UC Berkeley UC Berkeley Electronic Theses and Dissertations

Title

Essays in Public Economics and Development

Permalink https://escholarship.org/uc/item/1f55s25v

Author Gerard, Francois

Publication Date 2013

Peer reviewed|Thesis/dissertation

Essays in Public Economics and Development

by

François Gerard

A dissertation submitted in partial satisfaction of the requirements for the degree of Doctor of Philosophy

 in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Emmanuel Saez, Co-chair Professor Edward Miguel, Co-chair Professor David Card Professor Catherine Wolfram

Spring 2013

Essays in Public Economics and Development

Copyright 2013 by François Gerard

Abstract

Essays in Public Economics and Development

by

François Gerard Doctor of Philosophy in Economics University of California, Berkeley Professor Emmanuel Saez, Co-chair Professor Edward Miguel, Co-chair

The present thesis studies public economics questions in the context of developing countries. In particular, I investigate the impact and design of specific government policies in Brazil. Government interventions may be desirable when unregulated market economies deliver socially inefficient outcomes. Goods and services tend to be under-provided in the presence of imperfect or asymmetric information. Such market failures may be pervasive in the insurance market and prompt governments to provide certain types of insurance directly. Chapters 1 and 2 study social insurance programs, and more specifically unemployment insurance (UI). In contrast, goods and services tend to be over-provided if they generate negative externalities. In recent years, there has been a lot of interest in the negative externalities associated with energy consumption. Chapter 3 studies energy conservation policies, and more specifically residential electricity conservation. In each of the three essays, I develop a simple theoretical framework to guide my empirical analysis. I then estimate the relevant impacts and combine theory and empirics to inform the design of government programs.

There is vast literature in public economics (and related fields) on social insurance programs and energy conservation policies. Yet, as for most research in public economics, existing work focuses almost entirely on the context of developed countries. Arguably, social insurance and energy conservation are not first–order priorities in least developed countries. However, these topics are becoming increasingly relevant for developing countries. Most of the growth in energy demand is forecast to come from the developing world, especially for residential consumers. Social insurance programs have been adopted in a growing number of developing countries. Currently some form of UI exists in Algeria, Argentina, Barbados, Brazil, Chile, China, Ecuador, Egypt, Iran, Turkey, Uruguay, Venezuela and Vietnam; Mexico, the Philippines, Sri Lanka, and Thailand have been considering its introduction. Moreover, the severe data constraints that limited empirical work at the intersection of public and development economics are being removed. Today, large administrative datasets and high–quality surveys are available in many developing countries. Importantly, results from more advanced countries are unlikely to translate easily to a developing country context. For instance, the enforcement of social program eligibility is a major challenge in developing countries where the informal sector accounts for a large share of the economy. In Brazil, about half of the employed population works in jobs that escape oversight and monitoring from the government. The presence of a large informal sector is widely believed to increase the efficiency costs of social programs. The main concern is that informal job opportunities exacerbate programs' disincentives to work in the formal sector. The essay in the first chapter (joint work with Gustavo Gonzaga) evaluates such a claim.

We begin by developing a simple theoretical model of optimal UI that specifies the efficiency-insurance tradeoff in the presence of informal job opportunities. We then combine the model with evidence drawn from 15 years of uniquely comprehensive administrative data to quantify the social costs of the UI program in Brazil. We first show that exogenous extensions of UI benefits led to falls in formal-sector reemployment rates due to offsetting rises in informal employment. However, because reemployment rates in the formal sector are low, most of the extra benefits were actually received by claimants who did not change their employment behavior. Consequently, only a fraction of the cost of UI extensions was due to perverse incentive effects and the efficiency costs were thus relatively small — only 20%as large as in the US, for example. Using variation in the relative size of the formal sector across different regions and over time in Brazil, we then show that the efficiency costs of UI extensions are actually *larger* in regions with a larger formal sector. Finally, we show that UI exhaustees have relatively low levels of disposable income, suggesting that the insurance value of longer benefits in Brazil may be sizeable. In sum, the results overturn the conventional wisdom, and indicate that efficiency considerations may in fact become *more* relevant as the formal sector expands.

The findings of this essay have broader implications for our understanding of social policies in developing countries. Many social programs and taxes generate incentives for people to carry out their economic activities informally. For the same reasons as for UI, they are viewed as imposing large efficiency costs in a context of high informality. By going against the conventional wisdom, our results cast doubt on whether efficiency considerations actually limit the expansion of social policies in these cases too.

The essay in the second chapter (joint work with Gustavo Gonzaga) follows directly from the above results. Governments face two main informational constraints when implementing any program or regulation (e.g., welfare program). First, there is a screening issue. Government may fail to identify the ex-ante population of interest (e.g., poorest households). Second, there is a monitoring issue. Agents may adopt unobserved behaviors to join or escape the population of interest (e.g., reducing work efforts). The lack of strict monitoring policies for government programs is often considered to be a major issue in developing countries where non-compliance is widespread. Yet, we know surprisingly little about the magnitude of the behavioral responses that we wish to mitigate, relative to the cost of efficient monitoring policies. The Brazilian UI program offers a stark example of a weak monitoring environment. Until recently and for over 20 years, there was absolutely no monitoring of formal job search for UI beneficiaries in Brazil, even though many beneficiaries work informally when drawing UI benefits. In the second chapter, we argue that the results presented in the first chapter may rationalize the complete lack of monitoring in Brazil until 2011.

We begin by deriving a theoretical upper bound for the maximum price that a government should be willing to pay per beneficiary to perfectly monitor the formal job search of UI beneficiaries. We show that the bound corresponds to the share of program costs due to behavioral responses. Intuitively, there is little incentive to introduce monitoring if most beneficiaries draw UI benefits without changing their formal reemployment behavior. The overall scope of the monitoring issue is thus limited in Brazil because most beneficiaries would collect UI benefits absent any behavioral response, as shown in the first chapter. Yet, monitoring policies may still be cost-effective if the government is able to target them towards workers with relatively larger behavioral responses. In the empirical analysis, we investigate to what extent the government could use information readily available ex ante (a signal) to identify worker categories with relatively larger behavioral responses. We find that most of the heterogeneity is not easily captured by observable characteristics. Therefore, monitoring policies would be relatively costly even if the government used available signals to target them efficiently. These results motivate future work on the cost-effectiveness of job-search requirements for UI beneficiaries, which have been recently introduced in Brazil.

If there is little evidence on the impact of social insurance programs in developing countries, there is almost no evidence on the impact of energy conservation policies. Moreover, results from more advanced countries are also unlikely to translate easily to the context of developing countries. Households in the developing world own fewer appliances and consume much less energy on average. Average monthly residential electricity consumption in Brazil was below 200 kilowatt hours in 2000. Enforcement is also a major challenge. Electricity theft amounts to 15% of the total load for some utilities in Brazil. In the third chapter, I investigate the short– and long–term impacts on residential consumption of the largest electricity conservation program to date. This was an innovative program of economic (fines) and social (conservation appeals) incentives implemented by the Brazilian government in 2001–2002 in response to supply shortages of over 20%.

Achieving ambitious energy conservation targets through economic incentives is often considered infeasible. Yet, there is little evidence from ambitious conservation policies. I find that the Brazilian conservation program reduced average electricity consumption per customer by .25 log point during the nine months of the crisis. Importantly, the program induced sizable lumpy adjustments; it reduced consumption by .12 log point until at least 2011. Using individual billing data from three million customers, I show that average effects came from dramatic reductions by most customers. I also provide suggestive evidence that lumpy adjustments came from new habits rather than physical investments. Finally, I structurally estimate a simple model to quantify the role of social incentives and lumpy adjustments. Social incentives amounted to a 1.2 log point increase in electricity tariffs, and may thus be particularly powerful in times of crisis. Importantly, a .6 log point permanent increase in tariffs would have been necessary to achieve the observed consumption levels during and after the crisis absent any lumpy adjustment. The possibility of triggering lumpy adjustments may thus substantially reduce the incentives necessary to achieve ambitious energy conservation targets.

Beyond the specific issues it addresses, I hope that this dissertation will help convince senior and junior scholars alike of the relevance and feasibility of academic research at the intersection of public and development economics. More work is deeply needed.

To public higher education.

Acknowledgments

Completing this thesis has been a life-changing experience. It brought me from Brussels to Berkeley, with unexpected stops in Bamako, Busia, Rio de Janeiro, and Stockholm, and soon New York. It opened the door of an exciting new world and taught me how to navigate its waters. It allowed me to meet amazing mentors, outstanding colleagues, and dear friends. Like every journey, this one had a fair share of changing winds and rainy days. For helping me to stay afloat and enjoy the ride, I am deeply indebted to the following people.

First of all, I am incredibly grateful to my advisor, Emmanuel Saez, and his open door. His work and approach to research have been a permanent source of inspiration; his calm, clarity, confidence and focus a most needed counterpoint to my own shortcomings. Ted Miguel, my co-advisor, introduced me to the 85% of the planet's population that are too often studied in a single field of economics. A year of seminars, a summer in Western Kenya, and four months of his lectures, forever changed my research priorities. I also greatly benefited from the sharp and constructive criticism of David Card, and the example that his uncompromising dedication to economics as a science, and to the rigorous evaluation of public policies, constitutes. I would have lost a part of my research-self without the enthusiasm, guidance, and encouragements of Catherine Wolfram and Meredith Fowlie. I would also like to thank Fred Finan for his sincerity and unwillingness to settle for anything less than excellence. Many other faculty have enriched my experience by their support, teaching, and example. I would like to mention especially George Akerlof, Alan Auerbach, Raj Chetty, Lucas Davis, Pat Kline, Botond Koszegi, Matthew Rabin, and Betty Sadoulet.

None of the essays in the present thesis would have been possible without a protester who pulled a fire alarm in Evans Hall during a Labor seminar in December 2009. This typical act of Berkeleyism allowed me to meet my co–author Gustavo Gonzaga. I can hardly overstate his contribution. The first two chapters are the direct fruit of our collaboration. The third chapter would not exist without his many phone calls and emails on my behalf. Moreover, our conversations and my extended stays at PUC-Rio, where his colleagues welcomed with me with a wonderful hospitality, sparked my broad interest in his fascinating country. Our collaboration will (I hope!) continue in the many years to come.

I would also like to thank my parents for their unconditional love and support; Lena Nekby, for an amazing (even if unsuccessful research–wise) opportunity to discover Sweden, its beautiful capital, and its wealth of data; Patrick Allen, for his cheerful help with administrative matters; Gabe, Issi, Valentina, and Willa for forming the best study group ever and much more; Alex, Gianmarco, Jamie, Jonas, Josh, Mark, and many other fellow students for their help, suggestions, and friendship; the Convex optimizers, Touché, and especially its toughest defender, for reminding me that there is a life outside the office; the California weather; and UC Berkeley for being what it is, a unique center of excellence and collegiality.

This thesis was supported, in part, by fellowships from the Belgian American Educational Foundation, Wallonie–Bruxelles International, and the Center for Equitable Growth.

Contents

| Abstract | | 1 |
|------------------|---|--|
| Acknowledgements | | |
| 1 | Informal Labor and the Cost of Social Programs:Evidence from 15Years of Unemployment Insurance in Brazil1Background and Data | 1 6 9 14 17 25 29 30 49 |
| 2 | Job-Search Monitoring in a Context of High Informality1Background and data2Conceptual framework3Estimating incentives to monitor formal job-search4Conclusion | 70 73 74 77 82 |
| 3 | What Changes Energy Consumption, and For How Long? Evidencefrom the 2001 Brazilian Electricity Crisis1Background and data2Customers' responses to incentives: theoretical framework3Short- and long-term impacts of the conservation program4Adjustment mechanisms5The relative roles of social incentives and lumpy adjustments6ConclusionAAppendixBWeb Appendix | 88 93 97 101 108 111 115 130 137 |

Chapter 1

Informal Labor and the Cost of Social Programs: Evidence from 15 Years of Unemployment Insurance in Brazil

with Gustavo Gonzaga

We would like to thank Veronica Alaimo, Miguel Almunia, Alan Auerbach, Juliano Assunção, Richard Blundell, Mark Borgschulte, David Card, Raj Chetty, Julie Cullen, Claudio Ferraz, Fred Finan, Jonas Hjort, Maria Hedvig Horvath, Patrick Kline, Camille Landais, Attila Lindner, Ioana Marinescu, Jamie Mc-Casland, Pascal Michaillat, Edward Miguel, Torsten Persson, Emmanuel Saez, Rodrigo Soares, Owen Zidar, and seminar participants at the Annual Meetings of the Society of Labor Economists, Brown, Columbia, Duke, Chicago, the Inter–American Development Bank, the International Institute for Economic Studies, Insper, McGill, PUC-Rio, Toulouse, UC Berkeley, UC San Diego, University College London, University of Maryland, Urbana–Champaign, Wharton, Wisconsin–Madison, and the World Bank for useful comments and suggestions. We also thank the Ministério do Trabalho e Emprego for providing access to the data and CNPq (Gustavo Gonzaga), Wallonie–Bruxelles International, and the Center for Equitable Growth (François Gerard) for financial support. All errors are our own.

Chapter 1: Informal Labor and the Cost of Social Programs

The enforcement of tax compliance and social program eligibility is a major challenge in developing countries, where the informal sector accounts for 40% of GDP and 55% of the labor force.¹ In a context of high informality, the conventional wisdom dictates that taxes and social spending impose high efficiency costs (Gordon and Li, 2009). This is thought to be particularly the case for social programs that require beneficiaries to not be formally employed (Levy, 2008). The concern is that informal job opportunities exacerbate programs' disincentives to work in the formal sector.²

Despite this widespread view, the evidence behind it remains limited. First, due to data constraints, very few papers credibly estimate the impact of social programs on employment choices. Existing surveys often poorly measure eligibility and have sample sizes too small to exploit most sources of exogenous variation in program benefits. Large administrative datasets are only slowly becoming available in developing countries. Second, those studies finding that social programs induce some beneficiaries to not work in the formal sector lack a theoretical framework to interpret this evidence in terms of the relevant tradeoff between efficiency and equity (or insurance).³

This paper addresses both limitations for the case of Unemployment Insurance (UI) in Brazil. We develop a simple partial–equilibrium model of optimal unemployment insurance in the presence of informal job opportunities to guide our empirical analysis. We then provide new evidence on the size of the relevant effects using 15 years of restricted access administrative data, longitudinal survey data, and credible empirical strategies. As a result, we quantify the tradeoff between (formal) job–search incentives and insurance, and we provide the first estimates of efficiency costs for a typical social program in a setting where informal labor is prevalent.

UI is an ideal program to study these issues. It requires the beneficiaries — displaced formal employees — to not be formally (re)employed. It has recently been adopted or considered in a number of developing countries.⁴ Moreover, international development agencies have emphatically pointed to the heightened moral hazard problem it supposedly creates

³For instance, several papers investigate the impact of the Mexican Seguro Popular program, which extended health care coverage to the informally employed, on the size of the formal sector (Azuara and Marinescu, 2011; Campos-Vazquez and Knox, 2008; Bosch and Campos-Vasquez, 2010; Aterido, Hallward-Driemeier and Pagés, 2011).

¹Average in both Brazil and Latin America (Schneider, Buehn and Montenegro, 2010; Perry et al., 2007).

² "Because checking benefit eligibility imposes large informational and institutional demands, particularly under abundant and diverse employment opportunities in the unobservable informal sector, the resulting weak monitoring would make the incentive problem of the standard UI system much worse" (Robalino, Vodopivec and Bodor, 2009). The authors of this policy paper are the current and the former Labor Team leaders at the Social Protection anchor of the World Bank. The same concern applies to many different types of social programs. For example, welfare programs do not typically deny benefits to the formally employed but they condition transfers on income as observed by the government. Because informal wages are easier to hide, such programs create similar incentives.

⁴Currently some form of UI exists in Algeria, Argentina, Barbados, Brazil, Chile, China, Ecuador, Egypt, Iran, Turkey, Uruguay, Venezuela and Vietnam (Vodopivec, 2009; Velásquez, 2010). Mexico, the Philippines, Sri Lanka, and Thailand have been considering its introduction.

in the presence of a large informal sector.⁵ Brazil also constitutes a uniquely well–suited empirical setting because it offers wide variation in formal employment rates across space and time.⁶ This allows us to explore how efficiency costs may change with the relative size of the formal labor market.

We begin by adapting the canonical Baily model of optimal UI in two ways (Baily, 1978; Chetty, 2006). We introduce informal work opportunities and we consider extensions of the maximum benefit duration instead of changes in benefit levels (Schmieder, von Wachter and Bender, 2012). We show that the efficiency costs of UI extensions, from distorting incentives to return to a formal job, are captured by a pseudo-elasticity ($\tilde{\eta}$), the ratio of a *behavioral* cost to a *mechanical* cost. The former measures the cost of UI extensions due to behavioral responses. Beneficiaries may delay formal reemployment to draw additional benefits. The latter measures the cost absent any behavioral response. Beneficiaries who would not be formally reemployed after UI exhaustion in absence of the extension draw additional benefits without changing their behavior. The ratio measures the fraction of social spending lost through behavioral responses.⁷ A UI extension increases welfare if the social value of the income transfer to UI exhaustees exceeds $\tilde{\eta}$.

We then exploit a unique dataset matching the universe of formal employment spells in Brazil to the universe of UI payments from 1995 to 2010. We observe how rapidly each beneficiary returns to a formal job after regular UI benefits are exhausted. This allows us to estimate the mechanical cost of UI extensions. We estimate the behavioral cost using two empirical strategies: a politically–motivated UI extension (difference–in–difference) and a tenure–based eligibility cutoff (regression discontinuity). Finally, we use longitudinal survey data to estimate overall (formal and informal) reemployment rates and provide suggestive evidence for the social value of the extended benefits.

This paper has four main findings. First, beneficiaries respond to UI incentives. Formal reemployment rates spike at UI exhaustion and this spike shifts completely following exogenous UI extensions. Because we find no such spike in overall reemployment rates,

⁵See (Acevedo, Eskenazi and Pagés, 2006; Robalino, Vodopivec and Bodor, 2009; Vodopivec, 2009). These policy papers cite evidence of moral hazard from Slovenia (van Ours and Vodopivec, 2006), a country with relatively high levels of formality. The proposed alternative is a system of Unemployment Insurance Savings Accounts. The new Jordanian program, for instance, designed in consultation with the World Bank, is a forced savings scheme to which workers contribute when formally employed. "UI benefits" drawn by a worker in excess of what she contributed over her lifetime must be paid back at retirement.

⁶The variation in formal employment rates across Brazilian states over our 15 years of data covers the existing variation across Latin American countries today. Private–sector formal employment rates are strongly correlated with income per capita. The variation in income per capita across Brazilian states is very large, ranging from the levels in China in the poorest state to Poland in the richest (http://www.economist.com/content/compare-cabana).

⁷This is a common result in public finance. The mechanical effect on government revenues of increasing the income tax, for instance, corresponds to the tax base ex-ante. The behavioral effect corresponds to the change in the tax base due to the tax increase. Their ratio, equal to the marginal deadweight burden of the tax increase, captures efficiency costs (Saez, Slemrod and Giertz, 2012). Our measure of efficiency and our welfare formula apply to a broad class of models as long as an envelope condition applies to the agents' problem (Chetty, 2006).

the response comes from beneficiaries (re)employed informally. Second, formal reemployment rates are on average very low even after UI exhaustion. Most beneficiaries draw extra benefits without changing their behavior. Extending UI by two months, from five to seven months, mechanically increases average benefit duration by 1.7 months in Brazil. As a result, the behavioral cost is small compared to the mechanical cost. Our largest estimate of $\tilde{\eta}$ is around .2, less than one fifth of estimates for the US (Katz and Meyer, 1990).⁸ Third, we find a positive relationship between formal employment rates and how rapidly beneficiaries return to a formal job after UI exhaustion (the spike). This result holds in the cross-section, using variation across regions over time, and controlling for a rich set of worker characteristics. It implies that the mechanical cost of UI extensions decreases with formal employment. In contrast, the behavioral cost may increase when more beneficiaries are formally reemployed rapidly after UI exhaustion (larger spike). We find that the behavioral cost does increase with formal employment rates. Thus, contrary to the prevailing belief, the efficiency costs of UI extensions are relatively small in a context of high informality and in fact rise with the relative size of the formal labor market. Last, we find that UI exhaustees have relatively low levels of disposable income compared to similar workers prior to layoff and that a significant share of them remain unemployed. This suggests that the insurance value of longer benefits in Brazil may be sizable. Incorporating these findings in our framework, we find that the welfare effects of extending UI in our setting are likely positive.

This paper extends a large theoretical and empirical literature on social insurance in developed countries.⁹ The closest paper to ours is perhaps Schmieder, von Wachter and Bender (2012), which investigates how the impact of UI extensions varies over the business cycle in Germany. Consistent with our findings, they estimate smaller efficiency costs during recessions when base reemployment rates are low. Our paper differs in a key way. Informality is limited in Germany. Moreover, booms and busts occur periodically, but formal employment is persistently low in developing countries and is expected to rise with economic development. We also derive a new formula for the welfare effects of UI extensions, which takes into account the nature of labor markets in developing countries. Further, we contribute to a growing literature at the intersection of public finance and development.¹⁰ A theoretical literature argues that efficiency considerations force governments to resort to alternative, second-best, policies where enforcement is weak and informality is high. However, there is little empirical evidence on the impact of typical policies in such countries (Gordon and Li, 2009). We find that the efficiency costs of a common social program are low in Brazil even

⁸Formal reemployment rates are also very low after layoff for non–eligible displaced formal workers. The low formal reemployment rates after UI exhaustion are thus unlikely to result from long–term effects of receiving UI in the preceding months. $\tilde{\eta}$ provides an upper bound on efficiency costs if the behavioral cost does not fully result from distortions (e.g., if "hiding" costs are inferior to the extra benefits for behavioral beneficiaries).

⁹Chetty and Finkelstein (2012) review the literature. Katz and Meyer (1990), Card and Levine (2000), and Landais (2012) empirically investigate the impact of UI extensions on benefit collection and formal reemployment rates in the US. As in most of the literature, we find no effect of UI extensions on subsequent match quality in the formal sector.

¹⁰See, for example, Niehaus and Sukhtankar (2012), Olken and Singhal (2011), or Pomeranz (2012).

though informality is prevalent.¹¹

The two main complementary views on labor informality in developing countries shed light on why our findings might prevail (Perry et al., 2007). In the traditional "exclusion" view, formal jobs are associated with high search costs (Harris and Todaro, 1970; Fields, 1975; Zenou, 2008). The mechanical cost is large and the behavioral cost small because workers are unable to find a formal job rapidly. A decrease in formal search costs then reduces the mechanical cost but increases the behavioral cost if beneficiaries still have the option to work informally. This rationalizes the finding that efficiency costs rise with formal employment rates. In the "exit" view, workers are voluntarily informal to avoid paying for benefits they may not value (Maloney, 1999; Levy, 2008). The mechanical cost is large and the behavioral cost small because workers are unwilling to return to a formal job rapidly with or without UI. Both views imply similarly low efficiency costs, but very different insurance values. Beneficiaries who prefer to work informally do not need insurance.

Finally, our approach and findings contribute to the nascent empirical literature on the impact of social programs in countries with high informality.¹² Existing studies do not typically link their results to standard public finance theoretical frameworks, complicating interpretation. We use such a framework to guide our empirical analysis; we provide new empirical evidence that allows us to directly estimate the efficiency costs from distorting incentives to return to a formal job; and we evaluate the resulting partial–equilibrium welfare effects. We are also the first paper to empirically estimate how behavioral responses to a social program vary with the size of the formal sector. In so doing, our results overturn the conventional wisdom that social programs are particularly distortive in the presence of informal work opportunities. Whether to extend UI is not a question of efficiency in our setting; it mostly depends on the social value of redistributing resources to UI exhaustees. Efficiency considerations in fact become more relevant as the formal sector expands.

The remainder of this paper is structured as follows. Section 1 provides some background and describes our data. Section 2 presents the conceptual framework that guides our analysis. Section 3 estimates the mechanical cost of UI extensions. Section 4 exploits two empirical strategies to estimate the behavioral cost and the efficiency costs of UI extensions. Section

¹¹Similarly, Kleven and Waseem (2012) find that (intensive margin) taxable income elasticities are low in Pakistan even though tax evasion is widespread.

¹²In addition to previously cited papers, Bérgolo and Cruces (2010), Camacho, Conover and Hoyos (2009), and Gasparini, Haimovich and Olivieri (2009) also focus on impacts at the formal-informal employment margin. We are aware of two working papers, developed in parallel to our work, attempting to estimate the impact of UI on some labor market outcomes in non–OECD countries (IADB, in progress). We are aware of three working papers on UI in Brazil that are mostly descriptive (Cunningham, 2000; Margolis, 2008; Hijzen, 2011). A related literature investigates the impact of UI in macro-labor models with an informal sector (Zenou, 2008; Ulyssea, 2010; Robalino, Zylberstajn and Robalino, 2011; Meghir, Narita and Robin, 2012). In practice, there is no need for insurance in these models as they assume risk neutral workers. Moreover, they cannot study moral hazard because they typically model UI as a lump-sum transfer that formal workers are entitled to upon layoff. Finally, on the benefit side, Chetty and Looney (2006, 2007) highlight the likely high value of social insurance in developing countries given households' difficulty at smoothing consumption after employment shocks.

5 uses survey data to estimate overall reemployment rates and disposable income of UI exhaustees. We then incorporate our results in our framework and evaluate welfare effects. Section 6 discusses other sources of efficiency gains or costs from UI that are not captured in our framework. Section 7 concludes.

1 Background and Data

1.1 Labor markets in Latin America and Brazil

Labor markets in Latin America and elsewhere are characterized by the coexistence of formal employees and informal workers. Formal employees typically work in jobs with strict regulation of working conditions (e.g., overtime pay, firing costs) and relatively high payroll taxes. In exchange, they are entitled to a series of benefits (e.g., pensions, disability) that they may or may not value. Informal workers, who pay no income or payroll taxes and are not eligible for these benefits, encompass employees in non–complying firms (mostly smaller firms) and most self–employed (mostly unskilled). The same firm may hire both formal and informal employees.¹³

In contrast to other developing countries, formal employment is well-defined in Brazil. Every worker has a working card. When the employer signs the working card, the employee becomes formal and her hiring is reported to the government. Brazilian labor laws are among the strictest in the region. Payroll taxes amount to over 35% of wages. Firing costs are also high. In 2009, 42% of working adults were formal private–sector employees, 23% informal employees, and 24% self–employed. Brazil is an extremely diverse country, however. Formal employment rates and average income per capita across Brazilian states over our sample years range from the bottom to the top of the cross–country distributions in South America today. Figure 1 shows that average formal employment rates by state in two recent time periods strongly correlate with average income per capita. In the cross–sections, formal employment rates increase by over 25 percentage points from the poorest to the richest states. In the last decade, both income per capita and formal employment rates also increased, but not uniformly. We use this variation to explore how the efficiency costs of UI extensions change with the relative size of the formal labor market.¹⁴

¹³The 2002 World Bank's Investment Climate Survey in Brazilian manufacturing asks participating firms about the share of unregistered workers a similar firm likely employs. The median answer is 30% for small firms. In this paper, a job is defined as informal if it escapes monitoring by the government. This is the relevant definition in our context. Informal jobs cannot be offered UI and UI agencies cannot identify beneficiaries working informally. It may be rational for the government to allow informal labor to exist, depending on the costs and benefits of enforcement. Appendix Figure A.1 compares the prevalence of informal labor across countries in Latin America.

¹⁴The variation in formal employment rates is displayed on maps in Appendix Figure A.2. We focus on formal employment rates because they capture variation in both employment and in its formality. Income per capita is more noisily measured and is not frequently measured at low disaggregation levels in Brazil. Unemployment dropped from 13% to 7% over the last decade, but unemployment is often poorly measured compared to formal employment. We provide more information on labor legislation in the Appendix.

Chapter 1: Informal Labor and the Cost of Social Programs

Early work on labor informality assumed that formal and informal sectors were segmented (Fields, 1975). In practice, longitudinal survey data reveal that there is no such clear segmentation. Many workers transit between formal and informal labor statuses over the course of their lives in Latin American countries (Bosch and Maloney, 2010). Formal jobs may still be more difficult to get than informal jobs (Meghir, Narita and Robin, 2012). Formal wages are on average higher, though there is a lot of heterogeneity. Some informal workers (mostly the self–employed) may thus be better off than in their alternative options in the formal sector (Botelho and Ponczek, 2011). The two main views on informality, that informal workers are excluded from formal jobs or that they voluntarily avoid formal employment, are recognized today as complementary (Perry et al., 2007).

1.2 The Brazilian Unemployment Insurance program

The Brazilian UI program has been in place since the mid–1980s and is quite sizable. UI expenditures amount to 2.5% of total eligible payroll, more than three times the corresponding US figure (www.dol.gov). Workers involuntarily displaced from a private formal job with at least six months of tenure at layoff are eligible for three to five monthly UI payments. Maximum benefit duration depends on accumulated tenure over the three years prior to layoff. In this paper, for data reasons, we restrict attention to workers with more than 24 months of tenure at layoff. They are eligible for five months of UI, after a 30–day waiting period.¹⁵

Benefit levels are based on the average wage in the three months prior to layoff. Replacement rates start at 100% at the bottom of the wage distribution but are down to 60% for workers who earned three times the minimum wage.¹⁶ There was no monitoring of beneficiaries' formal job-search efforts before 2011. Workers applied in person for UI benefits in the first month only. Payments were then automatically made available for withdrawal at Caixa Economica, an official bank, every 30 days as long as the worker's name did not appear in a database where employers report new hirings monthly (CAGED, Labor Ministry). In a companion paper, we argue that our results may also rationalize this complete absence of monitoring (Gerard and Gonzaga, 2013*b*). Finally, unemployment insurance is financed through a .65% tax on firms' total sales in Brazil.

1.3 Data

We mainly exploit two very large restricted access administrative datasets covering 15 years of Brazil's recent history. RAIS (Relação Anual de Informações Sociais) is a longitudinal

¹⁵Survey data only record tenure in the lost job, not accumulated tenure. Our first source of exogenous variation is a temporary UI extension that took place in 1996. Because our administrative data start in 1995, we cannot measure accumulated tenure in the previous three years. Tenure in the lost job, reported in both survey and administrative data, is a sufficient statistic for the UI eligibility of these workers only.

¹⁶The full schedule is presented in Appendix Figure A.3. Our results hold if we exclude beneficiaries with very high and very low replacement rates.

matched employee–employer dataset covering by law the universe of formally employed workers, including public employees. All tax-registered firms have to report every worker formally employed at some point during the previous calendar year.¹⁷ Every observation in RAIS is a worker–establishment pair in a given year. It includes information on wage, tenure, age, gender, education, sector of activity, establishment size and location, hiring and separation dates, and reason for separation. Because every worker is uniquely identified over time, we observe all spells in formal employment and between formal jobs for each individual. We currently have data from 1995 to 2010. There were 41 million formal employees at the end of 2009.

We are the first researchers to be granted access to the second administrative dataset, the Unemployment Insurance registry. It includes the month and amount paid for every UI payment made from 1995 to 2012. On average, there were 680,000 new beneficiaries each month in 2009. Beneficiaries are identified with the same ID number as in RAIS. The data has one main limitation. If the benefit collection period of a given worker spanned two different years, UI payments from the second year were not reported in the data before 2006. We thus restrict attention to workers who start collecting benefits in the first six months of the year to avoid truncation issues in UI spells. Formal reemployment patterns based on RAIS are similar for workers displaced throughout the year.¹⁸

Finally, we exploit monthly urban labor force surveys (Pesquisa Mensal de Emprego, PME, 2003–2010) conducted by the Instituto Brasileiro de Geografia e Estatística (IBGE). PME has the same structure as the Current Population Surveys in the US. Households enter the sample for two periods of four consecutive months, eight months apart from each other. PME covers the six largest urban areas of Brazil and is used to compute official employment statistics. Each survey asks for the labor market status of every household member above ten years old, information on wage, and tenure in the job. Formality is captured by asking whether her employer signed the respondent's working card. The unemployed, whether or not searching for a job, are asked about their labor status and tenure in the last job, the reason for separation, and the length of their unemployment spell (in months). State–level formal employment rates are obtained from yearly household surveys (PNAD, Pesquisa Nacional por Amostra de Domicílios), also conducted by IBGE.

¹⁷The main purpose of RAIS is to administer a federal wage supplement (Abono Salarial) to formal employees. There are thus incentives for truthful reporting. RAIS has also been increasingly used by ministries administering other social programs to monitor formal job take-up. RAIS actually has better coverage of formal employment than the data used by the UI agency (MTE, 2008). Accordingly, we observe a few formally reemployed workers still collecting UI. As a result, our results slightly overestimate efficiency costs.

¹⁸We also obtain similar results for the impact of UI on formal reemployment rates (unconditional on UI take–up) for workers displaced throughout the year (available upon request). About 2% of ID numbers are also missing for the earlier years in the data.

2 Costs and Benefits of UI extensions: a framework

This section presents the framework that guides our empirical analysis. We build on the canonical Baily model for the optimal social insurance benefit levels in the presence of moral hazard (Baily, 1978). This allows us to focus on the tradeoff between the need for insurance and the efficiency costs from distorting incentives to return to a formal job. We introduce informal work opportunities in a dynamic partial–equilibrium model of endogenous job search (Chetty, 2006, 2008; Schmieder, von Wachter and Bender, 2012). We then show that the efficiency costs of UI extensions are captured by a pseudo–elasticity, defined as the ratio of a *behavioral* cost to a *mechanical* cost.¹⁹ The former measures the increase in benefit duration absent behavioral responses. The ratio measures the fraction of social spending lost through behavioral responses. A UI extension increases welfare if the social value of the income transfer to UI exhaustees exceeds this pseudo–elasticity. We focus on the intuition for the main results. The model and its derivations are in the Appendix.

Agent's Problem. The model describes optimal behavior of a representative worker who cycles in and out of formal employment. It captures both views on informal labor markets. On the one hand, formal jobs may be associated with high search costs (Fields, 1975; Zenou, 2008). On the other hand, informal jobs may be attractive (Maloney, 1999). The worker faces a fixed layoff probability q in the formal sector such that, on average, she stays employed $D^f = \frac{1}{q}$ periods. She earns formal wage w^f each period. Upon layoff, she becomes unemployed and eligible for UI for a maximum benefit duration of P periods. UI benefits b_t are defined as $b_t = rw^f$, with replacement rate r for period t = 1, 2, ..., P after layoff, and $b_t = 0$ otherwise.

While unemployed, she decides each period how much overall search effort e at a cost z(e) to invest in finding a new job. Search efforts are normalized to correspond to jobfinding probabilities. Cost functions are assumed to be convex. With probability 1 - e, she does not find a job and stays unemployed. With probability e, she finds a job. She can increase her probability of returning to a formal job by investing formal search effort fat a cost $\theta z(f)$. She thus finds a formal job with probability ef and an informal job with probability e(1 - f). She earns wage $w^i < w^f$ when working informally and can always search for a formal job at a cost $\theta z(f)$ in subsequent periods. We introduce enforcement in the model by assuming that informal jobs are detected by the government with probability p. If detected, an informal worker falls back into unemployment and loses her UI benefits. In many developing countries, detection probabilities p are low. Both the unemployed and the "undetected" informally employed draw UI benefits in the first P periods after layoff.

¹⁹As discussed in (Chetty, 2006), the measure of efficiency costs and the welfare formula derived in such a model are robust to relaxing many assumptions (such as introducing heterogeneity) or to introducing other margins of behaviors (endogenous savings accumulation and depletion, reservation wages, spousal labor supply, human capital decisions, job–search quality) as long as an envelope condition applies to the agents' problem. We discuss mechanisms beyond the scope of our framework (e.g., general equilibrium effects, fiscal externalities) in Section 6.

The choice situation is illustrated in Figure 2a.

The traditional view of informality implies high values of θ (high formal search costs). The more recent view corresponds to low values of θ and small wage differentials. We do not observe search costs empirically. Table 1 displays average net earnings upon reemployment in Brazil for displaced formal workers who are reemployed formally or informally in the first five months after layoff, relative to the average net earnings of comparable formal workers before layoff. The sample is restricted to workers eligible for five months of UI after layoff. The data come from repeated cross-sections of monthly urban labor force surveys (PME). The informally reemployed experience much lower earning levels than the formally reemployed (column 1), even controlling for gender, year, calendar-month, and area fixed effects (column 2). The difference is only slightly smaller controlling for education levels, age and tenure (column 3). However, workers may be willing to take these lower paid informal jobs while drawing UI benefits.²⁰

The workers' problem is to choose optimal levels of search intensity of both types in each period until formal reemployment. The solution to this dynamic problem determines the survival rate out of formal employment S_t in each period t after layoff, and thus the average duration between formal employment spells D^u and the average benefit duration $B \equiv \sum_{t=1}^{P} S_t$.

Mechanical and behavioral costs of UI extensions. Following (Schmieder, von Wachter and Bender, 2012), we assume that P can be increased by a fraction of one such that a marginal change in P can be analyzed. A marginal change in P then corresponds to a marginal change in b_{P+1} , the benefit amount after regular UI exhaustion, times $b (\equiv rw^f)$.

Extending the maximum UI duration by one period (dP) increases average benefit duration, and UI costs, through two channels. This is illustrated in Figure 2b. First, there is a mechanical cost. In absence of the extension, some workers would not have been formally reemployed after regular UI exhaustion. These workers (unemployed or informally employed) will draw the additional benefits without changing their behavior, increasing the average benefit duration B by S_{P+1} and UI costs by bS_{P+1} . Second, there is a behavioral cost, the increase in average benefit duration due to behavioral responses. Extending UI benefits reduces incentives to be formally reemployed. It reduces both overall search effort $(e \downarrow)$ and formal search effort $(f \downarrow)$ in period P + 1 and potentially in earlier periods as well. As a consequence, it increases average benefit duration B by $\sum_{t=1}^{P+1} \frac{dS_t}{dP}$ and UI costs by $b\sum_{t=1}^{P+1} \frac{dS_t}{dP}$. The cost of extending UI is the sum of the behavioral and the mechanical

²⁰Our model describes the situation of a representative worker. The literature finds that the traditional view better applies to informal employees and the more recent view to the self-employed (Bosch and Maloney, 2010). We find that most of the beneficiaries (re)employed in the informal sector are informal employees (67.5%) rather than self-employed (PME surveys). Our main conclusions are unaffected if workers receive heterogeneous wage offers in both sectors. Workers with high informal wages would simply never return to a formal job (as long as p is low). They would draw UI benefits, but would not change their behavior in response to UI extensions, and would therefore not generate efficiency costs. Our main conclusions are also robust to assuming that formally reemployed workers can pay some convex evasion costs to hide their new formal job, and that informal jobs can be lost.

costs.

Planner's Problem. The social planner's objective is to choose the maximum benefit duration P that maximizes welfare W, which is a weighted sum of individual utilities, such that a balanced-budget constraint holds. We focus on the planner's problem in the steady state of the dynamic model. In the steady state, a share $\frac{D^f}{D^f + D^u}$ is formally employed each period, a share $q \frac{D^f}{D^f + D^u}$ becomes eligible for UI, and a share $q \frac{D^f}{D^f + D^u} B$ draws UI benefits. UI taxes τ are typically levied on formal employees.²¹ A balanced-budget constraint must then satisfy:

$$\frac{D^f}{D^f + D^u} \tau w^f = q \frac{D^f}{D^f + D^u} Bb$$

$$\tau = qrB \tag{1}$$

Given q and r, equation (1) shows that changes in UI costs, and the resulting UI tax rate τ , are only driven by changes in average benefit duration B in our setting.²²

As workers choose search efforts (e, f) optimally, we use the envelope theorem to solve the planner's problem. The welfare effect of increasing P by one period is (first–order condition):

$$\frac{dW}{dP} = q \frac{D^f}{D^f + D^u} S_{P+1} b g^{U_{P+1}} - \frac{D^f}{D^f + D^u} w^f \frac{d\tau}{dP} g^E$$
$$\frac{dW}{dP} = q \frac{D^f}{D^f + D^u} r w^f S_{P+1} g^{U_{P+1}} - q \frac{D^f}{D^f + D^u} r w^f \left[S_{P+1} + \sum_{t=0}^{P+1} \frac{dS_t}{dP} \right] g^E$$
(2)

The first term in equation (2) is the welfare gain of the S_{P+1} displaced formal employees who would not have been formally reemployed absent the extension and now receive an additional benefit *b* (mechanical cost). $g^{U_{P+1}}$ denotes the average social value of \$1 for these UI exhaustees (unemployed or informally employed). There are no welfare gains from

²¹The incidence of sales and labor taxes may be similar and the fact that UI is financed through a sales tax in Brazil may not alter the analysis. We focus on the more typical financing of UI for our framework to apply beyond the Brazilian case. If the incidence of a sales tax falls on buyers (resp. sellers) instead of workers, g^E below becomes the average social value of \$1 for buyers (resp. sellers) of formal goods and services.

²²In particular, the change in the overall duration out of formal employment D^u following a UI extension, has no additional effect on the budget constraint. If D^u increases, it reduces the number of individuals paying UI taxes, but also the number of future beneficiaries. The two effects on the UI budget cancel out in the steady-state. Chetty (2008) and Schmieder, von Wachter and Bender (2012) assume instead that new jobs are never lost. Therefore, their model emphasizes the impact of UI extensions on the overall duration out of formal employment D^u because of a reduction in UI tax revenues. We adopt a steady-state approach (infinite horizon) because a significant share of the formally reemployed is laid off again in the following months in Brazil. We show empirically that UI extensions not only reduce the number of months formally employed in the two years after layoff but also the share experiencing a new layoff from the formal sector (Table A.8). We follow the literature by assuming a fixed layoff probability q. This assumes sufficient experience rating of benefits such that changes in UI have no effect at the layoff margin. We show that this assumption holds for the group of workers we consider.

beneficiaries drawing additional benefits because of behavioral responses (envelope theorem). To satisfy the budget constraint, the UI tax τ on formal wages must increase to finance the cost of the UI extension, or the sum of the behavioral and the mechanical costs. The second term in equation (2) captures the welfare loss from the tax increase for formal employees. g^E denotes the average social value of \$1 for formal employees. The social values, $g^{U_{P+1}}$ and g^E , depend on individuals' marginal utilities and on social planner preferences towards redistribution.

Reorganizing, we obtain:

$$\frac{1}{\frac{D^{f}}{D^{f} + D^{u}}} \frac{dW/dP}{g^{E}w^{f}} = qr \ S_{P+1} \left[\frac{g^{U_{P+1}} - g^{E}}{g^{E}} - \tilde{\eta} \right]$$
(3)

where $\tilde{\eta} \equiv \sum_{t=1}^{P+1} \frac{dS_t}{dP}/S_{P+1}$ is the ratio of the behavioral cost to the mechanical cost. Dividing by $\frac{D^f}{D^f+D^u}w^fg^E$, equation (3) expresses the welfare effects of a UI extension in terms of a money metric, the welfare gains from a percentage increase in the formal wage. Equation (3) shows the trade-off between insurance and efficiency. The first term in brackets, the *social* value of insurance $\frac{g^{U_{P+1}-g^E}}{g^E}$, measures the social value of transferring \$1 from the average taxpayer to the average UI exhaustee. The second term, the pseudo-elasticity $\tilde{\eta}$, measures the resources lost for each \$1 transferred to UI exhaustees.²³ If the average social value of \$1 is 20% larger for UI exhaustees than for taxpayers, a UI extension increases welfare as long as less than 20 cents are lost through behavioral responses for each \$1 transferred to UI exhaustees. At an optimum, these two terms must be equal.²⁴ Neither the social value of insurance nor the pseudo-elasticity $\tilde{\eta}$ are structural parameters. Evaluating equation (3) around the existing UI program, however, provides a local welfare test. From Figure 2b, the efficiency costs of UI extensions are likely increasing in the maximum benefit duration because survival rates are decreasing. In our setting, the social value of insurance is decreasing in the existing maximum benefit duration because more beneficiaries get informal jobs. Therefore, if equation (3) is positive around the existing program, UI should be extended in our setting.²⁵

 $^{^{23}}$ In the Baily model, the ratio of the behavioral to the mechanical cost corresponds to an elasticity.

²⁴That the ratio of a behavioral to a mechanical cost measures efficiency costs is a common result in public finance. The mechanical effect on government revenues of increasing the income tax, for instance, corresponds to the tax base ex-ante. The behavioral effect corresponds to the change in the tax base due to the tax increase. Their ratio, equal to the marginal deadweight burden of the tax increase, captures efficiency costs (Saez, Slemrod and Giertz, 2012). If part of the behavioral response is not due to any costly behavior (e.g., costless reporting behaviors), it generates no efficiency cost. In this case, the measure of efficiency cost we derive is an upper bound. However, it is unlikely that misreporting entails no cost for both workers and employers.

²⁵If beneficiaries have savings to deplete, the social value of insurance may not be monotonically decreasing. There is no data on savings for UI beneficiaries in Brazil. Another concern is that formal reemployment patterns after layoff in the absence of UI may differ from formal reemployment patterns after UI exhaustion. For instance, one could imagine a model where displaced formal workers would rapidly return to formal sector jobs in the absence of UI but switch to, and stay in, informal jobs when UI benefits are offered. The behavioral cost of introducing a small UI program could then be larger than the behavioral cost of extending

Chapter 1: Informal Labor and the Cost of Social Programs

Connecting theory to the data. To estimate efficiency costs $\tilde{\eta}$, we do not need to observe responses in overall (e) and formal search efforts (f) separately. The relevant combined response, formal reemployment, is recorded in administrative data. We capture the mechanical cost by estimating the exhaustion rate of regular UI benefits and how rapidly beneficiaries return to a formal job after regular UI exhaustion (Section 3). We capture the behavioral cost by estimating the change in the survival rates out of formal employment following an exogenous UI extension, up to the new maximum benefit duration (Section 4). We provide suggestive evidence for the social value of insurance using longitudinal survey data (Section 5). The survey data also allow us to estimate overall reemployment rates and compare them to formal reemployment rates. Differences must be due to beneficiaries (re)employed in the informal sector.

Efficiency, welfare and informality. A 13-week UI extension has been estimated to increase regular benefit duration (about 26 weeks), by one week (Card and Levine, 2000) and total benefit duration by 2.1–3 weeks (Katz and Meyer, 1990) on average in the US. Katz and Meyer (1990) estimate that 43% of the increase in benefit duration is due to a mechanical cost, or $\tilde{\eta} > 1$.

How would the cost of UI extensions differ in labor markets with a smaller formal sector? The conventional wisdom is that UI formal work disincentives (moral hazard) will be exacerbated. Many workers will delay formal reemployment and choose to work informally while drawing benefits. This is possible because the probability of being detected working informally, p, is low. The behavioral cost will be large, increasing both total costs and efficiency costs. This line of thinking assumes, however, that workers would be formally reemployed rapidly absent UI (small mechanical cost) and that there is a strong link between informality levels and the size of the response at the margin.

Instead, low formal employment rates may indicate high formal search costs ($\theta \uparrow$, traditional view) or low returns from formal search ($w^f \downarrow$, more recent view). In either case, workers will not be formally reemployed rapidly absent UI. The mechanical cost will be large. The behavioral cost, in contrast, will be small. A given change in benefits, for instance, has a smaller impact on formal search effort when formal search costs are high. A decrease in formal search costs will then *reduce the mechanical cost* and *increase the behavioral cost* if beneficiaries still have the option to work informally. This rationalizes our empirical find-

an existing program. Figure A.4 suggests that such a concern is limited. In Brazil, displaced formal workers must have at least six months of tenure at layoff to be eligible for UI. The maximum benefit duration then depends on the accumulated tenure over the previous three years. Figure A.4a displays the unconditional average benefit duration by tenure prior to layoff (in months) for a random sample of formal workers displaced between 2002 and 2009. Average benefit duration is very low for workers with low tenure levels who are, in theory, not eligible for UI benefits. Figure A.4b displays survival rates out of formal employment for four tenure categories. Displaced formal workers with low tenure levels return more rapidly to a formal job in the first few months after layoff. However, their survival rates remain high. About 40% of displaced formal workers in each category are still out of the formal sector 12 months after layoff. Clearly, the behavioral cost of offering some UI to currently non–eligible workers would also be small compared to the corresponding mechanical cost.

ings.²⁶ Both cases are cost–equivalent. Yet, the social value of insurance is likely small if UI exhaustees are informally employed with significant income levels. Our empirical evidence in Section 5 suggests the opposite. UI exhaustees have relatively low levels of disposable income compared to formal employees prior to layoff.

3 Estimating the mechanical cost of UI extensions

In the previous section, we derived that the efficiency costs of UI extensions depend on the ratio of a behavioral cost (the cost of UI extensions due to behavioral responses) to a mechanical cost (the cost absent any behavioral response). The first step of our empirical analysis estimates the mechanical cost for beneficiaries eligible for five months of UI in Brazil. By observing their formal reemployment rates after UI exhaustion, we measure how many beneficiaries would draw additional UI payments following a hypothetical two–month UI extension, absent any behavioral response. We also estimate how the mechanical cost varies with the relative size of the formal labor market using variation in formal employment rates across regions and time. We find that (i) the mechanical cost is large and (ii) that it decreases with formal employment rates.

We proceed as follows. First, we draw a random sample of workers eligible for five months of UI in every year between 1995 and 2009. Our sample includes full-time private-sector formal employees 18–54 years old with more than 24 months of tenure at layoff. Because of data limitations detailed in Section 1.3, we use only workers laid off between January and June. We oversample less formal labor markets to have enough observations at low levels of formal employment.

Second, we use workers' formal reemployment patterns to measure how many additional UI payments they would mechanically draw following a hypothetical two-month UI extension. We assume that workers who exhaust their regular UI benefits and are not formally reemployed within one month (resp. two months) of regular UI exhaustion would draw one extra payment (resp. two extra payments). The mechanical cost for a given beneficiary is the difference between her hypothetical extended benefit duration and her regular (no extension) benefit duration. We use individual data in order to control for composition effects across labor markets.²⁷

 $\mathbb{1}$ (exhaust regular UI benefits) $\times \sum_{j=1}^{2} \mathbb{1}$ (month_{back} > month_{regUI} + j)

²⁶The comparative statics are discussed in the Appendix. The ability to work informally may also decrease $(p \uparrow)$ when formal employment rates rise. In this case, both the mechanical cost (more difficult to work outside the formal sector) and the behavioral cost (more costly to delay formal reemployment) may decrease, with ambiguous effects on economic efficiency. The relationship between efficiency and formal employment rates is thus an empirical question.

