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Abstract

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This dissertation explores the intersection between behavioral economics and labor economics. We examine how behavioral mechanisms impact labor market decisions in the gig economy in the United States and for domestic workers in Brazil. The gig economy provides us with a setting to explore how cognitive biases impact job choices and labor supply decisions in flexible work environments. Studying domestic work in a developing country allows for an analysis of how social norms, driven by legislative changes, affect labor market outcomes such as formality rates and wages.

In the first chapter, we study gig jobs on online platforms. Flexibility is an increasingly prominent feature of many jobs. In the gig economy, workers can choose their work hours and face wages that vary across hours and weeks. This increased complexity adds challenges to predicting and understanding job outcomes. Incomplete information or behavioral biases can then lead to inaccurate beliefs about pay and labor supply. We test this hypothesis by collecting novel survey data on 454 delivery and ride share gig workers in the United States. Comparing gig workers' beliefs with data on their actual job performance, we find they overestimate their predictions (43%) and their recalls (31%) of weekly pay, despite it being reported prominently in their earnings statements. Furthermore, gig workers underestimate expenses and overestimate hours worked. The results are consistent with selective recall: when forming and updating their beliefs in noisy environments, workers overweight past high-paying periods. We then examine how biased beliefs affect labor market decisions. We derive predictions from a behavioral labor supply model and test them using survey data and a randomized de-biasing intervention. We find that job choices and labor supply decisions are significantly affected by mistaken beliefs in flexible gig jobs.

In the second chapter, we investigate the role of social norms in shaping labor market outcomes in the context of domestic work (DW) in Brazil. We study the effects of Constitutional Amendment (CA) 72/13, which aimed to give additional labor rights to domestic workers, and Complementary Law (CL) 150/15. We highlight a two-year period when: (i) this legislation was in effect and was heavily discussed, as shown by internet searches; (ii) its contents were on hold. This allows us to focus on the expressive power of the law in altering social norms around employing informal workers. We employ an event study framework with

a control group. Our results show that CA 72 led to a 3 to 6 percentage point increase in the formality rate of DWs, despite an increase in formal labor costs. This is coupled with a decrease in the total number of DWs. We also identify a decrease in average weekly hours worked and an increase in average monthly wages of DWs. These findings are consistent with changing social norms as an important mechanism. Our analysis reveal that DWs who live with their employers experience much larger formalization effects from CA 72 and CL 150. This suggests that the social norms channel is particularly significant in situations where the employer-employee relationship is close. Moreover, we investigate the neighborhood "peer effects" of worker exposure to other domestic and/or formal workers. Our analysis shows that a higher number of DWs (or formalization events) in a neighborhood increases the likelihood of DW formalization. Exposure to formality discussions may thus enhance the social norms impact of new legislation.

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Chapter 1

How Much Can You Make? Misprediction and Biased Memory in Gig Jobs

1.1 Introduction

Many jobs have some level of flexibility, with work hours and pay that vary from month to month. Examples include shift choices and tipping in service sector jobs and short-term contracts with piece rate pay in developing countries. A more prominent case are gig economy jobs, which have surged in popularity in the past decade (Collins et al., 2019). Over 9 percent of adults in the United States worked in a gig job for an online platform such as Uber, DoorDash, Instacart or TaskRabbit during the last 12 months (Pew Research Center, 2021). According to the gig economy literature (Chen et al., 2019; Koustas, 2018), flexible jobs generate surpluses of thousands of dollars per year, as workers are able to adjust work hours to accommodate changes in reservation wages and in demand.

Standard economic models assume that agents have accurate expectations of their labor market outcomes. Yet, the extra complexity involved in understanding and predicting pay and work hours in flexible jobs may lead workers to hold incorrect beliefs. For example, workers may not fully comprehend how predictable supply and demand shocks influence variation in earnings and hours. In this case, labor market decisions will not be optimal and prior estimates of worker surplus created by flexibility may no longer hold. For instance, a worker who misperceives how much they make may choose a sub-optimal number of total work hours and allocate them inefficiently across the month. Furthermore, they might misunderstand expected pay differentials across jobs and make incorrect job choices. Inefficient selection can also occur: workers who overvalue flexible jobs may stay at them longer.

