

Does Tax Debt Relief Stimulate Employment?

Many governments offer incentives or benefits for businesses – some form of subsidy or exemption – in the expectation of corresponding social or economic returns. Yet we know little about the impact of these controversial and often hidden ‘tax expenditures’. This study contributes to filling this gap by analyzing a Brazilian government ‘fiscal recuperation’ policy (referred to as ‘Refis’), which authorities justify on the basis of boosting employment. In the backdrop of high unemployment, the Brazilian federal government has offered nearly 40 Refis to defer and forgive tax debt since the year 2000. The first part of this article analyzes the Refis as a public policy through the prism of the policy cycle. The second part gauges the effects of the Refis on job creation or maintenance. Using a dataset of approximately 10,000 businesses, we match and then compare similar businesses that did and did not subscribe to a 2014 Refis program. Our results reveal a policy lacking adequate transparency and due process, which exerted a strong negative impact on employment. On average, businesses that participated in the 2014 Refis lost 6% more jobs than non-participants. These results echo much of the literature in enjoining governments to reevaluate the use of tax debt relief policies.

Do tax debt relief policies boost employment? Whether framed as tax breaks, amnesties or tax relief, these policies constitute a controversial category of ‘tax expenditures’ (Surrey, 1973), defined as revenue losses incurred by government subsidies or exemptions, including credits, tax relief or breaks and other forms of deferred or forfeited income.

Many countries offer some form of tax break to boost employment or stimulate economic growth, but very little is known about their impact. Ditto for Brazil, which is in the midst of reforming a tax system riddled with inconspicuous tax expenditures of relatively unknown impact. Brazil’s ‘fiscal recuperation’ policies (*recuperação fiscal* or simply ‘Refis’) are particularly significant. As of 2019, Brazil’s 2017 fiscal recuperation program¹ provided businesses and individuals with more than R\$39.2 billion (US\$10.4 billion) of tax relief on arrears.² At the time,³ this sum was 20% more than the price tag of Brazil’s seminal anti-poverty program, *Bolsa Família* (Brasil, Governo Federal, 2019; Fenwick, 2015). Refis policies imply that government authorities:

a) refrain from penalizing or prosecuting tax delinquency, b) forgive tax arrears⁴ in whole or in part,

¹ See law no. 13496/2017.

² Information obtained via freedom of information request no. 03006.004083/2019-62.

³ The foregone revenue associated with tax debt relief varies each year and depends on how many participants pay the whole installment plan. The participants who are not meeting the installment plan payments are excluded from the program and lose all the tax benefits.

⁴ Defined as unpaid and overdue taxes.

and c) provide favorable repayment plans, including low interest rates and long amortization periods.

This study questions the democratic standing and economic and social impact of these redistributive policies. In so doing, we contribute to an embryonic literature on the relative costs and benefits of one of two dimensions of tax expenditures – those policies that provide exemptions or tax breaks, as opposed to subsidies.

Our focus is on a chronic source of concern in Brazil – employment. The World Bank puts average global unemployment at 5.8% (2022), the highest rate since 1995 occurring during the pandemic, at 6.9% in 2020. By contrast, Brazil's lowest unemployment rate since 1995 was 6.8% (2014), with unemployment hovering above 10% from 2016 to 2022 (15% in 2020) (Brasil, Instituto Brasileiro de Geografia e Estatística, 2023a). Add to this an estimated 18 million underemployed citizens – those who seek full time jobs but work less – and Brazil's employment predicament becomes even more acute (Brasil, Instituto Brasileiro de Geografia e Estatística, 2023b). Given these figures, it should come as no surprise that Brazilian authorities justify many public policies based on employment rationales. This raises an important question, namely, the extent to which 'employment rationales' are justified for specific policies, especially when those policies aim to benefit particular economic sectors or demographics (Paci et al., 2012).

Although the public benefits of forgiving tax arrears remain unclear, the risks are patent. On the one hand, such policies may foster tax noncompliance, with businesses renegeing or continuously rolling over debts while finagling further tax debt relief policies. On the other hand, considerations of social and economic opportunity costs may counsel against such investments. Across Latin America, annual tax expenditures are about equal to public education budgets, approximately 4% of GDP or between 10% and 20% of total government revenue (de Renzio, 2019a). Might these funds be better collected than forfeited, invested in policies that reduce poverty and inequality?

Answers to this question are hard to find, primarily because data on tax relief policies suffer from neither-here-nor-there regulatory and reporting guidelines. Unlike direct expenditures such as

personnel or procurement, tax debt relief policies do not fit into ‘standard’ reporting silos and disclosure routines. As a result of their ‘irregular’ character, these policies tend to exhibit poor transparency. Granular data on policy details and beneficiaries of tax debt relief policies are frequently protected by secrecy and privacy laws (Fundar Centro de Análisis e Investigación, 2016). Unsurprisingly, the International Budget Partnership (Petrie et al., 2014) refers to the entire realm of tax expenditures as “Hidden Corners of Public Finance”.

Motivated by the uncertain policy salience, democratic standing, and regulation of these policies, the first part of this article analyzes tax debt relief as a public policy through the prism of the policy cycle. Examining the formulation, adoption, implementation, and oversight of Brazilian tax debt relief policies, our analysis reveals that Brazil’s *Refis* tax relief programs lack the due process and transparency expected of important fiscal instruments. For example, they are invariably introduced as urgent executive orders (*medida provisória*), bereft of legislative transparency and public deliberation.

The second part of this article features a quasi-experimental research design to gauge the impact of tax debt relief policies on employment. Focusing on a 2014 Brazilian *Refis* of approximately R\$9.4 billion (US\$ 3.5 billion at the time), we compare employment trends three years following the policy’s uptake by matching approximately 10,000 beneficiary businesses with similar non-beneficiary businesses. Our results show statistically significant negative effects on employment. On average, companies that participated in the *Refis* lost 6% more jobs than non-participating companies.

Although causal identification is outside the scope of this study, the most compelling hypothesis has to do with Brazil’s unfavorable business environment (Agência Brasil, 2019). We surmise that businesses are either investing tax savings in assets that yield higher returns than what otherwise might be obtained by reinvesting in businesses; or they use *Refis* to clear their accounts before beginning to shut down their businesses. Either way, the *Refis* appears to offer no apparent public interest benefits.

Tax Debt Relief as Public Policy

Comparing tax systems worldwide is complex due to the existence of diverse frameworks (Brixl et al., 2004; European Commission, 2014; Villela et al., 2010). This is especially true of tax expenditures (European Commission, 2014; International Monetary Fund, 2018; Kassim, L. & Mansour, M., 2018; Longinotti, 2019; Organisation for Economic Co-operation and Development (OECD), 2010; Redonda & Neubig, 2018). Tax expenditures can be included within a group of tax exceptions divided into tax incentives and tax benefits (Longinotti, 2021). Incentives are used to attract investment, promote employment, and spur regional development, among other purposes. Benefits are typically justified on the grounds that they serve a social purpose. Both incentives and benefits imply a renunciation of tax revenue, benefiting specific taxpayers.