²⁷For example, women's share of the formal labor force is positively correlated with formal employment rates. Define $month_{regUI}$, the month a beneficiary exhausts her regular benefits. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, the mechanical cost of a hypothetical two–month UI extension is:

Chapter 1: Informal Labor and the Cost of Social Programs

Third, we construct yearly formal employment rates for 137 mesoregions (*mesorregiões*), the second largest geographical subdivision in the country (after the 27 states), defined as groups of spatially articulated municipalities with similar socio-economic characteristics. Because mesoregions are not identified in yearly surveys, we use RAIS data to construct formal employment rates. We divide the average number of formal employees by official population estimates (IBGE) in each year in each mesoregion. We also use state–level formal employment rates from PNAD.

Finally, for individual i in mesoregion m in year t, we regress:

$$y_{i,m,t} = \alpha_m + \beta_t + \gamma \ Formal Employment Rate_{m,t} + X_{i,m,t} + \epsilon_{i,m,t} \tag{4}$$

Our main outcome of interest is the mechanical cost of a hypothetical two-month UI extension. We also consider other outcomes to better describe benefit collection and reemployment patterns in Brazil: UI take-up, regular benefit duration, and the probability of staying out of formal employment more than seven months after layoff. We present results from specifications with and without year fixed effects (β_t), mesoregion fixed effects (α_m) and a rich set of individual controls ($X_{i,m,t}$). Standard errors $\epsilon_{i,m,t}$ are clustered by mesoregion.

3.1 Graphical results

Figure 3 illustrates our main results. It displays formal reemployment patterns for workers eligible for five months of UI after losing a formal job in 2009 in Pernambuco, a poor state with low formal employment rates, or Rio Grande do Sul, a richer state with higher formal employment rates. Hazard rates of formal reemployment are below 4% a month in both states while workers draw UI benefits. They spike to 12%–18% a month after UI exhaustion, increasing relatively more in Rio Grande do Sul. Formal reemployment rates stay quite low, however, even after UI exhaustion. About 40% of workers are still out of formal employment 12 months after layoff. The spike in formal reemployment at UI exhaustion suggests a clear behavioral response to the incentives of the UI program.²⁸ In Section 4, we show that the spike is completely shifted following exogenous UI extensions. Nevertheless, the size of the behavioral cost is small compared to the mechanical cost. If UI was extended by two months in Figure 3, most beneficiaries (70%–80%) would mechanically collect additional UI payments, absent any behavioral cost larger in Rio Grande do Sul because the spike is larger. This suggests that efficiency costs rise with formal employment rates.

Figure 4 displays more systematic results. Each observation is a state average in a given year from 2002 to 2009. The left panel displays the relationship between regular benefit

In Table A.7, we use actual UI extensions and test (successfully) whether we accurately predict the increase in average benefit duration using workers' formal reemployment patterns after regular UI exhaustion in this way.

 $^{^{28}}$ Such a spike is not observed in most developed countries (Card, Chetty and Weber, 2007b). van Ours and Vodopivec (2006) find a sizeable spike in Slovenia.

duration for workers eligible for five months of UI and state-level formal employment rates. Average benefit duration decreases slightly with formal employment rates but remains very high at any level. Beneficiaries draw on average 4.85 to 4.95 months of UI. In comparison, beneficiaries eligible for 26 weeks of UI in the US drew on average 16 weekly UI payments over the same period (www.dol.gov). Average benefit duration is much higher in Brazil. High exhaustion rates have also been documented in Argentina (IADB, in progress) and China (Vodopivec and Tong, 2008).

The right panel displays the relationship between the mechanical cost of a hypothetical two-month UI extension for the same workers and state-level formal employment rates. Formal reemployment rates increase after UI exhaustion but remain low. As a consequence, extending UI by two months would be costly in Brazil absent any behavioral response. The mechanical cost varies from 1.75 months in states with low formal employment rates to 1.4 months in states with high formal employment rates. The relationship is negative because the magnitude of the spike in formal reemployment after UI exhaustion increases with the relative size of the formal labor market.²⁹

3.2 Regression results

We turn to a regression analysis to further investigate the relationship between the mechanical cost of UI extensions and formal employment rates. This allows us to control for general time trends, fixed differences across labor markets, and composition effects.

Table 2 reproduces the estimated coefficients on formal employment rates by mesoregion $(\hat{\gamma})$ for different outcomes and different specifications of equation (4). The mechanical cost of a hypothetical two-month UI extension (row 3) is high on average, at 1.67 months. The mechanical cost is large because most beneficiaries exhaust their five months of UI (regular benefit duration is 4.93 on average, row 2) and because 73% of beneficiaries are still out of the formal sector seven months after layoff (row 4). The mechanical cost decreases with formal employment rates. Estimates are larger in absolute value when using the full variation in formal employment rates (column 1), but they are similar when we include year fixed effects or both year and mesoregion fixed effects (columns 2 and 3). The relationship is not due to fixed differences across regions; it holds for marginal changes in formal employment rates. Moreover, the relationship is not simply due to composition effects. Controlling for a rich set of covariates, including wage and sector of activity, has no effect on our results (column 4). This latter estimate implies that increasing formal employment rates by 30 percentage points decreases the mechanical cost of a hypothetical two-month UI extension by .2 month or 12% (and regular benefit duration by only 1%).

A concern is that UI take-up is also correlated with formal employment rates (row 1), potentially creating selection issues when we consider only UI takers as above. The negative

²⁹If this equilibrium relationship is intuitive, it is nevertheless not trivial. Higher formal employment rates in a given labor market could also be due to lower separation rates in the formal sector, higher separation rates in the informal sector, or higher formal reemployment rates on average but not specifically in the first months after layoff.

relationship in columns (1) and (2) likely implies negative selection (UI takers are relatively less likely to return rapidly to a formal job) while the positive relationship in columns (3) and (4) likely implies positive selection (UI takers are relatively more likely to return rapidly to a formal job).³⁰ Yet, our main results are consistent across specifications and are robust to the inclusion of a rich set of individual controls. Such a concern is thus limited.

Our results hold using state-level formal employment rates, using only years after 2002, or including only mesoregions with average formal employment rates between the 5th and the 95th percentile (Appendix Table A.1). Taken together, they show that beneficiaries' propensity to return rapidly to a formal job after UI exhaustion is systematically higher where the formal sector is relatively larger, and it rises with formal employment rates. As a consequence, the mechanical cost of a UI extension decreases with formal employment rates, but the *potential* behavioral cost increases. There cannot be much distortion if beneficiaries are unwilling or unable to join the formal sector rapidly. How much of this *potential* behavioral cost translates into an *actual* behavioral cost is a question we address in the next section.

4 Estimating the behavioral cost of UI extensions

In this section, we use exogenous variation in maximum benefit duration to estimate the behavioral cost of UI extensions. We show that (i) the spike in formal reemployment at benefit exhaustion is fully shifted following UI extensions, (ii) the behavioral cost is small, however, compared to the mechanical cost, and efficiency costs are thus limited, and (iii) the behavioral cost increases with formal employment rates and, combined with a smaller mechanical cost, efficiency costs therefore rise with formal employment rates. Our first empirical strategy illustrates all these results using a temporary two-month UI extension in 1996 (difference-in-difference) and cross-sectional variation in the relative size of the formal sector across cities. Our second empirical strategy, a tenure-based discontinuity in eligibility, confirms our results. It provides local variation in maximum benefit duration (one month) in every year and in every labor market. Our results thus hold using variation in formal employment rates across regions over time.

4.1 The 1996 temporary UI extension

Beneficiaries who exhausted their regular UI benefits between September and November 1996 in specific urban areas were eligible for two additional months of UI. Importantly, the UI extension was politically motivated and the differential implementation was unrelated

³⁰UI take–up is high in Brazil: on average 86% of our eligible workers collect a first UI payment rapidly after layoff. The negative relationship is due to the 30–day waiting period: if the propensity to be formally reemployed increases with formal employment rates, workers are less likely to stay out of the formal sector in the first 30 days. More surprisingly, the relationship becomes positive when mesoregion fixed effects are included. Take–up rates were increasing over time and increased more where formal employment rates increased relatively more. We are currently investigating potential mechanisms behind this correlation.

to local labor market conditions. A UI extension for the city of São Paulo was proposed to the president by Jose Serra, a politician from the same political party (PSDB) who was struggling in his run for mayor of São Paulo. Jose Serra justified his proposal by the rising unemployment in the city. In response, workers' representatives defended a UI extension in all cities, arguing that "unemployment is increasing everywhere, not only where the PSDB candidate is doing badly" (Folha de São Paulo, 08/22/1996). This proposition was rejected because a national extension would have cost more than the budget threshold to avoid a parliamentary process. As a compromise, the UI extension was implemented in the nine historical metropolitan areas of the country and the Federal District.³¹ Unemployment was mildly increasing in 1996; it was higher in 1997 when no extension took place.

The timeline of the experiment is summarized in Figure 5. On August 14, the extension was first proposed. It was adopted a week later, on August 21, to start on September 1, 33 days before the first round of local elections. Formal employees displaced in April or May, and eligible for five months of UI, learned in August that they would be eligible for two additional months of UI after exhaustion of their regular benefits. No extra UI payment would be paid after December 31, so workers laid off in June could only draw one additional month of UI. The timing guarantees that workers could not be strategically laid off. It may also prevent us from estimating anticipation behaviors in the first months after layoff. In practice, nearly 100% of beneficiaries exhausted their full five months of UI in these years. There is thus no room for anticipation to matter.

We adopt a difference-in-difference strategy. Our sample includes full-time privatesector formal employees 18–54 years old, laid off in April or May, and eligible for five months of regular UI benefits (more than 24 months of tenure at layoff). We use 1995 and 1997 as control years. We have nine treatment areas since we exclude São Paulo to reinforce the exogeneity of our cross-sectional variation. We use all the urban centers granted the status of metropolitan area since 1996 as control areas (20). In total, we have about 230,000 workers. There are a few differences between control and treatment areas but these differences appear every year. Treatment and control areas are spread over the country and spanned a similar range of formal employment rates in these years. The distribution and composition of the sample are presented in Appendix Tables A.2 and A.3.³²

³¹Bélem, Belo Horizonte, Curitiba, Fortaleza, Porto Alegre, Recife, Rio de Janeiro, Salvador, and São Paulo. "the choice of the first nine metropolitan regions (in the 1970s) was more related to the objective of developing an urban system in the country according to the needs of a particular economic development strategy than to contemplating cities with actual characteristics of metropolitan regions. The proof of this claim was that Santos, Goiania and Campinas did not become metropolitan regions at that time, despite meeting some of the most important criteria to be considered a metropolitan area" (Guimarães, 2004, translation by the authors).

³²Workers in treatment areas are more likely to be older and to come from the service sector. Treatment areas are relatively larger, constituting 68% of the sample (22% of the sample is composed of workers from Rio de Janeiro). Control and treatment areas are displayed on a map in Appendix Figure A.5.

Graphical results

Our results can be seen graphically. Figure 6 displays survival rates out of formal employment and hazard rates of formal reemployment for UI takers in control and treatment areas in 1995, 1996, and 1997. Lines traced each other very closely in control areas or control years. But in 1996, in treatment areas, the spike in formal reemployment at regular UI exhaustion shifted by exactly two months. An additional 15% of workers were out of formal employment seven months after layoff. Survival rates out of formal employment for UI non-takers present no differential trend, supporting our identifying assumption of a common trend absent the UI extension (Appendix Figure A.6).

Figure 7 presents similar graphs for two treatment cities, Recife (Pernambuco) and Porto Alegre (Rio Grande do Sul), with formal employment rates around 24% and 35% at the time, respectively. In Recife and Porto Alegre in control years, hazard rates of formal reemployment at regular UI exhaustion spiked at 8% and 12%, respectively. In both cities, the spike shifted by exactly two months in 1996. Therefore, the mechanical cost of the UI extension was smaller but the behavioral cost larger in Porto Alegre, the city with a relatively larger formal sector.

Regression results

In the regression analysis, we estimate the following difference–in–difference specification for individual i from area m in year t:

$$y_{i,m,t} = \alpha_m + \beta_t + \gamma \left[Year 1996_t \times TreatArea_m \right] + \epsilon_{i,m,t} \tag{5}$$

where α is an area fixed effect and β a year fixed effect. γ is a difference–in–difference estimator for the impact of the UI extension on outcome y under a common–trend assumption. Estimates of $\hat{\gamma}$ are reported in Table 3. ϵ is an error term clustered by area.³³ We consider two outcomes using only the UI registry data, regular UI duration (first five months) and total benefit duration (up to seven months, columns 2 and 3). We also verify that we do not find an effect on UI take–up, a decision taken before the extension was announced (column 1). The behavioral cost is the difference between the total benefit duration of treatment workers and the benefit duration of the same workers had they not responded to the incentives of the UI extension (their mechanical cost). To capture such a counterfactual, we construct a new variable (columns 4 and 5) using workers' formal reemployment patterns to infer how many UI payments they would have collected had they all been eligible for seven months of UI. If they exhausted regular UI benefits, we assume that workers not formally reemployed within one month of exhaustion (resp. two months) would have collected one extra payment (resp. two extra payments). The mean in control years captures the mechanical cost of the UI extension; the difference–in–difference measures the behavioral cost.³⁴

³³Significance levels are similar if we bootstrap t–statistics by resampling our 29 clusters.

³⁴Define $month_{regUI}$, the month a beneficiary exhausts her regular benefits. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, this variable is defined as:

To estimate how the behavioral cost varies with formal employment rates, we use the following specification for the same outcome:³⁵

 $y_{i,m,t} = \alpha_m + \beta_t + \gamma \left[Year1996_t \times TreatArea_m\right] + \delta \left[Year1996_t \times FormalEmploymentAbove_m\right] + \zeta \left[Year1996_t \times TreatArea_m \times FormalEmploymentAbove_m\right] + X_{i,m,t} + \epsilon_{i,m,t}$ (6)

Both γ and ζ are reported in column (5). They capture the behavioral cost in areas with below average formal employment rates and the differential cost in areas with above average formal employment rates, respectively.

We find no effect on UI take-up or regular benefit duration (columns 1 and 2). At the time, beneficiaries collected on average 4.98 months out of their five months of UI. We would thus not have been able to find an effect on regular benefit duration even if beneficiaries had learned about the extension upon layoff. The extension increased benefit duration by 1.87 months in treatment areas in 1996 (column 3). We estimate that only 13% of that increase, .25 month, is due to behavioral responses (column 4). Indeed, had they been eligible for seven UI payments, beneficiaries in control years would have collected 1.58 (6.56-4.98) additional months of UI absent any behavioral response (mechanical cost). The behavioral cost is 40% larger, .08 month, in areas with a relatively larger formal sector (column 5). We use our estimates to quantify the efficiency costs $\tilde{\eta}$ in the bottom panel in Table 3. Because of the large mechanical cost, $\tilde{\eta}$ is relatively small, ranging from .12 to .175. In comparison, Katz and Meyer (1990) estimate $\tilde{\eta} > 1$ following a 13-week UI extension in the US. Efficiency costs increase by 45% from areas with low to high formal employment rates; the mechanical cost decreases by 5% and the behavioral cost increases by 40%.

We study the heterogeneity in our results in a companion paper (Gerard and Gonzaga, 2013b). The efficiency costs are larger for males and smaller for older, more educated, and more tenured workers. There is a nonlinear relationship with wages and firm size at layoff. Results are identical if we include a rich set of individual controls, if we exclude observations from Rio de Janeiro, if we restrict attention to workers with replacement rates between 20% and 80%, and if we use formal employment rates linearly (Appendix Table A.4).³⁶ They are also robust to using either one of the control years (available upon request). Finally, in Appendix Table A.8, we show that the UI extension decreased the number of months of formal employment in the two years after layoff but also the probability that workers experience a new layoff from the formal sector. These results motivate the steady state budget constraint in Section 2. We also find no effect on subsequent match quality in the formal sector (wage).

1 (exhaust regular UI benefits) × $\sum_{j=1}^{2} 1$ (month_{back} > month_{regUI} + j)

In Appendix Table A.7, we test (successfully) whether we accurately predict the increase in average benefit duration using workers' formal reemployment patterns after regular UI exhaustion in this way.

³⁵The indicator for above average formal employment does not enter the specification directly because we average formal employment rates over the three years. Our measures are based on yearly household surveys representative at the national level (PNAD). We average out formal employment rates over the years to increase the number of observations per area in the surveys.

³⁶We favor the use of two formal employment categories because of the small number of areas.

4.2 A tenure–based discontinuity in eligibility

Using the 1996 temporary UI extension, we showed that there is a behavioral cost of UI extensions but that it amounts to a small share of the increase in benefit duration. The resulting efficiency costs are thus small. We also established that efficiency costs rise with formal employment rates, based on cross–sectional variation across labor markets. Our second empirical strategy confirms these findings. Moreover, it allows us to show that the relationship between efficiency costs and formal employment rates holds using variation across regions over time. In Brazil, maximum benefit duration depends on accumulated tenure over the three years prior to layoff or since the last UI payments. Workers with more than 6, 12, and 24 months of accumulated tenure are eligible for 3, 4, and 5 months of UI, respectively. As discussed in the Appendix (Figure A.7), the distribution of tenure at layoff is only continuous around the third cutoff. In this section, we exploit the change in eligibility around this cutoff in a regression discontinuity design. This provides us with local variation in maximum benefit duration (one month) in every year and in every labor market.

Sample selection

We focus on formal workers who had no other formal job in the previous three years because accumulated tenure is measured with noise.³⁷ In this sample, workers with more than 24 months and less than 22 months of tenure at layoff are eligible for five months and four months of UI, respectively. Workers with tenure between 22 and 24 months are eligible for either four or five months of UI because of the following two rules. There is a mandatory one-month advance notice of layoff in Brazil. Many firms lay off workers immediately, paying an extra monthly wage. Others keep workers employed during the period. We cannot separately identify these two groups of firms and the advance notice period counts for UI eligibility. Moreover, 15 days of tenure count as one month for UI eligibility.

Our sample includes full-time private-sector formal employees 18–54 years old, laid off between 1997 and 2009. It has more than three million workers. We consider workers with tenure at layoff between 15 and 36 months. Again, we use only workers laid off between January and June because of data limitations detailed in Section 1.3. A worker with 24 months of tenure at layoff in our sample must then have been hired between January and June, while a worker with 22 months of tenure at layoff must have been hired between March and August. Our identifying assumption is that the distribution of workers' characteristics is continuous in tenure at layoff, conditional on hiring and separation calendar months. We thus avoid issues related to seasonality.³⁸

³⁷We are currently trying to tackle the following issues to replicate our results without this last selection condition. Because of a few missing worker IDs in the UI data, we cannot perfectly measure accumulated tenure since the last UI payments. Because of specific rules (see main text above), tenure in a formal job as counted for UI eligibility purposes is weakly higher than tenure as measured in our data. This noise increases with each previous employment.

³⁸We cannot use observations prior to 1997 as we must observe workers' formal employment history in the previous three years. Our results are similar when we add workers with tenure between 12 and 15 months at

Graphical results

Our results are easily presented graphically. Figure 8a displays actual benefit duration by tenure at layoff around the 24–month cutoff. Most workers collected all the UI payments for which they were eligible. Average benefit duration was thus constant and close to four months of UI for tenure levels below 22 months.³⁹ It increased to above 4.85 months for workers with 24 months of tenure. As expected, benefit duration for workers with tenure between 22 and 24 months lay in between. In the regression analysis, we simply exclude these observations.

Extending UI by one month increased average benefit duration by .9 month. To estimate the share of this increase due to behavioral responses, we adopt the same approach as for the 1996 UI extension. We construct a new variable, plotted in Figure 8b, using workers' formal reemployment patterns to infer how many UI payments they would have collected had they all been eligible for five months of UI. If they exhausted the first four months of UI, we assume that workers not formally reemployed within one month of UI exhaustion would have collected one extra payment. Observations to the left of the cutoff include only a mechanical cost. Observations to the right of the cutoff include both a mechanical and a behavioral cost. The discontinuity shows the behavioral cost.⁴⁰ It amounts to .08 month or only 9% of the total increase in benefit duration. Beneficiaries would have mechanically collected 4.8 UI payments if eligible for a fifth month of UI.

Figure 9 illustrates how these effects vary across labor markets with different formal employment rates. It presents monthly hazard rates of formal reemployment for workers with tenure at layoff between 20 and 22 months (eligible for four months of UI) and between 24 and 26 months (eligible for five months of UI) in Pernambuco and Rio Grande do Sul. On average between 2002 and 2009, formal employment rates were 15 percentage points higher in Rio Grande do Sul than in Pernambuco. The spike in formal reemployment rates at UI exhaustion is clearly shifted by one month in both states. Because formal reemployment rates were higher, the mechanical cost of a one-month UI extension was smaller and the behavioral cost larger in Rio Grande do Sul.

$$\mathbb{1}$$
 (draw 4th UI benefits) $\times \sum_{i=1}^{1} \mathbb{1}$ (month_{back} > month_{regUI} + j)

In Appendix Table A.7, we test (successfully) whether we accurately predict the increase in average benefit duration using workers' formal reemployment patterns after regular UI exhaustion in this way.

layoff. These workers may be negatively selected given the discontinuity in the tenure distribution around 12 months shown in Figure A.7. Our results are identical without controlling for hiring and separation calendar months but the distribution of covariates appears affected by seasonality patterns.

³⁹A very few beneficiaries supposedly eligible for four months of UI collected five months of UI.

⁴⁰Define $month_{regUI}$, the month a beneficiary exhausts her 4th month of UI benefits. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, this variable is defined as:

Validity checks

We present validity checks supporting our identification strategy before turning to the regression analysis. Results in Table 4 are obtained by estimating the following specification:

$$x_i = \alpha + \beta \ \mathbb{1}(T_i \ge 0) + \gamma \ T_i + \delta \ \mathbb{1}(T_i \ge 0) \times T_i + Z_i + \epsilon_i \tag{7}$$

where x_i is some characteristic of worker *i* and $T_i = Tenure - 24$ is the forcing variable. ϵ is an error term clustered by week of tenure. Z_i includes only fixed effects for hiring and separation calendar months. Our coefficient of interest, β , would capture any discontinuous change in the value of covariates at the tenure cutoff. Estimates of $\hat{\beta}$ are reported in Table 4. We perform a similar regression for the number of observations by week-of-tenure bin on each side of the cutoff (row 1). We exclude observations with tenure between 22 and 24 months but the results are similar in the overall sample. We consider the full tenure window around the cutoff in column (1) and a smaller tenure window — 18 to 30 months — in column (2). Estimates of $\hat{\beta}$ are neither economically nor statistically significant for gender, age, log wages, replacement rates, sectors of activity, firm size, local formal employment rates, and the number of observations per tenure bin. One estimate is marginally significant for years of education in column (1), but it is economically insignificant (.03 year). Appendix Figure A.8 graphically confirms our identifying assumption. The results below are identical when we control for individual characteristics.

Regression results

To quantify the average impact of a one-month UI extension at the tenure cutoff, we estimate similar specifications as in equation (7):

$$y_i = \alpha + \beta \ \mathbb{1}(T_i \ge 0) + \gamma \ T_i + \delta \ \mathbb{1}(T_i \ge 0) \times T_i + Z_i + \epsilon_i \tag{8}$$

where β captures a discontinuous impact at the tenure cutoff. Estimates of $\hat{\beta}$ are reported in Table 5. We consider similar outcomes y_i as for the 1996 UI extension using only the UI registry data: UI take-up, benefit duration censored at four months of UI, and total benefit duration (columns 1–3). We use the variable plotted in Figure 8b to estimate the increase in benefit duration due to a behavioral cost (column 4). β measures the behavioral cost. In Table 5, we use the larger tenure window and exclude observations with tenure between 22 and 24 months.

We find no effect on UI take–up (column 1). Average benefit duration for workers eligible for four months of UI was around 3.96 months (column 2). We estimate an increase of .91 month at the eligibility cutoff (column 3). The behavioral cost amounts to .08 month or 9% of the total increase in benefit duration (column 4). Interestingly, we even find a very small (.005 month) effect on benefit collection of the first four UI payments (column 2), suggesting some limited anticipation behaviors. Our results are robust to controlling for individual characteristics, to using a smaller tenure window, to considering only years after 2002, to restricting attention to workers with replacement rates between 20% and 80%, and to including only mesoregions with average formal employment rates between the 5^{th} and the 95^{th} percentile (Appendix Table A.5).⁴¹

We investigate how the behavioral cost and the resulting efficiency costs vary with local formal employment rates, using the following specification:

$$y_{i,m,t} = \alpha_m + \omega_t + \beta \ \mathbb{1}(T_{i,m,t} \ge 0) + \gamma \ T_{i,m,t} + \delta \ \mathbb{1}(T_{i,m,t} \ge 0) \times T_{i,m,t}$$

- + ζ FormalEmploymentRates_{m,t} + κ FormalEmploymentRates_{m,t} × $\mathbb{1}(T_{i,m,t} \ge 0)$
- $+\psi$ FormalEmploymentRates_{m,t} \times T_{i,m,t}
- + ξ Formal Employment Rates_{m,t} × $\mathbb{1}(T_{i,m,t} \ge 0) \times T_{i,m,t} + Z_{i,m,t} + \epsilon_{i,m,t}$ (9)

where α_m and ω_t are mesoregion and year fixed effects. We use demeaned formal employment rates linearly to fully exploit the cross-sectional and time variation. We consider the same outcome as in column (4) in Table 5. β measures the average behavioral cost at the tenure cutoff. ζ and κ measure how the mechanical and behavioral costs vary with formal employment rates, respectively. We report estimates of $\hat{\beta}$, $\hat{\zeta}$, and $\hat{\kappa}$ in Table 6 for specifications without fixed effects, with year fixed effects, with both year and mesoregion fixed effects, and with the addition of a rich set of individual controls (columns 1–4). We use formal employment rates by mesoregion as in Section 3.

We estimate a systematic negative relationship between the mechanical cost and formal employment rates and a systematic positive relationship between the behavioral cost and formal employment rates. These relationships are not due to fixed characteristics of labor markets. They are identical using variation over time across regions (column 3 compared to column 2). The results are not due to simple composition effects. They are identical controlling for a rich set of individual characteristics, including wage and sector of activity (column 4 compared to column 3). Our results are also robust to using formal employment rates by state, to using a smaller tenure window, to considering only years after 2002, to restricting attention to workers with replacement rates between 20% and 80%, and to including only mesoregions with average formal employment rates between the 5th and the 95th percentile (Appendix Table A.6).

Finally, the bottom panel in Table 6 uses estimates from column (4) to quantify the efficiency costs of the UI extension, $\tilde{\eta}$. The efficiency costs are low at any level of formal employment (around .1 at the sample mean) because most of the cost of extending UI is not due to distortions. Efficiency costs are increasing, however, with formal employment rates. Moving from 15 percentage points below to 15 percentage points above the sample mean (25th percentile and 99th percentile of the mesoregion-by-year distribution) increases

⁴¹In Appendix Table A.8, we find that the number of months of formal employment in the two years after layoff decreased at the cutoff as did the probability that workers experience a new layoff from the formal sector. We also find no effect on subsequent match quality in the formal sector (wage). These results confirm our findings using the 1996 temporary UI extension.

5 Benefits of UI extensions and welfare simulations

We have established that (i) UI extensions are costly in Brazil but generate small efficiency costs from moral hazard (formal work disincentives), and (ii) efficiency costs rise with formal employment rates. We can evaluate welfare effects of UI extensions locally by comparing the efficiency costs and the social value of the income transfer to UI exhaustees (Section 2). In this section, we investigate this social value using available survey data. We then evaluate welfare effects.

5.1 Social value of insurance and welfare effects of UI extension

We derive welfare effects from a marginal UI extension in Section 2 as:

$$\frac{1}{\frac{D^{f}}{D^{f} + D^{u}}} \frac{dW/dP}{g^{E}w^{f}} = \frac{dW}{dP} = qr \ S_{P+1} \left[\frac{g^{U_{P+1}} - g^{E}}{g^{E}} - \tilde{\eta} \right]$$
(10)

The social value of insurance $\frac{g^{U_{P+1}-g^E}}{g^E}$ corresponds to the social value of transferring \$1 from the average taxpayer (with marginal social value g^E) to the average UI exhaustee (with marginal social value $g^{U_{P+1}}$). It includes both the relative need for income support for UI exhaustees compared to taxpayers (ratio of marginal utilities), and social planner preferences towards redistribution. A UI extension increases welfare if the social value of insurance exceeds the pseudo-elasticity $\tilde{\eta}$, which measures efficiency costs. To investigate the social value of insurance, we proceed in three steps.

First, we distinguish between our two types of UI exhaustees, the unemployed and the informally reemployed. They may have different needs for income support. Define O as the share of unemployed UI exhaustees. The social value of insurance can be written as:

$$\frac{g^{U_{P+1}} - g^E}{g^E} = O\frac{g^{O_{P+1}} - g^E}{g^E} + (1 - O)\frac{g^{I_{P+1}} - g^E}{g^E}$$
(11)

where $g^{O_{P+1}}$ and $g^{I_{P+1}}$ are the social values of \$1 for unemployed and informally reemployed UI exhaustees, respectively. We estimate O using longitudinal urban labor force surveys (PME).

Second, the value of insurance can be decomposed as follows (Baily, 1978; Chetty, 2006):⁴²

$$\frac{g^{J_{P+1}} - g^E}{g^E} = \gamma \frac{c^E - c^{J_{P+1}}}{c^E}, \quad \text{with J=O,I.}$$
(12)

⁴²The decomposition assumes that third derivatives of utility functions are small.

where $\frac{c^E - c^{J_{P+1}}}{c^E}$ corresponds to the mean consumption gap between taxpayers and UI exhaustees of type J. γ captures both an average coefficient of relative risk aversion and social planner preferences toward redistribution. A high value of γ , or large consumption gaps, increases the social value of insurance. There is no data on consumption or savings for UI beneficiaries in Brazil. Instead, using the same longitudinal survey data, we measure average disposable income for the formally employed and the two types of UI exhaustees in order to approximate these consumption gaps (upper-bounds). Finally, we calibrate the social value of insurance for different values of γ .

5.2 Are UI exhaustees unemployed or informally reemployed?

We rely on the longitudinal structure of the Brazilian urban labor force surveys (PME 2003–2010) to estimate the share of unemployed UI exhaustees O in the six largest urban areas of the country covered by the surveys. Using consecutive interviews, we can estimate the job-finding probability in the subsequent month given respondents' unemployment duration. We estimate these hazard rates of overall reemployment (formal and informal) by maximum likelihood.⁴³ We want a likelihood function flexible enough to capture a possible spike in overall reemployment rates. We therefore assume a piece-wise constant hazard function with six parameters, accounting for different hazard rates in months 0, 1–2, 3–4, 5–6, 7–8, and 9–10. Our likelihood function also corrects for a stock sampling issue within a month. Define λ_m as the daily hazard rate constant over month m = 0, 1, ..., 10 since layoff. Assume a respondent is interviewed on day $b \in [0, 30]$ within month m. She can only be observed on day b if she survived b days without a job, given that she already survived m months. Define k(b) as the distribution of interviews over days within a month. Finally, define $d_{i,m} = 1$ if individual i, unemployed since month m, is reemployed by the time of the subsequent interview. The likelihood for a given observation is thus:

$$L_{i,m} = d_{i,m} \int_{0}^{30} \left[1 - \exp\left(-(30 - b)\lambda_{m} - b\lambda_{m+1}\right)\right] \frac{k(b) \exp\left[-b\lambda_{m}\right]}{\int_{0}^{30} k(s) \exp\left[-s\lambda_{m}\right] ds} db + (1 - d_{i,m}) \int_{0}^{30} \left[\exp\left(-(30 - b)\lambda_{m} - b\lambda_{m+1}\right)\right] \frac{k(b) \exp\left[-b\lambda_{m}\right]}{\int_{0}^{30} k(s) \exp\left[-s\lambda_{m}\right] ds} db$$
(13)

Our sample includes individuals 18–54 years old who were full-time private-sector formal employees with more than 24 months of tenure at layoff in their last job (eligible for five months of UI).⁴⁴ We have 30,749 observations contributing to the likelihood function. Our sample cannot be conditioned on UI take-up because survey questions do not cover UI

⁴³For workers who find a job, we are unable to estimate later transitions to other jobs because questions about past unemployment spells are not asked in that case and the panel is too short.

⁴⁴Although samples are representative of the overall labor force in the six metropolitan areas, this does not guarantee that they are representative of the unemployed labor force with more than two years of tenure in the last formal job. This is why we estimate overall reemployment rates from transitions across months rather than from the distribution of unemployment duration.
benefits. In the estimations, we assume that interviews are uniformly distributed, k(b) = 1/30. Estimations are performed using sampling weights and clustering standard errors by individual.

The estimated monthly hazard rates of overall reemployment are displayed in Figure 10a. Point estimates start at 22% in the first month after layoff and decrease to stabilize at around 18% three months after layoff. We display in the same graph hazard rates of formal reemployment using a random sample of similarly selected workers in our administrative data. Formal reemployment rates are higher than in previous figures in the first few months because the sample is not conditioned on UI take–up. They are particularly high during the 30–day waiting period. As usual, they spike five months after layoff (from .04 to .14). There are two main lessons from Figure 10a. First, overall reemployment rates are always higher than formal reemployment rates. Many UI beneficiaries are thus informally reemployed.⁴⁵ Second, confidence intervals rule out the existence of a large spike in overall reemployment is due to UI incentives and that the behavioral cost of UI extensions is entirely driven by a shift in the spike. Therefore, the absence of a spike in overall reemployment of beneficiaries (*f* margin in the model of Section 2).

Figure 10b displays the corresponding survival rates. We estimate that about 30% of workers are unemployed one month after typical UI exhaustion. In comparison, 65% are still out of formal employment. Therefore, even if informal reemployment is prevalent, a significant share of UI exhaustees remains unemployed. Our estimate is similar to exhaustion rates of the 26 weeks of UI in the US (around 35%). We use these estimates in our simulation and assume that 46% of UI exhaustees are unemployed.

5.3 Relative need of income support

Labor status does not directly provide information on UI exhaustees' relative need for income support. Beneficiaries (re)employed in the informal sector may earn a low wage. The unemployed may have family members with a high income. Using the same surveys and sample as above, we measure average disposable income by reemployment status. Disposable income is defined as household income per capita per month, with an equivalence scale of one half for children. Table 7 displays average disposable income for the unemployed around UI exhaustion and the formally and informally reemployed in the first five months after layoff, relative to the average disposable income of the formally employed before layoff (typical UI contributors). We observe the formally and informally reemployed, and their disposable income, only upon reemployment. We assume that they have similar income levels around UI exhaustion.

 $^{^{45}}$ Among the workers reemployed but not as formal employees, the surveys reveal that 30.5% are self-employed, 2% are employers, and 67.5% are informal employees.

Chapter 1: Informal Labor and the Cost of Social Programs

In Section 4, we estimated that efficiency costs were larger in labor markets with higher formal employment rates. Ceteris paribus, this decreases welfare effects of UI extensions. A greater need for insurance, however, may compensate for larger efficiency costs.⁴⁶ We therefore divide our sample in two groups: the two metropolitan areas from the poorer, less formal, Northeast (columns 1–3) and the four metropolitan areas from the richer, more formal, South–East and South (columns 4–6). The average formal employment rates were 27.9% and 36.4% in the first and second group, respectively (2003–2010). We also re– estimated our maximum likelihood separately for each group. We obtain comparable shares of unemployed UI exhaustees in the two groups (.48 and .43, respectively). This share is in fact slightly larger in the less formal labor markets.

Average disposable income levels are systematically higher in the South–East and South (R\$362 vs R\$248 prior to layoff). Disposable income ratios, however, are very similar across groups. Average disposable income for the informally reemployed is 35.6% (North–East) and 34.4% (South and South–East) smaller than for formal employees prior to layoff. Corresponding average disposable income for the unemployed UI exhaustees is 54.1% and 51.1% smaller (columns 1 and 4). Controlling for gender, year, calendar–month, and area fixed effects has little impact on our results (columns 2 and 5). Adding controls for education levels, age, and tenure suggests that there is some selection into informal reemployed remains 24.5% and 28.6% smaller than for formal employees prior to layoff. These results thus reveal large disposable income gaps, including for the informally reemployed. Average levels for the unemployed UI exhaustees may also understate the need for income support: 37% and 30% of them have no source of household income at all. None of the estimates provided in Table 7 offer any evidence of a greater need for income support among UI exhaustees from more formal labor markets.

5.4 Welfare simulations

We now use our results to evaluate the welfare effects of a UI extension in our context. Table 8 displays welfare effects of a marginal UI extension (in bold) obtained from evaluating equation (10). Welfare effects are measured in terms of an equivalent percentage change in total payroll of eligible formal employees. We use estimates of efficiency costs from Table 3 (low formal=.12, high formal=.175). The social value of insurance is calibrated using the decompositions in equations (11)–(12) and disposable income ratios from Table 7 (with full controls) for different values of γ , which captures both an average coefficient of relative risk aversion and social planner preferences towards redistribution. We use the same social value of insurance in labor markets with different formal employment rates because we did not find evidence of differential disposable income ratios in Table 7. For a given value of γ (column 1), the table displays the corresponding social value of insurance (column 2) and the resulting welfare effects in labor markets with relatively high and relatively low formal

⁴⁶For instance, the need for insurance may be greater if there are fewer informal employment opportunities.

employment rates (columns 3 and 4). Alternatively, without relying on our calibration, the table displays the welfare effects for a given social value of insurance.⁴⁷

Welfare effects are positive unless the social value of insurance is very low. For $\gamma = 1$, disposable income ratios imply a social value of \$1 that is 39% higher for UI exhaustees. Extending UI benefits by one month then has a similar effect on welfare as increasing wages of eligible formal employees by .27%–.36%. Welfare effects are 33% $\left(\frac{.36-.27}{.27}\right)$ higher in labor markets with low formal employment rates because of smaller efficiency costs. Welfare effects are in fact positive as long as the social value of \$1 is 17.5% larger for UI exhaustees than for individuals contributing to the UI budget, because the efficiency costs are at most .175. A similar bound on the social value of insurance for a UI extension to increase welfare in the US would be above 100%, using estimates from Katz and Meyer (1990). Chetty (2008) estimates that the social value of \$1 in the US is 150% larger for UI beneficiaries at the start of their unemployment spell than for employed individuals. Welfare effects in our case are equivalent to raising wages of eligible formal employees by 1.69%–1.85% for such a social value.⁴⁸ Incorporating our empirical findings in our framework, the welfare effects of a UI extension are thus likely positive and may be sizeable.

6 Discussion

We have established that UI extensions in Brazil impose small efficiency costs from distorting incentives to return to a formal job and are likely welfare—enhancing in our framework. We discuss here some limitations of this framework.

First, our measure of efficiency costs entails both an income and a substitution effect. Separating them (Card, Chetty and Weber, 2007a) provides information on the welfare gains from social insurance (Chetty, 2008). Yet, conditional on a social value of insurance, welfare consequences of extending UI depend solely on the ratio of the behavioral to the mechanical cost in a large class of models, as long as an envelope condition applies to the agents' problem (Chetty, 2006).

Second, layoffs may increase with UI benefits. We followed the literature and abstracted from this margin because the optimal policy is to introduce experience–rating (Blanchard and Tirole, 2006). Patterns in Appendix Figure A.7 suggest that UI affects layoffs at low tenure levels. Existing institutions, however, appear sufficient to prevent such responses for the workers we considered.

⁴⁷We use the average monthly layoff rate taking into account incomplete UI take–up ($q = .0291 \times .86$) and the average replacement rate (r = .65).

⁴⁸If individuals have significant liquid savings to deplete when unemployed, which is not the case in the US (Chetty, 2008), lower values of γ should be considered. The availability of liquid savings decreases local relative risk aversion (Chetty and Szeidl, 2007). Even if we assume that the social value of redistributing \$1 towards the informally reemployed is nil, welfare effects are positive as long as the social value of \$1 is 39% larger for unemployed UI exhaustees than for individuals contributing to the UI budget (available from the authors).

Third, there may be relevant general equilibrium effects. Welfare effects would increase in the presence of search externalities, but likely decrease in wage bargaining models (Landais, Michaillat and Saez, 2010). Entitlement effects could attract workers to the formal sector if they value UI (Hamermesh, 1979). In contrast, UI taxes may be more distortive in poorer countries. To our knowledge, there is no empirical evidence on the relative magnitude of these two mechanisms, even in developed countries. Almeida and Carneiro (2012) show that labor inspections targeting non-compliance with mandated benefits by formal firms increased formal employment in Brazil. Workers thus appear willing to trade off lower wages for mandated benefits, including benefits related to job-loss risk (severance payments).

Fourth, we followed the literature and considered UI in isolation. In reality, behavioral responses to UI incentives may create fiscal externalities. With fiscal externalities, changes in the overall duration out of formal employment D^u following a UI extension become relevant.⁴⁹. There may also be real externalities attached to UI–induced informal employment. In this case, the impact of UI extensions on informal employment, multiplied by the social cost or social value of the externality ζ , becomes relevant. There is no consensus, however, on the magnitude or sign of ζ .⁵⁰

Finally, a welfarist perspective may not be an accurate positive theory of governments. If governments consider their budget as fixed, our results would be reversed. UI extensions are costly and become relatively cheaper, even if more distortive, when formal employment rates increase.

7 Conclusion

This paper estimates the efficiency costs of UI extensions in a context where informal labor is prevalent by combining a model of optimal social insurance and an unusually rich dataset on Brazilian UI beneficiaries over 15 years. The main results are that the efficiency costs of UI extensions are rather small, but that they rise with the relative size of the formal labor market. These findings run counter to widespread claims in policy circles that heightened concerns of moral hazard preclude the expansion of unemployment insurance in developing countries.

Because Brazil contains regions with such widely divergent levels of income and labor market formality, we are optimistic about the external validity of our study. In fact, understanding the relationship between efficiency and formality in other settings is an exciting

⁴⁹With fiscal externalities, the budget constraint is: $\frac{D^f}{D^f + D^u} \tau w^f = q \frac{D^f}{D^f + D^u} Bb + R$, where R is the monthly average "other" public spending per individual financed through labor income tax. Then, we have:

$$\frac{d\widetilde{W}}{dP} = qr \ S_{P+1} \left[\frac{g^{U_{P+1}} - g^E}{g^E} - \widetilde{\eta} - \frac{R}{S_{P+1}} \frac{dD^u}{dP} \right]$$

⁵⁰Informal employment is often viewed as generating negative externalities (Levy, 2008). One could argue in our case that a behavioral cost caused by beneficiaries working informally generates positive externalities compared to a behavioral cost caused by beneficiaries not working at all. avenue for future research. We also discuss some mechanisms besides moral hazard and beyond the scope of our framework (e.g., general equilibrium effects) that could, in theory, increase or decrease efficiency costs from UI extensions. More research is needed to evaluate their empirical relevance.

The findings of this paper have broader implications for our understanding of social policies in developing countries. First, many social programs and taxes generate incentives for people to carry out their economic activities informally. For the same reasons as for UI, they are viewed as imposing large efficiency costs in a context of high informality. By going against the conventional wisdom, our results cast doubt on whether efficiency considerations actually limit the expansion of social policies in these cases too. Recent work by Kleven and Waseem (2012) points in the same direction: intensive-margin taxable income elasticities are small in Pakistan even though evasion is widespread. Of course, the expansion of social policies may be driven instead by the political process and by policymakers' preferences for redistribution (Acemoglu and Robinson, 2008).

Finally, our results suggest that weak governmental institutions may become even more policy relevant when a country's economy develops and its formal employment sector expands. Fiscal and social policies should adjust to these changing circumstances in rapidly developing countries — such as China and Brazil — and may be best partially decentralized to subnational governments, given how local labor market conditions affect the efficiency costs of social programs.



Figure 1: Formal employment rates and average income per capita in Brazil

The figures display formal employment rates and average income per capita by state in Brazil over the period 1995–2002 (panel a) and 2003–2009 (panel b). Formal employment rates by state (private–sector formal employees within the 18–54 years old population) and average income per capita (18–54 years old population; bottom 5% and top 10% trimmed) are obtained from yearly household surveys (PNAD, R\$1.9 \approx US\$1 in 2000). In the cross-sections, formal employment rates strongly correlate with income per capita and there is wide variation across states. Brazil experienced high economic growth in the second time period. Both income per capita and formal employment rates increased overall, but increases were not uniform across areas. We obtain similar patterns when we include public employees.



Figure 2: Connecting theory to the data

Panel (a) displays the choice situation that displaced formal employees face each period before formal reemployment in our dynamic model of endogenous job-search. While unemployed, a worker decides how much effort e at a cost z(e) to invest in finding a new job. Search efforts are normalized to correspond to job-finding probabilities. With probability 1 - e, she does not find a job and stays unemployed. With probability e, she finds a job. She can increase her probability of returning to a formal job by investing formal search effort f at a cost $\theta z(f)$. She thus finds a formal job with probability ef and an informal job with probability e(1-f). She earns wage $w^i < w^f$ when working informally. She can always search for a formal job at the same cost $\theta z(f)$ in subsequent periods (she starts from the "formal job search" node). Informal jobs are detected by the government with probability p. If detected, an informal worker falls back into unemployment and loses her UI benefits. Both the unemployed and the "undetected" informally employed draw UI benefits \hat{b} in the first P periods after layoff. The unemployed have a minimum consumption level o. The solution to this dynamic problem determines survival rates out of formal employment and therefore the average benefit duration. Panel (b) displays the mechanical and behavioral costs (in months) of extending UI by one period (UI costs are average benefit duration times the benefit level). Workers not formally reemployed one month after regular UI exhaustion before the extension draw the extra payment without changing their behavior (mechanical cost). A UI extension also reduces incentives to be formally reemployed ($e \downarrow, f \downarrow$). As a result, survival rates, and average benefit duration, may increase both during the period of extension and in earlier periods (behavioral cost). The ratio of the behavioral to the mechanical cost $(\tilde{\eta})$ captures efficiency costs. It measures the fraction of social spending lost through behavioral responses. A UI extension increases welfare if the social value of the income support provided to UI exhaustees exceeds $\tilde{\eta}$.



Figure 3: Reemployment patterns of UI beneficiaries in Brazil

Full-time private-sector formal employees 18-54 years old, eligible for five months of UI after having been laid off in 2009 in Pernambuco (resp. Rio Grande do Sul), a poor (resp. rich) state with relatively low (resp. high) formal employment rates in the Northeast (resp. South) of Brazil. The sample is restricted to UI takers. The left panel displays survival rates out of formal employment in the months following layoff. The right panel displays the corresponding monthly hazard rates of formal reemployment. While workers draw UI benefits, formal reemployment rates are very low in both states. After UI exhaustion, they spike and increase relatively more in the state with higher formal employment rates. This suggests a clear behavioral response to UI incentives, larger in Rio Grande do Sul. Formal reemployment rates stay quite low, however, even after UI exhaustion. About 40% of workers were still out of formal employment 12 months after losing their formal job. If UI had been extended by two months for these workers, most of them (70%–80%) would have mechanically collected additional UI payments without changing their behavior. In Section 4, we find that the spike in formal reemployment at benefit exhaustion is completely shifted following actual UI extensions. In Section 5, we find that there is no such spike in overall reemployment rates. The behavioral cost is thus driven by informally reemployed beneficiaries. Nevertheless, the size of this behavioral cost is small compared to the mechanical cost, and so the efficiency costs are limited. The spike being larger, the mechanical cost is smaller and the behavioral cost larger in Rio Grande do Sul, suggesting that efficiency costs rise with formal employment rates.



Figure 4: Regular benefit duration and mechanical cost of a hypothetical UI extension

(b) Mechanical cost of a hypothetical two–month extension

Full-time private-sector formal employees 18–54 years old, laid off between 2002 and 2009, and eligible for five months of UI. The sample is restricted to UI takers. Observations are averaged out by state and year. The left panel displays the relationship between regular benefit duration and formal employment rates (measured in PNAD surveys). The right panel displays the relationship between the mechanical cost of a hypothetical two-month UI extension (the increase in benefit duration absent any behavioral response) and formal employment rates. Regular benefit duration decreases slightly with formal employment rates but remains very high at any level. Beneficiaries draw on average 4.85 to 4.95 months of UI (out of a maximum of five months). Formal reemployment rates increase after regular UI exhaustion but remain low. As a consequence, extending UI by two months would be costly in Brazil absent any behavioral response. The mechanical cost varies from 1.75 months in states with low formal employment rates to 1.4 months in states with high formal employment rates. The relationship with formal employment rates after regular UI exhaustion increase with local formal employment rates.



Figure 5: Timeline of the 1996 temporary UI extension

Private–sector formal employees laid off in April and May 1996 who exhausted their five months of regular UI benefits between September and November 1996 in treatment areas were eligible for two additional UI payments. The UI extension was proposed on August 14 and was adopted on August 21 to start on September 1, 33 days before the first round of local elections. No extra payments would be paid after December 31.

Figure 6: The 1996 temporary UI extension, impacts on formal reemployment

(a) Survival rates out of formal employment, control areas



(c) Survival rates out of formal employment, treatment areas (b) Hazard rates of formal reemployment, control areas



(d) Hazard rates of formal reemployment, treatment areas



Full-time private-sector formal employees 18–54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996, in treatment areas, these workers were eligible for seven months of UI benefits. The sample is restricted to UI takers. The left panels display the survival rates out of formal employment after layoff in each year for displaced workers from control areas (panel a) and treatment areas (panel c). The right panels display the hazard rates of formal reemployment after layoff in each year for displaced workers from control areas (panel b) and treatment areas (panel d). In control areas or in control years, survival rates out of formal employment rates at regular benefit exhaustion shifted by two months in treatment areas. As a consequence, an additional 15% of workers were still out of formal employment seven months after layoff. The spike appears to be shifted by more than two months because month 7 corresponds to December for workers laid off in May, and hirings are systematically low in December. The spike is shifted by exactly two months for workers laid off in April (available upon request).



Figure 7: The 1996 temporary UI extension, impacts in different areas

Full-time private-sector formal employees 18–54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996, in treatment metropolitan areas, these workers were eligible for seven months of UI benefits. The sample is restricted to UI takers. The left panel displays the hazard rates of formal reemployment after layoff in control and treatment year for displaced workers from Recife (state of Pernambuco), a treatment area where formal employment rates were relatively low (24%). The right panel displays the hazard rates of formal reemployment after layoff in control and treatment year for displaced workers from Porto Alegre (state of Rio Grande do Sul), a treatment area where formal employment rates were relatively high (35%). Formal employment rates are obtained from yearly household surveys (PNAD). Hazard rates of formal reemployment at regular UI exhaustion spiked to 8% and 12% in control years in Recife and Porto Alegre, respectively. In both areas, the spike shifted by two months in 1996. Because the spike was larger in Porto Alegre, the mechanical cost of the UI extension was smaller and the behavioral cost larger in Porto Alegre.