In this paper, we examine whether workers in flexible jobs misperceive their labor market outcomes and analyze how this impacts their decisions. We collect novel data from 454 ride share and delivery gig workers in the United States in three online surveys. We begin by eliciting recalls and forecasts of key outcomes such as pay, hours, and expenses. We then collect actual job performance data by asking workers to submit screenshots from the gig platform app. We measure errors in remembering and predicting job outcomes by comparing workers' stated beliefs with their actual outcomes.

We find that gig workers overestimate their job performance by economically meaningful amounts. We first consider gross weekly pay, which is an important outcome for gig workers and is featured prominently in earnings statements. On average, gig workers overestimate a forecast (incentivized for accuracy with a bonus of up to $5) of next week's weekly pay by 43.7%. Likewise, a recall of weekly pay of either the last week or the last month is overestimated by, on average, 31.3%. These errors amount to $85 to $100 per week. More than 70% of our sample overestimates each measure. We show that forecast and recall mistakes are strongly correlated, suggesting a link between prediction errors and biased memory.

Gig workers' perceptions of their *post-expenses* pay is what should guide consumption choices and other decisions. Thus, to have a better understanding of mistaken beliefs, we estimate drivers' expected costs when doing ride share and delivery gig work. Our measure is a function of a car's category, such as small sedan or medium SUV, and age. We consider only variable costs, including maintenance and repair, and make conservative assumptions regarding taxes. Using our estimate of expenses, we find that recalls of net weekly pay are exaggerated by, on average, 46.3%.

Mistaken beliefs about net weekly pay can be broken down into three components. Indeed, the net weekly pay is the product of the average gross hourly pay, an expenses discount, and hours worked per week. We find that overestimation of labor supply and underestimation of expenses are the most influential factors in explaining aggregate errors. Misperceptions of gross hourly pay play only a minor role. In particular, gig workers over-recall work hours by, on average, 33.1% or 5.8 hours, while underestimating expenses by 22%. In addition, a majority of gig workers report ignoring several categories of costs, including depreciation and expected repairs, when calculating their take home pay.

The next step is to investigate why gig workers misperceive their job outcomes. The explanation should provide a reason for pay being consistently *over*-estimated, rather than equally misunderstood in both directions. We propose motivated beliefs (Bénabou and Tirole, 2002; Bénabou and Tirole, 2016) as a likely mechanism. In this theory, agents hold incorrect beliefs due to hedonic utility or as a motivation tool. For instance, a person might derive direct utility from believing they are highly paid or productive.

Our finding that forecast and recall errors are correlated suggests that memory biases are important in the development of overoptimistic beliefs. Accordingly, we show evidence of selective recall. In other words, gig workers' recalls are influenced more by high-paying than low-paying periods, despite the fact that they should be equally significant. Indeed, a pay increase of $100 in the highest-earning week out of the last four is associated with a rise in recall of weekly pay of $57, compared to $26 in the lowest-earning week. In this way, a motivated belief that gig work is highly paid can be justified.

Similarly, we show gig workers update their pay beliefs asymmetrically, reacting more after realizations of weekly pay that are greater than their previous belief. This finding provides a rationale for the persistence of mistaken beliefs over time. Accordingly, we document that there is a positive correlation between gig work experience and overestimation of pay. That is, gig workers who are experienced overestimate their pay by *more*. We show this striking result reflects both incomplete learning and a selection of biased workers in gig jobs.

When labor market outcomes are noisier, there is additional leeway for selecting unrepresentative memories that more easily justify motivated beliefs. Consistent with this, we show that overestimation is increasing in the variance of previous realizations of weekly pay. Increasing the coefficient of variation (standard deviation over the mean) of the four previous weeks by 0.1 is associated with a statistically significant rise in weekly pay over-recalling of $9. This result supports the view that the flexibility of gig jobs is the key aspect behind misperception of job outcomes. It can also explain why full-time salaried workers do not overestimate their earnings (Moore et al., 2000; Rothbaum, 2015): there is no way in these settings to select memories of being paid above the usual salary.