The IMF defines a tax debt relief program or tax amnesty “as a limited time offer by the government to a specified group of taxpayers to pay a certain amount, in exchange for forgiveness of a tax liability (including interest and penalties), relating to a previous tax period(s), as well as freedom from legal prosecution” (Baer & Le Borgne, 2008, p. 5). Although such policies are sometimes offered in periods of economic crisis to forgive tax noncompliance and quickly raise revenue, there are other rationales: to repatriate capital lost to flight or evasion, to decrease the stock of tax arrears,⁵ attract investment, promote employment or, alternatively, to satisfy interest group demands for forgiveness of existing tax debts. Tax relief in the form of ‘amnesties’ can be exemplified, for instance, by the experiences of Indonesia (Nuryanah & Gunawan, 2022) and India (Das-Gupta & Mookherjee, 1995). By contrast, Brazil’s Refis programs are more akin to those in Mexico (Fundar Centro de Análisis e Investigación, 2016) and Argentina (Asociación Civil por la Igualdad y la Justicia, 2018; Sanchez Villalba, 2017). The distinction between types of tax relief is important, because if the government knows the identity of debtors, the amount of their tax arrears

⁵ According to the IMF (Rehm & Parry, 2007), it is essential that countries report the stock of tax arrears that can effectively be collected, writing off bad debts. In this way, it is possible to assess the amount of tax debt collection that a government can expect.

and, most importantly, their capacity to pay, those who can pay should not be benefiting from relief in the first place. However, tax relief policies that aim to diminish the stock of tax arrears, which abound in Brazil, have not historically distinguished between those who can and cannot pay.

The ‘indirect’ and variegated ways in which tax expenditures are alleged to produce public goods (Redonda, 2020) render them a ‘grey’ area in both national accounting systems and among the fiscal regulatory frameworks of multilateral organizations, such as the International Monetary Fund (IMF), the Organisation of Economic Cooperation and Development (OECD), and the World Bank. According to the Interamerican Center of Tax Administration, tax expenditures lack standardized formats for reporting (Longinotti, 2019), which render policy appraisals difficult (de Renzio, 2019a; Kassim, L. & Mansour, M., 2018; Redonda, 2020). Muddying the waters even further, tax debt reliefs tend not to be reported in tax expenditure reports, despite having similar effects. In this sense, they remain ‘an untamed fiscal frontier’. A key cause and consequence is weak transparency. Nongovernmental organizations in Mexico have strived to increase the transparency of tax debt relief policies in this country (Fundar Centro de Análisis e Investigación, 2016); however, it remains unclear which other countries, apart from Brazil, are making use of similar policies to address tax arrears. What is clear is that tax debt relief policies stand at odds with multilateral fiscal initiatives to promote transparent reporting, shed light on the iniquitous distributions of state resources, and diminish corruption (Global Initiative for Fiscal Transparency, 2022).

In terms of impact, the literature tends to be pessimistic on the question of how tax debt relief policies or tax amnesties change taxpayer behavior and government revenue. The IMF has found that, even during crises, the use of such policies is counterproductive; they do not raise revenue in the long term and repeated iterations of relief programs negatively influence tax compliance (Baer & Le Borgne, 2008; Brondolo, 2009). Non-compliance is fostered by participants anticipating the re-introduction of new programs (Alm, 1998; Mikesell & Ross, 2012; Shevlin et al., 2017). On the government side of the equation, such policies are thought to lower the capacity and

willingness to apply tax sanctions. Unsurprisingly then, repeated tax debt relief programs and amnesties are associated with financial reporting irregularities (Buckwalter et al., 2014) and exert negative peer effects on the compliance of otherwise faithful taxpayers, creating something of a snowball effect (Castro & Scartascini, 2019; Sanchez Villalba, 2017). Finally, these policies may serve as a palliative, postponing efforts to fix broken tax and regulatory systems.

The Brazilian “Refis”

Brazil’s tax structure is complex. The country features more than 80 different taxes, most are indirect and “contribute little to reducing inequality” (Brockmeyer, 2018). With taxes at 31.6% of GDP in 2020, Brazil extracts nearly a third more tax revenue than the Latin American average (23% in 2019). And – albeit from much poorer citizens – its tax take is nearly equal to the OECD average (33.5%) (Organisation for Economic Co-operation and Development, 2020). These are heavily regressive, as approximately three-fifths of Brazilians are too poor for income tax eligibility. Personal and corporate income taxes in Brazil make up approximately 3.0% and 2.6% of GDP, respectively, whereas value-added taxes account for 7.1% of GDP (Brasil, Receita Federal do Brasil, 2019). Within this context, tax expenditures represent a significant piece of Brazil’s fiscal pie — 4.4% of GDP in 2020 (Redonda, A., von Haldenwang, C., & Aliu, F., 2022) – almost twice the value of corporate taxes. Furthermore, the Refis programs are not tabulated as tax expenditures, which means that Brazil’s numbers on tax expenditures should be significantly higher. Brazil’s last Refis program, for example, which involved 252,000 taxpayers (2017), forfeited revenue (as of 2019) equivalent to 0.56% of GDP.

The regulatory standing of tax expenditures appears to be a point of disagreement among Brazilian governmental institutions. The Brazilian Revenue Agency (*Receita Federal do Brasil*) provides a table on its website depicting “all alterations of tax legislation that negatively impact public revenue, independent of whether they are subsumed by the concept of tax expenditure” (Brasil, Receita Federal do Brasil, 2020b). However, the revenue foregone by tax debt relief programs is not included in this table (Brasil, Receita Federal do Brasil, 2020a). Rather, it is treated

as a separate item, as though it were not considered a tax expenditure at all or did not “negatively impact public revenue”. Although Brazil’s National Treasury (*Secretaria do Tesouro Nacional*) considers debt relief a tax expenditure (2019), the Auditing Courts (*Tribunal de Contas da União*) responsible for approving the federal government’s accounts classify them as “actions to recuperate credit/funds” (*ações de recuperação de créditos*)”.⁶ This classification implicitly assumes tax debt relief policies are needed to collect debts. At best, this is a doubtful presumption; at worst, it casts doubt on the efficacy of tax enforcement in Brazil (Campos, 2018). In summary, regulatory confusion surrounding the status of Refis tax debt relief programs enfeebles the very basis of these ‘public policies’.

Consequently, the nebulous regulatory environment surrounding these policies has had predictable effects on their transparency. Not only is there little public documentation on tax debt relief or amnesties, but extensive data on beneficiaries are concealed behind a wall of financial, commercial, and personal privacy and secrecy statutes. Opacity renders beneficiaries and administrators difficult to “name and shame”, and thus these programs receive less attention than they deserve (Khagram et al., 2013, p. 41). This scenario changed in 2021, after an amendment to Brazil’s Fiscal Code (*Código Tributário Nacional*) permitted the disclosure of foregone revenue received by legal entities.⁷ Yet news media coverage tends to focus on aggregate sums of public money disbursed as tax benefits rather than their distribution— and those responsible for giving and taking (Cejudo, 2012). In the upcoming section, we analyze what information exists, using the policy cycle to shed light on tax debt relief as public policy.

Examining Refis via the Policy Cycle

The policy basis of tax debt relief, subsidies and other forms of incentives and benefits originates in Article 150 § 6º of the Brazilian Constitution, which mandates that “specific laws” be legislated for their execution. The Constitution does not delineate what laws should contain to be considered legal or legitimate. Both the executive and legislative branches of government have the

⁶ See item 4.1.2.10 from Acórdão 1320/2017 (Brasil. Tribunal de Contas da União, 2017).

⁷ See law no. 5172/1966, art. 198, §3º, IV.

power to initiate legislation on revenue in Brazil (Correia Neto, 2016) and thus one might expect to see legislative or executive *agenda-setting* initiatives, such as consultations regarding the passage of Refis programs. However, tax debt relief has not been given public salience and does not benefit from the news media coverage and public attention allotted to core budgetary processes (de Renzio, 2019b; The Pew Charitable Trusts, 2017).