Figure 8: Regression discontinuity design, impacts on UI benefit duration

Full-time private-sector formal employees 18–54 years old, laid off between 1997 and 2009. Workers with more than 24 months of tenure at layoff were eligible for five months of UI. Workers with less than 22 months of tenure at layoff were eligible for four months of UI. Workers with tenure between 22 and 24 months at layoff were eligible for either four or five months of UI (see text). Outcomes are averaged out by month of tenure. The sample is restricted to UI takers. Panel (a) displays the actual benefit duration by tenure at layoff. The discontinuity at the tenure cutoff shows the effect of a one-month UI extension on average benefit duration. Panel (b) displays the mechanical and behavioral costs (in months) of the one-month UI extension. The scale of the y-axis is ten times smaller in panel (b). Observations to the left of the cutoff show the mechanical cost, the increase in benefit duration if beneficiaries eligible for four months of UI did not change their behavior but could have collected a fifth month of UI. Observations to the right of the cutoff shows the behavioral cost. Average benefit duration increases by .9 month following a one-month UI extension (panel a). Most of the increase is driven by the mechanical cost. The behavioral cost only amounts to .08 month (panel b).

Tenure [24,26] 5 months of UI

10 11 12

8 <u>9</u>

Figure 9: Regression discontinuity design, hazard rates of formal reemployment

Ч

08

.04 .06

02

C

ò

Tenure [20,22] 4 months of UI

2 3

4 5 6 7 8 9

(a) Pernambuco (mean formal employment rate 19%)

5 6

₽

-

80

90

02 .04

0

ò

Tenure [20.22] 4 months of UI

2 3

(b) Rio Grande do Sul (mean formal employment rate 34%)

Tenure [24,26] 5 months of UI

10 11 12





Figure 10: Comparing formal and overall reemployment patterns

Both panels compare *formal* reemployment rates from administrative data (RAIS) and *overall* (formal and informal) reemployment rates estimated by maximum likelihood from monthly urban labor force surveys (PME, see text). The samples include displaced formal employees (full–time private–sector 18–54 years old) laid off between 2003 and 2009 in the six largest metropolitan areas of Brazil (coverage of PME surveys) and eligible for five months of UI (more than 24 months of tenure). Because surveys do not include information on UI take–up, the samples are not restricted to UI takers. Overall reemployment rates are much higher than formal reemployment rates. They present no clear spike around benefit exhaustion. Therefore, most of the spike (behavioral cost) must be due to behavioral responses from informally reemployed beneficiaries. We estimate that about 30% of workers remain unemployed one month after UI exhaustion, while 65% are not yet back to the formal sector. The difference must be made up of informally reemployed beneficiaries.

Table 1: Net earnings by reemployment status compared to net earnings prior to layoff

| | (1) | (2) | (3) |
|-----------------------|------------|--------------|-----------|
| Average net earni | ings prior | to layoff: I | R\$ 806.7 |
| Formally reemployed | 1619*** | 1822*** | 186*** |
| | (.0354) | (.034) | (.031) |
| Informally reemployed | 4702*** | 4622*** | 4147*** |
| | (.0254) | (.0248) | (.0257) |
| First controls | No | Yes | Yes |
| Additional controls | No | No | Yes |
| Observations | 5371 | 5371 | 5371 |

Data from monthly urban labor force surveys covering the six largest metropolitan areas of Brazil (PME, 2003–2010). The sample includes full-time private-sector formal employees 18–54 years old with more than 24 months of tenure in the month before their layoff and workers who lost their formal job with more than 24 months of tenure at layoff. These workers are eligible for five months of UI in Brazil. The table displays the average net earnings of displaced formal workers (upon reemployment) who are reemployed formally (top) or informally (bottom) in the first five months after layoff, relative to the average net earnings prior to layoff (repeated cross-sections). Column 2 controls for gender, year, calendar month, and metropolitan area fixed effects. Column 3 adds fixed effects by education levels, and second-order polynomials in age and tenure. Exchange rate: R $1.9\simeq$ US1 (in R\$ of 2000).

| | | (1) | (2) | $(\overline{3})$ | $(\overline{4})$ | |
|--|------------|---|--|---|---|--|
| Outcomes | Mean | Coefficient on formal employment rates | | | | |
| UI take-up | .8601 | 1469*** | 2134*** | .142 ^{***} | .1355*** | |
| Regular benefit duration | 4.934 | (.0301) 3091*** (.0555) | (.0278) 1713*** (.0341) | (.0548) 2082*** (.0680) | (.0525) 1715^{***} (.0501) | |
| Mechanical cost of a two–month UI extension More than seven months without formal job | 1.667.7316 | (.0333) 9191*** (.1612) 4973*** (.0923) | (.0341) 5999*** (.1145) 372*** (.0795) | (.0089) 7211*** (.2116) 3761*** (.1198) | (.0391) 6683*** (.2053) 3744*** (.1245) | |
| Observations Year fixed effects Mesoregion fixed effects Other controls | | 2,901,159 No No No | 2,901,159 Yes No No | 2,901,159 Yes Yes No | 2,901,159 Yes Yes Yes | |

Table 2: Mechanical cost of UI extensions and formal employment rates

s.e. clustered by mesoregion (137 clusters). Significance levels: * 10%, ** 5%, ***1%. Random sample of full-time private-sector formal employees 18-54 years old, laid off between 1995 and 2009, and eligible for five months of UI. The table displays the coefficients from regressing various outcomes (listed in the left-hand-side column) on yearly formal employment rates by mesoregions. Outcomes, other than take-up, are conditional on take-up (take-up regressions use 3,870,398 observations). Column (2) includes year fixed effects. Column (3) includes year and mesoregion fixed effects. Column (4) adds dummies for (calendar) separation month, sector of activity, education, gender, and firm size, as well as fourth-order polynomials in age, tenure and log real wage before layoff. Our main outcome of interest is the mechanical cost (in months) of a hypothetical two-month UI extension, the increase in benefit duration absent any behavioral response (row 3; the construction of the outcome is detailed in the text). Extending UI from five to seven months would be costly in Brazil because 1.67 additional months of UI would be collected on average, absent any behavioral response. The mechanical cost is large because most beneficiaries exhaust their five months of UI (regular benefit duration is 4.93 on average, row 2) and because 73% of beneficiaries are still out of the formal sector seven months after layoff (row 4). The mechanical cost decreases with formal employment rates. The relationship is not due to fixed differences across areas but holds for marginal changes in formal employment rates (column 3). The relationship is not simply due to composition effects (column 4). The estimate in column (4) implies that increasing formal employment rates by 30 percentage points increases the mechanical cost by .2 month or 12% (and regular benefit duration by only 1%). We present many robustness checks in Table A.1. A concern is that UI take-up is also correlated with formal employment rates (row 1), potentially creating selection issues. The negative relationship in columns (1) and (2) likely implies negative selection while the positive relationship in columns (3) and (4) likely implies positive selection (see text). Yet such a concern is limited: our main results are consistent across specifications and are robust to the inclusion of a rich set of individual controls.

| | (1) UI take-up | (2) Regular UI duration | (3) Extended UI duration | (4) Extended | (5) UI duration sterfactual |
|--|----------------------|-------------------------------|--------------------------------|-----------------|-----------------------------------|
| TreatArea \times Year 1996 | 0178 | 0003 (.0024) | 1.867^{***} | $.2469^{***}$ | .1933*** (.0174) |
| $\begin{array}{l} {\rm TreatArea}\times{\rm Year1996}\\ \times{\rm Formality}{\rm rate}>{\rm average} \end{array}$ | (10100) | (10021) | ((0101)) | (10201) | $.0779^{**}$ (.0328) |
| Mean (treatment area, control years) | .74 | 4.98 | 4.98 | 6.56 | 6.56 |
| Observations | $229,\!878$ | $171,\!407$ | $171,\!407$ | $171,\!407$ | $171,\!407$ |
| | Mechan | ical and bel | navioral costs | of the UI | extension |
| | Mech. co | ost (month) | Beh. cost (| month) | $\widetilde{\eta}$ |
| Formality rate $<$ average | 1.6 | 641*** | .1964* | ** | $.1197^{***}$ |
| | (. | 0142) | (.0179) | 9) . | (.0118) |
| Formality rate $>$ average | 1.5 | 58*** | .2726* | ** | .175*** |
| | (. | 0165) | (.0284) | 4) | (.0195) |

Table 3: Difference–in–difference results for the 1996 temporary UI extension

s.e. clustered by area (29 clusters). Significance levels: * 10%, ** 5%, ***1%. The sample includes full-time private-sector formal employees 18-54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996, in treatment areas, these workers were eligible for seven months of UI benefits. In the top panel, the table displays estimates of the difference-in-difference estimator for various outcomes (listed above each column). The regressions include dummies for (calendar) separation month, year, and area. Outcomes in columns (2)–(5) are conditional on take-up. Column (1) shows that there is no treatment effect on UI take-up, a decision taken before the UI extension was announced. Column (2) shows that there is no treatment effect on regular UI duration (first five months). Because beneficiaries were already drawing 4.98 months without the UI extension, there was no room to increase regular UI duration. Column (3) shows that the UI extension increased benefit duration by 1.87 months on average. But column (4) shows that only .25 month is due to behavioral responses (the construction of the outcome in columns 4 and 5 is detailed in the text). Beneficiaries in control years would have collected 1.58 months (6.56 - 4.98) absent any behavioral response, had they been eligible for the extension. Column (5) shows that the behavioral cost is larger in metropolitan areas with higher formal employment rates. In the last regression, the dummy for above-average formal employment rates is absorbed by the area fixed effects. The regression includes this dummy interacted with a dummy for the treatment year and interacted with the difference-in-difference indicator (reported). We present many robustness checks in Table A.4. In the bottom panel, the table displays estimates of the behavioral and mechanical costs. To be able to estimate the mechanical cost, we use regressions without metropolitan area fixed effects. Regressions include dummies for (calendar) separation month, treatment year, treatment area, above-average formal employment rates, and the latter dummy interacted with the dummies for treatment year and treatment area. Regressions also include the difference-in-difference indicator directly and interacted with the dummy for above average formal employment rates. The mechanical cost is obtained as the linear combination of all the coefficients, except the difference-in-difference estimators, from a regression using the outcome of column (4) minus the same linear combination of coefficients from a regression using the outcome of column (2). The behavioral cost is the linear combination of the difference-in-difference estimators from a regression using the outcome of column (4). The pseudo-elasticity $\tilde{\eta}$, the ratio of the behavioral to the mechanical cost, measures efficiency costs (s.e. are obtained by the delta method). Estimates show that the mechanical cost is large and decreases with formal employment rates, and that the behavioral cost increases with formal employment rates. The resulting efficiency costs are small because most of the increase in benefit duration is not due to behavioral responses, but the efficiency costs rise with formal employment rates.

| Mean | (1) | (2) |
|-----------------------|--|---|
| $20 \leq Tenure < 22$ | Coefficient | on cutoff |
| 44972 | 679.4 | 1328 |
| | (5236) | (7894) |
| .5947 | .0065 | .0003 |
| | (.0052) | (.0077) |
| 29.62 | .067 | 0063 |
| | (.0518) | (.065) |
| 8.522 | 0347* | 0229 |
| | (.0208) | (.028) |
| 6.608 | .0359 | .0652 |
| | (.033) | (.05) |
| .7066 | 0107 | 0214 |
| 0051 | (.0106) | (.0157) |
| .3054 | .004 | 0022 |
| 2262 | (.0074) | (.01) |
| .3362 | 0047 | .0036 |
| 0200 | (.0036) | (.0052) |
| .2399 | .0002 | .0007 |
| 0071 | (.0067) | (.0098) |
| .2071 | 0040 | .0095 |
| 4.9 | (.0147) | (.0222) |
| .45 | .0020 | 0119 |
| 2007 | (.0218) | (.034) |
| .2097 | 00000 | 0002 |
| | (.0013) | (.002) |
| | 3,065,724 | $1,\!648,\!581$ |
| | 15 - 36 | 18 - 30 |
| | $\begin{array}{l} 20 \leq \stackrel{\text{Mean}}{Tenure} < 22\\ 44972\\ .5947\\ 29.62\\ 8.522\\ 6.608\\ .7066\\ .3654\\ .3362\\ .2399\\ .2071\\ .43\\ .2097\\ \end{array}$ | $\begin{array}{cccc} \mbox{Mean} & (1) \\ 20 \leq Tenure < 22 & Coefficient \\ 44972 & 679.4 \\ & (5236) \\ .5947 & .0065 \\ & (.0052) \\ 29.62 & .067 \\ & (.0518) \\ 8.522 &0347^* \\ & (.0208) \\ 6.608 & .0359 \\ & (.033) \\ .7066 &0107 \\ & (.0106) \\ .3654 & .004 \\ & (.0074) \\ .3362 &0047 \\ & (.0036) \\ .2399 & .0002 \\ & (.0067) \\ .2071 &0046 \\ & (.0147) \\ .43 & .0026 \\ & (.0218) \\ .2097 &0005 \\ & (.0013) \\ \end{array}$ |

Table 4: Validity of the regression discontinuity design

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full-time private-sector formal employees 18–54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for four months of UI. Those with more than 24 months of tenure at layoff were eligible for five months of UI (extension). Those with tenure between 22 and 24 months were eligible for four or five months of UI (see text) and are excluded from the regressions. The table displays the coefficients from regressing various workers' characteristics (listed in the left-hand column) on a dummy for having more than 24 months of tenure at layoff (tenure cutoff). The outcome in the first row is the number of observations by tenure bin to test for the smoothness of the tenure distribution at layoff (forcing variable). Column (1) uses observations with tenure at layoff between 15 and 36 months. Column (2) uses a smaller tenure window around the cutoff (18 to 30 months). The regressions include fixed effects for (calendar) separation and hiring months and linear controls in tenure on each side of the cutoff. We find no evidence of a discontinuous change in the value of the covariates or the number of observations at the tenure cutoff. The only (marginally) significant coefficient is economically insignificant (.03 years of education). These estimates support the validity of the regression discontinuity design. Figure A.8 in the Appendix graphically confirms our results.

| | (1) | (2) | (3) | (4) |
|---|-----------------|------------------------------|----------------------|--------------------------------|
| | UI take–up | UI duration | Extended UI | Extended UI duration |
| Tenure ≥ 24 months | .0312 | up to 4 months $.0055^{***}$ | duration .9091*** | vs. counterfactual .0821*** |
| | (.0226) | (.0013) | (.0043) | (.0037) |
| $\begin{array}{l} \text{Mean } 2002-2009\\ (20 \leq Tenure < 22) \end{array}$ | .74 | 3.96 | 4 | 4.77 |
| Observations | $3,\!065,\!724$ | $2,\!302,\!058$ | $2,\!302,\!058$ | $2,\!302,\!058$ |

Table 5: Overall regression discontinuity results

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full-time private-sector formal employees 18–54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for four months of UI. Those with more than 24 months of tenure at layoff were eligible for five months of UI (extension). Those with tenure between 22 and 24 months were eligible for four or five months of UI (see text) and are excluded from the regressions. The table displays the coefficients from regressing various outcomes (listed above each column) on a dummy for having more than 24 months of tenure at layoff (tenure cutoff). The sample includes observations with tenure at layoff between 15 and 36 months. The regressions include fixed effects for (calendar) separation and hiring months and linear controls in tenure on each side of the cutoff. Outcomes in columns (2)–(4) are conditional on take–up. Column (1) finds no effect on UI take–up. Column (3) shows that the one–month UI extension increased benefit duration by .91 month on average at the cutoff. But column (4) shows that only .08 month is due to behavioral responses (the construction of the outcome in column 4 is detailed in the text). Beneficiaries with tenure levels below the cutoff would have collected .81 month (4.77-3.96), had they been eligible for the extension. Column (2) suggests that there was some very limited behavioral response in the first four months of benefit collection. We present many robustness checks in Table A.5.

| | (1) | (2) | (3) | (4) | |
|---|-------------------|---------------|-------------|--------------------|--|
| | Extended | UI duration | vs. counter | factual | |
| Tenure ≥ 24 months | $.0819^{***}$ | .0833*** | .0832*** | .0831*** | |
| | (.0033) | (.0033) | (.0032) | (.0026) | |
| Formality rate in mesoregion | 5619^{***} | 3363*** | 4198*** | 3883*** | |
| | (.0188) | (.017) | (.0248) | (.0242) | |
| Tenure ≥ 24 months \times Formality rate | $.1142^{***}$ | $.1102^{***}$ | .1191*** | $.1207^{***}$ | |
| | (.0221) | (.0209) | (.0207) | (.0198) | |
| Observations | 2,302,058 | 2,302,058 | 2,302,058 | 2,302,058 | |
| Year fixed effects | No | Yes | Yes | Yes | |
| Mesoregion fixed effects | No | No | Yes | Yes | |
| Other controls | No | No | No | Yes | |
| Mechanical and behavioral costs of the UI extension | | | | | |
| Mech. | $\cos t \pmod{1}$ | Beh. cost | (month) | $\widetilde{\eta}$ | |
| Formality rate $= mean15$.8 | 8705*** | .065 |)*** | $.0747^{***}$ | |
| | (.0037) | (.00 |)37) | (.0043) | |
| Formality rate $= mean$.8 | 8269*** | .083 | 1*** | $.1005^{***}$ | |
| | (.0018) | (.00 | 026) | (.0033) | |
| Formality rate $= mean + .15$ | 7833*** | .101 | 2*** | $.1292^{***}$ | |
| | (.0036) | (.00 | (42) | (.0057) | |

Table 6: Regression discontinuity results and formal employment rates

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full-time private-sector formal employees 18-54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for four months of UI. Those with more than 24 months of tenure at layoff were eligible for five months of UI (extension). Those with tenure between 22 and 24 months were eligible for four or five months of UI (see text) and are excluded from the regressions. The sample includes observations with tenure at layoff between 15 and 36 months. In the top panel, coefficients in the first row capture the average behavioral cost (in months) at the cutoff of a one-month UI extension. Coefficients in the second row capture how the mechanical cost of a one-month UI extension varies with formal employment rates. Coefficients in the third row capture how the behavioral cost varies with formal employment rates. The regressions include fixed effects for (calendar) separation and hiring months, linear controls in tenure on each side of the discontinuity, and these controls interacted with formal employment rates (the construction of the outcome is described in the text). Column 2 includes year fixed effects. Column 3 includes year and mesoregion fixed effects. Column 4 adds dummies for sector of activity, gender, education, and firm size, as well as fourth-order polynomials in age and log real wage before layoff. Across specifications, the average behavioral cost is around .08 month, the mechanical cost is decreasing and the behavioral cost increasing with formal employment rates. These relationships are not due to fixed differences across areas but hold for marginal changes in formal employment rates (column 3). These relationships are not simply due to composition effects (column 4). We present many robustness checks in Table A.6. The bottom panel uses the specification in column (4) and provides estimates of the behavioral cost and the mechanical cost at different levels of formal employment rates. Estimates are obtained following a similar procedure as in Table 3. The pseudo-elasticity $\tilde{\eta}$, the ratio of the behavioral to the mechanical cost, measures efficiency costs (s.e. are obtained by the delta method). Estimates show that the mechanical cost is large and decreases with formal employment rates, and that the behavioral cost increases with formal employment rates. The resulting efficiency costs are small because most of the increase in benefit duration is not due to behavioral responses, but the efficiency costs rise with formal employment rates.

Table 7: Disposable income after v.s. before layoff

| | (1) | (2)North–Eas | (3)t | (4) South | (5) -East and | (6) South |
|------------------------------------|-----------|---------------|---------|--------------|------------------|---------------------|
| Formality rate Share unemployed | | $.279 \\ 478$ | | | $.364 \\ 434$ | |
| if UI exhaustee | | .110 | | | .101 | |
| Average disposable incom | ne levels | | | | | |
| Prior to layoff | | R 248.2 | | | R 361.8 | |
| Formally reemployed | 0169 | 0145 | 0473 | 0878** | 091** | 0979*** |
| vs. prior to layoff | (.0689) | (.0684) | (.0648) | (.0399) | (.0395) | (.0375) |
| Informally reemployed | 3559*** | 3205*** | 2454*** | 3438*** | 3358*** | 2864*** |
| vs. prior to layoff | (.0477) | (.0485) | (.0489) | (.029) | (.029) | (.0286) |
| $Unemployed^a$ | 5408*** | 5627*** | 5599*** | 5114*** | 532*** | 5229*** |
| vs. prior to layoff | (.0291) | (.0286) | (.0283) | (.0191) | (.0181) | (.0177) |
| First controls | No | Yes | Yes | No | Yes | Yes |
| Additional controls | No | No | Yes | No | No | Yes |
| Observations | 3245 | 3245 | 3245 | 11318 | 11318 | 11318 |

Data from monthly urban labor force surveys covering the six largest metropolitan areas of Brazil (PME, 2003–2010). The sample includes full-time private-sector formal employees 18–54 years old with more than 24 months of tenure in the month before their layoff and workers who lost their formal job with more than 24 months of tenure at layoff. These workers are eligible for five months of UI in Brazil. The table displays disposable income levels of displaced formal workers who are reemployed (in)formally in the first five months after layoff (upon reemployment) or unemployed around benefit exhaustion, relative to the average disposable income levels prior to layoff (repeated cross-sections). Columns (1)–(3) include workers from the two metropolitan areas in the North–East. Columns (4)–(6) include workers from the four metropolitan areas in the South–East and South. Columns (2) and (5) controls for gender, year, calendar month, and metropolitan area fixed effects. Columns (3) and (6) add fixed effects by education levels, and second–order polynomials in age and tenure. Exchange rate: R $1.9\simeq$ US\$1 (in R\$ of 2000). ^a 37% have no disposable income in the South–East and the South.

| (1) | (2) | (3) | (4) |
|----------------|--------------|--|--|
| | | Welfare | effects |
| Risk aversion | Social value | Low formal | High formal |
| + social pref. | of insurance | employment rates | employment rates |
| .25 | .1 | 03 | 1 |
| .44 | .17 | .07 | 0 |
| .75 | .29 | .23 | .15 |
| 1 | .39 | .36 | .27 |
| 2 | .78 | .88 | .77 |
| 3.85 | 1.5 | 1.85 | 1.69 |
| $2 \\ 3.85$ | $.78 \\ 1.5$ | $\begin{array}{c} .88\\ 1.85\end{array}$ | $\begin{array}{c} .77\\ 1.69\end{array}$ |

Table 8: Calibrated welfare effects of a marginal UI extension

The table displays welfare effects of a marginal UI extension (in **bold**) obtained from evaluating equation (10). Welfare effects are measured in terms of an equivalent percentage change in the total payroll of eligible formal employees. The social value of insurance captures the relative social value of \$1 for the average UI exhaustee (recipient) compared to the average formal employee before layoff (taxpayer). A value of 1.5 means that the social value of \$1 is 150% larger for UI exhaustees. A UI extension increases welfare if the social value of insurance exceeds efficiency costs $\tilde{\eta}$. The social value of insurance is calibrated using the decompositions in equations (11)-(12) and disposable income ratios from Table 7 for different values of γ (see text). γ captures both an average coefficient of relative risk aversion and social planner preferences towards redistribution. The social value of insurance is high if UI exhaustees have relatively little disposable income or if γ is high. We use the same social value of insurance in labor markets with different formal employment rates because we do not find evidence of differential disposable income ratios in Table 7. We use estimates of efficiency costs from Table 3 (low formal=.12, high formal=.175). For a given value of γ , the table displays the corresponding social value of insurance and the resulting welfare effects in labor markets with relatively high and relatively low formal employment rates. Alternatively, without relying on our calibration, the table displays the welfare effects for a given social value of insurance. Because efficiency costs are small, welfare effects are positive unless the social value of insurance is very low. Welfare effects are positive as long as the social value of 1 is 17.5%higher for UI exhaustees than for the formally employed. For high values of γ (or high social value of insurance), welfare effects may be sizable. For $\gamma = 1$ (social value of \$1 is 39% higher for UI exhaustees), extending UI benefits by one month has a similar effect on welfare as increasing wages of eligible formal employees by .27%-.36%. Welfare effects decrease with formal employment rates (a difference of 33% for $\gamma = 1$) because of increased efficiency costs.

A Appendix

A.1 Institutional background

The Brazilian UI program

Unemployment insurance was first introduced in March 1986, but with a very small scope. A more complete UI program was established in the 1988 Constitution and approved in January 1990. The law created the Workers' Support Fund (FAT), financed by firms' payments of a .65% tax on total sales. The fund is managed by a committee (CODEFAT) composed of representatives of the government, unions, and employers, and was designed to finance both the UI program and active labor market policies. In June 1994, Law 8900 reformed the UI program, giving it its current format. The 1994 UI legislation also enabled the committee to extend UI for some groups of workers (workers in specific regions and/or sectors of the Brazilian economy) for up to two months without approval of Congress. The only restriction is that expenditures generated by the additional payments should not cost more than 10% of the UI fund's liquidity reserves.

Workers involuntarily displaced from a private formal job with at least six months of tenure at layoff are eligible for three to five monthly UI payments. Maximum benefit duration depends on the number of months of formal employment in the 36 months prior to layoff T_{36} : three months of UI if $T_{36} \in [6, 12)$, four months of UI if $T_{36} \in [12, 24)$, and five months of UI if $T_{36} \geq 24$. There is a 30-day waiting period before a first UI payment can be collected. Benefit levels are based on the average wage in the three months prior to layoff. Replacement rates are constant and start at 100% at the bottom of the wage distribution, but they are down to 60% for workers who earned three times the minimum wage. Benefits can be used discontinuously over a period of 16 months after which a worker is again eligible for the full maximum benefit duration.

Brazilian labor legislation

The Brazilian labor code (Consolidação das Leis do Trabalho - CLT) was created in 1943. Two major revisions were implemented since then: in 1964, when the military regime restricted the power of labor unions, and in the 1988 Constitution, when workers' benefits were increased and workers' rights to organize were reintroduced. CLT is very broad and detailed, containing more than 900 articles (Gonzaga, 2003). Under Brazilian labor legislation, hiring a formal worker is costly. Payroll taxes are high, including 20% for Social Security contributions; 8% deposited in the worker's severance account (see below); and 7.8% for funding an array of programs (training, education, land reform, etc.). Formal workers are also entitled to receive at least the minimum wage, a 13th monthly wage, 30 days of paid leave per year remunerated at 4/3 of the average monthly wage, a maternity leave of 120 days, an overtime rate of 50% for hours exceeding 44 hours a week, etc.

Job protection institutions

Despite having very restrictive labor legislations, job and worker turnover rates are very high in Brazil compared to other countries. Dismissal costs are close to the average of other Latin American countries, but many authors argue that the design of job security programs in Brazil creates perverse incentives that stimulate labor turnover (Amadeo and Camargo, 1996; World Bank, 2002; Gonzaga, 2003).

Severance payment accounts. Since 1966, the main component of job security is the FGTS (Fundo de Garantia por Tempo de Serviço) system, a seniority fund scheme. Employers must deposit 8% of a worker's monthly wage into an individual account, managed by Caixa Econômica Federal, a state bank. Deposits are adjusted monthly but real rates of return are negative. Employees can usually only access the account upon layoff or retirement. In the case of layoff, employers currently must pay a fine equivalent to 50% of the amount deposited during the worker's tenure at the firm (40% is paid to the worker and, since 2001, 10% is paid to the government).

Advance notice of layoff. The other important component of job security legislation in Brazil is advance notification. The first three months of employment are considered a probationary period in Brazil. Employers laying off workers with more than three months of tenure must provide a worker with a one-month advance notice.⁵¹ During this month, wages cannot be reduced and employers must allow a worker up to two hours a day to look for a new job.

Mediation meeting. Any layoff of workers with more than 12 months of tenure must be signed by a representative of the Labor Ministry (or the unions) who verifies that workers received all payments they were entitled to. This increases oversight of the layoff process and constitutes a significant administrative burden, as officials are unable to visit every worksite each month.

A.2 A model of job–search with informality

We develop a model of endogenous job-search with informal work opportunities to highlight the tradeoff between insurance and efficiency faced by a social planner deciding on the maximum UI benefit duration. To simplify derivations and notations, we first assume a fixed horizon of T periods, but we set up the problem such that the budget constraint of the social planner is consistent with the steady state budget constraint (1). In particular, we assume that UI taxes are levied only on workers who do not lose their formal job (Chetty, 2006; Kroft, 2008). We later show how the results carry on to an infinite horizon model. The measure of efficiency cost and the welfare formula we derive are robust to relaxing many assumptions of the model (e.g., introducing heterogeneity) or to adding other margins of endogenous behaviors, as long as an envelope condition applies to the agents' problem (Chetty, 2006).

⁵¹Since 2011, workers have been entitled to an advance notice that increases from one to three months depending on seniority.

Workers' problem. Assume a population of formal employees of measure 1 living for T periods. At the beginning of period 1, they lose their formal job with some probability q. Workers who do not lose their formal job stay employed until T, earning wage w^f , and paying tax τw^f each period. Their per-period utility is $u \left(w^f \left(1-\tau\right)\right)$. u(.) is assumed to be strictly concave.⁵² In this setup, the average number of contribution periods to the UI system for a given layoff $(D^f$ in Section 2) is $\frac{\left[(1-q)T\right]}{q}$. Upon layoff, workers become unemployed and eligible for UI for P periods. UI benefits b_t are defined as $b_t = rw^f$, with replacement rate r for period t = 1...P after layoff, and $b_t = 0$ otherwise.

While unemployed, a worker decides each period how much effort e at a cost z(e) to invest in finding a new job. Search efforts are normalized to correspond to job-finding probabilities. Cost functions are assumed to be convex. With probability 1 - e, she does not find a job and stays unemployed. With probability e, she finds a job. She can increase her probability of returning to a formal job by investing formal search effort f at a cost $\theta z(f)$. She thus finds a formal job with probability ef and an informal job with probability e(1-f). Working informally, she earns wage $w^i < w^f$. She can always search for a formal job at the same cost $\theta z(f)$ in subsequent periods. To introduce enforcement in the model, we further assume that informal jobs are detected by the government with probability p. If detected, an informal worker falls back into unemployment and loses her UI benefits. Both the unemployed and the "undetected" informally employed draw UI benefits b in the first Pperiods after layoff. The unemployed have a minimum consumption level o. The traditional view of informality implies high values of θ (high formal search costs). The more recent view corresponds to low values of θ and small wage differentials. In many developing countries, detection probabilities p are low. When investigating the social planner problem below, we thus abstract from this and set p = 0.

The value function of being unemployed at the start of a period J_t^o solves:

$$J_{t}^{o} = \max_{e_{t}} (1 - e_{t}) U_{t} + e_{t} J_{t}^{i} - z (e_{t})$$

where J_t^i is the value function of having an informal job in period t with the option to look for a formal job. It solves:

$$J_t^i = \max_{f_t} \left(1 - f_t\right) Z_t + f_t V_t - \theta z(f_t)$$

V, Z and U are respectively the value function of being formally employed, informally employed or unemployed in a given period (after job search has occurred). We have:

$$V_{t} = u (w^{f}) + V_{t+1}$$

$$Z_{t} = (1 - p) [u (w^{i} + b_{t}) + J_{t+1}^{i}] + p [u (o) + J_{t+1}^{o}]$$

$$U_{t} = u (o + b_{t}) + J_{t+1}^{o}$$

⁵²Allowing for different utility functions in different labor statuses does not affect our main conclusions.

where $b_t = b$ for t = 1...P and $b_t = 0$ otherwise.⁵³

The workers' problem is to maximize J_1^o by choosing optimal levels of search intensity of both types in each period until formal reemployment. At an optimum, we have:

$$V_t - Z_t = \theta z'(f_t)$$
$$J_t^i - U_t = z'(e_t)$$

Define O_t and I_t as the share of displaced formal employees unemployed and informally reemployed at the end of period t, with $O_0 = 1$ and $I_0 = 0$. The hazard of formal reemployment in a given period is $O_{t-1}e_tf_t + I_{t-1}f_t$. The solution to this dynamic problem determines the survival rate out of formal employment and therefore the average UI benefit duration, B.

To illustrate the mechanisms discussed in the paper, we obtain the following comparative statics for one-period changes in the parameters, assuming O_{t-1} and I_{t-1} fixed.⁵⁴ The behavioral cost is obtained by the derivative of the search efforts with respect to b_t $(O_{t-1}\frac{de_t f_t}{db_t} + I_{t-1}\frac{df_t}{db_t})$. The change in the behavioral cost following a change in a parameter κ is obtained by the derivative of this behavioral cost with respect to the parameter $(O_{t-1}\frac{d^2e_t f_t}{db_t d\kappa} + I_{t-1}\frac{d^2 f_t}{db_t d\kappa})$. The change in the mechanical cost following a change in a parameter is obtained by the derivative of the search efforts with respect to the parameter $(O_{t-1}\frac{d^2e_t f_t}{db_t d\kappa} + I_{t-1}\frac{d^2 f_t}{db_t d\kappa})$.

The hazard of formal reemployment decreases with an increase in UI benefits (behavioral cost):

$$\frac{df_t}{db_t} < 0, \quad \frac{de_t}{db_t} < 0$$

The hazard of formal reemployment increases when formal search costs decrease (mechanical $cost\downarrow$); the impact of an increase in UI benefits is exacerbated when formal search costs decrease (behavioral $cost\uparrow$)

$$\frac{df_t}{d\theta} < 0, \quad \frac{de_t}{d\theta} < 0, \frac{d^2f_t}{db_td\theta} > 0, \frac{d^2e_t}{db_td\theta} > 0$$

The hazard of formal reemployment increases when formal wages increase (mechanical $\cot \downarrow$); the impact of an increase in UI benefits is exacerbated when formal wages increase relatively (behavioral $\cot \uparrow$)

$$\frac{df_t}{dw^f} > 0, \quad \frac{de_t}{dw^f} > 0, \frac{d^2f_t}{db_tdw^f} = 0, \frac{d^2e_t}{db_tdw^f} < 0$$

⁵³Simulations in Chetty (2008) suggest that this class of models is well defined.

⁵⁴There is very little room for anticipation behaviors to matter in Brazil, so the assumption is not restrictive for the Brazilian case. The impact of multi-period changes in the parameters includes cross–period effects whose signs will depend more heavily on functional form assumptions.

The impact of an increase in the detection probability p on the hazard of formal reemployment is ambiguous: it discourages overall search but encourages formal search conditional on searching for a job. Likewise, the impact of an increase in UI benefits on overall search effort is exacerbated but the impact on formal search effort is reduced.

$$\frac{df_t}{dp} > 0, \quad \frac{de_t}{dp} < 0, \\ \frac{d^2f_t}{db_tdp} > 0, \\ \frac{d^2e_t}{db_tdp} < 0$$

Social planner's problem. Following Schmieder, von Wachter and Bender (2012), we assume that P can be increased by a fraction of 1 such that a marginal change in P can be analyzed. A marginal change in P then corresponds to a marginal change in b_{P+1} , the benefit amount after regular UI exhaustion, times b.

To derive a welfare formula, we follow Saez (2002) and assume that there are M types of individuals in our population indexed by m = 1, ..., M, in proportion h_m , whose utilities enter the social welfare function with weight μ_m . Define S_t , the average survival rate out of formal employment in period t.

$$S_t = \int_m S_{m,t} h_m dm = \int_m \left[O_{m,t} + I_{m,t} \right] h_m dm$$

We have $B = \sum_{t=1}^{P} S_t$, the average benefit duration. The problem of the social planner is to choose the maximum benefit duration P that maximizes the social welfare function such that a balanced-budget constraint holds:

$$\max_{P} W = q \int_{m} \mu_{m} J_{1,m}^{o} h_{m} dm + (1-q)T \int_{m} \mu_{m} u_{m} \left(w^{f} \left(1 - \tau \right) \right) h_{m} dm$$

s.t. $\tau = rB \frac{q}{[(1-q)T]}$

The mechanical and behavioral costs (in months) of a marginal UI extension are then:

$$Mechanical = S_{P+1}$$
$$Behavioral = \sum_{t=1}^{P+1} \frac{dS_t}{dP}$$
$$\frac{dB}{dP} = Mechanical + Behavioral$$

As workers choose search efforts optimally, we use the envelope theorem to solve the planner's problem. The welfare effect of increasing P by one period is (first–order condition):

$$\frac{dW}{dP} = q \ b \ S_{P+1} \ g^{U_{P+1}} - T \ (1-q) \ w^f \ \frac{d\tau}{dP} \ g^E$$
$$\frac{dW}{dP} = q \ r \ w^f \ S_{P+1} \ g^{U_{P+1}} - q \ r \ w^f \ \frac{dB}{dP} \ g^E$$
$$\frac{dW/dP}{w^f g^E} = q \ r \ S_{P+1} \ \left[\frac{g^{U_{P+1}} - g^E}{g^E} - \tilde{\eta}\right]$$

where $\tilde{\eta} = \sum_{t=1}^{P+1} \frac{dS_t}{dP} / S_{P+1}$. $g^{U_{P+1}}$ and g^E are the social value of \$1 for the average UI exhaustee and the average UI contributors, respectively.

$$g^{U_{P+1}} = \frac{1}{S_{P+1}} \int_{m} \mu_m \left[O_{m,P+1} u'_m \left(o + b_{P+1} \right) + (I_{m,P+1}) u'_m \left(w^i + b_{P+1} \right) \right] h_m dm$$
$$g^E = \int_{m} \mu_m u'_m \left(w^f \left(1 - \tau \right) \right) h_m dm$$

The infinite horizon model

Consider the discrete time infinite horizon model where a representative agent cycles in and out of formal employment as in Section 2. Denote ω_t the agent's labor status in period tand n_{ω_t} the probability that the agent is in labor status ω in period t. Because UI benefits are limited in time and the agent can work in both formal and informal sectors, there are many possible labor statuses: (i) formally employed, (ii) informally employed without UI benefits, (iii) informally employed with UI benefits in period h=1,2,...,P since layoff from the formal sector, (iv) unemployed without UI benefits, (v) unemployed with UI benefits in period h=1,2,...,P since layoff from the formal sector. In each labor status, the agent consumes c_{ω} and invests search efforts e_{ω} (0 if employed) and f_{ω} (0 if formally employed). The search efforts and the layoff probability determine the transition matrix between labor statuses from one period (ω_{t-1}) to the next (ω_t) given the model in Section 2. Taking the UI program { b, P, τ } as given, the agent chooses search efforts to maximize the expected utility:

$$\mathbb{E}_{1}\sum_{t=1}^{+\infty} \delta^{t} \left\{ \sum_{\omega_{t}} n_{\omega_{t}} u\left(c_{\omega_{t}}\right) - \sum_{\omega_{t-1}} n_{\omega_{t-1}} \left[z\left(e_{\omega_{t-1}}\right) + \theta z\left(f_{\omega_{t-1}}\right) \right] \right\}$$

where $\delta < 1$ is the discount factor and \mathbb{E}_0 is the mathematical expectation given the agent's information in period 1.

In the steady state of this dynamic model, all variables are constant $(n_{\omega}, c_{\omega}, e_{\omega}, f_{\omega})$ and determine D^f , D^u , and B, the average length of a formal employment spell, of a spell out of formal employment, and of a benefit collection spell, respectively. Given UI benefits b, the planner's problem in steady state is to choose P to maximize the agents' per-period utility given the per-period budget constraint (1). Using the envelope theorem, we obtain the first-order condition (2). We can assume that there are M types of individuals as above to introduce preferences for redistribution beyond the insurance motive.

A.3 Figures and Tables

Figure A.1: Informal labor in Latin America and the Caribbean (% employed)



Comparing informality across countries is challenging because it is unclear which jobs are actually monitored by, or registered with, government agencies. In this figure, an individual is considered to be an informal worker if she is (i) an unskilled self-employed, (ii) a salaried worker in a small private firm, or (iii) a zero-income worker.

(a) Formal employment rates, 2000 census



Figure A.2: Geographical distribution and evolution of formal employment rates in Brazil

(b) Formal employment rates, 2010 census

The maps display the variation in formal employment rates (private-sector formal employees within the 18–54 years old population) across space in Brazil, based on the 2000 (panel a) and 2010 censuses (panel b). The darker lines identify state boundaries. The thinner lines identify mesoregion boundaries, the next geographical subdivisions in Brazil. The maps show that there is tremendous variation in formal employment rates across states in Brazil. The North and the Northeast are poorer and less formal. There is also variation within state, however. Brazil experienced rapid economic growth in the last decade. Formal employment rates increased across the country (darker shades on

panel b) but not uniformly. We obtain a similar pattern if we include public employees.

Figure A.3: Replacement rate in the Brazilian UI program



The black line displays the replacement rate of UI benefits as a function of the wage in the lost job (expressed in multiple of minimum wages). The grey line displays the density of the wage distribution at layoff. UI benefits cannot be inferior to the minimum wage. Since 1994, replacement rates depend on the wage (in multiples of the minimum wage) prior to layoff w as follows: 0.8 if w < 1.65; $\frac{(0.8)(1.65)+(0.5)(w-1.65)}{w}$ if $1.65 \le w \le 2.75$; $\frac{1.87}{w}$ if $w \ge 2.75$. The kernel density corresponds to the wage distribution at layoff for a random sample of 10,000 displaced formal employees (eligible for five months of UI) in each year between 1995 and 2009.

Figure A.4: UI eligibility, average benefit duration, and formal reemployment rates

Survival rate out of formal employment .1 .2 .3 .4 .5 .6 .7 .8 .9

0

Ó 1 ź Ś ά 5 6

Tenure<5

--- 12<Tenure<22

(a) Average benefit duration by tenure prior to layoff

9 12 15 18 21 2- -Tenure prior to layoff (months)

24 27

30 33 36

ß

Average benefit duration (months) 1 2 3 4

0

Ó Ś 6

(b) Survival rate out of formal employment by tenure prior to layoff

_ _ _ _ .

Months since involuntary layoff

6<Tenure<11

7

24<Tenure>36

9 8

10 11 12





Figure A.5: Treatment (T) and control (C) areas for the 1996 temporary UI extension

Highlighted are mesoregions that include treatment (T) and control (C) areas for the 1996 temporary UI extension. Treatment and control areas are similarly spread over the country and span a similar range of formal employment rates.

Figure A.6: Test of the common trend assumption for the 1996 temporary UI extension

(a) Survival rates out of formal employment for UI non-takers, control

(b) Survival rates out of formal employment for UI non-takers, treatment



Full-time private-sector formal employees 18–54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. The sample includes only workers who did not take up UI benefits. These workers should not have been affected by the 1996 temporary UI extension (the take-up decision was taken before the UI extension was announced). The left panel displays survival rates out of formal employment after layoff in each year for displaced workers from control areas. The right panel displays survival rates out of formal employment after layoff in each year for displaced workers from treatment areas. We find no sign of differential trends in treatment areas in treatment year. This supports our identifying assumption that trends would have been similar for UI beneficiaries in the absence of the UI extension.



Figure A.7: Tenure distribution at layoff and tenure–based discontinuities in UI eligibility

Private–sector formal employees with more than 6, 12, and 24 months of tenure at layoff are eligible for three, four, and five months of UI respectively (if they had no other job in the previous three years). These tenure–based discontinuities in eligibility provide potential regression discontinuity designs. The tenure density, however, is not continuously distributed across the first two relevant tenure cutoffs. The upward jump in the density at six months may be due to the absence of experience rating of UI benefits in Brazil. The discontinuity at 12 months cannot be due to the increase in UI benefits for which a worker is now eligible. Indeed, the layoff density jumps downward. In Brazil, firing costs are discontinuously increased at three months of tenure (end of probationary period) and 12 months of tenure (administrative burden and oversight of the layoff process). Firms clearly react to changes in firing costs by adjusting their layoff decisions. The tenure density at layoff is continuous beyond one year of tenure, in particular around the last relevant tenure cutoff (24 months). The higher firing costs that firms are facing at those tenure levels, the higher value of such jobs for workers, and the additional scrutiny over the layoff process appear sufficient to prevent responses at the layoff margin, even in the absence of experience rating.



Full-time private-sector formal employees 18–54 years old, laid off between 1997 and 2009. Workers with more than 24 months of tenure at layoff were eligible for five months of UI. Workers with less than 22 months of tenure at layoff were eligible for four months of UI. Workers with tenure between 22 and 24 months at layoff were eligible for either four or five months of UI (see text). Outcomes are averaged out by month of tenure. Panel (a) displays the share of observations by tenure bin. The other panels display (b) the logarithm of the real wage in the lost job, (c) the share male, (d) age, (e) years of education, and (f) formal employment rates in the mesoregion in the year of layoff. There is no discontinuity in the tenure distribution at layoff at the 24–month cutoff. There is also no clear discontinuity in the value of covariates at the 24–month tenure cutoff. This visually confirms regression results in Table 4.
| | | (1) | (2) | (3) | (4) |
|--------------------------|-------|-----------|-------------------------|--------------------------|-------------|
| Outcomes | Mean | Coefficie | nt on form | al employn | nent rates |
| | | Using sta | ate-level for | nal employn | nent rates |
| UI take-up | .8622 | 09** | 1559*** | .1602 | .1555 |
| _ | | (.041) | (.039) | (.1016) | (.0964) |
| Regular benefit duration | 4.938 | 2695*** | 1329*** | 2089** | 1679^{**} |
| | | (.0311) | (.0178) | (.0869) | (.076) |
| Mechanical cost of a | 1.687 | 8061*** | 4938*** | 7764* ^{**} | 7247*** |
| two–month UI extension | | (.0797) | (.0589) | (.2294) | (.2234) |
| | | . , | Years a | fter 2002 | . , |
| UI take-up | .8773 | 1817*** | 2018*** | 0135 | .0096 |
| | | (.0333) | (.0326) | (.1454) | (.1454) |
| Regular benefit duration | 4.907 | 3263*** | 2423*** | 5467*** | 4341*** |
| | | (.0546) | (.0463) | (.1367) | (.1254) |
| Mechanical cost of a | 1.605 | 9726*** | 7574*** | -1.231*** | -1.195*** |
| two–month UI extension | | (.1523) | (.1326) | (.2046) | (.1926) |
| | | Mesoregio | ns with aver | age formal e | mployment |
| | | rates | between 5 th | and 95 th per | centile |
| UI take-up | .8644 | 093** | 1885*** | .1452** | .1368** |
| | | (.0374) | (.042) | (.0569) | (.0547) |
| Regular benefit duration | 4.935 | 4071*** | 2121*** | 1969*** | 1605*** |
| | | (.0513) | (.0294) | (.0723) | (.0619) |
| Mechanical cost of a | 1.671 | -1.186*** | 7385*** | 6605*** | 6034*** |
| two–month UI extension | | (.137) | (.0948) | (.2112) | (.2017) |

Table A.1: Mechanical cost and formal employment rates (robustness checks)

Significance levels: * 10%, ** 5%, ***1%. Random samples of full-time private-sector formal employees 18–54 years old, laid off between 1995 and 2009, and eligible for five months of UI. The table presents robustness checks for the results in Table 2. The table displays coefficients from regressing the same outcomes on formal employment rates (see Table 2 for a description of the outcomes and the baseline specifications). The top panel (s.e. clustered by 27 states) uses formal employment rates by state from yearly household surveys (PNAD). The middle panel (s.e. clustered by 137 mesoregions) uses only workers laid off between 2002 and 2009. The bottom panel (s.e. clustered by 124 mesoregions) excludes mesoregions with average formal employment rates over the period below the 5th and above the 95th percentile. Results in this table confirm results from Table 2. The mechanical cost of a hypothetical two-month UI extension is high on average but it decreases with formal employment rates.