Misperceptions about job outcomes will likely not reduce gig workers' welfare unless their labor market decisions are affected. To this end, we discuss the labor market consequences of mistaken beliefs in gig jobs. We derive predictions from a simple behavioral labor supply model. According to our model, gig workers who overestimate their net hourly pay (in our data, this is 20.7%, on average) will: (i) sometimes not choose a higher-paying alternative job; (ii) backload work inefficiently over the pay cycle, since they earn less than expected at the beginning of the cycle and have to pay bills at its end; (iii) relative to the rational benchmark, work either too few hours (if the relevant margin is satisfying the household budget) or too many hours (if the relevant margin is weighting consumption benefits from work versus the effort cost of additional work hours).

We test these predictions with observational data and a randomized de-biasing intervention embedded in our surveys. We start by examining whether biased workers make mistakes when choosing a job. We find that 45% of workers in our sample move from being above to being below their stated reservation wage when we use their actual (rather than perceived) net hourly pay. This number is probably an overestimate and can be partially explained by other factors. Nevertheless, even in our most conservative specification, we find that 17% of gig workers would be put below their outside option if they knew their actual take home pay. Thus, we find evidence that biased gig workers make sub-optimal employment choices.

Next, we examine if work hours are under-smoothed across the household budget cycle. We elicit up to two days in the month in which workers have to pay major bills. Using this information, we find that gig labor supply is higher close to when bills are due. Gig workers in our sample work, on average, 40 percent (or 3 hours per week) more when a major bill is near due, compared to when it is at least 3 weeks away. In our model, this can be explained by workers earning less than expected at the beginning of the budget cycle, as they over-predict their pay. In order to pay the bills, they then need to work more hours than planned. This leads to a loss in welfare if effort costs are convex.

We further investigate how misperceiving job outcomes affects gig workers by implementing a randomized de-biasing treatment as part of our surveys. In this intervention, we show the treatment group a comparison between their beliefs and their actual expected net hourly pay. By doing so, we make gig workers aware of their mistaken beliefs. If workers become less biased as a result, this should push them to make different labor market choices.

We are underpowered to detect small to moderate effects. However, we find suggestive evidence that financially secure gig workers reduce work hours after learning they make less money than originally thought. On the other hand, de-biased gig workers facing stronger budget constraints tend to work the same or more. Treatment effects are larger for workers whose pay is initially underestimated. This is consistent with our discussion of motivated beliefs: it is more difficult to dissuade workers from believing they are highly paid.

Our paper builds on the behavioral economics literature on the existence and persistence of biased beliefs such as overconfidence.[1] In particular, we closely relate to the theoretical (Bénabou and Tirole, 2002; Bénabou and Tirole, 2016; Köszegi, 2006) and empirical (Eil and Rao, 2011; Godker et al., 2022; Gottlieb, 2010; Moebius et al., 2022; Saucet and Villeval, 2019; Sial et al., 2022; Zimmermann, 2020) literature linking mistaken beliefs to the functioning of memory.[2] We contribute to this literature by being one of the first to apply these ideas to the labor market (Hoffman and Burks, 2020; Huffman et al., 2022), by emphasizing the importance of flexibility in generating mistaken beliefs, and by documenting more thoroughly the implications of mistaken beliefs on labor market decisions.

This paper also relates to a growing literature in labor economics documenting how workers lack information about several variables necessary to make optimal labor supply decisions. For instance, they can have biased beliefs about their outside options (Jäger et al., 2022) and job market prospects (Bandiera et al., 2022; Banerjee and Sequeira, 2020; Conlon et al., 2018; Cortes et al., 2022; Mueller et al., 2021), as well as how their performance (Huffman et al., 2022) and compensation compare with that of their peers (Card et al., 2012; Cullen and Perez-Truglia, 2022). These beliefs have been shown to affect their search behavior, employment decisions (Bergman and Jenter, 2007; Larkin and Leider, 2012; Oyer and Schaefer, 2005) and other labor market choices. We contribute to this literature by investigating a parallel under-explored question: whether workers have accurate beliefs about their inside option – i.e., their current job.

