ONE STOP SHOPS FOR PUBLIC SERVICES -

EVIDENCE FROM CITIZEN SERVICE CENTERS IN BRAZIL[1]

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Abstract

One Stop Shops for public services, or Citizen Service Centers, have been implemented in at least 70 countries. We evaluate the impact of such centers on a range of citizen related-variables: the time it takes to undertake a typical licensing errand, the physical displacements involved, how information is obtained, and other variables representing transaction costs, red tape and transparency in the citizen-state interaction. The questions are addressed through a novel data collection on one of the most common errands at the Brazilian bureaucracy, driver´s license renewal. We also evaluate if the quality of the socially relevant components of the licensing procedure is affected. Using a Difference-in-Differences methodology, the study evaluates a program that has inspired One Stop Shop reforms in several countries, developed- and developing. We find large reductions in the time expended by citizens and in proxies for transaction costs, suggesting the reform is a good idea, but less encouraging results for the socially relevant variables. We discuss the extent to which incentives to speed up may have prevailed where other steering instruments would be more appropriate, and potential remedies. Based on our data on actual citizen-state interactions, we also discuss limitations to establishing a true One Stop Shop.

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1. INTRODUCTION

What is the impact of Citizen Service Centers, or One Stop Shops for public services, on the interaction between citizens and the state? Such centers have been implemented in at least 70 countries, developed- and developing. The reform is typically articulated as a means to improve service delivery: increase citizens' information about and access to public services, reduce the time it takes to undertake errands, eliminate the need to visit multiple locations, reduce the importance of personal contacts, and so forth. Despite its popularity, there are few analyses of the impact of the reform on these and other variables. The paper conducts, through a novel data collection, a detailed evaluation of the Citizen Service Center reform in Brazil's most populous state, São Paulo, a reform sharing many features with similar programs elsewhere. The objective is to assess the impact on the resources, time and money, that citizens expend when undertaking a typical errand at the government bureaucracy, to study the impact on transaction costs and related variables, to asses if and how the social quality of the public service is affected, to study the channels driving the results and to undertake a cost-benefit calculation. We chose renewal of driver´s license, one of the most common errands at the Brazilian bureaucracy, to study these questions. A list of countries with Citizen Service Centers, with literature references to each case, is provided in Appendix Table A1. We interchangeably use the terms Citizen Service Center and One Stop Shop, referring to buildings for in-person public service delivery and citizen attendance, where several government offices are physically co-located, such that there is a single location to which citizens need to displace themselves, to conduct errands.

Although most One Stop Shop reforms were implemented from the 1990's and onwards, predecessors exist. Government agents in British Columbia, Canada, had an integrating function for

public services from the 1850's (Bent, Kernaghan, & Marson, 1999). Scandinavian authorities cooperated from the 1930's (Askim et al., 2011). Australia tested One Stop Shops in the 1970's (Wettenhall & Kimber, 1996). Several other Anglo-Saxon countries (Canada, New Zealand, the United Kingdom) instituted programs in the 1990's, as did other European countries. The reform we evaluate, Poupatempo ("Savetime"), was initiated in 1997, and was second in Brazil.

In line with the above definition of a One Stop Shop, the São Paulo state government program Poupatempo consists of centrally placed government buildings, where a number of authorities are hosted/co-located. The literature recognizes several variants of One Stop Shops, however. Inside a Poupatempo unit, different authorities have separate offices/counters, meaning that it is not a "Single Window" One Stop Shop, which would require additional integration. Bent, Kernaghan, & Marson (1999), describing Canadian One Stop Shops, classify variants of co-located service delivery. In their terminology, Poupatempo has "owner-delivered" services in a co-located environment, that is, there is no functional integration of different authorities (although there is a coordinating back-office, running and monitoring performance of the Poupatempo unit itself). With offices that are staffed partly with Poupatempo employees (front office) and employees from the authorities themselves (specialized functions), the reform also has elements of a "Delegated Service delivery through a Corporate Service Utility". Another feature is that the physical channel of in-situ attendance is combined with a web and telephone platform for information/inquiries, meaning that Poupatempo is also a "Gateway" for improved citizen access. As with many other One Stop Shops, Poupatempo was implemented in addition to the pre-existing government bureaucracy. More specifically related to this study, a driver´s license could previously be renewed at the legacy bureaucracy or through an intermediary; implementing the One Stop Shop means that an additional option now exists. Similar cases with One Stops Shops as an addition rather than a substitution, are Chile (Chileatiende, 2014), Cyprus

(Alexandrou, 2008), Kazakhstan (Janenova and Kim, 2016) and Portugal (Esteves de Araújo, 2001). What is evaluated is how the One Stop Shop performs compared to the traditional means of attending to citizens. The focus is on citizen centered variables, introduced in section 2.

Poupatempo mainly provides state (as opposed to federal/municipal) services. Some common errands are getting an ID card or an excerpt from criminal records (from the Civic Identification Institute), getting an employment booklet (the Labor Secretary), renewing a driver's license or undertaking vehicle related matters (the Department of Transit - DETRAN), doing bank errands, and taking photos/photocopies. There are also some social welfare services, e.g. related to unemployment benefits and government housing. Citizens can also pay utility bills and file consumer protection complaints. Poupatempo inspired similar reforms in e.g. Portugal, Chile (referenced above) and Kenya (Firestone et al., 2017), and many countries have since followed suit.

The provisioning of different types of personal documents, as well as social services, is typical for Citizen Service Centers. At the risk of overgeneralizing, half of the Table A1 One Stop Shops (e.g. Poupatempo) primarily provide document related services, with the other half primarily focusing on social services, broadly defined.[2] As Heinrich (2016) convincingly argues, these areas are related. Heinrich shows that an important obstacle to providing a child support grant in South Africa is related to recipients not having the personal documents that, de jure or de facto, are required, and also discusses similar problems in the United States. Document related barriers in Brazil are long known (Rosenn, 1971; DaMatta, 1984; Grisham, 2005), but apply also in many other countries. If Citizen Service Centers succeed in improving access to personal documents—the object of this study, a positive consequence may be improved access also to social services.

The paper proceeds, in section 2, with a background to One Stop Shop reforms and evalua-

[2] An early study of One Stop Shops in 11 European countries similarly has an approximate 50/50 split between the two areas (Kubicek & Hagen, 2000). Some Citizen Service Centers entail more intergovernmental cooperation than Poupatempo, with many municipal-, state- and federal services, yet other programs attend to both citizens and firms.

tions thereof, which further sets the stage for the evaluation at hand. Section 3 has additional details on the Poupatempo reform and the expansion of the program, as well as on driver´s license renewals, both the regulation and the legacy procedure. Sections 4 to 6 discuss data, define the outcome variables, present summary statistics and outline the empirical strategy. Section 7 estimates the impact of Poupatempo. Section 8 discusses the results. The appendix has details on the data collection, additional analyses, robustness checks and a cost-benefit calculation.

2. BACKGROUND

2.1 Citizen Service Centers in the Public Administration Literature

Citizen Service Centers are not easily situated within one domain of Public Administration. A common interpretation is that such centers encompass some elements of New Public Management (NPM) reforms (Hood, 1991), especially in the focus on citizens/customers and on performance improvements and measurements, but not fully so (Esteves de Araújo, 2001; Askim et al., 2011; Login ASIA, 2016). Rather than a classification, we take a different approach, and briefly refer to three main theories of Public Administration—and corresponding modes of actual service delivery, i.e. the classical/"Weberian" bureaucracy, NPM, and the neopatrimonial state. The latter category refers to societies where public goods and services are fully or partly allocated to meet the private objectives of the ruling parties/groups/politicians/individuals, rather than the *res publica* (e.g. Médard, 1996). Each theory contains some feature, affecting the efficiency/quality of public service delivery, the response to which has been the justification of One Stop Shops. Obviously, in any one country/context, the institutional and public administration history differs, starting points are not the same, and different forms of bureaucratic organization may co-exist (Médard, 1996; Hydén, 2003; Olsen, 2008). As a result, not all reform rationales need apply

everywhere.[3] The review is useful, however, as it identifies citizen-centered outcome variables of interest. The question then is if Citizen Service Center reforms improve upon these variables.

Max Weber's rational-legal-technical bureaucracy (Weber, 1922/1978), as a theory and in its real-life implementation, has been criticized on efficiency and other grounds (Olsen, 2006, 2008, discusses the critique). Indeed, NPM partly arose as a response (e.g. Kettl, 1997; Osborne 2006). Some relevant aspects are that the centralized decision making and control in the classical bureaucracy may make processes slow, a focus on internal rules may create red tape (Bozeman, 1993) and imply a lack of flexibility (Al-Habil, 2011), and the low level of front-line staff discretion, with weak incentives for initiative, may make the bureaucracy unresponsive (Olsen, 2008). Woodrow Wilson, one of the founders of the Public Administration field, stated that the *esprit de corps* of contemporary bureaucracies implied that bureaucrats did not serve the public, but rather their superiors (Wilson, 1887, p. 221), although the issue is contested (Pepinsky, Pierskalla, & Sacks, 2017). More generally, Peters & Pierre (2003) argue that traditional public organizations were not designed to maximize on efficiency and flexibility. In addition, the scale and complexity of public services grew in the latter half of the twentieth century (e.g. Kubicek & Hagen, 2000). In this context, and as argued in most reform proposals, typical goals of One Stop Shops are to improve citizen access and to speed up and simplify processes. Partly related is that the functional specialization of the traditional bureaucracy may run counter to the nature of many citizen errands, which involve different government departments. A simple example is getting an ID, which may require an excerpt from criminal records. If the civil registry and police are functionnally separated, then the citizen may need to take the errand from the registry, to the police, and perhaps back, instead of an internal handling and coordination. Citizen Service Centers thus

[3] Analyzing reform rationales on a country level is outside the scope of the paper. Table A1 represents various institutional trajectories. Likewise, such trajectories may differ within countries. Mimicking reforms from other countries may also have played a role in the diffusion of One Stop Shops, a topic we touch upon in section 8.

aim at facilitating service delivery through physical co-location and coordination of different authorities. “Break down the silos” is a regular call for action (PwC, 2012), as is the call for an “outside-in”, rather than “inside-out”, organization of bureaucracy (Marson & Heintzman, 2009).

The fragmentation of service delivery is not only a result of the traditional public administration, however. Whereas NPM reforms sought to remedy efficiency-, incentive- and agency problems, identified by e.g. the New Institutionalists and Public Choice theorists (as discussed by Hood, 1991), they also led to fragmentation (Denhardt & Denhardt, 2000; Christensen, Fimreite, & Lægreid, 2007; Verhoest, Bouckaert, & Peters, 2007). The objective of co-location/integration can thus also be interpreted as a reaction to NPM, rather than as being part thereof. Another One Stop Shop objective, i.e. to enhance citizens’ information about public services and improve access, can instead be seen as inherent to the citizen-customer focus of NPM.[4]

In neopatrimonial states, some groups/individuals are favored, based on e.g. ethnicity, personal contacts, clientelistic networks or political concerns. The latter three attributes certainly apply to Brazil (Mainwaring, 1999).[5] In such contexts, and with respect to the citizen-bureaucrat interaction, Citizen Service Centers aim at reducing the importance of personal contacts and opportunities for corruption, increase transparency and establish an egalitarian treatment (obviously, the Weberian bureaucracy, where implemented, was also a response to some of these issues).

2.2 Evaluating Citizen Service Centers

The above discussion identifies citizen-centered variables that One Stop Shops aim at improving

[4] Partly a reaction to the fragmentation implied by NPM, new theories emerged. New Public Governance (Osborne, 2006), New Public Service (Denhardt & Denhardt, 2000), Joined-Up Government (Pollitt, 2003) and Whole of Government (Christensen and Lægreid, 2007) may all be interpreted such that Citizen Service Centers fall within their domains. The Weberian bureaucracy has also re-emerged (Olsen, 2008), with studies suggesting that such structures lead to growth (e.g. Evans and Rauch, 1999) and development (Cingolani, Thomsson, & de Crombrugghe, 2015).

[5] The term neopatrimonialism is mostly used for Africa, but applies to Brazil in that Weberian reforms were done in the 20th century, and a rational-legal structure now coexists with patrimonialism (Mainwaring, 1999; Bechle, 2010; Pereira, 2016). The 1990´s also saw NPM reforms (MARE, 1995; Bresser-Pereira, 1998). Arguably more common in some sectors, e.g. infrastructure, patrimonialistic practices in Brazil may affect also frontline services, through e.g. the shielding of some authorities from efficiency concerns and non-meritocratic hiring practices in certain areas.

upon: *facilitate access to and improve the quality of information* about public services, *reduce the time it takes to undertake errands, the displacements needed and the monetary resources citizens expend.* Virtually all reforms cite these objectives, e.g. Canada (Bent, Kernaghan, & Marson, 1999), Colombia (IDB, 2014), Georgia (ACSH, 2016), Kenya (Firestone et al., 2017), Sweden (Skatteverket, 2007) and Vietnam (Agarwal et al., 2017). Partly overlapping, but less straightforward to operationalize in an empirical study, are the common objectives of *reducing transaction costs*—e.g. Mongolia (ILO, 2016) and New Zealand (New Zealand Government, 1999), *reduce red tape*—Canada and Kazakhstan (Janenova and Kim, 2016), *increase transparency*—Georgia and Australia (PwC, 2012), *reduce the importance of personal contacts*—Kazakhstan and the West Bank (Global Communities, 2014), *treat all citizens equally*—Kenya and the Western Balkans (Agarwal, Pfeil, & Schott, 2017A), *reduce the use of intermediaries*—Vietnam and Australia (Australian Government, 2008), and *reduce corruption*—Georgia and Kenya. All these objectives were part also of the São Paulo reform (Paulics, 2003; Governo do Estado de São Paulo, 2005). Another goal can be inferred from Kubicek & Hagen (2000, p. 25): "The qualities of One-Stop Government, such as integrated and fast service, might..be at odds with quality factors for specific public services." Unlike a private sector perspective any true public sector reform should, beyond speed and its covariates, consider also possible negative externalities, and *assure the social quality of the service*. We evaluate also such aspects of the Poupatempo reform.

Despite many One Stop Shop reforms, few studies analyze the impact on the above variables. Esteves de Araújo (2001) and Askim et al. (2011) argue for such an evaluation. In discussing lessons learned from Service New Brunswick, a Canadian One Stop Shop, Bent, Kernaghan, & Marson (1999, p. 97) state that "information on the efficiency and satisfaction of the old method of operation was limited to anecdotal evidence. As a result, although progress on the project was

measured from the first opening of the Service Centres, it was not possible to compare it objectively with prior operations." That said, satisfaction with public services has been measured in Canada since the 1990's, using the "Citizen First" surveys (Marson & Heintzman, 2009). The subjective measures are useful for assessing citizens' perceptions and, potentially, identifying improvement areas, but are not specific to One Stop Shops. The surveys have been criticized, among other things, for sample representativeness problems (Howard, 2010).[6] Akin to such evaluations in the questions asked, and used in many One Stop Shops (e.g. Poupatempo), are "exit polls", in which individuals are asked about their service experience. The polls may escape some of the criticism of satisfaction measures (Heinrich, 2003; Howard, 2010), in that citizens are informed right after doing the errand, but there is still the issue, adhered to above, of whether *private* satisfaction is the correct measure to evaluate a *public* service. In addition, the surveys are short and uninformative about the cost effectiveness of the service. Yet another source of information, in line with Heinrich's (2003) discussion of administrative data, are systems that monitor wait times, queue lengths and errands resolved (e.g. Agarwal, Pfeil, & Schott, 2017A), Such data can potentially be part of a cost-benefit analysis. It only includes individuals that actually use the One Stop Shop, however, thus impeding conclusions about citizen access in general. Second, there is typically no corresponding information on interactions in the "old" bureaucracy, hence we have no counterfactual. Third, wait times and payments inside an office, for one specific visit to the One Stop Shop, are only part of the resources expended when doing an errand.

The present impact evaluation seeks to remedy some of the above issues. We select one of the most common errands at the Brazilian bureaucracy in general, as well as at Poupatempo, which is driver's license renewal. Driver and vehicle errands are also common elsewhere, with at least a

[6] Kernaghan (2010) reviews similar evaluations in other countries. Agarwal et al (2017) report results from a Vietnamese satisfaction survey that specifically targeted One Stop Shop users.

third of the One Stop Shops in Table A1 handling such errands. Besides the fact that having a driver's license is common per se, the document can sometimes, as adhered to in Heinrich (2016), also be used as an ID. Through a detailed data collection, combined with institutional data, we attempt at evaluating the impact of the One Stop Shop reform on how the renewal procedure is undertaken by citizens, with respect to all the above discussed variables, that is, on *the time, displacements and payments involved*; on *information, transaction costs, red tape* and *transparency*; on *the importance of personal contacts* and if there is an *egalitarian treatment*; on *corruption* and the *social quality* of the procedure; and on the *use of intermediaries*. Our survey registers all time- and other costs incurred by the citizen, in particular transport times, and not only those at the public entities, as well as details of the socially relevant parts of the procedure, which is a medical exam and a course/test. We aim at surveying individuals from the population of drivers at large, rather than only those using Poupatempo. Ideally, this creates a representative rather than a selected sample, important for an assessment of the societal impact of the reform. We interview both in locations with, and without, Poupatempo. Together with inherent features in the reform timing and in the renewal requirement, it allows for the combination of before- and after data in treatment and control locations, for a Difference-in-Differences study.

2.3 Additional Notes on Literature

Academic studies of citizens' time- and other costs at the bureaucracy are rare. A methodology for firms was developed by de Soto (1989), Djankov et al. (2002) and the World Bank Doing Business project. As for Citizen Service Centers, Esteves de Araújo (2001), for Portugal; Bussell (2010), for India; Askim et al. (2011) for Denmark, Norway and the United Kingdom and Mota Prado & da Matta Chasin (2011), for Poupatempo, analyze institutional and political aspects of such reforms. Closer to this study, the payment technology impact evaluation of Muralidharan,

Niehaus, & Sukhtankar (2016) measures citizens' time costs to collect wages in India. Gallagher, Struyk, & Nikonova (2003) study a Russian pilot One Stop Shop, interviewing 34 individuals about time spent, finding individual- and overall benefits. Only One Stop Shop users were surveyed, however, about experiences in the new and old structures, which may imply a selected sample, and some variables were conjectured, rather than asked about. Bussell (2009), in a cross-sectional study of service delivery technologies in India, surveyed 1003 individuals. Those using computerized offices for common errands (analyzed together) did fewer visits than users of the legacy structure, whereas time spent was similar. As Bussell notes, the samples may have differed in environmental or individual factors. Transport times and the ratio of citizens using different delivery modes, elements of a cost-benefit analysis, were not recorded. Bussell also studied private vs. public delivery and did a field experiment. Ferrer (2006) found a large impact in a Poupatempo cost-benefit study of issuance of criminal record certificates. Pre-reform data was conjectured, however, and the ratio of citizens using each delivery mode was omitted, which advocates a refinement (Mota Prado & da Matta Chasin, 2011).[7]

3. THE POUPATEMPO REFORM AND DRIVER'S LICENSE RENEWALS

Poupatempo was launched in São Paulo city in 1997. Additional metropolitan area units then followed. As of 2006, there were units also in four populous municipalities in the interior of the state but the geographical coverage was limited. From 2008 to 2011, an expansion program implemented new units in 16 municipalities in the state's interior. It is this expansion the paper is concerned with. The units were not randomly allocated, but rather placed in some of the largest and economically most important cities, yet assuring geographical coverage, instead of a clustering in high population density areas only. Figure 1A shows the new units on a map. São Paulo

[7] An additional study is Ryu & Rainey (2008). The authors compare employment outcomes, in Texas, for individuals using a One Stop Shop for employment services, to those using the legacy bureaucracy.

citizens can use any of the units throughout the state, whereas intermediaries are not allowed. It should be noted that the Poupatempo reform does not change rules and regulations for obtaining personal documents. The de jure procedure is the same, no matter the means used.

All holders of a driver's license in Brazil must do a medical exam every five years, effectively implying a five year renewal obligation. Individuals have a two month time window, around the date five years from the previous document's issuance, to do the renewal. With about 20 million licensed drivers in São Paulo, there is an average of four million renewals per year, making it one of the most common errands at the government authorities. The medical exam, done by an ophthalmologist, has eight compulsory checks. These are vision, hearing, reflexes, pulse, heart and lung auscultation, blood pressure and hand muscle strength, and are further discussed below.

A second component is a 2005 regulation requiring that those with the original license from before 1999 should get defensive driving and first aid training in their first post-2005 renewal, as it was not part of their original curriculum. The course should be 15 hours, if the classroom option is chosen, plus a test, or a self-study course, and the test. These statutes, i.e. a medical exam and a course/test, and the regularization of potential fines, are the socially relevant parts of the renewal. We evaluate the compliance with both requirements. The other parts of the renewal are largely administrative (handing in, paying and picking up the application/license).