Table A.2: Distribution of sample for the 1996 temporary UI extension

| Year | Month | Control | Rio | Other Treat | Total |
|------|-------|---------|----------------------|-------------|-------------|
| 1995 | April | .3 | .23 | .47 | $37,\!819$ |
| | May | .31 | .23 | .46 | 43,387 |
| 1996 | April | .33 | .22 | .45 | $33,\!994$ |
| | May | .36 | .2 | .44 | $34,\!453$ |
| 1997 | April | .32 | .23 | .46 | 40091 |
| | May | .33 | .22 | .45 | $40,\!134$ |
| All | All | .32 | .22 | .46 | $229,\!878$ |

Full-time private-sector formal employees 18–54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996, in treatment areas, these workers were eligible for seven months of UI benefits. The table displays the distribution of our sample across control and treatment areas (Rio de Janeiro and other treatment areas), and control and treatment years.

| Variable | Year | Control | Rio | Other Treat | Treat-C | ontrol |
|-------------------------|-----------|---------|-------|-------------|---------------|---------|
| Male | 1995&1997 | .6543 | .6737 | .6649 | .0135 | (.0122) |
| | 1996 | .654 | .6664 | .6547 | .0044 | (.0138) |
| Age | 1995&1997 | 32.15 | 34.04 | 32.94 | 1.155^{***} | (.308) |
| 0 | 1996 | 32.82 | 34.33 | 33.4 | .8707*** | (.2827) |
| Years of education | 1995&1997 | 7.299 | 7.606 | 7.392 | .1643 | (.1362) |
| | 1996 | 7.14 | 7.418 | 7.336 | .2219 | (.1405) |
| Log real wage | 1995&1997 | 6.927 | 6.922 | 6.895 | 023 | (.0888) |
| 0 | 1996 | 6.951 | 6.937 | 6.921 | 0253 | (.0923) |
| Replacement rate | 1995&1997 | .4789 | .4834 | .4912 | .0096 | (.029) |
| * | 1996 | .4881 | .4965 | .5026 | .0126 | (.03) |
| Commerce | 1995&1997 | .2647 | .2424 | .2542 | 0145 | (.02) |
| | 1996 | .2602 | .2534 | .2606 | 0019 | (.0215) |
| Services | 1995&1997 | .303 | .449 | .3657 | .0902** | (.0358) |
| | 1996 | .2984 | .4619 | .378 | .1062*** | (.0403) |
| Industry | 1995&1997 | .3689 | .2452 | .3015 | 0861 | (.0564) |
| | 1996 | .3772 | .2262 | .2927 | 1056* | (.06) |
| Firm size ≥ 100 | 1995&1997 | .3692 | .4026 | .4088 | .0376 | (.0238) |
| | 1996 | .377 | .3892 | .3796 | .0056 | (.0297) |
| Firm size < 10 | 1995&1997 | .2847 | .2496 | .2506 | 0344** | (.0152) |
| | 1996 | .2961 | .2549 | .271 | 0302 | (.0206) |
| Share formally employed | all | .3175 | .3121 | .3043 | 0106 | (.0233) |

Table A.3: Composition of sample for the 1996 temporary UI extension

s.e. clustered by area (29 clusters) in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Full-time privatesector formal employees 18–54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996, in treatment areas, these workers were eligible for seven months of UI benefits. The table displays the composition of our sample across control and treatment areas (Rio de Janeiro and other treatment areas), and control and treatment years. There are some differences between treatment and control areas but these differences appear in treatment and control years.

| | (1) | (2) | (3) | (4) | (5) |
|--------------------------------|---------|-------------|------------------|---------------|---------------|
| | UI | Regular UI | Extended UI | Extended | UI duration |
| | take-up | duration | duration | vs. coun | terfactual |
| | 1 | Controlling | for individual c | haracteristic | s |
| $TreatArea \times Year1996$ | - 0178 | - 0008 | 1 867*** | 2427*** | 1909*** |
| | (0151) | (0024) | (0129) | (02) | (0195) |
| $Troot Aroo \times Voor 1006$ | (.0101) | (.0024) | (.0125) | (.02) | 0740** |
| χ formality > around to | | | | | .0749 |
| \times lormanty > average | | ות | 1 | | (.0555) M |
| | 01.41 | Replacemen | t rate between 2 | 20% and $80%$ | /0 2000*** |
| $TreatArea \times Year1996$ | 0141 | .0007 | 1.871*** | $.2654^{***}$ | .2009*** |
| | (.0182) | (.003) | (.0132) | (.0251) | (.025) |
| $TreatArea \times Year1996$ | | | | | .0929** |
| \times formality > average | | | | | (.0406) |
| | | Excl | uding Rio de Ja | aneiro | · · · · |
| $TreatArea \times Year1996$ | 0084 | 0003 | 1.869*** | $.2579^{***}$ | $.1939^{***}$ |
| | (0166) | (0029) | (0185) | (0239) | (0175) |
| TreatArea × Vear1006 | (.0100) | (.0020) | (.0100) | (.0200) | 1117*** |
| \times formality $>$ average | | | | | (0258) |
| × Iormanty > average | | II | | ator linearly | (.0238) |
| | 0170 | Using forma | al employment i | ates inearr | y 0.400*** |
| $TreatArea \times Year1996$ | 0178 | 0003 | 1.807 | .2469*** | .2492*** |
| | (.0153) | (.0024) | (.0137) | (.0207) | (.015) |
| $TreatArea \times Year1996$ | | | | | 1.084^{***} |
| \times formality rate | | | | | (.2463) |
| | | | | | . , |

Table A.4: Diff-in-diff results for the 1996 temporary UI extension (robustness checks)

s.e. clustered by area (29 clusters). Significance levels: * 10%, ** 5%, *** 1%. The sample includes full-time private-sector formal employees 18–54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996, in treatment areas, these workers were eligible for seven months of UI benefits. The table displays robustness checks for the results in Table 3. The table displays estimates of the difference-in-difference estimator for various outcomes (listed above each column; see Table 3 for a description of the outcomes and the baseline specifications). The top panel includes dummies for education, sector of activity, gender, and firm size, as well as fourth-order polynomials in tenure, age, and log real wage before layoff. The second panel restricts the sample to workers with replacement rates between 20% and 80%. The third panel excludes observations from Rio de Janeiro, more than 20% of our sample. The bottom panel uses formal employment rates entered linearly (demeaned) instead of a categorical variable. Results in this table confirm results from Table 3. A two-month UI extension increased benefit duration by 1.87 months on average, but only .25 month is due to behavioral responses. The behavioral cost is larger in metropolitan areas with higher formal employment rates at the time.

| | (1) | (2) | (3) | (4) |
|------------------------|------------|--------------------|---|----------------------|
| | UI take–up | UI duration | Extended UI | Extended UI duration |
| | | up to 4 months | duration | v.s. counterfactual |
| | | Controlling for i | ndividual chara | acteristics |
| $Tenure \ge 24 months$ | .0305 | .0059*** | .9098*** | .0829*** |
| | (.0224) | (.0011) | (.004) | (.0026) |
| | · · · · | Tenure at layoff b | between 18 and | 30 months |
| $Tenure \ge 24 months$ | .0469 | .0048*** | .8974*** | $.0762^{***}$ |
| | (.033) | (.0018) | (.006) | (.0052) |
| | · · · | Year | rs after 2002 | |
| $Tenure \ge 24 months$ | .0373 | .008*** | .9123*** | .094*** |
| | (.0237) | (.0018) | (.0043) | (.0041) |
| | · · · · | Replacement rat | te between 20% | and 80% |
| $Tenure \ge 24 months$ | .0306 | .0053*** | .8957*** | .0779*** |
| | (.021) | (.0014) | (.0047) | (.0043) |
| | Ì | Mesoregions with a | average formal e | employment |
| | | rates between | 5^{th} and 95^{th} pe | ercentile |
| $Tenure \ge 24 months$ | .0321 | .0052*** | .9124*** ` | .0832*** |
| | (.0227) | (.0016) | (.0047) | (.0043) |

Table A.5: Overall regression discontinuity results (robustness checks)

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full-time private-sector formal employees 18–54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for four months of UI. Those with more than 24 months of tenure at layoff were eligible for five months of UI (extension). Those with tenure between 22 and 24 months were eligible for four or five months of UI (see text) and are excluded from the regressing various outcomes (listed above each column) on a dummy for having more than 24 months of tenure at layoff (tenure cutoff; see Table 5 for a description of the outcomes and the baseline specifications). The top panel includes dummies for year, mesoregion, sector of activity, gender, education, and firm size, as well as fourth-order polynomials in age and log real wage before layoff. The second panel considers a smaller tenure window around the 24–month tenure cutoff. The third panel uses only workers displaced between 2002 and 2009. The fourth panel restricts the sample to workers with replacement rates between 20% and 80%. The bottom panel excludes mesoregions with average formal employment rates over the period below the 5th and above the 95th percentile. Results in this table confirm results from Table 5. A one-month UI extension increased benefit duration by .91 month on average at the cutoff, but only .08 month is due to behavioral responses.

| | (1) | (2) | (3) | (4) |
|---|---------------|--------------------------|-------------------------|---------------------|
| | Extended | d UI dúratio | n v.s. count | erfactual |
| Tenure at layoff | between 18 | and 30 mon | ths | |
| Tenure ≥ 24 months | $.0765^{***}$ | .0772*** | $.0771^{***}$ | $.0781^{***}$ |
| | (.0047) | (.0048) | (.0046) | (.0038) |
| Formality rate in mesoregion | 5932*** | 3612* ^{**} | 4341* ^{**} | 3951* ^{**} |
| | (.0288) | (.0278) | (.0395) | (.0385) |
| Tenure ≥ 24 months \times Formality rate | .1414*** | $.1339^{***}$ | .14*** | $.1412^{***}$ |
| | (.0331) | (.0311) | (.0313) | (.028) |
| Ye | ars after 200 | 02 | | |
| Tenure ≥ 24 months | .0908*** | $.0901^{***}$ | .0899*** | .0895*** |
| | (.0038) | (.0039) | (.0037) | (.0033) |
| Formality rate in mesoregion | 5626*** | 39*** | 8535*** | 8279*** |
| | (.0228) | (.0211) | (.0622) | (.0624) |
| Tenure ≥ 24 months \times Formality rate | $.1051^{***}$ | $.1044^{***}$ | .1118*** | .1139*** |
| | (.027) | (.0258) | (.0255) | (.0258) |
| Replacement ra | ate between | 20% and 80 | 1% | |
| Tenure>24 months | .0766*** | .0808*** | .0807*** | .0793*** |
| — | (.0041) | (.0039) | (.0038) | (.0032) |
| Formality rate in mesoregion | 5872*** | 2386* ^{**} | 3427*** | 3225*** |
| v C | (.0334) | (.0329) | (.0396) | (.0393) |
| Tenure>24 months \times Formality rate | $.1234^{***}$ | $.1262^{***}$ | $.1369^{***}$ | .1366*** |
| | (.0366) | (.0356) | (.0359) | (.0353) |
| Using state-leve | el formal em | ployment ra | tes | |
| Tenure ≥ 24 months | $.0766^{***}$ | .0804*** | .0802*** | .0787*** |
| | (.0042) | (.0039) | (.0039) | (.0033) |
| Formality rate in mesoregion | 6338*** | 2299*** | 5799*** | 5732*** |
| , C | (.0281) | (.0279) | (.0506) | (.0498) |
| Tenure ≥ 24 months \times Formality rate | $.167^{***}$ | $.165^{***}$ | $.1682^{***}$ | $.1691^{***}$ |
| | (.031) | (.0298) | (.0297) | (.0291) |
| Mesoregions with average form | ality rate b | etween 5 th a | nd 95^{th} per | centile |
| Tenure ≥ 24 months | $.0868^{***}$ | .0879*** | $.0877^{***}$ | .0879*** |
| | (.004) | (.0038) | (.0037) | (.003) |
| Formality rate in mesoregion | 5558*** | 3365*** | 6097*** | 5895*** |
| , C | (.0179) | (.0181) | (.0338) | (.0323) |
| Tenure ≥ 24 months \times Formality rate | $.1727^{***}$ | $.1681^{***}$ | $.1686^{***}$ | $.1725^{***}$ |
| | (.0217) | (.0218) | (.0218) | (.0214) |
| Vear fixed effects | No | Ves | Ves | Vos |
| Mesoregion fixed effects | No | No | Ver | Vee |
| Ather controls | No | No | No | Vos |
| | INU | INU | INU | 162 |

Table A.6: Regression discontinuity and formal employment rates (robustness checks)

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. The table displays robustness checks for the results in Table 6 (where the outcome and the baseline specifications are described). The top panel uses a smaller tenure window. The second panel uses only workers displaced between 2002 and 2009. The third panel uses only workers with replacement rates between 20% and 80%. The fourth panel uses state–level instead of mesoregion–level formal employment rates. The bottom panel excludes mesoregions with average formal employment rates over the period below the 5th and above the 95th percentile. Results in the table confirm results from Table 6. The average behavioral cost is around .08 month (first row), the mechanical cost is decreasing (second row) and the behavioral cost (third row) increasing with formal employment rates.

| | (1) Extended UI duration | (2) Extended UI duration |
|--|-------------------------------|---|
| | (using UI data) 1996 tempo | (using formal reemployment) prary UI extension |
| $TreatArea \times Year1996$ | 1.867*** | 1.846*** |
| | (.0137) | (.0168) |
| Mean (control years) | 4.98 | 4.98 |
| Observations | $171,\!407$ | 171,407 |
| | Tenure–ba | sed discontinuity |
| Tenure $\geq 24 \text{ months}$ | .9091*** | .9095*** |
| | (.0043) | (.0027) |
| Mean 2002–2009 ($20 \leq Tenure \leq 22$) | 4 | 3.96 |
| Observations | $2,\!302,\!058$ | $2,\!302,\!058$ |

| | | \mathbf{T} | 11 | | c | | | · . 1 | 1 |
|---------|-------|--------------|------|----------|-----|-----|----------|--------|----------|
| Table A | · · · | Testing | the | accuracy | OT. | Our | counterf | actual | approach |
| 10010 1 | | robuing | UIIC | accuracy | OI | our | counterr | actuar | approach |

Significance levels: * 10%, ** 5%, ***1%. The table provides a test for the accuracy of our approach using workers' formal reemployment patterns to construct the (counterfactual) benefit duration of beneficiaries throughout the paper. The table displays estimates of the difference-in-difference estimator (1996 temporary UI extension, treatment beneficiaries eligible for seven months of UI instead of five months, s.e. clustered by area) and the regression discontinuity estimator (tenure-based discontinuity in eligibility, treatment beneficiaries eligible for five months of UI instead of four months, s.e. clustered by week of tenure) for various outcomes (listed above each column). The regressions in the top panel include (calendar) separation month, year, and area fixed effects (see Table 3 for a full description of the sample). The regressions in the bottom panel include fixed effects for (calendar) separation and hiring months and linear controls in tenure on each side of the discontinuity (see Table 5 for a full description of the sample). Outcomes are conditional on take-up. Column (1) estimates treatment effects on the actual benefit duration using the UI registry data. Formally, in the top panel, the outcome is defined as: $\sum_{i=1}^{5} \mathbb{1}$ (draw ith UI payment) + $\sum_{i=6}^{7} \mathbb{1}$ (draw ith UI payment). Formally, in the bottom panel, the outcome is defined as: $\sum_{i=1}^{4} \mathbb{1}$ (draw ith UI payment) + $\sum_{i=5}^{7} \mathbb{1}$ (draw ith UI payment). The treatment increased average benefit duration by 1.87 months in the top panel and .91 month in the bottom panel. Column (2) shows that the treatment effects on the actual benefit duration in column (1) can be well approximated using workers' formal reemployment patterns after UI exhaustion (coefficients are very similar in columns 1 and 2). Define month r_{regUI} , the month a beneficiary exhausts her "regular" benefit duration (five months in the top panel, four months in the bottom panel). We assume that a beneficiary who exhausts her regular UI benefits and is not formally reemployed within one month (resp. two months) of regular UI exhaustion would draw one extra payment (resp. two extra payments) if she is eligible. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, in the top panel, the outcome is defined as:

$$\sum_{i=1}^{5} \mathbb{1} \left(\text{draw i}^{\text{th UI payment}} \right) \\ + \left[\mathbb{1} \left(\text{draw 5}^{\text{th UI payment}} \right) \times TreatArea \times Year 1996 \times \sum_{j=1}^{2} \mathbb{1} \left(month_{back} > month_{regUI} + j \right) \right]$$

Formally, in the bottom panel, the outcome is defined as:

-

$$\sum_{i=1}^{4} \mathbb{1} \left(\text{draw i}^{\text{th UI payment}} \right) + \left[\mathbb{1} \left(\text{draw 4}^{\text{th UI payment}} \right) \times \mathbb{1} \left(\text{Tenure} \ge 24 \text{ months} \right) \times \sum_{j=1}^{1} \mathbb{1} \left(\text{month}_{back} > \text{month}_{regUI} + j \right) \right]$$

| | (5) Log real wage (if formal in Dec. 2 years later) | 0164 (.0122) | 6.71 6.71 68,589 | .0041 (.0068) | 6.7 | 858,940 | tor (1996 temporary UI rea) and the regression is of UI instead of four essions in the top panel ample). The regressions uure on each side of the ture on each side of the intcome in column (1) is or experiencing at least m formal reemployment from the formal sector in Section 2: delaying or edficient in column (4) sefficient in column (4) |
|-------------------|--|---|---|---------------------------------------|------|--|---|
| ensions | (4) Log real wage (if formal in Dec. 2 years later) | ension 0343*** (.0114) | $\begin{array}{c} 6.71 \\ 6.71 \\ 68,589 \end{array}$ | inuity0061 (.0074) | 6.7 | 858,940 | tee-in-difference estima ths, s.e. clustered by a s eligible for five month each column). The regr ull description of the sz and linear controls in ter nal on take-up. The or olumn (2) is a dummy f (3)-(5) are conditional or age in workers' first nel December two years afte ment. UI extensions red f (frequent) new layoffs in steady state approach I budget because it also . In the top panel, the o e effects as treated bem |
| effects of UI ext | (3) Log real wage (at formal reemployment) | cemporary UI ext .0016 (.0112) | $\begin{array}{c} 6.55 \\ 6.55 \\ 103, 452 \end{array}$ | re-based disconti .0024 (.0052) | 6.5 | 1,348,187 | mates of the different instead of five mon treated beneficiaries comes (listed above (see Table 3 for a f d hiring months, an it comes are conditio ars after layoff; in o ut comes in column (garithm of the real w rmally employed in 7 ut of formal employi tee the occurrence o on). This supports on throng effect on the U on subsequent wages may be due to tenur |
| A.8: Long-term | (2) New layoff (in next 2 years) | 1996 10148*** (.0027) | $\begin{array}{c} .1\\ .1\\ 171,407\end{array}$ | Tenu 0071*** (.0017) | .13 | 2,073,090 | The table displays estiints ar seven months of UI minuity in eligibility, t various long-term out month, year, and area calendar) separation an ion of the sample). Of employee in the two ye employee in the two ye employee in the two of m column (3) is the log by a worker if she is fo andogenous) duration o lumn 1), but also redu d to be displaced again action does not have a s find no impact of UI c m (5). The difference |
| Table | (1) Months employed (in next 2 years) | -1.211^{**} (.11) | 8.97 8.97 171,407 | 1999***(.0478) | 8.58 | 2,073,090 | 10%, ** 5%, ***1%. ' beneficiaries eligible for cor (tenure-based disco di by week of tenure) for (calendar) separation: include dummies for (di include dummies for (di include dummies for to is working as a formal is working as a formal is formal sector in the th of the real wage earned if the real wage earned if collection period (co be formally reemploye beyond the benefit du remaining columns, we ger significant in colum |
| | | $\begin{array}{c} {\rm TreatArea} \\ \times {\rm \ Year1996} \end{array}$ | Mean (control years) Observations | Tenure $\geq 24 \text{ months}$ | Mean | 2002-2008 $(20 \le T < 22)$ Observations | Significance levels: * extension, treatment discontinuity estimat months, s.e. clustered months, s.e. clustered include dummies for in the bottom panel discontinuity (see Ta the number of month 1 new layoff from the within two years aftee (4) is the logarithm c in column (5) include beyond the UI benef (column 2; one must formal reemployment UI collection. In the is smaller and no lon |

Chapter 1: Informal Labor and the Cost of Social Programs

reemployment.

Chapter 2

Job–Search Monitoring in a Context of High Informality

with Gustavo Gonzaga

We would like to thank David Card, Frederico Finan, Edward Miguel, and Emmanuel Saez for useful comments and suggestions. We also thank the Ministério do Trabalho e Emprego for providing access to the data and CNPq. All errors are our own.

Governments face two main informational constraints when implementing any program or regulation (e.g., welfare program). First, there is a screening issue. Government may fail to identify the ex-ante population of interest (e.g., poorest households). Second, there is a monitoring issue. Agents may adopt unobserved behaviors to join or escape the population of interest (e.g., reducing work efforts). These two issues increase implementation costs and/or reduce governments' effectiveness at achieving their goals.¹

In developing countries, non-compliance with regulations and eligibility criteria for government programs is widespread. Yet, the enforcement environment is often particularly weak.² On the one hand, these countries may suffer from chronic policy or political failures. Governments may not implement suitable policies. Even if policies are suitable, corrupt officials may prevent effective implementation.³ On the other hand, existing screening or monitoring policies may be optimal given enforcement costs and the actual scope of the issues.⁴ If there is some work on screening, there is much less evidence on monitoring issues in developing countries. We know surprisingly little about the magnitude of behavioral responses to the incentives created by government programs.⁵ We also have little information on the cost of effective monitoring policies to mitigate such behavioral responses. In this paper, we address both limitations for the case of the Unemployment Insurance program (UI hereafter) in Brazil.

UI is a relevant program to study monitoring issues. It has recently been adopted or considered in a number of developing countries.⁶ Moreover, international development agencies have emphatically pointed to the larger behavioral responses it supposedly creates in developing countries where the informal sector is very large. UI requires beneficiaries — displaced formal employees — to not be formally (re)employed. The concern is that informal job op-

¹Screening and monitoring issues are conceptually different, but are not easily separated in practice. For instance, ordeal mechanisms, by increasing participation costs, may screen social program recipients. Increasing participation costs is also likely to mitigate behavioral responses to the incentives created by a social program.

²The informal sector, for instance, accounts for 40% of GDP in Brazil (Schneider, Buehn and Montenegro, 2010). Moreover, inspections aimed at enforcing labor regulations almost exclusively target non–compliance by formal firms in Brazil, even though non–compliance is systematic in the informal sector (Almeida and Carneiro, 2012).

 $^{^{3}}$ A growing body of research investigates strategies to screen recipients of social benefits (Alatas et al., 2012) or align the behavior of government officials with the public interest (Ferraz and Finan, 2011) in developing countries.

⁴For instance, Alderman (2002) finds that local officials use private information, unlikely to be obtained on the basis of a questionnaire or formula, to efficiently target social assistance in Albania. Kleven, Kreiner and Saez (2009) argue that the cost of acquiring information on taxable economic activities decreases over the development process, thus enabling governments to tax more and reach their optimal size.

⁵Camacho and Conover (2011) provide evidence of manipulations of eligibility criteria for a welfare program in Colombia. Manipulations, however, appear to result from the behavior of corrupt officials rather than households' behavioral responses.

⁶Currently some form of UI exists in Algeria, Argentina, Barbados, Brazil, Chile, China, Ecuador, Egypt, Iran, Turkey, Uruguay, Venezuela and Vietnam (Vodopivec, 2009; Velásquez, 2010). Mexico, the Philippines, Sri Lanka, and Thailand have been considering its introduction.

portunities exacerbate UI disincentives to return to a formal job.⁷ Finally, the Brazilian UI program offers a stark example of a weak monitoring environment. Until recently and for over 20 years, there was absolutely no monitoring of formal job–search for UI beneficiaries in Brazil, even though many beneficiaries work informally while drawing UI benefits (Gerard and Gonzaga, 2013a).⁸

We proceed in two steps. First, we derive a theoretical upper bound on the maximum price that a government should be willing to pay, per UI beneficiary, to perfectly monitor formal job search. The welfare gain from monitoring is to enforce (first-best) levels of formal reemployment that would prevail in the absence of UI. This allows the government to save on the *behavioral* cost of the program, the cost due to behavioral responses. However, for monitoring to be effective in a context of imperfect information, monitoring costs must also be paid for beneficiaries who would not be formally reemployed in the absence of UI. The welfare loss from monitoring is thus proportional to the *mechanical* cost of the program, the cost absent behavioral responses. The ratio of the behavioral to the mechanical cost in fact constitutes an upper bound on the maximum price that a government should be willing to pay, per beneficiary, to eliminate behavioral responses. Intuitively, there is little incentive to introduce monitoring if most beneficiaries draw UI benefits mechanically, unless the government is able to target monitoring towards workers with relatively larger behavioral responses.

We then estimate the ratio of the behavioral to the mechanical cost empirically, for a temporary policy that extended UI benefits from five to seven months in Brazil in 1996. The extension was limited to specific urban areas and the differential implementation was unrelated to local labor market conditions. We exploit this quasi-exogenous variation through a difference-in-difference strategy, taking advantage of a unique dataset that matches the universe of formal employment spells in Brazil to the universe of UI payments. Importantly, we investigate to what extent the government could use information readily available ex ante (a signal) to identify worker categories with larger ratios of the behavioral to the mechanical cost. Exploiting the richness of the data, we also show that our results apply beyond the case of a UI extension.

This paper has three main empirical findings. First, there are clear behavioral responses to the incentives of the UI program in Brazil. Nearly no displaced formal worker returns to a formal job before exhausting her UI benefits. Formal reemployment rates then spike at UI exhaustion. The behavioral cost of an exogenous two–month UI extension comes entirely from a two–month shift in the spike. Second, the scope of the monitoring issue is limited.

⁷See Acevedo, Eskenazi and Pagés (2006), Robalino, Vodopivec and Bodor (2009), and Vodopivec (2009). These policy papers cite evidence of moral hazard from Slovenia (van Ours and Vodopivec, 2006), a country with relatively high levels of formality. The proposed alternative is a system of Unemployment Insurance Savings Accounts. The new Jordanian program, for instance, designed in consultation with the World Bank, is a forced savings scheme to which workers contribute when formally employed. "UI benefits" drawn by a worker in excess of what she contributed over her lifetime must be paid back at retirement.

⁸States in the US impose work–search requirements on UI beneficiaries, hoping to reduce UI disincentive effects.

Formal reemployment rates are still very low after UI exhaustion. They are also very low after layoff for non–eligible displaced formal workers. Most beneficiaries would thus draw UI benefits absent any behavioral response. Extending UI by two months, from five to seven months, mechanically increased average benefit duration by 1.7 months. The behavioral cost amounted to only 15.8% of the mechanical cost. As a result, the government should not be willing to pay more than 15.8% of the average benefit level (or 27% of a minimum wage), per beneficiary and month of extension, for a (hypothetical) perfect monitoring technology. To provide some perspective, the ratio of the behavioral to the mechanical cost of a UI extension would be above 100% of the average benefit level in the US, based on estimates from Katz and Meyer (1990). Last, we find that a readily available signal ex ante, the spike in formal reemployment rates at regular UI exhaustion, helps identify worker categories with larger ratios of the behavioral to the mechanical cost. We predict the propensity to be formally reemployed in the two months after regular UI exhaustion in control areas and years using workers' characteristics flexibly. We then estimate the behavioral and mechanical costs separately, by quartile of the predicted propensity. The ratio of the behavioral to the mechanical cost increases by 145% from the first to the fourth quartile. Nevertheless, most of the heterogeneity in behavioral responses is not easily captured by observable characteristics. The maximum price a government should be willing to pay for a monitoring technology that eliminates all behavioral responses in the top quartile remains relatively low, at 22.7% of the average benefit level.⁹ Our results may thus rationalize the lasting absence of formal job-search monitoring in Brazil.

The remainder of this paper is organized as follows. Section 1 briefly describes the necessary background and the data. Section 2 introduces our theoretical framework. The empirical analysis is the focus of Section 3. Section 4 concludes.

1 Background and data

The background and the data are presented at length in Gerard and Gonzaga (2013a).

1.1 The Brazilian UI program

Workers involuntarily displaced from a private formal job with at least six months of tenure at layoff are eligible for three to five monthly UI payments. Workers with more than 24 months of tenure at layoff, our focus for data reasons, are eligible for five months of UI after a 30-day waiting period. Benefit levels are based on the average wage in the three months prior to layoff. Replacement rates start at 100% at the bottom of the wage distribution but are down to 60% for workers who earned three times the minimum wage. Importantly, for more than 20 years (until 2011), there was absolutely no monitoring of beneficiaries' formal job-search efforts. Workers applied in person for UI benefits in the first month only.

⁹The upper bound would tend to infinity if the signal perfectly predicted a worker's propensity to respond to the incentives of the UI program.

Payments were then automatically made available for withdrawal at Caixa Economica, an official bank, every 30 days as long as the worker's name did not appear in a database where employers report new hirings monthly (CAGED).

1.2 Monitoring and informality

The monitoring issue with UI programs in developed countries is that UI reduces the cost of not searching for a job while unemployed, potentially prolonging the length of insured unemployment. States in the US typically impose work-search requirements on UI beneficiaries, hoping to reduce such disincentives. In developing countries, it is widely believed that disincentive effects may be exacerbated by the coexistence of formal and informal employment opportunities. Informal workers, employees in non-complying firms and most self-employed in Latin America, escape oversight from the government. In 2009, 23% and 24% of working adults were informal employees or self-employed in Brazil, respectively. Moreover, many workers transit between formal and informal labor statuses over the course of their lives in Latin American countries (Bosch and Maloney, 2010). The government cannot deny UI to beneficiaries reemployed informally, which constitute a significant share of beneficiaries in Brazil (Gerard and Gonzaga, 2013a). The concern is thus that displaced formal workers not only have an incentive to stay unemployed, but also have an incentive to work informally while drawing UI benefits. Yet, it is unclear whether this extra margin of behavioral response actually exacerbates the magnitude of the associated monitoring issue. Displaced formal workers may be reemployed informally even in the absence of UI.

1.3 Data

We exploit a unique dataset matching the universe of formal employment spells to the universe of UI payments in Brazil from 1995 to 2010. For every displaced formal worker, we observe whether she is eligible for UI, how many UI payments she draws, and how rapidly she returns to a formal job. Moreover, we have information on wage, tenure, age, gender, education, sector of activity, establishment size and location, hiring and separation dates, and reason for separation.

2 Conceptual framework

This section presents the conceptual framework that guides our analysis. We focus on the intuition for the main results. The model that we have in mind is a partial–equilibrium dynamic model of endogenous job–search with no internalities or externalities to search efforts (Chetty, 2008; Schmieder, von Wachter and Bender, 2012), where we introduce informal work opportunities. The model is presented in Gerard and Gonzaga (2013*a*).

A. Setup. Assume a population of displaced formal workers who are eligible for UI benefits b for up to P periods after layoff. They are only denied eligibility when they return

to a formal sector job because the government does not observe informal jobs. Each period, they must decide how much effort to invest in finding a new job in the formal or the informal sector. Search efforts are costly. UI thus increases the value of staying out of the formal sector until benefit exhaustion and likely reduces efforts to search for a formal job.¹⁰

The solution to the workers' dynamic search problem determines the survival rate out of formal employment S_t in each period t after layoff, and thus the average benefit duration $B(b, P) \equiv \sum_{t=1}^{P} S_t(b, P)$. The average cost per beneficiary of providing UI is simply: $b \times B(b, P)$. It can be divided into two components as illustrated in Figure 1 for a UI program offering benefits for two periods. First, some displaced formal workers would not return rapidly to a formal job in absence of UI. These workers (unemployed or informally employed) draw UI benefits mechanically, without changing their behavior. The *mechanical* cost amounts to: $b \times B(0, P)$. Second, there is a behavioral cost. Providing UI benefits reduces incentives to search for and return to a formal job, increasing average benefit duration, and thus the cost of the program. Behavioral responses induce an additional cost equal to: $b \times [B(b, P) - B(0, P)]$.

B. Monitoring formal job-search. In Gerard and Gonzaga (2013a), we take as given the enforcement environment and consider changes in program generosity. In this paper, we study the incentives for the government to monitor the formal job search of UI beneficiaries taking as given program generosity (b, P). This is related to but different from the problem of targeting benefits among a given pool of potential beneficiaries (Alatas et al., 2012).

Assume the government can pay α per beneficiary to monitor and enforce the (first-best) formal reemployment levels that would prevail in the absence of UI, in each month until benefit exhaustion. The welfare gain from monitoring is to reduce the behavioral cost to zero and save on benefit payments: $b \times [B(b, P) - B(0, P)] \times MCPF$, where MCPF stands for the marginal cost of public funds or the average value of \$1 for taxpayers. If monitoring is efficient, the threat of monitoring will be sufficient to deter behavioral responses. Yet, to be effective in a context of imperfect information, monitoring costs must still be paid for workers who would not be formally reemployed anyway: $\alpha \times B(0, P) \times MCPF$.¹¹ An additional source of welfare loss may come from the utility loss experienced by behavioral beneficiaries, who are now denied UI benefits and must return to a formal job: $[B(b, P) - B(0, P)] \times \Delta g^b$, where $\Delta g^b \geq 0$ stands for the average social value of the loss experienced by behavioral beneficiaries. Comparing welfare gains and losses, we derive an upper bound for the maximum price a government should be willing to pay for this (hypothetical) perfect monitoring

¹⁰An entitlement effect could affect search efforts in the opposite direction (Hamermesh, 1979). We follow the optimal UI literature and also abstract from responses at the layoff margin. This assumes sufficient experience rating of benefits such that changes in UI have no effect on layoff rates. This assumption holds in the empirical analysis.

¹¹Monitoring could instead impose a cost on beneficiaries. In this case, the welfare loss amounts to: $\alpha \times B(0, P) \times g^m$, where g^m stands for the average social value of \$1 for mechanical beneficiaries. If $g^m \geq MCPF$, the maximum price a government should be willing to pay for this perfect monitoring technology will be smaller and the bound we derive looser. The average social value of \$1 for mechanical beneficiaries must be larger than the average social value of \$1 for taxpayers for the optimal size of a UI program to be positive (Baily, 1978; Chetty, 2006).

technology α_{max} :

$$\frac{\alpha_{max}}{b} = \tilde{\eta} \frac{MCPF - \Delta g^b/b}{MCPF} \le \tilde{\eta} \tag{1}$$

where $\tilde{\eta} = \frac{b[B(b,P)-B(0,P)]}{bB(0,P)}$ is the ratio of the behavioral to the mechanical cost of providing UI benefits. If behavioral responses amount to a small share of the cost of providing UI, $\tilde{\eta}$ is low and the government should not be willing to pay much to monitor formal job–search. The bound is loose if the average social value of the utility loss experienced by behavioral beneficiaries is large. Moreover, monitoring technologies are imperfect in practice, so the behavioral cost is never reduced to zero. Nevertheless, an estimate of $\tilde{\eta}$ provides useful information for a government that considers monitoring formal job–search of UI beneficiaries, as recently introduced in Brazil.¹²

C. Targeting formal job-search monitoring. The above reasoning assumes that the government cannot distinguish behavioral from mechanical beneficiaries ex ante. In practice, it may be possible to identify worker categories which are more likely to be responding to the incentives of the UI program and thus have larger values of $\tilde{\eta}$. Suppose that the government observes a noisy signal p^n of the probability that a worker from category n = 1...N is a behavioral beneficiary. Condition (1) may be satisfied for categories with large values of p^n and the government may then target monitoring towards these workers only. In the extreme case where a signal is fully informative for some worker category $(p^n = 1)$, condition (1) always holds as $\tilde{\eta}^n$ tends to infinity.

D. Connecting theory to the data. In the empirical analysis, we estimate the ratio of the behavioral to the mechanical cost $\tilde{\eta}$ for a temporary policy that extended UI benefits from five to seven months in Brazil in 1996. We then investigate to what extent the government could use information readily available ex ante (a signal) to identify worker categories with larger values $\tilde{\eta}$.

The policy that we study has the advantage of providing us with a credible empirical strategy to estimate the ratio of the behavioral to the mechanical cost of a UI extension, and a credible signal of the probability that workers from different categories are behavioral beneficiaries. Yet, it imperfectly applies to our framework. Extending an existing program may not be comparable to introducing UI in the first place. Such a concern is limited in Brazil. First, nearly all UI beneficiaries exhaust their regular UI benefits. The pool of displaced formal workers eligible for an extension is similar to the pool of UI takers. There is also no room for anticipation behaviors to matter. Second, we show that formal reemployment patterns after layoff for non–eligible displaced formal workers are similar to formal reemployment patterns after regular UI exhaustion for beneficiaries.

¹²An upper bound for the maximum price a government should be willing to pay for a perfect monitoring technology in the case of tax evasion can be derived in a similar way. Assume that there are *B* tax evaders and *M* tax compliers. The welfare gain from monitoring is: $x \times B \times MCPF$, where *x* is the average amount evaded per taxpayer *B*. The welfare loss is: $\alpha \times M \times MCPF + B \times \Delta g^B$. Therefore, we have: $\frac{\alpha_{max}}{x} = \frac{B}{M} \frac{MCPF - \Delta g^B/x}{MCPF} \leq \frac{B}{M}$. If the government can identify groups of taxpayers that are more likely to evade, the ratio $\frac{B}{M}$ increases.

Before turning to the empirical analysis, it is important to underline that $\tilde{\eta}$ is not independent of the generosity of the UI program (b, P). Therefore, condition (1) must be interpreted as specifying a local upper bound around a given program, in the same way as typical sufficient statistics formulas in the public finance literature specify local welfare tests.

3 Estimating incentives to monitor formal job-search

The natural experiment that we exploit here is discussed and analyzed at length in Gerard and Gonzaga (2013a). We thus present a succinct description and focus on results specific to this paper.

3.1 The 1996 temporary UI extension

Beneficiaries who exhausted their regular UI benefits between September and November 1996 in specific urban areas were eligible for two additional months of UI. The UI extension was politically motivated and the differential implementation was unrelated to local labor market conditions. A UI extension for the city of São Paulo was proposed to the President by Jose Serra, a politician from the same political party (PSDB) who was struggling in his run for mayor of São Paulo that year. Jose Serra justified his proposal by the rising unemployment in the city. In response, workers' representatives defended a UI extension in all cities, arguing that "unemployment is increasing everywhere, not only where the PSDB candidate is doing badly" (Folha de São Paulo, 08/22/1996). This proposition was rejected because a national extension would have cost more than the budget threshold to avoid a parliamentary process. As a compromise, the UI extension was implemented in the nine historical metropolitan areas of the country and the Federal District.¹³ Unemployment was mildly increasing in 1996; it was actually higher in 1997 when no extension took place.

The timeline of the experiment is summarized in Figure 2. The extension was first proposed on August 14 1996; it was adopted a week later, on August 21, to start on September 1. Formal employees displaced in April or May, and eligible for five months of UI (more than 24 months of tenure at layoff), learned in August that they would be eligible for two additional months of UI after exhaustion of their regular benefits. No extra UI payment would be paid after December 31. Workers laid off in June could only draw one additional month of UI. The timing guarantees that workers could not be strategically laid off. Beneficiaries also had little room to adjust their behavior in anticipation of the extension.

¹³Bélem, Belo Horizonte, Curitiba, Fortaleza, Porto Alegre, Recife, Rio de Janeiro, Salvador, and São Paulo. "the choice of the first nine metropolitan regions (in the 1970s) was more related to the objective of developing an urban system in the country according to the needs of a particular economic development strategy than to contemplating cities with actual characteristics of metropolitan regions. The proof of this claim was that Santos, Goiania and Campinas did not become metropolitan regions at that time, despite meeting some of the most important criteria to be considered a metropolitan area" Guimarães (2004), translation by the authors.

We adopt a difference-in-difference strategy to estimate behavioral responses to the UI extension. Our sample includes full-time private-sector formal employees 18–54 years old and laid off in April or May. The maximum benefit duration depends on accumulated tenure over the three years prior to layoff in Brazil. We cannot observe workers' formal employment history in the previous three years because our data start in 1995. Therefore, we restrict attention to workers with more than 24 months of tenure at layoff. For these workers, tenure at layoff is a sufficient statistic for their UI eligibility. They were eligible for five months of UI benefits. We use 1995 and 1997 as control years. We have nine treatment areas since we exclude São Paulo to reinforce the exogeneity of our cross-sectional variation. We use all the urban centers granted the status of metropolitan area since 1996 as control areas (20). In total, we have about 230,000 workers. There are a few differences between workers from control and treatment areas but these differences appear every year. Treatment and control areas are spread over the country and spanned a similar range of formal employment rates in these years. The distribution and composition of the sample are presented in Gerard and Gonzaga (2013*a*).

3.2 Graphical results

Our main result can be seen graphically. Figure 3 displays survival rates out of formal employment, and hazard rates of formal reemployment (their derivative), for UI takers in control and treatment areas in 1995, 1996, and 1997. Survival rates stay very high when displaced formal workers are drawing regular UI benefits. In fact, we show in the next subsection that nearly 100% of beneficiaries exhausted their five months of UI in these years.¹⁴ Survival rates start to decrease faster after regular benefit exhaustion. In fact, hazard rates of formal reemployment spike after benefit exhaustion. Patterns are very similar in control areas or control years. But in treatment areas in 1996, the spike in formal reemployment after regular benefit exhaustion shifts by exactly two months. As a result, an additional 15%of beneficiaries did not return to a formal job seven months after layoff.¹⁵ There are thus clear behavioral responses to the UI extension. However, the behavioral cost is relatively small compared to the mechanical cost. Survival rates out of formal employment stay high after regular UI exhaustion in control areas or control years. Most UI beneficiaries would draw the extended benefits absent any behavioral response. Incentives to monitor formal job search are therefore limited, unless monitoring could be targeted toward displaced formal workers who are more likely to be behavioral beneficiaries.

As discussed in Section 2, there are two main reasons why studying the extension of an existing program may not be comparable to studying the introduction of UI in the first place. First, the pool of displaced formal workers eligible for an extension may differ from the pool

¹⁴The formal reemployment of the few beneficiaries who return to a formal job before benefit exhaustion in our data, reported yearly, is thus not monitored by the UI agency, which uses data reported monthly (CAGED).

¹⁵Survival rates out of formal employment for UI non–takers present no differential trend, supporting our identifying assumption of a common trend absent the UI extension (Gerard and Gonzaga, 2013).

of UI takers. This is not a concern in our case because nearly 100% of beneficiaries exhaust their regular UI benefits. Second, formal reemployment patterns after layoff in the absence of UI may differ from formal reemployment patterns after UI exhaustion. For instance, one could imagine a model where displaced formal workers would rapidly return to formal sector jobs in the absence of UI but systematically switch to, and stay in, an informal job when UI benefits are offered. The behavioral cost of introducing a small UI program could then be much larger than the behavioral cost of extending an existing program. Figure 4 suggests that such a concern is also limited. In Brazil, displaced formal workers must have at least six months of tenure at layoff to be eligible for UI. The maximum benefit duration then depends on the accumulated tenure over the three years prior to layoff. Figure 4a displays the unconditional average benefit duration by tenure prior to layoff (in months) for a random sample of formal workers displaced between 2002 and 2009. Average benefit duration is very low for workers with low tenure levels who are, in theory, not eligible for UI benefits. Figure 4b displays survival rates out of formal employment for four tenure categories. Displaced formal workers with low tenure levels return more rapidly to a formal job in the first few months after layoff. However, their survival rates remain high. About 40% of displaced formal workers in each category are still out of the formal sector 12 months after layoff. Clearly, the behavioral cost of offering some UI to currently non-eligible workers would also be small compared to the corresponding mechanical cost. Figure 4b also reveals that formal reemployment rates in our empirical setting are in line with formal reemployment rates in more recent years across Brazil.¹⁶ Our results are thus likely to apply broadly.

3.3 Overall regression results

We first present overall difference–in–difference results for the impact of the temporary extension. Details and robustness checks are provided in Gerard and Gonzaga (2013*a*). We discuss how to estimate $\tilde{\eta}$, the ratio of the behavioral to the mechanical cost of the UI extension. In the next subsection, we then investigate to what extent we can use information readily available ex ante (a signal) to identify worker categories with larger values of $\tilde{\eta}$.

We estimate the following specification for individual i from area m in year t:

$$y_{i,m,t} = \alpha + \beta \ TreatArea_m + \gamma \ Year 1996_t + \delta \left[Year 1996_t \times TreatArea_m\right] + \epsilon_{i,m,t}$$
(2)

where $TreatArea_m$ and $Year1996_t$ are treatment area and treatment year fixed effects, respectively. δ is a difference-in-difference estimator for the impact of the UI extension on outcome y under a common-trend assumption. Estimates of $\hat{\delta}$ are reported in Table 1. $\epsilon_{i,m,t}$ is an error term clustered by area.¹⁷ We consider two outcomes using only UI data,

¹⁶The sample in Figure 4 is obviously not restricted to UI takers. Workers who are eligible for UI, but do not take it, return more rapidly to a formal job. Therefore the ratio of the behavioral to the mechanical cost would be even larger for UI takers. This also explains why average benefit duration does not reach five months of UI for displaced formal workers with more than 24 months of tenure at layoff.

¹⁷Significance levels are similar if we bootstrap t–statistics by resampling our 29 clusters.

regular UI benefit duration (first five months, column 2) and total UI benefit duration (up to seven months, column 3). We also verify that we do not find an effect on UI takeup, a decision preceding the announcement of the extension (column 1). The behavioral cost (in months) is the difference between the total benefit duration of treated workers and the benefit duration of the same workers had they not responded to the incentives of the UI extension (their mechanical cost, in months).¹⁸ To capture such a counterfactual, we construct a new variable (column 4) using workers' formal reemployment patterns to infer how many UI payments they would have collected had they all been eligible for seven months of UI. If they exhausted regular UI benefits, we assume that workers not formally reemployed within one month of exhaustion (resp. two months) would have collected one extra payment (resp. two extra payments). The mechanical cost is then the difference between the sum of

difference-in-difference estimator in column (4), $\hat{\delta}$, measures the behavioral cost.¹⁹ We find no effect on UI take-up or regular benefit duration (columns 1 and 2). Beneficiaries collected on average 4.98 months out of their five months of UI. Nearly 100% of beneficiaries exhausted their regular UI benefits. The extension increased benefit duration by 1.87 months in treatment areas in 1996 (column 3). We estimate that only 13% of that increase, .25 month, is due to behavioral responses (column 4). Indeed, had they been eligible for seven UI payments, beneficiaries in control years would have mechanically collected 1.58 (6.56-4.98) additional months. The ratio of the behavioral to the mechanical cost of the UI extension $\tilde{\eta}$ is thus: $\frac{.25}{1.58} = .158$. Therefore, the government should not be willing to pay more than 15.8% of the average benefit level (or 27% of a minimum wage), per beneficiary and month of extension, for a monitoring technology that perfectly enforces pre-existing formal reemployment levels. To provide some perspective, the ratio of the behavioral to the mechanical cost of a UI extension would be above 100% of the average benefit level in the US, based on estimates from (Katz and Meyer, 1990).

 $\hat{\alpha}, \hat{\beta}, \hat{\beta}, \hat{\beta}, \hat{\beta}, \hat{\gamma}$ from the specification in column (4) and the specification in column (2). The

The relatively small share of the cost due to behavioral responses may rationalize the absence of monitoring in Brazil until 2011, unless the government was able to target monitoring towards workers with relatively larger behavioral responses. We explore this possibility below.

 $\mathbb{1}$ (exhaust regular UI benefits) $\times \sum_{j=1}^{2} \mathbb{1}$ (month_{back} > month_{regUI} + j)

¹⁸The behavioral cost (resp. mechanical cost) in money terms is simply the behavioral cost (resp. mechanical cost) in months times the average benefit level.

¹⁹Define $month_{regUI}$, the month a beneficiary exhausts her regular benefits. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, the variable in column (4) is defined as:

In Gerard and Gonzaga (2013a), we test (successfully) whether we accurately predict in this way the increase in average benefit duration for workers eligible for the extended benefits.

3.4 Targeting results

We investigate whether a readily available signal, the spike in formal reemployment rates after regular benefit exhaustion, helps identify worker categories with larger ratios of the behavioral to the mechanical cost. We adopt a two-stage procedure. First, we predict the propensity to be formally reemployed in the two months after regular UI exhaustion in control areas and control years only, using workers' characteristics flexibly.

P (Formally reemployed in the 2 months after UI exhaustion = 1|X) = $F(X\beta)$ (3)

We use a logit model in Table 2, but we obtain similar results with a linear probability or probit model (available upon request). Second, we estimate the behavioral and mechanical costs separately by quartile of the predicted propensity.

The upper panel of Table 2 presents marginal effects at the mean from a simple version of equation (3) to highlight the relevant heterogeneity. Formal reemployment rates after benefit exhaustion are higher for males, and for younger, low-educated, and low-tenured workers. Interestingly, formal reemployment rates are also higher in labor markets with higher formal employment rates. Gerard and Gonzaga (2013*a*) finds that behavioral responses are relatively larger in relatively more formal labor markets in Brazil. As a result, the maximum price the government should be willing to pay for a perfect monitoring technology increases with local formal employment rates. Firm size and wages at layoff have nonlinear marginal effects (replacement rates decrease in wages). Finally, formal reemployment rates after benefit exhaustion are the largest (resp. smallest) for displaced formal workers from the footwear and construction industry (resp. financial and mining industry).

In our two-stage procedure, we use workers' characteristics more flexibly. We include fixed effects by year (3), area (29), education (9), sector of activity (50) and firm size (9). We also include fourth-order polynomials in tenure, age, and log real wages. The bottom panel of Table 2 displays estimates of the behavioral and mechanical costs (in months) by quartile of the predicted propensity from this augmented model. We obtain standard errors through bootstrapping of the two-stage procedure (resampling clusters) because quartiles are based on a constructed variable.

Our signal is informative: the behavioral cost increases by 100% from the first to the fourth quartile, from .16 to .34 month. Because the mechanical cost also decreases (by construction), the ratio of the behavioral to the mechanical cost $\tilde{\eta}$ increases by 145% from the first to the fourth quartile, from .09 to .23. Therefore, the government could use information at hand to target monitoring towards worker categories with larger values of $\tilde{\eta}$. Nevertheless, the maximum price it should be willing to pay for a (hypothetical) perfect monitoring technology remains quite low even for the top quartile, at 22.7% of the average benefit level per beneficiary and month of extension. Most of the heterogeneity is thus not easily captured by observable characteristics; $\tilde{\eta}^n$ tends to infinity if a signal perfectly predicts the probability that a worker of some category n is a behavioral beneficiary. Our result may once again rationalize the lasting absence of formal job-search monitoring in Brazil since it is unlikely that a government would use more sophisticated specifications.

4 Conclusion

The lack of strict monitoring policies for government programs is often considered to be an issue in developing countries. Yet, we know typically little about the magnitude of the behavioral responses that should be mitigated and about the cost of efficient monitoring technologies. In this paper, we derive a theoretical upper bound on the maximum price that a government should be willing to pay, per beneficiary, to perfectly monitor the formal job search of UI beneficiaries. We then estimate the bound empirically for the Brazilian case. We find that the scope of the monitoring issue is limited; the bound is relatively low, because most UI beneficiaries would collect UI benefits absent any behavioral response. Monitoring costs may thus exceed savings from deterring behavioral responses, potentially rationalizing the complete absence of formal job–search monitoring for UI beneficiaries in Brazil over 20 years.

Even if behavioral responses amounted to a larger share of program costs, it is unclear whether the government could find a cost–effective way to monitor the formal job search of UI beneficiaries. Stricter enforcement and verification of work search behaviors have limited impacts on UI payments in the US (Ashenfelter, Ashmore and Deschênes, 2005). In contrast, the threat of mandatory employment and training services does reduce UI claims (Black et al., 2003). In a natural follow-up to this paper, we plan to investigate the cost-effectiveness of formal job-search requirements for UI beneficiaries, which have been recently introduced in Brazil. In so doing, we will also tackle two limitations of the present paper. First, UI only induces behavioral responses at the *reemployment* margin (disincentives to return to a formal job) in our framework. In practice, UI may also induce behavioral responses at the *layoff* margin. We followed the literature and abstracted from this margin since the optimal policy is to introduce experience rating (or to finance UI through a layoff tax), eliminating such responses (Blanchard and Tirole, 2006). However, in most countries including Brazil, there is no experience rating and UI is financed through payroll taxes or general revenues. In such second-best worlds, formal job-search requirements may address a broader monitoring issue and mitigate behavioral responses at both margins. Second, while screening and monitoring issues are conceptually different, policies addressing one issue often have an impact on the other one as well. For instance, enforcing minimum formal job-search efforts (e.g., proof of interviews with formal firms) may reduce behavioral responses conditional on UI take-up, but may also affect take-up among mechanical beneficiaries who would not return rapidly to a formal job in the absence of UI.



Figure 1: Mechanical and behavioral costs of a UI program

The figures illustrate the average mechanical and behavioral costs (per beneficiary) of providing UI benefits b for up to P = 2 periods. The solution to the workers' dynamic search problem determines the survival rate out of formal employment S_t in each period t after layoff, and thus the average benefit duration $B(b, P) \equiv \sum_{t=1}^{P} S_t(b, P)$. The average cost of providing UI (per beneficiary) is simply: $b \times B(b, P)$. It can be divided into two components. First, some displaced formal workers would not return rapidly to a formal job in absence of UI. These workers (unemployed or informally employed) draw UI benefits mechanically, without changing their behavior. The mechanical cost amounts to: $b \times B(0, P)$. Second, there is a behavioral cost. Providing UI benefits reduces incentives to search for and return to a formal job, increasing average benefit duration, and thus the cost of the program. Behavioral responses induce an additional cost equal to: $b \times [B(b, P) - B(0, P)]$. The ratio of the behavioral to the mechanical cost $\tilde{\eta}$ constitutes an upper bound on the maximum price that a government should be willing to pay, per beneficiary, to eliminate behavioral responses. Intuitively, there is little incentive to introduce monitoring if most beneficiaries draw UI benefits mechanically, unless the government is able to target monitoring towards workers with relatively larger behavioral responses.





