show that our results are generally not driven by a particular region, state or year as could be the case with some types of OVB. We also conduct other robustness tests to spillovers, anticipation effect and other dynamic specifications.

4 Data

We compile a comprehensive annual state-level panel data set from different sources which covers several macro variables such as value-added output, BF transfers, formal and informal employment between 2003 and 2019.[14] 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 source 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.

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

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). To improve the accuracy of the estimates, we exclude two volatile sectors from state output that are unlikely affected by BF because they are tradable: (i) agriculture, and (ii) mining, quarrying and petroleum (henceforth "mining" for simplicity).[15] As the resulting series is still fairly volatile, we use the Cook's distance to identify outliers and drop observations based on the standard cutoff in the literature.[16]

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 data allows us to examine the impact of cash transfers on both employment and labor income for formal employees. 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

Data on informal employment mostly comes from the national household survey PNAD/PNAD-Continua, available from IBGE. PNAD is representative of the population at the state level so—unlike other papers that study the effects of fiscal policy at the municipality level—we can estimate the effect on informal employment. The national household survey asks if individuals worked during the reference week, which sector and occupation, and if they had an active work card and were contributing to social security, which is an indicator of formal-sector employment. 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 PNAC-Continua, which provides a more comprehensive

[15] As shown in Appendix Table A1, the excluded sectors accounted for about 10% of value added on average over 2003-2019, ranging from 8% to 13% across regions.

[16] More specifically, we drop 22 observations where the Cook's distance is greater than 4/N where N=432 is the number of observations (27 states x 16 years of BF).

territorial coverage and quarterly information on the employment nationwide. The PNAD-Continua is available from 2016 to 2019. We use the 2010 Census to supplement the two household surveys. We then connect the PNAD, PNAD-Continua and 2010 Census to construct informal employment series from 2003 to 2019.

Data Quality. To ensure the reliability of PNAD data, we cross-check the accuracy of the PNAD *formal* employment data series with the administrative data taken from RAIS. The level and trends of formal employment are similar in PNAD and RAIS, and the Census, but the growth rates of state-level formal employment are more volatile from PNAD that RAIS.[17] The detailed comparison is plotted in Appendix Figure A4. To further validate PNAD data, we estimate the relative formal employment jobs multipliers using both RAIS data and PNAD data. Appendix Table A6 shows that the estimates are similar, but PNAD has larger standard errors. As for output, we exclude outliers identified by the common cutoff of Cook's Distance $> 4/Obs$ for all employment regressions. As informal employment growth is highly volatile, we winsorize the top and bottom 20% of employment growth.[18]

In Brazil, 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. Additional details on the main characteristics of the informal labor market in Brazil are reported in Appendix Table A2. Informal employment is most pervasive in agriculture, construction, and services. Consistent with our output data, we exclude employment in the agriculture and mining.

Our measure of informal employment includes informal employees and informal self-employed, but not informal employers or unpaid workers. Combined, the considered categories represent nearly 90% of informal employment, shown in the right panel of Appendix Table A2. 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. Self-employed are defined as self-employed who don't have a tax registration number (*Cadastro Nacional de Pessoas Jurídica*), and make up 37% of informal employment in Brazil. We do not include informal employers and unpaid employees as informal employers are relatively rare, only 3% of informal employment, and unpaid employees, which make up 14% of informal employment, have an unknown labor contribution.

[17] The correlation of formal employment growth over 2004-2019 between RAIS and PNAD is 0.58, after dropping the outlier Roraima state.

[18] The results for the less-volatile formal employment are also winsorized at this level, but are similar with a 10% winsorized. The coefficients are also similar without winsorizing, but only marginally significant at the 10% level (p-values below 0.11 for both OLS and 2SLS) with standard errors more than twice as large.

4.4 Summary Statistics

Table 1 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 poor North 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.

Table 1: **Summary Statistics for Brazil between 2004 and 2019**

	Output growth	BF transfer growth rate	Average BF (% of output)	Formal employment growth rate	Informal employment growth rate	Poverty rate (% of Pop)	Population growth rate
National	2.2	10.1	0.5	3.1	1.0	7.4	1.0
	(2.9)	(12.5)	(0.1)	(3.4)	(5.1)	(0.2)	(1.2)
North	2.2	14.3	1.2	4.2	2.9	9.6	1.8
	(3.7)	(14.9)	(0.4)	(4.6)	(6.6)	(0.5)	(1.9)
Northeast	2.5	9.7	1.8	3.5	1.2	11.8	0.9
	(2.9)	(11.2)	(0.4)	(3.9)	(5.0)	(0.2)	(1.1)
Southeast	1.9	10.8	0.2	2.8	0.7	5.5	1.0
	(3.1)	(16.0)	(0.0)	(3.5)	(5.5)	(0.3)	(1.3)
South	1.9	6.3	0.2	3.1	-0.1	4.4	0.8
	(3.0)	(15.5)	(0.0)	(2.9)	(6.1)	(0.2)	(1.3)
Midwest	3.1	10.8	0.2	3.4	1.3	5.5	1.7
	(2.5)	(21.4)	(0.0)	(3.4)	(7.7)	(0.6)	(1.0)

Notes: We report the average in the national and regional levels between 2004 and 2019. The numbers in parentheses are standard deviations. As in the rest of the paper, we exclude the mining sector from the aggregate variables. 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.

5 Main Results

This section presents the main results of our paper. We first discuss the value-added output relative multiplier, then break down the effects into traded and non-traded goods. Finally, we discuss the findings on the labor market including both formal and informal employment.

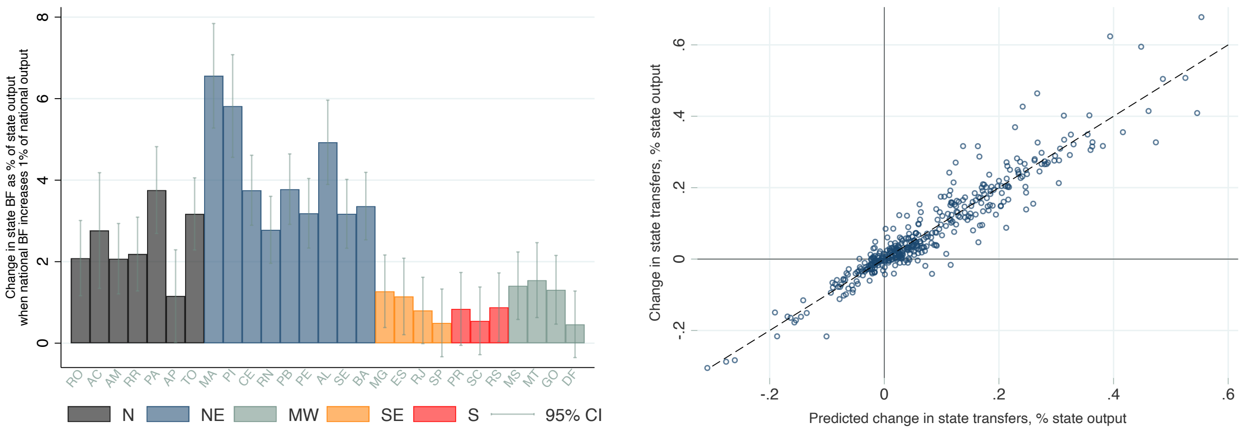
5.1 First Stage

As plotted in Figure 2(a), poorer areas have greater exposure to changes in national BF transfers with $\gamma_s > 2$ in equation 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 64, 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.

Figure 2: **The first-stage: state transfers predicted by the national variation in BF**

(a) State exposure shares (γ_s from equation (3))

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



Notes: γ_s are calculated from the benchmark specification, dropping outliers. Value added output excludes Agriculture, Mining and Quarrying sectors. N, NE, MW, SE, S denote North, Northeast, Midwest, Southeast, and South regions, respectively. *Source*: Authors' calculations.

5.2 Output Multipliers

Columns (1) and (2) of Table 2 present the benchmark estimates for the relative transfer multiplier: the effects of relative BF transfers on relative value-added output using ordinary least squares (OLS) and two stage least squares (2SLS). The OLS estimate of the relative output multiplier is 1.65, significant at the 10% level, although, as previously mentioned, we expect OLS estimates to be downward biased. The point estimate using 2SLS, which is our preferred specification, is 2.19, which is statistically significant at the five percent level. This means that when a state receives an extra R$1 of Bolsa Familia transfers relative to other states, output in that state tends to be R$2.2 higher than the other states not receiving the extra transfer. The standard errors of these estimates are around 1.

Our relative output multiplier estimate of 2.2 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 estimate of about 2 is comparable to the village-level multiplier 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 as reported 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).

Table 2: **Relative output multipliers**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	**Total output** (Benchmark)		**Non-tradable output**		**Tradable output**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
GDP Multiplier	1.65*	2.19**	1.16	1.64*	-0.32	0.29
	(0.90)	(1.04)	(0.80)	(0.95)	(0.57)	(0.69)
State & year fixed effects	YES	YES	YES	YES	YES	YES
Observations	410	410	415	415	404	404
First stage:						
F statistics		58		55		58
Exposure shares (γ): North = 2.5; Northeast = 4.1, Other regions = 1						

Notes: The numbers in parentheses are standard deviations.

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 demand for those goods lies largely in 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 growth rate of tradable or non-tradable value-added output, and the right-hand side variable is the change in BF transfers as a share of lagged tradable or non-tradable value-added output. The common denominator of left and right hand side variables means that our coefficient retains a multiplier interpretation.[19] As reported in Columns (3) and (4) of Table 2, we find that an R$1 relative increase in BF transfers increase the relative non-traded output by R$1.2 if estimated by OLS, and R$1.64 by 2SLS, with the latter statistically significant at the 10% level. In all of these cases, instruments continue to be relevant with first-stage F-statistics about 50. In contrast, the

[19]The multipliers in tradable or non-tradable specifications would normally *approximately* add to the aggregate multiplier if the controls and sample were the same. However, agriculture and mining were previously excluded from state GDP as they were excessively noisy in some states, and so the all-sector multiplier reported in the first two columns of Table 2 is more representative of the non-tradable sector.

effects of BF transfers on tradable output is small and insignificant from zero, as shown in Columns 5-6. These results are consistent with the view that transfers affect output through an increase in demand for locally produced goods and services.[20]

The increase in 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 A3 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-traded goods and services such as housing, health, education and other services, and less than 20% on manufactured goods and other durable goods.