Driver's licenses are administered by the Department of Transit, DETRAN. The traditional/ official procedure is to renew it at the DETRAN office in one's home municipality. The second option is at a driving school. Apart from providing driving lessons, these act as intermediaries for services such as undertaking the administrative steps of the renewal on behalf of the individual and regularizing traffic fines, and also provide the 15h theoretical course, compulsory for some in our interview sample. The third option is at a despachante, a professional intermediary specializing in conducting errands at the authorities, with a long history in Brazil (de Góis, 1554/2001;

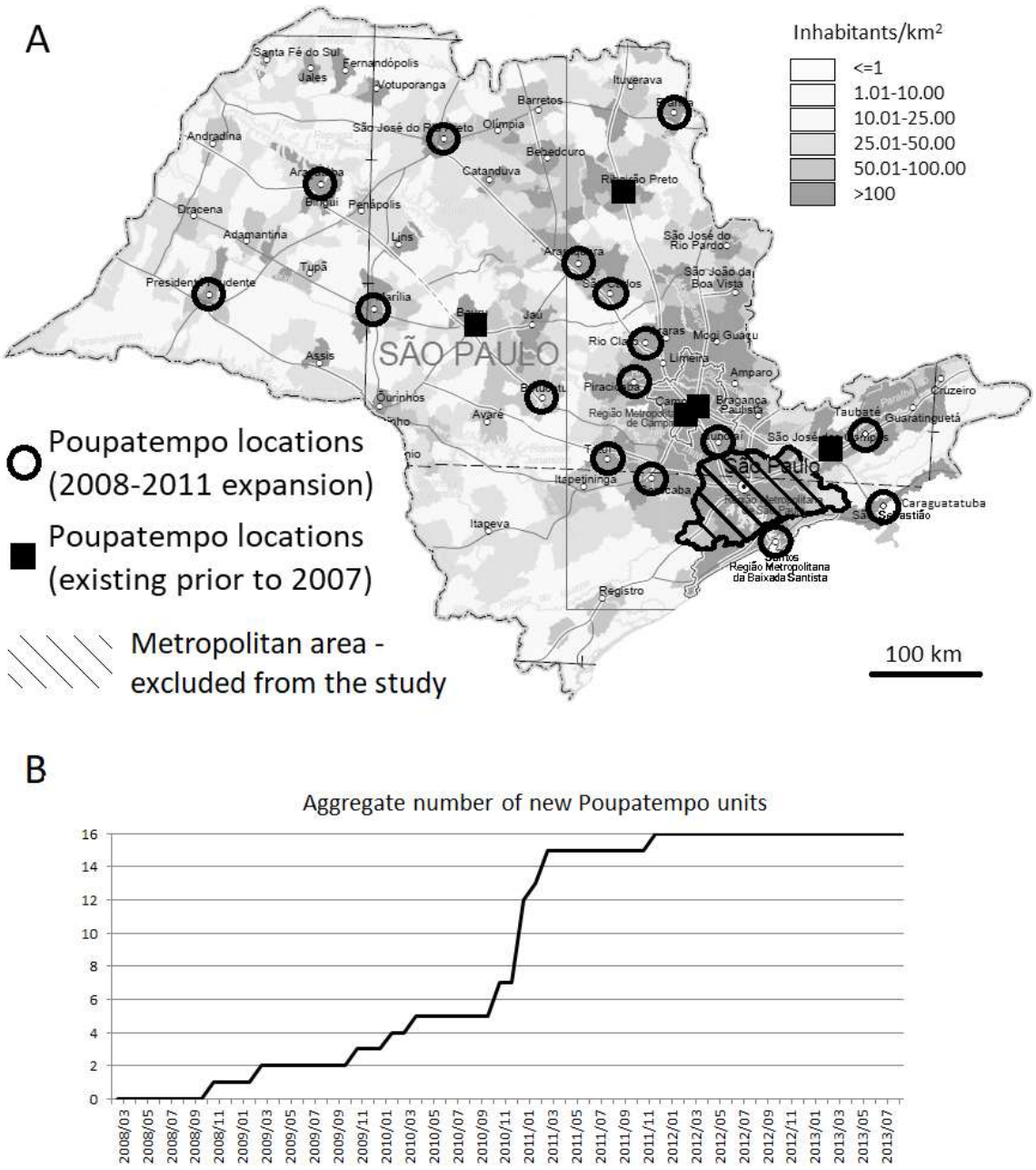
Rosenn, 1971).[8] With Poupatempo, a fourth option is introduced, centralizing some/most of the renewal activities. More specifically, inside the Poupatempo building, there will be a DETRAN office, ophthalmologists doing the medical exam and auxiliary services (bank, photocopies, etc.).

Some additional context on vehicle/driver's license errands in Brazil is that many authorities have been known, anecdotally, for an unresponsive and inefficient service. License-seekers would be sent back and forth and face long lines, wait times and uncertainty, both in how to conduct errands and of outcomes (e.g. Paulics, 2003). DETRAN in São Paulo was no different. Partly as a response, the bureaucracy intermediary (despachante) sector proliferated, in order for citizens to be able to undertake errands at all. Systems were also in place that gave despachantes (and driving schools) an advantage over citizens doing errands on their own, as they could access some of the DETRAN computerized registries. Individuals doing errands on their own would instead need to visit the offices in person. Bureaucrats may also have slowed processes and colluded with intermediaries, in order to benefit from the additional fees charged by the sector.

4. DATA COLLECTION AND INSTITUTIONAL DATA

Three main data sources are used in order to evaluate the impact of Poupatempo. The first consists of detailed interviews with citizens about their driver's license renewal. The survey questionnaire was designed to capture all aspects of the renewal, in particular all steps the individual had followed when undertaking the procedure. It included questions about where and at what entity the license was renewed, the time spent, at visit(s) to the bureaucracy (DETRAN/Poupatempo) and/or the intermediary (driving school/despachante) and transport times. A standard set of questions were asked to all interviewees, registering socioeconomic status and all steps undertaken. We recorded if and how the respondent informed herself (e.g. internet or a visit); how the

[8] de Góis (1554/2001) describes an intermediary function in 16th century Portugal that later transferred to Brazil.



Notes: (A) The area is 250.000 km^2, with 44 million inhabitants and 35 million Poupatempo visits in 2013. Half of the population lives outside the metropolitan area, which is our area of interest. Four cities already had Poupatempo, and are excluded (black squares, with two units in the city of Campinas). The reform's target area was thus the interior and coastal areas, excluding the metropolitan area and the four cities, and is referred to as "interior".
(B) The horizontal axis corresponds to the time interval for which we have renewal data from the interview project. A list of the new Poupatempo units and the implementation dates is provided in Appendix Table A2.

Figure 1. (A) São Paulo State Map With Pre-existing and New Poupatempo Units. (B) Evolution of the Number of New Units, 2008–2013.

procedure was started, for instance through a trip to the entity itself; if a doctor was visited; if the course/test was made; if a copy store, photo machine, photographer, bank or internet café was visited; if the application was handed in and if/how the renewed license was picked up. If an individual had pursued a specific step (e.g. an information trip), we asked detailed questions about this step (e.g. trips made, time spent and payments). There were also a few subjective questions.

The interview project was conducted from March to August 2013. It exploited the five year renewal obligation and the reform timing, to get pre- and post-reform data, in Poupatempo and non-Poupatempo locations. We interviewed adults, screening on if they had a driver's license and had renewed it at least once, inquired about the last renewal, and interviewed those that had renewed since March 2008. Appendix 2 has details about the interview project, the selection of the non-Poupatempo interview locations and comparisons to the Poupatempo locations, and discusses the sample representativeness compared to the population of São Paulo driver's license holders at large. A summary is given here: We interviewed 720 individuals in 31 municipalities, 16 of which were the locations where Poupatempo was implemented from 2008 to 2011, thus constituting the treatment group. We used propensity score matching to select 15 control group interview municipalities. 20 to 25 interviews were conducted in each of the 31 municipalities, on weekends, primarily in shopping malls/streets, where the aim was to get as representative a sample as possible. Interviewers would approach every x-th adult walking in a certain direction in the mall or street, where x would depend on the amount of people around, ask if the person had a driver's license, then, if it had been renewed the last five years, do the interview. With such a (quasi-) random selection, we should get a renewal date distribution over March 2008 through August 2013 that roughly maps the population distribution. Given the reform timing, shown in

Figure 1B, with the horizontal axis corresponding to the interview sample interval of renewal dates, we also get a division into those that had, and had not, access to Poupatempo when renewing.

One aspect of the data collection is that we did not screen out individuals living outside the interview municipalities, and 18 percent of the sample lives elsewhere. This was for two reasons. Those living close to a Poupatempo municipality are likely to use the service there established (and analogously for non-Poupatempo municipalities, had these gotten the service). Such nearby places fall naturally into the treatment and control groups. Second, we can use the data to analyze how the reform take-up and impact depend on distance. In the main specifications, those living in or less than 20 km from any of the 16 reform municipalities form the treatment group, every-one else control.[9] Other specifications and a distance analysis are in the robustness sections.

The second data source is the Statistics Brazil 2008–2009 household budget survey (IBGE, Pesquisa Orçamental Familiar). It is used to compare the income distribution of the interview sample, to the relevant population of interior São Paulo at large. The third source is an anony-mized DETRAN database of all drivers' licenses. Being a March 2014 snapshot, it has data on each license holder´s last interaction with the authorities, including the date of the medical visit. We thus have access to renewals for the five years ending in March 2014, which is an 80 percent overlap with the interview project interval. The data is used to check how well the selection of interview individuals worked for gender, age, residence and time of renewal, to analyze reform take-up and in the cost-benefit calculation. The interview and DETRAN data averages for age and gender are similar (42.3 years, 63 percent men; 43.9 years, 66 percent men). Comparing the temporal renewal distribution and income with the secondary sources, as we do in Appendix A2, also indicates that the interviews captured a representative sample of license holders. This is

[9] A 25 minute car travel time cutoff is also used, i.e. both criteria should be met, to be considered as "living close".

important for the study´s internal validity, the cost-benefit calculation and other aspects.

5. OUTCOME VARIABLES

The background section discussed typical One Stop Shop reform objectives. Having defined the study object and the data to use, Table 1 operationalizes the main outcome variables. The time, displacements and payments involved when undertaking the errand, as well as the information, personal contacts and medical visit variables have a precise quantitative definition and are analyzed with the Difference-in-Differences framework in section 7, as indicated in the last column.

The minutes, trips, days and payment variables are perhaps the most directly perceived by citizens in the service delivery process and have a straightforward definition. As for information, citizens' searching for and obtaining information on how to conduct errands at the government bureaucracy can be interpreted as a transaction cost (North, 1990) and most reforms aim at improvements in this area. We define two variables: a dummy for information-only trip(s) and the time spent to obtain information. Next, the personal contacts dummy indicates whether the respondent knows someone at the entity of renewal. The medical variables are proxies for quality: the number of checks made by the doctor (out of eight compulsory) and the duration of the exam.

A few variables, i.e. red tape, transparency and other transaction costs, are not as precisely defined, which is mostly related to that these concepts per se are not as precisely defined in the literature. They are analyzed in Appendix 5, as is the defensive driving and first aid course/test, which was not compulsory for all. Appendix 5 also discusses the use of intermediaries.[13]

5.1 Summary Statistics of the Main Outcome Variables

Table 2 presents summary statistics for the minutes, trips, days and payments variables. The data is broken down by means of renewal (DETRAN, Poupatempo, driving school, despachante). Panel A contains all the data, panel B excludes individuals that did the course/test, and panel C ex-

Table 1. List of different outcomes analyzed in the study.

Broad outcome category	Variable name	Variable details	Section
Time, displacements and payments involved when undertaking errands at the government bureaucracy	*Minutes*	Sum of all time spent (in minutes) in doing the renewal, e.g. getting information, visits to the entities involved, waiting, in attendance at counters, in transport, etc. For comparability reasons, the course/test component is excluded (as it was not compulsory for all).	7.1
	Minutes, without idle time	Same as above but excluding idle time. Idle time is the voluntary time spent waiting to retrieve a renewed license, once all steps are completed, rather than returning in a new visit (something primarily occuring at Poupatempo).	
	Trips	Number of return trips (A–entity–A). Can also be an outbound displacement from e.g. home and an inbound displacement to e.g. work (A–entity–B) or half-trips (A–entity), excluding course/test trips (and to adjacent* places).	
	Days	Number of days elapsing from the moment the individual starts the procedure (typically getting information) until the renewed license is available at the entity of renewal.	
	Days to process	Time elapsing (in days) at the entity of renewal, from the moment the individual has handed in the full application, until the renewed license is available.	
	Payments	Sum of all payments made, in Reais (R$), discounted to 2013.	
Transaction costs and transparency	*Dummy for information trips*	Equals one if an individual has undertaken trip(s) solely for information purposes.	7.2
	Time getting information	Time (in minutes) spent obtaining information about how to conduct the renewal. Typical information activities are use of internet, phone, conversations with family/friends and information trips.	
	Red tape	Trips made in addition to those strictly needed in a "de jure" renewal.	Appen-dix
		Time spent in addition to the time that would be spent in a "de jure" renewal.	
	Transparency	Correctness of information obtained.	
		Whether information was obtained as to when the license would be ready.	
	Other transaction costs	The extent to which the steps undertaken are non-standard or unnecessary.	
		Variability in outcomes.	
Equality of treatment	*Dummy for having personal contacts*	Equals one if an individual answers "yes" to whether he/she knows someone at the entity of renewal.	7.3
	Uniformity of impact	Reform impact across socioeconomic groups: Whether reform impact differs by gender and by age- and income group.	
If the socially relevant legislation is adhered to or if rules are circumvented	*Medical exam dummy*	Whether the compulsory medical exam is undertaken or not.	7.4
	Number of medical checks	The number of medical checks undertaken, out of the eight compulsory.	
	Duration of the medical exam	Duration of the medical exam, in minutes.	
	Course/test dummy	Whether the defensive driving and first aid course/test is done, when mandatory	Appen-dix
	Duration of the course	Duration of the course, in hours (15 hours is the stipulated minimum duration).	
The extent to which middlemen are used	*Use of intermediaries*	Equals one if the renewal was done through an intermediary.	Appen-dix

* Adjacent places are e.g. copy stores next to DETRAN, visited in conjunction. We count the time but not the trips.

cludes also those that did any other errand while renewing the license (the potential other errands are transfer of municipality, regularization of fines and change/addition of category). Although it hides the time dimension, Table 2 illustrates key points guiding the subsequent analysis. An average renewal (top left column) takes 4 hours and 23 minutes over 18 days, has 4 return trips and costs 194 R$ (averaging 100 USD, 2008 to 2013). Using a driving school or despachante entails less time in minutes and fewer trips than using DETRAN, is more common, and costlier. Using Poupatempo, the time spent is similar to using an intermediary, but with less trips/days/cost.

Panel B excludes the 28 percent of the sample (205/720) that did the course/test. Going from panel A to B, there is a more than proportional drop in the use of driving schools (from 17 to 11 percent of the total). Expressed differently, the course/test takers have an extra incentive to do the entire renewal at a driving school, as the course/test is typically offered in situ. Days and payments are lower in panel B, as the course/test component cannot be netted out from these variables in panel A. Between panels B and C there is also some selection, as those that e.g. transfer the municipality of the license (the most common other errand, occurring in eight percent of cases) typically (must) use DETRAN. The DETRAN ratio drops somewhat. In sum, Table 2 summarizes some of the main outcome variables, hints at a time saving function of Poupatempo and intermediaries and suggests relevant control variables or subsamples for the analysis that follows.

Table 3 summarizes the other variables analyzed in section 7: whether individuals undertake trip(s) solely to obtain information, the time spent getting information, if the individual has personal contacts at the entity of renewal and the medical exam variables. The sample is as in panel A of Table 2. Time getting information looks a bit like total time spent: It is similar for Poupatempo and intermediary cases, but less than for DETRAN. Poupatempo users are thereto less likely to do information trips and to know someone at the entity of renewal. Almost all individu-

Table 2. Summary interview data: Minutes, trips, days and payments.

	Average	#	DETRAN	#	Poupa-tempo	#	Driving school	#	Despa-chante	#
2A: All data										
Minutes	263	719	307	181	253	266	236	123	245	127
Minutes, without idle time	254	713	307	181	228	260	236	123	245	127
Trips	4.04	720	5.45	181	2.34	266	4.72	123	4.84	128
Days	18.2	713	25.1	179	6.35	265	29.5	119	21.1	128
Days to process	9.5	710	13.5	178	2.28	264	16.2	119	11.7	127
Payment, discounted to 2013, R$	194	573	170	143	122	217	282	95	291	99
2B: Sample without course/test takers										
Minutes	264	514	315	123	251	242	233	56	241	79
Minutes, without idle time	252	509	315	123	227	237	233	56	241	79
Trips	3.76	515	5.53	123	2.28	242	4.7	56	4.75	80
Days	14	513	21.3	122	5.27	241	25.3	56	19.1	80
Days to process	7.8	510	12.7	120	2.09	240	14.8	56	11.4	80
Payment, discounted to 2013, R$	152	427	132	103	107	197	210	49	257	66
2C: Sample without course/test takers and without Individuals doing transfer/regularization/alteration										
Minutes	257	443	296	99	252	219	231	50	231	66
Minutes, without idle time	244	438	296	99	225	214	231	50	231	66
Trips	3.66	444	5.48	99	2.24	219	4.72	50	4.72	67
Days	12.6	443	19.7	98	4.63	219	24.2	50	18.3	67
Days to process	7.23	441	11.8	97	1.79	218	15.3	50	11.4	67
Payment, discounted to 2013, R$	145	369	128	83	106	180	188	43	256	55

Notes: There are 720 interviews. The sample size differs slightly between the different rows, as each variable has missing data for a small number of individuals (except "trips"). Data on total payments is available for around 80% of the sample. A few outliers with extreme values on e.g. the trips variable, combined with highly untypical renewals, were excluded. The (row) number of renewals do not sum up, as three percent of cases could not be classified as one entity only, typically being DETRAN+intermediary or driving school+despachante renewals.
Source: Interviews.

Table 3. Summary interview data: Information, personal contacts and medical exam.

	Average	#	DETRAN	#	Poupa-tempo	#	Driving school	#	Despa-chante	#
All data										
Information trip dummy	0.35	715	0.46	180	0.24	265	0.37	122	0.43	126
Time getting information, in minutes	23.8	690	31.2	178	20.7	254	22.3	116	21.7	122
Personal contacts dummy	0.28	449	0.16	114	0.05	149	0.55	89	0.6	80
Medical exam dummy	0.99	720	1	181	1	266	0.98	123	0.96	128
Number of medical checks	3.77	700	3.81	177	3.3	261	4.39	118	4.09	122
Duration of medical exam, in minutes	11.4	708	12.8	180	8.91	263	12.4	121	13.1	122

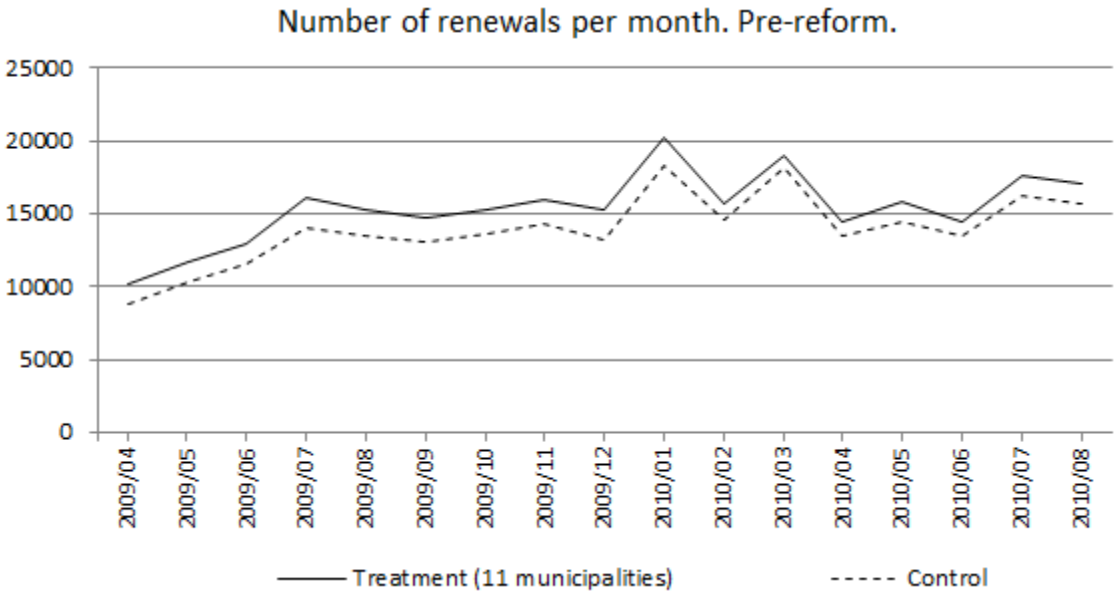
Notes: The sample size differs slightly between the different rows, as each variable has missing data for some individuals. The question regarding personal contacts was part of the final two thirds of the interview project.
Source: Interviews.

als report having done the medical exam, i.e. there is basically full extensive margin compliance, a fact to which we return in section 8. Instead focusing on the intensive margin variables, Table 3 suggests that the medical exam is substandard: Out of eight statutory medical checks, the average number actually done is about four. The table also seems to suggest that medical exams are less rigorous at Poupatempo (in the number of checks made and in the exam duration), but this is basically an effect of a general decline, which we discuss further in section 7.4.