Private–sector formal employees laid off in April and May 1996 who exhausted their five months of regular UI benefits between September and November 1996 in treatment areas were eligible for two additional UI payments. The UI extension was proposed on August 14 and was adopted on August 21, to start on September 1, 33 days before the first round of local elections. No extra payments would be paid after December 31.

Chapter 2: Job–Search Monitoring in a Context of High Informality



(c) Survival rates out of formal employment, treatment areas



Figure 3: The 1996 temporary UI extension, impacts on formal reemployment (a) Survival rates out of formal employment, con- (b) Hazard rates of formal reemployment, control

(b) Hazard rates of formal reemployment, control areas



(d) Hazard rates of formal reemployment, treatment areas

Extended UI exhaustion



Full-time private-sector formal employees 18-54 years old from the main urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996 in treatment areas, these workers were eligible for seven months of UI. Left panels display the survival rates out of formal employment in each month after layoff for UI takers from control areas (panel a) and treatment areas (panel b) in 1996, 1996, and 1997. Right panels display the hazard rates of formal reemployment in each month after layoff (derivative of survival rates) for UI takers from control areas (panel b) and treatment areas (panel d) in 1996, 1996, and 1997. Survival rates stay very high when displaced formal workers are drawing regular UI benefits. Survival rates start to decrease faster after regular benefit exhaustion. In fact, hazard rates of formal reemployment spike after benefit exhaustion. Patterns are very similar in control areas or control years. But in treatment areas in 1996, the spike in formal reemployment after regular benefit exhaustion shifts by two months. As a result, an additional 15% of beneficiaries did not return to a formal job seven months after layoff. The spike appears to be shifted by more than two months because month 7 corresponds to December for workers laid off in May, and hirings are systematically low in December. The spike is shifted by exactly two months for workers laid off in April (available upon request). There are thus clear behavioral responses to the UI extension. However, the behavioral cost is relatively small compared to the mechanical cost. Survival rates out of formal employment stay high after regular UI exhaustion in control areas or control years. Most UI beneficiaries would draw the extended benefits absent any behavioral response. Incentives to monitor formal job search are therefore limited, unless monitoring could be targeted toward displaced formal workers who are more likely to be behavioral beneficiaries.

Figure 4: UI eligibility, average benefit duration, and formal reemployment rates

l employment .7 .8 .9

Survival rate out of formal .1 .2 .3 .4 .5 .6

0

Ó

Tenure<5

--- 12<Tenure<22

2 3 4 5 6

(a) Average benefit duration by tenure prior to lay-off

18 21

Tenure prior to layoff (months)

24 27

30 33 36

ß

Average benefit duration (months) 1 2 3 4

0

ό ż

9 12 15

6

(b) Survival rate out of formal employment by tenure prior to layoff

----- 6<Tenure<11

7

Months since involuntary layoff

24<Tenure>36

8 9 10

11 12



Table 1: Difference-in-difference results for the 1996 temporary UI extension

| TreatArea × Year 1996 | $(1) \\ UI \\ take-up \\0161 \\ (.0153)$ | (2) Regular UI duration .0002 (.0024) | (3) Extended UI duration 1.867*** (.0137) | (4) Extended UI duration vs. counterfactual .2497*** (.0204) |
|---|--|---|---|--|
| Mean (treatment area, control years) | .74 | 4.98 | 4.98 | 6.56 |
| Observations | 229878 | 171407 | 171407 | 171407 |

s.e. clustered by area (29 clusters). Significance levels: * 10%, ** 5%, ***1%. The sample includes full-time privatesector formal employees 18–54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996 in treatment areas, these workers were eligible for seven months of UI benefits. The table displays estimates of the difference–in–difference estimator for various outcomes (listed above each column). The regressions include treatment and year dummies. Outcomes in columns (2)–(4) are conditional on take–up. There is no treatment effect on UI take–up, a decision preceding the announcement of the extension (column 1). There is no treatment effect on regular UI duration (first five months, column 2). Beneficiaries were already drawing 4.98 months of their five months of UI without the extension. The UI extension increased benefit duration by 1.87 months on average (column 3). But only .25 month is due to behavioral responses (column 4). Beneficiaries in control years would have collected 1.58 months (6.56 – 4.98) absent any behavioral response, had they been eligible for the extension. The ratio of the behavioral to the mechanical cost of the UI extension $\tilde{\eta}$, is thus: $\frac{.25}{1.58} = .158$. Therefore, the government should not be willing to pay more than 15.8% of the average benefit level, per beneficiary and month of extension, for a monitoring technology that perfectly enforces pre–existing formal reemployment levels. To provide some perspective, the ratio of the behavioral to the mechanical cost of a UI extension would be above 100% of the average benefit level in the US, based on estimates from (Katz and Meyer, 1990).

| Signal: formally reemployed in the two months | | | | | | | | | |
|--|-------------------------------------|------------------------|-----------------------|--|--|--|--|--|--|
| afte | after regular UI exhaustion (logit) | | | | | | | | |
| Male | .0439*** | Log real wage | 0602*** | | | | | | |
| | (.0025) | | (.0071) | | | | | | |
| Age | 0013*** | Replacement rate | 1894*** | | | | | | |
| | (.0001) | - | (.022) | | | | | | |
| Years of education | 0015*** | Footwear | .1084*** | | | | | | |
| | (.0004) | | (.0078) | | | | | | |
| Firm size < 10 | 015*** | Construction | $.0536^{***}$ | | | | | | |
| employees | (.0028) | | (.0058) | | | | | | |
| Firm size ≥ 100 | 0234* ^{**} * | Financial institutions | 0494* ^{**} * | | | | | | |
| employees | (.0026) | | (.0089) | | | | | | |
| Tenure | 0002*** | Mining | 0552*** | | | | | | |
| | (0) | | (.0199) | | | | | | |
| Local formal | $.251^{***}$ | (Reference industry: T | extile) | | | | | | |
| employment rate | (.0193) | ν υ | , | | | | | | |
| Mechanical and behavioral costs by quartile of the | | | | | | | | | |

Table 2: Targeting monitoring towards workers with larger behavioral responses

Mechanical and behavioral costs by quartile of the predicted propensity to be formally reemployed in the two months after regular UI exhaustion

| | Mech. cost (month) | Beh. cost (month) | $\widetilde{\eta}$ |
|------------------|--------------------------|--------------------------|--------------------------|
| First quartile: | 1.731^{***} | .1591*** | .0919*** |
| Second quartile: | (.0157) 1.644^{***} | (.0168) $.2208^{***}$ | (.0104) $.1343^{***}$ |
| Third quartile: | (.021) 1.584^{***} | (.0224) $.2518^{***}$ | (.015) $.159^{***}$ |
| Fourth quartile: | (.0206) 1.479^{***} | $(.026)$ $.3365^{***}$ | (.0178) $.2275^{***}$ |
| | (.0349) | (.0425) | (.0329) |

Significance levels: * 10%, ** 5%, ***1%. The sample includes full-time private-sector formal employees 18-54 years old from the largest urban areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for five months of UI benefits. In 1996 in treatment areas, these workers were eligible for seven months of UI benefits. In this table, we investigate whether a readily available signal, the spike in formal reemployment rates after regular benefit exhaustion, helps identify worker categories with larger ratios of the behavioral to the mechanical cost. We adopt a two-stage procedure. First, we predict the propensity to be formally reemployed in the two months after regular UI exhaustion in control areas and control years only, using workers' characteristics flexibly (logistic regression). Second, we estimate the behavioral and mechanical costs separately by quartile of the predicted propensity. The top panel of Table 2 presents marginal effects at the mean (robust s.e. in parentheses) from a simple version of our first-stage regression to highlight the relevant heterogeneity. In our two-stage procedure, we use workers' characteristics more flexibly. We include fixed effects by year (3), area (29), education (9), sector of activity (50) and firm size (9). We also include fourth-order polynomials in tenure, age, and log real wages. The bottom panel of Table 2 displays estimates of the behavioral and mechanical costs (in months) by quartile of the predicted propensity from this augmented model. We obtain standard errors through bootstrapping of the two-stage procedure (resampling clusters) because quartiles are based on a constructed variable. Our signal is informative: the behavioral cost increases by 100% from the first to the fourth quartile, from .16 to .34 month. Because the mechanical cost also decreases (by construction), the ratio of the behavioral to the mechanical cost $\tilde{\eta}$ increases by 145% from the first to the fourth quartile, from .09 to .23. Therefore, the government could use information at hand to target monitoring towards worker categories with larger values of $\tilde{\eta}$. Nevertheless, the maximum price it should be willing to pay for a (hypothetical) perfect monitoring technology remains quite low even for the top quartile, at 22.7% of the average benefit level per beneficiary and month of extension. Most of the heterogeneity is thus not easily captured by observable characteristics; $\tilde{\eta}^n$ tends to infinity if a signal perfectly predicts the probability that a worker of some category n is a behavioral beneficiary. Our result may thus rationalize the lasting absence of formal job-search monitoring in Brazil since it is unlikely that a government would use more sophisticated specifications.

Chapter 3

What Changes Energy Consumption, and For How Long? Evidence from the 2001 Brazilian Electricity Crisis

I would like to thank Severin Borenstein, Lucas Davis, Marina de Mello, Claudio Ferraz, Meredith Fowlie, Ryan Kellogg, Jamie McCasland, Edward Miguel, Emmanuel Saez, Edson Severnini, Catherine Wolfram, and seminar participants at the BERC Energy Symposium and at UC Berkeley for their comments and suggestions. Stéphanie Dinoá, Tiago Lazier, Gustavo Macedo, and John Pease provided outstanding research assistance. I would also like to thank Luiz Cesar, Rodrigo Ferreira, Gustavo Gonzaga, Angela Gomez, Jerson Kelman, Gilberto Rocha, Hálisson Rodrigues, Emerson Salvador, Reinaldo Souza, and the Department of Economics at the Pontifícia Universidade Católica do Rio de Janeiro for sharing important data. I benefited from the support of an Excellence Scholarship of Wallonie–Bruxelles International. All errors are my own.

Energy conservation is on the policy agenda around the globe. Residential electricity consumption, in particular, has attracted a lot of attention.¹ Because of the low price elasticity of residential electricity demand, achieving ambitious conservation targets through economic incentives (e.g., prices) is often considered politically infeasible. Accordingly, there is a lot of interest in alternative policies, such as the use of social incentives (e.g., conservation appeals, social comparison).

In fact, there is little evidence from policies aimed at achieving ambitious conservation targets. It is thus unclear whether social incentives could prompt large reductions in energy consumption.² Moreover, smaller incentives are less likely to trigger lumpy adjustments inherent in the use of energy (e.g., appliances, habits).³ The possibility of triggering such adjustments may reduce the incentives necessary to achieve ambitious conservation targets, and may induce persistent effects once incentives are removed. There is also little evidence on the impact of conservation policies in the developing world. Yet, most of the growth in energy demand is forecast to come from developing countries. Poorer households, who own fewer appliances and consume less energy, are more likely to reduce consumption through behavioral changes, which may be less persistent.⁴

The present paper addresses these limitations by studying the short– and long–term impacts on residential electricity consumption of the most ambitious electricity conservation program to date. This was an innovative program of economic and social incentives implemented by the Brazilian government from June 2001 to February 2002 in areas facing supply shortages of over 20%. Residential customers were assigned individual quotas, typically 20% below baseline consumption. Larger consumers were charged fines for exceeding their quotas; smaller consumers were offered bonuses for consuming below their quotas. The government also carried out a large conservation appeal campaign in cooperation with utilities and media outlets.⁵ The crisis was caused by exceptionally low rainfall, and insufficient capacity

¹Improving the energy efficiency of residential electricity demand is often viewed as the most costeffective policy to abate greenhouse gas emissions around the world (McKinsey, 2009). Utilities have to meet specific energy saving targets through customer conservation programs in at least 24 states in the US.

²The renowned US Opower program, which features personalized feedback and social comparison, reduces electricity use by at most 2% (Ayres, Raseman and Shih, 2009; Allcott, 2011; Allcott and Rogers, 2012).

³Allcott and Greenstone (2012) argue that large fixed costs hinder households' adoption of energy conservation strategies and that there is thus no evidence for the "energy efficiency gap" (McKinsey, 2009).

⁴Average monthly residential consumption in Brazil in 2000 was below 200 kWh, compared to 940 kWh in the US in 2011 (http://www.eia.gov/tools/faqs). With vulnerable infrastructure, and the difficulty of accurately planning capacity investments, a rapidly rising demand also brings the risk of dramatic supply shortages in developing countries (Wolfram, Shelef and Gertler, 2012). In more advanced countries, imbalances between supply and demand may arise from catastrophic events, such as the Japanese earthquakes, or demand shocks, such as hot summer days (Meier, 2005).

⁵I focus on residential customers in this paper. Other customer categories were also subject to conservation measures. Because economic incentives were nonlinear, they cannot be simply translated as a given increase in linear tariffs. I use the term "social incentives" to refer to policies appealing to customers' social preferences. Reducing electricity use in response to conservation appeals amounts to contributing anonymously to a public good. Indeed, there was no real way to observe conservation efforts among neighbors during the crisis and the chances for a given household to be "pivotal" in averting generalized blackouts was

investments, in a country relying heavily on hydro–electric generation. Hydro–reservoirs' water levels were at their lowest in 40 years after the 2000–2001 summer in the two affected electric subsystems (North–East and South–East/Midwest; Figure 1a). In contrast, generous rain dissipated any risk of shortages for utilities in the third subsystem (South), which were exempted from the conservation program. Importantly, this differential treatment was entirely due to weather and to limited transmission capacity across subsystems.

I begin by presenting a simple model of electricity consumption and lumpy adjustments decisions in the presence of economic and social incentives to guide the empirical analysis. I then estimate the short- and long-term impacts by comparing utilities subject or not to the conservation program. I use data on average residential consumption per customer from 20 years of monthly administrative reports for every utility in Brazil. I estimate the overall impacts through a difference-in-difference strategy and utility-specific impacts through synthetic control methods. To study the distribution of customers' responses, I exploit the sharp times-series variation and monthly billing data for three million customers of one affected utility (LIGHT). Researchers are rarely able to identify the nature of customers' responses and the presence of sizable lumpy adjustments. In this paper, I infer the presence and magnitude of lumpy adjustments from the persistent impacts after temporary incentives were removed. Different data sources then provide evidence on adjustment mechanisms. Finally, I structurally estimate the model by combining the billing data, individual variation in economic incentives, and an estimate of the price elasticity of residential electricity demand obtained out-of-crisis. This allows me to quantify the role of social incentives and the incentives necessary to achieve observed consumption levels without lumpy adjustments.

This paper has three main findings. First, a combination of economic and social incentives induced substantial electricity conservation. Figure 1b displays seasonally adjusted trends in average residential consumption per customer by electric subsystem. In June 2001, consumption decreased dramatically in the two affected subsystems (North–East, South– East/Midwest). Consumption stayed low until February 2002; no blackouts were ever necessary.⁶ I attribute a .25 log point average reduction during the crisis to the conservation program (difference–in–difference). This is a very large effect. LIGHT customers would have saved less by switching off all refrigerators or light bulbs (engineering estimates). The result holds across seasons and controlling for changes in base electricity tariffs. The impact is above .2 log point for every affected utility (synthetic control). Moreover, average effects came from large responses by most customers (billing data). Energy theft, prevalent in developing countries, is unlikely to have played any major role.

Second, the conservation program induced sizable and persistent lumpy adjustments. It reduced average residential consumption per customer by .12 log point in the long run (difference–in–difference, using comparable utilities). Consumption only partially rebounded

essentially nil. I thus define social preferences broadly to encompass phenomena such as altruism, patriotism, social comparison, or "moral suasion" Reiss and White (2008).

⁶Consumption also decreased, to a lesser extent, in the third subsystem (South) because of national policies, such as tax incentives to adopt compact fluorescent light bulbs (CFLs), and possible spillovers from conservation appeals.

once incentives of the conservation program were removed (Figure 1b). Consumption levels, higher in the South–East/Midwest than in the South prior to the crisis, were similar after the crisis and until 2011. The result holds when I control for electricity tariffs and other relevant variables (e.g., median household income) matched to the concession area of each utility. The impact is persistent for every affected utility (synthetic control). Average effects came again from widespread responses across the distribution of consumption levels (billing data). The persistence appears to be mostly due to behavioral adjustments. Sales of domestic appliances did not increase during the crisis.⁷ In contrast, in surveys conducted in 2005, households reported systematic and persistent changes in the way they used domestic appliances and consumed electricity. Popular conservation strategies during the crisis, unplugging freezers and avoiding standby power use, were still more prevalent at the time of the surveys among households that had been subject to the conservation program.

Last, social incentives and lumpy adjustments played a major role in achieving the substantial electricity conservation. I estimate a price elasticity of residential electricity demand of about -.2 out-of-crisis by exploiting variation in electricity tariffs over time and across utilities. The persistent average impacts (lumpy adjustments) thus correspond to a .6 log point permanent increase in tariffs. During the crisis, many customers faced price increases of less than 50%. Customers with no economic incentives to reduce consumption below quotas reduced consumption by 20% below their quotas. Moreover, a 20% quasi-exogenous increase in individual quotas for specific customers, who faced only fines for exceeding their guotas, increased electricity use by only 3%.⁸ I structurally estimate the model parameters for these customers by minimizing the distance between moments predicted by the model and empirical moments (Gouriéroux and Monfort, 1996). The four parameters are the priceequivalent of social incentives, the degree of consumption uncertainty, and two parameters capturing the propensity to consume electricity (appliance stock and habits) before and after the crisis. The four moments are the median consumption levels before, during, and after the crisis, and the change in consumption following an exogenous increase in quotas. The model takes as inputs electricity tariffs, the estimated price elasticity, and the schedule of economic incentives for these customers.⁹ Social incentives amounted to a 1.2 log point increase in electricity tariffs. Median electricity consumption for these customers would have been 23.5%higher during the crisis in absence of social incentives. Importantly, incentives would have

⁷The adoption of CFLs did increase but not differentially in affected areas.

⁸The quotas of customers who moved into their metered housing units after the baseline period (May–July 2000) were based on their first three billing months. Larger consumers use air conditioning in the summer in Rio de Janeiro. Consequently, larger consumers who moved in the summer of 2000–2001 were allocated more generous quotas.

⁹Because of nonlinearities, it is important to capture customers' uncertainty about their realized consumption during the crisis (Borenstein, 2009). I underestimate the role of social incentives and lumpy adjustments if customers confuse *marginal* with *average* prices (Ito, 2012a). Intuitively, the persistent impacts identify the change in the propensity to consume electricity. Given a price elasticity, the difference between consumption levels during and after the crisis (after lumpy adjustments were made) identifies the price-equivalent of the "overall" incentives. Customers' responses to the increase in quotas separate social incentives and consumption uncertainty.

had to be .58 log point higher during the crisis to achieve the observed consumption levels absent any lumpy adjustment.

The findings of this paper contribute to several areas of the literature. Sizable reductions in residential electricity demand in developed countries are only achieved through several fold price increases, for heavy users of air conditioning or electric heating, and with advanced control technologies.¹⁰ I find a very large effect in absence of these features in a developing country context. Yet, I estimate a price elasticity of residential electricity demand (-.2) that is comparable to recent estimates from the US.¹¹ My results are due to two factors. First, social incentives appear to have played a major role. There is a growing literature on the impact of social incentives on economic decisions. Social incentives have been shown to have a positive, but limited, effect on residential electricity demand. My results imply that appeals to social preferences may be particularly powerful at stimulating contributions to essentially public goods in times of crisis.¹² Second, incentives of the conservation program were large enough to trigger lumpy adjustments, permanently reducing the incentives necessary to achieve observed consumption levels by about .6 log point. This finding contributes to the literature on the impact of policies under adjustments costs, on the long-run impacts of temporary policies, and on the cost of environmental policies.¹³ My results indicate that persistent effects may also arise from behavioral adjustments, thus relating to the literature on habit formation (Becker and Murphy, 1988). Finally, the Brazilian experience shows that direct rationing policies may not be necessary to face supply shortages, avoiding harmful

¹¹Existing estimates in developing countries rely mostly on time series (e.g. for Brazil, Schmidt and Marcos, 2004; Pimenta, Notini and Maciel, 2009). Ito (2012a) obtains a similar figure in the US. On the one hand, poorer households may be more responsive. On the other hand, they may have fewer margins of response because they own fewer domestic appliances. In a Latin American context, Bastos et al. (2011) find a price elasticity of -.15 for natural gas.

¹²Among other work on the impact of social incentives, DellaVigna, List and Malmendier (2012) also structurally estimate the price equivalent of social incentives (for charitable giving). Meier (2005) reviews qualitative evidence of non-pecuniary policies from several episodes of supply shortages. Reiss and White (2008) argue that public appeals reduced electricity demand during the California crisis. In a different context, appeals to social preferences (patriotism) may explain the high civilian labor supply in the US during World War II (Mulligan, 1998). Voluntary contributions to public goods are common (Andreoni, 2006). In lab experiments, this phenomenon is amplified when contributions aim at avoiding the loss of an existing public good, particularly if the loss is large (Iturbe-Ormaetxe et al., 2011).

¹³Acemoglu et al. (2012) and Aghion et al. (2012) argue that temporary policies promoting greener technologies may have persistent effects on the supply side through directed technical change. Distortions in the US natural gas market had persistent consequences because of lumpy investments in domestic appliances (Davis and Kilian, 2011). Larger changes in tax rates generate proportionally larger responses because of adjustments costs (Chetty et al., 2011). Temporary incentives to attend a gym still had an effect a few weeks post–intervention Charness and Gneezy (2009). An information and social comparison intervention still had a small effect (1.5%) on residential electricity consumption in the US a few months after the intervention was discontinued (Allcott and Rogers, 2012).

¹⁰Faruqui and Sergici (2010) review 15 experiments across several countries. A large rebate program in California had an impact only on heavy users of air conditioning (Ito, 2012b). Electricity demand fell by 25% in Alaska during a three–month supply crisis following a 500% price increase (Leighty and Meier, 2011).

allocative inefficiencies.¹⁴

The paper proceeds as follows. Section 1 describes the background and the data. Section 2 presents the model that guides the empirical analysis. Section 3 estimates the impacts of the conservation program. Section 4 provides evidence for the underlying mechanisms. Section 5 investigates the relative roles of social incentives and lumpy adjustments. Section 6 concludes.

1 Background and data

1.1 Electricity distribution in Brazil

The National Interconnected System, the major electricity system in Brazil, is divided into four subsystems that had limited transmission capacity at the time of the electricity crisis: North (6.5% of total load in 2000), North–East (14.5%), South–East/Midwest (62%), and South (17%). In 2000, 81% of the production capacity relied on hydropower.¹⁵ More than 60 local monopolies (utilities) distribute electricity to end consumers. Housing units are typically metered and billed separately every month. Readings and bills are staggered during the month. Electricity theft (illegal connections) is a serious concern in Brazil. It amounts to 15% of the total load for some utilities.

Electricity prices are regulated by a federal agency (Agência Nacional de Energia Elétrica, ANEEL) and are relatively high. The main residential tariff is a flat unit price per kilowatt hour (kWh). An alternative tariff for low-income and small consumers offers percentage discounts on the main tariff depending on the quantity consumed. Price changes typically modify the main tariff and therefore imply a proportional change in every marginal price. In contrast to, e.g., California, the regulatory framework is a price-cap mechanism in Brazil. Yearly price *adjustments* only factor in changes in non-manageable costs (e.g., transmission or energy). Demand risk falls entirely on utilities. Every four to five years, prices are then *revised* to guarantee utilities' economic viability.¹⁶

1.2 The 2001–2002 electricity crisis

There is little work on the impacts of the Brazilian electricity crisis and its conservation program on electricity consumption. Bardelin (2004) and Maurer, Pereira and Rosenblatt

¹⁴Faced with shortages, most governments ration energy (Maurer, Pereira and Rosenblatt, 2005). Allocative inefficiencies from past rationing in the US natural gas market amounted to \$3.6 billion a year (Davis and Kilian, 2011).

¹⁵This share is now around 72% (http://www.ons.org.br). Appendix Figure B.1 presents a map of Brazil.

¹⁶See ANEEL (2005). The price–cap mechanism is aimed at encouraging utilities to address electricity theft. Price *revisions* and *adjustments* occur at different times for different utilities. In June 2001, the main tariff was R\$.208/kWh (US\$.08) in Rio de Janeiro. Marginal prices in the alternative tariff were R\$.073 (up to 30 kWh), R\$.125 (up to 100 kWh), R\$.188 (up to 140 kWh), and R\$.208 (above 140 kWh). Minimum consumption levels are also charged, and local taxes increase what customers eventually pay.

(2005) provide some descriptive evidence with aggregate data. Pimenta, Notini and Maciel (2009) use time-series techniques. In concurrent but independent work, Costa (2012) studies some of the questions addressed in this paper with more limited data.¹⁷

History of the crisis

The major cause of the crisis was a particularly unfavorable rainfall pattern combined with insufficient capacity investments. Figure 1a displays the evolution of hydro–reservoirs' water levels in the main subsystems. Levels were low in every subsystem in 2000, but generous rain dissipated the risk of shortages in the South. In contrast, because of exceptionally low rainfall in the 2000–2001 summer, water levels were at their lowest in 40 years in the North-East and South–East/Midwest by March 2001 (for the season). This differential impact was entirely due to weather and the limited transmission capacity across subsystems.¹⁸ By late April, it was clear that consumption had to decrease to avoid generalized blackouts.¹⁹ Details were unclear but a conservation program based on economic incentives was announced, to start on June 1 (*Globo*, April 23, 2001). The Brazilian Association of Distribution Utilities (ABRADEE) supported instead the use of blackouts because "financial penalties were unlikely to succeed, in part due to the lack of demand elasticity" and the expected length of the crisis (Veja, May 3, 2001; Maurer, Pereira and Rosenblatt, 2005). The conservation program came into force on June 4, 2001. It involved both economic incentives and a massive information and conservation appeal (social incentives) campaign in collaboration with utilities and media outlets. The objective was to reduce electricity use by 20% in the North-East and South–East/Midwest subsystems. Measures were expected to apply until February 2002 (Veja, July 19, 2001). Rolling blackouts were part of a never-implemented plan B. Mation and Ferraz (2011) provide ample evidence that the crisis, the conservation program, and its

¹⁸The crisis would have been avoided, however, had capacity been expanded adequately. Realized demand was never above projected demand between 1998 and 2001, but growth in demand outpaced growth in generation capacity prior to 2001. Several infrastructure projects were delayed or canceled, for instance. See Kelman (2001), Maurer, Pereira and Rosenblatt (2005), and Mation and Ferraz (2011) for more discussion on the cause of the crisis and the exogenous role of rainfall in the differential treatment across subsystems.

¹⁹This was despite a first set of national policies in early April. Among these measures were the giveaway of efficient light bulbs in low–income neighborhoods, a 15% reduction in electricity consumption in federal public buildings, the import of energy from Argentina, and the construction of new thermoelectric facilities (*Veja*, April 5, 2001).

¹⁷Mation and Ferraz (2011) use a similar difference–in–difference strategy to investigate impacts on firms' productivity. Costa (2012) only studies aggregate effects of the conservation program. My work innovates in both content and data. I use monthly billing data for three million customers to investigate distributional effects, to address the question of energy theft, and to study the relative roles of social incentives and lumpy adjustments. Moreover, I provide more robust estimates of aggregate effects by constructing a unique dataset of monthly residential electricity tariffs for every utility from 1996 to 2011 and by matching census data (2000 and 2010), population estimates (IBGE), and formal employment records (RAIS) for each municipality to the concession area of every utility. Finally, I provide additional evidence on persistence mechanisms. The first versions of our respective work are available at http://papers.srn.com/sol3/papers.cfm?abstract_id=2028684 and http://papers.srn.com/sol3/papers.cfm?abstract_id=2028684

differential implementation across subsystems were mostly unanticipated by residential customers.²⁰ On February 19, the president announced the end of the crisis and its conservation measures. Bonuses were maintained for the February–March billing cycle. The government hoped that electricity conservation might persist because "the population had been educated and its awareness had been raised during the threat of blackout" (*Veja*, February 19, 2002). According to a specialized periodical, "people were giving signals that they learned how to avoid wasting electricity" (*Energia Elétrica*, March 15, 2001).

Economic incentives of the conservation program

The rules for residential customers were frequently repeated in the media and on electricity bills.

A. Quotas. Typical residential customers were assigned a quota equal to 80% of a baseline, their average consumption from May to July 2000. Quotas for smaller consumers were set at 100% of baseline or 100 kWh, whichever was smaller. Customers were informed of their quotas by mail prior to their first affected billing cycle.²¹ Finally, quotas were revised upward in December 2001 and January 2002. The situation was improving and consumption is higher in the summer.

B. Fines and bonuses. Customers exceeding their quota were charged a per–unit fine for every kWh consumed above 200 kWh (50% of the marginal price up to 500 kWh and then 200%). Bonuses targeted mostly smaller, and poorer, consumers. A customer consuming less than her quota and less than 100 kWh was offered a per–unit bonus for every kWh reduced below her quota (200% of the marginal price). Fines and bonuses were directly passed on in monthly bills. Bills could not be negative, limiting the payment of bonuses. Figure 2 illustrates how these incentives modified the cost of electricity. In September 2001, an additional per–unit bonus was offered for individuals with quotas below 225 kWh (100% of the marginal price). Fines were suspended in February but bonuses were still paid for the February–March billing cycle.

C. Threats of disconnections. Customers could, in theory, be subject to power cuts of three to six days for exceeding their quotas. In practice, utilities did not have enough staff to implement this rule. Importantly, power cuts were prohibited by a municipal law in Rio de Janeiro (Lei Municipal 3266/2001). Customers in my billing data could not have their power cut.

 $^{^{20}}$ For instance, President Cardoso's approval rates dropped differentially in areas subject to the conservation program after its announcement. Measures were expected to end when reservoirs reached 50% of their maximum capacity (*Veja*, February 16, 2002). The conservation program was extended to three utilities in the North subsystem from August 2001 to December 2001. These utilities' many customers served by isolated electricity systems were not subject to any measure. Because the data do not differentiate utilities' residential consumption from "isolated" and "connected" customers, I do not consider utilities from the North.

 $^{^{21}\}mathrm{A}$ letter is reproduced in the Web Appendix. Figure B.3 displays the mapping between baseline and quotas.

Information and conservation appeals (social incentives)

The government also carried out a massive information and conservation appeal (social incentives) campaign in collaboration with utilities and media outlets. Daily reports on TV compared achievements to government targets. Energy conservation advice and stories of "exemplary" behaviors were shared repeatedly in the media to promote awareness and encourage participation. Media reports and messages on electricity bills included appeals to social preferences and patriotism. The government made sure to impose a more stringent conservation target for public buildings to set the example. Reviewing episodes of shortages around the world, Meier (2005) refers to the *strong national commitment to conservation* as a main component of the Brazilian conservation program.

Other factors

No extraordinary tariff *adjustment* took place in June 2001 or February 2002. Tariffs were increased by a mere 2.9% on December 21, 2001, for utilities subject to the conservation program.²² Other tariff changes followed the usual regulatory framework. Additional policies may have affected consumption levels in the short and long run. Taxes on efficient light bulbs were reduced, and taxes on electric showers, water heaters, and incandescent light bulbs were temporarily increased (Decreto 3827, May 21, 2001). Efficiency standards for domestic appliances were adopted (Lei 10295, October 17, 2001). These policies applied nationally and are unable to explain the differential impact across subsystems. Moreover, they relate to household investments and are thus unable to explain why crisis consumption fell so much below post–crisis levels.

1.3 Data

In this paper, I mostly rely on three sets of data that are further detailed in the Web Appendix.

A. ANEEL administrative data. I obtained monthly administrative data from utility reports on total electricity consumption, total revenues, and total number of customers by category (e.g., residential) from 1991 to 2011. I construct a unique dataset of monthly electricity tariffs by category and utility from copies of every tariff regulation published by

²²Camara de Gestão da Crise de Energia, Resolução 91. Customers may have also updated their beliefs about the risk of future shortages. In the Web Appendix, Figure B.2 shows that the rainfall pattern in 2000–2001 was a unique outlier. Even in the South, reservoir levels were very low in 2000. The situation of the reservoirs was stable in the South–East/Midwest but more variable in the South after the crisis. The risk of new shortages was thus not smaller in the South. Accordingly, an *insurance fund* established to avoid subsequent crises was financed through a nationwide increase in electricity tariffs (R\$.49 per 100 kWh; Camara de Gestão da Crise de Energia, Resolução 115). Moreover, the country had already experienced weather–induced electricity shortages in the South (January–March 1986), the North–East (March 1987–January 1988), and the North (late 1990s; Maurer, Pereira and Rosenblatt, 2005). Generation and transmission capacity have increased nationally, reducing the risk of localized shortages.

the regulator (ANEEL) from 1996 to 2011. As a result, I have a balanced panel of monthly average consumption per customer, average price, and actual tariff by category for 44 utilities in the North–East, the South–East/Midwest, and the South (47 and 48 utilities from 2000 and 2002, respectively, due to modified concession areas). I also match census data (2000 and 2010), yearly population estimates (IBGE), and yearly formal employment records (RAIS) for each municipality to the concession area of each utility.

B. LIGHT billing data. I obtained individual monthly billing data for the universe of low voltage customers of LIGHT, the utility serving Rio de Janeiro and 31 surrounding municipalities (South-East) from January 2000 to December 2005 (three million residential customers in 2000). The data detail every bill component and include metering and billing dates, meter location, and the quantity consumed. Customers are uniquely identified over time unless they move.

C. PROCEL surveys and supplementary data. I obtained micro-data from household surveys conducted in 2005 by PROCEL, the National Electrical Energy Conservation Program. The surveys capture appliance ownership and consumption habits, and retrospective information on conservation behaviors before and during the electricity crisis. The sample includes 4975 residential customers from 18 utilities in the three main subsystems (PRO-CEL, 2007*a*). I also use time-series data on sales of appliances from manufacturers' reports, on imports of compact fluorescent light bulbs (CFLs) from PROCEL, and on sales of electric showers from a leading manufacturer. Finally, in the Web Appendix, I use the Household Expenditure Surveys (POF, Pesquisa de Orçamentos Familiares; 1996–1997, 2002–2003, and 2008–2009) and the yearly National Household Surveys (PNAD, Pesquisa Nacional por Amostra de Domicílios) to confirm the findings of Section 4. These surveys do not identify municipalities and cannot be matched to the concession area of each utility.

2 Customers' responses to incentives: theoretical framework

This section presents a simple model of electricity consumption and lumpy adjustments decisions in the presence of economic and social incentives. I discuss how the possibility of triggering lumpy adjustments reduces the incentives necessary to achieve ambitious conservation targets. I use the model and a price elasticity estimated out-of-crisis to predict consumption responses to the economic incentives of the conservation program. In section 5, I structurally estimate the model to quantify the role of social incentives and lumpy adjustments.

2.1 Framework

A customer chooses billing-cycle electricity consumption q subject to economic incentives p (e.g., prices) and social incentives s (e.g., conservation appeals) to maximize:²³

$$U(q, p, s) = G(V(q) + W - pq - sq)$$

$$\tag{1}$$

with wealth W, G'(.) > 0, V'(.) > 0, and V''(.) < 0. In contrast to economic incentives, social incentives' relative magnitudes can hardly be measured without observing agents' responses. I thus assume that $\forall s, \exists! \tilde{p}$ such that $U(q, p, s) = U(q, p + \tilde{p}, 0)$, and define s > s' if and only if $\tilde{p} > \tilde{p}'$.

Customers can make both (continuous) reversible changes to their electricity consumption and (discrete) lumpy adjustments when incentives change. To capture these two types of responses in a simple way, I assume that V(.) takes the following form:

$$V(q) = a_j \frac{1}{1 + 1/\eta} q^{1 + 1/\eta}$$
(2)

where $\eta \in (-1, 0]$ and a_j is a "reduced-form" parameter aimed at capturing customers' propensity to consume electricity through lumpy physical investments (e.g., appliances) and behavioral adjustments (e.g., consumption habits).²⁴ Holding a_j constant, first-order conditions give:

$$\ln q = \eta \ln \left(p + s \right) - \eta \ln a_j \tag{3}$$

For a given set of incentives, q is lower for a smaller propensity to consume a_j . $\eta = \frac{d \ln q}{d \ln (p+s)} |_{a_j}$ captures the first type of response to incentives. The observed elasticity of residential electricity consumption with respect to incentives $\frac{d \ln q}{d \ln (p+s)} = \eta \left(1 - \frac{d \ln a_j}{d \ln (p+s)}\right)$ will be larger if customers also make lumpy adjustments. However, such adjustments entail discrete costs. Assume that customers can switch to a smaller propensity to consume electricity, $a_1 < a_0$, at a utility cost c. Customers will make the adjustment if and only if:

$$\sum_{t=0}^{T} \frac{U^*\left(a^1, p_t, s_t\right) - U^*\left(a^0, p_t, s_t\right)}{\left(1 + r_t\right)^t} > c \tag{4}$$

$$V(q) = \begin{cases} a_j \ [\ln(q) - M(\eta)], & \eta = -1\\ a_j \left[\frac{1}{1 + 1/\eta} q^{1 + 1/\eta} - M(\eta)\right], & \eta < -1 \end{cases}$$

where $M(\eta)$ guarantees that V(q) is negative on the relevant support of $q \in [0, Q]$.

 $^{^{23}}$ Income effects are generally assumed away (Borenstein, 2009; Ito, 2012*a*). I model social incentives as a *tax* instead of a *subsidy*. Consumption choices would be identical in this model. I discuss non–linear incentives below.

 $^{^{24}\}eta$ captures a lower-bound on the price elasticity of residential electricity demand, which is typically inelastic. In case $\eta \leq -1$, the discussion follows through if we assume:
for discount rate r_t , horizon T, and indirect utility U^* . Allcott and Greenstone (2012) argue that c has often large unobserved (non-monetary) components, even for physical investments. Lumpy adjustments are therefore more likely to take place following relatively large changes in incentives.

In theory, the possibility of triggering lumpy adjustments thus reduces the incentives necessary to achieve large electricity conservation targets, and the utility cost of ambitious conservation programs. This is illustrated graphically in Appendix Figure A.1. Without lumpy adjustments, incentives must increase in the conservation target following $d\ln(p+s) = \frac{d\ln(q)}{\eta}$. However, when incentives reach some threshold, customers make lumpy adjustments and consumption drops discontinuously $(a_0 \rightarrow a_1)$. To further reduce consumption, incentives must increase again but they are lower than the necessary incentives absent any lumpy adjustments.

In practice, it is unclear whether the possibility of triggering lumpy adjustments substantially reduces the incentives necessary to achieve ambitious conservation targets. First, there is little evidence that the phenomenon is empirically relevant. Researchers often exploit limited variation in incentives and are rarely able to identify the nature of customers' responses and the presence of sizable lumpy adjustments. The phenomenon may also be less relevant in a developing country context where major domestic appliances have low penetration rates. In this paper, I infer the presence and magnitude of lumpy adjustments from the persistent effect after incentives of the conservation program were suspended. I then provide suggestive evidence that the persistence is mostly due to behavioral adjustments. Second, it is challenging to assess the magnitude of the incentives necessary to achieve similar consumption levels without lumpy adjustments. I structurally estimate the model to address this question in Section 5. For a given value of η , equation (3) pins down a_0 and a_1 by setting p (in real terms) and q at their pre– and post–crisis levels, assuming s = 0 out of crisis. Equation (3) also recovers the unknown price-equivalent of social incentives \tilde{p} during the crisis by setting $a_i = a_1$, and q and p at their crisis levels. Holding $a_i = a_0$ in the parameterized model, we then derive the increase in incentives necessary to achieve observed consumption levels during and after the crisis without lumpy adjustments.

The above strategy has two caveats. First, electricity prices are often nonlinear. In Brazil, the main electricity tariff is linear but fines of the conservation program changed marginal prices and increased the cost of consuming above the quota discontinuously. This is illustrated in Figure 2a for customers with quotas around 250 kWh. Moreover, when prices are nonlinear, customers often do not know the relevant marginal price at the time of consumption. Borenstein (2009) thus proposes a model in which customers set consumption rules based on some expectation of electricity costs, and only update these rules upon receiving feedback from electricity bills:

$$U(\overline{q}) = G\left(V(\overline{q}) + W - \int C(q, p, s) f(q|\overline{q})\right), \text{ with } q \sim \mathcal{N}(\overline{q}, \sigma \overline{q})$$
(5)

with \overline{q} the expected quantity given the consumption rules. C(.), the nonlinear cost of electricity, is uncertain because of demand shocks ($\sigma > 0$). Figure 2b shows how uncertainty

smoothes out the known nonlinear schedule of economic incentives during the crisis CC(q, p), which is a function of the main tariff p. In Section 5, I assume that $\exists!\tilde{p}$ such that incentives took the form $\int CC(q, p + \tilde{p}) f(q|\bar{q})$ during the crisis. This allows me to compare social and economic incentives in a meaningful way despite the nonlinearities. I then use quasi-exogenous variation in quotas for a specific group of LIGHT customers to separately identify the two unknown parameters during the crisis, \tilde{p} and σ . Second, the strategy is conditional on a value of η . In the next subsection, I obtain a first credible estimate of the price elasticity of residential electricity demand in Brazil ϵ by exploiting tariff variation over time across utilities. $|\epsilon|$ provides an upper-bound for $|\eta|.^{25}$

2.2 Estimating a price elasticity of residential electricity demand in Brazil

I rely on the utility-level panel of average consumption and electricity tariffs. Specifically, I regress the logarithm of average residential consumption on the logarithm of the main residential tariff:

$$Log(kWh_{d,r,t}) = a_d + \beta_{r,t} + \epsilon Log(Price_{d,t}) + \nu_{d,r,t}$$
(6)

where a_d and $\beta_{r,t}$ are fixed effects for utility d, and year t by region r. $\nu_{d,r,t}$ is an error term clustered by utility. I consider yearly variations, averaging prices and quantities, because demand typically responds with a lag (Ito, 2012*a*). I use all the years post–crisis.

There are two major concerns with equation (6). First, there is rarely a unique price of electricity. In Brazil, the main residential tariff is essentially linear, but an alternative tariff for low-income and small consumers offers nonlinear percentage discounts on this unit price. Changes in residential prices, however, typically apply to the main tariff. Therefore, percentage changes in the main tariff capture percentage changes in every marginal price.

Second, changes in prices may be endogenous to changes in quantities. The price–cap mechanism limits such a concern in Brazil. Between *revision* years, demand risk entirely falls on utilities and yearly price *adjustments* are not endogenous to changes in quantities by design.²⁶ Price revisions every four to five years may still create some endogeneity, biasing estimates of ϵ away from 0. I directly assess the extent of endogeneity in two ways. First, I run the same regression instrumenting the main tariff by its cost–of–energy component (exogenous to the firm on a yearly basis) available for every utility since 2005. Second, I estimate equation (6) excluding years of price *revisions* and including utility–specific fixed effects for each between–revision period. The only variation left comes from price *adjustments*.

Results are presented in Table 1. I estimate $\hat{\epsilon}$ at -.214 (column 1) and -.183 (column 2) with the full variation in tariffs from 2003 and 2005, respectively. Estimates using only

²⁵I underestimate the role of social incentives and lumpy adjustments if customers confuse marginal with average prices during the crisis (Ito, 2012a). Average and marginal prices are equivalent for larger consumers out–of–crisis. Using an upper bound for $|\eta|$ also underestimates the role of social incentives and lumpy adjustments in Section 5.

 $^{^{26}}$ See ANEEL (2005). This was confirmed through personal communications with ANEEL.

the variation from price adjustments (column 3) or the IV strategy (column 4) fall within the same range. Price endogeneity does not appear to be a major issue in our setting.²⁷

2.3 Responses to the economic incentives of the conservation program

Before turning to the empirics, I use the above framework to display predicted responses to the economic incentives of the conservation program in Figure 2. I assume $\eta \simeq \hat{\epsilon} =$ -.2, and consider changes in the cost of electricity in the first five months of the crisis, before any change in quotas. I focus on customers with quotas around 250 kWh, here and throughout the paper. These customers face linear prices out-of-crisis and were subject only to fines during the crisis, simplifying the analysis. Results are qualitatively similar for other categories. In panel (a), I assume no uncertainty ($\sigma = 0$). In panel (b), I use a degree of uncertainty ($\sigma = .15$) estimated as in Borenstein (2009).²⁸ I show how consumption levels differ whether or not customers made lumpy adjustments at the onset of the crisis consistent with median consumption levels after the crisis (\simeq quota for these customers). With no uncertainty, customers are predicted to bunch at the quota because they have no incentive to further reduce consumption. Uncertainty increases marginal prices below the quota and the model predicts consumption levels slightly below the quota with lumpy adjustments. Median consumption levels during the crisis were in fact 21.8% below the quota for these customers. Social incentives (e.g., conservation appeals) may thus have played a major role.

3 Short– and long–term impacts of the conservation program

This section provides a graphical and statistical analysis of the short– and long–term impacts of the electricity conservation program. By exploiting a monthly panel of utilities, I compare

²⁷Instrumenting average prices by the main residential tariffs provides similar results. Ito (2012*a*) obtains a similar estimate for the US. Reiss and White (2005) obtain an elasticity of -.39 in the US. The authors note, however, that their result is at the upper end of existing estimates. They find much smaller price elasticities for households without electric heating or air conditioning. $|\hat{\epsilon}|$ overestimates $|\eta|$ if the identifying variation in Table 1 led to lumpy adjustments. The identifying variation is displayed graphically in the Web Appendix and is in fact of limited magnitude.

²⁸Borenstein (2009) uses a balanced panel of California households to explore the degree of uncertainty. For each customer, he separately estimates: $ln(kWh_t) = \sum_{j=1}^{12} \alpha_j + \beta ln(kWh_{t-1}) + \gamma time_trend + \nu_t$, where α_j is a calendar month fixed effect and β is the serial correlation in monthly consumption. The root mean squared error (RMSE), the standard deviation of the regression, indicates how well the model predicts consumption. Borenstein (2009) obtains a median RMSE of 0.17 (average 0.2), implying that a median customer using this model is able to predict consumption with a standard error of 17%. I replicate this approach for a balanced panel of 6610 randomly selected customers from Rio de Janeiro with quotas around 250 kWh who were observed continuously from 2000 to 2005. I obtain a median RMSE of 0.14 (average 0.16). The perceived uncertainty may differ and is structurally estimated in Section 5 ($\hat{\sigma} = .2$).

trends in average electricity consumption per customer between utilities subject or not to the program (difference-in-difference, synthetic controls). I reinforce a causal interpretation by controlling for trends in other relevant variables. By exploiting individual billing data, I go beyond average effects and investigate the distribution of conservation efforts among customers. I first provide key descriptive statistics, then present the graphical evidence, and finally turn to the statistical analysis.

3.1 Descriptive statistics

Columns (1)–(4) in Table 2 compare averages of relevant descriptive variables (range in brackets) across utilities in the North–East, the South–East/Midwest (and LIGHT), and the South in 2000. Average residential electricity consumption per customer was higher in the South–East/Midwest and lower in the North–East, following differences in median household income. Overall, average consumption was lower than in more developed countries. First, electricity is relatively expensive in Brazil. For instance, the main residential electricity tariff for LIGHT customers was higher than the US average price in 2000. Second, penetration rates of major domestic appliances were low in Brazil. Most households owned a refrigerator in the South–East/Midwest and the South, but only 50% owned a washing machine and less than 10% had air conditioning. Ownership rates were much lower in the poorer North–East. In fact, median household income, penetration rates, and average consumption levels were systematically lower for utilities in the North–East than in the South. In contrast, the distributions overlap between the South–East/Midwest and the South–East/Midwest and the South–

A parallel trend assumption between utilities in the North–East and in the South may not hold with very different initial values. To explore such a concern, I compare trends in the same variables between 2000 and 2010 (census years t) using the following specification:

$$log(y_{d,t}) = a_d + \beta \ \mathbb{1} \ (t = 2010) + \gamma \ \mathbb{1} \ (t = 2010 \ \& \ Treat_d = 1) + \nu_{d,t}$$
(7)

where a_d is a fixed effect for utility d, and *Treat* indicates a utility from the North–East or the South–East/Midwest. $\nu_{d,t}$ is an error term clustered by utility. Columns (5)–(6) report estimates of $\hat{\gamma}$ for models excluding the North–East or the South–East/Midwest, respectively.

Median household income grew relatively more in the North–East than in the South, while ownership rates of refrigerators and washing machines grew much more over the 10– year period (.088, .235, and .38 log point, respectively; column 6). This pattern is consistent with a "S–curve" relationship between income and appliance ownership (Wolfram, Shelef and Gertler, 2012). The common–trend assumption is clearly violated. Moreover, because distributions do not overlap prior to the crisis, I am unable to control for these differential patterns without relying on arbitrary parametric assumptions. Therefore, I do not consider the North–East in the remainder of this paper.

Trends in appliance ownership are mostly comparable between the South–East/Midwest and the South (column 5). Median household income grew more in the South on average over the 10–year period (.1 log point). However, I can control for changes in median household income in the empirical analysis because distributions overlap in the South–East/Midwest and in the South. Mean electricity consumption grew more in the South over the 10–year period (.12 log point). I attribute this difference to the conservation program in the following subsections.²⁹

3.2 Graphical evidence

A. Overall average effects. Figure 1b already provided some evidence for the impacts of the conservation program. In June 2001, seasonally adjusted average residential electricity consumption per customer decreased by over 30% in the South–East/Midwest. National policies or spillovers from conservation appeals may explain a smaller drop (8%) in the South, which also persisted. Consumption partially rebounded at the end of the crisis in the South–East/Midwest but stayed about 20% below pre–crisis levels. Since then, it evolved similarly in the South–East/Midwest and in the South, suggesting a sizable persistent effect of the conservation program.