Output per capita and population growth. Many papers in the literature focus on explaining per capita output growth to remove possible confounding trends in population growth. The first two columns of Table 3 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 continue to drop influential observations as identified by Cook's distance. We find that both the OLS and 2SLS point estimates, 2.33 and 2.76, respectively, are higher than the benchmark output result, and significant at the five percent significance level,suggesting that the relative output increase is not driven by changes in population.

We also investigate whether BF transfers leads to changes in the state-level population growth rate. Our measure of population growth is taken from PNAD for the 2004-2015 period, except 2010 which is interpolated, and PNADC for the 2016-2019 period.[21] We drop outliers based on Cook's distance and other 7 observations with annual population growth greater than 10%, though results are similar if we winsorize population growth like employment growth. The left-hand side variable in this specification is state population growth rate, $\frac{Pop_{s,t}-Pop_{s,t-1}}{Pop_{s,t-1},}$, and on the right-hand side is changes in BF transfers scaled by R\$100,000, as a share of lagged population. We control for state and year fixed effects as well as lagged state population growth. The coefficient of interest measures the increase in population in state s when this state receives extra R\$100,000 of transfers. Results

[20] In the Appendix, we report the multiplier for each sector in Table A4. The non-tradable sector result is driven by the effect of BF transfers on the construction sector, and all of the tradable sector multipliers are small and insignificantly different from zero.

[21] IBGE provides information on population at the municipal level. However, the IBGE population data are extremely volatile even at the state level, with many observations $> 30\%$ in absolute value. Furthermore, the IBGE data is not consistent with the UN-World Population Prospects at the national level, so we opt to use the population data from PNAD.

shown in Columns (3) and (4) of Table 3 suggest no statistically significant relationship between BF transfers and population growth.

Table 3: **Population and output per capita**

	(1)	(2)	(3)	(4)
Dependent variable:	**Output per capita**		**Population Growth**	
	OLS	**2SLS**	**OLS**	**2SLS**
Multiplier	2.33**	2.76**	0.70	1.25
	(0.99)	(1.10)	(1.95)	(1.00)
State & year fixed effects	YES	YES	YES	YES
Control lagged LHS	NO	NO	YES	YES
Observations	416	416	410	410
First-stage F statistics		88		36

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:

$$\sum_{j=0}^{h} \frac{y_{s,t+j} - y_{s,t-1}}{y_{s,t-1}} = \beta_h \sum_{j=0}^{h} \frac{b_{s,t+j} - b_{s,t-1}}{y_{s,t-1}} + X'_{s,t}\delta + \eta_s + \eta_t + e_{s,t}, \tag{3}$$

where $\sum_{j=0}^{h} \frac{y_{s,t+j}-y_{s,t-1}}{y_{s,t-1}}$ is the sum of $h+1$ state value added growth rates over horizons up to h periods and $\sum_{j=0}^{h} \frac{b_{s,t+j}-b_{s,t-1}}{y_{s,t-1}}$ is the sum of changes in BF transfer 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.

Table 4 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 cumulative multiplier estimated by 2SLS at the two-year horizon is 2.3, with a standard errors of about 0.8, so the estimates are statistically significantly different from zero at the one percent significant level. The first stage F-statistics continues to be well above the rule-of-thumb cutoff of 10. The cumulative multipliers are substantial and statistically significant: at the four- and five-year horizons, the cumulative multipliers are 1.92 and 1.37, respectively. These estimates suggest that the increase in output is also highly persistent, as the BF increases are persistent.

Table 4: **Cumulative multipliers over longer horizons**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Horizon:	**Contemporaneous**		**2 years**		**3 years**		**4 years**		**5 years**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Cumulative Multipliers	1.65*	2.19**	2.31***	2.27***	1.71***	1.59**	2.29***	1.92***	1.66***	1.37**
	(0.90)	(1.04)	(0.71)	(0.77)	(0.63)	(0.66)	(0.6)	(0.62)	(0.57)	(0.61)
State & year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	410	410	381	381	357	357	331	331	301	301
First-stage F statistics		58		85		144		197		191

Notes: The numbers in parentheses are robust standard errors.

5.4 Labor Market Multipliers: Formal & Informal Employment (Cost-per-job)

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), which we define below. To estimate the effects of relative BF transfers on the labor market, we estimate a variation of equation 1 as follows:

$$\frac{Emp^j_{s,t} - Emp^j_{s,t-1}}{Emp^{formal}_{s,t-1}} = \beta^j_0 \frac{(b_{s,t} - b_{s,t-1})/R\$100000}{Emp^{formal}_{s,t-1}} + \eta_s + \eta_t + e_{s,t} \quad j \in \{\text{Formal, Informal, FWE, Total}\} \tag{4}$$

where the left-hand side variable is the change of formal (informal, FWE, or total) employment normalized by the lagged formal employment, and in the right-hand side, BF transfers in multiples of R$100,000 is also normalized by lagged formal employment. Gerard, Naritomi, and Silva (2021) and Corbi, Papaioannou, and Surico (2019) use the same specification, but only for formal-sector workers. In all of our results below, we use this parsimonious specification, as adding controls does not change the results in a quantitatively meaningful way.

Table 5: **Employment Multipliers**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	**Formal employment**		**Informal employment**		**Total employment**		**FWE employment**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Jobs per R$100K	2.75***	3.02***	3.66	8.72***	8.65***	12.14***	5.83***	5.38**
("Jobs multiplier")	(1.02)	(1.08)	(2.45)	(3.07)	(2.55)	(2.96)	(2.02)	(2.29)
State & year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	408	408	406	406	399	399	401	401
First-stage F statistics		53		50		44		49

Notes: The numbers in parentheses are robust standard errors. FWE is Formal Wage Equivalent employment, where jobs are weighted by their income relative to the jobs in the formal sector as defined above.

Formal Employment. We report in columns (1) and (2) of Table 5 the OLS and 2SLS estimates of the effect of relative BF transfers on relative formal employment. A state that receives extra R$100,000 gets about three more formal jobs relative to other states. Both the OLS and 2SLS estimates are significant at the one percent significant level, with the standard errors around one. The first stage F-statistics continues to be well above the common threshold level of 10. The 2SLS point estimate implies that the cost per formal job is about R$33,000. Our cost per formal job is slightly above the estimates in Corbi, Papaioannou, and Surico (2019), which ranges between R$13,000 and R$28,000.[22] At an exchange rate of R$3 per US$, our estimates of cost per job is about US$11,000, remarkably lower than estimates for the U.S. of US$30,000-US$35,000, as in Serrato and Wingender (2016); Shoag (2013); Chodorow-Reich et al. (2012).[23]

Informal Employment. Columns (3) to (4) of Table 5 report the OLS and 2SLS estimates for informal employment in the parsimonious specification. The point estimates for the multiplier are large: a state that receives R$100,000 more than other states creates nearly 9 (2SLS) more informal jobs, though many of these jobs are part-time or poorly paid. The standard errors for these estimates are large, about 3, as informal employment data from PNAD have large variations. Since both the formal and informal employment multipliers are positive, we do not have enough information to analyze whether people switch from formal to informal jobs. However, the fact that BF transfers stimulates substantial informal employment suggests that it is important to account for informal employment when considering the macroeconomic effects of the cash transfer program.

Total Employment. To account for the effects of BF transfers on all types of employment in the economy, we sum formal and informal employment to compute the relative employment multiplier. Columns (5) and (6) of Table 5 presents the OLS and 2SLS estimates for total employment. The point estimate is large and statistically significant at one percent level: a state that receives R$100,000 in BF transfers gains about 9–12 total jobs (of any type).

Formal-Wage Equivalent (FWE). One of the puzzles of the results above is that the informal and total jobs multipliers are so large. However, since informal employment is associated with low earnings and sometimes lower hours, a job in terms of earnings in the informal sector is not

[22] The values in Corbi, Papaioannou, and Surico (2019) are denominated in R$ in constant 1998 prices. To compare to our results, in R$ in constant 2010 prices, we inflate their cost per job using the Brazilian Consumer Price index (IPCA).

[23] Using PPP-adjusted exchange rates of R$1.6 per US$, the estimated cost per job would be substantially higher at about US$20,000, but still much lower than the estimates for the U.S.

the same as a job in the formal sector. To properly account for the differences between informal and formal employment, we construct a formal-wage equivalent (FWE) employment in state s and year t, $FWE_{s,t}$. The FWE is a weighted sum of all types of employment that discounts for lower productivity in informality. More specifically,

$$FWE_{s,t} = Emp_{s,t}^{formal} + \rho_{s,t}^{infemp} Emp_{s,t}^{infemp} + \rho_{s,t}^{infself} Emp_{s,t}^{infself}.$$

where $\rho_{s,t}^{infemp}$ is the relative wage of informal employees to that of formal employees in state s and year t. $\rho_{s,t}^{infemp}$ is the analogous for informal self-employed.[24]

Columns (7) and (8) of Table 5 report the OLS and 2SLS estimates along with the standard errors for the relative formal-wage employment multiplier. A state that receives an extra R$100,000 in BF transfers generates five extra formal-wage equivalent jobs relative to other states. The point estimate is statistically significant at five percent level. These employment estimates all point to a substantially large impact of BF transfers on relative employment at the state level.

Sectoral Employment. Table 6 decomposes the employment multiplier across the tradable and non-tradable sectors. As in section 5.2, the tradable sector includes agriculture, mining and manufacturing, and the non-tradable everything else, particularly construction and services. Consistent with the GDP multiplier, the relative employment multipliers in the non-tradables sector are large and significant at the one or five percent levels. In contrast, the contribution of tradables is either small or insignificant. Appendix table A5 further decomposes the non-tradables into services and construction. The table shows that, consistent with the sectoral output estimates, both sectors respond strongly to BF transfers. For example, a state that receives R$100,000 more transfers than other states creates 2.5 formal-wage equivalent jobs in construction and 5.5 in services (2SLS estimates). These results are consistent with the above result that most of the changes in relative output come from the non-tradable sector.

Implications for back-of-the-envelope (implied) multipliers. Several papers in the literature with only formal employment data seek to produce back-of-the-envelope (implied) estimates of

[24] The discount factor is defined as $\rho_{s,t}^{i} = w_{s,t}^{i}/w_{s,t}^{formal}$ where $w_{s,t}^{j}$ is the average nominal wage of informal employment type i (employees or self-employed) in state s and year t. $w_{s,t}^{formal}$ is the analogous for formal employees. We calculate 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 $\rho^{self} = 0.55$, 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.