6. EMPIRICAL STRATEGY

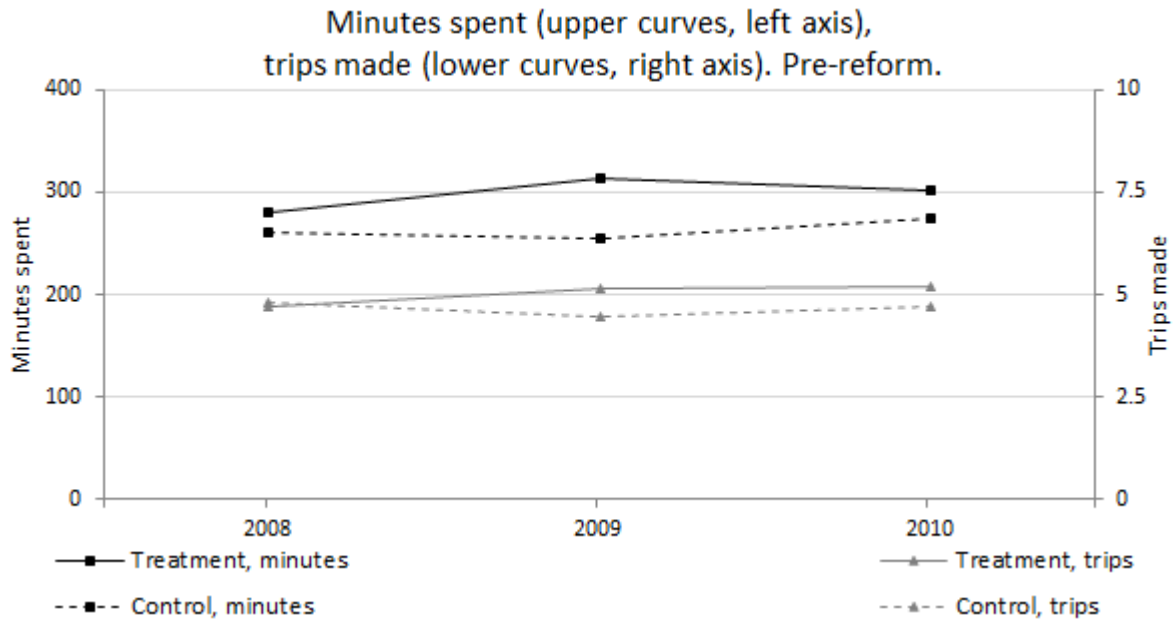
The goal of the empirical part is to evaluate the impact of the Poupatempo reform on the above discussed variables. The inherent timing of the driver's license renewal requirement and of the reform, combined with the 2013 data collection, gives us a treatment group interview sample of individuals that had, and had not, access to Poupatempo, when renewing the license (i.e. after/ before data). In addition, we interviewed also in control locations, thus enabling a Difference-in-Differences study. The main assumption of the method is that the treatment group would have followed, for the relevant outcomes, a time trend parallel to that of the control group, absent the reform. Figure 2 shows the pre-reform number of renewals in treatment and control, with similar trends.[10] Figure 3 shows pre-reform data for two key outcomes, minutes and trips. Renewals from 2008 to 2010 roughly correspond to the pre-reform period, but there are a few early treatment locations (Figure 1B). To get a pre-reform graph proper, a treatment location is removed from the 2008 to 2010 data as soon as Poupatempo is implemented. The few control group individuals that use the new units ("spillovers") are also removed. Figure 3 indicates slightly more time and trips in treatment, but a largely constant difference, suggesting the parallel trends

[10] There are three issues in displaying pre-reform data: the DETRAN data time period, the staggered reform (Figure 1B) and the interview project sample size. Figure 2 uses the DETRAN data, which starts 2009/04. We omit from the treatment data the five early Poupatempo locations and get a pre-treatment graph for the 11 (out of 16) locations where the reform occurred post-2010/09. Figure 3 uses the interview data and we exclude the early reform locations only once treated, to use as many data points as possible (see also the discussion of the regression specification).



Note: Pre-treatment graph for the 11 (out of 16) locations where the reform occurred post-2010/09. *Source:* DETRAN.

Figure 2. Number of Driver's License Renewals, in Treatment and Control.



Notes: Once treated, the 2008–2010 Poupatempo municipalities are excluded, as are a few control group observations, due to take-up. *Source:* Interviews.

Figure 3. Pre-treatment Graph for the Average Time Spent With the Renewal ("Minutes") (Upper Curves, Left Axis) and Average Number of Return Trips (Lower Curves, Right Axis).

assumption holds. Appendix 3 has similar graphs for the other outcomes analyzed in the main text and Section 7.5 has formal tests of potential pre-reform "effects" (placebo tests).

Figure 4 shows the fraction of individuals that use Poupatempo, indicating a roughly constant pre-reform level in both groups, followed by a large increase in the treatment group, but not in control.[11] Overall, the data support a Difference-in-Differences (DD) specification.

We start by estimating the impact of Poupatempo using the traditional linear DD model:

$$y_{ist} = \alpha_s + \eta_t + \delta T_{st} + \beta X_{ist} + \varepsilon_{ist} \quad (1)$$

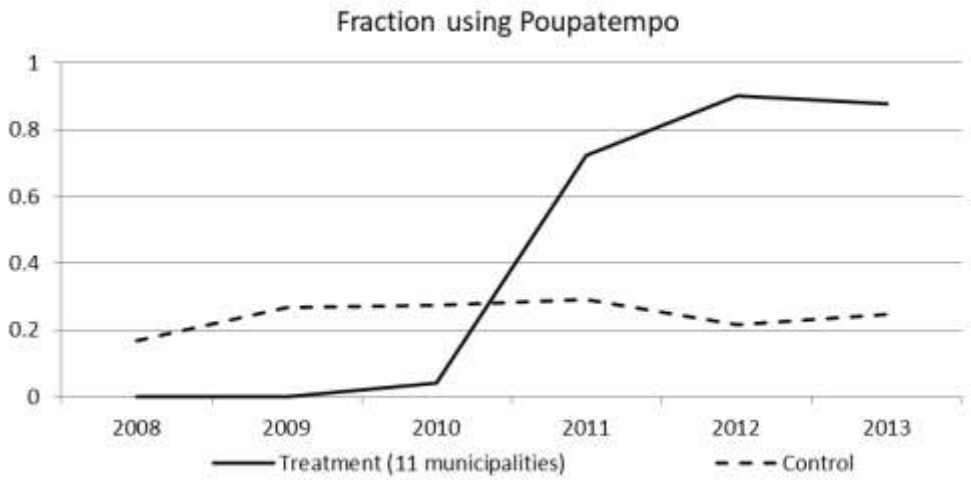
Index *i* is for individual, *s* for location and *t* for time. α_s are treatment/control location dummies and η_t time dummies. T_{st} indicates if the reform has been implemented in location *s* at time *t* and δ is the main coefficient of interest ("aftertreatment"). In section 7, η_t will be a before/after dummy. 2008 to 2010 roughly corresponds to before and 2011 to 2013 to after reform. To make it fully so, we exclude the 2008 to 2010 post-reform observations that are from the early treatment locations and the few control group observations, from 2008 to 2010, that took up the re-form (as was done in Figure 3). Some analyses use course/test/other errand- and age/gender/income controls (X_{ist}). Variants of this specification are analyzed in Appendix 6, with similar results.[12]

A specific concern with Model 1 is that its underlying assumptions (e.g. Gaussian errors) may not hold. The outcomes are non-negative count or dichotomous variables, rather than continuous (e.g. days, which is also right-skewed). We thus estimate also Generalized Linear Models (GLM, e.g. Agresti, 2015). A link function $g()$ relates the expected value of y_{ist} to the linear predictor:

$$g(\mu) = \alpha_s + \eta_t + \delta T_{st} + \beta X_{ist}, \text{ where } \mu \equiv E(y_{ist}) \quad (2)$$

Distributional assumptions for y_{ist} are also made. More specifically, we assume the dichotomous

[11] The other means of renewal see a corresponding decrease. Appendix 5 further discusses the use of intermediaries.
[12] We also omit one 2011 interview (from the last treatment location, with a pre-implementation renewal). In total, six percent of the sample is excluded. Instead using all data and a full set of period dummies gives similar treatment effect estimates (Appendix 6.2).



Notes: Pre-2010/09 reform municipalities are excluded (as in Figure 2). Course/test individuals are excluded. The control group use is mainly citizens traveling to places where Poupatempo existed before 2007. *Source:* Interviews.

Figure 4. Use of Poupatempo.

outcomes are binomially distributed and use the logit link (hence run logistic regression). We assume the minutes, trips, days and medical checks variables have a negative binomial distribution and use the log link. Estimation is done with maximum likelihood. When comparing with OLS, we follow Puhani (2012) and report marginal effects (as in e.g. Courtemanche and Zapata, 2014).

7. IMPACT OF THE POUPATEMPO REFORM

This section estimates and discusses the reform impact on the minutes/trips/days/payments, information, personal contacts and medical variables, ending with some robustness checks.

7.1 Minutes, Trips, Days, Payments

Table 4 shows estimates of the impact on the variables representing time, displacements and payments in the licensing procedure, for the linear and GLM models (panels 1 and 2). There is a significant and sizeable impact for the minutes, trips and days to process variables. Based on panel 2, the time spent is reduced with 104 minutes, involving 2 trips and 5.8 processing days less for the average license holder (columns 1B, 3, 5). The effects are reported also as percentages of the pre-reform averages, suggesting that Poupatempo is indeed a time-saver. The OLS and GLM es-

timates are similar, except for the days variable, which departs a lot from a Gaussian distribution.

Intention To Treat (ITT) estimates, as above, is the proper measure for a cost-benefit calculation, as it considers also individuals not taking up the reform (Duflo, Glennerster, & Kremer, 2008). To get at the impact on those actually using Poupatempo (Treatment on the Treated, TT), Table 5 omits, from the 2011 to 2013 data, all treatment individuals not using Poupatempo (circa 20 percent), as well as the few spillovers. The same outcomes as above are in columns 1 to 6. Exemplifying with time spent, the difference between the ITT and TT estimates is mainly due to a mechanical increase of the TT estimate from the take-up adjustment and the effect of removing spillovers, which, in our case, have longer travel- and renewal times, omitting them thus reduces the difference. In our case, these effects balance and we get the same number (104 minutes).

Decomposing the reduction in minutes into its transport and "counter" time components gives that four fifths is due to less transport time. The One Stop Shop thus seems to have the intended effect of reducing displacements, while the time "at counters" is not necessarily reduced by much.[13] Tables 4 and 5 suggest that the cost of doing the renewal decreases (column 6), but the payments variable has more missing data, is noisier, and the estimate is typically not significant.

7.2 Information Variables

The information variables are analyzed in Table 5, columns 7 to 8. As in the other columns, the estimates are for those actually using Poupatempo (TT estimates). The analysis suggests that the information process is simplified: There is a significant drop, with about 40 percent of the pre-reform level, in both the fraction of individuals that did (one or more) information-only trip(s) and in the time spent getting information. For a dummy of "any information activity" there is instead no effect (regression not shown). Prior to doing the errand, those using Poupatempo thus seek

[13] For trips, half of the 2.7 trip decrease (Table 5, column 3) is due to fewer doctor- and "handing in application" trips (once having started the procedure), the rest is split between 10 types, e.g. bank- and information trips (regressions not shown).

Table 4. Intention To Treat (ITT) estimates of the reform impact on minutes, trips, days and payments.

Dependent variables	Minutes	Minutes	Minutes, w/o idle time	Trips	Days	Days to process	Payments
Panel 1 - OLS							
aftertreatment	-109.9***	-100.2***	-118.4***	-2.033***	-6.21	-5.347**	1.733
	(21.04)	(21.35)	(19.92)	(0.276)	(3.673)	(2.458)	(17.48)
R-squared	0.133	0.158	0.144	0.308	0.301	0.245	0.265
Panel 2 - GLM (link/distribution)	log / nb	log / nb	log / nb	log / nb	log / nb	log / nb	log / nb
aftertreatment: Coefficient	-0.431***	-0.397***	-0.477***	-0.548***	-0.770***	-1.014***	-0.0543
	(0.0767)	(0.0796)	(0.0735)	(0.0675)	(0.222)	(0.258)	(0.0937)
aftertreatment: Marginal effect	-116.4***	-104.3***	-125.7***	-2.024***	-8.593***	-5.768***	-6.148
	(24.13)	(23.75)	(22.87)	(0.295)	(3.073)	(1.882)	(10.87)
Controls:							
- Course/test and other errands	No	Yes	No	No	Yes	Yes	-
- Socioeconomic	No	Yes	No	No	No	No	Yes
Observations	663	636	657	664	640	638	346
Estimated impact (from the GLM marginal effects) in percent of the treatment group pre-reform average							
Pre-reform average	299 minutes	299 minutes	294 minutes	5.03 return trips	17.6 days	11.9 days	-
Reduction	39%	35%	43%	40%	49%	48%	
Column	1A	1B	2	3	4	5	6

Notes: Panel 1 shows the OLS aftertreatment estimate and panel 2 the GLM aftertreatment coefficient- and marginal effect estimates as per the model in the first line (nb–negative binomial). In all regressions, 2008–2010 is the before- and 2011–2013 the after period. The 2008–2010 post-reform observations from the early treatment locations are excluded, as are a few 2008–2010 control group spillovers related to those locations. All regressions have treatment/control location dummies and an after dummy. Columns 1A, 2, 3 use no controls. Columns 4-5 use the course/test, transfer, regularization and change/addition of category controls as, by construction, the days variables cannot net out these components (see section 5.1). Analogously, the column 4-5 pre-reform averages omit renewals with course/test/other errands. The payments data is noisier and the analysis is always restricted to "basic" renewals (as in Table 2C). Column 1B has course/test/other errands- and socioeconomic controls (age/gender/income) and is used in the cost-benefit calculation. Using all controls also in columns 3/4/5 affects the estimates very little (regressions not shown). We calculate marginal effects (Puhani, 2012), at the 2011-2013 treatment group control variable averages. *Source:* Interviews.

Robust Standard Errors in parenthesis, clustered on treatment/control locations. * Statistically significant at the 10 percent level, ** Statistically significant at the 5 percent level, *** Statistically significant at the 1 percent level.

Table 5. Treatment on the Treated (TT) estimates of the reform impact on minutes, trips, days, payments, information, personal contacts and the medical exam variables.

Dependent variables	Minutes	Minutes, w/o idle time	Trips	Days	Days to process	Payments	Information trip dummy	Time getting information	Personal contact dummy	# Medical checks	Duration of medical exam
Panel 1 - OLS											
aftertreatment	-102.3***	-115.7***	-2.677***	-12.08***	-9.116***	-18.36	-0.178**	-10.40**	-0.228**	0.389	-0.0477
	(22.34)	(22.22)	(0.241)	(3.475)	(2.424)	(18.09)	(0.0769)	(4.506)	(0.107)	(0.416)	(1.106)
R-squared	0.166	0.189	0.430	0.358	0.314	0.336	0.101	0.100	0.162	0.139	0.148
Panel 2 - GLM (link/distribution)	log / nb	log / nb	log / nb	log / nb	log / nb	log / nb	logistic / binomial	log / nb	logistic / binomial	log / nb	log / nb
aftertreatment: Coefficient	-0.412***	-0.482***	-0.756***	-1.411***	-1.841***	-0.231**	-0.855**	-0.502**	-1.718***	0.101	-0.0751
	(0.0837)	(0.0819)	(0.0607)	(0.222)	(0.271)	(0.103)	(0.359)	(0.199)	(0.575)	(0.110)	(0.0872)
aftertreatment: Marginal effect	-104.0***	-118.4***	-2.738***	-14.24***	-8.964***	-26.50**	-0.195**	-12.55**	-0.239***	0.294	-0.656
	(23.90)	(23.15)	(0.275)	(3.102)	(1.900)	(13.05)	(0.086)	(5.830)	(0.090)	(0.313)	(0.780)
Controls:											
- Course/test and other errands	Yes	Yes	Yes	Yes	Yes	-	Yes	Yes	Yes	Yes	Yes
- Socioeconomic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	580	575	581	575	574	316	576	558	398	570	578
Estimated impact (from the GLM marginal effects) in percent of the treatment group pre-reform average											
Pre-reform average							0.49	29.6 minutes	0.32	-	-
Reduction							40%	42%	75%		
Column	1	2	3	4	5	6	7	8	9	10	11

Notes: Panel 1 shows the OLS aftertreatment estimate and panel 2 the GLM aftertreatment coefficient- and marginal effect estimates as per the model specified in the first line (nb–negative binomial). In all regressions, 2008–2010 is the before- and 2011–2013 the after period. The 2008–2010 data is as in Table 4. The 2011–2013 treatment group data excludes those individuals that did not use Poupatempo (non-compliers). 2011–2013 control group individuals that used the new Poupatempo units (spillovers) are also excluded. The regressions use treatment/control location dummies, an after dummy and the course/test/other errands- and socioeconomic controls. We calculate marginal effects (Puhani, 2012), at the 2011–2013 treatment group control variable averages. The personal contacts variable was collected during the last two thirds of the interviews; the estimate is thus based on less data (for this variable, in panel 2, we use a treatment group dummy instead of the full set of treatment/control location dummies, to avoid problems with perfect separation in the logistic regression). *Source:* Interviews. Robust Standard Errors in parenthesis, clustered on treatment/control locations. * Statistically significant at the 10 percent level, ** Statistically significant at the 5 percent level, *** Statistically significant at the 1 percent level.

information to the same extent as users of the legacy structure, but through other channels.

7.3 Personal Contacts and Uniformity of Impact

The ratio of individuals knowing someone at the renewal entity is much lower for Poupatempo users (Table 3), suggesting the reform had an impact on this variable. We estimate a 24 percentage point drop in the ratio with contacts, or 75 percent of the pre-reform level (Table 5, panel 2, column 9). We cannot precisely ascertain why personal contacts decrease, but features such as an open space, low walls inhibiting private conversations and a queueing number system may have contributed. Firestone et al., 2017, discuss similar effects in Kenya. A full evaluation may be premature, however, as Poupatempo is still new. But what is the effect of having personal contacts?

Having personal contacts is correlated with less time spent. In separate DETRAN/Poupatempo/Driving school/Despachante regressions, using the pooled cross section of the data, we include a personal contact dummy as a regressor. There is some evidence, for DETRAN, Poupatempo and driving schools, that having personal contacts is conducive to a faster process (Table A5). The effects are quite large, at 45 to 60 minutes faster renewals, and typically significant at the 10 percent level. Perhaps surprisingly, we see no effect for despachantes. In alternative specifications we sometimes get insignificant results for Poupatempo and Driving schools, while the DETRAN estimate stays significant. We studied if the effect operates via the information channel, by repeating the regressions after having deducted time to get information from the minutes variable. Significance levels then drop, with DETRAN still being the most significant. Instead using time to get information as the dependent variable, the personal contact dummy has the expected sign (10 to 15 minute decrease) and is significant for Poupatempo and driving schools. Overall, the results suggest that part of any time saving from personal contacts is due to less effort to get

information. Other factors may be present, at least at DETRAN. Interacting personal contacts with treatment in Table 5-style analyses gives insignificant results (regressions not shown).

We find evidence of a similar reform impact across socioeconomic groups. Take-up of Poupatempo is just slightly higher for the young, which can be rationalized through their somewhat longer pre-reform renewal times. Gender- and income differences are also small, with a bit lower take-up for men and high-income earners (data not shown). We ran numerous DD regressions to which we added e.g. a gender-treatment interaction term, without finding significant effects.

7.4 Compliance With the Socially Relevant Components: Medical Exam

We next study compliance with the socially relevant component that is compulsory for all, i.e. the medical exam. This requirement is stricter than in many other countries, and should assure that drivers are physically and mentally apt. We based the questionnaire on the National Department of Transit legislation of what the exam must contain (DENATRAN, 2008), asking respondents if the eight compulsory medical tests ("checks") were done by the doctor-ophthalmologist. We also inquired about the duration of the exam, and if respondents considered their vision and capacity to drive were correctly evaluated. Table 3 shows that there is basically full extensive margin compliance with the exam (99%), which we discuss further in section 8. On the intensive margin however, the results strongly suggest that the exams are too fast, with much less content than stipulated. Although 98 percent of respondents report their vision was checked, an average of 2.8 other checks was done by the doctor (out of the other seven compulsory components). The average duration is 11.4 minutes and a third of the sample report an exam of five minutes or less.

What is the impact of the Poupatempo reform on the quality of the exam? Starting from a pre-reform situation of poor average compliance with the intended social statute, we find no effect whatsoever. The lack of impact is suggested by Figure 5, which uses the same sample as in the TT regressions, to focus on the impact at Poupatempo itself. The graph shows that, during the

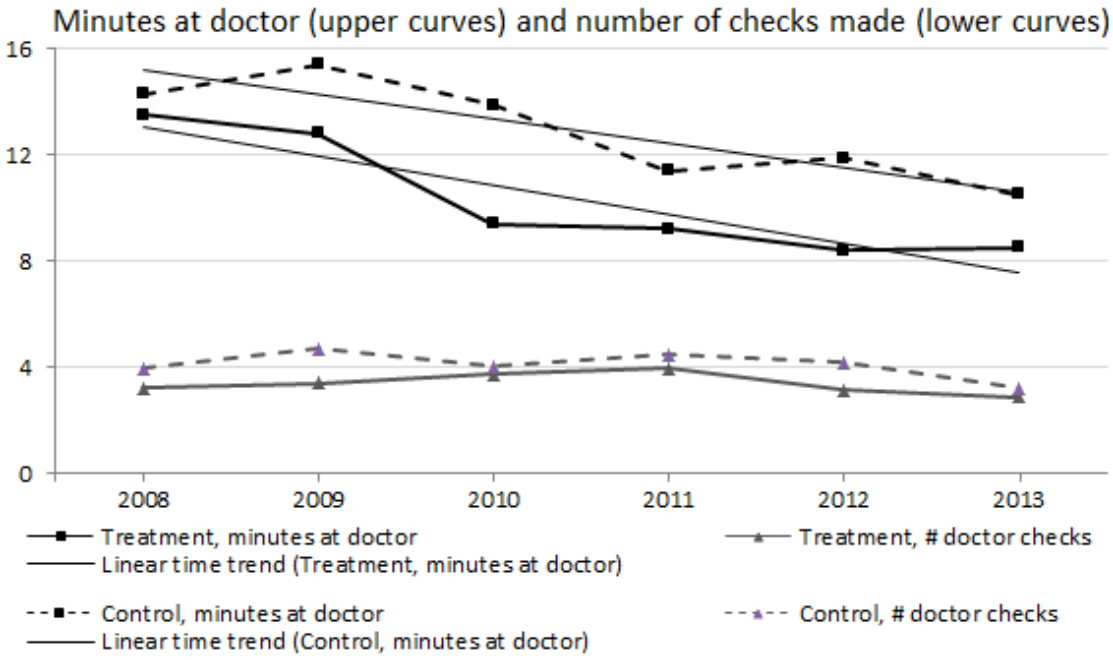
period of the study, there is a downward trend in the quality of the exam, as measured by the time it takes and its contents compared to the statutory requirement. The pattern is present in treatment and control. During and after the establishment of the 16 units, with exams done by ophthalmologists inside Poupatempo, the trend continues. The regressions in Table 5, columns 10 and 11, confirm the lack of reform impact on the two variables. Our subjective questions about whether vision and capacity to drive were correctly tested confirm this pattern (data not shown). These results contrast to the other parts of the study, as the stated social quality objectives (Governo do Estado de Sao Paulo, 2005) were not met, which we discuss in section 8.