Our work also contributes to the literature on flexible jobs (Camerer et al., 1997; Crawford and Meng, 2011; Farber, 2015; Fehr and Goette, 2007; Thakral and Tô, 2021) and the gig economy (Angrist et al. 2017; Bernhard et al., 2022; Chen et al., 2019; Collins et al., 2019; Cook et al., 2021; Hall et al., 2021; Katz and Krueger, 2019; Koustas, 2018; Mas and Pallais, 2017; Parrott and Reich, 2020). These jobs allows workers to choose their work schedule and are characterized by pay that fluctuates over time. A key question in this literature is how labor supply decisions are made. Our paper provides additional insight into this topic by finding that workers misunderstand key job outcomes in this setting, and that these biases influence labor supply choices.

The paper is structured as follows. Section 2 presents the survey design and describes the data. Section 3 presents the baseline results on mistakes in recalls and forecasts of job outcomes. Section 4 shows that these mistakes can be explained by a combination of motivated beliefs and selective recall. Section 5 derives and tests the empirical predictions

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1 See, for instance, Camerer and Lovallo (1999), Grubb and Osborne (2015), Healy and Moore (2007), Malmendier and Tate (2015), Puri and Robinson (2007) and Sharot (2011).

2 In addition, see Adler and Pansky (2020), Carlson et al., (2020), Chammat et al. (2017), Di Tella et al. (2015), Enke et al. (2020), Maréchal (2020), Mischel et al. (1976) and Schacter (2008).

of a simple behavioral labor supply model in which workers have mistaken beliefs. Section 6 concludes and shows directions for future work.

1.2 Survey Design and Data

We study ride share and delivery gig workers for online platforms in the United States. These jobs are characterized by the use of platform apps to accept gigs, pay that varies with demand and skill, full flexibility in hours and a responsibility for workers of paying for most expenses. We only consider companies for which the earnings page in the gig platform app includes not only information on pay but also on hours worked. This choice, made to increase the range of outcomes we observe at these jobs, implies we do not study gig work done for Postmates, Amazon Flex, Shipt, and others.

We consider in our data work done for five gig economy companies: Uber, Lyft, Uber Eats, DoorDash and Instacart. Uber and Lyft combine to be almost the entirety of the ride share market, while DoorDash and Uber Eats represent over 80% of the food delivery market in the United States. Finally, Instacart has a market share of around 45% of the American grocery delivery market (Bloomberg Second Measure, 2022).

We collected online survey data from gig workers from April to November of 2022. We recruited participants to our study using social media posts, social media ads, and gig economy newsletters. We invited participants with the following prompt: "*If you're a driver in the gig economy, answer our online survey to get $10 and a personalized report*".[3] Only people who worked in the past 3 months for at least one of the five gig companies we consider could take part in our study.

If a participant finishes our baseline survey, we invite them through email to answer two follow-up surveys: the midline and the endline surveys. The midline happens 1 to 2 weeks after the baseline, depending on when the first full week (starting on a Monday) following the baseline survey ends. The endline, in turn, happens 2 to 5 months after the baseline survey. We collected all endline surveys in October and November of 2022. Figure 1.1 summarizes the design of our surveys. We paid participants $10 for participating in our baseline and midline surveys and $20 for answering our endline survey.[4]

Belief Elicitation

Unless otherwise noted, all beliefs of job outcomes elicited over our three surveys refer exclusively to the gig company that the worker worked the most for in the previous 3 months.