Table 6: **Sectoral Jobs Multipliers**
(Jobs per R$100,000 by sector and type employment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sector	**Formal employment**		**Informal employment**		**Total employment**		**FWE**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Benchmark	2.75***	3.02***	3.66	8.72***	8.65***	12.14***	5.83***	5.38**
	(1.02)	(1.08)	(2.45)	(3.07)	(2.55)	(2.96)	(2.02)	(2.29)
Tradable	-0.01	0.29*	-0.89	1.04	-1.89*	-1.29	0.39	0.95
	(0.15)	(0.17)	(0.90)	(1.13)	(0.98)	(1.14)	(1.02)	(1.27)
Non-tradables	2.94***	3.13***	8.21***	11.95***	9.04***	14.36***	7.58***	5.87**
	(1.09)	(1.10)	(2.42)	(3.00)	(2.84)	(3.24)	(2.01)	(2.31)

Notes: The numbers in parentheses are robust standard errors. FWE is Formal Wage Equivalent employment, where jobs are weighted by their income relative to the jobs in the formal sector as defined above.

GDP multipliers from the formula of Chodorow-Reich (2019), $\mathcal{M} \approx (1-\alpha)(1+\chi)(Y/E)\beta_E$, where $\mathcal{M}$ is the output multiplier, β_E is the jobs multiplier (jobs per R$100,000), Y/E is the output per worker, where output is expressed in units of $R100,000, $(1-\alpha)$ is the labor share and $(1+\chi)$ is the elasticity of total hours to employment. A more general version of this formula that takes account of informality is $\mathcal{M} \approx (1-\alpha)(1+\chi)(Y/FWE)(dFWE/dFE)\beta_{FE}$ where Y/FWE is output per formal-wage-equivalent worker (informal or formal), and dFWE/dFE is how many formal-wage-equivalent jobs are created for each formal sector job. Our analysis suggests that the implied multiplier is likely too small ignoring the response of informal employment (dFWE/dFE=1), but that a wide of multipliers can be generated if the informal employment response in dFWE/dFE is interpolated rather than measured. However, even if both formal and informal employment multipliers are estimated, the calculation is sensitive to other parameter choices, so it is important to have a direct estimate of the output multiplier as in this paper.

Preliminary calculation for Brazil. We first consider a preliminary calculation of the implied multiplier which uses Brazilian data but without explicitly adjusting for informal jobs created (dFWE/dFE=1). We use data from 2010, roughly the middle of our sample, and the base year for our deflators. The labor share in Brazil from Penn World Tables 10 is $(1-\alpha) = 0.55$. We calibrate the elasticity of total hours to employment $(1+\chi) = 1.12$ as in Corbi, Papaioannou, and Surico (2019) and Cunha et al. (2022), which is lower than the US value of $1+\chi = 1.5$ used by Chodorow-Reich (2019) because the Brazilian formal labor market is fairly rigid, so hours per worker are hard to adjust in the short term. Output per formal-wage-equivalent worker in 2010 in Brazil is roughly R$53,000, so $Y/FWE = 0.53$.[25] Hence the benchmark formal jobs multiplier

[25] Formal-wage equivalent employment includes formal and informal employment, but down-weights the latter by $\rho = 50\%$ to adjust for the lower productivity and lower hours per informal worker. We use this preferred measure throughout this section. Alternative preliminary approaches are output divided by total employment, resulting is

of 3 per R$100,000 BF transfers (Column 2 of Table 5) would translate into a preliminary implied output multiplier of 1 ($= 0.53 \times 1.12 \times 0.55 \times 3.02$). This is less than *half* of our 2SLS output multiplier in Table 2, suggesting the formal jobs multiplier is missing a large share of the effect.

Ad-hoc adjustments for informality. Several papers try to adjust the calculation above for the unmeasured effect of fiscal policy on informal employment. This involves adjustments for both the number of informal jobs created and the lower productivity of those jobs. Corbi, Papaioannou, and Surico (2019) scale their estimates by dFWE/dFE=1.2, based on the assumption that for each private formal-sector job, 0.5 informal jobs are created, though at $\rho = 0.55$ productivity.[26] Cunha et al. (2022) pursue a similar, but more elaborate adjustment and with a different calibration (we provide a simplified version here). They argue that informal workers are $\rho = 0.81$ as productive as formal-sector workers, and the elasticity of informal to formal employment is 2.35. Combined with a informality-to-formality ratio of 0.7 in our data, this results in a scaling factor of roughly dFWE/dFE=2.3.[27] Hence, the implied GDP multiplier with an ad-hoc adjustment for informality ranges from 1.2 to 2.3 based on our formal job multiplier estimates.[28] The wide range of implied multiplier estimates suggests that, if possible, it is advantageous to directly estimate the effects of fiscal policy on GDP and informal employment, as we do in this paper.

Direct estimates using the informal job multiplier. Implied multiplier estimates can be greatly improved by using job multipliers that measure effects on informal employment, rather than interpolating dFWE/dFE using a rule-of-thumb elasticity for the co-movement between informal and formal employment. Our preferred estimates using this approach utilize the Formal-Wage Equivalent (FWE) employment multiplier of 5.4 from Table 5, which allows to use the simple formulas of Chodorow-Reich (2019), but using FWE definition of employment (in other words

a back-of-the-envelope 20% lower, or output divided formal employment resulting in a multiplier 30% higher—both being broadly similar. Output and employment exclude agricultural and mining sectors not used in our empirical estimates, though this has little effect on estimates.

[26] The adjustment factor is calculated as $1.2 = 1 + 0.55 \times 0.5 \times 0.75$, where the last 0.75 appears as only three-quarters of the formal-sector jobs created in Corbi, Papaioannou, and Surico (2019) are private.

[27] More specifically, this is calculated as $dFWE/dFE = 1 + \rho(IE/FE)(dlnIE/dlnFE) = 1 + 0.81 \times 2.35 \times 0.7$. Note the huge difference between scaling factors in the two papers is mostly due to the differences in assumed relationships between formal and informal employment. The elasticity of 2.35 in Cunha et al. (2022), is roughly equivalent to assuming 1 formal job generates 1.64 informal jobs, which is 3-4 times that of Corbi, Papaioannou, and Surico (2019). Cunha et al. (2022) argue the informality-to-formality ratio is 1, with the differences explained by our exclusion of agriculture and mining, informal unpaid workers, and informal employers. Applying the ratio of 1, their scaling factor increases to 2.9.

[28] This calculation is done using the formula above, where $(Y/FWE) = R\$53,000$ is calculated using our estimates of $\rho = 0.5$ (a compromise between the relative wage of formal and informal workers in 2010 of 0.43, Engbom et al. (2022)'s estimate of 0.45 and 0.55 from Corbi, Papaioannou, and Surico (2019)), dFWE/dFE=1.21 for Corbi, Papaioannou, and Surico (2019) and dFWE/dFE=2.32 for Cunha et al. (2022). An alternative calculation uses the value of ρ reported in those other papers to calculate Y/FWE, so that the implied multipliers becomes 1.17 and 1.98, respectively.

$(dFWE/dFE)\beta_{FE} \equiv \beta_{FWE})$. This yields an implied multiplier of 1.8 (=$0.55 \times 1.12 \times 0.53 \times 5.4$), which is also fairly similar to our GDP multiplier from Table 2 of 2.2. Compared with a implied multiplier of 1 computed from the formal jobs multiplier only, this estimate of 1.8 suggests that around half of the employment effect of BF transfers works through informal employment. At the same time, the implied multiplier of 1.8 is still below our benchmark output multiplier, and also can vary depending on the measurement of the relative productivity of informal workers (ρ) and other parameters, underlining the importance of directly estimating GDP multipliers.

5.5 Labor Income

In this subsection, we estimate the relative labor income multipliers as a cross-check of our output and employment multipliers. The labor share in Brazil from Penn World Tables 10 is 50%, so we expect the labor income multiplier to be slightly greater than one (computed as 0.5 times the benchmark output multiplier). Moreover, considering our findings from the FWE employment multiplier, we expect the BF to have a similar impact on income across the formal and informal sectors. As shown below, our estimates for the labor income multiplier conform with these two predictions.

To estimate the labor income multiplier, we modify Equation 1 so that $y_{s,t}$ is the labor income in state s at time t. Similar to output, we do not consider labor incomes from the highly volatile agriculture and mining sectors. We also winsorize the 10% top and bottom of the distribution of labor income growth.[29] Labor income data come from the same sources as employment data. The informal labor income data in 2010 is interpolated due to the lack of PNAD data in that year, and 2010 is not influential for the results.

Columns (1) and (2) of Table 7 present the OLS and 2SLS estimates for the total (formal and informal) labor income multipliers along with their standard errors. The 2SLS estimate implies that a state that receives an extra R\$1 of BF transfers gets another R\$1.26 in labor income different from zero at the five percent significance level. The total labor income multiplier estimated by 2SLS is roughly half of the output relative multiplier, which is consistent with a labor share of around 50% in Brazil.

Formal and informal labor income multipliers are also presented in Columns (3) to (6) of Table

[29]We do not winsorize the top and bottom 20% as in the employment regressions because labor income is less volatile and the standard errors are small. Winsorizing 20% leads to lower coefficients and standard errors but similar significance levels, with the exception of the 2SLS for informal labor income which would be only significant at 18%.

7.[30] The 2SLS estimates suggest that a state that receives an extra R$1 of BF transfers gets R$0.94 of formal labor income and another R$0.94 of informal income. Both estimates are different from zero at conventional significance levels. Although the sum of the formal and informal labor income multiplier (1.84) is higher than the total multiplier of 1.26, they are not statistically different from each other.

Finally, while we do not report the results here for brevity, we also find that the relative multipliers for the labor income are driven by the non-tradable sectors, consistent with the results for output.

Table 7: **Labor Income Multipliers**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	**Total labor income**		**Formal Labor income**		**Informal labor income**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Multiplier	1.31**	1.26***	1.01***	0.92***	1.31**	0.92*
	(0.58)	(0.58)	(0.33)	(0.34)	(0.51)	(0.55)
State & year fixed effects	YES	YES	YES	YES	YES	YES
Observations	409	409	405	405	392	392
First-stage F statistics		89		82		46

6 Robustness Checks

This section presents the robustness exercises for our estimated relative transfer multipliers. We consider additional controls, placebo tests, spillovers and different samples to analyze the stability of our benchmark results. We also examine possible anticipation effects and estimates using alternative specifications.