7.5 Robustness Analyses

Of particular importance in Difference-in-Difference studies is the parallel trends assumption, which was analyzed graphically in Section 6 and in Appendix A3. Following e.g. Bertrand, Duflo, & Mullainathan (2004), we also conduct a placebo analysis, in which we should see no pre-reform "treatment effects". We thus add to the regression model pre-reform year dummies and pre-reform year×treatment interaction terms. The GLM estimates of this specification are in Table 6, where 2010 is the reference category. The table confirms the lack of differential pre-reform trends for the minutes, trips, days, information time and personal contacts variables, as the 2008 and 2009 interaction terms had otherwise been significant.[14] [15]

The results in section 7 are robust to a number of alternative specifications, including the use of additional control variables, limiting the sample to the most similar treatment and control locations or using the full sample. The estimates change little, as is shown in Appendices 6 and 7.

[14] The exception of interest is the 2008 term for the information trip dummy. Figure A5 shows the pre-reform data for this variable, where the 2008 averages are based on less data. The other two years show a parallel trend. When omitting 2008 in the Table 5 OLS and GLM regressions, we get similar impact estimates and significance levels.
[15] Each medical variable has one significant pre-reform estimate. Dropping the year of these effects does not result in significant reform estimates in Table 5. Figure 5 supports the conclusion of no reform impact on these variables.



Notes: There are eight statutory medical checks. Sample as in TT regressions. *Source:* Interviews.

Figure 5. Average Duration in Minutes of the Exam and Number of Medical Checks Made.

In particular this is true for the minutes variable. A "preferred" time-saving estimate of 95 minutes is obtained and used in a cost-benefit calculation, which is also discussed in section 8. One variable that sometimes has an insignificant reform estimate is "days" (as in Table 4, panel 1), but not when studying the effect on actual Poupatempo users (TT regressions). In contrast, "days to process" is always significant and is more relevant as a reform impact measure.

8. DISCUSSION

We first summarize some of the project specific findings, then discuss general lessons and implications. The paper evaluates a Citizen Service Center reform in Brazil's most populous state and shows that it reduces the time and resources citizens expend in interactions with the government bureaucracy. Tables 4 and 5 report large reductions in the time spent, trips made and number of days required to undertake a common licensing procedure, suggesting that the reform simplified the citizen-bureaucracy interaction and that co-location is a good idea. We also find that the reform is rather equitable, with similar take-up and reform impact in different age-, gender- and

Table 6. Estimated "treatment effects" before the reform (placebo test) and after the reform (actual treatment), for the minutes, trips, days, payments, information, personal contacts and medical exam variable (Treatment on the Treated regressions).

Dependent variables	Minutes	Minutes, w/o idle time	Trips	Days	Days to process	Payments	Information trip dummy	Time getting information	Personal contact dummy	# Medical checks	Duration of medical exam
GLM regression (link/distribution)	log / nb	log / nb	log / nb	log / nb	log / nb	log / nb	logistic / binomial	log / nb	logistic / binomial	log / nb	log / nb
Coefficient estimates											
2008×Treat	-0.0771	-0.143	-0.103	0.0230	0.282	-0.0958	-1.088*	-0.596	-0.425	-0.161	0.275
	(0.172)	(0.155)	(0.0853)	(0.213)	(0.230)	(0.297)	(0.624)	(0.425)	(0.784)	(0.170)	(0.210)
2009×Treat	0.0477	0.0291	0.0409	0.156	0.246	0.0977	-0.333	-0.0620	-0.483	-0.314*	0.293*
	(0.127)	(0.144)	(0.107)	(0.246)	(0.257)	(0.271)	(0.553)	(0.348)	(0.552)	(0.161)	(0.151)
aftertreatment	-0.425***	-0.521***	-0.781***	-1.357***	-1.677***	-0.232	-1.316***	-0.728***	-1.976***	-0.0543	0.119
	(0.121)	(0.124)	(0.0778)	(0.279)	(0.363)	(0.170)	(0.421)	(0.274)	(0.681)	(0.0982)	(0.115)
Controls:											
- Course/test and other errands	Yes	Yes	Yes	Yes	Yes	-	Yes	Yes	Yes	Yes	Yes
- Socioeconomic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression type	TT	TT	TT	TT	TT	TT	TT	TT	TT	TT	TT
Observations	580	575	581	575	574	316	576	558	398	570	578
Column	1	2	3	4	5	6	7	8	9	10	11

Notes: In these regressions, 2008-2010 is the before- and 2011-2013 the after period. The 2008-2010 data is as in Tables 4 and 5, hence contains no treated observations. The regressions include treatment/control location dummies, 2008- and 2009 year dummies (2010 is the reference period), an after dummy and the course/test/other errands- and socioeconomic controls. The personal contacts variable was collected during the last two thirds of the interviews; the estimate is thus based on less data (for this variable, in panel 2, we use a treatment group dummy instead of the full set of treatment/control location dummies, to avoid problems with perfect separation in the logistic regression). nb–negative binomial, TT–Treatment on the Treated. *Source:* Interviews.

Robust Standard Errors in parenthesis, clustered on treatment/control locations. * Statistically significant at the 10 percent level, ** Statistically significant at the 5 percent level, *** Statistically significant at the 1 percent level.

income groups. Poupatempo users report few personal contacts at the bureaucracy, which suggests that a neutral rather than particularistic handling of errands and citizens was established. Transaction costs are inherently difficult to measure but there is evidence that the One Stop Shop also improves upon how citizens inform themselves. Those using Poupatempo typically undertake fewer information-only trips, and spend significantly less time to get information, via the internet. Importantly, regressions such as Table 5, columns 7 to 8, only partly capture lower transaction costs, as e.g. better information improves also upon other outcomes. The appendix shows that the uncertainty in undertaking the errand is reduced, along with related measures. Moreover, the cost-benefit calculation in the appendix shows that, while the marginal benefit of new units is decreasing, the implementation of the 16 units as a whole led to an opportunity cost value of time saved at least as high as Poupatempo's operational cost for the licensing procedure studied.

8.1 General Lessons, Issues Discovered and Policy Implications

The above results suggest that One Stop Shops are a good idea. What limitations, issues and problems did the study find, however, constituting potential lessons for similar reforms elsewhere? A first observation relates to whether a true *One Stop* public service was implemented. In fact, only 10 percent of the treatment group Poupatempo users do one trip only. Instead, 50 percent undertakes two return trips. This turns out to be rational, in that the total time spent would have been longer, had these individuals chosen to wait at Poupatempo, instead of returning in a new trip.[16] Instead of speeding up (even more) the in-house production of personal documents, which involves security checks, the policy suggestion is rather to establish routines for mailing documents (once the identity has been verified, at the initial visit). Such a routine was implemented at the end of our study. A reliable mail service is needed, however. Even if trust in Brazil's

[16] We use information on idle time, transport times and home municipality-to-Poupatempo distance for this analysis.

mail service is high, some respondents were skeptical of getting personal documents by mail.

Interestingly, the Citizen Service Centers we studied are not really *Two Stop* Shops either. 30 percent of the treatment group Poupatempo users instead undertake three trips, which is mainly explained by information trips. As reported in section 7, information trips decreased, yet the reduction is less than for other parts, where there is a mechanical effect of co-location (e.g. bank- and medical trips). "Obtaining information" is different in nature than e.g. making a payment at a bank counter. A typical three-trip Poupatempo renewal thus has trips to obtain information, to do the medical exam and hand in the application, and to pick up the renewed license. We have slightly more men and older-than-average individuals that do information trips, which may suggest persistence of habits, even if the renewal is now done at Poupatempo, but no systematic income differences. The ratio with information trips drops a bit from 2011 to 2013 (from 30 to 20 percent), suggesting the average individual gets better informed over time (the control group has no such trend). Even if all information is available online and over the phone, more can be done. A second policy suggestion is to proactively inform citizens, for licensing procedures that allow it. An information letter was implemented at the end of our study. The information trip ratio (for the 19 individuals that got a letter) seems to drop. Overall, these results are important as they show how "supply factors", such as the production time of personal documents, and "demand factors", e.g. individual preferences with respect to waiting, jointly decide how a de facto procedure works, in an actual Citizen Service Center, and also suggest avenues for improvement.[17]

The positive reform results do not carry over to the socially relevant components of the licensing procedure studied. In a setting where a compulsory medical exam has been long known,

[17] Although a driver's license renewal requires a physical visit, the procedure is also affected by e-government developments. An example is the online scheduling of Poupatempo visits, which, post-project, has become mandatory. More generally, Brazil has some established e-government services (e.g. voting), yet other sectors have struggled to unify registries, with fraud, etc. Most errands still require physically presenting the two IDs (RG, CPF) and a proof of address. A preference for physical visits has also existed. Agarwal et al., 2017, depict the same habit in Vietnam.

anecdotally, to only comply with a small part, if at all, of its statutory requirement (Portal da Oftalmologia, 2006), the reform seems to do nothing to alter this fact. As section 7.4 shows, doctors, inside and outside of Poupatempo, before and after the reform, on average spend too short time with citizen-drivers, conducting less than half of the compulsory medical checks. The exam is poorly evaluated by citizens. In addition, over the study period, the trend is deterioration rather than the opposite. These results are troublesome, as the social controls are the reason for having a renewal requirement. Other licensing procedures also contain socially relevant components. As discussed in section 2, One Stop Shops not only aim at speed, but also to assure the social quality of procedures (Kubicek & Hagen, 2000). In São Paulo, this was stated as that services should be correctly executed by the service-providing institution (hosted inside Poupatempo), that deburocratization be sought while obeying all laws, and that the ***public character*** of services be restored (Governo do Estado de São Paulo, 2005, pp. 25, 41-42, emphasis in original).

The reason for the substandard medical exam, in general, seems straightforward: Doctors are paid per exam, and have an incentive to speed it up. The DETRAN president, who had raised the issue of non-rigorous exams prior to assuming the presidency (Annenberg, 2010), acknowledged the incentive issues. Negotiations with the physicians' employers' organization, the Brazilian Traffic Medicine Association, had not succeeded in changing the outcome. Conversations with DETRAN staff confirm that some doctors can pocket 600 R$ per hour (309 USD), and that the exam, for parts of the profession, is an important source of income, rather than a careful check-up.[18] The likely reason for no specific reform impact is that the exam responsibility, organization and supervision is under the Traffic Medicine Association, irrespective of where it is done. Unlike other entities, improving their performance once inside Poupatempo (e.g. DETRAN itself),

[18] An example of an important issue for doctors is to be part of a rotational arrangement, inside and outside of Poupatempo, as being inside is very lucrative. This is rather different from a discussion of quality aspects.

the medical exam retained a substandard quality, on average. A policy implication is to recognize that, for errands with socially relevant checks, speed of service is not a good steering instrument. In addition, and assuming that the One Stop Shop embraces a differentiation of objectives, all entities under its auspices should probably be monitored internally. The medical association would then lose autonomy, however, which echoes concerns about the extent of cooperation One Stop Shops can achieve (Askim et al., 2011). As many individuals are happy with a fast exam, citizen satisfaction is also not a good steering instrument, which may hold also for other errands.

An additional remark, potentially context-specific, is that we did not find outright corruption, but rather "soft" rule-breaking and moderate compliance with the social components. Few people will fear being rejected due to a failed medical exam, bribing someone to avoid it altogether thus seems unnecessary (although we cannot rule out some selection in our interview sample). The observed "rule flexibilization" is probably more common the more difficult it is to monitor the actions taken, e.g. by doctors (Bertrand et al., 2007, discuss this argument in the Indian context).

We next discuss some institutional and political aspects, with additional implications. In the post-reform data, around 80 percent of the treatment group individuals use the One Stop Shop. The ratio stays about constant, rather than converging to 100 percent. In part, this is because individuals that had moved to another municipality typically needed to do the renewal at a DETRAN office, as the license first had to be "transferred". Historically, driver´s licenses were registered at the local police offices and there is some path dependence affecting how services work today. Related to this explanation is that there are technological (database integration) challenges, affecting the degree to which One Stop Shops can offer complete services.[19]

An alternative interpretation is as follows: In order for a reform to succeed, it needs political

[19] Bent, Kernaghan, & Marson (1999) and Kubicek & Hagen (2000) describe similar challenges for change of residence, in Canada and Germany. Agarwal, Pfeil, & Schott (2017A) also discuss challenges with database integration.

support, and cannot be hampered by interest groups related to the legacy bureaucracy. For the licensing procedure we study, a successful reform would mean that DETRAN offices would lose clients and intermediaries lose business. Resistance to the reform can be expected, with different groups organizing and lobbying, trying to halt the Poupatempo implementation or limit its service offer. Kubicek & Hagen (2000) discuss reform inertia in Europe and Bussell (2010) finds evidence of such resistance in India. In order to circumvent some of the obstacles, Poupatempo was implemented alongside the traditional bureaucracy, and not all services were offered. Mota Prado & da Matta Chasin (2011) label this type of reform "Institutional Bypass", arguing that it succeeded because it created an alternative for citizens, without aiming for a complete replacement of the existing structures. Once implemented, the usage and popularity of Poupatempo increased, which led to an increased budget and service offer. A policy suggestion with support in the Brazilian experience is therefore that of a gradual implementation. Relating to discussions of breadth vs. depth in One Stop Shops (Askim et al., 2011; Agarwal, Pfeil, & Schott, 2017B), perhaps the limited depth we observe with respect to change of residence can be understood through this political-institutional lens, i.e. that DETRAN retains control over parts of the procedure.

The time frame of the evaluation is too short to definitely ascertain whether most errands will ultimately be handled by Poupatempo or if the reform could instead be reverted. In our mapping of One Stop Shops in different countries, we found several cases (later excluded) where we were unable to confirm if a reform had actually been implemented. In some cases, this may be a signal of "window dressing", or mimicry without an adaptation to local contexts, which affects reform sustainability (resonating with arguments in Pritchett, Woolcock, & Andrews, 2013). More generally, there are many examples of institutional reforms that have not fully fulfilled objectives. An example is the Semi-Autonomous Revenue Authorities, a popular developing country reform aiming to limit day-to-day political interference in tax collection and to increase revenue. Short-

term advances allegedly occurred, but it is unclear if the reform objectives were met (Fjeldstad & Moore, 2009; Dom, 2018). As for Poupatempo, developments posterior to our data collection project provide some guidance. Starting in 2014, additional units were planned and implemented. This reflects a high demand and that most individuals are content with the changed nature of the citizen-state interaction. The state government is also interested in expanding the service, for political and other reasons. Other authorities, including DETRAN, have adopted the "Poupatempo service standard" (a concept marketed by the state government). From this perspective, Poupatempo seems sustainable and has the political support often stressed as a reform prerequisite (e.g. Agarwal, Pfeil, & Schott, 2017A); yet future evaluations can provide further long-term insights.

A final point concerns the evaluation exercise itself. Most reforms have a compelling diagnosis of the legacy bureaucracy, yet rarely collect pre-reform data. This study relied on a combination of a reform and a licensing procedure that allowed for collection of pre- and post-reform data, which is not always possible. Even if other empirical strategies may be available, an evaluation plan should be elaborated from the start. An ongoing Colombian reform contemplates a pre-reform data collection and an impact evaluation, albeit with citizen perception data, rather than time spent (IDB, 2014). The present project can provide insights into such evaluations.

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tent failure in state capability for implementation. Journal of Development Studies, 49, 1-18.

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APPENDIX

Appendix A1 lists countries with Citizen Service Centers, A2 has more details on the data collection project, A3 contains pre-treatment graphs and A4 additional regressions referred to in the main text. A5 analyzes red tape, transparency, other transaction costs, the course/test requirement and the use of intermediaries. A6 has robustness checks and A7 a cost-benefit calculation.

A1. List of Countries With Citizen Service Centers, Including References

Table A1 lists countries that have implemented physical One Stop Shops that are primarily oriented towards citizens. Within each country, other programs may exist, and the list of countries is likely to be incomplete. The table excludes countries/centers primarily focusing on e.g. business startup and also initiatives based on a web page/gateway, but without physical centers.

Table A1. List of countries with Citizen Service Centers (non-exhaustive).

	Region	Countries	References
1	**Africa**	Angola	Aires, E. (2014). A reforma da administação pública em Angola com ênfase na qualidade dos serviços : Estudo de caso do serviço integrado de atendimento ao cidadão. Master thesis at Universidade de Lisboa, Portugal.
2		Burundi	Ministère de la Fonction Publique, du Travail et de l'Emploi, 2018. Remise des premiers documents administratifs dans les GUPs. Retrieved August 20, 2018, from http://www.ministerefptss.gov.bi/?q=content/remise-des-premiers-documents-administratifs-dans-les-gups
3		Cape Verde	UCRE (Unidade de Coordenação da Reforma do Estado), 2017. Casa do cidadão. Retrieved August 20, 2018, from http://www.reformadoestado.gov.cv/index.php/modernizacao-adminstrativa/casa-do-cidadao?showall=1&limitstart=
4		Egypt	Abdalla, A.G., Kiragu, J.K., Ono, F.T., Kariuki, J.W. & Ikua, D.M. (2015). Effect of Huduma centers (One Stop Shops) in service delivery – A case study of Mombasa Huduma centre. International Journal of Academic Research in Business and Social Sciences, 5, 102–117.
			UNDP (2015A). The political economy of public administration. A study of the Arab transitions.
5		Ghana	Agarwal, S., Pfeil, H. & Schott, B. L. (2017B). Recent developments and key considerations impacting the operations of One-Stop Shops for citizens. A summary of major trends and a design guide for Citizen Service Centers. World Bank Brief.
6		Kenya	Abdalla et al. (2015)
			Commonwealth (2016). Key principles of public sector reform. Case studies and frameworks.
			Firestone, R.S., Kinuthia, M., Omollo, A., Schott, B. (2017). Citizen Service Centers in Kenya: The role of Huduma centers in advancing citizen-centered service delivery in a context of devolution and digitization. World Bank Brief.
7		Lesotho	Agarwal, Pfeil, & Schott (2017B)
8		Liberia	UN (2017). Government of Liberia, UN and partners open service centers in Maryland and Sinoe counties. Retrieved August 20, 2018, from http://lr.one.un.org/content/unct/liberia/en/home/one-voice/news/Government_of_Liberia_UN_and_Partners_Open_Service_Centers_in_Maryland_and_Sinoe_Counties.html
9		South Africa	Accenture (2005). Leadership in Customer Services: New Expectations, New Experiences. The Government Executive Series.
10		Tunisia	UNDP (2015A)
11	**Asia**	Afghanistan	UNDP (n.d.). Enabling responsive governance, cutting time in service delivery. Retrieved August 20, 2018, from http://www.af.undp.org/content/afghanistan/en/home/ourwork/democraticgovernance/successstories/CustomerService.html
12		Bangladesh	Login ASIA (2016). One-Stop Shops as a Mode of Public Service Delivery. Experience Collation. Local Governance Initiative and Network.
13		Bhutan	SDC (2010). One Stop Shops – in the service of the population of Vietnam. Swiss Agency for Development and Cooperation Asia Briefing Paper Series.
			Login ASIA (2016)
14		Cambodia	Post, D. & Agarwal, S. (2011). Citizen Service Centers: Enhancing access, improving service delivery, and reducing corruption." How-To Notes, Social Development. World Bank.
			World Bank (2013). Citizen Service Centers - Systems and processes. Demand for Good Governance (DFGG) Learning Note 13.
15		Hong Kong	PwC (2012). Transforming the citizen experience. One Stop Shop for public services. PricewaterhouseCoopers Australia.
16		India	Bussell, J.L. (2010). Why get technical? Corruption and the politics of public service reform in the Indian states. Comparative Political Studies, 43, 1230–1257.
			Post & Agarwal (2011)
17		Laos	SDC (2010)
			Login ASIA (2016)
18		Macau	Agarwal, Pfeil, & Schott (2017B)

Note: The complete reference is stated the first time it appears in the table.

Table A1 (continued). List of countries with Citizen Service Centers (non-exhaustive).