B. Utility-specific average effects. Figure 1b pooled utilities within subsystems. In Figure 3a, I display average impacts of the conservation program for each utility. I use synthetic control methods (Abadie, Diamond and Hainmueller, 2010), which compare the evolution of an outcome in a *treated* utility to the evolution of the same outcome in a synthetic control utility. The outcome of interest, $Y_{d,t}$, is the demeaned seasonally adjusted logarithm of average monthly residential consumption. Formally, define T_0 as the number of precisis periods in the monthly balanced panel of utilities, and index utilities in the South by c = 1...C. The synthetic control estimator of the impact of the conservation program in $t > T_0$ is given by:

$$\alpha_{d,t} = Y_{d,t} - \sum_{c=1}^{c=C} w_{d,c}^* Y_{c,t}$$
(8)

The synthetic control, a weighted sum of the outcome for utilities in the South, provides an estimate of the counterfactual for a given utility d in the South–East/Midwest. Define $W = (w_{d,1}, ..., w_{d,C})$, a vector of positive weights that sum to one. Weights W are chosen to minimize:

$$||Y_{d0} - Y_{c0}W|| = \sqrt{(Y_{d0} - Y_{c0}W)' V (Y_{d0} - Y_{c0}W)}$$
(9)

where Y_{d0} and Y_{c0} are vectors containing the values of the outcome in pre–crisis periods $(t \leq T_0)$ in the treated utility and in control utilities, respectively. An optimal choice of V minimizes the mean squared error of the synthetic control estimator (Abadie, Diamond and Hainmueller, 2010).

²⁹In the Appendix, Figure A.3 displays the distribution of median household income, electricity prices, and average consumption across utilities in 2000 and the distribution of changes over the 10–year period. In the Web Appendix, Tables B.1 provides additional descriptive statistics. The 2010 census does not record ownership of air conditioners.

In Figure 3a, I display the estimated $\hat{\alpha}_{d,t}$ for each utility in the South–East/Midwest separately, but also for each (control) utility in the South as a placebo.³⁰ I aggregate monthly estimates into six pre–crisis periods (every year from 1996 to 2000 and the first months of 2001), one crisis period, and ten post–crisis periods (the remaining months in 2002 and every year from 2003 to 2011). Pre–crisis differences between each utility and its synthetic control are successfully minimized. During the crisis, average residential electricity consumption per customer dropped by at least .2 log point for every utility in the South–East/Midwest compared to its synthetic control, and not for a single utility from the South. The effect reaches .4 log point for LIGHT. At the end of the crisis, consumption rebounded but remained persistently below estimated counterfactuals for every utility in the South–East/Midwest. For most utilities, the synthetic control estimator is also below any placebo effect for utilities in the South not subject to the conservation program. LIGHT average consumption was still .175 log point below its synthetic control in 2011. Differential patterns in Figure 1b are thus not due to outliers. Moreover, a similar strategy reveals no impact of the conservation program on residential tariffs.³¹

C. Average effects for panels of LIGHT customers. Compositional changes in utilities' customer bases may affect average consumption levels. For instance, some customers may have connected themselves illegally to the grid to avoid paying for metered electricity. Figure 3b therefore compares the evolution of average residential electricity consumption for different samples of LIGHT customers. I consider each billing month from 2001 to 2005 compared to the same months in 2000 to limit seasonality issues. I use first (i) a 2% random sample of customers in each month (50,000–60,000 customers per month), and (ii) a balanced panel of 44,817 randomly selected customers billed continuously between 2000 and 2005. By design, the balanced panel is not subject to serious composition issues. Moreover, electricity theft may only occur if customers have both legal and illegal connections to the grid because I drop customers with zero metered consumption in three consecutive months. Theft is more prevalent among smaller and poorer consumers in Brazil. I therefore also consider (iii) the top decile of the panel in each month, and (iv) another balanced panel of 12,054 customers billed continuously between 2000 and 2005 from Leblon, a wealthy neighborhood of Rio de Janeiro. Patterns are indistinguishable among the four samples. Therefore, composition issues are unlikely to severely bias estimates of average effects. Consumption fell more than 30% below 2000 levels during the crisis and remained about 20% lower until 2005. The pattern observed

³⁰In this case, the synthetic control is a weighted average of the outcome for the other utilities in the South. I use six pre–crisis periods in the estimation: every year from 1996 to 2000 and the first months of 2001. As of 1996, I have 23 and 11 utilities in the South–East/Midwest and in the South, respectively. I demean the outcome of interest because I only care about matching trends and average monthly residential consumption for some utilities in the South–East/Midwest falls outside the support for utilities in the South.

³¹See Appendix Figure A.2. Aggregate impacts on consumption cannot be due to any direct effect of low rainfall in the summer because several utilities, such as LIGHT, mostly serve urban areas. Appendix Figure A.3 displays the relationships between long–term changes in average electricity consumption, median household income, and the main residential electricity tariff for every utility. Impacts on average consumption, comparing the South–East/Midwest and the South, hold for a given change in median income and electricity price.

in Figure 1b thus holds for balanced panels of continuously metered customers.

Figure 3b also provides information on the precise timing of customers' responses. Because of staggered billing, bills sent in month t cover consumption in months t and t-1. The government program applied to billing cycles starting after June 4, 2001. In most cases, the June bill thus covered consumption after that date but that was not yet subject to fines and bonuses. Yet average consumption reductions had already reached 22.5% in the June bill. Conservation appeals started on June 4. Moreover, tax changes on goods such as efficient light bulbs came into force on June 1. Fines were suspended for the March 2002 bill. Average consumption rebounded immediately even though bonuses were still offered to smaller consumers.

D. Distributional effects for LIGHT customers. In contrast to utility-level data, individual billing data allow us to investigate the distribution of customers' responses. Figure 3c displays Kernel densities for electricity consumption billed in August (winter consumption, less sensitive to weather) in 2000, 2001, 2002, and 2005 for the same balanced panel of 44,817 randomly selected customers. Densities are unimodal. The 2001 density is stochastically dominated by the other ones. The post-crisis densities are very similar in 2002 and 2005 and fall exactly between the crisis and pre-crisis densities. Average consumption reductions thus came from sizable reductions at every level of consumption.³²

Figure 3d displays the distribution of conservation efforts for customers facing the same economic incentives during the crisis. I construct a balanced panel of 10,341 LIGHT customers from Rio de Janeiro with quotas around 250 kWh. These customers were subject to the economic incentives (fines) illustrated in Figure 2. I present Kernel densities for consumption levels normalized to the quota in the first five months of the crisis (before any change in quotas) and in the same months in 2002 and 2005. I find no bunching at the quota. During the crisis, 92.5% of these customers consumed less than their quotas on average. In 2002 and 2005, 55% and 55.2% were still consuming below the quota. The median customer consumed 21.8%, 3.3%, and 4.1% below the quota during the crisis, and in 2002 and 2005, respectively. Appendix Figure A.5 also shows that there is a strong correlation between consumption reductions during and after the crisis.³³

The above evidence indirectly addresses the question of electricity theft. Establishing

³³Similar results hold for other customer categories. Median consumption levels for customers with quotas around 190 kWh (resp. 340 kWh) were 18.8% (resp. 26.1%) and 2.5% (resp. 7.1%) below the quota during the crisis and in 2002, respectively. I find no bunching at the quota in monthly graphs, for different customer categories, and using small bandwidths. Similarly, Borenstein (2009) finds no bunching around kinks induced by block–pricing in California.

 $^{^{32}}$ In the Web Appendix, Figure B.5 shows mean consumption compared to quotas during and after the crisis by consumption level at baseline for customers with quotas set at 80% of baseline. For each category, consumption was more than 15% below the quota or about 32% below baseline during the crisis. Larger consumers reduced consumption by more than 25% below their quotas or 40% below baseline. After the crisis, average consumption was still below the quota for all but the smallest consumption categories. Considering shifts in the distribution of consumption in Figure 3c avoids mean reversion issues (Borenstein, 2009; Ito, 2012*a*). I also reproduced Figure B.5 as if the crisis happened in 2004 (placebo). Mean reversion cannot explain the low consumption levels on the graph (available upon request).

an illegal connection to the grid is a (lumpy) investment, so electricity theft is unlikely to explain the difference in consumption levels during and after the crisis. However, it may explain part of the difference between pre– and post–crisis consumption levels. Utility–level data on distribution losses yield inconclusive results.³⁴ Individual billing data are therefore particularly useful. Average consumption reductions are not due to customers fully disconnecting themselves from the official grid, and are similar for customer categories less likely to establish illegal connections (Figure 3b). Consumption was reduced throughout the whole distribution of consumption levels, both in the short and long run (Figure 3c). Finally, even if some relatively large or wealthy consumers have illegal connections, this share is likely to be small. Figure 3d shows that median effects were comparable to mean effects and that the majority of customers dramatically reduced consumption. Electricity theft played at most a minor role in the average impacts of the conservation program.

3.3 Statistical analysis

I now turn to a statistical analysis. I exploit the balanced panel of utilities in a difference– in–difference strategy comparing utilities in the South–East/Midwest and in the South (not subject to the conservation program) over time. I control for changes in electricity tariffs and other variables available yearly. I then investigate the robustness of the estimated long– term effects by matching additional information from the 2000 and the 2010 censuses to the concession area of each utility.

Main difference-in-difference results

I regress the logarithm of average residential consumption per customer for utility d from region r in month m of year t on dummies for various time periods p:

$$Log(kWh_{d,r,m,t}) = \alpha_d + \beta_{r,m} + [\gamma_p + \delta_p SouthEast/Midwest_d] + X_{d,r,m,t} + \nu_{d,r,m,t}$$
(10)

where α_d , $\beta_{r,m}$, and γ_p are utility, calendar month-per-region, and time-period fixed effects. SouthEast/Midwest_d indicates a utility from the South-East/Midwest. δ_p captures a difference-in-difference estimator for the impact of the conservation program in each time period. $\nu_{d,r,m,t}$ is an error term clustered by utility. I consider yearly indicators before and after the crisis. I divide the crisis years into pre-crisis (early 2001, reference time period),

³⁴Total electricity load did decrease during the crisis, but the decrease also came from other sectors of activity (industry, commerce, government). Utilities report yearly information on distribution losses to the regulator. Unfortunately, many utilities did not provide this information prior to 2000. The data are also very noisy when divided into technical (engineering estimates) and non-technical (load residuals, including theft) losses. I use yearly reports of technical and non-technical losses from 1998 to 2009 for 20 utilities in the South–East/Midwest and in the South in Web Appendix Table B.2. I find large persistent, but not significant, reductions in technical losses. This is mechanical if engineering losses are proportional to load. Estimates for non-technical losses to specific neighborhoods in their concession area.

crisis (June 2001–February 2002), and post–crisis (rest of 2002) periods. $X_{d,r,m,t}$ controls for the logarithm of the main residential tariff (available since 1996), and yearly data on population size, formal employment, and median formal wages (in logs) that can be matched to the concession area of each utility (until 2010).

Estimates of $\hat{\delta}_p$ are displayed in Figure 4a with 95% confidence intervals (and in Appendix Table A.1). In Figure 4b, I restrict the sample to utilities with overlapping support in average electricity consumption and household median income at baseline. Pre–crisis differences were small (even if sometimes significant), supporting the difference–in–difference strategy. Average electricity consumption dropped sharply when the conservation program came into force. I estimate an impact of .25 log point during the crisis. Consumption levels rebounded after the crisis but were still lower in the South–East/Midwest by .115 log points until the last sample year. Improving sample comparability between utilities in the two subsystems only confirms the results (panel b). Results are similar without controls and excluding summer months.³⁵

Given the estimated price elasticity in Table 1 (-.2), average impacts of the conservation program are equivalent to a price increase of 1.25 log points during the crisis and of .6 log point permanently. Economic and social incentives of the conservation program were suspended after the crisis. The persistence of the impacts thus provides strong evidence that households made lumpy adjustments to their propensity to consume electricity during the crisis, and estimates their magnitude. In Section 4, I provide evidence that lumpy adjustments came from behavioral adjustments rather than physical investments. In Section 5, I estimate the necessary increase in incentives to achieve the observed consumption levels in the absence of lumpy adjustments. However, I first test the robustness of the long-term impacts in the next subsection.

Robustness of the long-term effects

I investigate whether estimated long-term impacts are robust to additional controls available from the 2000 and 2010 censuses. Median household income grew faster on average in the South than in the South-East/Midwest from 2000 to 2010 (column 5 in Table 2). Other relevant variables may have also experienced different trends over the same period. I estimate the following regression:

$$log(kWh_{d,t}) = \alpha_d + \beta \ \mathbb{1} \ (t = 2010) + \gamma \ \mathbb{1} \ (t = 2010) \times SouthEast/Midwest_d + X_{d,t} + \nu_{d,t}$$
(11)

³⁵See Appendix Tables A.1 and A.2. I obtain similar results controlling for average electricity prices, instrumented or not by the main residential tariff (available upon request). Because of nonlinearities, average prices are endogenous to consumption levels. This is not the case for the main electricity tariff (see Table 1). Estimates of $\hat{\gamma}_p$ and of the differential trend between utilities in the North–East and in the South (which cannot be interpreted causally, see Section 3.1) are available upon request and in earlier working paper versions (Gerard, 2012, 2013).

where $\nu_{d,t}$ is an error term for utility d in census year t clustered by utility. Table 3 displays estimates of the difference-in-difference coefficient $\hat{\gamma}$. Average residential electricity consumption decreased by about .12 log point in the long-run for utilities in the South-East/Midwest compared to the South. Results are similar without including any timevarying covariates (columns 1 and 4), controlling for changes in the main residential tariff and in median household income (columns 2 and 5), and controlling for changes in population size, average household size, urbanization, employment, and the share of housing units with bathrooms (columns 3 and 6). Results are also similar when I restrict the sample to utilities with overlapping levels of average electricity consumption and median household income in 2000 (columns 4–6). Long-term impacts in Figure 4 are thus robust to controlling for additional relevant variables.³⁶

4 Adjustment mechanisms

Households reduced electricity use dramatically during the crisis. The persistent impacts of the conservation program imply that households made substantial lumpy adjustments to their propensity to consume electricity. In this section, I provide suggestive evidence that household responses and lumpy adjustments came from behavioral adjustments, which are more likely in a context of low penetration rates of many domestic appliances. Persistent effects may thus arise from new habits.

Reducing electricity use by over 30%, and by 20% through lumpy adjustments (LIGHT customers, Figure 3a and 3b), requires drastic changes in the efficiency (e.g., investments) or the use (e.g., habits) of domestic appliances. In Table 4, I decompose average residential electricity consumption for LIGHT customers by source, using an engineering model constructed to estimate load curves from residential customers in 1999.³⁷ The model uses data on average penetration rate, average power, and average daily usage for seven sources of electricity use. Lighting and refrigeration amounted to about 27% and 31% of total electricity use prior to the crisis (222 kWh), respectively. Electric showers, which heat water through an electrical device in the shower head, amounted to over 19%, or twice the electricity use of TVs. Air conditioning reached 14% on average but is concentrated in summer months. The model omits a few other sources. For instance, standby power use could amount to 10 kWh–20 kWh a month at the time.³⁸

³⁶Coefficients on these controls are imprecisely estimated. Sample size and degrees of freedom considerations limit the number of controls one can add. However, results are similar with controls for housing unit size, formal employment, and agricultural employment (not shown). Appliance ownership is potentially endogenous if customers changed their purchasing decisions during and after the crisis. Estimates remain large (9%) including such controls. This difference likely reflects nonlinear income effects given the evidence in Section 4. Results are robust to controlling for the logarithm of average electricity prices instrumented or not by the logarithm of the main residential tariff.

³⁷Personal communication with Professor Reinaldo Souza, Pontifícia Universidade Católica do Rio de Janeiro.

³⁸Personal communication with PROCEL and Correio Braziliense (May 26, 2001).

4.1 Appliance replacement

In the PROCEL household surveys conducted in 2005 (see Section 1.3 and below), few respondents in the South–East/Midwest reported replacing appliances with more efficient ones during the crisis.³⁹ Replacing domestic appliances is particularly expensive in Brazil due to the high cost of credit. Ex ante, appliances' manufacturers expected net losses from the electricity crisis (Folha de São Paulo, June 5, 2001). Ex post, large chain stores considered that sales of appliances suffered from the crisis (Folha de São Paulo, March 6, 2002). Figure 5a displays time-series on yearly sales of various electricity-intensive domestic appliances from manufacturers' reports (Mascarenhas, 2005). There was no particular increase in 2001 or 2002, except for air conditioners. Difference–in–difference results, however, hold when considering only winter months (Appendix Table A.2). Other household survey data reveal no differential trends in ownership rates or in the purchase of major domestic appliances across subsystems (Web Appendix Table B.3, Figures B.6 and B.7). In the Appendix, I also display monthly sales data from one of the leading manufacturers of electric showers in Brazil (Appendix Figure A.4). Sale volumes did not increase differentially in the South-East/Midwest compared to the South. The average power of models sold during the crisis only decreased slightly (10%). Therefore, possible energy savings from appliances bought in 2001–2002 are unlikely to explain the impacts of the conservation program.

4.2 Adoption of compact fluorescent light bulbs

In the PROCEL household surveys, 45% of households in the South–East/Midwest reported adopting compact fluorescent light bulbs (CFLs) during the crisis and most of them continued using them afterward (Web Appendix Table B.5). Figure 5b displays data on yearly imports of CFLs, which were not produced in Brazil. Imports, encouraged by a reduction of federal taxes, more than doubled in 2001. They returned to their pre–crisis levels afterward but kept rising over the years. As a result, the penetration rate of CFLs in residential units was much higher after the crisis (PROCEL surveys conducted in 1997 and 2005). Interestingly, the increase was large in every region and even larger in the South, not subject to the conservation program. The engineering model used in Table 4 was revised in 2002 to include new data on light bulbs' penetration rates. Holding constant other usages, CFL adoption reduces electricity use by 12 kWh or 5.5% in the model (Table 4, row a). It may thus explain part of the drop in electricity consumption during and after the crisis, including in the South (Figure 1b), but not the differential impact in the South–East/Midwest.

³⁹See Web Appendix Tables B.4. Only one in eight households reported such a substitution during the California crisis (Lutzenhiser, 2002). These surveys have been used in other research (Ghisi, Gosch and Lamberts, 2007).

4.3 Behavioral adjustments

Conservation appeals encouraged households to modify consumption behaviors during the electricity crisis. Specific behaviors were suggested in the media and in electricity bills. Anecdotal evidence suggests that households did adjust consumption behaviors. The PRO-CEL surveys provide evidence on the persistence of behavioral adjustments adopted during the crisis. Panel A in Table 5 summarizes retrospective information on 14 conservation behaviors and whether households adopted such behaviors before, during, and after the crisis. Panel B summarizes retrospective information on the use of eight major domestic appliances and whether households used these appliances less in 2005 than they did before the crisis.⁴⁰ This information was only collected for households subject to the conservation program (not in the South). Over 50% of households adopted a new conservation behavior during and after the crisis. In all cases, the share of respondents adopting a particular behavior was higher during and after the crisis. Differences are particularly large for behaviors associated with the use of electric showers, refrigerators, and washing machines. In 2005, households report having reduced usage compared to before the crisis for about 40% of their domestic appliances (conditional on appliance ownership). About 70% of households reduced usage of at least one appliance. Many households purchased freezers in the high-inflation years prior to 1995 to buy food on payday and store it (Meier, 2005). Some of these were likely superfluous at the time of the crisis. Accordingly, 38% of households reduced their use of freezers.

Panels A and B only provide time–series evidence of behavioral adjustments. In panel C, I use information on consumption behaviors in 2005 asked of every household, including in the South. I compare responses by subsystem for three conservation strategies often mentioned in relation to the crisis (private communication with PROCEL; Meier, 2005): unplugging freezers, avoiding standby power use, and adopting CFLs. Column (1) controls for seven electricity consumption categories. Column (2) adds controls for several household characteristics. Unplugging freezers and avoiding standby power use remained more prevalent in the South–East/Midwest in 2005. Households in the South were more likely to report leaving their appliances on standby for almost every appliance. In contrast, CFL penetration rates were higher in the South, suggesting again that CFL adoption cannot explain the differential impacts on electricity use in the South–East/Midwest.⁴¹

The persistent impacts of the conservation program in the South–East/Midwest are thus

⁴⁰Media reports on changes in consumption behaviors include keeping lights off (*O Globo*, June 4, 2001), reducing appliance usage (*Com Ciência*, July 10, 2001), and buying groceries more often after turning freezers off (*Folha de São Paulo*, March 5, 2002). Web Appendix Tables B.6 and B.4 present data for specific behaviors and appliances.

⁴¹The difference is reduced by half when controlling for household size, housing tenure, number of bathrooms (linearly), household earnings, gender and education of household head, housing size and type, residence condition, neighborhood type, roof, floor, and wall material, and type of water access (dummies). Other cross-sectional comparisons could be misleading. For instance, households in the warmer South– East/Midwest are more likely to set their electric showers to colder "summer mode." Web Appendix Table B.7 displays results on standby power for each appliance.

likely due to behavioral adjustments. In Table 4, I illustrate the possible role of specific conservation behaviors on average electricity use for LIGHT customers. Reducing lighting by half, in combination with CFL adoption, saves 36 kWh or 16% of electricity use (row b). Unplugging half of freezers only saves about 4% (row c). Reducing TV use by half has a similar effect (row d); reducing the use of electric showers by half saves about 10% (row e). A decrease in the use of air conditioning cannot explain the similarly large drop in consumption in winter months. Yet, it could have had a large effect in the summer. Reducing air conditioning by half saves 15 kWh on average and could thus save about 60 kWh in the summer (row f). These simulations show that households subject to the conservation program must have resorted to a series of severe behavioral adjustments to achieve the consumption levels observed during and after the crisis.⁴²

5 The relative roles of social incentives and lumpy adjustments

The conservation program induced large and lasting reductions in electricity use. On the one hand, customers were responding to contemporaneous incentives during the crisis, given the rebound in consumption levels when conservation measures were suspended. On the other hand, households made lumpy behavioral adjustments to their propensity to consume electricity, given the persistent impacts after the crisis and the available survey evidence. In this section, I structurally estimate the model of Section 2. This allows me to evaluate the role of social incentives and lumpy adjustments. I estimate a price-equivalent of the social incentives \tilde{p} and the increase in incentives necessary to achieve the observed reductions in electricity use absent any lumpy adjustment.⁴³

I assume that households choose expected quantity \overline{q} every month to maximize:

$$U(\overline{q}) = G\left(a_j \frac{\overline{q}^{1+1/\eta}}{1+1/\eta} + W - \int CC(q, p+\widetilde{p}) f(q|\overline{q})\right), \text{ with } q \sim \mathcal{N}\left(\overline{q}, \sigma \overline{q}\right)$$
(12)

where CC(.) is the known schedule of economic incentives and p is the main electricity tariff. Out-of-crisis, social incentives were nil ($\tilde{p} = 0$) and CC(.) was linear for larger consumers. First-order conditions imply: $\ln \bar{q} = \eta \ln p - \eta \ln a_j$. For a given value of η , this expression

 $^{^{42}}$ Lutzenhiser (2002) interviewed 400 households that experienced price spikes and public appeals in 2000–2001 during the California crisis. The most typical conservation behavior was a reduction in the use of existing appliances.

⁴³Typical reduced–form techniques do not allow me to identify the role of specific incentives. The distribution of consumption levels for LIGHT customers is smooth over the few kinks and discontinuities in economic incentives. Customers who consumed just below their quotas and were granted bonuses did not behave differently in later months than customers who missed the bonus by just one kilowatt hour (available upon request). Most customers subject to fines reduced consumption well below their quotas and never received fines. Any impact of being "discontinuously" charged a fine is thus obtained from a selected group (see Web Appendix Figure B.8).

pins down a_j before (a_0) and after the crisis (a_1) by setting tariffs and consumption levels at their pre– and post–crisis levels. I assume that $a_{crisis} = a_1$. Therefore, during the crisis, the model must only explain consumption reductions beyond post–crisis levels. Customers with no economic incentives to reduce consumption below their quotas during the crisis, only fines for exceeding them, consumed 20% below their quotas (Figure 3d). On the one hand, customers may have been concerned about consuming above the quota because of a high degree of consumption uncertainty ($\sigma > 0$). On the other hand, social incentives may have increased the perceived cost of electricity below the quota ($\tilde{p} > 0$). Quota assignment rules for customers who moved into their housing units after the baseline period ("movers") provide me with quasi–exogenous variation in quotas to separately identifies \tilde{p} and σ . I thus estimate the model for these customers after estimating the impact of quotas on consumption in the next subsection.⁴⁴ The variation in quotas also provides reduced–form evidence on the limited role of the economic incentives.

5.1 Movers and quasi–exogenous variation in quotas

The quotas of customers who moved into their metered housing units after the baseline period (May–July 2000) were based on their first three billing months. Larger consumers use air conditioning in the summer in Rio de Janeiro. Consequently, larger consumers who moved in the summer of 2000–2001 were allocated more generous quotas. Figure 6a displays average electricity use prior to the crisis for a balanced panel of LIGHT customers (dash line). Consumption is lower in the winter and higher in the summer. The solid line shows average quotas by moving date for relatively large consumers who moved into their metered housing units in any given month (sample described below). After May 2000, the solid line follows the seasonality in consumption.

A. Sample selection. In this section, I focus on movers whose first monthly bill was sent between March 2000 and February 2001 and who were billed continuously for three years. I restrict attention to the 18,293 movers from Rio de Janeiro whose average consumption in the three months prior to the crisis falls in the top quartile of the movers' consumption distribution. Seasonality is stronger for larger consumers. Larger consumers are only subject to the main tariff out–of–crisis. Their economic incentives during the crisis were also simpler to understand. They were mostly subject to fines. Finally, the rebound of consumption levels in the February–March 2002 billing cycle suggests that marginal conservation efforts during the crisis were not due to bonuses.⁴⁵ The variation in quotas by moving date is large in this sample. Figure 6b compares the distribution of quotas between customers who

⁴⁴I underestimate the role of social incentives and lumpy adjustments if customers confuse marginal with average prices (Ito, 2012a). I underestimate the role of social incentives if $a_{crisis} > a_1$. Assuming that incentives took the form $\int CC(q, p + \tilde{p}) f(q|\bar{q})$ during the crisis allows me to compare social and economic incentives in a meaningful way despite the nonlinearities.

⁴⁵I select customers who received their first bill between March 2000 and February 2001 to verify that they did not receive bills in earlier months (actual movers) and to have at least three months of pre–crisis consumption.

moved in around baseline (May–July) and later in 2000 (October–December). The latter distribution stochastically dominates the former. The median quota differs by 28%. It is around 250 kWh for customers who moved in at baseline (May–July). The associated change in economic incentives at the median thus corresponds to offering a non–binding quota to the representative customer in Figure 2.

B. The impact of quotas. I estimate the impacts of quotas on consumption by regressing the logarithm of average consumption during the crisis on the logarithm of the quota. I instrument the quota of mover i by the average quota of movers (excluding i) who received their first bill in the same week w as i. Defining ν and ρ as individual error terms clustered by moving week, we have:

$$log(kWh_{i,w}^{crisis}) = \alpha + \beta \ log(quota_{i,w}) + X_{i,w} + \nu_{i,w}$$
(13)

$$log(quota_{i,w}) = \gamma + \delta \ log(Avquota_sameweek_w) + X_{i,w} + \rho_{i,w}$$
(14)

I consider only average consumption in the first five months of the crisis, before any extension in quotas. The instrument is valid if customers who moved in at different times are comparable. Figure 6c compares the distribution of average consumption levels in the three months prior to the crisis for the same two groups of movers. The distributions overlap closely. Customers who moved at different times had similar pre-crisis consumption levels in my sample. I test this statistically below and control for the logarithm of pre-crisis consumption in $X_{i,w}$. I also control for neighborhood fixed effects. Finally, I am interested in responses at the median because fines are likely to be the only economic incentives at the median (median quota = 250 kWh for movers at baseline).

Figure 6d offers a preview of the results. It displays the distribution of average consumption levels during the crisis for the same two groups of movers. Customers who moved in later in the year, and had larger quotas, consumed more electricity. However, the effect is very small.

Column (1) in Table 6 displays estimates of $\hat{\delta}$. The instrument is strong. Because the coefficient is close to 1, I present reduced-form results in the remaining columns. I find no effect of the instrument on consumption prior to the crisis (column 2). The quota elasticity $\hat{\beta}$ is around .17 without controls and .16 with controls at both the mean and the median (columns 3 and 4). Increasing quotas by 20% increased consumption by only 3% during the crisis. The impact is 50% smaller but significant in 2002 (column 5). In later years, it becomes smaller and noisier (not shown). Because the estimated impact is small during the crisis, idiosyncratic shocks or general equilibrium effects may rapidly weaken the link between quotas and consumption after the crisis.⁴⁶

 $^{^{46}}$ Yet, there is a high correlation between overall consumption changes during and after the crisis (Appendix Figure A.5). Quantile regressions do not include neighborhood effects because estimators would be inconsistent. As a robustness check, I performed a placebo analysis assuming that the crisis occurred in 2004–2005, selecting movers in a similar way. Placebo estimates are never significant and are very close to 0 (results available upon request). If electricity use was increasing in the first months after moving in, my estimates would provide upper bounds, and I would underestimate the role of social incentives and lumpy adjustments in the next subsection.

5.2 Parameter estimates and policy simulations

I structurally estimate the model for movers whose quotas were based on the baseline period and were around 250 kWh. The quota elasticity has only been estimated for movers. Fines were the only economic incentives for customers with quotas around 250 kWh (Figure 2). Focusing on customers with the same quota allows me to model a single nonlinear schedule of incentives. I then use the parameterized model for counterfactual simulations. Results are presented in Table 7.

A. Estimation. I use indirect inference techniques (Gouriéroux and Monfort, 1996) and minimize the distance between moments predicted by the model and empirical moments. The four parameters are the difference between the perceived $(p + \tilde{p})$ and the actual tariff (p) during the crisis, the degree of consumption uncertainty (σ) , and the two parameters capturing the propensity to consume electricity (appliance stock, habits) before and after the crisis (a_0, a_1) . Define m a (4×1) vector of empirical moments and $\mu_s(\phi)$ a (4×1) vector of simulated moments given parameter values $\phi = (\tilde{p}, \sigma, a_0, a_1)$. I obtain an estimator of ϕ by minimizing:

$$\left(m - \frac{1}{s}\mu_s(\phi)\right)'\widehat{W}\left(m - \frac{1}{s}\mu_s(\phi)\right) \tag{15}$$

The first three moments are the median of the average consumption levels between June and October before (311.63 kWh in 2000), during (203.68 kWh in 2001), and after (265.06 kWh in 2002) the crisis. June to October corresponds to the first five months of the crisis in 2001, before any chance in quotas. The last moment is the median of the average consumption levels during the crisis if quotas had been increased by 20% (209.71 kWh) using a quota elasticity of .16 (Table 6). The model takes as inputs a value of η (-.2, the estimated price elasticity in Table 1) and values of the electricity tariffs before, during, and after the crisis (in real terms; R\$.187/kWh, R\$.208/kWh, and R\$.238/kWh, respectively). I use the inverse of the variance–covariance matrix of the empirical moments as weighting matrix \widehat{W} (estimated through 100 bootstraps). Asymptotic standard errors are obtained as in Gouriéroux and Monfort (1996). I provide more details in the Web Appendix.⁴⁷

Estimation results are presented in the upper panel in Table 7 (rows a–d). Social incentives (e.g., conservation appeals) appear to have played a major role during the crisis. The perceived tariff $\widehat{p} + p$ is very large, 1.233 log points above the actual tariff p. The estimated degree of uncertainty ($\widehat{\sigma} = .2$) is close to the realized uncertainty estimated in Section 2. The parameterized model is able to closely predict the empirical moments used in the estimation (rows e–h). It slightly underestimates the median of the average consumption levels during the crisis for the panel of customers in Figure 3d (out–of–sample moment; row i).

⁴⁷The estimated price elasticity is an upper bound for $|\eta|$. I may therefore underestimate the role of social incentives and lumpy adjustments. A limitation is that the price elasticity was not estimated for this specific group of customers. The role of social incentives and lumpy adjustments is smaller for large values of η . It is unclear, however, whether these customers would have been relatively more responsive. For instance, there was little air conditioning or electric heating from June to October in Rio de Janeiro.

B. Policy simulation. In the bottom panel in Table 7, I use the parameterized model for counterfactual simulations. If I turn off the social incentives $(\tilde{p} + p = p, a_{crisis} = \hat{a_1}; \text{ row}$ j), consumption levels would have been 24% higher during the crisis. Customers may have been sensitive to the conservation appeals and voluntarily contributed to avoid blackouts and severe shortages. Players voluntarily (over-)contribute to avoid losing a public good in laboratory experiments, particularly if the loss is large (Iturbe-Ormaetxe et al., 2011). Appeals to social preferences may be particularly powerful in times of crisis (Mulligan, 1998). If I turn off the lumpy adjustments ($\tilde{p} + p = \widehat{p} + p, a_j = \hat{a_0}$; rows k and l), the tariffs should have been .58 log point (resp. .57 log point) higher than the perceived tariff (resp. the main tariff; rows l and m) to achieve the consumption levels observed during the crisis (resp. after the crisis). In our setting, the possibility of triggering lumpy adjustments thus reduces substantially the incentives necessary to achieve ambitious conservation targets.⁴⁸

6 Conclusion

The conservation program implemented during the 2001–2002 Brazilian electricity crisis induced substantial and widespread reductions in residential electricity consumption. An average impact of .25 log point over a nine-month period was obtained in a context of low baseline consumption levels, despite the fact that many customers faced limited economic incentives. This is due to two factors. First, conservation appeals appear to have played a major role. I structurally estimate a price-equivalent for these social incentives that amounts to a 1.2 log point increase in electricity tariffs. Second, incentives were large enough, and maintained long enough, to trigger lumpy behavioral adjustments. The conservation program reduced electricity consumption by .12 log point in the long run. In 2011, the impact still amounted to \$1.2 billion reduction in electricity bills or a spared capacity of 850MW in the South–East/Midwest. I estimate that incentives would have had to be .58 log point higher to achieve observed consumption levels during the crisis in the absence of these lumpy adjustments. This paper thus provides strong evidence that the possibility of triggering lumpy adjustments may substantially reduce the incentives necessary to achieve

⁴⁸Other models may also rationalize customers' behaviors. The price responsiveness may have been larger during the crisis ($\eta_{crisis} > \eta$). Customers may have overestimated the economic cost of exceeding their quotas. In the Web Appendix, I show that rationalizing the same empirical moments requires a value of η_{crisis} fivefold larger than the estimated elasticity out-of-crisis. This is far outside the range of estimates in the literature, especially given that there was little use of air conditioning or electric heating at the time. Rationalizing the same empirical moments requires a penalty for exceeding the quota 24 times higher than the actual economic cost. This is a very large degree of misunderstanding, even if customers were loss averse. Finally, the estimated degree of uncertainty σ in these alternative models is unrealistically high (at .4-.5). This is more than twice the realized degree of uncertainty estimated in Section 2. To better test whether customers overestimated the cost of exceeding the quota, one would ideally compare the behaviors of customers who received different feedback on the actual cost of non-compliance. I present graphical results for a related exercise in Web Appendix Figure B.8. Finally, customers may have been uncertain about future conservation policies. Policy uncertainty, however, could have pushed customers to consume *more* rather than *less* electricity because of the use of grandfathering in the first quota assignment rules.

ambitious energy conservation targets. It also indicates that appeals to social preferences may be particularly powerful at stimulating behavioral changes (e.g., energy conservation) whenever a common threat is widely accepted and perceived as imminent.⁴⁹ These features have yet to be associated with major environmental concerns such as climate change.

A welfare evaluation of the conservation program remains beyond the scope of this paper. This is in fact a general issue for policies involving social incentives. Their welfare cost may be high if social incentives act like a tax rather than a subsidy, and induce a sense of moral coercion or a feeling of guilt rather than a sense of moral duty or a warm glow. Estimating their impact on the behavior of economic agents is not enough to tell these cases apart. The innovative design of recent field experiments has begun addressing this concern (e.g., DellaVigna, List and Malmendier, 2012). However, a welfare framework has yet to be developed to meaningfully evaluate the respective appeals of economic and social incentives. This is a particularly important avenue for future research if social incentives are to become common policy instruments.

 $^{^{49}}$ In Japan, after the 2011 earthquakes, peak summer electricity demand was also reduced without economic incentives (by 15%; http://www.nytimes.com/2011/09/26/opinion/in-japan-the-summer-of-setsuden.html?r=1).



Figure 1: Cause and consequences of the electricity crisis and its conservation program

(a) Level of the hydro–reservoirs

Data from ONS and ANEEL. Panel (a) displays the evolution of hydro-reservoirs' capacity in the three main electric subsystems in Brazil (dotted lines indicate January). In the summer of 2000–2001, rainfall was exceptionally unfavorable in the North-East and the South-East/Midwest, leading to dangerously low reservoir levels. The need to reduce electricity demand was first acknowledged in March 2001 (dashed line). In the South, generous rainfall in 2000 eliminated any risk of shortage. The conservation program was implemented in the North-East and the South-East/Midwest from June 2001 to February 2002 (solid lines). Panel (b) displays the overall impacts of the conservation program on monthly average residential electricity consumption per customer for utilities in each subsystem (unweighted, seasonally adjusted; subsystems' shares of total residential consumption —North excluded— in parentheses). Trends were similar prior to June 2001. Consumption then dropped, especially for utilities subject to the conservation program (no blackout took place). Average residential consumption for these utilities partially rebounded after February 2002. Comparing patterns in the South-East/Midwest and in the South suggest that an impact has persisted until now.

(b) Average residential electricity consumption



Figure 2: Economic incentives of the conservation program and consumption choices

The figures display the economic incentives of the conservation program for customers with a quota of 250 kWh (80% of baseline). I consider the first five months of the crisis, before any change in quotas. I use the model, estimated price elasticity (-.2), and degree of uncertainty ($\sigma = .15$) from Section 2 to predict consumption responses. I also assume that customers may have made lumpy adjustments consistent with median consumption levels after the crisis (249 kWh at pre–crisis prices). I assume a budget of R\$500 and a tariff p of R\$.208/kWh (LIGHT, June 2001). The cost of electricity is nil if consuming below 100 kWh because of bonuses. Conditional on exceeding the quota, fines are paid for every kWh above 200. Above the quota, fines (i) increase the marginal price (by 50% up to 500 kWh, then 200%) and (ii) increase the cost discretely (by R\$5.2). In panel (a), I assume no uncertainty ($\sigma = 0$). Customers are predicted to bunch at their quotas (change in marginal price: $A \rightarrow B$; cost increase at the quota: $B \rightarrow B'$). With lumpy adjustments, customers consume at their new baseline a (249 kWh). In panel (b), I assume that there is some uncertainty ($\sigma = .15$), smoothing out the budget constraint. With lumpy adjustments, customers now consume 7.5% below the quota because uncertainty increases expected marginal prices below the quota ($a \rightarrow b$). Median crisis consumption levels were in fact 21.8% below the quota for these customers.



Figure 3: Evidence of short– and long–term impacts of the conservation program

(a) Utilities' average kWh vs. synthetic control

Panel (a) displays synthetic control estimators of the impacts of the conservation program for each utility in the South-East/Midwest, and in the South as a placebo, on the demeaned seasonally adjusted logarithm of average monthly residential consumption. Synthetic controls are weighted sums of utilities in the South. Weights minimize the distance between pre-crisis outcomes. Estimates are averaged into six pre-crisis periods (every year from 1996 to 2000 and the first months of 2001), one crisis period, and ten post-crisis periods (the rest of 2002 and every year from 2003 to 2011). They are large and negative for every utility in the South–East/Midwest during the crisis, and remain persistently negative afterward. Panel (b) displays the evolution of average residential electricity consumption in each billing month relative to the same months in 2000 for: (i) a 2% random sample of LIGHT customers in each month, (ii) a balanced panel of 44,817 randomly selected customers, (iii) the top decile of this panel in each month, and (iv) another balanced panel of 12,054 customers from Leblon, a wealthy neighborhood of Rio de Janeiro. Bills sent in month t cover consumption in t and t-1. Consumption fell more than 30% during the crisis. When conservation measures were suspended (except for bonuses), consumption rebounded immediately. It stayed about 20% lower until 2005. Patterns are indistinguishable among samples. Panel (c) uses the balanced panel in (ii), and displays kernel densities for electricity consumption billed in August in 2000, 2001, 2002, and 2005. The short- and long-term average reductions came from large reductions at every level of consumption. Panel (d) displays kernel densities for average consumption levels normalized to the quota in the first five months of the crisis (before any change in quotas) and in the same months in 2002 and 2005 (post-crisis) for customers facing the same economic incentives (see Figure 2). It uses a balanced panel of 10,341 LIGHT customers from Rio de Janeiro with quotas around 250 kWh. The median customer in panel (d) consumed 21.8%, 3.3%, and 4.1% below the quota during the crisis, in 2002, and in 2005, respectively. Kernel densities use Epanechnikov kernels and optimal bandwidths.

(b) Average kWh relative to 2000 (LIGHT)



Figure 4: Difference–in–difference results for the impact of the conservation program

95% confidence interval in dash (s.e. clustered by utility). Data for utilities (as of 1996) in the South–East/Midwest and in the South from 1996 to 2010. The figures display coefficients from regressing the logarithm of monthly average electricity consumption per customer for each utility on time–period dummies (yearly dummies, three dummies for 2001–2002 to isolate the crisis period) interacted with an indicator for utilities subject to the conservation program during the crisis (difference–in–difference estimators in each time period). The reference period corresponds to the first months of 2001. Regressions include uninteracted time–period dummies, utility and calendar month–per–region fixed effects, and control for the logarithm of the main residential electricity tariffs and available municipal yearly data matched to the concession area of each utility (log population, log share formally employed, log real median formal wage). Panel (b) restricts the sample to utilities with overlapping average consumption and median income levels in 2000. The conservation program reduced average electricity consumption by .25 log point during the crisis and by .115 log point in the long run.



Figure 5: Trends in appliance sales in Brazil around the crisis

Panel (a) displays yearly sales of electricity-intensive domestic appliances relative to 1994 (manufacturers' reports; Mascarenhas, 2005). There was no particular increase in 2001 and 2002, except for air conditioners. Difference-in-difference results, however, hold when considering only winter months (Appendix Table A.2). Household survey data reveal no differential trends in ownership rates or in the purchase of major domestic appliances across subsystems (Web Appendix Table B.3, Figures B.6 and B.7). Sales of electric showers for one of the leading manufacturers in Brazil did not increase differentially in the South-East/Midwest compared to the South (Appendix Figure A.4). Therefore, possible energy savings from appliances bought in 2001–2002 are unlikely to explain the impacts of the conservation program. Panel (b) displays yearly imports of compact fluorescent light bulbs (CFLs, not produced in Brazil) and their penetration rates in 1997 and 2005 (from PROCEL). Imports, encouraged by a reduction of federal taxes, more than doubled in 2001. They returned to their pre-crisis levels afterward but kept rising over the years. As a result, the penetration rate of CFLs in residential units was much higher after the crisis. Interestingly, the increase was even larger in the South, not subject to the conservation program. CFL adoption may thus explain part of the drop in electricity use during and after the crisis, including in the South, but not the differential impacts in the South–East/Midwest.

Figure 6: Quasi-exogenous variation in quotas and consumption responses during the crisis



(a) Seasonality in electricity consumption and vari-

(c) Pre-crisis consumption for different moving dates (check)

Kernel density

004

003

002

001

C 200

400

6Ó0

8Ó0 kWh

1000





(d) Crisis consumption for different moving dates (impact)



Sample of LIGHT customers from Rio de Janeiro whose first bill was sent between March 2000 and February 2001 (movers), observed continuously over a period of at least three years, whose average consumption in the three months prior to the crisis falls in the top quartile of the movers' consumption distribution. Quota assignment rules generated variation in quotas entirely due to different moving dates in this sample because of seasonality in electricity use. Panel (a, dash) shows the clear seasonality in average monthly electricity consumption prior to the crisis for the same panel of LIGHT customers as in Figure 3c (less electricity use in winter, June-September). Panel (a, solid) shows average quota levels by moving month for my sample of movers. Their quotas were based on their first three monthly bills if they moved in after May 2000. The seasonality in consumption translates into larger quotas for customers who moved in after the winter. Panel (b) shows that the quota distribution for customers who moved in later in the year in 2000 stochastically dominates the quota distribution for customers who moved in around the baseline period. The median quota differs by 28%. It is around 250 kWh for customers who moved in at baseline (May–July). The associated change in economic incentives at the median thus corresponds to offering a non-binding quota to the representative customer in Figure 2. Panel (c) shows that the same two groups had similar consumption levels in the three months prior to the crisis. Panel (d) shows that the group with larger quotas consumed only a little more electricity in the first five months of the crisis. A large increase in quotas thus had only a small effect on electricity consumption. Kernel densities use Epanechnikov kernels and optimal bandwidths.