6.1 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 8. For ease of comparison, we also repeat the OLS and 2SLS benchmark estimates in Columns (1) and (2) under "Benchmark Output".

First, to address the concern that we might be picking up mean reversion in state-level growth, we add a lag of state level value-added output growth as a control, which has almost no effect on multipliers for either OLS (column 3) or 2SLS (column 4). The coefficient on the lag of state level value-added output is also insignificant (not reported).

[30]We drop Piauí from the informal labor income estimation, as it is a clear outlier.

Second, 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. For both the OLS (Column 5) and 2SLS (Column 6) estimates, 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.

Third, 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 two lags of the poverty rate, which flexibly control for the level or change in the poverty rate in each state. Both the OLS (Column 7) and 2SLS (Column 8) estimates do not differ much from the benchmark, and the controls themselves are insignificant and not reported.

Fourth, we 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 type 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 parcipacao 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).[31] We include the contemporaneous change of total other federal transfers between t and $t-1$ normalized by the lagged state 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. Columns (9) and (10) report the OLS and 2SLS estimates of the relative output multiplier controlling for other transfers. Both OLS and 2SLS coefficients increase by around 0.2-0.4 relative to the benchmark, and are now significant at the five percent level. The coefficient on the contemporaneous changes in the other federal transfer programs is small (-0.2) but statistically significant.

Fifth, we address an important concern that there might be a spurious correlation between transfers and growth as states with large (or small) BF transfers might be more cyclically sensitive than others, and the *national* Bolsa Familia program might respond to *national* economic shocks.

[31] 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 state and municipalities are 1.2% and 1.1% of GDP each, 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* (BFC) program starts in 2004. In this specification, we assume that BFC does not change between 2003 and 2004. The results are similar if we instead drop 2004 in our sample: the 2SLS estimate is 2.60, significant at the 5 percent level.

To that end, we control for different sensitivities to the national cycle by adding 27 controls of state dummy variables interacted with contemporaneous national value-added output growth rate. The OLS and 2SLS multipliers with these controls are displayed in Columns (11) and (12) of Table 8. Both OLS and 2SLS estimates of the relative transfer multipliers with these controls are slightly larger than the benchmark estimates. The estimated relative output multiplier with OLS and 2SLS are 1.9 and 2.66, respectively. Both estimates are statistically significant at the 5 percent level.

Sixth, 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). Columns (13) and (14) display the estimates using both OLS and 2SLS: the relative output multiplier is substantially larger than the benchmark, and remains significant at the 1 percent level. In this specification, the 2SLS estimate of the relative multiplier for output is now 3.69 and statistically significant at 1 percent, but this estimate is insignificantly different from our benchmark multiplier of 2.19.

Finally, we include all these additional controls into the specification. The estimates in columns (15) and (16) are significantly different from zero at the 1 percent level. The point estimate is 3.8 in the 2SLS specification, similar to the specification where we control for state interactions with commodity price cycle. The takeaway from this exercise is that our estimated multipliers are fairly robust to different controls and, if anything, our benchmark specifications are conservative on the size of the multipliers. While all the multipliers are significant, they also come with fairly large standard errors, of around 1.[32]

6.2 Spillovers

One concern about the interpretation of the relative multiplier is that 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 and the first lag of average BF transfers in neighboring states. The OLS and 2SLS point estimates are 1.76 and 1.93, respectively, and both significant at the 10% level. The coefficients on the neighboring

[32]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 A8. We always deflate state GDP with the GDP deflator.

Table 8: **Relative output multiplier: robustness to additional controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Additional controls:	**None** (Benchmark)		**lag state VA growth** $\Delta \ln VA_{s,t-1}$		**Population** $\ln Pop_{s,t}$ $\ln Pop_{s,t-1}$		**Poverty** $Pov_{s,t-1}$ $Pov_{s,t-2}$	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Multiplier	1.65*	2.19**	1.65*	2.18**	1.8*	2.34**	1.63*	2.26**
	(0.90)	(1.04)	(0.90)	(1.04)	(0.92)	(1.09)	(0.91)	(1.06)
State & year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	410	410	410	410	410	410	410	410
First-stage F statistics		58		57		52		55
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Additional controls	**Other federal transfers** $\frac{GFed_{s,t}-GFed_{s,t-1}}{Y_{s,t-1}}$		**National business cycles** $\Delta \ln VA_{N,t} \times I_s$		**Commodity price cycle** $\Delta \ln P^{Commodity}_{N,t} \times I_s$		**All controls**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Multiplier	1.88**	2.59**	1.94**	2.66**	2.48***	3.69***	2.53***	3.8***
	(0.90)	(1.05)	(0.96)	(1.11)	(0.95)	(1.04)	(0.94)	(1.01)
State & year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	410	410	410	410	410	410	410	410
First-stage F statistics		50.5		113		46.5		102

Notes: The first two columns (1) and (2) are the benchmark specification estimated using OLS and 2SLS, without additional controls. Columns (3) and (4) display the estimates when one lag of the state value added (VA) is added. Columns (5) and (6) report the estimates when we add log state population (and first lag) as a control. Columns (7) and (8) display the estimates when we control for the state poverty rate (first and second lags). Columns (9) and (10) show the estimates when we control for other federal transfer changes for each state in the regression. Columns (11) and (12) show multiplier estimates when we control for state sensitivities to the national business cycle by adding national GDp growth interacted with state dummy variables. Columns (13) and (14) are the estimates when we control for contemporaneous commodity price index interacted with state dummies. Columns (15) and (16) report the results with all controls simultaneously.

states' BF transfers are small and insignificant. This result suggests that the spillover effects are likely small.

6.3 Alternative Instruments

Our benchmark 2SLS 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 estimated in the first stage regression. While this 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.

An alternative approach, as discussed in Section 3, is to *calibrate* 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. This calculation may be less sensitive to BF shocks or mis-measurement, and is also much closer to a classic Bartik instrument, making it easier to calculate and interpret.

Table 9 presents estimates of the impact multiplier using the alternative instrument for differ-

Table 9: **2SLS Relative Output Multiplier with Alternative IVs**

	(1)	(2)	(3)	(4)
Dependent variable:	**Benchmark**	**With Alternative Instrument (γ_s')**		
		2003	**2019**	**2003-2019**
GDP multiplier	2.19**	1.89*	2.64**	2.26**
	(1.04)	(1.08)	(1.13)	(1.06)
State & Year FEs	YES	YES	YES	YES
Observations	410	410	410	409
First stage:				
F statistics	58	428	416	525
First-stage coefficient (ϕ):		0.82***	0.88***	0.92***

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).

ent base years, including an average over the whole sample. The results suggest that estimated multipliers are similar to our benchmark specification of around 2, and are statistically significant. Specifically, when using the average BF spending shares over the whole sample, as reported in column (4), the multiplier is 2.26, significant at the five percent level, which is almost identical to our benchmark 2SLS estimates. The multiplier is slightly larger using BF transfers shares in the final year of the sample (2019) as the base year, and slightly smaller when using the first year of BF (2003) as the base year, with the p-value increasing to 0.08 in the latter case. Note that the calculated exposure shares are extremely strong instruments, with a first-stage F-stat of over 400. The first-stage coefficients on the predicted BF transfers range from 0.82 in 2003 to 0.92 for the whole sample—indicating that predicted and actual transfers move almost one-for-one. The lower coefficient in 2003 is not all that surprising as the program was just being implemented, with the transfer measure including pre-BF components such as Bolsa Escola, so the cross-state allocation of BF expenditure may be less representative. The point estimates are also similar using 2003 or 2019 as base years and dropping the base year from the estimation sample, though the standard errors are slightly larger with fewer observations.

The reason the alternative (calibrated) and main (estimated) instruments generate similar impact multipliers is that state BF expenditure shares are very good proxies for the estimated γ_s, with a correlation around 0.95. Appendix Figures A6(a) and A6(b) show that the estimated and calibrated exposure shares line-up close to the 45 degree line for both 2003 and 2019.

6.4 Placebo Tests

Even if we cannot control for potential confounding variables directly, we can test for a more general kind of OVB by running a placebo regression when the confounding factor might be present but no transfers were actually paid. A non-zero multiplier might indicate a spurious relationship. For example, it could be that some poorer north and north-eastern states—which happened to receive the largest BF transfers—always had faster growth due to a neoclassical convergence effects.[33] To that end, we counterfactually move the outcome variable 20 years earlier when no actual BF transfers took place. That is, we regress GDP growth or formal employment growth in the 1980s and 1990s on BF transfers in the 2000s and 2010s.[34] The results, reported in Table 10, show that for both GDP and formal employment growth, the estimated placebo multiplier is not statistically different from zero at conventional significance levels.

Table 10: **Placebo tests**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	**A. Output multiplier**				**B. Formal employment**			
	Benchmark (2004-2019)		**Placebo (1985-2000)**		**Benchmark (2004-2019)**		**Placebo (1985-2000)**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Multiplier	1.65*	2.19**	0.08	-0.95	2.75***	3.02***	0.88	-0.85
	(0.90)	(1.04)	(1.06)	(1.25)	(1.02)	(1.08)	(0.87)	(1.12)
State & year fixed effects	YES	YES	YES	YES	YES	YES		
Observations	410	410	408	408	408	408	395	395
First-stage F statistics		58		65		53		50

Notes: Columns (1) to (4) display the OLS and 2SLS estimation for output multiplier, where columns (1)-(2) are our baseline estimates using data between 2004 and 2019, and columns (3) and (4) show placebo test where we regress BF changes on output between 1985 and 2000. Columns (5)-(8) display the analogous placebo test for formal employment.

6.5 Influential States, Regions or Years

We next examine whether our results are driven by specific influential years, states or regions, which we drop one-by-one.

The results dropping years one-by-one are summarized in Table 11 and suggest that although two years are influential, their effects are mostly offsetting. Specifically, columns (3)–(4) present the estimated relative multiplier when dropping 2012 which makes both OLS and 2SLS multipliers smaller and insignificant. The main reason for this is 2012 involved the introduction of the Benefit to Overcome Poverty, which expanded the size of payments to poor households (an intensive margin transfer), which turned out to have a large multiplier.[35] The other influential year is 2015, which

[33] Generally, faster growth in these states will be partialled-out by state fixed effects, but some bias might remain if the growth gap slowed at the end of the sample.