	Region	Countries	References
19	**Asia**	Malaysia	Accenture (2005)
20		Mongolia	SDC (2010)
			ILO (2016). A one-stop shop for accessible, transparent and efficient public service delivery. ILO Social Protection Brief.
21		Myanmar	UNDP (2015B). One Stop Shops across Myanmar – A step towards bringing services closer to people. United Nations Development Program.
22		Pakistan	Login ASIA (2016)
23		Vietnam	SDC (2010)
			Agarwal, S., Blunt, M., Davidsen, S., Pfeil, H. & Schott, B. L. (2017). One-Stop Shops in Vietnam: Changing the face of public administration for citizens and businesses through a single door to multiple services. World Bank Brief.
24	**EurAsia**	Armenia	Post & Agarwal (2011)
25		Azerbaijan	ACSH (2016). One-Stop public service delivery model: The case of Azerbaijan. Regional Hub of Civil Service in Astana.
26		Georgia	ACSH (2017). One-Stop public service delivery model: The case of Georgia. Regional Hub of Civil Service in Astana.
27		Kazakhstan	Janenova, S. & Kim, P.S. (2016). Innovating public service delivery in transitional countries: The case of One Stop Shops in Kazakhstan. International Journal of Public Administration, 39, 323–333.
			Jarosiewicz, A. (2016). Perestroika - the Nazarbayev way. Crisis and reforms in Kazakhstan. OSW Centre for Eastern Studies Point of View, 58.
			Janenova, S. & Yesdautelov, I. (2017). Innovative public service reforms: What Kazakhstan can learn from Canada? Manuscript.
28		Russia	Gallagher, L.J., Struyk, R.J. & Nikonova, L. (2003). Savings from integrating administrative systems for social assistance programmes in Russia. Public Administration and Development, 23, 177-195.
29		Tajikistan	Login ASIA (2016)
30		Ukraine	USAID (2015). President participates in opening of the Citizens Service Center in Odesa - a symbol of government reforms and transparency. United States Agency for International Development. October 16, 2015. Retrieved July 27, 2016, from http://radaprogram.org/en/content/president-participates-opening-citizens-service-center-odesa-symbol-government-reforms-and
31		Uzbekistan	Agarwal, Pfeil, & Schott (2017B)
32	**Middle East**	Abu Dhabi	Wiseman, J. (2017). Improving service delivery through information integration: Building a single view of the citizen. Interamerican Development Bank. Discussion Paper IDB-DP-507.
33		Kuwait	Wettenhall, R. & Kimber, M. (1996). One-stop Shopping: Notes on the concept and some Australian initiatives, Public Sector Articles 2/96. Canberra: Centre for Research in Public Sector Management, University of Canberra.
34		West Bank	Global Communities (2014). Improving local governance in the West Bank. Brief.
35	**Europe**	Albania	Hart, C., Mullahi, C., 2017. Delivering customer care and cutting corruption in public services. A case study on citizen-centric service delivery reform in Albania. Harvard University, Center for International Development report.
36		Austria	Kubicek, H. & Hagen, M. (2000). One Stop government in Europe: An overview. In M. Hagen & H. Kubicek (Eds.), One Stop government in Europe. Results from 11 national surveys (pp. 1–36). Bremen, Germany: University of Bremen.
			Torres, L. (2004). Trajectories in public administration reforms in European continental countries. Australian Journal of Public Administration, 63, 99–112.
37		Belgium	Kubicek & Hagen (2000)
38		Bosnia Herzegovina	Global Communities (2014)
39		Croatia	Contiades, X. (2007). Information Centers and One-Stop-Shops. Albania, Montenegro, Croatia. Council of Europe CARDS Social Institutions Support Project report.
40		Cyprus	Alexandrou, M. (2008, October 15). One-stop-shops at the service of citizens - Cyprus Public Administration. [Blog post]. Retrieved June 21, 2017, from https://joinup.ec.europa.eu/document/one-stop-shops-service-citizens-cyprus-public-administration-cscs
41		Denmark	Kubicek & Hagen (2000)
			Torres (2004)
			Askim, J., Fimreite, A.L., Moseley, A. & Pedersen, L.H. (2011). One-stop shops for social welfare: The adaptation of an organizational form in three countries. Public Administration, 89, 1451–1468.
42		Finland	Kubicek & Hagen (2000)
			Caulfield, J. (2004). Measuring autonomy in social security agencies: A four country comparison. Public Administration and Development, 24, 137-145.
			Torres (2004)
43		France	Kubicek & Hagen (2000)
			ACSH (2017)
44		Germany	Kubicek & Hagen (2000)
			Jacumeit, V. (2002). Administration 2000 - Networking municipal front and back offices for One-Stop government. In R. Traunmüller and K. Lenk (Eds.), EGOV 2002 (pp. 157–162). Berlin Heidelberg, Germany: Springer Verlag.
45		Greece	Pateli, A. & Philippidou, S. (2008). Public management change and One-Stop government: Experience from the Greek Citizen Service Center (CSC): Proceedings, 2nd Int´l Conference on Methodologies, Technologies and Tools enabling e-Government. Corfu, Greece.
46		Hungary	Kovács, É. & Hajnal, G. (2014). Government windows: One-Stop Shops for administrative services in Hungary. Organizing for coordination in the public sector: Practices and lessons from 12 European countries, 237-247.
47		Ireland	Kubicek & Hagen (2000)
			Accenture (2005)
48		Italy	Kubicek & Hagen (2000)
			Pateli & Philippidou (2008)
49		Kosovo	Agarwal, S., Pfeil, H. & Schott, B. L. (2017A). Municipal Citizen Service Centers in South-eastern Europe: Survey results on success factors, challenges, and the human rights approach of municipal One-Stop Shops in the Western Balkans. World Bank Brief.
50		Macedonia	Agarwal, Pfeil, & Schott (2017A)
51		Montenegro	Agarwal, Pfeil, & Schott (2017A)
52		Netherlands	Kubicek & Hagen (2000)
			Caulfield (2004)
			Torres (2004)

Note: The complete reference is stated the first time it appears in the table.

Table A1 (continued). List of countries with Citizen Service Centers (non-exhaustive).

	Region	Countries	References
	Europe	Netherlands (cont´d)	Contiades (2007
53		Norway	Torres (2004)
			Askim et al. (2011)
54		Portugal	Esteves de Araújo, J.F. (2001). Improving public service delivery: The crossroads between NPM and traditional bureaucracy. Public Administration, 79, 915–932.
			OECD (n.d.). Citizen Shops. "Lojas do Cidadão". OECD Observatory of Public Sector Innovation. Retrieved July 7, 2017, from https://www.oecd.org/governance/observatory-public-sector-innovation/innovations/page/citizenshops.htm
55		Serbia	Agarwal, S., Kumagai, S., Pfeil, H. & Schott, B. L., 2017. The city of Pančevo's Citizen Service Center, Serbia. Streamlining service delivery and fostering inclusion at the municipal level. Report from the World Bank/Nordic Trust Fund project "Effectice Citizen Service Centers and human rights: Mutually reinforcing dynamics".
56		Slovak Republic	ACSH (2017)
57		Spain	Córdoba, A. E. (1999). El proyecto "Ventanilla Única": Una experiencia de acercamiento al ciudadano basada en la cooperación y la tecnologia. Revista IberoAmericana de Administratión Pública, 3, 93–105.
			Kubicek & Hagen (2000)
			Pateli & Philippidou (2008)
			Malyshev, N. (n.d.). The evolution of regulatory policy in OECD countries. Organization for Economic Co-Operation and Development.
58		Sweden	Torres (2004)
			Skatteverket (2007). Servicekontor – Det personliga mötet. Swedish Tax Authority Report.
59		Switzerland	Torres (2004)
60		United Kingdom	Bellamy, C. (1996). Transforming social security benefits administration for the twenty-first century: Towards One-Stop services and the client group principle? Public Administration, 74, 159–179.
			Kubicek & Hagen (2000)
			Wiggan, J. (2007). Reforming the United Kingdom's public employment and social security agencies. International Review of Administrative Sciences, 73, 409–424
			Kernaghan, K. (2010). International innovations in public sector external service delivery.
			Askim et al. (2011)
61	**North America**	Canada	Bent, S., Kernaghan, K. & Marson, D.B. (1999). Innovations and good practices in single-window service.
			Marson, B. & Heintzman, R. (2009). From research to results: A decade of results-based service improvement in Canada.
			PwC (2012)
62		USA	Holcomb, P., Seefeldt, K. & Trutko, J. (1993). One Stop Shopping service integration: Major dimensions, key characteristics and impediments to implementation. Washington, DC: Urban Institute.
			SPR (1997). Creating workforce development systems that work: An evaluation of the initial One-Stop implementation experience. Final Report. Washington, DC: Social Policy Research Associates.
			Finn, D. (2000). Welfare to work: The local dimension. Journal of European Social Policy, 10, 43–57.
			Ryu, J.E. & Rainey, H.G. (2008). Collaborative public management and organization design: One-Stop Shopping structures in employment and training programs. In R. O'Leary & L.B. Bingham (Eds.), The collaborative public manager: New ideas for the twenty-first century (pp. 177-194). Washington, D.C.: Georgetown University Press.
			Mathematica (2015). Coordinating employment services across the TANF and WIA programs. Mathematica Policy Research Report.
63	**Latin America**	Brazil	MARE (1998). Serviço integrado de atendimento ao cidadão.Ministry of Federal Administration and State Reform. Cadernos MARE 17.
			Paulics, V. (2003). Poupatempo, Central de atendimento ao cidadão.
			Governo do Estado de São Paulo (2005). Reconstruindo valores públicos: Padrão Poupatempo em recomendações.
			Annenberg, D. (2006). Poupatempo program: The citizen service center and its innovations. Presentation, World Bank, Shared Service Delivery Infrastructure: Single Window Citizen Service Centers, 30 May 2006. Retrieved July 27, 2016, from http://web.worldbank.org/WBSITE/EXTERNAL/TOPICS/EXTINFORMATIONANDCOMMUNICATIONANDTECHNOLOGIES/EXTEDEVELOPMENT/0,,contentMDK:20920866~menuPK:559467~pagePK:64020865~piPK:149114~theSitePK:559460,00.html
			Ferrer, F. (2006). Avaliação de custos pela inovação na prestação de serviços: Atestado de antecedentes criminais eletrônicos.
			Mota Prado, M. & da Matta Chasin, A.C. (2011). How innovative was the Poupatempo experience in Brazil? Institutional bypass as a new form of institutional change. Brazilian Political Science Review, 5, 11–34.
			Post & Agarwal (2011)
			Scharff, M. (2013). A higher standard of service in Brazil: Bahia's one-stop shops, 1994–2003. Innovations for Successful Societies, Princeton University.
			Majeed, R. (2014). A second life for one-stop shops: Citizen services in Minas Gerais, Brazil, 2003–2013. Innovations for Successful Societies, Princeton University.
			Schifnagel Avrichir, A. (2018). Uma análise de incentivos contratuais em arranjos de parceria de atendimento ao cidadão. Revista de Administração Pública, 52, 1214-1236.
64		Chile	Chileatiende (2014). Diagnóstico y propuesta de desarrollo ChileAtiende.
			Gobierno de Chile (2014). ChileAtiende: un relato de personas al servicio de personas. In Gobierno digital en acción: Proyectos
65		Colombia	Post & Agarwal (2011)
			IDB (2014). Colombia. Citizen service efficiency project. Interamerican Development Bank.
66		Ecuador	Agarwal, Pfeil, & Schott (2017B)
67		Trinidad and Tobago	Agarwal, Pfeil, & Schott (2017B)
68		Uruguay	Agarwal, Pfeil, & Schott (2017B)
69	**Oceania**	Australia	Bellamy (1996)
			Wettenhall & Kimber (1996)
			Askim et al. (2011)
			Post & Agarwal (2011)
			PwC (2012)
70		New Zealand	New Zealand Government (1999). Integrated service delivery. State Services Commission Occasional Paper No. 12.

Note: The complete reference is stated the first time it appears in the table.

A2. FURTHER DETAILS ON THE EMPIRICAL PROJECT

A2.1 Selection of Licensing Procedure and Empirical Strategy

In choosing a licensing procedure about which to interview citizens, we required that it should be one of the most common errands at the government bureaucracy, and, post-reform, available at Poupatempo. Furthermore, the licensing procedure should allow for a before- and after data collection. As discussed in section 3, it is compulsory to renew the driver's license every five years (in fact, every three years, for individuals above 65). The (individual) renewal date will ultimately depend on when one first got the license, together with renewal rule changes that have occurred over time (and how these rules have been followed and enforced). The nature of the renewal requirement, together with the timing of the Poupatempo reform, thus made it possible to interview individuals at "one point in time", in 2013, yet obtaining before- and after data.

Identifying the effect of a One Stop Shop reform requires more than before- and after data, however, as we do not want to attribute to the reform such variation that results from other changes. These considerations made us opt for a Difference-in-Differences (DD) strategy, with the aim of collecting pre- and post- reform data in many Poupatempo and non-Poupatempo locations. All locations would need to be in the interior of the state, as there could be no pre-reform data in the metropolitan area, where Poupatempo was first established in 1997. We thus opted to collect data in all 16 interior São Paulo municipalities with Poupatempo implementations from 2008 to 2011. This is the treatment group used in the analysis.

A2.2 Selection of Control Group Interview Municipalities

The control group interview municipalities were selected using a propensity score matching (PSM) procedure, following Caliendo & Kopeinig (2008), the result of which is in the below Table A2. We first obtained from Poupatempo the technical considerations that were important

when choosing where to implement a unit. These criteria, primarily municipality population and a dummy for how dense a region is, and potentially whether a city was a regional capital, explain 50 to 60 percent of the variation in the Poupatempo dummy. We added to these regressions other control variables that were also significant, and that could have an impact on the outcomes under study (e.g. time spent). Poupatempo also informed that a lower population threshold of 100.000 was used to consider a city to be a candidate to get a unit, which was somewhat counterfactual, as Caraguatatuba, with 94.000 inhabitants, got a unit. In the regressions, we therefore included also municipalities with a smaller population, down to 67.000, resulting in 58 candidate municipalities (out of the total of 606 municipalities in the interior of the state).[20]

In different PSM specifications, using linear or logit models, we thus controlled for municipality population or a correlate thereof; regional population density, which, conditional on municipality population, should have a negative effect, as one objective was to spread out the Poupatempo units, rather than a concentration in one region; a dummy for being a regional capital; population growth; and a political variable, showing if the state governor's party (the Social Democracy Party, PSDB, in power since 1994) also held the mayor office in the municipality. Our preferred specification[21] gave a region of common support with 13 control municipalities. We added a small capital city (Registro) and a populous city (Guarujá), for a total of 15 municipalities, constituting the control group in all regressions in the main text (one of the

[20] The selection of control group interview municipalities used the following data sources (in addition to the sources described in section 4): the São Paulo state data entity (SEADE), the state cartography entity (IGC), the national traffic authority (DENATRAN), the Superior Election Tribunal (TSE), Google Maps and OpenStreetMap, DETRAN data on despachantes, information from Poupatempo, and Statistics Brazil census information.

[21] The Poupatempo dummy (Table A2, column 3) regressed, in a logit specification, on the pre-reform number of renewals (which is correlated with population), the regional density of despachantes per area (proxying for population- and business density, in addition to that having more despachantes may, in itself, imply a lower probability to get a Poupatempo), an "Administrative Capital" dummy, population growth (2003-2007 average) and a variable equaling the number of pre-reform election periods (2000-2004, 2004-2008) with PSDB in power (0, 1 or 2).

Table A2. Results of propensity score regression. Treatment and control municipalities.

1		2	3	4	5	6	7
Municipality	*(implementation date)*	**Population 2007**	**Poupatempo**	**Predicted score**	**Common support**	**Treatment/Control**	**Interviews conducted?**
Registro		54380	0	-		C[i]	YES
Várzea Paulista		102575	0	1.19E-06		T[ii]	
Americana		202406	0	4.80E-06		C[iii]	
Bebedouro		75218	0	0.000021			
Cubatão		115882	0	0.0000466			
Guarujá		283414	0	0.0000487		C[iv]	YES
Salto		101814	0	0.0000633			
Cruzeiro		76133	0	0.0000715			
Itapira		67137	0	0.0000987			
Lorena		81224	0	0.0001577			
Jaboticabal		70627	0	0.0003934			
Leme		88568	0	0.0004194			
Votorantim		105210	0	0.0005091		T[v]	
Ubatuba		75484	0	0.0005866			
São Sebastião		69024	0	0.0005995			
São João da Boa Vista		81984	0	0.0008171			
Matão		75613	0	0.0008273			
Assis		92686	0	0.0011368			
Guaratinguetá		110004	0	0.0014903			
São Vicente		324003	0	0.0038767			
Pindamonhangaba		140881	0	0.0067888			
Itapeva		86966	0	0.0091502			
Lins		69815	0	0.0092215			
Mogi Guaçu		133497	0	0.0100733		C[vi]	YES[vi]
Itapetininga		139055	0	0.012338			
Avaré		80992	0	0.013691			
Birigui		104238	0	0.0156181			
Araras		114237	0	0.0196408			
Itatiba		95324	0	0.020914			
Itu		148619	0	0.0249879			
Praia Grande		240918	0	0.0570719	CS	C	YES
Catanduva		110733	0	0.0871529	CS	C	YES
Santa Bárbara d'Oeste		177202	0	0.0965	CS	C	YES
Moji Mirim		85006	0	0.1010544	CS	C	YES
Itanhaém		82610	0	0.1156586	CS	C	YES
Pirassununga		68502	0	0.1430318	CS	C	YES
Barretos		109525	0	0.1946175	CS	C	YES
Bragança Paulista		140374	0	0.2157976	CS	C	YES
Limeira		268419	0	0.2567883	CS	C	YES
Ourinhos		100350	0	0.3499769	CS	C	YES
Votuporanga		81953	0	0.3753468	CS	C	YES
Indaiatuba		184663	0	0.407775	CS	C	YES
Jaú		125364	0	0.4778276	CS	C	YES
Rio Claro	*Dec-2010*	180672	1	0.0495706	CS	T	YES
Tatuí	*Dec-2010*	103231	1	0.3287283	CS	T	YES
Caraguatatuba	*Oct-2010*	94099	1	0.3580895	CS	T	YES
Botucatu	*Jan-2011*	121534	1	0.4287498	CS	T	YES
Santos	*Oct-2008*	420107	1	0.8401568		T	YES
Presidente Prudente	*Dec-2010*	202480	1	0.9797015		T	YES
Araçatuba	*Feb-2011*	178059	1	0.991703		T	YES
Araraquara	*Oct-2010*	200588	1	0.9948919		T	YES
Taubaté	*Jan-2010*	268360	1	0.9973184		T	YES
São Carlos	*Dec-2010*	213169	1	0.9995571		T	YES
São José do Rio Preto	*Feb-2009*	392682	1	0.9996492		T	YES
Marília	*Feb-2011*	211119	1	0.9997252		T	YES
Jundiaí	*Oct-2009*	355627	1	0.9999726		T	YES
Piracicaba	*March-2010*	354214	1	0.9999956		T	YES
Franca	*Dec-2010*	309996	1	0.9999979		T	YES
Sorocaba	*Nov-2011*	558377	1	1		T	YES

Notes: Column 3 shows if a municipality has a Poupatempo. Regressed on the explanatory variables, its predicted value is in column 4. Column 5 shows the overlap in predicted score ("Common Support"), column 6 the treatment and control locations (see details in *table footnotes* i-vi), column 7 the interview municipalities.

Table footnotes: i. Registro is a regional capital, and all other such capitals were included, ii. Várzea Paulista is a twin city to Jundiaí. Residents are classified as those of Jundiaí, iii. Americana is a twin city to Santa Bárbara d'Oeste, and residents are classified accordingly, iv. Guarujá is a large city excluded by the algorithm where we chose to interview, v. Votorantim is a twin city to Sorocaba, and residents are classified accordingly, vi. Mogi-Guaçu is a twin city to Moji-Mirim. Interviews in the Mogiana region were divided between the two cities.

below robustness checks uses only the 13 municipalities, with results unchanged).[22] Table A2 lists the candidate municipalities, population, whether the municipality has a Poupatempo (the Poupatempo municipalities are at the bottom, with implementation dates in column 1), the propensity score from the preferred specification, the common support region, a Treatment/Control column that also considers factors such as if cities are immediate neighbors, and the interview locations. We ran many alternative linear and logit PSM regressions, with similar results.

A2.3 Comparison of Treatment and Control Group Interview Municipalities

As is clear from the above, Poupatempo was not randomly allocated. The treatment municipalities are typically larger and somewhat richer than the control group interview municipalities, as shown by the first set of municipality indicators in Table A3. Next, the three growth indicators show no difference for population, but faster GDP growth in the treatment municipalities, and the opposite for automobiles/capita. If these variables affect interactions at the bureaucracy, the differential growth rates might pose a threat to the identification strategy. The last two rows show insignificant differences in two bureaucracy related indicators, the fraction of individuals with no birth certificate and the frequency of driver's license renewals. The most crucial part of DD is the parallel trends assumption, discussed in sections 6, 7.5 and the below section A3.

A2.4 Interview Localities, Pre-study, Interviews and Sample Representativeness

The study aimed to interview a representative sample of driver's license holders in the interior of São Paulo state. The ideal sample would be to randomly draw individuals from the DETRAN register of all driver's license holders (potentially adjusted for the exact definitions of the

[22] Registro is a regional capital, with a population less than 67.000, hence was excluded from the regression. As all other regional capitals were included, however, we decided to also include Registro. Guarujá, the most populous municipality not selected, is penalized by the algorithm, as it is situated in a dense region. Being surrounded by water, however, the city is more isolated than what is reflected by a density variable (average travel speed is low), which is why it was included (note that São Vicente, more populous than Guarujá, is a twin city to Santos).

Table A3. Summary statistics, treatment and control locations.

Municipal data (N=31)	Year	All	Treatment (N=16)	Control (N=15)	Difference		Data source
					P-value	Significant at 5(10)%	
Population	2007	203k	260k	141k	0.004	YES	SEADE - state data entity
Household head income (R$)	2000	972	1063	875	0.001	YES	SEADE - state data entity
Human Development Index	2000	0.822	0.832	0.811	0.005	YES	SEADE - state data entity
Education (years)	2000	7.5	8.02	7.34	0.001	YES	SEADE - state data entity
GDP/capita (R$)	2003	11054	12325	9699	0.065	(YES)	SEADE - state data entity
# businesses/1000 inhabitants	2007	23.6	25	22.1	0.093	(YES)	SEADE - state data entity
Autombiles/capita	2007	0.264	0.287	0.239	0.046	YES	DENATRAN - national transit dept.
Inhabitants/bank branch	2003	9216	8606	9865	0.124	NO	SEADE - state data entity
Population growth (%, yearly)	2003-2007	1.051	1.05	1.052	0.794	NO	SEADE - state data entity
Nominal GDP/capita growth (%, yearly)	2003-2009	9.76	10.7	8.79	0.012	YES	SEADE - state data entity
Automobiles/capita growth (%, yearly)	2003-2007	9.73	8.9	10.6	0.005	YES	DENATRAN - national transit dept.
Driver's license renewals/capita	2008(Q1-2)	2.68%	2.73%	2.62%	0.646	NO	DETRAN - state transit dept.
No birth certificate	2010	0.55%	0.46%	0.64%	0.322	NO	IBGE- Statistics Brazil

Sources: See table.

treatment and control groups), and then interview these individuals, but we did not get access to the confidential information needed for such a sampling. The sample was instead collected "in the field". We decided early on to interview primarily inside shopping malls, as these gather a large and diverse public. We interviewed on weekends, when population representativeness further increases. The malls are typically reached by car, which was in line with the objective. A list was made of all malls in the interview municipalities, if there was more than one a random selection was made. We then requested interview permission. If denied, another mall in the same municipality was contacted, and so forth. This resulted in interview permission in malls in 21 municipalities, out of 25 Treatment/Control municipalities with a mall. The permissions were crucial, in order to be able to interview during four to six hours. Mall employees were not interviewed. A pre-study was made, comprising 25 live interviews. Enumerators were then hired and trained extensively, including live test interviews, and could give feedback on the questionnaire. A typical interview day consisted of five to six interviews for each of four enumerators, in a given municipality.