Table 1: Estimating a price elasticity of residential electricity demand in Brazil

| | (1) | (2) | (3) | (4) |
|--|---------------|---------------|-------------|---------------|
| Dependent variable: Log(yearly m | ean of aver | age residen | tial consum | ption) |
| Log(yearly mean of main residential tariff) | 2144*** | 1829*** | 1982*** | 1889* |
| | (.02911) | (.02611) | (.04715) | (.09728) |
| First stage dependent variable: Lo | g(yearly me | ean of main | residential | tariff) |
| Log(yearly mean of the cost of energy in the | e main reside | ntial tariff) | | $.1768^{***}$ |
| | | , | | (.05446) |
| | OLS | OLS | OLS | IV-2SLS |
| Years | 2003 - 2011 | 2005 - 2011 | 2005 - 2011 | 2005 - 2011 |
| Exclude variation from revision years | No | No | Yes | No |
| Observations | 432 | 336 | 278 | 336 |
| Clusters | 48 | 48 | 48 | 48 |

Monthly administrative data for utilities (as of 2002) from the North–East, South–East/Midwest, and South. Significance levels: *10%, **5%, ***1% (s.e. clustered by utility in parentheses). The table displays the coefficient from regressing the yearly mean of average residential electricity consumption on the yearly mean of the main residential tariff (in logs). Regression includes year–by–region and utility fixed effects. Column (1) includes all years post–crisis. Column (2) uses data from 2005. I can test for endogeneity in price setting after 2005 (see text). I exclude *revision* years and include utility–specific fixed effects for each between–revision period (column 3). I instrument the tariff variation by the variation in the utility–specific cost of energy (column 4).

| Table 2: Descript | tive statisti | cs in 2000 |) and 10-year | · difference in | trends (2000 | -2010) |
|--|--|--|--|--|---|---|
| | (1) | (2) | (3) (3) (3) | (4) | (5) 10-vear 6 | (6) iffindiff |
| | | | uin-max] | | compared to | South, in logs |
| | South | LIGHT | South-East | North-East | South-East | North-East |
| | | ANEEL : | & Midwest administrative | data | & Midwest | |
| Average residential | 168 | 225 | 192 | 109 | 12*** | 028 |
| electricity use (kWh) | [134 - 191] | | [143-261] | [72.6 - 129] | (.026) | (.039) |
| Average residential | .152 | .185 | .162 | .149 | 083 | 113 |
| electricity price (R\$) | [.136171] | | [.143185] | [.135.162] | (.068) | (60.) |
| Main residential | .155 | .186 | .167 | .152 | 077 | .01 (10) |
| electricity tariff (K&) | [.136173] | | [.143–.191] Jansiis data | [.142164] | (.071) | (970.) |
| Median household | 616 | 800 | 649 | 295 | 1** | .088* |
| income (R\$) | [430 - 800] | | [453 - 1000] | [240 - 350] | (.05) | (.05) |
| Has electricity | 978 | 666. | .984 | .903 - | 006 | .068** |
| 2 | [.949997] | | [.896-1] | [.759989] | (.007) | (.027) |
| Has refrigerator | .927 | .972 | .924 | .656 | 004 | $.235^{***}$ |
| | [.828994] | 1 | [.82985] | [.555758] | (.018) | (.035) |
| Has washing machine | .45 | .004 | .371 | .092 | 000 | $.381^{***}$ |
| | [.145661] | | [.16618] | [.039137] | (.094) | (.104) |
| Has air conditioner | .071 | .311 | .062 | .045 | N/A | N/A |
| | [.005183] | | [.008311] | [.01408] | | |
| Utilities | 13 | 1 | 24 | 10 | 37 | 23 |
| Units of observation: utilities : column) in 2000 (range in bracl per customer was relatively lo Average electricity use, media were below minimum values for were below minimum values for display estimates of a differen (column 5) and in the North- (air conditioners not recorded Median household income grev 2010 (column 6). The commo that distributions don't overla consumption grew more in the attribute to the 2001–2002 con | as of 2000. Co kets; exchange w in Brazil: e a household in a household in t ce-in-different ce-in-different ce-in-different in 2010). Sig w more and p in 2010). Sig w more and p n-trend assum up at baseline. South than in servation prog | dumns (1)–(rate in 2000 lectricity wo come, and o e South, whi ce estimator 6) to utilit nificance le enetration r aption is cle Therefore, a the South- sram in this | (4) display averaging the set of the set | ges for several va Before the crisis, penetration rate f domestic applia constitute a suita constitute a suita les comparing u . Regressions in 6, ***1% (s.e. cl in the North-Ea in the North-Ea ler the North-Ea ver the 10-year I | riables (listed in residential electric s of domestic ap ble control group tilities in the Sou clude utility and tustered by utility st than in the So rds cannot be co ist in this paper. | the left-hand side ricity consumption pliances were low. In the North-East Columns (5)–(6) tth-East/Midwest year fixed effects year fixed effects y in parentheses). outh from 2000 to ntrolled for, given Mean electricity), a pattern that I |

Table 3: Long-term difference-in-difference results (2000-2010)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-------------------------|-------------|------------|---------------|--------------|-------------|-------------------|
| Dependent varia | able: Log(y | early aver | age resideı | ntial electr | icity consu | mption) |
| Treat \times Year2010 | 1195**** | 1197*** | 1172*** | 1223*** | 1136*** | 1217*** |
| | (.0262) | (.0308) | (.0327) | (.0305) | (.0326) | (.0442) |
| Log main | ~ / | 1915** | 1503 | · · · · | 1897* | `131 [´] |
| residential tariff | | (.0875) | (.1238) | | (.1083) | (.1409) |
| Log median | | .135 | $.3378^{***}$ | | .2367 | $.4917^{***}$ |
| household income | | (.1107) | (.1287) | | (.168) | (.178) |
| Restricted sample | No | No | No | Yes | Yes | Yes |
| Clusters | 37 | 37 | 37 | 30 | 30 | 30 |
| Other controls | No | No | Yes | No | No | Yes |

Monthly administrative data for utilities (as of 2000) from the South–East/Midwest and South, combined with data from the 2000 and 2010 censuses matched to the concession areas of each utility. Significance levels: *10%, **5%, ***1% (s.e. clustered by utility in parentheses). The table displays estimates of a 10–year difference–in–difference estimator (top row) for the logarithm of average residential electricity consumption comparing utilities in the South–East/Midwest (subject to the conservation program) and in the South. Regressions include utility and census–year fixed effects (2000 and 2010). Columns (4)–(6) restrict the sample to utilities with overlapping levels of consumption and income at baseline. Columns (1) and (4) do not include additional controls. Columns (2) and (5) control for the logarithm of the main residential tariff and the logarithm of median household income. Columns (3) and (6) control for the logarithm of population size, average household size, share urban, share employed, and share of housing units with bathrooms. Results are consistent across specifications; the temporary conservation program reduced average electricity use by .12 log point in the long run.

| | (1) | (2) | (3) |
|--------------------------------------|-------------|---------------------|-----------------|
| (in 1999) | Average | Average | Relative |
| | number | consumption | consumption |
| Lighting (incandescent) | 6.16 | 55.77 kWh | $2\bar{5}.10\%$ |
| Lighting (other) | 1.37 | $3.9 \mathrm{~kWh}$ | 1.76% |
| Refrigerator | 0.98 | 50.80 kWh | 22.87% |
| Freezer | 0.23 | 17.88 kWh | 8.05% |
| Electric shower | 0.62 | 42.97 kWh | 19.34% |
| Air conditioning | 0.35 | 30.45 kWh | 13.71% |
| TV | 1.51 | 20.39 kWh | 9.18% |
| Total | | 222.16 kWh | 100% |
| Simulated impact of different | conservatio | n behaviors | |
| a. CFLs adoption | | -12.14 kWh | -5.47% |
| b. CFLs adoption and reduce light | 50% | -35.91 kWh | -16.16% |
| c. Unplug 50% of freezers | 0 0 | -8.94 kWh | -4.03% |
| d. Reduce TV use by 50% | | -10.19 kWh | -4.59% |
| e. Reduce the use of electric showe | er by 50% | -21.48 kWh | -9.67% |
| f. Reduce air conditioning by 50% | - | -15.23 kWh | -6.85% |

Table 4: Electricity use by source for LIGHT customers

Electricity use by source for residential customers of LIGHT in 1999, from an engineering model developed to estimate load curves (personal communication with Professor Reinaldo Souza, PUC-Rio). The model includes seven sources of electricity consumption: incandescent light bulbs, other light bulbs, refrigerator, freezer, electric shower, air conditioner, and TV. Usage per day is assumed to be 40min for electric showers, 2h-5h for light bulbs (depending on their type/power), 2h for air conditioners (average over the year; 8h/day in the summer), and 5h for TVs. Lighting, refrigeration, and electric showers were the main sources of electricity use prior to the crisis. The model was revised in 2002 to incorporate new data on penetration rates of appliances and light bulbs. In the bottom panel, I use the model to simulate the impact of different conservation behaviors reported during the crisis. Holding constant other usages, the adoption of compact fluorescent light bulbs (CFLs), as measured in 2002, decreases electricity use by about 5.5%. Reducing lighting by half, in combination with CFL adoption, saves 16% of electricity use (row b). Unplugging half of freezers only saves about 4% (row c). Reducing TV use by half has a similar effect (row d); reducing the use of electric showers by half saves about 10% (row e). Reducing electricity use from air conditioning cannot explain the similarly large drop in consumption in winter months. Yet, it could have had a large effect in the summer. Reducing air conditioning by half saves 15 kWh on average and could thus save about 60 kWh in the summer (row f). These simulations show that households subject to the conservation program must have resorted to a series of severe behavioral adjustments to achieve the consumption levels observed during and after the crisis.

| | Mean | (1) Co | mnared to | (2) the South | | Z |
|---|--|---|--|--|---|--|
| Panel A: Conservation behaviors adopte | ed befor | e, during, | and aft | er the crisi | is (2005) | 1 |
| 1+ behavior adopted during crisis | .9405 | ou | t asked ir | the South | ~ | 2825 |
| 1+ new behavior adopted during vs. before crisis | .5129 | no | t asked ir | the South | | 2825 |
| 1+ behavior adopted after crisis | .8619 | ou | t asked ir | the South | | 2825 |
| 1+ new behavior adopted after vs. before crisis | .4867 | no | t asked ir | the South | | 2825 |
| Panel B: Appliances | , usage | intensity | in 2005 | <u>-</u> כ | | |
| At locat 1 conditioners used less than before crists | .388 | ou | t asked ir + agled iv | the South | | 2792 2702 |
| Freezer used less than before crisis | .3774 | | t asked it t asked ir | the South | | 530 530 |
| Panel C: Electricity-co | nsumin | g behavio | rs in 200 | 5 | | |
| Freezer used permanently (if owned) | .6723 | 2825*** | (.0233) | 2853^{***} | (.0425) | 986 |
| Share of appliances on standby when not in use | .3339 | 2161^{***} | (.0125) | 2418*** | (.0173) | 3674 |
| Share of light bulbs that are not CFLs | .6484 | $.3407^{***}$ | (.0155) | $.1745^{***}$ | (.0203) | 3701 |
| Household controls | | N_{O} | | Yes | | |
| Data from household surveys conducted in 2005 (PROCEL, conservation behaviors and whether households in the South- crisis. Panel B uses retrospective information about the use shower, washing machine, standby appliances, microwave, lig these appliances in 2005 less than they did before the crisis. In subject to the conservation program. Over 50% of households r the crisis. In 2005, households report having reduced usage of a (conditional on appliance ownership). About 70% of household on consumption behaviors in 2005 asked of every household, and in the South (not subject to the conservation program) for crisis: unplugging freezers, avoiding standby power use, adop categories (dummies). Column (2) adds controls for household he type, roof material, wall material, floor material, and the type (robust s.e. in parentheses; geographic information only ident the SouthEast, Widwest, in 2005, but CFT, nenetration rates we | , 2007). F East/Midd s of eight , of eight and formation for three or for three or for three or for three or for three or for three or for water a of water a actual | anel A sumi west adopted appliances (r d whether hd used in pane ing adopted a of their dome usage of at le s responses fi onservation s onservation s ag unit size a g unit size a ccess (dummi llv higher in: | narizes retu- such behaa efrigerator, puscholds in bis A and B is A and B is A and B is A and B is a conset strategies of trategies of trategies of trategies of trategies of trategies of trategies of the South the South | cospective infective infective infectives before, d air conditioned and the South-Fe avas collected of was collected of vation behavio creation behavio diliance. Panel d olds in the So olds in the So often mentioned or seven elected of bathrooms (sidence condit sidence condit cance levels: * | rmation al uuring, or a ar, freezer, last/Midwe ast/Midwe anly for hou or during a to before t to before t lin relation ling, **5% more prev | pout 14 fter the electric set used useholds and after the crisis rmation Midwest 1 to the umption usehold orhood orhood alent in |

Table 5. Household electricity conservation behaviors during and after the crisis

| t) (5) crisis Log postcrisis aption consumption nonths) (same 5 months) | 8*** .0699*** 71) / 0001) | (14) | 8*** | (.0288) | 293 18293 I | ss Yes |
|--|------------------------------|---------------------------|---------------|----------------|-------------|----------|
| (4) isis Log c otion consum onths) (first 5 r | ** .1568 | nedian) (. ^{.01} | ** .1618 | 7) (.02 | 3 182 | Ye |
| (3) trisis Log cri btion consump ths) (first 5 mo OLS | 5 .1653* | e regressions (r | $.1759^{*}$ | (.0247 | 3 1829: | No |
| 1) (2) quota Log prec consump (3 mont | 4***004 197) / 0193 | Quantile | 3^{***} 0 | 154) (.0155 | 293 1829 | Vo No |
| Log | quota of 1.08 | 0.) STAVOIII NAA | quota of .952 | eek movers (.0 | ons 18 | ~ |
| | Log mean | M_AIIIS | Log mean | same-w | Observati | Controls |

| $\operatorname{ers})$ |
|-----------------------|
| (move |
| quotas |
| in |
| variations |
| f exogenous |
| of |
| impact |
| The |
| 6: |
| Table |

economic incentives at the median (median quota = 250 kWh for movers whose quotas were based on the baseline period). For quantile regressions, standard errors are obtained by bootstrapping (100 replications). The average quota of same-week movers strongly predicts variations in quotas (column 1). I find no effect on average consumption levels prior to the crisis (column 2). I find only a small effect on electricity consumption in the first five months of the crisis, before any quota increase (columns 3 and 4). Specifications in column (4) include controls for neighborhood (dumnies; not for quantile regressions) and pre-crisis Sample of LIGHT customers from Rio de Janeiro whose first bill was sent between March 2000 and February 2001 (movers), observed continuously over a period of at least three years, whose average consumption in the three months prior to the crisis consumption (logs). Coefficient estimates imply that increasing quotas by 20% increased consumption by only 3% during the crisis. The effect on consumption levels in the same months in 2002 is much smaller (column 5). In later years, the effect in quotas entirely due to different moving dates in this sample (seasonality, see text). I thus instrument the quota of mover i by the average quota of movers (excluding i) who received their first bill in the same week as i. The table displays the coefficients from regressing several outcomes (listed above each column) on the logarithm of my instrument. Significance levels: *10%, **5%, ***1% (s.e. clustered by moving week in parentheses). I consider both OLS and median regressions. Fines are likely to constitute the only falls in the top quartile of the movers' consumption distribution. Quota assignment rules generated variation becomes even smaller (and noisier, not shown).

| Table | 7: | Parameter | estimates | and | counterfactual | simulations |
|--------|----|---------------|-------------|------|------------------|--------------|
| TOURIO | •• | T OILOUTIOUOL | 00011100000 | ouro | counteerideedaar | ominationomo |

| Estimation (indirect inference) | | | | | | | |
|---------------------------------|--|--|-----------------------|--|--|--|--|
| | Estimated pa | arameters | | | | | |
| a. | Difference <i>perceived</i> vs. actual crisis tariff (| $\overline{\ln\left(\widetilde{p}+p\right)} - \ln p$ | 1.233 (.102) | | | | |
| b. | Standard deviation of consumption (σ) | | .2 (.088) | | | | |
| с. | Propensity to consume electricity pre-crisis | $(\ln a_0)$ | 27.033 (.038) | | | | |
| d. | Propensity to consume electricity post–crisi | 26.465(.14) | | | | | |
| | <u>Fit of the model</u> | | | | | | |
| Estir | nation moments | Empirical | Predicted | | | | |
| e. | Median kWh pre–crisis | 311.63 | 311.63 | | | | |
| f. 1 | Median kWh crisis | 203.68 | 203.68 | | | | |
| g. | Median kWh post–crisis | 265.033 | | | | | |
| h. | Median kWh crisis if 20% quota increase | 209.71 | 209.697 | | | | |
| Out- | -of–sample moment | Empirical | Predicted | | | | |
| i.] | kWh crisis (panel of customers) | 196.06 | 190.026 | | | | |
| Sim | ulations: Predicted kWh crisis | | | | | | |
| j. ' | j. with lumpy adjustments but only economic incentives (kWh) 251.709 | | | | | | |
| Sim | ulations: to achieve crisis and post- | -crisis kWh with | out lumpy adjustments | | | | |
| k. | Difference necessary vs. perceived crisis tari | iff (log point) | .579 | | | | |
| l. 1 | Difference necessary vs. actual post–crisis ta | ariff (log point) | .567 | | | | |

In the top panel, I structurally estimate the model in Section 2 for movers whose quotas were based on the baseline period and were around 250 kWh. I use indirect inference techniques (Gouriéroux and Monfort, 1996) and minimize the distance between moments predicted by the model and empirical moments. The four parameters are the difference between the *perceived* $(p + \tilde{p})$ and the actual tariff (p) during the crisis, the degree of consumption uncertainty (σ), and the two parameters capturing the propensity to consume electricity (appliance stock, habits) before and after the crisis (a_0, a_1) . I assume that lumpy adjustments consistent with observed consumption levels after the crisis were made at the start of the crisis $(a_{crisis} = a_1)$. The first three moments are the median of the average consumption levels between June and October before (311.63 kWh in 2000), during (203.68 kWh in 2001), and after (265.06 kWh in 2002) the crisis. June to October corresponds to the first five months of the crisis in 2001. before any chance in quotas. The last moment is the median of the average consumption levels during the crisis if quotas had been increased by 20% (209.71 kWh) using a quota elasticity of .16 (Table 6). The model takes as inputs a value of η (-.2, the estimated price elasticity in Table 1) and values of the electricity tariffs before, during, and after the crisis (in real terms; R\$.187/kWh, R\$.208/kWh, and R\$.238/kWh, respectively). I use the inverse of the variance – covariance matrix of the empirical moments as weighting matrix \widehat{W} (estimated through 100 bootstraps). Asymptotic standard errors are obtained as in Gouriéroux and Monfort (1996). I provide more details in the Web Appendix. Social incentives (e.g., conservation appeals) appear to have played a major role during the crisis. The perceived tariff $\hat{p} + p$ is very large, 1.233 log points above the actual tariff p. The estimated degree of uncertainty $(\hat{\sigma} = .2)$ is close to the realized uncertainty estimated in Section 2. The parameterized model is able to closely predict the empirical moments used in the estimation (rows e-h). It slightly underestimates the median of the average consumption levels during the crisis for the panel of customers in Figure 3d (out-of-sample moment; row i).

In the bottom panel, I use the parameterized model for counterfactual simulations. If I turn off the social incentives $(\tilde{p} + p = p, a_{crisis} = \hat{a_1}; \text{ row j})$, consumption levels would have been 24% higher during the crisis. Customers may have been sensitive to the conservation appeals and voluntarily contributed to avoid blackouts and severe shortages. Players voluntarily (over-)contribute to avoid losing a public good in laboratory experiments, particularly if the loss is large (Iturbe-Ormaetxe et al., 2011). Appeals to social preferences may be particularly powerful in times of crisis (Mulligan, 1998). If I turn off the lumpy adjustments ($\tilde{p} + p = \tilde{p} + p, a_j = \hat{a_0}$; rows k and l), the tariffs should have been .58 log point (resp. .57 log point) higher than the perceived tariff (resp. the main tariff) to achieve the consumption levels observed during the crisis (resp. after the crisis). In our setting, the possibility of triggering lumpy adjustments thus reduces substantially the incentives necessary to achieve ambitious conservation targets.

A Appendix

A.1 Figures and Tables

Figure A.1: Conservation targets and necessary incentives with/without lumpy adjustments



The figure displays the necessary incentives to achieve given conservation targets in the model in Section 2 and illustrates customers' resulting utility levels. Without lumpy adjustments, incentives increase in the reduction target: $d\ln(p+s) = \frac{d\ln(q)}{\eta}$. When incentives reach some threshold, customers make lumpy adjustments and consumption drops discontinuously $(a_0 \rightarrow a_1)$. The possibility of triggering lumpy adjustments thus reduces the incentives (resp. utility cost) necessary to achieve ambitious conservation targets. The pattern of utility levels is only illustrative. I assume that incentives correspond to a tax and that there are no externalities from energy conservation on customers' utility levels. Utility levels have no units, given that any monotonic and increasing transformation would capture the same behaviors.

Figure A.2: Utilities' main electricity tariff relative to synthetic control (in logs)



The figure displays a synthetic control exercise, as in Figure 3a using the (demeaned) logarithm of the monthly main electricity tariff (in real terms) as the outcome. The distribution of synthetic control estimators for utilities in the South–East/Midwest (solid lines) is around 0, both in the short and the long run. Moreover, the distributions are similar for utilities in the South–East/Midwest and in the South (placebo, dashed lines).



Figure A.3: Income, price, and consumption before, during, and after the crisis

(c) Changes in electricity consumption and price

O

0

.'3

.2

0

0

IGH1

-.1

Ó

 South–East/Midwest (T) Before: June 2000-Feb 2001; After: June 2010-Feb 2011

Average kWh after vs before crisis .2 -.15 -.1 -.05 0 .05 .1

-.2





Each observation corresponds to a utility and its concession area. In 2000, the exchange rate was about R1.9\simeq US1 . Panels (a) and (b) display the pre-crisis relationship between average residential electricity consumption per customer and (a) average residential electricity prices or (b) median household income. There is some overlap in income, price, and consumption levels between utilities in the South-East/Midwest and the South. Panels (c) and (d) display the relationship between 10-year changes in average residential electricity consumption per customer after the crisis relative to before the crisis and (c) 10-year changes in the main residential electricity tariffs or (d) 10-year changes in median household income (in real terms). For similar changes in tariffs, consumption growth was lower in the South-East/Midwest. For similar income growth, consumption growth was lower in the South-East/Midwest. Therefore, the long-term effects of the conservation program are unlikely to be confounded by changes in tariffs or household income.



Figure A.4: Quantity and type of electric showers sold around the crisis

Data from Fame, one of the leading manufacturers of electric showers in Brazil. Electric showers are responsible for about 20% of residential electricity consumption in the South–East/Midwest and in the South, where their penetration rates are very high (PROCEL, 2007b). Panel (a) displays the monthly volume of sales in the South-East/Midwest and in the South. Panel (b) displays the mean power (in kWh) of the models sold each month. Mean power is higher in the South because of the colder weather. In early May 2001, the government announced that it would increase federal taxes on the sale of electric showers on May 21, particularly on the most electricity-intensive models. The increase was soon reversed, on June 27. Sales spiked right before the tax change, in both the South-East/Midwest and the South. The type of electric showers sold right before the tax change was no less electricity-intensive than in earlier months. In June 2001, when the tax increase was in place, sales returned to their usual levels. The average power of the electric showers sold dropped by more than 10% in both the South–East/Midwest and the South. During the crisis, sales levels were not particularly high or differentially higher in the South–East/Midwest. The average power of the model sold stayed lower in the South-East/Midwest by about 10%, revealing a moderate substitution away from more electricity-intensive models. Overall, because of the sales pattern, the "total power" (volume times power) sold in 2001 was similar to other years and there was no differential trend in the South-East/Midwest and the South. As a result, customers' purchases of electric showers cannot explain the short- and long-term impacts of the conservation program on electricity consumption.



Figure A.5: Consumption levels during and after the crisis (compared to the quota) (a) Post crisis 2002 (b) Post crisis 2005

Panels (a) and (b) display correlations between consumption levels during and after the crisis (2002 and 2005). The sample is similar to the sample of Figure 3d (balanced panel of randomly selected LIGHT customers, continuously observed from 2000 to 2005, with a quota around 250 kWh). Average consumption levels from June to October in each year (before any change in quota during the crisis —first five months) are normalized to the quota. There are clear correlations between relative consumption levels during and after the crisis.
| | | | (6) | | (6) | | 2014 TIOMA | |
|--|------------------|----------------------------|----------------------------|--------------|-----------------|------------------------------|------------------|----------------------------|
| | (1) | | (7) De | penden | t variable: | | (4) | |
| | Lo | g(month | nly average | residen | tial electri | city con | sumption) | |
| 1996 | .0067 | (.0126) | .0133 | (.0133) | .0005 | (.0134) | 0004 | (.015) |
| 1997 | 0.354^{***} | (.0081) | 0401^{***} | (0079) | $.0287^{***}$ | (.0093) | 0.283^{***} | (9600.) |
| 1998 | $.0443^{***}$ | (0089) | $.0443^{***}$ | (9600.) | $.0372^{***}$ | (.0102) | $.0324^{***}$ | (.0113) |
| 1999 | $.0254^{***}$ | (8700.) | 024^{***} | (0000) | $.0165^{\circ}$ | (.0094) | .0108 | (.0114) |
| 2000 | $.0193^{***}$ | (.0052) | $.0193^{***}$ | (.0059) | $.0143^{**}$ | (.0063) | $.0127^{*}$ | (.0075) |
| Crisis | 2552^{***} | (.0082) | 2486^{***} | (.0087) | 256^{***} | (6200.) | 2499^{***} | (.0082) |
| Rest of 2002 | 1707^{***} | (6600.) | 1721^{***} | (.0113) | 1716^{***} | (.0103) | 1737*** | (.0116) |
| 2003 | 1347^{***} | (.0088) | 1336^{***} | (.0104) | 1329^{***} | (1000.) | 1309^{***} | (.0112) |
| 2004 | 1373^{***} | (.0135) | 136*** | (.0157) | 1362^{***} | (.0147) | 1339^{***} | (.0167) |
| 2005 | 131*** | (.0145) | 1325^{***} | (.0168) | 1315^{***} | (.016) | 128*** | (.0176) |
| 2006 | 1224*** | (.0157) | 1231^{***} | (.018) | 1266^{***} | (.0171) | 1225^{***} | (.0185) |
| 2007 | 1168^{***} | (.0182) | 1221*** | (.021) | 1262*** | (.0191) | 1267*** | (.0206) |
| 2008 | 1096^{***} | (.0184) | 1137^{***} | (.0212) | 1207*** | (.0182) | 125*** | (.0186) |
| 2009 | 1107^{***} | (.0192) | 1141*** | (.0219) | 1188*** | (.0187) | 1226^{***} | (.0183) |
| 2010 | 1057^{***} | (.0187) | 1081*** | (.0212) | 1125^{***} | (.0197) | 1165*** | (.0193) |
| 2011 | 1148*** | (.018) | 1148*** | (.0205) | | | | |
| Log main residentis | al tariff | | | | 0738** | (.0334) | 1252*** | (.0368) |
| Log population size | - - | | | | .0451 | (.0819) | 0043 | (10077) |
| Log share formally | employed | | | | .188U. | (.0379) | 2860. | (.0488) |
| Log median formal | real wage | | | | 080 / | (.0119) | 048 | (susus) |
| Restricted sample | N_{O} | | $\mathbf{Y}_{\mathbf{es}}$ | | No | | Yes | |
| Observations | 6528 | | 5376 | | 6120 | | 5040 | |
| Clusters | 34 | | .58 | | 34 | | .58 | |
| Significance levels: *10% | 6, **5%, ***1% | (s.e. clust | ered by utility | in parenth | teses). Data fo | or utilities (| as of 1996) in . | the South– |
| East/Midwest and in th | le South from 1 | 996 to 201 | 1. Thế table c | lisplays coe | efficients from | regressing | the logarithm | of monthly |
| average electricity consu- 2001-2002 to isolate the | umption per cu | stomer for nteracted v | each utility or | n time-per | iod dummies | (yearly dun +he conseru | nmies, three du | ummies for during the |
| crisis (difference-in-diffe | erence estimato | rs in each | time period). | The referer | tes subject to | the courser v responds to | the first mont | hs of 2001. |
| Regressions include unit | iteracted time- | period dun | nmies, utility a | nd calenda | r month-per- | region fixed | effects. Colum | ns (2) and |
| (4) restrict the sample t | o utilities with | overlappin | g average cons | umption a | nd median inc | ome levels i | n 2000. Colum | (3) and |
| of each utility available | until 2010 (log | r residentia populatior | n, log share for | mally emp | loyed, log real | median for | rmal wage). Re | ession area esults from |
| Columns (3) and (4) are | e reproduced o | ı Figure 4. | The conservat | ion progra | m is estimate | d to have re | educed average | electricity |
| consumption by .25 log slowly. | point during ti | ne crisis an | [goi eil. ya bi | point in th | e long run. 1 | ne impact i | nas decayed at | most very |

| Table A.2: Differen | ce-in-differ | ence esti | mates for th | impac | ts of the co | nservatio | n program | (winter) |
|---|--|--|---|--|--|--|---|--|
| | (1) | | (2) De | nenden | (3) t. variable: | | (4) | |
| | Lc | og(month | uly average | residen | itial electri | city cons | sumption) | |
| 1996 | 01 | (.0116) | 0024 | (.0119) | 0125 | (.0116) | 0097 | (.0123) |
| 1997 | $.0209^{***}$ | (.0078) | $.0266^{***}$ | (.0087) | $.0182^{**}$ | (.0082) | $.0207^{**}$ | (.0088) |
| 1998 | $.0281^{***}$ | (.0081) | $.0269^{***}$ | (.0093) | $.0249^{***}$ | (.0084) | $.0207^{**}$ | (900.) |
| 1999 | .0059 | (.0081) | .0034 | (.0087) | 0009 | (.0088) | 0058 | (.0104) |
| Crisis | 2665^{***} | $(\overline{6}200.)$ | 2624^{***} | (8200.) | 2636^{***} | (.0074) | 258^{***} | (.0082) |
| 2002 | 1838*** | (1110) | 1872*** | (.0128) | 1816*** | (1119) | 1841*** | (.013) |
| 2003 | 1547^{***} | (6600.) | 1546^{***} | (.0115) | 15*** | (.011) | 1478^{***} | (.0123) |
| 2004 | 1664*** | (.0194) | 1683*** | (.0214) | 1618*** | (0.216) | 1608*** | (.0237) |
| 2002 | 1451*** | (.0162) | 1487*** | (.0183) | 1425^{***} | (0.185) | 139*** | (.02) |
| 2006 | 1319^{***} | (.0178) | 133*** | (.0201) | 1325^{***} | (.0198) | 1266*** | (.0213) |
| 2007 | 1326^{***} | (.0191) | 1376^{***} | (.022) | 139*** | (.0211) | 138*** | (.0226) |
| 2008 | 1221*** | (.0192) | 1296^{***} | (.0218) | 1308^{***} | (.0198) | 1369^{***} | (.0201) |
| 2009 | 126*** | (.0209) | 1301^{***} | (.0239) | 1305^{***} | (.0212) | 1325^{***} | (.0212) |
| 2010 | 126^{***} | (.02) | 1298*** | (.0229) | 1301^{***} | (.022) | 1335^{***} | (.0221) |
| 2011 | 1342*** | (.0196) | 1358^{***} | (.0232) | | | | |
| Log main residentia | ul tariff | | | | 077** | (.0325) | 1275^{***} | (.0349) |
| Log population size | | | | | .0513 | (.0842) | .0086 | (.0995) |
| Log share formally | employed | | | | $.0804^{*}$ | (.0428) | .0548 | (.0568) |
| Log median formal | real wage | | | | 0834 | (.0753) | 0444 | (.0845) |
| Restricted sample | No | | Yes | | No | | Yes | |
| Observations | 3230 | | 2660 | | 3026 | | 2492 | |
| Clusters | 34 | | 28 | | 34 | | 28 | |
| Significance levels: *10 ⁶ South-East/Midwest an coefficients from regressis period dummies (yearly subject to the conservati period corresponds to 2 ² dummies, utility and cal average consumption and electricity tariffs and mu | %, **5%, *** d in the South ng the logarit dummies —wi dummies —wi 000 (pre-crisis lendar month- 1 median incon | 1% (s.e. cl hm of month inter month uring the cr s months in per-region me levels in data match | ustered by ut i to 2011, rest ihly average el is were all in 20 isis (difference i 2001 were no fixed effects. 2000. Column ed to the conco | ility in parificted to v ectricity co 001 during →in-differe ot in winte Column (2 s (3) and (ssion area | rentheses). D vinter months onsumption pe the crisis) inte the crisis) int | ata for util (May–Octoo (May–Octoo r customer r r customer r aracted with i n each tin ns include 1 ns include 1 sample to the logarith available u | lities (as of 19 ber). The tah for each utilit an indicator ne period). Th uninteracted t utilities with a m of the main ntil 2010 (log | 996) in the ole displays by on time- for utilities for utilities ine-period overlapping residential |
| log share formally emple electricity consumption h in Table A.1 is due to th | yed, log real 1 yy .26 log poin te different refe | median form t during the erence perio | ial wage). The ecrisis and by id. | e conservat .135 log po | tion program i bint in the long | s estimated 5 run. This | to have reduc slightly larger | ced average effect than |

B Web Appendix

B.1 Timeline of the crisis

| 2001 | |
|-----------|---|
| March | Conservation measures inevitable. Still uncertain how to proceed. |
| April | Conservation program will begin June 1 st . General idea of the incen- |
| | tives. Random blackouts may be necessary too. |
| May | Economic incentives revealed: individual quotas, bonuses, fines, and |
| | threats of disconnection. Not sure if program will continue in 2002. |
| May/June | Letters with quotas sent to customers. Cuts only if repeatedly above |
| | quota (second chance) and small consumers not subject to power cuts. |
| July | Conservation program expected to last until February 2002 but may |
| | be suspended or modified earlier. |
| September | New bonus rule. Power cuts restricted to large over-users and not |
| | legal in the city of Rio de Janeiro. Very few power cuts in practice. |
| November | Situation in the reservoirs is improving. Conservation program should |
| | end between December and April. |
| December | New quotas based on consumption levels in the previous summer or |
| | on the initial quotas multiplied by an adjustment factor, whichever is |
| | higher. |
| 2002 | |
| January | New quota increase for February. Conservation program will stop at |
| | the end of February. Only bonuses will still apply in February–March |
| | bill. |
| February | Electricity crisis officially over. |

B.2 Data

I provide here additional information on the various datasets used in the paper.

A. ANEEL administrative data (I use utilities' names used in the ANEEL registries)

The "consumption" data comes from two registries at ANEEL. The first one spans a period from January 1991 to April 2005; the second one from January 2003 to (at least) December 2011 for every utility. I use the overlapping period to make the two series fully consistent. A very few wrong entries were corrected by hand (file available upon request). The data include seven fields: name of the utility, year, month, customer category, number of customers, total kWh charged, total revenues. There are eight customer categories: residential, commercial, industrial, rural, own consumption, public lighting, public services, government. Using these data, one can construct a measure of average monthly electricity consumption per customer (total kWh charged/number of customers) and average monthly

electricity price (total revenues/total kWh charged) by category. The data are publicly available but must be requested from ANEEL.

The "tariff" data are unique to this paper. The dates of every price setting regulation were provided by ANEEL. Legal documents for each regulation were then found in the online version of the Diário Oficial da União and information concerning the prevailing tariffs at each point in time was copied in spreadsheets. From 2005 onward, regulations also specify the "exogenous" cost-of-energy component for every tariff. The data include every tariff by customer category in each month. They were compared with the online database of electricity tariffs for residential customers available on ANEEL website since 2004 only.

These datasets were then matched to census data (2000 and 2010), yearly population estimates (IBGE, Instituto Brasileiro de Geografia e Estatística), and yearly formal employment records (RAIS, Relação Anual de Informações Sociais). ANEEL provided a list of all the municipalities within the concession area of every utility. Publicly available micro-data from the censuses identify municipalities. IBGE publishes population estimates by municipality every year. RAIS is a longitudinal matched employee-employer dataset covering by law the universe of formally employed workers, including public employees. RAIS is not publicly available for obvious confidentiality reasons. I used a version granted by the Labor Ministry to researchers at the Pontifícia Universidade Católica do Rio de Janeiro. Using RAIS, I construct a panel of formal employment and formal wages for every municipality from 1995 to 2010. Other survey data, such as the Household Expenditure Surveys (POF, Pesquisa de Orçamentos Familiares; 1996–1997, 2002–2003, and 2008–2009), the yearly National Household Surveys (PNAD, Pesquisa Nacional por Amostra de Domicílios), and the Monthly Labor Force Surveys (PME, Pesquisa Mensal de Emprego) do not identify municipalities and therefore cannot be matched to the concession area of every utility.

ANEEL also provided yearly data on distribution losses by utility. The data include six fields: name of the utility, year, electricity load, total distribution losses, "technical" distribution losses, "non-technical" distribution losses. "Non-technical" distribution losses are supposed to capture electricity theft (distribution losses unexplained by engineering estimates). The data is available until 2009 but utilities enter the dataset in different years. The data is very noisy.

I provide a list of all utilities in the data for the three main subsystems (the data also contain utilities from the North). A few concession areas were divided over time. When using utilities' definitions as in 1996 (resp. 2000) in the statistical analysis, I aggregate the data over the prevailing concession areas in 1996 (resp. 2000).

B. LIGHT billing data

Customer–level data was provided by LIGHT for every low–voltage customer in its concession area through five monthly registries from January 2000 to December 2005. For obvious confidentiality reasons, the data are not publicly available. They were made available for this specific research project.

The "client" registry includes ten fields: period identifier (year and month), client identifier, address identifier (2 fields), and six fields (strings) for the location of metered housing units, including fields identifying zip codes and more than 900 neighborhoods. The "reading" registry includes 12 fields: period identifier (year and month), invoice identifier, new reading, new reading date, previous reading, previous reading date, a coefficient to convert readings into kWh (depending on meter type), difference between the two readings in kWh, number of days between the two readings, average consumption between the two readings, connection type (monophasic, biphasic, triphasic), reading type (interior, exterior, estimated).

The "invoice" registry includes six fields: period identifier (year and month), invoice identifier, client type (e.g., residential main tariff, residential alternative tariff, commercial, rural), invoice creation date, client identifier, invoice value.

The "detailed invoice" registry includes every invoice component. It includes seven fields: period identifier (year and month), invoice identifier, a code identifying the specific invoice element (e.g., fine or bonus during the crisis), a code identifying the tariff/price category associated with the specific invoice element, the quantity (e.g., metered kWh), the prevailing price/tariff, and the value associated with the specific invoice element.

Finally, the "crisis" registry includes six fields during the months of the electricity crisis: period identifier (year and month), invoice identifier, the prevailing quota adjusted for the number of days between the two readings, the quota originally assigned to each customer, and two fields capturing any trade in quotas between industrial firms.

In June 2001, there were about 2,615,300 residential customers on the main tariff; 482,800 residential customers on the alternative tariff; 14,200 low-voltage industrial customers; 247,400 commercial customers; 9,500 rural customers; and 600 "public services" customers. *C. PROCEL surveys and other complementary data*

The questionnaire, sampling design, and aggregate statistics of the household surveys conducted by PROCEL in 2005 are provided in PROCEL (2007*a*). The micro–data are not publicly available but can be obtained for research purposes from PROCEL. Aggregate data on penetration rates by regions and imports of compact fluorescent lights bulbs were also obtained from PROCEL. Penetration rates are calculated from surveys conducted in 1997 and 2005. Import data come from http://aliceweb.desenvolvimento.gov.br/.

The time-series of sales of appliances come from Table 5.2 in Mascarenhas (2005). The author obtained these proprietary data from ELETROS, the National Association of Manufacturers of Appliances and Electronics, specifically for his research project. ELETROS includes every major brand of domestic appliances in Brazil since its creation in 1994. The same data were used in de Melo (2009). The data on sales of electric showers was obtained directly from FAME, a leading manufacturer in Brazil. The data include the quantity of each type of electric showers (models' power range from 3000MW to 7000MW) sold in a given month in a given state from 2000 to 2003.

Questionnaires and micro–data of the Brazilian Household Expenditure Surveys (POF, Pesquisa de Orçamentos Familiares; 1996–1997, 2002–2003, and 2008–2009), National Household Surveys (PNAD, Pesquisa Nacional por Amostra de Domicílios, yearly), and Censuses (Censos Demográphicos, 2000 and 2010) can be obtained on the IBGE website. These surveys are representative of the overall Brazilian population, with the exception of the early POF surveys. The 1996–1997 POF surveys were only conducted in the official metropolitan

| Name | Consumption data from | Tariff data from | Loss data from | Notes |
|-----------|--------------------------|---------------------|-------------------|-----------------------------|
| AES SUL | 04/1997 | 04/1997 | 1997 | Previously in CEEE |
| CEEE | 01/1991 | 11/1995 | 1998 | |
| CELESC | 01/1991 | 11/1995 | 1997 | |
| CFLO | 01/1991 | 11/1995 | 1997 | |
| COCEL | 01/1991 | 11/1995 | 1997 | |
| COPEL | 01/1991 | 11/1995 | 2002 | |
| EFLUL | 01/1991 | 11/1995 | N/A | No data on technical losses |
| ELETROCAR | 01/1991 | 11/1995 | 2009 | |
| FORCEL | 01/1991 | 12/1995 | N/A | No data on losses |
| HIDROPAN | 01/1991 | 11/1995 | 1997 | |
| IENERGIA | 01/1991 | 11/1995 | 1997 | |
| RGE | 08/1997 | 08/1997 | 1997 | Previously in CEEE |
| UHENPAL | 01/1991 | 12/1995 | 1997 | |

List of utilities in the South

List of utilities in the North–East

| Name | Consumption | Tariff | Loss | Notes |
|---------|-------------|-----------|-----------|-------|
| | data from | data from | data from | |
| CEAL | 01/1991 | 11/1995 | 1997 | |
| CELPE | 01/1991 | 11/1995 | 1997 | |
| CEMAR | 01/1991 | 11/1995 | 1997 | |
| CEPISA | 01/1991 | 12/1995 | 1998 | |
| COELBA | 01/1991 | 11/1995 | 1997 | |
| COELCE | 01/1991 | 11/1995 | 1997 | |
| COSERN | 01/1991 | 11/1995 | 1997 | |
| EBO | 01/1991 | 12/1995 | 2003 | |
| EPB | 01/1991 | 11/1995 | 2003 | |
| ESE | 01/1991 | 11/1995 | 2003 | |
| SULGIPE | 01/1991 | 11/1995 | 1997 | |

areas in Brazil at the time (only two cities in the South, for instance). The POF surveys (i) only ask for the purchase of light bulbs in the previous week and therefore capture very few such purchases, (ii) ask for the year of purchase of every major domestic appliance owned at the time of the interview.

B.3 Structural estimation

I structurally estimate the parameters $\phi = (\tilde{p}, \sigma, a_0, a_1)$ of the model in Section 2 for the 308 movers whose quotas were based on the baseline period and were around 250 kWh. The quota elasticity has only been estimated for movers. Fines were the only economic incentives for customers with quotas around 250 kWh. Focusing on customers with the same quota allows me to model a single nonlinear schedule of economic incentives.

| Name | Consumption | Tariff | Loss | Notes |
|---------------|-------------|-----------|-----------|-------------------------------|
| | data from | data from | data from | |
| AMPLA | 01/1991 | 11/1995 | 1997 | |
| BANDEIRANTE | 01/1998 | 01/1998 | 2001 | Previously in ELETROPAULO |
| BRAGANTINA | 01/1991 | 11/1995 | 1997 | |
| CAIUA | 01/1991 | 11/1995 | 1997 | |
| CEB | 01/1991 | 11/1995 | 1997 | |
| CELG | 01/1991 | 11/1995 | 1997 | |
| CEMAT | 01/1991 | 01/1996 | 1997 | |
| CEMIG | 01/1991 | 11/1995 | 2005 | |
| CJE | 01/1991 | 11/1995 | 2001 | |
| CPEE | 01/1991 | 11/1995 | 2001 | |
| CPFL PAULISTA | 01/1991 | 12/1995 | 2001 | |
| CSPE | 01/1991 | 11/1995 | 2001 | |
| DME-PC | 01/1991 | 11/1995 | N/A | No data on technical losses |
| ELEKTRO | 06/1998 | 06/1998 | 2002 | |
| ELETROPAULO | 01/1991 | 12/1995 | 1999 | |
| EMG | 01/1991 | 11/1995 | 2003 | |
| ENERSUL | 01/1991 | 11/1995 | 1997 | |
| ENF | 01/1991 | 11/1995 | 2003 | |
| ESCELSA | 01/1991 | 12/1995 | 1997 | |
| EVP | 01/1991 | 11/1995 | 1997 | |
| LIGHT | 01/1991 | 11/1995 | 1997 | Loss data from LIGHT directly |
| MOCOCA | 01/1991 | 11/1995 | 2001 | |
| NACIONAL | 01/1991 | 11/1995 | 1997 | |
| PIRATININGA | 10/2001 | 10/2001 | 2002 | Previously in BANDEIRANTE |
| SANTACRUZ | 01/1991 | 12/1995 | 2001 | |
| SANTAMARIA | 01/1991 | 12/1995 | 1997 | |

List of utilities in the South–East/Midwest

The estimator minimizes the distance between the moments predicted by the model $\mu(\phi)$ and the empirical moments m (Gouriéroux and Monfort, 1996). The first three moments are the median of the average consumption levels between June and October before (311.63 kWh in 2000), during (203.68 kWh in 2001), and after the crisis (265.06 kWh in 2002). The last moment is the median of the average consumption levels between June and October during the crisis if quotas had been increased by 20% (209.71 kWh) using a quota elasticity of .16 (Table 6).

I assume that customers choose expected monthly consumption \overline{q} to maximize:

$$U(\overline{q}) = a_j \frac{\overline{q}^{1+1/\eta}}{1+1/\eta} + W - \int CC(q, p+\widetilde{p}) f(q|\overline{q}), \text{ with } q \sim \mathcal{N}(\overline{q}, \sigma \overline{q})$$

where $a_j = a_0$ before the crisis and $a_j = a_1$ during or after the crisis. Out-of-crisis, the cost of electricity is linear: CC(q, p) = pq, for main electricity tariff p. During the crisis, CC(.)

is the known nonlinear schedule of economic incentives:

$$CC(q,p) = \begin{cases} pq, & q \le quota \\ pq + .5p (q - 200), & quota < q \le 500 \\ pq + .5p (500 - 200) + 2p (q - 500), & q > 500 \end{cases}$$

The model takes as inputs a value of η (-.2, the estimated price elasticity in Table 1) and values of the electricity tariffs (in real terms; R\$.187/kWh, R\$.208/kWh, and R\$.238/kWh, before, during, and after the crisis, respectively). In the estimation routine, customers form expectations on CC(.) from 100,000 draws of $\mathcal{N}(\bar{q}, \sigma \bar{q})$.

Maximizing the utility function for a given set of parameters, I obtain values of \overline{q} for four cases: before the crisis, after the crisis, during the crisis for a quota of 250 kWh, and during the crisis for a quota of 300 kWh. I then simulate five values (five months) of realized monthly consumption and average them for each of the 308 customers. The vector of predicted moments $\mu_s(\phi)$ is the median of these average consumption levels among the 308 customers for each of the four cases. I perform 10,000 simulations s. I obtain an estimator of ϕ by minimizing:

$$\left(m - \frac{1}{s}\mu_s(\phi)\right)'\widehat{W}\left(m - \frac{1}{s}\mu_s(\phi)\right)$$

I use the inverse of the variance–covariance matrix of the empirical moments as weighting matrix \widehat{W} (estimated through 100 bootstraps). Estimates are obtained using the Nelder–Mead algorithm in Matlab. To provide initial values for the algorithm, I first perform an extended grid search over possible values of \widetilde{p} and σ . Initial values of a_0 and a_1 are directly obtained from the first–order conditions: $\ln q = \eta \ln (p) - \eta \ln a_i$.

The fit of the model is first evaluated by comparing empirical m and predicted moments: $\frac{1}{s}\mu_s(\hat{\phi})$. I also assess whether the parameterized model accurately predicts an out-of-sample empirical moment. I use the median of the average consumption levels between June and October during the crisis for customers for the same balanced panel as in Figure 3d (196.06 kWh). The median of the average consumption levels between June and October after the crisis for these customers was 242.335 kWh. I obtain a value of $a_{1,out}$ for these customers from: $\ln 242.335 = \eta \ln (p) - \eta \ln a_{1,out}$. I obtain the predicted out-of-sample moment by maximizing the utility function during the crisis for $(\tilde{p}, \sigma, a_1) = (\hat{p}, \hat{\sigma}, a_{1,out})$ and a quota of 250 kWh.

Asymptotic standard errors are obtained from the variance–covariance matrix

$$\left(\widehat{G}'\widehat{W}\widehat{G}\right)^{-1}\widehat{G}'\widehat{W}\widehat{V}\widehat{W}\widehat{G}\left(\widehat{G}'\widehat{W}\widehat{G}\right)^{-1}$$

where $\widehat{G} = \frac{\left(m - \frac{1}{s}\mu_s(\widehat{\phi} - h)\right) - \left(m - \frac{1}{s}\mu_s(\widehat{\phi})\right)}{h}$ and \widehat{V} is the variance–covariance matrix of the empirical moments. I use $h = .05\widehat{\phi}$.

Finally, in Table B.8, I estimate alternatives to the above model. I follow exactly the same procedure, assuming that there are no social incentives $\tilde{p} = 0$. In the top panel, I estimate a crisis-specific value of η : $\eta_{crisis} > \eta$. In the bottom panel, I assume that customers overestimated the economic cost of exceeding their quotas. I assume that the nonlinear schedule of economic incentives during the crisis takes the form:

$$CC(q,p) = \begin{cases} pq, & q \le quota \\ pq + .5p (q - 200) + penalty, & quota < q \le 500 \\ pq + .5p (500 - 200) + 2p (q - 500) + penalty, & q > 500 \end{cases}$$

I estimate the value of this penalty. Because a_0 and a_1 are identified by first-order conditions out-of-crisis, their estimates are similar in Table 7 and in both panels in Table B.8.

B.4 Figures and Tables



Figure B.1: Regions and states of Brazil

The National Interconnected System includes the following subsystems:

- South subsystem: South region. Not subject to the conservation program.
- South–East/Midwest subsystem: South–East and Midwest regions with the exception of a few isolated customers in Mato Grosso and Mato Grosso do Sul. Subject to the conservation program from June 2001 until February 2002.
- North-East subsystem: North-East region with the exception of parts of Maranhão. Subject to the conservation program from June 2001 until February 2002.
- North subsystem: Northern states of Para and Tocantins, and parts of the state of Maranhão, with the exception of a few isolated systems. Subject to a conservation program from August 2001 until December 2001. Northern utilities' many customers served by isolated electricity systems were not subject to any conservation measures. Because the data do not differentiate utilities' residential consumption from "isolated" and "connected" customers, I do not consider Northern utilities.

Customers in other states (mostly in the Amazonia) are served by isolated systems.



Figure B.2: Level of the hydro–reservoirs in the South–East/Midwest and in the South

Data from ONS. The figures display the evolution of hydro-reservoirs' capacity in the South-East/Midwest (panel a) and in the South (panel b; dotted lines indicate January in each year). In panel (a), there is a clear seasonal pattern, with rainfall replenishing reservoirs in the summer. In the summer of 2000–2001, rainfall was exceptionally unfavorable in the South-East/Midwest, leading to dangerously low reservoir levels. Over a period of 20 years, the rainfall pattern in the summer of 2000–2001 was a unique outlier. In the South, generous rainfall in 2000 eliminated any risk of shortage. The conservation program was implemented in the South-East/Midwest from June 2001 to February 2002 (solid lines). The situation of the reservoirs was stable in the South-East/Midwest but more variable in the South after the crisis. The risk of new shortages was thus not smaller in the South.

Figure B.3: Main assignment rule for quotas of LIGHT residential customers



The baseline was defined as the average billed monthly consumption from May to July 2000. Quotas were set at 80% of the baseline for most customers with three exceptions: (i) customers with a baseline below 100 kWh had their quotas set at 100% of baseline; (ii) customers with a baseline above 100 kWh but quotas below 100 kWh with the 80% rule had their quotas set at 100 kWh; (iii) because quotas were based on billed consumption and bills always charge minimum consumption levels in Rio de Janeiro (30 kWh, 50 kWh, and 100 kWh for monophasic, biphasic, and triphasic connections, respectively), quotas were at least equal to these minimum levels.



Figure B.4: Variation behind the price elasticity estimates in Table 1

Monthly administrative data for utilities (as of 2002) from the North–East, South–East/Midwest, and South from 2003 to 2011. The graph displays the correlation between the logarithm of the yearly mean of average residential electricity consumption and the logarithm of the yearly mean of the main residential tariff (residuals). It presents graphically the variation behind the price elasticity estimates in column (1) in Table 1. Residuals are obtained from first regressing each variable on year–by–region and utility fixed effects.



Figure B.5: Consumption compared to quota (averaged by baseline consumption levels)

Panels (a) and (b) display the average electricity consumption during the first five months of the crisis (a) and the same months in 2002 (b) compared to quotas, as a function of *baseline* consumption levels. Data from the same balanced panel of LIGHT customers as in Figure 3c. I restrict attention to customers whose quotas were set at 80% of baseline. Consumption was more than 15% below quota or 32% below baseline during the crisis at every baseline level. Larger consumers at baseline reduced consumption by more than 25% below their quotas or 40% below baseline. After the crisis, average consumption was still below quota for all but the smallest consumption categories. A similar analysis with a placebo sample (as if the crisis happened in 2004) reveals that mean reversion cannot explain most of the decreasing slope (available upon request).

Figure B.6: Distribution of appliances' years of acquisition (aggregated)

(a) Share of households who bought at least one domestic appliance in a given year (conditional on ownership)

(b) Share of households who bought more than one domestic appliance in a given year (conditional on ownership)



Data from the 2003 and 2009 Brazilian expenditure surveys (POF). Respondents who report owning a given appliance at the time of the interview also report the year of acquisition. This allows me to look for unusual patterns in the purchase of appliances around the crisis. The figures estimate the share of households who bought at least one (panel a) or more than one (panel b) of the domestic appliances owned at the time of the interview in each year. I use sampling weights for estimates to be representative of the overall population. I only consider appliances associated with larger electricity consumption in the 2003 surveys. The appliances considered are refrigerator, washing machine, air conditioner, dishwasher, dryer, freezer, fan, color TV, and microwave. There is no evidence of a (differential) increase in purchases around the electricity crisis. Respondents are more likely to report having bought their current appliances in 2000 than in 2001. Households in the South–East/Midwest were actually less likely to have bought appliances in 2001–2002 than households in the South. However, this difference is small and in no way an outlier. The higher share of households having bought more than one appliance after 2004 in the South may be due to a differential increase in household income (see Table 2).



Figure B.7: Distribution of appliances' years of acquisition (disaggregated)

35



(d) Fan

(b) Washing machine

(c) Air conditioner, dishwasher, dryer, freezer



Data from the 2003 and 2009 Brazilian expenditure surveys (POF). Respondents who report owning a given appliance at the time of the interview also report the year of acquisition. The figures estimate the share of households who bought a given type of appliance owned at the time of the interview in each year. I use sampling weights for estimates to be representative of the overall population. I only consider appliances associated with larger electricity consumption in the 2003 surveys. Appliances with low ownership rates have been pooled. Ownership rates in 2003 in the South–East/Midwest were: refrigerator, 93%; washing machine, 44.3%; air conditioner, 7.8%; dishwasher, 4.6%; dryer, 4.1%; freezer, 16.3%; fan, 61%; color TV, 90%; microwave, 24.6%; mixer, 40.5%. In every case, respondents are more likely to report having bought their current appliances in 2000 than in 2001. For none of the appliances considered is there any strong evidence of a (differential) increase in purchases around the electricity crisis.