[3] We also used other similar formulations: "*Drivers in the gig economy: Join our online academic study and receive a $10 gift card.*", "*Do you do food delivery or rideshare gig work? Participate in our online survey to receive at least $10.*" and "*Complete a 15 min survey from a UC Berkeley graduate student researcher for US drivers of DoorDash, Uber Eats, Instacart, Uber or Lyft. Receive a $10 Amazon gift card and see how you compare to other drivers.*"

[4] Participants that answered our follow-up surveys were also given a personalized report summarizing (i) their own pay and (ii) the average pay in our sample at their gig company. As described below and detailed in Appendix E, half of participants actually received information (i) at the end of the baseline survey in the form of a randomized information treatment.

We start the baseline survey by eliciting recalls from workers about their job outcomes. We ask two thirds of respondents for a recall of the last month they worked for the gig company and the other one third for a recall of the last week. In our setting, weeks are defined to always start on a Monday and end on a Sunday. We vary the recall period to test how the accuracy of beliefs differs depending on the time frame they refer to.

We focus on recalls of gross weekly pay, weekly expenses, weekly hours, gross hourly pay and net hourly pay. We construct the recall of net weekly pay by subtracting the recall of weekly costs from the recall of weekly gross pay. We calculate the recall of expenses share out of total pay by dividing the recall of weekly costs by the recall of weekly pay. Appendix Figure A.1 shows an example of our recall belief elicitation questions.

Next, we ask workers to forecast their job outcomes in the next week (starting on the following Monday) after the baseline survey. We only ask for forecasts of workers who say they are very or somewhat likely to work during this week, which is around 85% of our sample. The forecast for gross weekly pay is incentivized. In particular, we use a Quadratic Scoring Rule to define a payoff based on accuracy, with a bonus that goes up to $5 and is $0 if the absolute value of the prediction error is equal to around $160 or higher. Thus, workers have an incentive for truth-telling.[5] Forecasts for weekly hours and gross hourly pay are also elicited but are not incentivized. Figure A.2 shows an example of our forecast belief elicitation questions. The final part of the baseline survey is the randomized information treatment. We provided half of the sample with information on whether they correctly assess their net hourly pay. We detail our information treatment in Appendix E.

Both the midline and the endline surveys are simplified versions of the baseline. In the midline survey, we measure the accuracy of the forecasts elicited in the baseline. We again elicit recalls for key job outcomes. The recall period is the same week as the forecasts refer to. We then show participants how their forecast of weekly pay matched reality, and inform them of their accuracy bonus. For the information treatment group, we repeat the information we gave them on the baseline survey.

In the endline survey, we elicit recalls referring to the average of the four previous weeks in which participants worked for the gig company. This survey was mainly designed to measure the medium-run effects of the information treatment. Those in the control group receive the information treatment at end of the endline survey. We only elicit beliefs about the baseline gig company during the follow-up surveys if participants report doing work for that company during the relevant time period. Throughout all surveys, we gather a rich set of covariates referring to demographics, job market history, work habits, and other secondary beliefs.

[5] We used the Quadratic Scoring Rule (QSR) due to its simplicity. In particular, the payoff function is defined as $\max\left\{5 - \frac{(X-\tilde{X})^2}{5000}, 0\right\}$, where X is the actual weekly pay for the relevant week and $\tilde{X}$ is its forecast. Participants have to click a button to see an example and the formula of the payoff function. The QSR has been shown to not be robust to risk-aversion, but we believe more complex alternatives would be more problematic, as discussed in Danz et al. (2020) and Charness and Gneezy (2021).

Labor Market Outcomes

We obtain information on labor market outcomes by asking workers to submit screenshots of the weekly earnings page from the gig company app. As with beliefs, this gig company is the one they worked the most for in the past three months. In all three surveys, we ask for these screenshots only *after* gig workers state their beliefs, and have no possibility of modifying them. Figure 1.2 shows examples of the screenshots we request. They have information on, among other variables, weekly hours and gross weekly pay for a particular week. Each screenshot prominently displays the gross weekly pay in large font at the top. We require participants to submit at least one screenshot to complete each survey.