[34] We start the placebo sample in 1985 due to data availability. We cannot run placebo tests for informal employment due to the lack of data.

[35] However, excluding the Benefits to Overcome Poverty after 2012 as a whole results in similar multipliers as in the benchmark specification.

involved a large reduction in the real value of transfers due to a surge in inflation without a compensatory increase in nominal benefits. Excluding 2015 increases the OLS multiplier from 1.65 to 2.7 (Column 5), and the 2SLS multiplier from 2.19 to 3.9 (Column 6), and the 2SLS estimate is significant at the one percent level. Nonetheless, one can see that the effect of 2012 and 2015 are mostly offsetting. Consequently, dropping *both* 2012 and 2015 (Columns (7)–(8)) yields multipliers fairly similar to the benchmark results of around 1.7–2.6. Panel A of Appendix Table A10 show no other individual year is influential.

Appendix Table A10 Panel B shows that our results are generally robust to dropping states one-by-one.[36] Dropping the North, Southeast, Midwest regions one-by-one (Panel C) generally yields similar GDP or formal/informal employment multipliers as in the baseline. However, dropping the Northeast increases the output multiplier, and dropping the South reduces it, with the effects being mostly offsetting in our full-sample results.[37]

Table 11: **Robustness to alternative samples**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Benchmark		**Drop 2012**		**Drop 2015**		**Drop 2012 & 2015**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Multiplier	1.65*	2.19**	0.83	1.21	2.70**	3.90***	1.77	2.64**
	(0.90)	(1.04)	(0.92)	(1.06)	(1.07)	(1.32)	(1.09)	(1.33)
State & year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	410	410	384	384	383	383	357	357
First-stage statistics		58		49		31		27

Notes: The first two columns (1) and (2) are the benchmark specification estimated using OLS and 2SLS. Columns (3) to (8) display the estimation when the samples do not have 2012, or 2015, or both 2012 and 2015, respectively. Columns (9) and (10) show the estimates when we exclude benefits to overcome poverty (BTOP) after 2012.

6.6 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, and 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) with future BF as an additional explanatory variable:

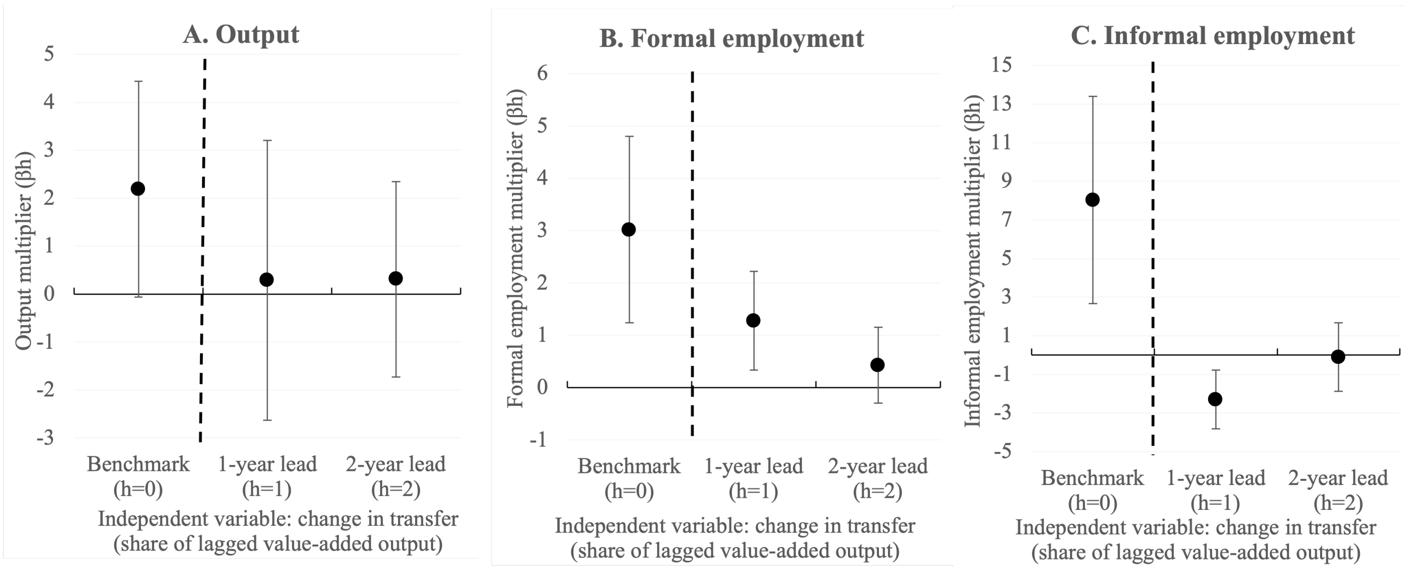
$$\frac{y_{s,t}-y_{s,t-1}}{y_{s,t-1}} = \beta_0\frac{b_{s,t}-b_{s,t-1}}{y_{s,t-1}} + \beta_h\frac{b_{s,t+h}-b_{s,t+h-1}}{y_{s,t-1}} + \eta_s + \eta_t + e_{s,t} \qquad \text{with} \qquad h=\{1,2\}. \quad (5)$$

[36]We note that dropping the Southern state of Santa Catarina lowers the 2SLS multiplier to 1.57, but this estimate falls within the bound of our benchmark estimate.

[37]Excluding the north-east also increases the formal employment multiplier and reduces the informal employment multiplier.

The anticipation effect, β_h for $h = 1, 2$, captures the response of output to changes in BF transfers one or two years in the future. Analogous regressions are estimated for anticipation in formal and informal employment. The results, plotted in Figure 3, suggest that there are no statistically significant anticipation effects on output. Perhaps surprisingly, formal employment rises and informal employment falls one-year ahead of increases in BF transfers. While these coefficients are significant, they are quantitatively small relative to the contemporaneous multipliers. For two-year leads, the multipliers are close to zero and insignificant.

Figure 3: **Bolsa Familia anticipation effects**



Notes: The figure plots the 2SLS results with the 95% confidence bands for the estimates of β_h in equation 5. The standard errors are robust. Leads correspond to $h = 1, 2$.

6.7 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 multiplier in our main results, alternative approaches could be (1) regressing two-year changes in output on two-year changes in BF transfers as in Nakamura and Steinsson (2014), or (2) 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. Appendix Table A9 shows that the estimates from the two-year change specification are similar to our baseline (2.06) and statistically significant at one percent significance level. In the dynamic specification, the sum of the coefficients is larger than our baseline (3.05) and has larger standard errors, so they are not distinguishable from our baseline estimates. Employment multipliers are also broadly similar to our main results using these alternative specifications, as detailed in Appendix Table A9. These analyses suggest that our baseline contemporaneous and cumulative multipliers broadly capture the effects of BF transfers on the local economy.

7 Interpretation of the Empirical Estimates in a New Keynesian Model

This section investigates whether a relatively standard open-economy New Keynesian model, which has been shown to explain the local transfer multiplier across US states in Pennings (2021), can also explain the large and persistent BF multipliers in Brazil.

We provide a summary of the model here and refer the reader to Pennings (2021) 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, representing a state in Brazil whose residents receive a net BF transfer, and a large foreign region with the rest of the population, who pays for the BF transfer via lump sum taxes. Households have standard separable preferences over consumption, c_t, and labor, L_t, each period, $log(c_t) - \frac{L_t^{(1+\varphi)}}{(1+\varphi)}$, and 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 borrow and save through a risk-free bond. Each region produces its own variety of imperfectly substitutable goods, which are used for local and foreign consumption. Output in each region is produced using only labor. Both prices and wages are sticky and there is home bias in consumption.

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 it is spent rather than saved and (ii) that spending is on locally produced goods. This depends on the fraction of hand-to-mouth households, the extent to which transfers are targeted towards them, the persistence of transfers, if untargeted, and home bias in consumption. As sticky prices and wages make output partially demand-determined in the short run, an increase in local demand increases output. But in the medium term, prices and wages adjust and higher local demand feeds into higher prices and wages rather than higher output. In the long run, local output falls due to wealth effects on labor supply—that is, richer households in the home regions want spend some of their additional income from transfers on leisure.

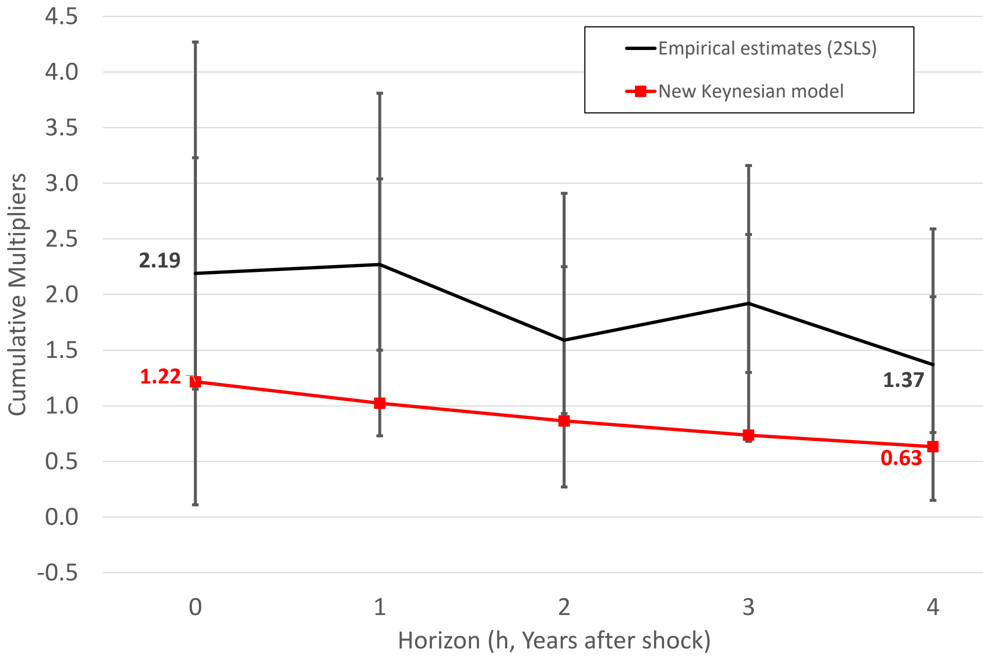
Calibration. To compute the model-implied multipliers, we use the standard calibration in the literature, and re-calibrate two main sets of parameters of the model to reflect the Brazilian context. First, as Brazil is a developing country with higher poverty rates, we calibrate the share of hand-to-mouth households, $\omega = 0.54$, as the fraction of the Brazilian households that is unable to come up with 1/20 Gross National Income per capita in emergency funds from the Global Findex database (2017) in Bracco et al. (2021), compared to 1/3 for US states in Pennings (2021). Second, we also calibrate BF transfer process to be almost permanent, with transfers targeted at the hand-to-mouth households, who are often poorer, relative to untargeted transfers for the U.S. The rest of the important parameters are the same as Pennings (2021).[38]

Results. Figure 4 plots the model-implied cumulative multipliers over several horizons along with their empirical counterparts. At all horizons, the model-implied multiplier is less than that observed in the data. The impact multiplier implied by the model is 1.22, which is around one unit lower than 2.19 estimated in the data. At longer horizons, the model-implied cumulative multipliers fall below unity, while the estimated counterparts are always above 1. While the model-implied multipliers are often within one standard error of the estimated multipliers, the fact that they are persistently lower suggested the calibrated NK model does not fully capture the large and persistent multipliers observed in Brazil.