Figure B.8: Discontinuous effect of consuming above the quota in September 2001

Sample of LIGHT customers from Rio de Janeiro (i) with quotas above 225 kWh (only subject to fines), (ii) who are observed consuming at least 15% below their quotas in the first two months of the crisis, and (iii) who are consuming between 10% below and 10% above their quotas in the September bill (third month of the crisis). To test the hypothesis that customers overestimated the cost of exceeding the quota, one would ideally compare the behaviors of customers who received different feedback on the actual cost of non-compliance. Panels (a)-(d) display the result of a related exercise. The idea behind the sample selection is to have customers who reduced consumption severely at the start of the crisis, maybe because they overestimated the cost of not complying with the quota, but for some reason consumed closer to their quotas in September. Customers consuming just above the quota (right of the vertical line) were then fined and potentially learned the actual cost of non-compliance. I aggregate customers by bins of 4 kWh of electricity consumption in September 2001 compared to the quota (forcing variable). The distribution of consumption levels (panel a) and the distribution of quota levels (panel b) are smooth around the quota in September. Customers who consumed just below or just above the quota in September were similarly consuming below their quotas in the first two months of the crisis (panel c). However, in the two months after September (before quotas were extended), customers who received a fine in September apparently responded by further reducing consumption (panel d). This result suggests that these customers were not overestimating the cost of non-compliance before they actually received a fine (assuming away income effects). It is not straightforward to generalize the result because the sample above is very selected: these customers likely consumed closer to their quotas in September for non-exogenous reasons. Indeed their conservation efforts were smaller after September than before.

| Table B.1: Descriptive statis | stics in 2 | 000 (additional) and 1 | 2010 | |
|---|---------------------|---------------------------|---------------------|-------------------|
| | $^{(1)}_{ m South}$ | (2) South-East/Midwest | $^{(3)}_{ m LIGHT}$ | (4) North-East |
| Cens | us data 20 | 000 | | |
| Has bathroom | .907 | .954 | .974 | .689 |
| Share urban | .7958 | .8618 | .9913 | .7312 |
| Average household size | 3.458 | 3.508 | 3.29 | 4.113 |
| Average number of rooms | 6.164 | 5.82 | 5.473 | 5.619 |
| Has computer | .1006 | .1089 | .1793 | .0453 |
| Has TV | .581 | .578 | .479 | .618 |
| Share employed | .6929 | .6611 | .6135 | .5773 |
| Share formally employed | .31 | .2972 | .3038 | .1672 |
| Share with agricultural job | .1259 | .1016 | .0049 | .142 |
| ANEEL adm | inistrative | $e \ data \ 2010$ | | |
| Average residential electricity use (kWh) | 167 | 169.8 | 203.3 | 106 |
| Average residential electricity price (R\$/kWh) | .1683 | .1658 | .1593 | .1486 |
| Main residential electricity tariff (R\$) | .1793 | .1785 | .1668 | .1776 |
| Cens | us data 20 | 010 | | |
| Median household income (R\$) | 896 | 850.4 | 865.2 | 473.2 |
| Share urban | .827 | .8857 | 9000. | .759 |
| Average household size | 3.056 | 3.102 | 2.985 | 3.516 |
| Average number of rooms | 6.213 | 5.874 | 5.423 | 5.735 |
| Has bathroom | .9803 | .9912 | .9941 | .8802 |
| Has electricity | .9964 | .9968 | .9994 | .9828 |
| Has TV | 7076. | .9681 | .986 | .9341 |
| Has refrigerator | .981 | .9744 | .9853 | .8759 |
| Has washing machine | .6497 | .5477 | .705 | .198 |
| Has computer | .4439 | .4344 | .5304 | .2216 |
| Share employed | .7498 | .7233 | .6746 | .575 |
| Share formally employed | .4777 | .4547 | .444 | .2624 |
| Share with agricultural job | .102 | .0844 | .0046 | .1209 |
| Utilities | 13 | 1 | 24 | 10 |
| | | | | |

Units of observation: utilities as of 2000. Columns (1)-(4) display averages for several variables (listed in the left-hand side column) in 2000 and 2010. Price and income levels are in R\$ of 2000. The exchange rate in 2000 was about R\$1.9 \simeq US\$1. Shares employed, formally employed, and with an agricultural job are calculated for individuals 18–55 years old.

| | (1) | | (2) | | (3) | | (4) | |
|--------------|---------------|------------|-------------------|-----------------|--------|---------|--------|---------|
| | Average re | esidential | Tot | al | Tech | nical | Non-te | chnical |
| | consumpti | on (logs) | \mathbf{losses} | (logs) | losses | (logs) | losses | (logs) |
| 1998 | $.0385^{***}$ | (.0132) | 0247 | $(.\bar{1}356)$ | .0391 | (.1351) | 1624 | (.293) |
| 1999 | .0118 | (.0082) | 0346 | (.136) | .0517 | (.1829) | 2485* | (.1405) |
| 2001 | 1486*** | (.0075) | 1718 | (.1856) | 1829 | (.1641) | 1764 | (.3962) |
| 2002 | 1913*** | (.0148) | 0857 | (.15) | 1309 | (.1355) | 0357 | (.3722) |
| 2003 | 1482*** | (.0147) | 129 | (.1248) | 1961* | (.1077) | 0727 | (.3781) |
| 2004 | 1588*** | (.0216) | 0843 | (.2167) | 1396 | (.1847) | 0783 | (.4843) |
| 2005 | 1536*** | (.0301) | 1769 | (.1514) | 1855 | (.1556) | 2047 | (.3551) |
| 2006 | 1471*** | (.0321) | 1807* | (.1081) | 152 | (.125) | 2934 | (.3374) |
| 2007 | 1358*** | (.0344) | 2285** | (.1064) | 2067* | (.115) | 3628 | (.2615) |
| 2008 | 1288*** | (.035) | 253* | (.1396) | 2304 | (.1486) | 34 | (.2895) |
| 2009 | 1224*** | (.0363) | 156 | (.1318) | 1424 | (.1447) | 0199 | (.4596) |
| Clusters | 21 | | 21 | | 21 | | 21 | |
| Observations | 252 | | 252 | | 252 | | 252 | |

Table B.2: Difference-in-difference results for reported distribution losses

Significance levels: *10%, **5%, ***1% (s.e. clustered by utility in parentheses). Data for utilities in the South–East/Midwest (12) and in the South (9) reporting technical and non–technical losses since 1998. Yearly data from 1998 to 2009. Distribution losses are the share of the load not charged to particular customers. Distribution losses are divided into technical (engineering estimates) and non–technical (residual, including theft) losses. It is unclear how companies separately identify the two categories and the resulting information is noisy. The table displays coefficients from regressing several outcomes (listed above each column) on year dummies interacted with an indicator for utilities subject to the conservation program during the crisis (difference–in–difference estimators in every year). The reference year corresponds to 2000. Regressions include uninteracted year dummies and utility fixed effects. Column (1) replicates results from Table A.1 at the yearly level for this sample of utilities (consumption was slightly higher in 1998 in Table A.1). The long–term effects on average residential electricity consumption are very similar. Columns (2)–(4) use the data on losses. Results are very noisy. I find large persistent reductions in total and technical losses (mostly non–significant; columns 2 and 3). This may be mechanical if engineering losses are proportional to load. Estimates for non–technical losses are inconclusive (column 4). They are noisy and vary widely from year to year.

| | (1) | | (2) | | (3) | |
|--------------|---------|---------|---------|--------------------------|---------|-----------|
| | Refrig | erator | Free | $\mathbf{e}\mathbf{zer}$ | Washing | g machine |
| 1995 | 007 | (.0169) | 0102 | (.0103) | 0016 | (.0341) |
| 1996 | 0055 | (.0109) | .0047 | (.0063) | 0049 | (.0194) |
| 1997 | 0032 | (.0068) | .0005 | (.008) | 0041 | (.0236) |
| 1998 | 0017 | (.0038) | .0019 | (.0077) | 0032 | (.0108) |
| 2001 | .0014 | (.0083) | 0109 | (.0094) | 0183* | (.0107) |
| 2002 | 0049 | (.0114) | 0188 | (.012) | 0118 | (.013) |
| 2003 | 0008 | (.0116) | 0241* | (.0138) | .0077 | (.0148) |
| 2004 | .0003 | (.0131) | 0215 | (.0179) | 0225 | (.0161) |
| 2005 | 0048 | (.0137) | 02 | (.0174) | 0183 | (.0153) |
| 2006 | 0072 | (.0167) | 0227 | (.021) | 0133 | (.0171) |
| 2007 | 0049 | (.0181) | 0088 | (.0211) | .0064 | (.0189) |
| 2008 | .0001 | (.0205) | 0119 | (.0215) | 0205 | (.0152) |
| 2009 | 0027 | (.0215) | 0186 | (.0255) | 0411* | (.021) |
| Observations | 882,774 | | 882,741 | | 882,785 | |
| Clusters | 26 | | 26 | | 26 | |

Table B.3: Difference-in-difference estimates on appliance ownership

Data from the National Household Surveys (PNAD, conducted in September each year), which are representative of the Brazilian population, restricted to the South–East/Midwest and the South from 1995 until 2009. Significance levels: *10%, **5%, ***1% (s.e. clustered by state–area in parentheses; areas can be of three types: metropolitan, other urban, rural). Surveys were not conducted in 2000. Ownership rates of refrigerators, freezers, and washing machines in 1999 were 91.6% (resp. 91.6%), 20.9% (resp. 35%), and 41.3% (resp. 46.2%) in the South–East/Midwest (resp. in the South). The table displays coefficients from regressing an indicator for whether a household owns a refrigerator, a freezer, or a washing machine on year dummies interacted with an indicator for states subject to the conservation program during the crisis (difference–in–difference estimators in every year). The reference year corresponds to 1999. Regressions include uninteracted year dummies and state and area type fixed effects. Observations are weighted by the survey weights. Results indicate no clear differential trend in appliance ownership after the crisis. There may be a small effect for freezers or washing machines, but never statistically significant at conventional levels (we have a limited number of clusters).

| | | (1) | (2) | (3) | (4) | |
|-------------------------|----------------------------|---------------|---------------|-----------------|------------------|------|
| Appliance | Consumption | Use as before | Use lèss than | Disconnected or | Substituted with | Z |
| | category | Crisis | before crisis | disposed of | more efficient | |
| Refrigerator | $< 200 \ \mathrm{kWh}$ | .87 | .11 | 0 | .02 | 1901 |
| | $\geq 200 \text{ kWh}$ | 6. | .08 | 0 | .02 | 809 |
| Air | $< 200 \ \mathrm{kWh}$ | .25 | .67 | .08 | 0 | 62 |
| conditioning | $\geq 200 kWh$ | .31 | .65 | .02 | .02 | 133 |
| Freezer | $< 200 \ \mathrm{kWh}$ | .51 | .27 | .21 | 0 | 193 |
| | $> 200 \ \mathrm{kWh}$ | .68 | .19 | .12 | .01 | 337 |
| Electric | $\leq 200 \ { m kWh}$ | .54 | .44 | .01 | 0 | 1767 |
| shower | $\geq 200 kWh$ | .62 | .37 | .01 | 0 | 747 |
| Washing | $< 200 \ { m kWh}$ | .37 | .62 | .01 | 0 | 1259 |
| machine | $\geq 200 kWh$ | .49 | ਹ | .01 | .01 | 723 |
| Standby | $\leq 200 \ \mathrm{kWh}$ | .55 | .38 | .07 | 0 | 1367 |
| appliances | $\geq 200 kWh$ | .7 | .26 | .04 | 0 | 676 |
| Microwave | $< 200 \ \mathrm{kWh}$ | .47 | .5 | .03 | 0 | 425 |
| | $\geq 200 \; \mathrm{kWh}$ | .6 | .39 | .02 | 0 | 458 |
| Lighting | $< 200 \ \mathrm{kWh}$ | .47 | .46 | 0 | .07 | 1928 |
|) | $\geq 200 \; \mathrm{kWh}$ | .46 | .42 | 0 | .11 | 807 |

electricity consumption categories. These questions were only asked of households in areas subject to the conservation program during the electricity crisis. I report share of responses, conditional on ownership of the appliance. In most cases, respondents report using their appliances less than before the crisis (except for refrigerators, which is reassuring given that we don't expect much flexibility in refrigerator usage). Few report replacing appliances with more efficient products (however, households could during or after the crisis, or (4) substituted a more efficient appliance during or after the crisis. I separate households into two only provide one response for each appliance).

| | | (1) | (2) | (3) | |
|--|------------------------|-----|------|-----|------|
| | Category | Âĺl | Some | No | Obs. |
| Did you substitute CFLs for incandescent | < 200 kWh | .29 | .1 | .61 | 1901 |
| light bulbs during the crisis? | $\geq 200 \text{ kWh}$ | .44 | .16 | .39 | 791 |
| Did you keep using | < 200 kWh | .66 | .09 | .25 | 708 |
| the CFLs afterwards? | $\geq 200 \text{ kWh}$ | .72 | .08 | .2 | 455 |

Table B.5: Adoption of more efficient light bulbs around the crisis (South-East/Midwest)

Data from household surveys conducted by PROCEL in 2005. I use retrospective information about the adoption of compact fluorescent light bulbs (CFLs) during and after the crisis. I separate households into two electricity consumption categories. These questions were only asked of households in areas subject to the conservation program during the electricity crisis. I report share of responses. Many households substituted CFLs for incandescent light bulbs during the crisis and persisted in doing so.

Table B.6: Adoption of conservation behaviors around the crisis (South-East/Midwest)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|-------------------------|-------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| | < 200 |) kWh/m | onth | ≥ 200 |) kWh/m | onth |
| | Before | During | After | Before | During | After |
| | crisis | crisis | crisis | crisis | crisis | crisis |
| Turn off lights when away | .79 | .91 | .84 | .8 | .94 | .86 |
| for more than half an hour | | | | | | |
| Open refrigerator/freezer | .51 | .75 | .63 | .51 | .75 | .61 |
| fewer times | | | | | | |
| Do not keep warm food | .58 | .79 | .71 | .62 | .83 | .72 |
| in refrigerator/freezer | | | | | | |
| Reduce shower time when | .45 | .69 | .68 | .45 | .71 | .66 |
| using electric shower | | | | | | |
| Use summer vs. winter setup | .46 | .63 | .62 | .52 | .66 | .63 |
| for electric shower | | | | | | |
| Use washing machine and dishwasher | .29 | .44 | .44 | .41 | .6 | .58 |
| at full capacity | | | | | | |
| Accumulate clothes | .39 | .57 | .56 | .47 | .67 | .64 |
| to iron | | | | | | |
| Switch off air conditioner when | .02 | .03 | .02 | .06 | .08 | .07 |
| away for more than half an hour | | | | | | |
| Turn off electronic devices not in use | .51 | .64 | .59 | .54 | .66 | .61 |
| for more than half an hour | | | | | | |
| Observations | 1996 | 1996 | 1996 | 829 | 829 | 829 |

Data from household surveys conducted by PROCEL in 2005. I use retrospective information on the adoption of specific conservation behaviors before, during, or after the crisis (in 2005). I separate households into two electricity consumption categories. These questions were only asked of households in areas subject to the conservation program during the electricity crisis. I report unconditional adoption shares. Nine out of the 14 conservation behaviors are reported in the table. The other conservation behaviors are (a) do not dry clothes behind refrigerator/freezer, (b) periodically verify if the rubber seal of the refrigerator is in good condition, (c) do air conditioner maintenance, (d) consider natural ventilation and lighting when buying, renting, remodeling, or building a housing unit, (e) explain to household members and/or house employees how to best use energy to avoid waste. In all cases, the share of households adopting a particular behavior was higher during and after the crisis.

Table B.7: Standby power use after the crisis (South–East/Midwest vs. South)

| | | (1) | | (2) | | |
|--------------------|-----------|------------|----------|--------------|-------------|------|
| Dependent | variable: | Appliance | on stan | dby when 1 | not in use? | ? |
| | Mean | Difference | South-Ea | ast/Midwest | vs. South | Ν |
| TV | .5307 | 1771*** | (.0174) | 2058*** | (.0245) | 3592 |
| Air conditioner | .1429 | 2834*** | (.0478) | 3387*** | (.0557) | 373 |
| Sound system | .3939 | 2075*** | (.0207) | 3238*** | (.0287) | 2531 |
| Radio | .1449 | 0728** | (.0363) | 0873* | (.0451) | 1164 |
| Video | .362 | 1816*** | (.0334) | 1833*** | (.0478) | 1162 |
| DVD | .4688 | 2894*** | (.0307) | 3815*** | (.0465) | 979 |
| Computer | .2339 | 1707*** | (.0355) | 1489*** | (.0487) | 835 |
| Printer | .2179 | 0109 | (.0413) | 0021 | (.059) | 493 |
| Microwave | .2106 | 3386*** | (.0295) | 2837*** | (.0398) | 1236 |
| Electric oven | .0221 | 0063 | (.026) | 0373 | (.0463) | 302 |
| Ceiling fan | .0582 | .0435*** | (.012) | $.0656^{**}$ | (.0303) | 916 |
| TV subscription be | ox .6667 | 0123 | (.0382) | .0454 | (.073) | 763 |
| Household controls | 3 | No | | Yes | | |

Data from household surveys conducted by PROCEL in 2005. Significance levels: *10%, **5%, ***1% (robust s.e. in parentheses; geographic information only identifies regions). The table displays the coefficient from regressing an indicator for whether, in 2005, households reported leaving each appliance (listed on the left–hand side column) on standby when not using it (conditional on ownership) on an indicator for households living in areas subject to the conservation program during the crisis (simple difference between households in the South–East/Midwest and in the South). Column (1) controls for seven electricity consumption categories (dummies). Column (2) adds controls for household size, housing tenure, number of bathrooms (linear), household earnings categories, gender and education of the household head, housing unit size and type, residence condition, neighborhood type, roof material, wall material, floor material, and the type of water access (dummies). In most cases, households are more likely to avoid wasting standby electricity in the South–East/Midwest. The only exception is for ceiling fans; usage of fans is associated with hot weather and temperatures are higher in the South–East/Midwest.

| Estimation (indirect inference): crisis-specific price responsiveness η_{crisis} | | | | | |
|---|--|--|--|--|--|
| | Estimated paran | neters | | | |
| a. | Crisis–specific price responsiveness (η_{crisis}) |) | 95(.024) | | |
| b. | 5. Standard deviation of consumption (σ) | | .456 (.089) | | |
| c. | Propensity to consume electricity pre–crisis $(\ln a_0)$ | | 27.032(.039) | | |
| d. | Propensity to consume electricity post-crisis $(\ln a_1)$ | | 26.469(.136) | | |
| | <u>Fit of the model</u> | | | | |
| Esti | mation moments | Empirical | Predicted | | |
| e. | Median kWh pre–crisis | 311.63 | 311.833 | | |
| f. | Median kWh crisis | 203.68 | 203.538 | | |
| g. | Median kWh post–crisis | 265.06 | 265.498 | | |
| h. | Median kWh crisis if 20% quota increase | 209.71 | 209.512 | | |
| Out | -of-sample moment | Empirical | Predicted | | |
| i. | kWh crisis (panel of customers) | 196.055 | 192.202 | | |
| Estimation (indirect inference): <i>perceived</i> penalty to exceed quota | | | | | |
| LSU. | mation (multect merence): perceived | i penany i | o exceeu quota | | |
| ESU. | Estimated paran | neters | o exceeu quota | | |
| a. | Extra perceived penalty for exceeding the | $\frac{1}{\text{penalty t}}$ | 125.39 (27.57) | | |
| a. b. | Extra perceived penalty for exceeding the Standard deviation of consumption (σ) | $\frac{1}{\text{penalty t}}$ | 125.39 (27.57) .438 (.05) | | |
| а. b. c. | Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi | $\frac{penalty}{peters}$ $\frac{quota}{quota} (R$)$ $s (\ln a_0)$ | 125.39 (27.57) .438 (.05) 27.029 (.038) | | |
| a. b. c. d. | Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri | $\frac{a \text{ penalty t}}{\text{quota }} (\text{R}^{\$})$ $\frac{(\ln a_0)}{(\ln a_1)}$ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) | | |
| a. b. c. d. | Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri Fit of the mod | $\frac{a \text{ penalty t}}{\text{quota (R$)}}$ $\frac{a \text{ s}}{(\ln a_0)}$ $\frac{a \text{ s}}{(\ln a_1)}$ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) | | |
| a. b. c. d. Esti | Estimated param Estimated param Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri <u>Fit of the mon</u> mation moments | $\begin{array}{l} \begin{array}{l} \begin{array}{c} \text{penalty t} \\ \hline neters \\ \hline quota \ (R\$) \\ \end{array} \\ \text{s } (\ln a_0) \\ \hline \text{sis } (\ln a_1) \\ \hline \frac{del}{} \\ \end{array} \end{array}$ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) Predicted | | |
| a. b. c. d. Esti e. | Estimated param Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri <u>Fit of the mon</u> Median kWh pre-crisis | $\begin{array}{c} \begin{array}{c} \text{penalty t} \\ \begin{array}{c} \text{neters} \\ \hline \text{quota} & (\text{R}\$) \\ \text{s} & (\ln a_0) \\ \begin{array}{c} \text{sis} & (\ln a_1) \\ \hline \begin{array}{c} \text{del} \\ \hline \end{array} \\ \hline \end{array} \\ \begin{array}{c} \text{Empirical} \\ 311.63 \end{array} \end{array}$ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) Predicted 311.586 | | |
| a. b. c. d. Esti e. f. | Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri <u>Fit of the mom</u> Median kWh pre-crisis Median kWh crisis | $\begin{array}{c} \text{penalty t}\\ \begin{array}{l} \text{neters}\\ \hline \text{quota} (\text{R}\$)\\ \text{s} (\ln a_0)\\ \text{sis} (\ln a_1)\\ \hline \underline{\text{del}}\\ \hline \text{Empirical}\\ 311.63\\ 203.68 \end{array}$ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) Predicted 311.586 203.925 | | |
| a. b. c. d. Esti e. f. g. | Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri mation moments Median kWh pre-crisis Median kWh post-crisis | $\begin{array}{c} \text{penalty t}\\ \begin{array}{l} \text{neters}\\ \hline \text{quota} \ (\text{R}\$)\\ \text{s} \ (\ln a_0)\\ \text{sis} \ (\ln a_1)\\ \hline \underline{\text{del}}\\ \hline \\ \text{Empirical}\\ 311.63\\ 203.68\\ 265.06 \end{array}$ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) Predicted 311.586 203.925 265.261 | | |
| a. b. c. d. Esti e. f. g. h. | Estimated param Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri <u>Fit of the mon</u> mation moments Median kWh pre-crisis Median kWh post-crisis Median kWh crisis if 20% quota increase | $\begin{array}{c} \text{penalty t} \\ \begin{array}{l} \text{neters} \\ \hline \text{quota} \ (\text{R}\$) \\ \text{s} \ (\ln a_0) \\ \text{sis} \ (\ln a_1) \\ \hline \underline{\text{del}} \\ \hline \\ \end{array} \\ \begin{array}{c} \text{Empirical} \\ 311.63 \\ 203.68 \\ 265.06 \\ 209.7092 \end{array}$ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) Predicted 311.586 203.925 265.261 211.716 | | |
| a. b. c. d. Esti e. f. g. h. Out | Extra perceived penalty for exceeding the Standard deviation of consumption (σ) Propensity to consume electricity pre-crisi Propensity to consume electricity post-cri mation moments Median kWh pre-crisis Median kWh crisis Median kWh crisis if 20% quota increase -of-sample moment | $\begin{array}{c} \begin{array}{c} \begin{array}{c} \begin{array}{c} \begin{array}{c} \begin{array}{c} \begin{array}{c} \begin{array}{c} $ | 125.39 (27.57) .438 (.05) 27.029 (.038) 26.467 (.138) Predicted 311.586 203.925 265.261 211.716 Predicted | | |

 Table B.8: Parameter estimates for alternative models

I structurally estimate alternatives to the model in Section 2 for movers whose quotas were based on the baseline period and were around 250 kWh. I use indirect inference techniques (Gouriéroux and Monfort, 1996) and minimize the distance between the moments predicted by the model and the same empirical moments as in Table 7. Instead of estimating a price–equivalent for the social incentives \tilde{p} as in Table 7, I assume that there are no social incentives and estimate a potentially higher price responsiveness during the crisis (η_{crisis} ; top panel) or a *perceived* penalty for exceeding the quota, had customers overestimated the cost of exceeding the quota (bottom panel). Details on the estimation strategy are provided in the Appendix (asymptotic standard errors in parentheses). Rationalizing the same empirical moments requires a value of η_{crisis} fivefold larger than the estimated elasticity out–of–crisis. This is far outside the range of estimates in the literature, especially given that there was little use of air conditioning or electric heating at the time. Rationalizing the same empirical moments requires a penalty for exceeding the quota 24 times higher than the actual economic cost. This is a very large degree of misunderstanding, even if customers were loss averse. Finally, the estimated degree of uncertainty σ in these alternative models is unrealistically high (at .4–.5). This is more than twice the realized degree of uncertainty estimated in Section 2.



| ENERGIA ATIVA Número Medición Medição Atual Data Medição Anterior Data Teitura 03/01/2002 Const. Data Const. Medición Const. | N° Media Diana kWh Fanor de Potencia 360 30 12.00 Fanor de Potencia Ita Fiscal Media Diana kWh Fanor de Potencia NTIDADE PREÇO UNIT RS VALOR RS 360 0.34402 123.84 0.17201 27.52 0.00 |
|--|---|
| IRESIDENCIAL TRIFÁSICO DESCRIÇÃO UNIDADE FORNECIMENTO DE ENERGIA ELÉTRICA kWh SOBRETAXA - CONSUMO 201 A 500KWH kWh BÔNUS - CONSUMO ABAIXO DA META MÊS ANTERIOR Image: Consumo abaixo da meta mês anterior | NTIDADE PREÇO UNIT RS VALOR RS 360 0.34402 123.84 0.17201 27.52 0.00 |
| FORNECIMENTO DE ENERGIA ELÉTRICA kWh SOBRETAXA - CONSUMO 201 A 500KWH kWh BÔNUS - CONSUMO ABAIXO DA META MÊS ANTERIOR | 360 0.34402 · 123.84 0.17201 27.52 0.00 |
| | |
| ICMS RS ISS RS PAGANDO ATÉ O V Base de Cálculo 151, 36 - Corte no fornecimento Aliquota 251 - Cobrança de custos de Valor Ijá incluído no preço) 37, 84 - Cobrança de custos de Consultas sobre tarifas, tributos e condições de fornecimento | VENCIMENTO VOCÊ EVITA: o de energia ça na sua proxima conta) e serviços previstos pela ANEEL (Resolução 456/2000) |
| poderão ser feitas nas Agências ou pelo Disque-Light. Visite a Agência Virtual: www.light.com.br 18/02/2002 | 2 *******151,36 |
| AWh FEV2001 HAR2001 ABR2001 HAI2001 JUL2001 AG02001 SET200 450 360 360 335 90 11,16 90 11,16 11,16 11,83 13,00 10,80 3,56 2,10 450 2,10 | 01 OUT2001 NOV2001 DEZ2001 JAN2002 FEV2002 |

| /EJA COMO | REDUZIR O | CONSUMO | DE ENERGIA |
|-----------|------------------|---------|-------------------|
| | | | |

| APARELHO | DICAS PARA ECONOMIZAR |
|-----------------------------|--|
| Geladeira | Evite abrir a porta muitas vezes e guardar alimentos e líquidos quentes. |
| Chuveiro elétrico | Reduza o tempo de uso do banho para 6 minutos, não esquecendo de desligar o chuveiro ao ensaboar-se. |
| Ferro elétrico | Acumule a roupa e estabelecer dias para passá-las, evitando deixar o ferro ligado mais de 1h por dia. |
| Ventilador de teto | Só ligue quando estiver no ambiente. |
| Aparelho de ar-condicionado | Reduza a potência e diminua em, pelo menos, 1 hora por dia de uso. |
| Aparelho de som | Evite ligar quando já tiver acionando outro aparelho. |
| Lâmpadas incandescentes | Substitua por lâmpadas fluorescentes. São 80% mais econômicas e durar 10 vezes mais. |
| Televisão | Desligue quando ninguém estiver assistindo. |
| Aspirador de pó | Use, no máximo, dois dias na semana. O aspirador consome o mesmo que 1 aparelho de ar-condicionado. |
| Torradeira elétrica | Retire da tomada após o uso. |
| Microondas | Use suportes que permitam esquentar mais de um prato. |
| Microcomputador | Desligue o computador quando não estiver em uso. |
| Lavadora de roupas | Acumule a roupa e estabeleça dois dias na semana para lavá-la. |
| Freezer | Faça uma lista diária de tudo o que precisa e retire os congelados de uma única vez. |

A Light com você para enfrentar o desafio do racionamento

SAIBA COMO REDUZIR O CONSUMO DE ENERGIA ELÉTRICA

CHUVEIRO ELÉTRICO (uso diário) - usar menos 5 minutos por dia 1 LÂMPADA - trocar por 1 fluorescente TELEVISÃO (uso diário) - usar menos 1 hora por dia LIQUIDIFICADOR (uso diário) - usar menos 5 minutos por dia

LAVADORA DE ROUPAS (2 x por semana) - usar menos 1 hora por dia MICROONDAS (uso diário) - usar menos 5 minutos por dia VENTILADOR (uso diário) - usar menos 1 hora por dia AR CONDICIONADO (7.500 BTU's) - usar menos 1 hora por dia

3 TELEVISÕES (uso diário) - usar menos 1 hora por dia cada uma 2 APARELHOS DE SOM (uso diário) - usar 1 hora por dia cada um COMPUTADOR (5 dias por semana) - usar menos 1 hora por dia LAVADORA DE ROUPAS (2 x por semana) - usar menos 1 hora por dia LAVA LOUÇAS (uso diário) - usar menos 1 hora por dia AR CONDICIONADO (7,500 BTU's) - usar menos 1 hora por dia MICROONDAS (uso diário) - usar menos 10 minutos por dia CAFETEIRA (uso diário) - usar menos 10 minutos por dia SECADOR DE CABELO (uso diário) - usar menos 10 minutos por dia ASPIRADOR DE PÓ (2 x na semana) - usar menos 10 minutos por dia LÂMPADAS (uso diário) - usar menos 3 horas por dia 2 LÂMPADAS (uso diário) - usar menos 3 horas por dia 2 LÂMPADAS COMUNS - trocar por duas fluorescentes VENTILADOR - usar menos 2 horas por dia

A Light está trabalhando muito para que o desconforto de racionamento de energia seja o menor possível. Esse é o papel e a responsabilidade de uma empresa de serviço público: estar sempre a seu lado.



References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program." *Journal of the American Statistical Association*, 105(490): 493–505.

Acemoglu, Daron, and James Robinson. 2008. "Persistence of Power, Elites, and Institutions." *American Economic Review*, 98(1): 267–293.

Acemoglu, Daron, Philippe Aghion, Leonardo Bursztyn, and David Hemous. 2012. "The Environment and Directed Technical Change." *American Economic Review*, 102(1): 131–166.

Acevedo, German, Patricio Eskenazi, and Carmen Pagés. 2006. "Unemployment Insurance in Chile: A New Model of Income Support for Unemployed Workers." *Social Protection Discussion Paper, The World Bank.*

Aghion, Philippe, Antoine Dechezleprêtre, David Hemous, Ralf Martin, and John Van Reenen. 2012. "Carbon Taxes, Path Dependency and Directed Technical Change: Evidence from the Auto Industry." *NBER Working Paper*, 18596.

Alatas, Vivia, Abhijit Banerjee, Rema Hanna, Benjamin Olken, and Julia Tobias. 2012. "Targeting the Poor: Evidence from a Field Experiment in Indonesia." *American Economic Review*, 102(4): 1206–1240.

Alderman, Harold. 2002. "Do Local Officials Know Something We Don't? Decentralization of Targeted TTransfer in Albania." *Journal of Public Economics*, 83(2): 375–404.

Allcott, Hunt. 2011. "Social Norms and Energy Conservation." *Journal of Public Economics*, 95(9-10): 1082–1095.

Allcott, Hunt, and Michael Greenstone. 2012. "Is There an Energy Efficiency Gap?" Journal of Economic Perspectives, 26(1): 3–28.

Allcott, Hunt, and Todd Rogers. 2012. "The Short–Run and Long–Run Effects of Behavioral Interventions: Experimental Evidence from Energy Conservation." *NBER Working Paper*, 18492.

Almeida, Rita, and Pedro Carneiro. 2012. "Enforcement of Labor Regulation and Informality." *American Economic Journal: Applied Economics*, 4(3): 64–89.

Amadeo, Edward, and José Camargo. 1996. "Instituições e o Mercado de Trabalho Brasileiro." *Flexibilidade do mercado de trabalho no Brasil, edited by José Camargo.*

Andreoni, James. 2006. "Philanthropy." Handbook of the Economics of Giving, Altruism, and Reciprocity, 2: 1201–1269.

ANEEL. 2005. "Tarifas de fornecimento de energia elétrica." Agência Nacional de Energia Elétrica, Cadernos Temáticos ANEEL, 4.

Ashenfelter, Orley, David Ashmore, and Olivier Deschênes. 2005. "Do Unemployment Insurance Recipients Actively Seek Work? Evidence From Randomized Trials in Four US States." *Journal of Econometrics*, 125: 53–75.

Aterido, Reyes, Mary Hallward-Driemeier, and Carmen Pagés. 2011. "Does Expanding Health Insurance beyond Formal–Sector Workers Encourage Informality? Measuring the Impact of Mexico's Seguro Popular." World Bank Policy Research Working Paper, 5785.

Ayres, Ian, Sophie Raseman, and Alice Shih. 2009. "Evidence from Two Large Field Experiments That Peer Comparison Feedback Can Reduce Residential Energy Usage." *NBER Working Paper*, 15386.

Azuara, Oliver, and Ioana Marinescu. 2011. "Informality and the Expansion of Social Protection Programs: Evidence from Mexico." *Mimeo, University of Chicago*.

Baily, Martin. 1978. "Some Aspects of Optimal Unemployment Insurance." *Journal of Public Economics*, 10: 379–402.

Bardelin, Cesar. 2004. "Os efeitos do racionamento de energia elétrica ocorrido no Brasil em 2001 e 2002 com ênfase no consumo de energia elétrica." *Dissertação de Mestrado em Sistemas de Potência, Universidade de São Paulo: Escola Politécnica.*

Bastos, Paulo, Lucio Castro, Julian Cristia, and Carlos Scartascini. 2011. "Does Energy Consumption Respond to Price Shocks? Evidence from a Regression–Discontinuity Design." *IDB Working Paper Series*, 234.

Becker, Gary, and Kevin Murphy. 1988. "A Theory of Rational Addiction." *Journal of Political Economy*, 96(4): 675–700.

Bérgolo, Marcelo, and Guillermo Cruces. 2010. "Labor Informality and the Incentive Effects of Social Security: Evidence from a Health Reform in Uruguay." *CEDLAS Working Paper*.

Black, Dan, Jeffrey Smith, Mark Berger, and Brett Noel. 2003. "Is The Threat Of Reemployment Services More Effective Than The Services Themselves? Evidence From Random Assignment In The UI System." *American Economic Review*, 93(4): 1313–1327.

Blanchard, Olivier, and Jean Tirole. 2006. "The Joint Design of Unemployment Insurance and Employment Protection: A First Pass." *Journal of the European Economic Association*, 6(1): 45–77.

Borenstein, Severin. 2009. "To What Electricity Price Do Consumers Respond? Residential Demand Elasticity Under Increasing-Block Pricing." *Mimeo, Energy Institute, University of California at Berkeley.*

Bosch, Mariano, and Raymundo Campos-Vasquez. 2010. "The Trade–Offs in the Labor Market of Social Assistance Programs: the Case of the Seguro Popular Program in Mexico." *Mimeo*.

Bosch, Mariano, and Willam Maloney. 2010. "Comparative Analysis of Labor Market Dynamics Using Markov Processes: An Application to Informality." *Labour Economics*, 17(4): 621–631.

Botelho, Fernando, and Vladimir Ponczek. 2011. "Segmentation in the Brazilian Labor Market." *Economic Development and Cultural Change*, 59(2): 437–463.

Camacho, Adriana, and Emily Conover. 2011. "Manipulation of Social Program Eligibility." *American Economic Journal: Economic Policy*, 3: 41–64.

Camacho, Ariana, Emily Conover, and Alejandro Hoyos. 2009. "Effects of Colombia's Social Protection System on Workers' Choice between Formal and Informal Employment." *Cede Working Paper*, 2009–18.

Campos-Vazquez, Raymundo, and Melissa Knox. 2008. "Social Protection Programs and Employment: the Case of Mexicos Seguro Popular Program." *Mimeo*.

Card, David, and Phillip Levine. 2000. "Extended Benefits and the Duration of UI Spells: Evidence from the New Jersey Extended Benefit Program." *Journal of Public Economics*, 78: 107–138.

Card, David, Raj Chetty, and Andrea Weber. 2007*a*. "Cash–on–Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market." *Quarterly Journal of Economics*, 122(4): 1511–1560.

Card, David, Raj Chetty, and Andrea Weber. 2007b. "The Spike at Benefit Exhaustion: Leaving the Unemployment System or Starting a New Job?" American Economic Review, 97(2): 113–118.

Charness, Gary, and Uri Gneezy. 2009. "Incentives to Exercise." Econometrica, 77(3): 909–931.

Chetty, Raj. 2006. "A General Formula for the Optimal Level of Social Insurance." *Journal of Public Economics*, 90: 1879–1901.

Chetty, Raj. 2008. "Moral Hazard versus Liquidity and Optimal Unemployment Insurance." Journal of Political Economy, 116(2): 173–234.

Chetty, Raj, and Adam Looney. 2006. "Consumption Smoothing and the Welfare Consequences of Social Insurance in Developing Economics." *Journal of Public Economics*, 90: 2351–2356.

Chetty, Raj, and Adam Looney. 2007. "Income Risk and the Benefits of Social Insurance: Evidence from Indonesia and the United States." Fiscal Policy and Management in East Asia. NBER East Asia Seminar on Economics 16 (eds. T. Ito and A. Rose): Chicago, University of Chicago Press.

Chetty, Raj, and Adam Szeidl. 2007. "Consumption Commitments and Risk Preferences." *Quarterly Journal of Economics*, 122(2): 831–877.

Chetty, Raj, and Amy Finkelstein. 2012. "Social Insurance: Connecting Theory to Data." *Handbook* of *Public Economics*, 5 (forthcoming).

Chetty, Raj, John Friedman, Tore Olsen, and Luigi Pistaferri. 2011. "Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records." *Quarterly Journal of Economics*, 126(2): 749–804.

Costa, Francisco. 2012. "Just Do It: Temporary Restrictions and New Consumption Habits." *Mimeo, London School of Economics.*

Cunningham, Wendy. 2000. "Unemployment Insurance in Brazil: Unemployment Duration, Wages, and Sectoral Choice." *Mimeo, The World Bank.*

Davis, Lucas, and Lutz Kilian. 2011. "The Allocative Cost of Price Ceilings in the US Residential Market for Natural Gas." *Journal of Political Economy*, 119(2): 212–241.

DellaVigna, Stefano, John List, and Ulrike Malmendier. 2012. "Testing for Altruism and Social Pressure in Charitable Giving." *Quarterly Journal of Economics*, 127(1): 1–56.

de Melo, Conrado. 2009. "Padrões de Eficiência Energética Para Equipamentos Elétricos de Uso Residencial." *PhD Thesis, Department of Mechanical Engineering, Universidade Estadual de Campinas.*

Faruqui, Ahmad, and Sanem Sergici. 2010. "Household Response to Dynamic Pricing of Electricity: A Survey of 15 Experiments." *Journal of Regulatory Economics*, 38(2): 193–225.

Ferraz, Claudio, and Frederico Finan. 2011. "Electoral Accountability and Corruption: Evidence from the Audits of Local Governments." *American Economic Review*, 101(4): 1274–1311.

Fields, Gary. 1975. "Rural–Urban Migration, Urban Unemployment and Underemployment, and Job Search Activity in LDCs." *Journal of Development Economics*, 2(2): 165–187.

Gasparini, Leonardo, Francisco Haimovich, and Sergio Olivieri. 2009. "Labor Informality Bias of a Poverty–Alleviation Program in Argentina." *Journal of Applied Economics*, 12(2): 181–205.

Gerard, François. 2012. "Energy Crisis Management, Temporary Incentives, Long-Term Effects: Evidence from the 2001 Brazilian Electricity Crisis." Available at SSRN: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2097195.

Gerard, François. 2013. "What Changes Energy Consumption, and for How Long? New Evidence from the 2001 Brazilian Electricity Crisis." *RFF Discussion Paper 13–06, E3 Working Paper WP-050.*

Gerard, François, and Gustavo Gonzaga. 2013*a*. "Informal Labor and the Cost of Social Programs: Evidence from 15 Years of Unemployment Insurance in Brazil." *Phd Thesis, Department of Economics, University of California, Berkeley.*

Gerard, François, and Gustavo Gonzaga. 2013b. "Job–Search Monitoring in a Context of High Informality." Phd Thesis, Department of Economics, University of California, Berkeley.

Ghisi, Enedirr, Samuel Gosch, and Roberto Lamberts. 2007. "Electricity End–Uses in the Residential Sector of Brazil." *Energy Policy*, 35(8): 4107–4120.

Gonzaga, Gustavo. 2003. "Labor Turnover and Labor Legislation in Brazil." *Economía: Journal of the Latin American and Caribbean Economic Association*, 4(1): 165–207.

Gordon, Roger, and Wei Li. 2009. "Tax Structure in Developing Countries: Many Puzzles and a Possible Explanation." *Journal of Public Economics*, 93 (7-8): 855–866.

Gouriéroux, Christian, and Alain Monfort. 1996. "Simulation–Based Econometric Methods." Oxford University Press.

Guimarães, Nathália. 2004. "Regiões Metropolitanas: Aspectos Juridicos." Mimeo.

Hamermesh, Daniel. 1979. "Entitlement Effects, Unemployment Insurance and Employment Decisions." *Economic Inquiry*, 17(3): 317–332.

Harris, John, and Michael Todaro. 1970. "Migration, Unemployment and Development: a Two-Sector Analysis." *American Economic Review*, 60: 126–142.

Hijzen, Alexander. 2011. "The Moral–Hazard and Liquidity Effects of Unemployment Compensation in Brazil: Evidence and Policy Implications." *Mimeo OECD*.

IADB. in progress. "Protecting Workers against Unemployment in Latin America and the Caribbean." *Inter-American Development Bank*.

Ito, Koichiro. 2012a. "Do Consumers Respond to Marginal or Average Price? Evidence from Nonlinear Electricity Pricing." *Working Paper, Energy Institute @ Haas*, 210R.

Ito, Koichiro. 2012b. "Does Conservation Targeting Work? Evidence from a Statewide Electricity Rebate Program in California." *Mimeo, Stanford University*.

Iturbe-Ormaetxe, Iñigo, Giovanni Ponti, Josefa Tomás, and Luis Ubeda. 2011. "Framing Effects in Public Goods: Prospect Theory and Experimental Evidence." *Games and Economic Behavior*, 72(2): 439–447.

Katz, Lawrence, and Bruce Meyer. 1990. "The Impact of the Potential Duration of Unemployment Benefits on the Duration of Unemployment." *Journal of Public Economics*, 41: 45–72.

Kelman, Jerson. 2001. "Desequilíbrio entre Oferta e Demanda de Energia Elétrica." Relatório da Comissão de Análise do Sistema Hidrotérmico de Energia Elétrica.

Kleven, Henrik, and Mazhar Waseem. 2012. "Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan." Working Paper, London School of Economics.

Kleven, Henrik, Claus Kreiner, and Emmanuel Saez. 2009. "Why Can Modern Government Tax So Much? An Agency Model of Firms as Fiscal Intermediaries." *NBER Working Paper*, 15218.

Kroft, Kory. 2008. "Takeup, Social Multipliers and Optimal Social Insurance." *Journal of Public Economics*, 92: 722–737.

Landais, Camille. 2012. "Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design." *Mimeo, London School of Economics.*

Landais, Camille, Pascal Michaillat, and Emmanuel Saez. 2010. "Optimal Unemployment Insurance over the Business Cycle." *NBER Working Paper*, 16526.

Leighty, Wayne, and Alan Meier. 2011. "Accelerated Electricity Conservation in Juneau, Alaska: A Study of Household Activities that Reduced Demand 25%." *Energy Policy*, 39.

Levy, Santiago. 2008. "Good Intentions, Bad Outcomes: Social Policy, Informality and Economics Growth in Mexico." *Brookings Institution Press*, 357pp.

Lutzenhiser, Loren. 2002. "An Exploratory Analysis of Residential Consumption Survey and Billing Data: Southern California Edison, Summer 2001." *Sacramento: California Energy Commission Report*, 400-02-006F.

Maloney, William. 1999. "Does Informality Imply Segmentation in Urban Labor Markets? Evidence from Sectoral Transitions in Mexico." World Bank Economic Review, 13: 275–302.

Margolis, David. 2008. "Unemployment Insurance versus Individual Unemployment Accounts and Transitions to Formal versus Informal Sector Jobs." *CREST Working Paper*, 2008–35.

Mascarenhas, Henrique Ribeiro. 2005. "O setor de eletrodomésticos de linha branca: um diagnóstico e a relação varejo-indústria." *Master's thesis, Fundação Getulio Vargas, São Paulo*.

Mation, Lucas, and Claudio Ferraz. 2011. "How Do Firms React to Infrastructure Constraints? Evidence from Brazil's Energy Shortage." Working paper, Department of Economics, Pontifícia Universidade Católica do Rio de Janeiro.

Maurer, Luiz, Mario Pereira, and José Rosenblatt. 2005. "Implementing Power Rationing in a Sensible Way: Lessons Learned and International Best Practices." *Energy Sector Management Assistance Program Report, World Bank.*

McKinsey. 2009. "Pathways to a Low-Carbon Economy: Version 2 of the Global Greenhouse Gas Abatement Cost Curve." *Report from McKinsey & Company*.

Meghir, Costas, Renata Narita, and Jean-Marc Robin. 2012. "Wages and Informality in Developing Countries." *NBER Working Paper*, 18347.

Meier, Alan. 2005. "Saving Electricity in a Hurry: Dealing with Temporary Shortfalls in Electricity Supplies." *OECD/IEA*.

MTE. 2008. "CAGED e PME: Diferenças Metodológicas e Possibilidades de Comparação." Nota Técnica Ministerio do Trabalho e Emprego, IBGE, mimeo.

Mulligan, Casey. 1998. "Pecuniary Incentives to Work in the United States during World War II." *Journal of Political Economy*, 106(5): 1033–1077.

Niehaus, Paul, and Sandip Sukhtankar. 2012. "The Marginal Rate of Corruption in Public Programs: Evidence from India." *Mimeo, University of California at San Diego*.

Olken, Benjamin, and Monica Singhal. 2011. "Informal Taxation." American Economic Journal: Applied Economics, 3(4): 1–28.

Perry, Guillermo, Willam Maloney, Omar Arias, Pablo Fajnzylber, Andrew Mason, and Jaime Saavedra-Chanduvi. 2007. "Informality: Exit and Exclusion." *The World Bank, Washington DC*.

Pimenta, Amanda, Hilton Notini, and Luiz Felipe Maciel. 2009. "Brazilian Electricity Demand Estimation: What Has Changed after the Rationing in 2001? An Application of Time Varying Parameter Error Correction Model." *Mimeo, Getulio Vargas Foundation*.

Pomeranz, Dina. 2012. "No Taxation without Information: Deterrence and Self–Enforcement in the Value Added Tax." *Mimeo, Harvard Business School.*

PROCEL. 2007*a*. "Posses e Hábitos de Uso de Aparelhos Elétricos: Classe Residencial." *ELETROBRÁS - Centrais Elétricas Brasileira*.

PROCEL. 2007b. "Simulação de Potenciais de Eficiência Energética para a Classe Residencial." *ELETROBRÁS - Centrais Elétricas Brasileira*.

Reiss, Peter, and Matthew White. 2005. "Household Electricity Demand, Revisited." *Review of Economic Studies*, 72(3): 853–883.

Reiss, Peter, and Matthew White. 2008. "What Changes Energy Consumption? Prices and Public Pressures." *RAND Journal of Economics*, 39(3): 636663.

Robalino, David, Eduardo Zylberstajn, and Juan Robalino. 2011. "Incentive Effects of Risk Pooling, Redistributive and Savings Arrangements in Unemployment Benefit Systems: Evidence from a Job– Search Model for Brazil." *IZA Discussion Paper*, 5476.

Robalino, David, Milan Vodopivec, and András Bodor. 2009. "Savings for Unemployment in Good or Bad Times: Options for Developing Countries." *IZA Discussion Paper*, 4516.

Saez, Emmanuel. 2002. "Optimal Income Transfer Programs: Intensive Versus Extensive Labor Supply Responses." *Quarterly Journal of Economics*, 117: 1039–1073.

Saez, Emmanuel, Joel Slemrod, and Seth Giertz. 2012. "The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review." *Journal of Economic Literature*, 50(1): 3–50.

Schmidt, Cristiane, and Lima Marcos. 2004. "A Demanda por Energia Elétrica no Brasil." *Revista Brasileira de Economia*, 58(1).

Schmieder, Johannes, Till von Wachter, and Stefan Bender. 2012. "The Effects of Extended Unemployment Insurance Over the Business Cycle: Evidence from Regression Discontinuity Estimates over Twenty Years." *Quarterly Journal of Economics*, 127(2): 701–752.

Schneider, Friedrich, Andreas Buehn, and Claudio Montenegro. 2010. "Shadow Economies All Over the World: New Estimates for 162 Countries from 1999 to 2007." World Bank Policy Research Working Paper Series, 5356.

Ulyssea, Gabriel. 2010. "Regulation of Entry, Labor Market Institutions and the Informal Sector." *Journal of Development Economics*, 91: 87–99.

van Ours, Jan, and Milan Vodopivec. 2006. "How Shortening the Potential Duration of Unemployment Benefits Affects the Duration of Unemployment." *Journal of Labor Economics*, 24(2): 351–378.

Velásquez, Mario. 2010. "Seguros de Desempleo y Reformas Recientes en America Latina." Macroeconomia del desarrollo (United Nations), 99.

Vodopivec, Milan. 2009. "Introducing Unemployment Insurance to Developing Countries." World Bank Social Protection Discussion Paper, 49170. Vodopivec, Milan, and Minna Tong. 2008. "China: Improving Unemployment Insurance." World Bank Social Protection Discussion Paper, 0820.

Wolfram, Catherine, Orie Shelef, and Paul Gertler. 2012. "How Will Energy Demand Develop in the Developing World?" *Journal of Economic Perspectives*, 26: 119–138.

World Bank, . 2002. "Brazil Jobs Report, vol. 1." World Bank, Washington.

Zenou, Yves. 2008. "Job Search and Mobility in Developing Countries: Theory and Policy Implications." *Journal of Development Economics*, 86: 336–355.