With respect to the *formulation* or design of these policies, Brazil's Refis programs deviate from international norms and basic common sense in important ways. The IMF recommends that tax arrears should be paid back via installment plans limited to two years or less— if tax debt relief is to occur at all (Baer & Le Borgne, 2008; Brondolo, 2009). These recommendations appear to be generally heeded across the OECD (2014); a study of 27 countries finds that most countries offer from 12 to 24-month terms for the payment of debts, conditional on some sort of collateral. However, Brazil deviates in true *jabuticaba*⁸ style. Regular installment plans offer 5 years for the payment of debts; with Refis, they can be extended to 15 or even 20 years (O Globo, 2014) and there is no mandate that debtors provide collateral.

The design of Refis programs is also lacking in consistency. Some are open to any taxpayer, others, to specific taxpayers such as financial institutions or sports-related entities. This design inconsistency seems to suggest that Refis programs respond to particularistic interests. Furthermore, the more than 40 Refis that have occurred to date (Brasil, Receita Federal do Brasil, 2017) have consistently been justified based on “economic crisis”. It is hardly possible that all Refis have occurred during crises, which lends credence to notions of vested interests; once a tax benefit has been introduced, beneficiaries seek to maintain the *status quo* (Schneider, 2013).

Previously, the Refis relief programs admitted all comers. However, law 13988/2020 (converting MP no. 899/2019) now stipulates that benefits will go to taxpayers who cannot pay their tax arrears, similar to “Offer in Compromise” policies adopted in the United States. These benefits are standardized depending upon the size of business and do not admit debtors who can pay

⁸ A “jabuticaba” is a type of fruit said only to exist in Brazil. Likening something to a jabuticaba is to say that it is distinct or unique to Brazil.

their arrears. This policy is recent and its impact merits analysis. However, it appears to address only one among several ostensible policy design flaws embedded in the Refis. Moreover, as described below, legislators have written in exceptions to previous Refis and there is no guarantee that they will not do the same to nullify the effects of law 13988.

In terms of the *adoption* of tax relief policies, the passage of Refis has followed a predictable unidirectional pattern: the executive branch has invariably sent an urgent executive order (*medida provisória*) to Congress, which expires within 60 days. In practice, either Congress has amended these urgent executive orders to detail and expand a tax relief's scope, or legislatures have appended a tax relief inside an urgent executive order where none previously existed. In the latter case, Congress effectively creates a tax debt relief 'earmark' on top of an unrelated bill. Critically, in neither case are legal measures allotted the legislative deliberation and public consultation expected of such an important fiscal instrument. Quite to the contrary, these executive orders are railroad through the legislative process in line with their "urgent" legislative regimes (60 days).

Several potential illegalities mar this adoption process. First, this last strategy, of effectively creating an omnibus-style bill by grafting a tax debt relief provision onto an unrelated bill, transgresses Article 150 § 6° of the Brazilian Constitution, which requires a "specific law" to introduce a tax measure (Correia Neto, 2016). A second potential problem relates to previous Refis beneficiaries entering new programs. Although new Refis laws include explicit prohibitions for reentry by previous beneficiaries, legislators usually apply an exception to this rule.⁹ Third, Congress approves Refis even though they are noncompliant with the Fiscal Responsibility Law (LRF): they omit fiscal and budgetary impact estimates and justifications for forfeited revenue. In sum, one can hardly call the adoption process of Refis anything but anomalous; it is a top-down, expedited initiative, replete with potential illegalities, and lacking the sort of legislative deliberation and public consultation owed to any large public investment. This assessment is shared by Brazil's

⁹ See law no. 13496/2017, art. 1º, § 4º, IV and art. 11, § 1º.

Audit Courts (similar to the Congressional Budget Office in the United States) (Brasil, Tribunal de Contas da União, 2018).

The *implementation* of tax debt relief illustrates how formulation and adoption processes culminate in perverse incentives. Conflicts of interest abound. Dozens of senators, deputies and their families benefit from tax debt relief (BBC Brazil, 2019; Gazeta do Povo, 2017; VEJA, 2017). In 2017, for example, 81 federal legislators benefited from R\$138.6 million (US\$43 million) of tax arrears forgiven by the Refis (Folha de São Paulo, 2018). Although cumulative numbers on tax forgiveness are difficult to come by, examples of benefits abound. The National Treasury's largest debtor, Petrobras, posted tax arrears valued at R\$6,569,688,109.46 (around US\$2 billion) in the 2017 Refis.¹⁰ This debtor benefited from a reduction in penalties levied (50%), reduced interest rates (80%), and avoided legal expenses that would have otherwise been incurred in negotiating debt. As a result, Petrobras benefited from R\$2,961,241,539.01 (US\$925 million at that time) in forfeited taxes – nearly half the debt in question.¹¹ The company also boasts an installment plan of 145 months for paying outstanding debt. As tax debt relief programs in Brazil apply reductions in terms of percentages, it follows that the greater the debt, the greater the amount of debt forgiveness.

Concerning the *oversight* of tax debt relief in Brazil, enforcement is difficult to gauge, as there is little public information on processes of auditing or penalties. What the data do show, however, is that the treasury tends to experience initial inflows of recuperated taxes, as a high number of taxpayers sign on to the Refis (Paes, 2014). However, very few of these taxpayers adhere to the Refis and effectively pay their debts (Paes, 2014). Since the year 2000, approximately 90% of tax arrears registered in each Refis have gone unpaid (Brasil, Receita Federal do Brasil, 2017; Morais et al., 2011).

The main problem appears to be what Faber and Silva (2016) call the “expectation effect”. Certain that a new program will be forthcoming, beneficiaries forestall payment (Brasil, Receita Federal do Brasil, 2017). During the last large Refis in 2017, 36% of participating businesses had

¹⁰ Law no. 13496/2017.

¹¹ Information obtained via freedom of information request no. 03005.096625/2023-10.

already benefited from two or more debt relief programs. Moreover, fully 83% of tax arrears in the 2017 tax relief came from taxpayers deemed to have a high or medium capacity to pay tax arrears (Brasil, Procuradoria-Geral da Fazenda Nacional, 2018). The auditing arms of the Brazilian state appear to concur that Refis furnish strong incentives for opportunistic entry and re-entry, fostering abuses and tax delinquency (Brasil, Tribunal de Contas da União, 2018).

According to the Brazilian revenue agency (Brasil, Receita Federal do Brasil, 2017), more than two thousand taxpayers with annual revenues of more than R\$150 million (US\$40 million at the time) have already participated in three or more tax debt relief programs. When a Refis is enacted, many beneficiaries clear their accounts with the tax authorities (Brasil, Controladoria-Geral da União, 2016). In other words, many beneficiaries should never have been admitted. The IMF policy is clear in this respect: it is necessary to adopt coercive measures to ensure that those who have the capacity to pay, do so (Brondolo, 2009). As discussed earlier, a 2020 law (13988) may be helping to address the issue of (re)entry by businesses that are indeed able to pay their taxes. Overall, however, enforcement appears to be lacking.

To complete an analysis of the Refis policy cycle, the upcoming sections present an evaluation of this policy's impact. The preceding examination, detailing the democratic standing of the Refis as public policy, already conveys the relative value of these legal instruments. Their impact on social and economic variables, however, is perhaps the more important question.