Discussion. While the changes to the calibration to fit the Brazilian context moves multipliers in the direction of the empirical evidence—0.35 higher than the US calibration in Pennings (2021), as shown in Appendix Figure A5—they are still not large enough quantitatively. The hand-to-mouth fraction of $\omega = 0.53$ is large but only has a small impact on multipliers in our context. The reason is that the BF transfers are close to permanent, so transfers would be spent even by Ricardian households. Targeting in the model is more important—especially in the medium run—because of its impact on the labor market rather than the marginal propensity to consume. Because we assume sticky wages are set based on the labor-leisure first order condition of Ricardian households only—as is common in the literature such as Erceg, Henderson, and Levin (2000) and Cogan et al. (2010)—targeting the transfers at hand-to-mouth households reduces the boost in consumption of the home Ricardian households, and reduces the strength of wealth effects on the labor market and wages in the medium run. If transfers were untargeted, Ricardian households would want to work less when their wages reset, reducing the multiplier. We note that, although we do not consider in

[38] We also calibrate the population size of the home state to be $n = 1/27$, to reflect the number of states in Brazil, up from 1/50 in the U.S., though this has little impact on the results. Other model parameters are: a quarterly discount factor of 0.99; prices and wage both adjust once a year, on average; the elasticity of substitution between home and foreign goods is 2; the consumption home bias parameter is 0.69 and the Frisch labor supply elasticity is set at 1. We aggregate results to annual frequency to be consistent with the frequency of the empirical estimates.

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



Notes: Black lines with 2 standard error bounds (and 1 standard error tick marks) plot 2SLS cumulative multipliers as reported in Table 4. The red line reports multipliers calculated using the New Keynesian model of Pennings (2021) but calibrated to Brazil and Bolsa Familia transfers. Nominal regional GDP in both NK model and data are deflated using the regional GDP deflators, so reflect changes in quantities.

the model calibration due to the lack of empirical evidence, prices and wages in Brazil are arguably less sticky than in the U.S., so multipliers in the baseline NK model could be even lower.

As such, in order to explain the effect of BF transfers in the data, the models may need to be tailored to features of a developing country. These potential modifications could capture an increase in supply—human and physical capital, and TFP due to lower misallocation—and are motivated by the microeconomic development literature.

First, there is little evidence in the micro-development literature in favor of negative wealth effects from cash transfers targeted at poor households that are standard in NK models (with separable preferences) and which drive the fall in multipliers over longer horizons (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).

Second, BF transfers can facilitate human and physical capital accumulation, which can increase output. 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, and Paul Schultz (2004) finds a 10% increase in schooling from a cash transfer program in Mexico. Finally, transfers may help to increase allocation efficiency, such as enabling entrepreneurship or better labor-market matches.[39] These three types of gains, even if in the future, can generate higher current aggregate demand in the short term as households anticipate higher output in the future. All of these channels are missing from our NK model, but form an important future research agenda.

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. Unlike other papers in the literature, our macroeconomic focus—at the state level with many years of policy changes—allows us to directly estimate GDP multipliers and utilize data on informal employment in addition to formal employment.

We find a substantially 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 increase in output, and the effect persists for several years. Furthermore, we find that much of the relative output increase comes from the non-tradable sector. We estimate large increases in both formal and informal employment. In fact, incorporating the difficult-to-measure effect of informal employment approximately *doubles* the employment multiplier (measured in comparable terms), relative to the standard approach of only measuring the effect on formal employment.

We investigate whether an open-economy New Keynesian model, which has been shown to be able to rationalize the effect of transfers across US states, can also explain the large and persistent effect of BF transfers. Despite calibrating key parameters to the Brazilian context, the model-implied cumulative multipliers tend to be much smaller than the empirical counterparts. We hypothesize that incorporating some of the supply-side benefits of cash transfers—labor, capital (human and physical) and TFP (misallocation)—as documented in the microeconomics literature may be important in order for the model to come closer with the data.

[39] Egger et al. (2022) find some empirical evidence in Kenya that small businesses in poor areas may be operating at below maximum utilization and so can increase output to meet demand without employing additional factors of production. However in our Brazilian context, increases in GDP correspond with large increases in employment, which was not the case in Kenya.

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Appendix

A Additional details on Bolsa Familia

Quotas are updated *infrequently* with information lag of two to three years after the release of a new PNAD or Census, as shown in Appendix Figure A3(a)).[40] 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 sizeable 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 takes time to implement quotas, which we plot in Appendix Figure A3(b). Rules for coverage also change over time: with the rise in income, the initial eligibility income threshold in 2004 is revised in 2006, then the MDS passes 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.[41] 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.

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.`

[40] Though the 2023 quota update was based on the 2010 Census, though these are not in our sample.

[41] 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 A3(c).

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 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 Escolar in that state.

- Cartão Alimentação (Food Card) designed to eradicate extreme hunger. Total expenditure (mostly in the North and Northeast regions) was taken from Balsadi el al. (2004) – download here. The number of beneficiaries per state, used to calculate state-level expenditure, was taken from Pasquim (2006) (Grafico 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 from the Brazilian Statistics Bureau (Instituto Brasileiro de Geografia e Estatistica, IBGE). To compute GDP and Gross Value Added (VA) at different levels of aggregation, we used several tables provided by the 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: $NVA^i_{st} = NVA_{BR,t} \times \eta^i_{st} \times \gamma^i_t$

- Volume of sectoral state VA Index: $I^{VA,i}_{st}$

  – Source: IBGE spreadsheet "Tabela 5"

- Real sectoral state RVA^i_{st}

  – Computed iterating back and forwards $NVA^i_{s,2010}$ with the growth rate of $I^{VA}_{st^i}$

Formal employment. Formal employment data is 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 Additional figures and tables

Figure A1: **Bolsa Familia Variation over time**, % of value-added output

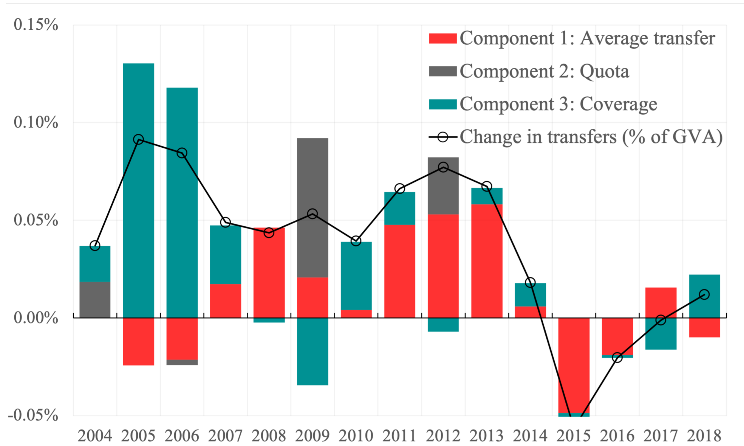
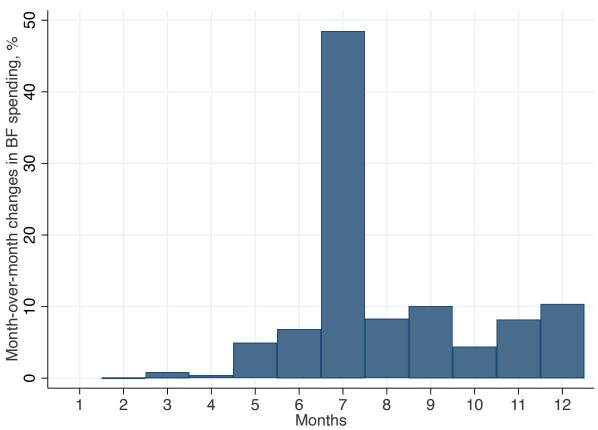


Figure A2: **Timing of Bolsa Familia**:
Average change in spending across states for each month

(a) 2004



(b) Average 2005-2019,

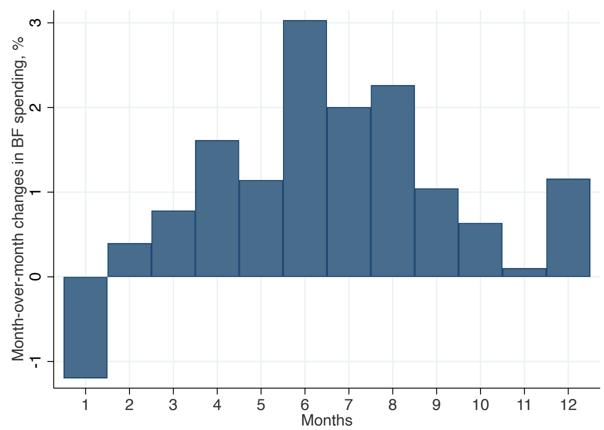
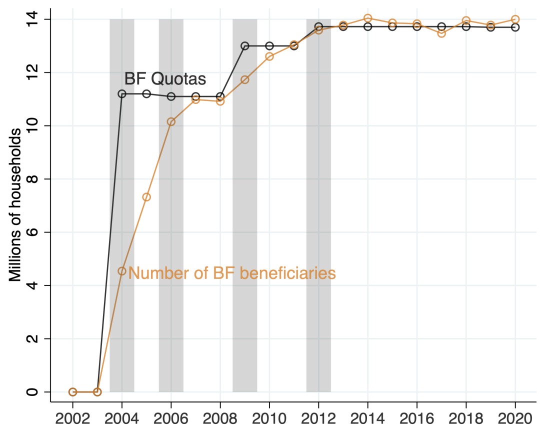
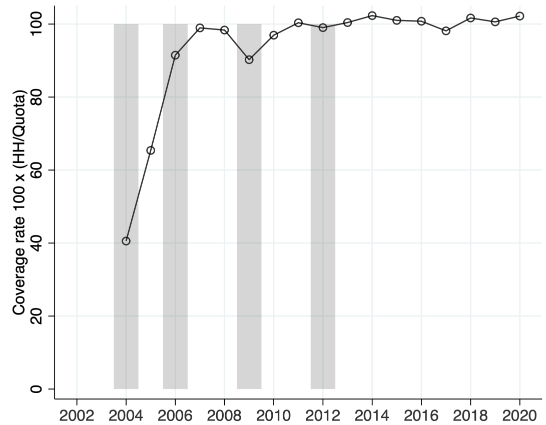