Enumerators were assigned a physical interview spot and had been trained to approach "every

x-th" adult individual coming from a specific direction (think of a shopping mall corridor, or a shopping street), where x depended on the amount of people around. Enumerators introduced the project, and asked if an interview could be conducted. The project leader was present, controlling that the "x-rule" was followed. If there were few people, the instruction was to approach every adult individual. Minor deviations may exist in how well the rule was followed, but, at large, the deviations should be minor, and the enumerators were committed to the project throughout. On a few occasions, interview localities were changed ad-hoc, if there were too few people. Having classified malls in terms of the socioeconomic characteristics of the public attracted, there was a concern we would get a slightly too rich sample. Mid-project, we therefore compared family income of our interview individuals (those with a car in the household, which was 91 percent), to the corresponding individuals in the Statistics Brazil data (for urban areas in interior São Paulo). The deviation was small, as we had also interviewed in malls catering to the low-end of the spectrum and in shopping streets ("calçadão"), public squares and parks. The ratio of such interviews was increased slightly in the remaining interview municipalities. A municipality interview day typically consisted of first doing 8 to 12 interviews "in the street", then 8 to 12 interviews in the mall. Half of the final sample is mall interviews, the other half mainly from shopping streets.

An interview took on average 25 to 30 minutes. Upon completion, interviewees were given a 20 R$ (10.3 USD) gift card, for participating. The cards were presented, at the start of the interview, as a compensation for the time spent with enumerators. The percentage of individuals accepting to be interviewed, of those that stopped to listen to the introductory phrases of the project, was around 60 percent. Individuals were interviewed if they had made their last renewal in the interior of São Paulo state, after March 2008, and lived in the interior of the state, excluding those living in the four municipalities that had Poupatempo prior to 2007 (Bauru,

Campinas, Ribeirão Preto, São José dos Campos). We excluded professional drivers, as these have a different procedure. After interviews, questionnaires were controlled for completeness and consistency. Interviewees were sometimes contacted by phone to gather missing data or correct errors. 720 interviews were done in 31 municipalities, during 20 weekends, from March 23 to August 31, 2013.

Table A4 shows interview project data. Income/education differences are in line with Table A3, although insignificant, and the data has age- and gender averages (42.3 years, 63 percent men) similar to the DETRAN data (43.9 years, 66 percent men). This comparison is for the data-sets' overlapping period. Figure A1, panel A, shows that the income distribution is similar to the Statistics Brazil data, and panel B a temporal renewal distribution similar to the DETRAN data. Lastly, the proportion of individuals using Poupatempo in the interview data (Figure 4) is very similar to the proportion in the population (from the DETRAN data, not shown).[23]

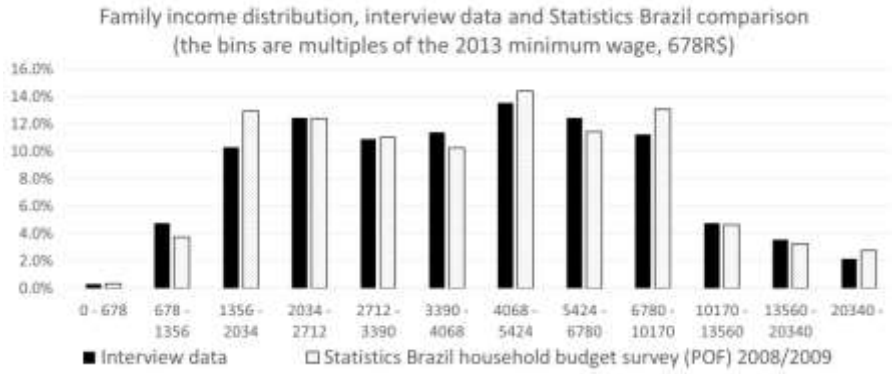
Table A4. Summary statistics, treatment and control locations (interview data).

Interview data	All	Treatment	Control	P-value
# Interviews	720	358	362	-
# Interview locations	31	16	15	-
% of sample living in interview location	82%	93%	72%	-
# municipalities where interviewees live	117	31	86	-
Interview-weighted municipality population (2007)	185k	252k	120k	-
Age	42.3	43	41.7	0.157
Fraction men	0.62	0.599	0.64	0.256
Individual income (2013 R$)	3016	3186	2850	0.161
Fraction with college/university education	0.44	0.459	0.425	0.363
Hours worked/week	37.8	37.5	38.1	0.707
Critério Brasil	28.5	28.7	28.3	0.349
Fraction household with car	0.91	0.901	0.913	0.57

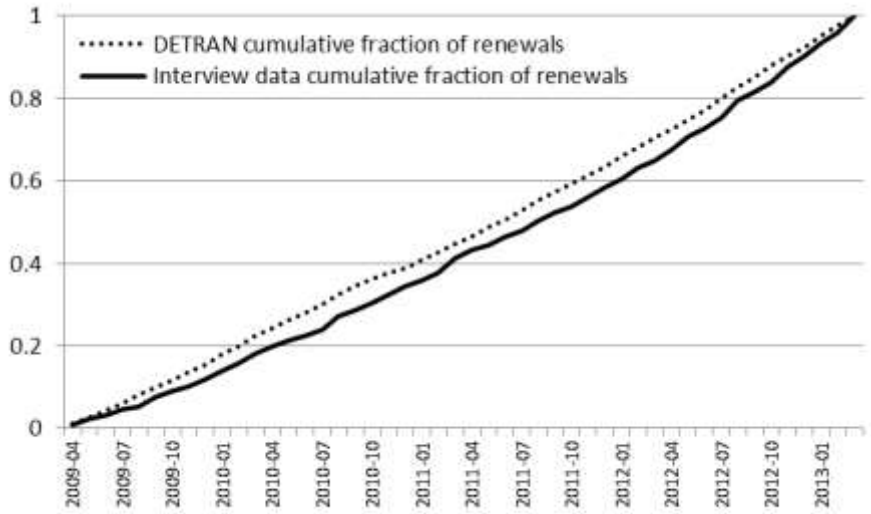
Note: Critério Brasil is a socioeconomic (education/assets) index. *Source:* Interviews.

[23] We cannot rule out that our sample somehow differs from the population of drivers at large, as to the renewal behavior. Some individuals, e.g. those that never stroll, are not part of the sample. Weekend shopping is very common, however. With a large build-out of commercial spaces, frequently led by São Paulo, Brazil is often characterized as a consumption-centered society (which GDP shares confirm). The "new middle class" has acquired many consumer habits of the upper classes. The current (2014–) economic crisis began after our interviews.

A



B

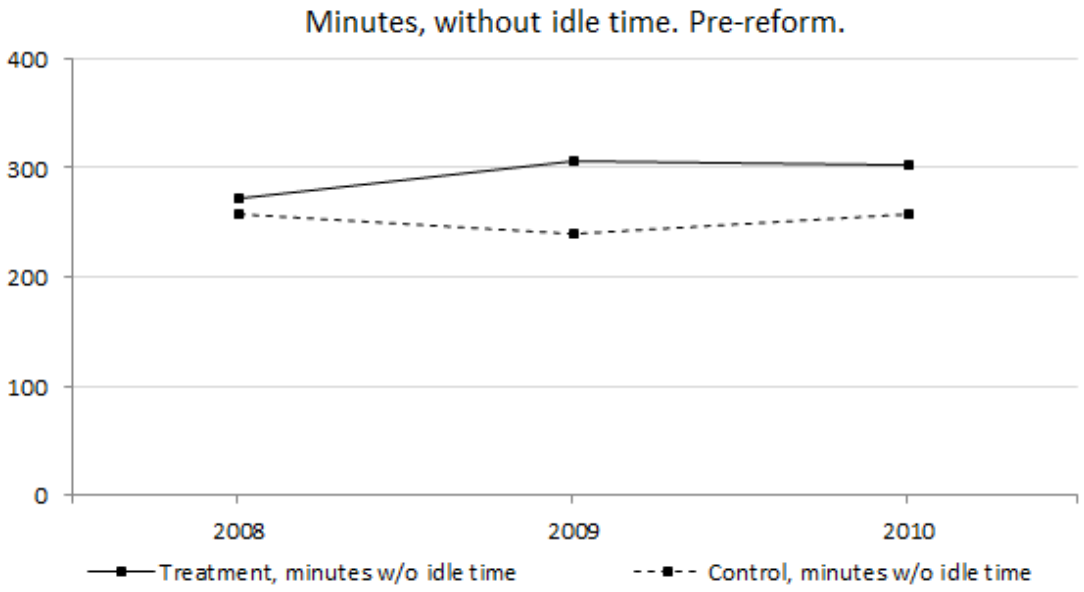


Notes: (A) The Statistics Brazil (IBGE) data is the 2008–2009 household budget survey (Pequisa Orçamental Familiar, POF) for interior São Paulo urban areas, inflated to 2013 using the IPCA index. Families with at least one car. 678 R$ is the 2013 minimum wage. The POF sample weights were not used. 1 USD=1.94 R$ (average 2008–2013). *Sources:* Interviews, IBGE, DETRAN.

Figure A1. (A) Family Income Distribution, Interview- and Statistics Brazil Data. (B) Cumulative Renewal Fraction, for the Period of Overlap Between the Interview- and DETRAN Data.

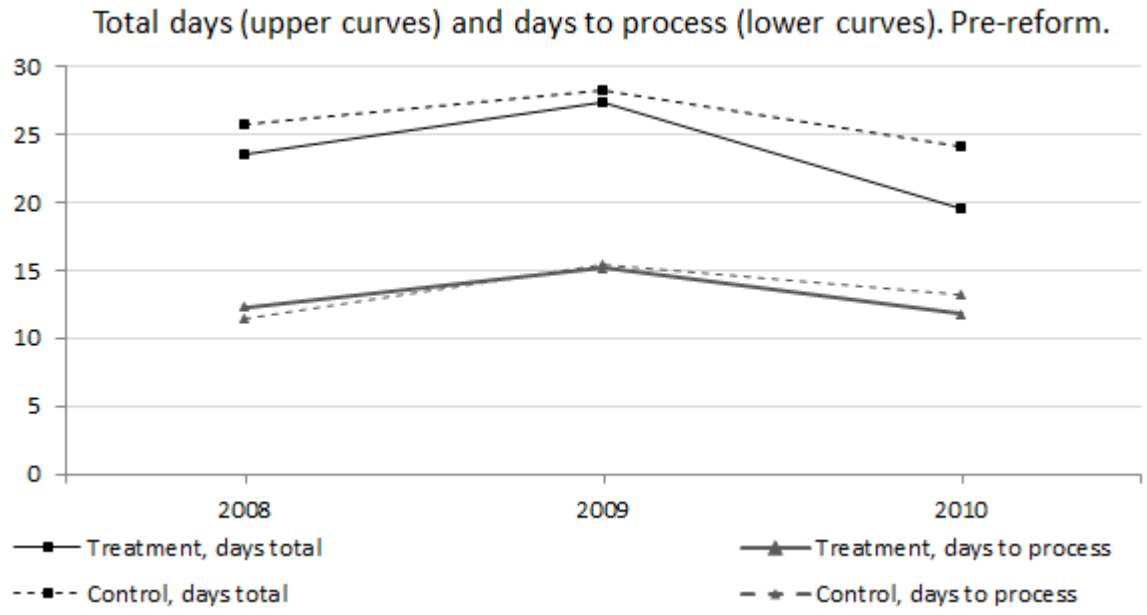
A3. PRE-TREATMENT GRAPHS

Figures A2 to A7 complement the "minutes" and "trips" data in Figure 3 (and the medical variables in Figure 5) and show pre-reform averages for the remaining outcomes analyzed in section 7. The data is the same as in Figure 3 and the 2008 to 2010 data in Tables 4/5/6. In these graphs, the treatment and control groups show no markedly different trends (see also the placebo regressions in section 7.5).



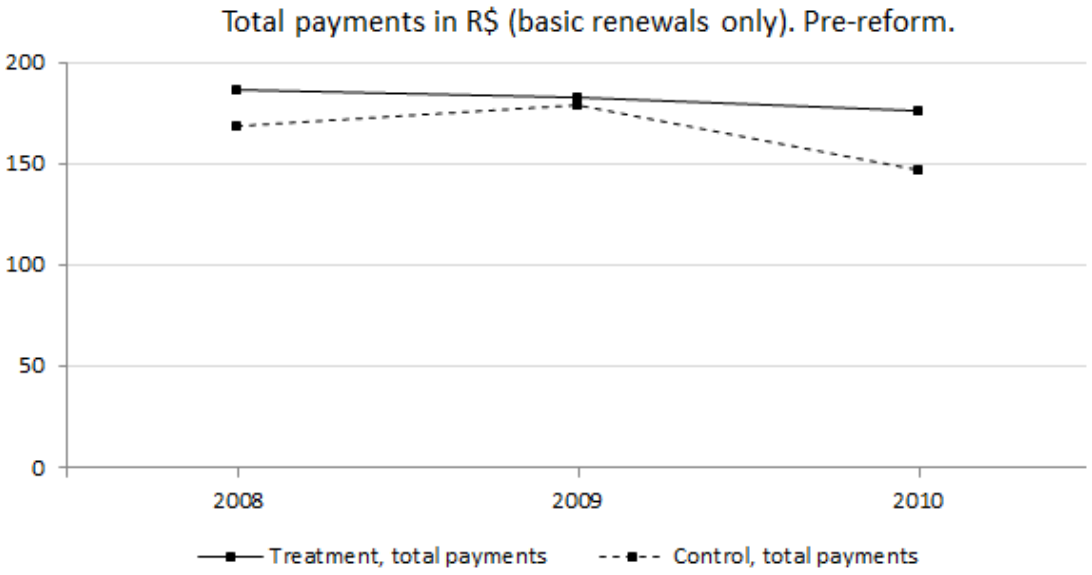
Notes: Once treated, the 2008–2010 Poupatempo municipalities are excluded, as are a few control group observations, due to take-up. *Source:* Interviews.

Figure A2. Pre-treatment Graph for the Average Time Spent With the Renewal, Excluding Idle Time.



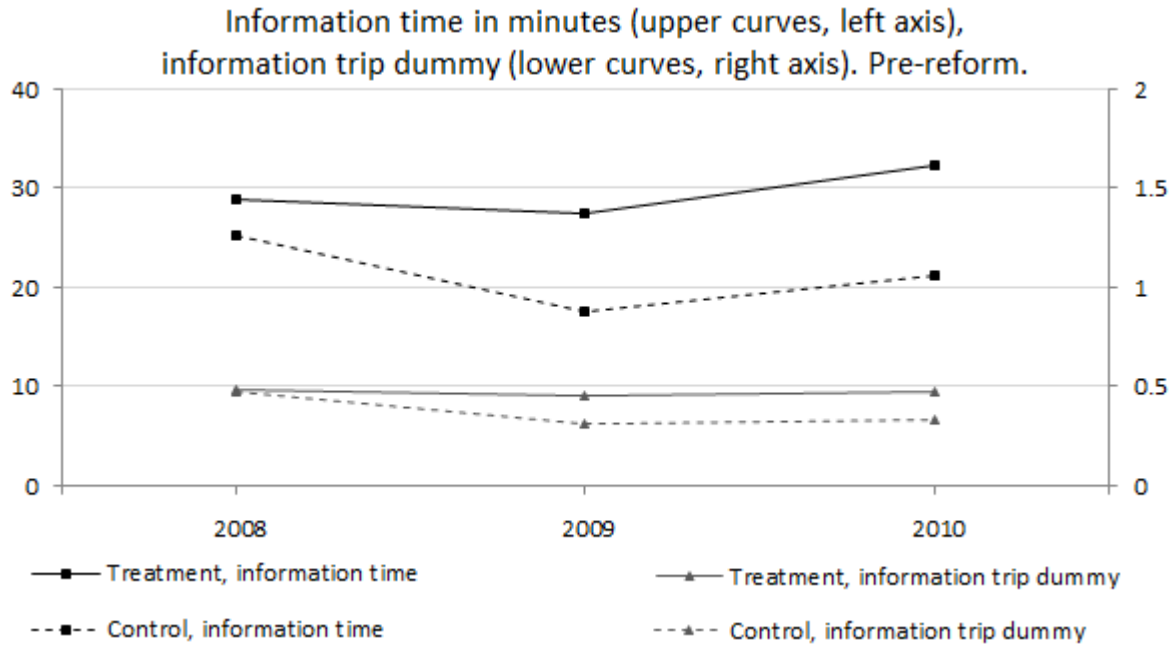
Notes: Once treated, the 2008–2010 Poupatempo municipalities are excluded, as are a few control group observations, due to take-up. *Source:* Interviews.

Figure A3. Pre-treatment Graph for Days (Upper Curves, Black) and Days to Process (Lower Curves, Gray).



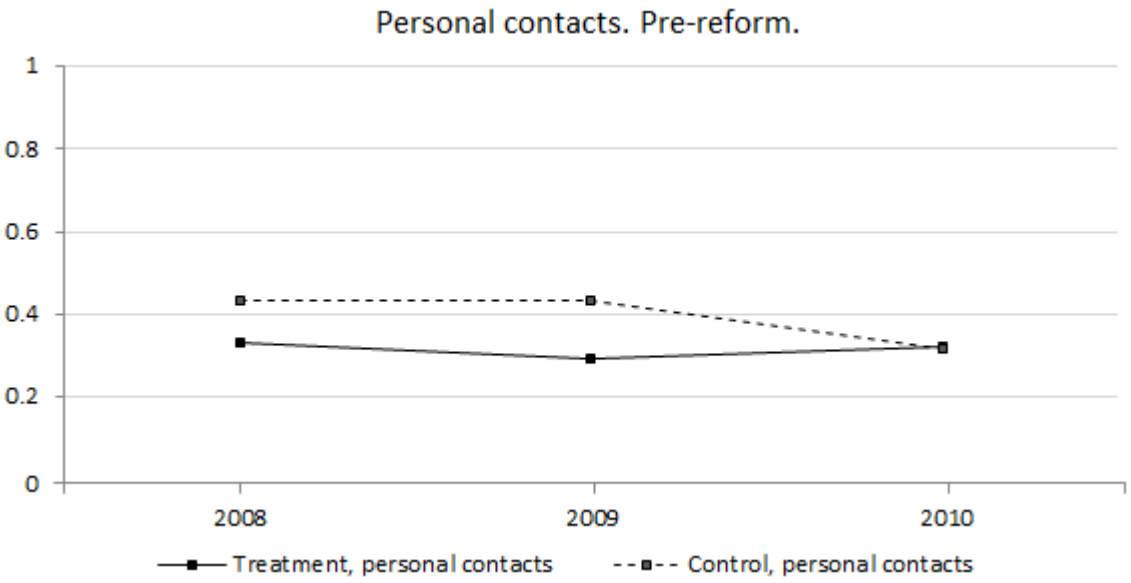
Notes: The payments variable is noisier than the other variables. The data is restricted.to "basic" renewals, excluding individuals that did the course/test or other errands (as in Table 2C). Once treated, the 2008–2010 Poupatempo municipalities are excluded, as are a few control group observations, due to take-up. 1 USD=1.94 R$ (average 2008-2013). *Source:* Interviews.

Figure A4. Pre-treatment Graph for Payments in R$ (All Values in 2013 R$).



Notes: Once treated, the 2008–2010 Poupatempo municipalities are excluded, as are a few control group observations, due to take-up. *Source:* Interviews.

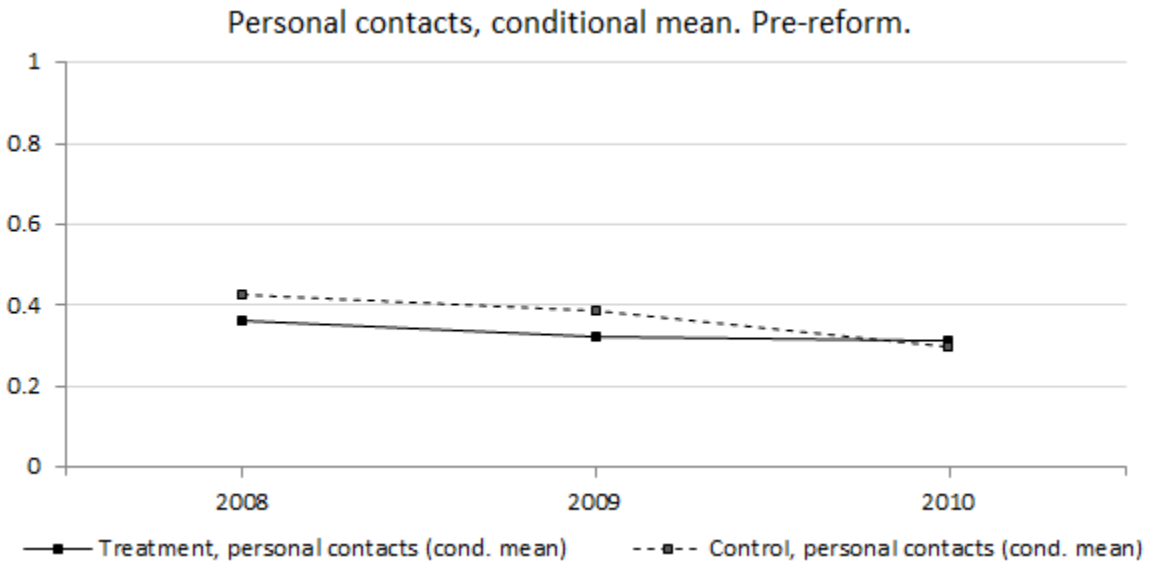
Figure A5. Pre-treatment Graph for Time Spent Obtaining Information (Upper Curves, Black, Left Axis) and the Information Trip Dummy (Lower Curves, Gray, Right Axis).



Notes: Once treated, the 2008–2010 Poupatempo municipalities are excluded, as are a few control group observations, due to take-up. *Source:* Interviews.