As part of the baseline survey, we ask workers to upload a screenshot of their last week of working for the gig company. Participants receive a bonus of $2 if they agree to submit an additional three screenshots, referring to their three previous weeks working for the gig company. In the midline survey, we ask for a screenshot of the weekly earnings page for the forecast week. In the endline survey, we ask workers to upload screenshots of the weekly earnings page for up to twelve weeks in the period between the midline and endline surveys. In this survey, we pay participants a bonus of $1 per screenshot submitted beyond the first one.[6]

Our measure of weekly hours is the total amount of hours spent online in the app (that is, time spent actively on gigs plus time spent waiting for gigs) in a given week. This is the same definition of labor supply as some of the previous literature on the gig economy (Chen et al., 2019; Angrist et al., 2021; Cook et al., 2021). It is also a measure available for all the gig companies we consider. In addition, this measure captures the nature of a standard job, in which only part of the time is spent actively working. We calculate the average gross hourly pay by dividing the gross weekly pay by weekly hours.

Accounting for Expenses

We do not observe work expenses for gig workers in our sample. As a result, we use an estimate of expected costs of driving to do gig work to calculate the expected actual net pay, a measure relevant for consumption decisions. We take a conservative approach and only consider the main variable costs, which are: fuel, maintenance and repair, variable depreciation and taxes. Our cost measures for maintenance, repair and variable depreciation are based on the AAA Your Driving Costs 2022 guide. Fuel costs are an average of gas prices, also from AAA, in the three months before the baseline survey.

Using survey information on which car is used to do gigs, we estimate expected expenses across 27 groups, combining 9 car categories and 3 car age groups (0-5, 5-10 and 10+). Estimates from the AAA Your Driving Costs guide apply only to the first 5 years of owning a car. We adjust for variation of maintenance, repair and depreciation costs over a car's lifespan by using information from CarEdge. In particular, we find the ratio of how (i)

[6] In some cases, workers are asked for recalls referring to the past month but only agree to submit screenshot information referring to one week. When studying mistaken beliefs about job outcomes, we use this one week as a proxy for the last four.

depreciation and (ii) maintenance and repair costs compare for a car that is either 5 to 10 or over 10 years old and a car that is between 0 and 5 years old. We then apply this ratio to the estimates from the AAA guide.

We apply the IRS mileage rate deduction in 2022 ($.585/mile) to calculate self-employment taxes. We estimate federal income taxes by combining reported yearly household income, an estimate of gig income over the year and by applying the standard deduction. We ignore state income taxes for simplicity. Appendix Table A.13 shows an example of our calculation of expected expenses. We do not estimate expenses for workers who rent a car to do gig work or that use a bike or a scooter. We discuss additional details of our expenses estimation in Appendix B.

Sample Selection and Descriptive Statistics

We screen out non-active gig workers from our sample by only including participants who submit valid screenshots from the gig company app. For a screenshot to be considered valid, it has to satisfy three conditions: (i) weekly pay is visible and legible, (ii) the screenshot is not findable on reverse image search, and (iii) the week at the top of the screenshot is at most 3 months before to when the baseline survey was taken. A total of 13 survey responses with identical emails or identical screenshots as other responses were also excluded from our sample.

When we make these restrictions, we end up with 454 baseline survey responses. 51% of workers in our sample have DoorDash as their main gig company for the past three months. This number is 20% for Instacart, 13% for Uber Eats, 10% for Uber and 5% for Lyft. Furthermore, we have 210 midline survey responses and 202 endline survey responses. These numbers imply a response rate of 46% at the midline survey and of 50% at the endline survey. We trim the top and bottom 1% of all forecasts, recalls and actual job outcomes. Our results are robust to changing this trimming cut-off to 2% or 3%.

Summary statistics for our baseline survey sample are presented in Table 1.1. In Panels A through C, we present information for our main covariates. The vast majority of gig workers in our sample are between 18 and 54 years old. They are over 70% white and majority female. Around 38% of them have at least a complete college degree, and a little over half of our sample has a household income of $40,000 per year or less. We find that 42% of respondents are struggling financially and 83% of them consider the pay from their gig work to be essential.