Impact Analyses of Tax Debt Relief Policies

Evaluations on the impact of tax expenditures on economic or social variables are more common in higher income countries. In reviewing impact analyses of tax incentives on job creation for example, Hanson (2019) reviews “primarily U.S. based policies”, which present both negative and positive effects. By contrast, Latin American countries provide little to no information regarding the objectives, beneficiaries, or impacts of tax expenditures (de Renzio, 2019a). To our knowledge, no impact evaluations have been undertaken on socioeconomic variables specifically in

relation to tax debt relief programs, and thus we can only begin to theorize their merit as policies via analogy, comparison, and logic.

Most forfeitures of government revenue are predicated on the notion that they boost employment. Yet at least three challenges stand in the way of gauging effects on employment. First, any evaluation is incomplete without gauging opportunity costs: comparing the benefits of tax debt relief policies versus other possible public investments (Garsous et al., 2017). Second, impact evaluations suffer from inextricable methodological problems, including issues of endogeneity and the lack of counterfactuals. It is particularly difficult to prove that job creation would not have occurred in the absence of tax incentives (Gobillon et al., 2012; OPEGA, 2019). Third, some argue that the effectiveness of fiscal incentives is highly contingent on context, evincing larger or smaller impacts depending upon the region (Neumark & Kolko, 2010). Nevertheless,

Several takeaway points from the literature on fiscal impact merit distinction. On the one hand, and as noted by Bernini and Pellegrini (2011), evidence is frequently contradictory, lacks rigor, and contains inherent methodological shortcomings. On the other hand, although it may not be possible to assess the impact of individual tax benefits, analyses should be able to gauge general effectiveness of economic variables. Such analyses are crucial for debates on the merits of tax expenditures (de Renzio, 2019b).

In the case of the Refis, job creation or maintenance is a stated goal of these programs. Goal-related statements are inscribed on the Refis executive orders sent to congress.¹² Yet we have no idea whether the Refis create or maintain jobs. The government-funded Institute for Applied Economic Research in Brazil (IPEA - *Instituto de Pesquisa Econômica Aplicada*), has examined the *general* relationship between *tax benefits* and employment. They find no evidence of a positive effect (Garcia et al., 2018). The Brazilian Auditing Court (TCU) recently examined tax incentives for the automotive industry, finding that job creation did not compensate for the costs of forfeited

¹² See exposition of motives and legislative report of executive order no. 783/2017.

revenue (Brasil, Tribunal de Contas da União, 2023). With respect to tax debt relief policies, however, we give the executive and legislative branches the benefit of the doubt. Formally stated:

Hypothesis 1: The Refis maintains or increases the number of employees in participating businesses.

An important concern in the design of debt recuperation programs is the size of businesses included (Brasil, Tribunal de Contas da União, 2018). Small businesses with revenue of up to R\$4.8 million are responsible for more than half of Brazil's employment (SEBRAE, 2018). It therefore follows that if tax debt relief is to be offered, it may be advantageous to focus on small businesses in need (Li & Rama, 2015), particularly because nearly 70% of all tax debtors are small businesses accounting for approximately 22% of total tax arrears.¹³ As previously mentioned, however, larger businesses are those that benefit the most from such programs: the greater the debt, the greater the amount of debt forgiveness. In other words, many small businesses can be helped at comparatively smaller costs, but Brazil's tax debt relief policies are not targeted primarily at small businesses. Given these observations, our second hypothesis follows:

Hypothesis 2: The effect of Refis on employment is greater for medium and large companies than for smaller companies.

Research Design

The outcome variable of this study is employment, defined as job creation or maintenance, which is not only the stated objective of many tax relief programs in Brazil but also the most salient public-interest benefit.

In selecting programs to be evaluated, several critical issues had to be addressed. The last major Refis occurred in 2017. However, a labor reform passed in 2017 (Law no. 13467/2017) that implied changes in labor relations (and employment). Therefore, we chose to analyze the impact of

¹³ Information obtained via a freedom of information request (nº 03005.085225/2023-71).

the penultimate tax relief (2014) on job creation or maintenance during the years 2015, 2016 and 2017, until the labor reform took effect in November of this last year.

We did not include the years subsequent to 2017 (2018 to 2023) in the analysis for two reasons. First, we only had employment data available until 2017. Furthermore, beginning in February 2018, participants who were not meeting the installment plan payments were excluded from the program. This exclusion could have interfered in the analysis of the program as a whole: from 2018 until the beginning of 2023, 62% of the businesses are not participating in the program anymore.

Data and sample

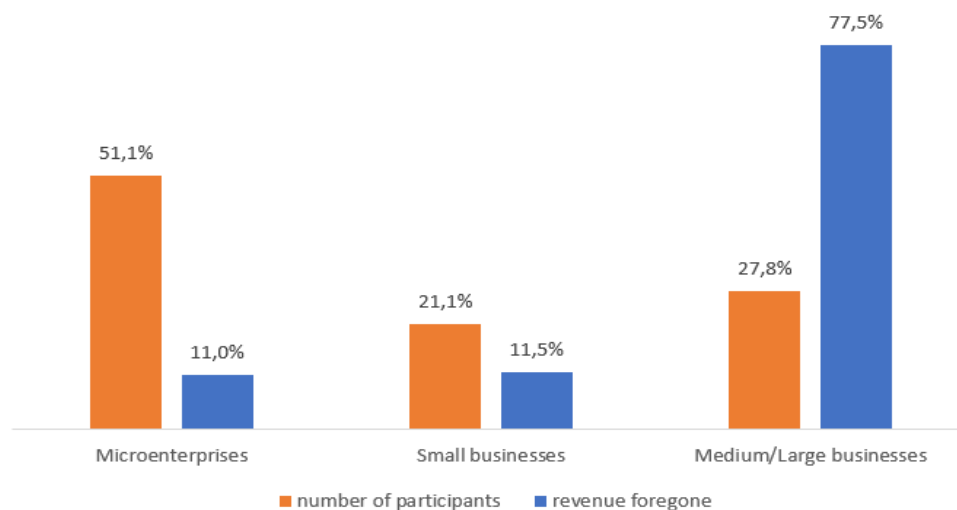
Federal tax debt is managed by the Federal Revenue Service (*Receita Federal do Brasil*) and The National Treasury Attorney's Office (*Procuradoria-Geral da Fazenda Nacional* or PGFN). Even though Refis covers debts managed by both agencies, we were only able to obtain data managed by the PGFN, for the disclosure of these data is legally authorized.¹⁴ For the intents and purposes of this research, debtors under the management of both agencies are equivalent.

For our study, we combined three different databases. The first is the *Sistema de Informação Gerencial da Procuradoria-Geral da Fazenda Nacional* (SIG-PGFN) database, with information about which businesses included their tax arrears in the 2014 Refis. The second is the RFB database (*Dados Abertos*), with information about the characteristics of companies, such as their size and longevity. The third is the RAIS database, translated as the Annual Report on Social Information (*Relação Anual de Informações*), which includes census information on employment and employee-employer relationships in Brazil between 2003 and 2017.

In total, 62,964 companies fulfilled all the steps to participate in the 2014 Refis (the population in the treatment group). As depicted in Figure 1, among these companies, medium and large businesses received 78% of the total amount of revenue foregone, but accounted for only 28% of tax relief participants.

¹⁴ See law no. 5172/1966, art. 198, §3º, II and III.

Figure 1: Number of Participants and Foregone Revenue by Business Size in the 2014 Refis



Source: elaborated by the authors

To avoid endogeneity, we chose to analyze companies that had not participated in previous Refis. Therefore, our initial sample in the treated group consisted of 35,272 companies, which participated only in the 2014 Refis. We merged the three databases to obtain a final sample of Brazilian companies that participated only in the 2014 Refis and which included the variable ‘number of employees’ for a period of three years before and three years after the tax relief program (2011-2017). The sample of firms we examine for the ‘treatment’ amounted to 10,013 companies in total.