Figure A3: **Decomposition of the National BF: Quota, Coverage, and Average benefits**

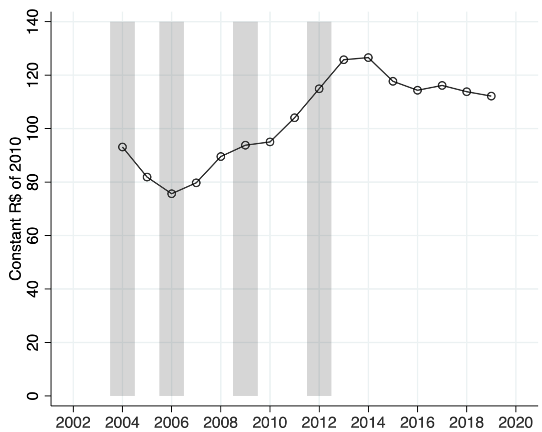
(a) Quotas



(b) Coverage rate

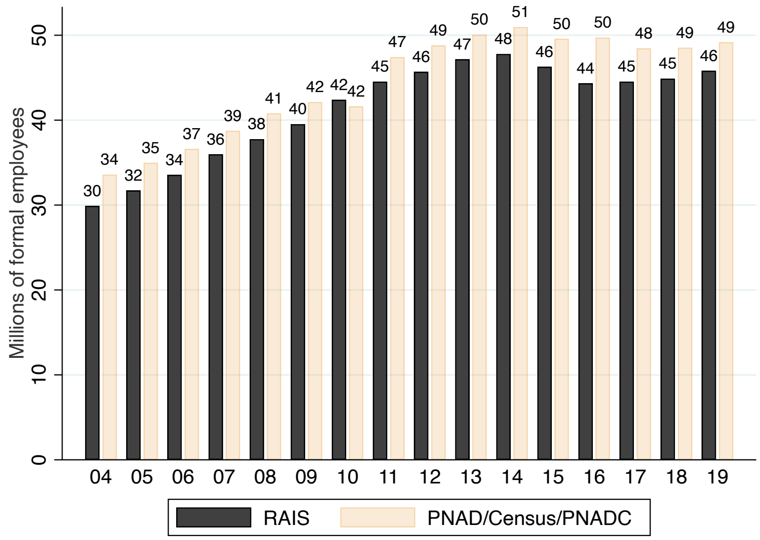


(c) Average monthly benefit

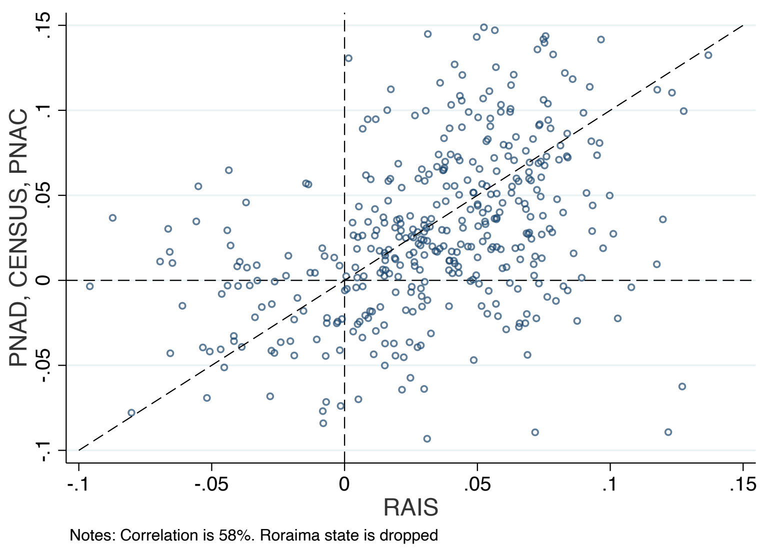


Notes: Figure A3(a) plots the evolution of national quotas, Figure A3(b) the coverage rate, and Figure A3(c) the average benefit per household. *Data source*: Author's calculations based on data from the Ministry of Social Development.

Figure A4: **Formal Employment Data Comparison: RAIS versus PNAD**



(a) Total Brazilian formal employment



(b) State-level growth rates of formal employment

Figure A5: **Alternative Calibration of the NK model**

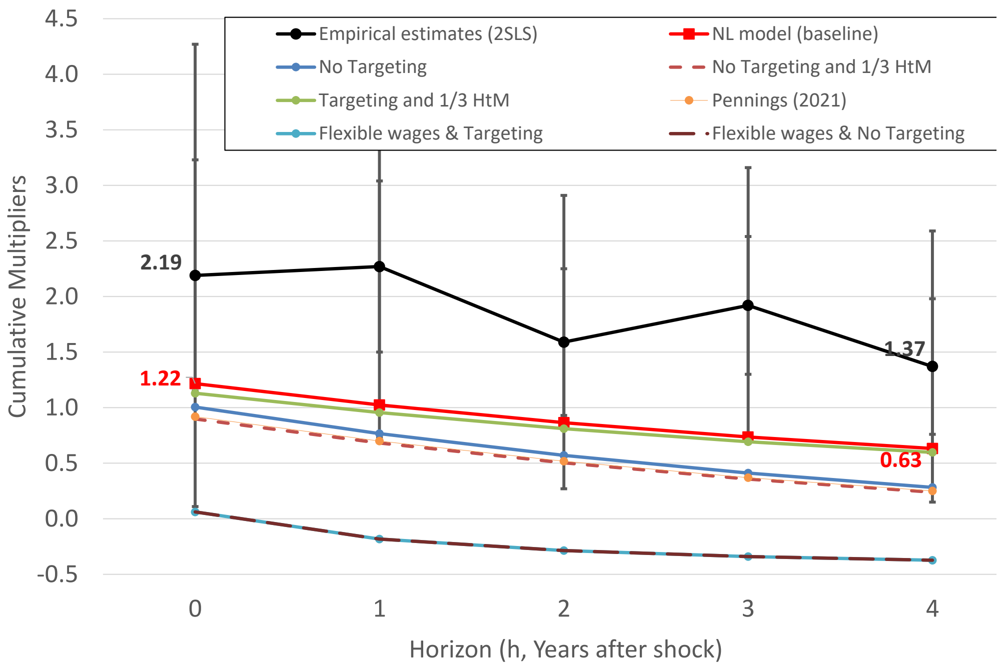


Table A1: **Sectoral decomposition of Gross Value Added**:
Percent of VA, average 2003-2019

	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%

Table A2: **Descriptive Statistics: Informal Employment in 2010**

	A. The size of informality			**B. Decomposition of Informality** (Type of informal emp. as % of total informality)			
	Millions*	**% of employment**					
		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 A3: **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 A4: **Relative Sectoral Multipliers**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	**Services**		**Construction**		**Agriculture & mining**		**Manufacturing**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
GDP Multiplier	-0.40	0.25	1.12***	0.86**	-0.53	-0.75	-0.53	-0.75
	(0.65)	(0.75)	(0.29)	(0.33)	(0.56)	(0.66)	(0.56)	(0.66)
State & year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	415	415	403	403	403	403	403	403
F statistics		60		43		38		38

Table A5: **Sectoral Jobs Multipliers**
(Jobs per R$100,000 by sector and type employment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sector	**Formal employment**		**Informal employment**		**Total employment**		**FWE**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Manufacturing	0.28***	0.46***	0.31	1.00	-0.50	-0.29	0.22	0.26
	(0.11)	(0.13)	(0.54)	(0.63)	(0.63)	(0.74)	(0.36)	(0.48)
Construction	0.98***	0.95***	1.99**	3.03***	3.02***	4.03***	2.09***	2.57***
	(0.31)	(0.32)	(0.89)	(1.10)	(0.95)	(1.16)	(0.73)	(0.87)
Services (non construction)	2.39**	2.66***	6.77***	8.46***	5.08*	5.54*	5.27***	5.56***
	(0.96)	(1.00)	(2.20)	(2.79)	(2.59)	(2.94)	(1.82)	(1.95)

Notes: The numbers in parentheses are robust standard errors. FWE is Formal Wage Equivalent employment, where jobs are weighted by their income relative to the jobs in the formal sector as defined in the main text.