Over half of gig workers in our sample has another job – either a gig or a non-gig one. Around 10% were unemployed before starting doing gigs, while 36% had a full-time job. In addition, 23% of workers have at least a year of gig ride share experience, while that number is 57% for gig delivery experience. The five most common cars that gig workers in our sample drive are: Hyundai Elantra, Toyota Corolla, Honda Civic, Honda Accord and Hyundai Sonata. About 80% of participants drives either a small sedan, a medium sedan, a compact SUV or a medium SUV. In addition, the median car age is 8 years old.

In Panels D through F of Table 1.1, we present summary statistics for our main outcome variables. Gross pay is, on average, $18 per hour and $284 per week. Average hours worked is equal to 17 per week. Note this includes only weeks with a positive amount of work hours. Our estimated expected expenses share is, on average, 32% of total gross earnings.

We observe that, generally, recalls and forecasts have higher averages than actual job outcomes. As in the rest of the paper, we pool together recalls for the past week and the past month. For instance, the average forecast of gross weekly pay is around $376/week, while the average recall of weekly hours is around 22 hours per week. In addition, recalls and forecasts of job outcomes are very similar. Outcome variables vary widely across workers, reflecting the nature of flexible gig work.

We analyze selective attrition across our three surveys in Appendix Table A.1 and Appendix Table A.2. There is some indication that participants in our follow-up surveys are more likely to be more college educated and are perhaps a little richer. However, taken broadly, we cannot reject that the mean observable characteristics are overall the same across the surveys.

Appendix Table A.3a compares the share of workers in our sample who have done any gigs for different gig companies to how many people each of those companies contract in the United States. We find evidence that DoorDash workers, and delivery workers more broadly, are over-represented in our sample relative to Uber and Lyft drivers. Appendix Table A.3b shows how key summary statistics in our sample of gig workers compares to recent previous surveys on the gig economy (Parrott and Reich, 2020; Pew Research Center, 2021; Doordash, 2021). We find suggestive evidence that our sample is younger, more white and more educated. In addition, participants in our study appear to work more hours than the median gig economy worker.

1.3 Mistaken Beliefs about Job Outcomes

Forecasting Errors

In this section, we explore whether job outcomes are misperceived by gig workers. We begin by looking at forecasts of gross weekly pay for the next week. This is a consequential belief, essential for labor supply and consumption choices. Weekly pay is also the most salient job variable when checking the gig platform app, as the screenshots in Figure 1.2 show. Furthermore, our sample of gig workers had a special incentive to get this prediction right, as we provided them with an accuracy bonus of up to $5.

Figure 1.3a shows a scatter plot comparing the forecast and the actual gross weekly pay for each gig worker. The forecast refers to the first week following our baseline survey. We only include participants who answered our midline survey, as otherwise we cannot measure the accuracy of their forecast.[7] We draw a 45 degree line to represent the case where forecasts exactly match actual realizations. Points above this line indicate overestimation, while points below this line imply underestimation of gross weekly pay.

[7] We include workers in both the treatment and control groups of our information treatment in this analysis, as we asked them both to forecast their job outcomes. We allowed treated individuals to review their forecast of weekly pay after the information treatment. Mistakes are larger when we only the control group is considered.

Due to noise and variation in gig pay over time, points are unlikely to be positioned neatly on the 45 degree line even if workers' beliefs are correctly calibrated. However, in the absence of systematic biases, points should be positioned *symmetrically* around this line. Looking at the results, we find that points are concentrated above the 45 degree line, indicating that the majority of the sample over-predicted their weekly pay. Furthermore, this asymmetry holds across most of the distribution, and is not dependent on outliers.

We define overestimation of a job outcome (either for a recall or a forecast) as the belief minus the actual realization. We say a worker underestimates a job outcome if this measure is negative. We present statistics using this variable in Table 1.2 Panel A. We find that overestimation of forecasts of gross weekly pay is significant: it is 43% of the actual weekly pay, or around $110, on average. 72% of our sample is identified as overestimating their forecast. These measures are statistically significant at 1% against the null of no overestimation. As seen in Figure 1.3a, the slope of a linear regression of forecasts on the actual weekly pay is around 0.8. This implies that forecasts are significantly related to actual outcomes and that overestimation is, broadly, decreasing on actual weekly pay.