The merging process with the RAIS database implied a large loss in the number of companies, since many were not represented in the RAIS database. This spottiness occurs because active businesses may accidentally submit a declaration of inactivity or even fail to submit a declaration, despite the obligation to do so. Nevertheless, the RAIS database is regularly used by researchers to study the job market in Brazil and is considered reliable (Santos et al., 2018) and precise (de Negri et al., 2001).

To investigate possible bias between the initial and final samples, we compared the characteristics (covariates) of 25,584 companies that appeared for at least one year in the RAIS database (72.5% of the initial sample) with the 10,013 companies in our sample that presented

complete data (28.4% of the initial sample). As Table 1 illustrates, the covariates between the two groups of companies show no significant differences. It is worth noting that although the final sample has fewer small companies than the initial sample, the standardized difference is very close to the 25% threshold (Rubin, 2001). Hence it is possible to assert that our final sample was representative of the population of companies that participated in the 2014 Refis.

Table 1: Descriptive Statistics and Matching Procedure Tests

NUMBER OF COMPANIES	25.584		10.013			6.290		63.989			9.449		9.449		
	Treated sample - Original		Treated sample - Used		Standardized difference	Treated - Matched		Control - Matched		Standardized difference	Treated - Matched		Control - Matched		Standardized difference
	Mean Treated	SD Treated	Mean Treated	SD Treated		Mean Treated	SD Treated	Mean Control	SD Control		Mean Treated	SD Treated	Mean Control	SD Control	
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)	(K)	(L)	(M)	(N)	(O)
% FOREGONE REVENUE															
Total	27,9%	4,8%	27,5%	5,1%	0,080	27,4%	5,0%				27,5%	5,0%			
Microenterprises	28,0%	4,0%	27,4%	3,8%	0,174	27,5%	3,7%				27,4%	3,7%			
Small Businesses	27,5%	4,1%	27,0%	4,0%	0,131	26,9%	4,0%				27,0%	4,0%			
Medium/Large Businesses	28,0%	6,2%	28,0%	6,5%	0,003	27,8%	6,2%				27,9%	6,3%			
NUMBER OF EMPLOYEES															
Total	37,35	191,97	49,92	234,70	-0,059	35,68	121,02	31,18	119,22	0,037	42,03	142,03	45,47	159,44	-0,023
Microenterprises	9,34	26,26	11,53	30,72	-0,077	6,95	8,58	6,94	8,57	0,001	8,20	10,08	7,46	8,63	0,078
Small Businesses	16,78	30,75	19,76	32,83	-0,094	15,20	21,28	15,03	20,29	0,008	17,52	24,18	16,09	20,90	0,064
Medium/Large Businesses	82,15	315,07	103,84	367,96	-0,063	72,18	184,14	74,95	202,68	-0,014	86,95	217,67	97,38	244,44	-0,045
ECONOMIC SECTOR OF OPERATION															
Retail Businesses	19,3%	39,5%	18,8%	39,1%	0,013	25,0%	43,3%	25,6%	43,6%	-0,014	18,7%	39,0%	18,4%	38,7%	0,008
Wholesale Trade Except Motor Vehicles and Motorcycles	9,7%	29,6%	7,7%	26,7%	0,071	9,3%	29,0%	9,0%	28,6%	0,010	7,9%	27,0%	8,0%	27,1%	-0,004
Health Care Activities	5,7%	23,2%	6,6%	24,8%	-0,037	7,8%	26,8%	8,5%	27,9%	-0,026	6,7%	25,0%	6,7%	25,0%	0,000
Road Transport	6,1%	23,9%	6,4%	24,5%	-0,012	6,6%	24,8%	6,3%	24,3%	0,012	6,3%	24,3%	6,8%	25,2%	-0,020
Building Construction	4,6%	20,9%	4,6%	20,9%	0,000	3,9%	19,4%	3,6%	18,6%	0,016	4,5%	20,7%	4,6%	20,9%	-0,005
Food Product Manufacturing	2,2%	14,7%	3,1%	17,3%	-0,056	2,5%	15,6%	2,3%	15,0%	0,013	3,0%	17,1%	2,8%	16,5%	0,012
Sales and Repair of Motor Vehicles and Motorcycles	3,4%	18,1%	2,9%	16,8%	0,029	3,4%	18,1%	3,7%	18,9%	-0,016	2,9%	16,8%	2,9%	16,8%	0,000
Legal, Accounting, and Auditing Services	3,1%	17,3%	2,8%	16,5%	0,018	3,1%	17,3%	2,9%	16,8%	0,012	2,8%	16,5%	2,7%	16,2%	0,006
Special Services for Construction	2,4%	15,3%	2,7%	16,2%	-0,019	2,5%	15,6%	2,4%	15,3%	0,006	2,8%	16,5%	2,6%	15,9%	0,012
Auxiliary Activities of Financial Services, Insurance, Private Pensions and Health Plans	3,1%	17,3%	2,6%	15,9%	0,030	2,9%	16,8%	3,1%	17,3%	-0,012	2,7%	16,2%	2,5%	15,6%	0,013
Manufacturing of Steel Products, Except Machinery and Equipment	1,5%	12,2%	2,1%	14,3%	-0,045	2,1%	14,3%	2,0%	14,0%	0,007	2,1%	14,3%	2,1%	14,3%	0,000
Food	2,2%	14,7%	2,1%	14,3%	0,007	2,4%	15,3%	2,6%	15,9%	-0,013	2,1%	14,3%	2,1%	14,3%	0,000
Education	1,9%	13,7%	2,1%	14,3%	-0,014	1,8%	13,3%	2,1%	14,3%	-0,022	2,0%	14,0%	2,2%	14,7%	-0,014
Administrative Support Office Services and Other Business Services	2,5%	15,6%	1,9%	13,7%	0,041	1,9%	13,7%	2,0%	14,0%	-0,007	1,9%	13,7%	1,9%	13,7%	0,000
Manufacturing of Rubber and Plastic Products	1,2%	10,9%	1,8%	13,3%	-0,049	1,6%	12,5%	1,5%	12,2%	0,008	1,8%	13,3%	1,7%	12,9%	0,008
Others	31,1%	46,3%	31,8%	46,6%	-0,015	23,2%	42,2%	22,4%	41,7%	0,019	31,8%	46,6%	32,0%	46,6%	-0,004