Table A6: **Formal Employment Results: RAIS data vs. PNAD data.**

Dependent variable:	Benchmark (RAIS)		PNAD	
	OLS	2SLS	OLS	2SLS
Jobs per R$100K	2.75***	3.02***	4.25*	3.66
("Jobs multiplier")	(1.02)	(1.08)	(2.21)	(2.60)
State & Year FEs	YES	YES	YES	YES
Observations	408	408	404	404
F statistics		53		42

Table A7: **Informal employment multiplier: More details**

	(1)	(2)	(3)	(4)
Dependent variable:	**Informal Employee**		**Informal Self-Employed**	
	OLS	**2SLS**	**OLS**	**2SLS**
Jobs per R$100,000	2.79	5.95***	1.95	7.50**
("Jobs Multiplier")	(1.72)	(1.89)	(2.43)	(2.93)
State & year fixed effects	YES	YES	YES	YES
Observations	402	402	416	416
F statistics		56		45

Table A8: **Robustness: Different deflators for BF transfers**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BF deflated by:	**CPI (Baseline)**		**National INPC**		**State GDP deflator**		**National GDP deflator**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Multiplier	1.65*	2.19**	1.77**	2.33**	2.38***	1.87*	1.66*	2.48**
	(0.90)	(1.04)	(0.89)	(1.03)	(0.70)	(1.02)	(0.98)	(1.17)
State & year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	410	410	410	410	409	409	409	409
F statistics		58		60		20		35

Table A9: **Robustness: Timing of BF changes**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Output growth			Formal employment			Informal employment		
	Benchmark	Contemp + lag	Two-year changes	Benchmark	Contemp + lag	Two-year changes	Benchmark	Contemp + lag	Two-year changes
Multiplier	2.19**	3.05***	2.06***	3.02***	3.6***	2.41**	8.72***	8.6**	9.65***
	(1.04)	(1.12)	(0.75)	(1.08)	(1.21)	(1.03)	(3.07)	(3.57)	(2.08)
Observations	410	409	411	408	408	407	406	406	411
First-stage F statistics	58	7.5	31	53	5.8	85	50	5.2	74

	(10)	(11)	(12)	(13)	(14)	(15)
	Total employment			Formal-wage equivalent		
	Benchmark	Contemp + lag	Two-year changes	Benchmark	Contemp + lag	Two-year changes
Multiplier	12.14***	13.72***	7.73***	5.38**	6.16***	4.72***
	(2.96)	(3.31)	(1.80)	(2.29)	(2.52)	(1.62)
Observations	399	400	405	401	401	404
First-stage F statistics	44	5.2	78	49	4.6	89

Notes: Columns (1) to (3) display the 2SLS estimation for output multiplier, where column (1) presents our baseline estimate, column (2) shows the sum of the coefficients on contemporaneous BF transfer changes and its lag (dynamic specification), and column (3) shows the estimate when we regress two-year output changes on two-year BF transfer changes. The rest of the tables show the results for formal, informal, total and FWE employments.

Table A10: **Multipliers dropping years, states, and regions one-by-one. The estimates are from the 2SLS specification.**

	(1)	(2)	(3)
	A. Dropping years one-bye-one		
	GDP multiplier	Formal jobs multiplier	Informal jobs multiplier
Benchmark	2.19**	3.02***	8.72***
	(1.04)	(1.08)	(3.07)
Dropping:			
2004	2.20**	3.05***	8.74***
	(1.05)	(1.09)	(3.11)
2005	1.98*	2.19*	5.84**
	(1.04)	(1.13)	(2.75)
2006	2.21**	2.64**	8.22**
	(1.12)	(1.19)	(3.26)
2007	2.31**	2.99***	8.76***
	(1.06)	(1.11)	(3.11)
2008	2.23**	2.84**	8.65***
	(1.05)	(1.10)	(3.05)
2009	2.11**	3.02***	9.76***
	(1.07)	(1.08)	(3.14)
2010	2.24**	2.99***	8.76***
	(1.04)	(1.10)	(3.09)
2011	2.30**	3.09***	9.36***
	(1.05)	(1.10)	(3.17)
2012	1.21	4.07***	7.22**
	(1.06)	(1.14)	(3.12)
2013	2.54**	2.98***	10.41***
	(1.10)	(1.12)	(3.21)
2014	2.44**	3.22***	9.71***
	(1.06)	(1.08)	(3.18)
2015	3.90***	3.86***	7.95*
	(1.32)	(1.40)	(4.18)
2016	1.86*	2.81**	6.96**
	(1.13)	(1.16)	(3.04)
2017	2.22**	3.42***	8.01***
	(1.06)	(1.10)	(3.05)
2018	1.99*	2.75**	9.03***
	(1.06)	(1.08)	(3.16)
2019	1.73∗	2.38**	11.44***
	(1.06)	(1.08)	(3.52)
2012 & 2015	2.64**		
	(1.33)		

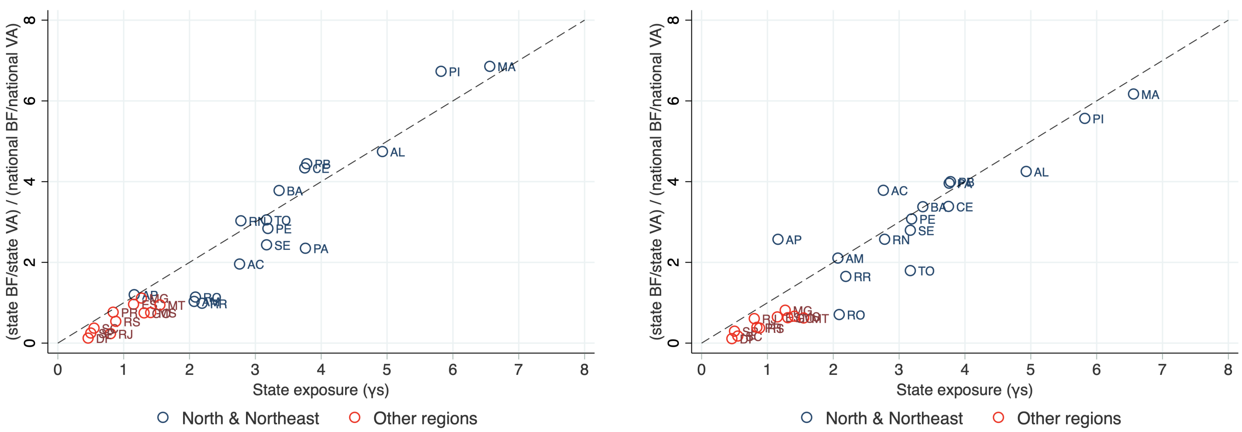
	(4)	(5)	(6)
	B. Dropping states one-bye-one		
	GDP multiplier	Formal jobs multiplier	Informal jobs multiplier
Benchmark	2.19**	3.02***	8.72***
	(1.04)	(1.08)	(3.07)
Dropping:			
RO	2.22**	2.99***	8.73***
	(1.04)	(1.08)	(3.06)
AC	2.19**	2.99***	8.87***
	(1.04)	(1.08)	(3.06)
AM	2.18**	3.02***	8.56***
	(1.04)	(1.08)	(3.11)
PR	2.20**	3.27***	8.35***
	(1.04)	(1.09)	(3.15)
PA	2.07**	3.02***	8.84***
	(1.05)	(1.08)	(3.20)
AP	2.35**	3.11***	9.22***
	(1.06)	(1.07)	(3.13)
TO	2.14**	2.94***	9.01***
	(1.05)	(1.08)	(3.06)
MA	2.22*	3.51**	12.28***
	(1.27)	(1.65)	(3.57)
PI	2.71**	3.69***	8.38***
	(1.09)	(1.10)	(3.21)
CE	2.21**	2.97***	8.52***
	(1.06)	(1.08)	(3.09)
RN	2.22**	3.03***	8.69***
	(1.05)	(1.09)	(3.07)
RO	2.06*	2.88***	8.38***
	(1.05)	(1.08)	(3.10)
PE	2.06**	2.80***	8.67***
	(1.04)	(1.08)	(3.12)
AL	2.44**	3.19***	8.41***
	(1.11)	(1.09)	(3.12)
SE	2.32**	2.96***	8.93***
	(1.05)	(1.08)	(3.06)
BA	2.17**	3.15***	7.61**
	(1.05)	(1.09)	(3.06)
MG	2.11**	2.70**	8.50***
	(1.06)	(1.09)	(3.09)
ES	2.24**	3.16***	8.84***
	(1.05)	(1.11)	(3.13)
RJ	2.39**	3.02***	8.03***
	(1.08)	(1.12)	(3.06)
SP	2.42**	2.81**	8.78***
	(1.10)	(1.14)	(3.22)
PR	2.02*	2.88***	8.69***
	(1.06)	(1.08)	(3.12)
SC	1.58	3.03***	9.13***
	(1.06)	(1.13)	(3.20)
RS	1.98*	2.65**	9.10***
	(1.04)	(1.11)	(3.18)
MS	2.25**	3.06***	8.57***
	(1.06)	(1.10)	(3.03)
MT	2.31**	2.89***	8.15***
	(1.05)	(1.08)	(3.01)
GO	2.32**	3.34***	8.41***
	(1.06)	(1.10)	(3.00)
DF	1.94*	2.54**	9.03***

	(7)	(8)	(9)
	C. Dropping regions one-bye-one		
	GDP multiplier	Formal jobs multiplier	Informal jobs multiplier
Benchmark	2.19**	3.02***	8.72***
	(1.04)	(1.08)	(3.07)
Dropping:			
Northeast	5.40**	5.25**	7.06
	(2.26)	(2.27)	(4.95)
North	2.18**	3.17***	9.26***
	(1.06)	(1.06)	(3.41)
Southeast	2.66**	2.54**	7.75**
	(1.20)	(1.24)	(3.34)
South	0.98	2.45**	9.65***
	(1.07)	(1.17)	(3.45)
Midwest	2.29**	2.78**	7.69**
	(1.11)	(1.14)	(3.01)
Northeast and South	2.95		
	(2.28)		

Notes (∗) In column (1), the coefficient on 2019 has a p-value of 0.104.

Figure A6: **Alternative IVs**:
Estimated state exposure (γ_s) (X-axis) versus calibrated Bolsa Familia shares (Y-axis)

Panel A. 2003 shares **Panel B. 2019 shares**



Notes: VA excludes agriculture and mining. Correlation is 0.96 in Panel A and 0.94 in Panel B.

Intensive and extensive margins

Changes in BF transfers to a state's residents can be either at the intensive or extensive margin. For intensive margin changes involve increasing the size of benefits to existing BF households, whereas for the extensive-margin changes they 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 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 A11 suggest that multipliers on extensive margin changes are below 1 and insignificant, whereas intensive margin changes are responsible for the large multipliers in Table 2. Note that the 2SLS coefficient of 1.92 for intensive margin is marginally significant with a p-value=0.108.

Table A11: **Decomposing multiplier: Intensive and Extensive Margins**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	**Benchmark**		**Intensive margin**		**Extensive margin**	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
Multiplier	1.65*	2.19**	2.77***	1.92	1.24	1.02
	(0.90)	(1.04)	(1.04)	(1.19)	(1.11)	(1.15)
CONTROLS	NO	NO	NO	NO	NO	NO
State & Year FEs	YES	YES	YES	YES	YES	YES
Observations	410	410	383	383	384	384
First-stage statistics		58		96.67		66.42