Figure A6. Pre-treatment Graph for the Personal Contacts Dummy.

The personal contacts variable (Figure A6) shows a slightly diverging behavior between the treatment and control groups in 2010. In Figure A7 we use a procedure similar to e.g. Aragón & Rud (2013) and plot also the conditional mean of the dependent variable. The concern is that the treatment/control group difference in a particular year is driven by differences in the control variables. We thus regress, for treatment and control separately, the personal contacts dummy on the course/test/other errand dummies and the socioeconomic controls (the same controls as are included in the Table 5 regressions), and on year dummies. We then plot the predicted values from the two models, at each year´s average value of the control variables (the joint treatment and control average). The procedure improves the comparability of the data (comparing Figures A6 and A7), and the diverging behavior in 2010 seems largely driven by differences in the control variables. It should be noted that we have less data for the personal contacts variable (as mentioned in the notes to Tables 3 and 5), which makes it more susceptible to the above discussed effects.



Notes: Predicted values from separate treatment/control regressions on year dummies, the course/test/other errand dummies and the socioeconomic controls, at each year´s treatment+control group average value of these controls. Once treated, the 2008–2010 Poupatempo municipalities are excluded, as are a few control group observations, due to take-up. *Source:* Interviews.

Figure A7. Pre-treatment Graph for the Personal Contacts Dummy (Conditional Means).

A4. ADDITIONAL REGRESSIONS RERERRED TO IN THE MAIN TEXT

Table A5. Impact of personal contacts on the time to renew the license (cross sectional evidence).

	Dependent variable: Minutes spent							
Sample:	Detran		Poupatempo		Driving school		Despachante	
Has personal contact	-70.33**	-62.28*	-46.76*	-55.29*	-42.39*	-45.98*	-9.892	-0.139
	(30.02)	(33.00)	(26.86)	(31.74)	(21.86)	(24.37)	(33.54)	(41.36)
Controls:								
- Course/test and other errands	NO	YES	NO	YES	NO	YES	NO	YES
- Socioeconomic	NO	YES	NO	YES	NO	YES	NO	YES
Observations	114	112	149	149	89	88	77	76
R-squared	0.088	0.153	0.265	0.293	0.081	0.096	0.058	0.187
Column	1	2	3	4	5	6	7	8

Notes: Separate regressions for each means of renewal, without and with controls for course/test/other errands, age, gender and income. All regressions include treatment and year dummies. *Source:* Interviews.

Robust Standard Errors in parenthesis. * Statistically significant at the 10 percent level, ** Statistically significant at the 5 percent level, *** Statistically significant at the 1 percent level.

A5. OTHER OUTCOMES

A5.1 Red Tape

There is no generally agreed-upon definition of red tape. In our context we understand red tape as those parts of the citizen-state interaction that serve no specific socially useful purpose, in line with much of the Public Administration literature (e.g. Bozeman, 1993). Unlike the course/test (which is netted out from the minutes and trips variables), the medical exam is compulsory for all. We focus on the latter as the reason for having a compulsory driver´s license renewal. The other parts are administrative steps. A de jure renewal at DETRAN could then, in principle, proceed as follows: The individual visits the doctor, who reports the exam results to DETRAN. The new document is produced, and the citizen then picks it up, with two return trips in total. As Poupatempo co-locates the doctor, the theoretical number of trips is instead one. To sketch a reform impact on red tape, we adopt the (perhaps extreme) view that a real-life renewal has two parts: the theoretically necessary and socially justified amount of steps/trips, plus red tape. The estimated reduction of 2.7 trips (Table 5, panel 2, column 3) would then consist of a one trip co-location gain, and 1.7 trips of red tape reduction. An estimate of the reduction in time spent, due to less red tape, can be obtained by simply converting these 1.7 trips into a time measure. A return trip takes about 30 minutes in the treatment group, before as well as after the reform; the red tape reduction would thus be 51 minutes (1.7×30). This is about half of the total reduction in time spent, from Table 5. Section 7 discussed that also "counter time" was (slightly) reduced due to the reform; hence our red tape reduction measure is likely to be conservative.

A5.2 Transparency

Transparency is defined by Transparency International as an attribute of governments of being open in disclosure of information, rules, processes and other aspects (TI, 2017). Information was

analyzed in section 7.2, but an additional aspect is its correctness. We find only minor differences, for individuals that did an information trip, of having received erroneous information (seven percent for DETRAN, five percent for Poupatempo). We also asked if individuals were informed at the renewal entity of when the new license would be ready. Five percent at DETRAN, and one percent at Poupatempo, say they got no information. A minor improvement is suggested.

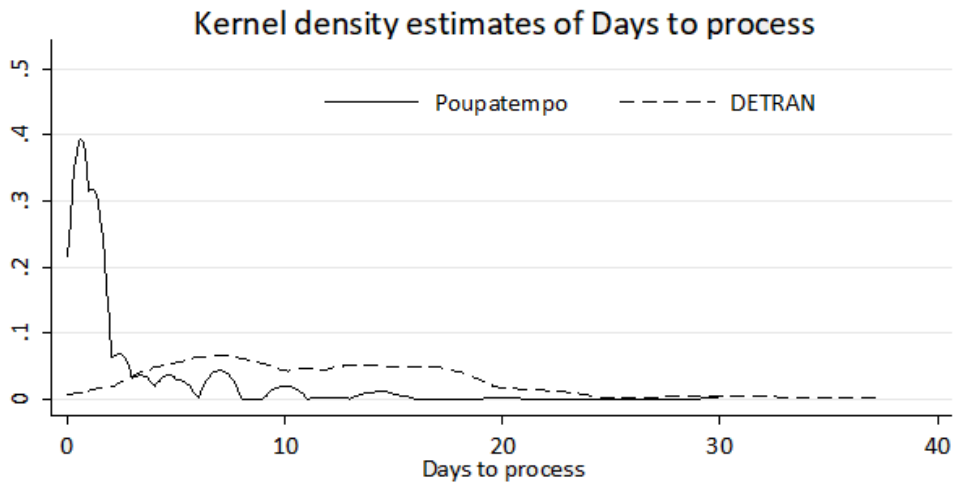
A5.3 Other Transaction Costs

The certainty or predictability with which a citizen can undertake the renewal can also be interpreted as a transaction cost. Figure A8 shows, for DETRAN and Poupatempo, the kernel density estimates of the number of days, at the entity of renewal, before the license is ready ("days to process"). Embodied in the numbers are cases where the license was not ready as stipulated, with the individual sometimes visiting the entity several times to pick it up, etc. The higher average and standard deviation for DETRAN suggest that the reform reduced not only the time to undertake the transaction, but also the uncertainty of when the license will be ready.[24] Other outcomes depict similar differences, with a lower variance at Poupatempo, which holds also when comparing with all non-Poupatempo renewals. Another facet of transaction costs is that in 15 percent of non-Poupatempo cases there were extra/non-standard trips (e.g. returning home for missing documents or unforeseen demands), whereas this happened only in four percent of Poupatempo cases.

A5.4 Course/test Requirement

The second socially relevant component of the driver´s license renewal is a defensive driving and first aid course/test requirement, for individuals that did not have it as part of their original curriculum. One of the project findings was that there were two different implementations of this

[24] A robust Levene´s test of equality of variances is rejected.



Note: The data is restricted to the post-reform period, 2011–2013. *Source:* Interviews.

Figure A8. Kernel Density Estimates of the Number of Days to Process a Driver's License Renewal Application, Once Handed in, at DETRAN and Poupatempo.

statute. We therefore calculate the extensive margin compliance for both "versions" (see the Table A6 caption for details). The timing and uncertainty of the regulation makes it difficult to evaluate the impact of Poupatempo. In addition, the course/test is not administered by Poupatempo, which differs from the medical exam. We therefore present cross-sectional evidence. In our sample, an average of 15 to 24 percent of respondents obliged to do the course/test, did not comply (Table A6, columns 3, 6). Of those that did comply, and chose the classroom option of the course, one fourth did too short a course, if we allow for measurement error and use 500 minutes as the cutoff for the compulsory 15 hour course (column 9). Table A6 also shows the numbers for different means of renewal, suggesting that driving school renewals are the most correctly implemented, as to the course/test statute. The fact that individuals that should do the course/test seek out driving schools may imply a selection effect that exaggerates the differences observed in columns 3 and 6. Few course/test individuals renew at Poupatempo, but from this small group, there are more non-compliers for both the extensive and intensive margins. If anything, the evidence is against Poupatempo improving upon the fulfillment of the requirement.

Table A6. Extensive- and intensive margin non-compliance with the course/test requirement.

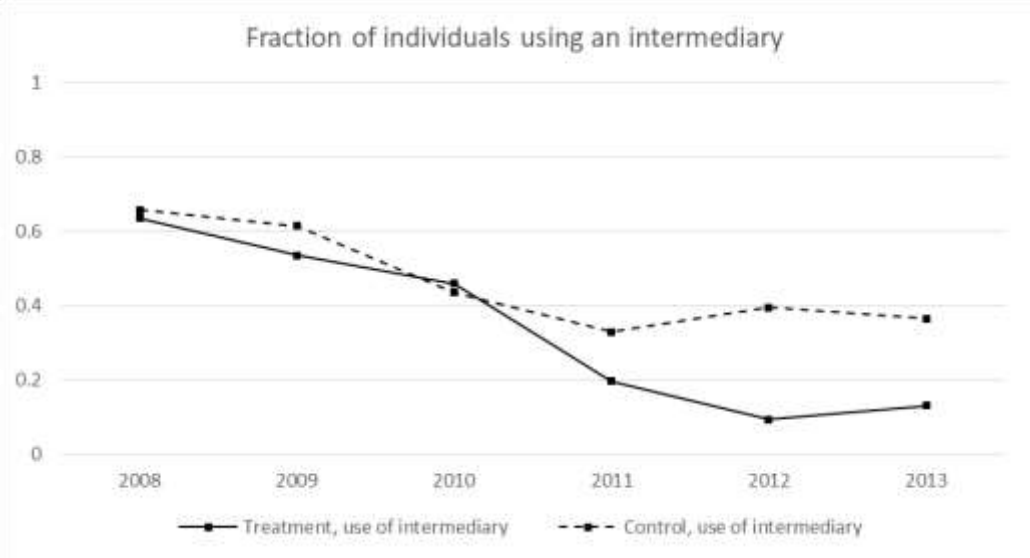
	1	2	3	4	5	6	7	8	9
	Should do course/test ("Version 1")			Should do course/test ("Version 2")			Classroom course takers		
	# Should do course/test	# No course/test	% No course/test	# Should do course/test	# No course/test	% No course/test	# Classroom	# Course <500 min	% Course <500 min
DETRAN	47	11	23%	29	4	14%	28	6	21%
Poupatempo	22	10	45%	18	10	56%	17	7	41%
Driving School	53	7	13%	41	3	7%	47	8	17%
Despachante	52	13	25%	29	2	7%	33	8	24%
Average			24%			15%			25%

Notes: Version 1 of the requirement is that old enough licenses (original license pre-1999/12), should do the course/ test in the first post-2005/10 renewal. Version 2 is that related courses, done during 2000–2005, exempt the license holder from the requirement. We adapted the survey questionnaire after a third of the interview project, and the last two thirds of the interviews capture not only version 1 but also version 2 of the requirement. *Source:* Interviews.

A5.5 Use of Intermediaries

In the paper, we find evidence that Poupatempo is a means through which to save time and that it simplifies individuals' going through the licensing procedure. The evidence suggests intermediaries perform a similar role, but are being replaced by Poupatempo. Pre-reform, using an intermediary is a means through which to get a speedier process, for instance because individuals using the service usually need not undertake bank- and copy trips (data not shown), and intermediaries can charge in excess of DETRAN (Table 2). Poupatempo performs similar to intermediaries for the time spent variable, but using the One Stop Shop involves less trips, days and payments, explaining the high take-up (Figure 4). As a result, intermediary usage drops, as Figure A9 shows.

The analysis of the medical exam and course/test requirements also suggests a time-saving role of intermediaries. Unlike e.g. Bertrand et al. (2007), we do not find that individuals undertake the errand at an intermediary to circumvent rules, that is, to avoid, fully or partly, the medical exam or the course/test. This suggested role of intermediaries as a time-saving-, rather than rule-breaking device, may, at least in part, depend on the specific licensing procedure studied.



Note: All data (as in Table 2A). *Source:* Interviews.

Figure A9. Intermediary Use.

APPENDIX A6. ROBUSTNESS

A6.1 Alternative Treatment and Control Group Definitions

One issue of concern is that Poupatempo was implemented in (mostly) larger cities, as shown in Tables A3 and A4, and that the reform would have had less of an impact in smaller cities. In order to address this issue, we run the same regressions as in Table 4, column 1B, but for different definitions of the treatment and control groups, with results in Table A7. In the baseline specification, used in section 7, individuals living close to the treatment municipalities are considered as treated, and everyone else as control (also individuals not living close to the control group interview locations).

Table A7 shows that the estimated time saving varies little when going from the baseline sample (column 1), to using the <20km (and <25 min travel time) definition also for the control group (column 2), as well as when restricting the sample to only those individuals that live in the interview municipalities (column 3). Column 4 excludes from the original regression those con-

trol group interview locations that themselves are close to the treatment locations (here we use a 30 km cutoff).[25] Column 5 instead excludes the two control group municipalities that were selected through other means than matching (Registro and Guarujá). Also these estimates are similar. In columns 6 to 7 we restrict the sample to those individuals living in the municipalities within the common support (from the control group selection, Table A2), which is about half of the sample. In particular, the treatment group is restricted to the four cities, in Table A2, that fall within the common support, and for which the average population is 125.000, similar to the control group average in Tables A3 and A4. The estimated reform impact is slightly higher, which in part is due to a higher take-up of Poupatempo (but see also the discussion in the next paragraph). The coefficient estimates in columns 6 to 7 are also similar to those that would be obtained from a matching regression, as the matching procedure reduces the sample to fewer municipalities, within or close to the common support. For the trips, days and days to process variables, in regressions corresponding to Table A7 (not shown), we get only minor variation in the treatment effect, except for the estimates corresponding to columns 6 and 7 of Table A7, with larger effects for trips and days. This is in part driven by a larger take-up in the four common support treatment municipalities, and potentially a larger influence of outliers, due to a smaller sample.

As a further step to address the treatment- and control group difference, and to hypothesize what the treatment effect may have been had Poupatempo been implemented in the control group, we decompose the minutes spent variable into "counter time" and "transport time" (which was also discussed in section 7.1). A first observation is that the treatment/control pre-treatment differences are due to transport time differences. In fact, for the 2008 to 2010 minutes data in Figure 3, the sum of counter time (which also includes queueing, in telephone, internet use, etc.),

[25] This basically amounts to excluding the control interview locations in the "Baixada Santista" (Guarujá, Praia Grande) and surrounding municipalities, all close (in kms, but not in travel time) to Santos (part of the treatment), and a few other locations, for a total of 54 excluded interviews.

Table A7. ITT estimates for minutes spent (as in Table 4, column 1B), for different definitions of the treatment/control areas.

Dependent variable	Minutes	Minutes	Minutes	Minutes	Minutes	Minutes	Minutes
Treatment group definition	<20km & <25min	<20km & <25min	Interview municipalities	<20km & <25min	<20km & <25min	Common support (CS) +<20km & <25min	CS
Control group definition	All other	<20km & <25min	Interview municipalities	>30km from treatment	All other excl. "ad hoc" control municipalities	Common support (CS) +<20km & <25min	CS
Panel 1 - OLS							
aftertreatment	-100.2***	-104.7***	-108.2***	-107.2***	-105.3***	-122.7***	-121.9***
	(21.35)	(24.69)	(27.46)	(21.57)	(22.11)	(31.74)	(37.52)
Panel 2 - GLM (link/distribution)	log / nb	log / nb	log / nb	log / nb	log / nb	log / nb	log / nb
aftertreatment: Marginal effect	-104.3***	-109.1***	-113.5***	-116.9***	-111.1***	-121.2***	-123.2***
	(23.75)	(28.50)	(31.64)	(25.55)	(25.17)	(33.90)	(38.63)
Observations	636	551	518	582	607	317	300
Column	1	2	3	4	5	6	7

Notes: The dependent variable in all regression is "Minutes". Panel 1 reports the OLS "aftertreatment" estimate and panel 2 the marginal effect from a GLM model, as detailed in the first line of panel 2 (nb–negative binomial). All regressions include treatment/control location dummies, an after dummy and the course/test/other errands- and socioeconomic controls. Column 1 reproduces the regressions of Table 4, column 1B. The treatment and control samples are then restricted to different subsets of the data, according to the top rows in the table, and as described in the text. Marginal effects are calculated as in Puhani (2012), at the 2011 to 2013 treatment group control variable averages. *Source:* Interviews.

Robust Standard Errors in parenthesis, clustered on treatment/control locations.* Statistically significant at the 10 percent level, ** Statistically significant at the 5 percent level, *** Statistically significant at the 1 percent level.

is 134 and 133 minutes for treatment and control, respectively. The pre-reform transport time is instead around 40 minutes longer in the treatment group. Running separate treatment- and control group pre-reform regressions of the transport time on municipality population (linear and squared), gives a predicted transport time, in a municipality of 120.000 inhabitants, of 151 minutes for the treatment group, and 125 minutes for the control group. For a city with a population equaling the average control group size (from Table A4), the transport time is thus 26 minutes longer in the treatment group. Dividing these predicted total transport times with the pre-reform average number of return trips (5.03 in the treatment group and 4.67 in the control group), gives an average time for one return trip of 30 minutes in the treatment group, and 27 minutes in the control group. This may be due to geographic conditions, and other factors. Under the assumption that this per-trip time difference of three minutes would remain unchanged, also for trips to Poupatempo, had Poupatempo been implemented in the control group, and that the average number of return trips (2.4) for Poupatempo renewals would be the same, we would get a travel time of around 7 minutes less in the hypothetical control group Poupatempo locations of 120.000 inhabitants, than in treatment locations of the same size. Based on these transport time calculations and assumptions, we would get a hypothesized treatment effect in the control group of 18 minutes less (25 minus 7).[26] Smaller Poupatempo municipalities also have shorter times at the counter, however, which may be due to less congestion. In order to construct the hypothesized counter time saving in the control group locations, we regress, for the treatment Poupatempo renewals, time at the counter on municipality population (linear and squared). The predicted counter time for a city of 120.000 inhabitants is approximately 25 minutes shorter than the corresponding average for all treatment group Poupatempo renewals. If these results carry over

[26] The reduction would stem from the sum of fewer pre-treatment trips (5.03 minus 4.67), hence $30 \times 0.36 \approx 11$ minutes less time saved, and 7 minutes less saved for the return trips in the reduction from 4.67 to 2.4.

to the hypothesized control group Poupatempo implementations, we would thus see a larger treatment effect (less gain in transport, but more gain at the counter and in total). In line with this reasoning, we typically estimate a slightly larger treatment effect in the smaller Poupatempo municipalities (compare columns 1 and 6 of Table A7, a result which holds also if such a table is produced based on TT regressions, although the difference is then smaller.)[27] These results are unlikely to carry over to hypothetical implementations in much smaller locations.

A6.2 Use of the Full Sample

The staggered nature of the Poupatempo reform implies that we would get misleading results if we attempt to estimate the two-period model used in section 7 with all the data. The analysis in section 7 does not make use of those post-reform observations, from 2008 to 2010, that are from the few locations that saw early Poupatempo implementations, nor the 2008 to 2010 control group observations/individuals that used said units (spillovers). About six percent of the overall sample was thus excluded, in order for the data to constitute clearly defined before- (2008 to 2010) and after (2011 to 2013) groups, allowing for a simple DD model.

Another approach is to use the entire sample, but replace the after/before dummy with period dummies, where a new period starts as soon as a new Poupatempo is implemented (month dummies could be used as well). T_{st} shifts from zero to one as the reform is implemented in location *s*. This approach takes the staggered nature of the reform (Figure 1B) fully into account (instead of dropping some data) and constitutes the multiple groups and time periods DD generalization (e.g. Bertrand, Duflo, & Mullainathan, 2004). Using this approach we get a slightly lower ITT reform estimate on the minutes variable (91 minutes, when including the

[27] These calculations do not consider idle time, as described in conjunction with Table 1. Its inclusion would slightly change the numbers but not affect the qualitative reasoning.

standard control variables, i.e. course/test/other errand dummies, age/gender/income). The estimates for trips and days are similar to those of Table 4 (regressions not shown).[28]

Yet an alternative is to use all the data and estimate a separate treatment effect for each period that saw a new Poupatempo implementation (again referring to Figure 1B). We thus replace T_{st} in Model 1 with a sum of period×treatment interaction terms, and estimate a separate treatment effect for each period. Using this approach, the weighted average of the estimated coefficients is 95 minutes. This figure is in turn similar to the estimate of 104 minutes we get when again restricting the sample as in section 7, and run the same regression as in Table 4, column 1B, but only using a treatment group dummy (instead of individual treatment/control location dummies). All estimates are rather similar.

A6.3 Additional Control Variables

Another robustness check is to include further control variables in the regressions in section 7. We first added pre-reform municipality population and population growth, a municipality level variable for the surrounding regional population density and a dummy for whether the mayor was from the PSDB party. The inclusion of these variables was primarily based on the Appendix 2 propensity score regression for the control group selection. We used a single treatment group dummy instead of the individual treatment/control location dummies. We restricted the sample to the interview municipalities themselves (corresponding to Table A7, column 3), excluding two locations with missing data, and ran the modified ITT regressions for each of the Table 4 dependent variables. From Table A3 we added another variable, i.e. the municipality level percentage without a birth certificate. As an alternative to pre-reform population and population

[28] Drawing on earlier literature, Goodman-Bacon (2018) shows that the interpretation of the treatment effect estimate in models that fully incorporate the variation in treatment timing is not straightforward, and in general does not correspond to a sample average treatment effect. The estimate is a weighted average of all possible two-group/two-period DD estimates from the data, where the different timing of treatment will affect the weight each such pair gets in the overall estimate.

growth, in some specifications we instead used municipality level population for the before (2008-2010 average) and after (2011-2013 average) periods. We also used (municipality level) data, for the before- and after periods, on GDP/capita. In none of the regressions do the coefficient estimates or significance levels change by much. In additional specifications we used municipality level pre- and post data on the number of vehicles, as an alternative to population data, also without large changes (regressions not shown).