STATE OF FISCAL DOMICILE															
São Paulo	28,8%	45,3%	29,9%	45,8%	-0,024	36,0%	48,0%	34,7%	47,6%	0,027	29,8%	45,7%	29,6%	45,6%	0,004
Minas Gerais	10,8%	31,0%	10,1%	30,1%	0,023	10,9%	31,2%	11,4%	31,8%	-0,016	10,0%	30,0%	9,9%	29,9%	0,003
Rio Grande do Sul	9,5%	29,3%	9,3%	29,0%	0,007	9,7%	29,6%	10,1%	30,1%	-0,013	9,5%	29,3%	9,9%	29,9%	-0,014
Santa Catarina	8,0%	27,1%	8,5%	27,9%	-0,018	8,3%	27,6%	8,3%	27,6%	0,000	8,7%	28,2%	8,7%	28,2%	0,000
Paraná	7,2%	25,8%	6,6%	24,8%	0,024	6,2%	24,1%	6,4%	24,5%	-0,008	6,5%	24,7%	6,6%	24,8%	-0,004
Rio de Janeiro	6,2%	24,1%	6,6%	24,8%	-0,016	6,9%	25,3%	6,9%	25,3%	0,000	6,5%	24,7%	6,4%	24,5%	0,004
Bahia	4,0%	19,6%	4,1%	19,8%	-0,005	4,1%	19,8%	4,1%	19,8%	0,000	4,1%	19,8%	4,0%	19,6%	0,005
Pernambuco	2,8%	16,5%	2,9%	16,8%	-0,006	2,4%	15,3%	2,5%	15,6%	-0,006	2,9%	16,8%	2,9%	16,8%	0,000
Ceará	2,6%	15,9%	2,9%	16,8%	-0,018	2,4%	15,3%	2,4%	15,3%	0,000	2,8%	16,5%	2,8%	16,5%	0,000
Goiás	2,3%	15,0%	2,5%	15,6%	-0,013	2,1%	14,3%	2,1%	14,3%	0,000	2,4%	15,3%	2,6%	15,9%	-0,013
Others	17,8%	38,3%	16,6%	37,2%	0,032	11,0%	31,3%	11,1%	31,4%	-0,003	16,8%	37,4%	16,6%	37,2%	0,005
SIZE OF COMPANY															
Microenterprises (yes =1)	44,1%	49,7%	30,6%	46,1%	0,282	28,5%	45,1%	37,4%	48,4%	-0,190	30,1%	45,9%	30,1%	45,9%	0,000
Small businesses (yes =1)	26,6%	44,2%	30,6%	46,1%	-0,089	31,5%	46,5%	30,6%	46,1%	0,019	30,5%	46,0%	30,5%	46,0%	0,000
Medium/Large businesses (yes =1)	29,3%	45,5%	38,9%	48,8%	-0,204	40,1%	49,0%	32,0%	46,6%	0,169	39,3%	48,8%	39,3%	48,8%	0,000
TYPE OF COMPANY															
Limited liability company	78,0%	41,4%	80,6%	39,5%	-0,064	84,4%	36,3%	83,3%	37,3%	0,030	80,6%	39,5%	81,1%	39,2%	-0,013
Single-person business	11,9%	32,4%	8,8%	28,3%	0,102	7,7%	26,7%	8,9%	28,5%	-0,044	8,9%	28,5%	8,8%	28,3%	0,004
Others	10,1%	30,1%	10,6%	30,8%	-0,016	7,9%	27,0%	7,8%	26,8%	0,004	10,5%	30,7%	10,1%	30,1%	0,013
TIME IN ACTIVITY															
	14,55	8,20	15,81	9,00	-0,146	15,78	7,49	16,08	7,35	-0,039	15,241	7,659	15,261	7,462	-0,003

Source: elaborated by the authors.

To build the control group, we browsed the SIG-PGFN database in search of companies with tax arrears and that were eligible to participate in 2014 Refis. Analogous to our ‘treatment’ sample, these companies had not participated in previous Refis and presented a complete range of data between 2011 and 2017. We found 223,202 companies that met these criteria.

Identification Strategies

In this paper, we are interested in assessing the effect of a tax relief program on job creation or maintenance. The dependent variable is the number of employees. The set of independent variables includes observable characteristics of companies, such as the state (federal unit - UF) of their fiscal domicile, the economic sector of operation (CNAE), the number of years they have been operating, and the size and type of company.

This study requires a quasi-experimental framework, which is a common strategy when evaluating public policies (Caiumi, 2011). The challenge is to control for potential selection bias, as companies with eligible tax arrears could all choose to participate in the 2014 Refis. We therefore combined a matching method with a difference-in-differences (DiD) regression to evaluate the extent to which the 2014 Refis had an impact on job creation or maintenance. The aim was to replicate a randomized experiment as closely as possible.

Matching

The main purpose of matching procedures is to reduce the imbalance in the distribution of pre-treatment confounders between the treated and control groups. The most common method is propensity score matching (PSM), in which we compute the probability that a unit in the control group will enroll in the program based on the observed values of its characteristics (Gertler et al., 2015).

The literature emphasizes several shortcomings associated with PSM (King & Nielsen, 2019). Hence we opted for coarsened exact matching (CEM) (Iacus et al., 2012). Although PSM can be combined with (CEM) (Hembre et al., 2021; Moulton et al., 2022), we chose CEM as the first strategy, using PSM (1, 1) as a robustness check. Both treatment and control groups were

paired in 2014 (the baseline for our study). The covariates used to perform the matching are the state (federal unit - UF) of the fiscal domicile, the economic sector in which the business operates (CNAE), the number of years in operation, and the type and size of the company.

Before matching, we excluded 560 outliers from the treatment sample (observations above three standard deviations). Afterwards, to achieve coarsened exact matching, we used exact matching in the qualitative variables. In the quantitative variables, we divided the distribution in 5 parts and matched exactly in the quintile. This way we could guarantee that the imbalance between matched treatment and control groups was not larger than the previous one (Iacus et al., 2012).

To check the quality and robustness of the matching procedure, we tested (standardized difference) the balance between treatment and control groups of companies and found no significant differences (columns (A) to (E) of Table 1).

Difference-in-Differences Regression

After matching the treatment and control groups, we ran a difference-in-differences (DiD) regression model to eliminate unobserved heterogeneity and time trends that persisted after the implementation of CEM. The DiD model is expressed as follows:

$$Y_{it} = \beta_1(REFIS_i \times AFTER_t) + \delta_t + \mu_i + \varepsilon_{it} \quad (1)$$

Here, Y_{it} is the number of employees for company i in the year t , $REFIS_i = 1$ for Refis participant company i , and 0 otherwise, $AFTER_t = 1$ for $t > 2014$, and 0 otherwise, δ_t is a year fixed effect, μ_i controls for unobserved heterogeneity in companies, and ε_{it} indicates the error term. The coefficient of interest is β_1 , which represents the causal effect of Refis on employment. In this model, β_1 measures the differential increase, subsequent to 2014, of employees in companies participating in the Refis versus those that did not participate. To more precisely evaluate the impact of Refis given the heterogeneity of the companies, we also analyzed different subsamples according to size.

One key underlying assumption of a DiD analysis is the parallel trends assumption (PTA). This assumption means that the control group is an appropriate counterfactual of the trend that the

treated companies would have followed if they had not participated in the program. When the PTA is violated, DiD regressions are biased (O’Neill et al., 2016). No test can be implemented to check whether the PTA holds or not, because it is impossible to know whether the outcomes of both control and treated companies would have followed the same trend after the treatment. However, Gertler et al. (2011) suggest checking the validity of the PTA by comparing changes in outcomes for the two groups repeatedly before the treatment, i.e., before participating in the program.

We checked the validity of the PTA using a graphic representation of outcomes in treated and control groups three years before Refis. The visual evidence is complemented with regression results (Muralidharan & Prakash, 2017). We estimated the following regression model on the sample for 2011-2013 (pre-REFIS years):

$$Y_{it} = \alpha(REFIS_i \times TREND) + \delta_t + \mu_i + \varepsilon_{it} \quad (2)$$

Here, *TREND* is a linear time trend, and all other variables are the same as in (1). Our interest is in the coefficient α , which allows us to test if the average number of employees in the treatment group is growing or decreasing faster than in the control group (non-Refis) for the period before the 2014 Refis.

Empirical Results

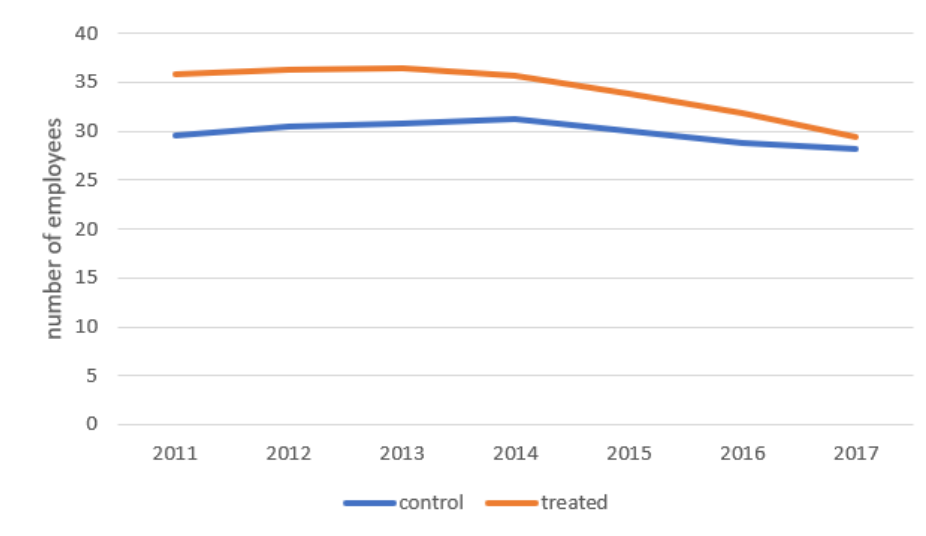
Accuracy of the Matching Procedure

Before turning to DiD estimation results, we checked the validity of our matching method. Results of the tests are reported in columns (F) to (J) of Table 1 and provide evidence of the accuracy of the CEM matching procedure for most variables and years. Most of the tests strongly support the null hypothesis of means equality between treated and matched companies.

We also checked the validity of the PTA using a graphical representation of outcomes before the treated companies participated in 2014 Refis, as can be seen in Figure 2. It appears that treated and control companies followed the same trend before the treatment occurred in most cases and

years. These results suggest that the PTA holds when using CEM matching on three pre-treatment periods, confirmed by the regression results on the pre-Refis years (Table 2).

Figure 2: Parallel Trends between Treated and Control Groups



Source: elaborated by the authors.

Table 2: Parallel Trends Before 2014

	Without Matching (1)	Total (2)	Micro (3)	Small (4)	Medium/Larg e (5)
	b/SE	b/SE	b/SE	b/SE	b/SE
$REFIS_i \times TREND$	-1.1041*** (0.3958)	-0.3252 (0.3161)	-0.0928 (0.0540)	-0.1882 (0.1415)	-0.9614 (0.8597)
Intercept	129.9288 (34.1692)	88.6476 (56.7922)	19.8188 (7.5467)	50.1178* (26.1214)	281.8308 (189.0551)
Observations	1,632,505	491,953	180,194	150,766	160,993
Number of firms (treated)	10,013	6,290	1,791	1,979	2,520
Number of firms (control)	223,013	63,989	23,951	19,559	20,479
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the number of employees in the companies i and year t . Standard errors (in parentheses) are clustered at the firm level to consider potential autocorrelation. Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Source: elaborated by the authors.

Main Effects

We estimated the difference-in-differences model using the number of employees as the dependent variable. Comparing the number of employees in the baseline (2014) with follow-up years (2015-2017) in treatment and control groups, the interaction showed statistical significance ($\beta_1 = -2.895$, $SE = 0.616$, $p < 0.01$). On average, the 2014 Refis had a negative impact on job maintenance, with an effect of approximately 3 fewer employees in the treated group when compared to the control group. Hence hypothesis 1 was rejected.

With respect to the size subsamples, only microenterprises showed no statistical significance ($\beta_1 = -0.091$, $SE = 0.124$, $p > 0.1$), while small and medium/large companies presented statistically significant coefficients ($\beta_1 = -0.718$, $SE = 0.296$, $p < 0.05$; $\beta_1 = -6.389$, $SE = 1.612$, $p < 0.01$, respectively). Results can be observed in Table 3 below. Thus hypothesis 2 was also rejected, since we found significant negative effects not only for medium and large companies but also for small companies too.

Table 3: Difference-in-Differences Models with CEM

	Matched - CEM				
	Without Matching (1)	Total (2)	Micro (3)	Small (4)	Medium/Large (5)
	b/SE	b/SE	b/SE	b/SE	b/SE
$REFIS_i \times AFTER_t$	-3.666*** (1.256)	-2.895*** (0.616)	0.091 (0.124)	-0.718** (0.296)	-6.389*** (1.612)
Intercept	34.598 (0.176)	30.100 (0.324)	6.828 (0.055)	15.334 (0.142)	69.978 (0.978)
Observations	1,632,505	491,953	180,194	150,766	160,993

Number of Firms (treated)	10,013	6,290	1,791	1,979	2,520
Number of Firms (control)	223,013	63,989	23,951	19,559	20,479
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the number of employees in the enterprise i and year t . Standard errors (in parentheses) are clustered at the firm level to consider potential autocorrelation. Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Source: elaborated by the authors.

Given the distribution's asymmetry on the number of employees, we also estimated the difference-in-differences model using the natural logarithm of the number of employees as the dependent variable. Companies participating in the relief lost 6.2% more jobs than nonparticipating companies (Table 4). Therefore, we find no empirical support for the hypotheses proposed in this paper: Refis programs are associated with the loss of job rather than their creation, and medium and larger businesses are not more likely to maintain or create jobs than smaller companies.

Table 4: Difference-in-Differences Models (natural logarithm) with CEM

	Matched - CEM				
	Without Matching (1)	Total (2)	Micro (3)	Small (4)	Medium/Larg e (5)
	b/SE	b/SE	b/SE	b/SE	b/SE
$REFIS_i \times AFTER_t$	-0.1092*** (0.0069)	-.061591*** .0087606	-0.0108 (0.0139)	-0.0548*** (0.0154)	-0.0985*** (0.0154)
Intercept	2.0406*** (0.0010)	2.1754*** (0.0036)	1.4299*** (0.0064)	2.1331*** (0.0062)	3.049*** (0.0062)
Observations	1,632,505	491,953	180,194	150,766	160,993
Number of firms (treated)	10,013	6,290	1,791	1,979	2,520
Number of firms (control)	223,013	63,989	23,951	19,559	20,479
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the natural logarithm of the number of employees in company i and year t . Standard errors (in parentheses) are clustered at the firm level to consider potential autocorrelation. Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Source: elaborated by the authors.

Robustness Checks

In this section, we report several robustness tests to check the sensitivity of our results to alternative econometric specifications. We employed an alternative matching method (PSM(1,1)), collapsed the pre and post-treatment periods into two periods and performed falsification tests.

Alternative Matching Method (PSM)

To test the sensitivity of our results, we used propensity score matching (nearest neighbor). Balance results are summarized in columns (K) to (O) of Table 1. We note that the results of the difference-in-differences model are highly similar (Table 5), in terms of both signs and magnitude of the effects, to those using the CEM method.