A6.4 Standard Errors

The standard errors in all DD regressions are clustered on treatment/control location. Another option is to use two-way clustering (Cameron, Gelbach, & Miller, 2011). When clustering on treatment/control location and year in the OLS specifications, we get on average slightly lower standard errors. All conclusions with respect to the significance of the regression results remain.

A6.5 Multiple Hypothesis Testing and Correction of p-values

With more than one outcome variable of interest, the probability of observing a significant effect, by chance, increases. Several methods have been developed to adjust the significance levels of individual parameter estimates. In Table A8 we correct p-values using the Bonferroni-Holm method (e.g. Duflo, Glennerster & Kremer, 2008, and references therein). The method consists of first listing, in ascending order, the p-values of the treatment effect estimates from N different regressions (i.e. N different dependent variables). The first/smallest p-value is then adjusted to account for the fact that, with N different outcome variables (which are assumed to be independent), the chance of finding a significant effect (when there is none) is higher than the individual p-value itself signals. The correction is to multiply the p-value with N. If the dependent variable corresponding to this first (now adjusted) p-value remains significant, the procedure is repeated, now with the second smallest p-value in the original ordering, and with $N - 1$ different

outcomes. The p-value is thus adjusted for the fact that, with $N-1$ different outcomes, the chance of getting a significant outcome (when there is none) is higher than signaled by the individual p-value (the correction is to multiply it with N-1, and it must also be at least as large as the previously adjusted p-value). This procedure is repeated for the next dependent variable in the original ordering, and so on, until an adjusted p-value turns insignificant. The remaining dependent variables are then also declared insignificant.

One important consideration when applying the method is that the dependent variables under study are not independent. As an example, the correlation between the variables "minutes" and "minutes without idle time" is 0.96. We therefore apply the above procedure on seven outcomes that can be considered to represent, to some extent, different outcomes of interest: minutes, trips, days to process, payments, time to get information, the personal contacts dummy and the number of medical checks. Table A8 reproduces the GLM coefficient estimates for these variables (from Table 5) and shows the unadjusted and Bonferroni-Holm adjusted p-values. The treatment effects of interest remain significant at the five percent level or better and the results of the paper are robust to these p-value adjustments.

Table A8. Correction of p-values using the Bonferroni-Holm method.

Dependent variable	Minutes	Trips	Days to process	Payments	Time getting information	Personal contact dummy	# Medical checks
GLM regressions (as in Table 5)							
aftertreatment coefficient estimate	-0.412	-0.76	-1.841	-0.231	-0.502	-1.718	0.101
Unadjusted p-value	8.54E-07	0	1.02E-11	0.025	0.012	0.0028	0.36
Bonferroni-Holm adjusted p-value	4.27E-06	0	6.14E-11	0.05	0.036	0.0112	0.36

Notes: GLM regressions as in Table 5. The unadjusted p-values correspond to the standard errors shown in Table 5.

A6.6 Differences in Recall in Treatment and Control?

Another issue of concern is if respondents that renewed their license at the newly established Poupatempo units, would, for one reason or the other, remember their experience better than

control group individuals. Although we cannot verify this directly, in Figure A10 we plot the average number of questions for which respondents answered "Do not remember" (panel A), and also an indicator for if any such answer exists in a questionnaire in the final data (panel B).[29] The gray lines (all data) confirm that recall is better the more recent is the renewal. We also plot the two variables for 2011-2013 treatment group Poupatempo renewals and for 2011-2013 control group non-Poupatempo renewals. Although the control group data for 2011 shows somewhat lower values, there are no other marked treatment/control differences in these measures.

A6.7 Distance Analysis

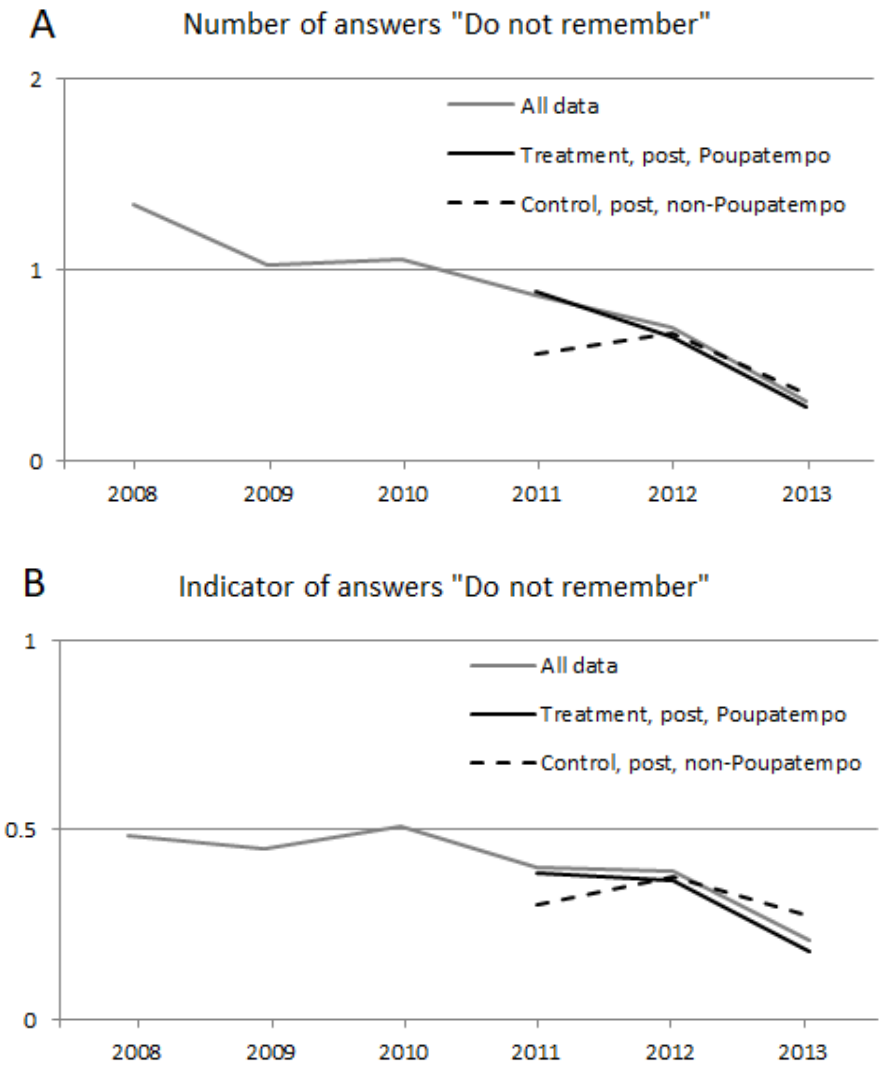
We next estimate the Difference-in-Differences reform impact as a function of distance. The motivation for this analysis is to use it in the cost benefit calculation in section A7. As detailed below, there is a minor difference between the interview and population samples with respect to how far the average treatment group Poupatempo user lives from the closest Poupatempo unit. If the treatment effect depends on distance, which is likely, we may overestimate the population-wide benefit of the reform.

We add, to the specification in (1), an interaction between the aftertreatment indicator T_{st} and the distance in kilometers from an individual's home municipality to the closest treatment Poupatempo municipality, to get

$$y_{ist} = \alpha_s + \eta_t + \delta T_{st} + \phi d_{ist} + \beta X_{ist} + \varepsilon_{ist} \quad (3)$$

The interaction term d_{ist} is positive for the treatment group observations in municipalities close to the treatment locations and where the renewal occurred after the respective Poupatempo implementation (and equals the distance to that Poupatempo municipality). Otherwise it is zero.

[29] On average, respondents answered around 130 questions. The questions not remembered are mostly of the type "Did you pay a parking fee while doing the errand at the entity of renewal?", or "Did you leave your fingerprints while at the doctor?", typically not affecting the calculations of the main variables. Interviews where respondents did not remember crucial information had already been interrupted, and are not part of the data. Some interviews were checked/completed by contacting interviewees, on telephone, after the interview had taken place.



Notes: Gray - all data, Black, solid - all treatment group Poupatempo renewals for 2011–2013, Black, dashed - all control group non-Poupatempo renewals for 2011–2013. Course/test questions excluded. *Source:* Interviews.

Figure A10. (A) Average Number of Questions for Which Respondents Answered "Do Not Remember" and (B) Average Value of an Indicator Variable for if There is at Least One Such Answer.

Corresponding to Table 4, column 1B, we get a larger (in absolute value) negative coefficient on aftertreatment (-105.1 minutes, i.e. the effect for those living in the 16 treatment municipalities), and a ϕ-coefficient of (+) 4.25 minutes/km, thus implying a treatment effect that decreases with distance (as expected). Whereas the average distance to the closest Poupatempo is just above 1 km in the interview data, in the population data it is 2.4 km. Plugging in the latter value in the estimated model thus gives an average treatment effect of $-105.1+4.25\times2.4\approx-95$ minutes, an estimate that will be used in the cost-benefit calculation.

APPENDIX A7. COST-BENEFIT CALCULATION

This section undertakes a cost-benefit calculation of the Poupatempo reform. The question we aim at addressing is to what extent the cost of the reform is offset by gains for citizens. We cannot evaluate the question in full, as only one of the services at Poupatempo was studied. The analysis therefore focuses on the costs and benefits with respect to driver´s license renewals.[30]

We first discuss costs. The São Paulo state government data entity, PRODESP[31], is responsible for the overall operation and management of Poupatempo. PRODESP in turn outsourced the operation of the 16 new units to third party contractors. In order to arrive at an approximate operational cost for these units, we reviewed monthly summaries of all PRODESP contracts from 2008 and onwards (around 5000 contracts, although most are unrelated to Poupatempo) (PRODESP, 2018). The main cost is the Operation and Maintenance (O&M) of the 16 units themselves, which we estimate at 7.2 MR$ per month (for 2012/2013, when all units had been implemented).[32,33] We add 0.48 MR$ for the centralized Poupatempo telephone platform for

[30] The analysis is kindred to the Muralidharan, Niehaus & Sukhtankar (2016) cost-effectiveness analysis of the introduction of a new payments technology in India, which affected citizens' time to collect wages.
[31] Companhia de Processamento de Dados de São Paulo.
[32] 1 USD=1.94 R$, average 2008–2013.
[33] The O&M contracts for the 16 units are from different dates and sometimes of different length. Each unit's first contract had a minor amount for installation costs, although the building infrastructure itself is outside the contract.

information/inquiries and 0.82 MR$ for other costs not covered by the third party contracts, for instance rent and security.[34] We get a total cost of 8.5 MR$ per month, or 102 MR$ per year, for the 16 units. A few minor operational items may be missing in this figure, such as the overhead costs of the centralized Poupatempo planning unit, and possibly data interconnect charges and the like.

The above costs are for the Poupatempo operation itself. From the discussion in section 1 we emphasize that some of the specialized back-office functions are not part of the operational contracts, and are instead executed by personnel from the authorities themselves. This is the case for the technical parts of the driver´s license renewal (i.e. verifying registries), which is handled by DETRAN personnel. We assume that it is equally costly for DETRAN to renew a driver´s license inside a Poupatempo unit, as it is at the legacy structure (this cost hence does not enter into the cost-benefit calculation). The assumption may not have been true in the beginning, with some DETRAN start-up costs inside Poupatempo. In a study of the issuance of certificates of criminal record, Ferrer (2006) concluded that the total internal cost of emitting a document was slightly higher at Poupatempo than in the legacy structure, but such differences are likely to diminish through Poupatempo productivity improvements over time.[35]

Of all errands handled at Poupatempo, we estimate that seven percent are driver´s license renewals. This figure was derived by dividing the total amount of driver´s license renewals al

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To estimate an average monthly O&M cost, we used an approximate 10 year contract period for each unit (typically two or three contracts, spanning from around 2009/2010 until 2019/2020). We deflated/inflated all values to 2013, then divided each unit's total amount with the contract length in months, then summed the 16 rates thus obtained.

[34] To get the cost of the telephone platform we multiplied the total contract value (121 MR$ for the study period) with the fraction of errands handled by the 16 new units (22%) (thus assuming that the number of information inquiries and the cost thereof are a fixed proportion of the errands subsequently handled at the Poupatempo units) and divided by the contract length in months. The 0.82 MR$ item covers rent, training, canteens, uniforms, security etc. If no rental contract existed (e.g. when another public entity granted use rights to a building), the rent was imputed.

[35] One of the main cost components, wage, is stipulated by (DETRAN) civil service careers, rather than if a person works inside Poupatempo or not. Those parts of the procedure that involve checking the identity of individuals, etc., are done using the same computer/software infrastructure and should be considered to have the same cost.

Poupatempo (from the DETRAN dataset), with the total number of errands at Poupatempo.[36] As the handling of errands/citizens at Poupatempo is very similar across errands (reception, determination/screening of the errand to be conducted, getting a queueing number, waiting inside the premises, etc.), we assign an equal proportion of the Poupatempo operational costs to each type of errand. We thus arrive at a yearly Poupatempo operational cost for driver´s license renewals, at the 16 new units, of 7.14 MR$ (0.07×102 MR$).

We next discuss benefits. One effect of the reform is that individuals get their renewed licenses in fewer days (Tables 4 and 5, column 5), but it is difficult to assign a monetary value to this gain. As for the time the procedure itself takes, an average treatment group license holder uses 95 minutes less to renew the license (this is the Table 4 OLS estimate from column 1B, corrected for distance, as discussed in section A6.7). In total there were 496000 driver's license renewals per year in the treatment locations when all the 16 units had been implemented (from the DETRAN data), which gives an estimated time saving of 47.1 Million minutes per year (95×496000 minutes). How should this gain be valued?

The treatment group sample consists of 85% of individuals that have a monthly income and that work (i.e. both income and hours worked are larger than zero). For these individuals doing "market work", we use the hourly after tax wage as the opportunity cost of time (i.e. the monthly work income times the stipulated tax rate divided by the number of weeks per month divided by the reported hours of work per week).[37] The remaining 15% of the sample are either retirees, work at home (typically with household chores) or are unemployed. For this group, we base the

[36] This calculation was first done for 2011, for which we have access to a more granular dataset of Poupatempo errands, then verified for the study period as a whole.

[37] The average wage in the treatment sample was reported in Table A4. In order to arrive at the net wage for those that do market work, we multiply the reported monthly income with the stipulated income tax rate, using the (2013) Brazilian tax code (the average tax rate in the sample is 11%). In order to get the weekly wage we divide by 4.345 weeks per month (365/12/7).

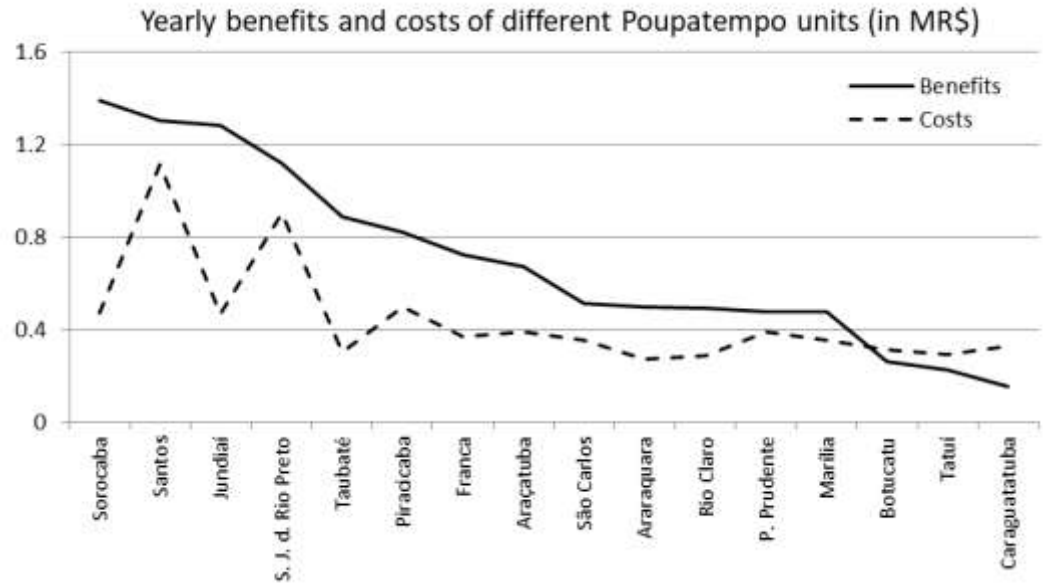
calculation on a monthly reference occupation that earns 1000 R$ for 40 hours of work, representing how much it would cost to replace activities foregone, and thus derive a per hour opportunity cost of time.[38] The age of interviewees and the reported occupational categories were also checked to make sure that the division into the two groups and the calculations make sense. The distinction between those that do market work and those that do not follows the approach suggested by e.g. Posnett and Jan (1996). These authors also discuss other subtleties of opportunity cost of time calculations. Using this approach, the average opportunity cost of time in the treatment group sample becomes 14.4 R$ per hour (0.24 R$ per minute). The opportunity cost valuation of the time saved in the renewal of driver´s licenses thus becomes 11.3 MR$ (0.24×47.1 MR$). With these estimates we thus get a time saving that is valued about 50% higher than the costs of the Poupatempo operation that pertain to driver´s license renewals. As discussed above, we may be missing minor Poupatempo operational costs in the 7.14 MR$ figure, but the time saving induced by the reform is non-negligible and is on par with or higher than the operational costs.

The above analysis of costs and benefits shows that the average benefit of the 16 new units exceeds the average operational cost. Figure A11 shows the benefits and costs per unit. The units are ranked in order of descending benefit, which is the same as ranking in order of the number of renewals in the treatment area of each unit. The Poupatempo units to the right in the figure thus correspond to areas with less renewals/less population. The graph illustrates that even if costs decrease with size, benefits decrease faster, which corresponds to the fact that operating a unit entails some fixed costs. According to the measures here presented, three units have higher costs

[38] The salary for the reference occupation is based on the São Paulo state minimum salary of 755 R$ for a maid/housekeeper (empregada doméstica), to which is added legally stipulated contributions (such as the thirteenth salary, transport and meals).

than benefits. The marginal net benefit of implementing new units thus turns negative. This type of marginal analysis should be contemplated when considering expansions of existing programs.

A few caveats are worth mentioning. Similarly to Muralidharan, Niehaus & Sukhtankar (2016), the cost-benefit calculation does not include all outcomes under study. This is because it is difficult to assign monetary values to certain outcomes, such as the reduced uncertainty in undertaking the errand. The fact that some control group individuals use Poupatempo, in spite of longer travel times, probably signals that improvements in other outcomes compensate for the extra effort and time of travelling (we would thus have underestimated overall benefits). That said, it should be noted that the driver's license population is relatively rich on average, and it is not clear that similar time gains for other services would translate into as high valuations of time



Notes: The cost for each unit is the value from the PRODESP contracts, the total of which was reported in the main text. The benefit reported in the graph is the average Intention-to-Treat estimate of 95 minutes multiplied by the per-minute value of 0.24 R$ multiplied by the number of renewals in the treatment area of each Poupatempo unit (renewals in all municipalities that are <20 km and <25 minutes driving distance away). The units are in order of decreasing benefits. Santos and São José do Rio Preto were the first installed units, which may explain why they have higher costs. *Source:* DETRAN, Poupatempo, Interviews.

Figure A11. Yearly Benefits and O&M Costs of the 16 New Poupatempo Units, As Regards Driver's License Renewals.

saved. If a Citizen Service Center primarily functions to give better access to previously undocumented and poor citizens, the cost-benefit calculation should find ways of incorporating the wider social gains, beyond opportunity cost of time. A final caveat repeats what was said above, that we have only studied one of many errands at Poupatempo hence we do not evaluate the full program.

To conclude, we conduct a different analysis altogether, in that we use the distance to Poupatempo as a continuous measure of treatment, for all observations, and analyze how the renewal time depends on this distance. We then compare the pre- and post- reform average distances, to construct an alternative measure of the aggregate time saving. We thus regress the minutes spent variable on municipality dummies (instead of treatment/control location dummies), year dummies and distance (Table A9, column 1). We then add the course/test/other errands and socioeconomic control variables. Table A9, column 2 shows that each extra kilometer to the closest Poupatempo is associated with 0.58 minutes of additional renewal time. In 2007, before the reform, the average distance to the closest Poupatempo for an inhabitant in the interior of São Paulo, from 2007 population- and Poupatempo locations data, was 98 kilometers. After the implementation of the 16 units, it was 40 kilometers, the reduction is thus 58 kilometers. Multiplying with 0.58 minutes gives an average estimated time saving of 33.6 minutes per renewal. A total of 1495000 individuals renewed their license per year in the interior of São Paulo (from the DETRAN data, for the post-reform period), the estimated total time saving is thus 50.2 million minutes (1495000×33.6). This alternative estimate is very close to the previously calculated aggregate time saving (47.1 million minutes). The average wage in the full sample is similar to that of the treatment group, and the two methods thus arrive at very similar aggregate reform impacts.

Table A9. Estimates of the impact of distance to Poupatempo on the time spent with the renewal.

Dependent variable	Minutes	Minutes
Distance to the closest Poupatempo (km)	0.629***	0.583***
	(0.190)	(0.197)
Controls:		
- Course/test and other errands	No	Yes
- Socioeconomic	No	Yes
R-squared	0.315	0.337
Observations	663	636
Column	1	2

Notes: OLS regressions of the minutes variable on the distance to the closest Poupatempo. All regressions include year and municipality dummies. *Source:* Interviews, Poupatempo, Google Maps and OpenStreetMap.

Robust Standard Errors in parenthesis. * Statistically significant at the 10 percent level, ** Statistically significant at the 5 percent level, *** Statistically significant at the 1 percent level.