Table 5: Difference-in-Differences Models with PSM

	Matched – PSM (1 to 1)				
	Without Matching	Total	Micro	Small	Medium/Large
	(1)	(2)	(3)	(4)	(5)
	b/SE	b/SE	b/SE	b/SE	b/SE
$REFIS_t \times AFTER_t$	-3.666*** (1.255)	-1.995** (0.971)	-0.018 (0.143)	-0.404 (0.340)	-4.745** (2.450)
Intercept	34.598*** (0.176)	45.513*** (0.414)	8.503*** (0.069)	18.593*** (0.163)	94.736*** (1.043)
Observations	1,632,505	132,286	39,858	40,376	52,052
Number of Firms (treated)	10,013	9,449	2,847	2,884	3,718
Number of Firms (control)	223,013	9,449	2,847	2,884	3,718
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the number of employees in the enterprise i and year t . Standard errors (in parentheses) are clustered at the firm level to consider potential autocorrelation. Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Source: elaborated by the authors.

Collapsing Pre and Post Treatment Periods

Because of serial correlation, conventional DiD standard errors may understate the standard deviation of the estimated treatment effects, leading to an overestimate of t-statistics (Bertrand et al., 2004). Hence collapsing the data into pre- and post- periods can produce consistent standard errors and robust estimations. We applied this solution to our analysis and obtained 140,558 observations. As illustrated in Table 6, the results are qualitatively similar to those of Table 3, alleviating any doubts regarding validity.

Table 6: Difference-in-Differences Models with Collapsed Pre and Post Treatment Periods

	Matched – CEM			
	Total	Micro	Small	Medium/Large
	(2)	(3)	(4)	(5)
	b/SE	b/SE	b/SE	b/SE
$REFIS_i \times AFTER_t$	-3.600*** (0.546)	-0.241** (0.098)	-1.510*** (0.250)	-7.434*** (1.354)
Intercept	22.324*** (0.050)	5.370*** (0.008)	12.436*** (0.025)	50.560*** (0.151)
Observations	140,558	51,484	43,076	45,998
Number of Firms (treated)	6,290	1,791	1,979	2,520
Number of Firms (control)	63,989	23,951	19,559	20,479
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variable is the number of employees in company i and year t . Standard errors (in parentheses) are clustered at the firm level to consider potential autocorrelation. Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Source: elaborated by the authors.

Falsification Tests

As previously discussed, one key assumption for DiD is the PTA. To check whether the PTA holds, we applied a placebo test using periods before the tax relief program and excluded data at the date of treatment and subsequently. We also assigned the year before the (real) treatment as the “placebo” treatment period and re-estimated the DiD. Significant estimates obtained in this placebo test would have cast doubt on the violation of the PTA. Results are summarized in Table 7, showing no significant effect of the placebo program on employment.

Table 7: Placebo Test on Pre-treatment Period

	CEM		PSM	
	$AFTER_t=2011$	$AFTER_t=2012$	$AFTER_t=2011$	$AFTER_t=2012$
$REFIS_i \times AFTER_t$	-0.5877 (0.5570)	-0.3881 (0.4316)	-0.6091 (0.6956)	-0.8334 (0.6708)
Intercept	30.1009 (0.2167)	30.1009 (0.2167)	45.5139 (0.2318)	45.5139 (0.2318)
Observations	210,837	210,837	56,694	56,694
Number of Firms (treated)	6,290	6,290	9,449	9,449
Number of Firms (control)	63,989	63,989	9,449	9,449
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variable is the number of employees in the company i and year t . Standard errors (in parentheses) are clustered at the firm level to consider potential autocorrelation. Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Source: elaborated by the authors.

Conclusions

As a form of ‘tax expenditure’, tax debt relief forfeits current public expenditures, benefiting a comparatively small number of taxpayers in the expectation that they contribute, in kind, to the public interest. Our analysis of Brazil, which has offered more than 40 fiscal recuperation (‘Refis’)

programs since the year 2000, finds little evidence to support the merit of tax debt relief policies. The first part of this article shows how the formulation, adoption, implementation, and oversight of these policies suffer from alarming deficits of due process and transparency. These results conform to general knowledge about tax expenditures, which, as episodic ad hoc policies, tend to be un(der)regulated around the world.

In Brazil, tax debt relief policies are usually formulated in the executive branch, sent to Congress as urgent executive orders, and expanded by congressional amendments. They lack public consultation, adequate legislative deliberation, and transparent due process. Beyond the policy process, the Refis offers private benefits – including debt-forgiveness, sub-prime interest rates, and up to 240-month repayment plans – which contain little in the way of a public value proposition. On most dimensions, these taxpayer benefits are woefully out of tune with the prescriptions of multilateral institutions, such as the IMF. Although recent policy changes promise greater transparency, the rule until now has been secrecy, weak due process, and exorbitant benefits.

In the second part of the article, we address the question of impact. Our object of study is a 2014 tax debt relief program's impact on job creation or maintenance three years after it took effect. The 2014 Refis forfeited R\$9.4 billion in business tax arrears, equivalent to US\$3.5 billion when calculated using 2014 average exchange rates. Deploying a difference-in-differences quasi-experiment to gauge the Refis' impact on approximately 10,000 businesses for three years after its onset, our results show negative impacts in all but microenterprises (which showed no statistical significance). Admittedly, data on employment from the RAIS contain significant omissions, affecting the number of businesses that could be included in control and treatment groups. Businesses that should have reported to the RAIS did not, reducing the number of treated businesses by 71.6%. However, our analysis nonetheless meets statistical standards, and our findings are robust to multiple specifications. These findings show that beneficiaries of the Refis tax debt relief program exhibited 6% fewer jobs, on average, than non-participants.

Although causal identification is beyond the scope of this paper, there are few explanations that might account for these results. The most likely explanation has to do with the pecuniary strategies of beneficiaries. Either beneficiaries are using Refis to pay all their debts and begin the formal procedures to shut down their businesses; or, rather than doing ‘the right thing’ and reinvesting savings from deferred or forgiven taxes back into their businesses, business owners are instead investing these funds into assets that yield higher returns. This last opportunity costs rationale, stressed by several other authors, is based on assumptions of non-enforcement (Lima et al., 2017; Paes, 2012; Rezende et al., 2018). This theory seems especially plausible because of the comparative benefits of different types of investments. Doing business in Brazil is expensive because of this country’s regulatory environment, high taxes, and high interest rates. Brazilian corporations not only pay one of the world’s most elevated rates of corporate income tax (34%), but they also pay innumerable other levies to comply with labor and regulatory obligations (World Bank, 2020). These costs impose enormous burdens on the private sector in Brazil, making it highly uncompetitive in all but a few industries. Within this context, business owners may be investing the savings from tax relief into assets that yield higher dividends, particularly given this country’s historically high interest rates.

For the time being, explaining job losses among tax debt relief beneficiaries is less important than considering the general result. Not only do Brazil’s Refis programs lack due process and transparency, but their effect on employment – 6% fewer jobs among relief beneficiaries – leads to the conclusion that Brazil’s tax debt relief policies waste taxpayer money. They also increase inequality, because Refis policies forgive arrears proportional to the size of debt. This means that those who stand to gain the most from tax debt relief are medium and large businesses, i.e. those who already have considerable resources, as depicted in Figure 1. The poor performance of tax debt relief policy undoubtedly helps explain why their development and passage occurs in the shadows.

That Brazil’s Refis programs represent a self-serving policy of a select elite seems a fair interpretation; that they inefficiently compensate businesses for an unpropitious tax and regulatory

environment is perhaps a more generous conjecture. Either way, Brazil's tax debt relief policies are symptomatic of larger fiscal and regulatory dysfunctions. In the absence of reform, further research is needed to understand the extent to which other tax expenditures and elements of the fiscal framework – in Brazil, as in other countries – abet or detract from the public good.

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