Recall Errors

We have shown that workers make large mistakes when they predict their weekly pay. Nevertheless, forecasting job outcomes is by nature a complex task in flexible jobs. As such predictions require taking into account a complex set of shocks and understanding their exact relationship with hours and pay, it might not be surprising that workers can fail to do them correctly.

We now tackle a much simpler problem: *recalling* gross weekly pay. A gig worker has easy access to the actual value of this variable for all periods in which they worked for the company. That is, they can know their exact gross total pay in every week. Consequently, errors in recall will indicate additional difficulties in understanding gig pay.

We pool recalls of the previous week and month before the baseline survey. On Figure 1.3b, we see a similar pattern as with forecasts: points are concentrated above the 45 degree line, and most participants seem to over-recall their weekly pay. The statistics in Table 1.2 confirm this suspicion. Overestimation of recalls is both statistically and economically significant. We find that gig workers exaggerate their actual weekly pay by over 30%, or around $90/week. Over 70% of gig workers in our sample overestimate their recall of weekly pay. These measures are statistically significant at 1% against the null of no overestimation.

Thus, biases in forecasting and recalling weekly pay are similar in magnitude. This is true despite the fact that weekly pay predictions included incentives for accuracy while recalls did not. This is consistent with workers stating their true beliefs even without being incentivized. The similarity between recall and forecast errors may also indicate that mistaken beliefs about the future are tied to incorrectly remembering the past.

This suspicion is tested in Figure 1.4b by graphing, at the individual level, forecast overestimation against recall overestimation for weekly pay. The slope coefficient of the regression between these two types of mistakes is 0.51, which is statistically significant at 1%. Figure 1.4a re-does this analysis for forecast versus recall *beliefs*. Here, the relationship is even stronger (coefficient of 0.9), suggesting that gig workers see these two beliefs as nearly equivalent.

We elicited recalls and forecasts at different points in the survey and on pages with very different layouts, as one can see by comparing Appendix Figure A.1 and Appendix Figure A.2. Therefore, confusion is not likely to be the cause for this result. From now on, we will sometimes focus on recalls as a short-hand for both types of errors due to the similarity between them and to allow us to take advantage of a larger sample size.

Gross versus Net Pay

The previous sections have documented that gig workers overestimate their future and past weekly pay. However, we have not taken into account the expenses and taxes associated with gig work. Costs are a significant margin at these jobs, since workers are generally responsible for paying them. This adds a new layer of complexity to understanding pay. At the same time, the net weekly pay, which represents the amount of income from gig work left over for consumption, is likely to be of special importance to workers.

Taking expenses into account is also relevant when thinking about the consequences of errors in understanding job outcomes in gig work. For instance, a gig worker that overestimates both gross weekly pay and expenses can end up holding correct beliefs about their expected net weekly pay. In this scenario, their mistaken beliefs might not result in sub-optimal labor supply and consumption decisions.

As previously mentioned, we calculate a measure of expected costs by car category and car age group, taking into account variable operating expenses and taxes (see Appendix B for more details). Our estimates will likely not reflect true costs at the individual level in particular weeks, but they should be close to reality on average. We calculate the actual expected net weekly pay by discounting the gross weekly pay by our measure of the expected expenses share.[8]

We find more evidence of mistaken beliefs after adjusting for expenses. In Table 1.2, we see that workers significantly overestimate their recall of net weekly pay by about 46%, or around $90 per week. As shown in Appendix Figure A.3, a scatter plot relating beliefs and the actual expected net weekly pay reveals a sizable grouping of individuals above the 45 degree line, where recalls are greater than the expected net pay.[9]

Decomposition of Mistakes

Net weekly pay is the product of three factors: average gross hourly pay, an expenses discount and weekly hours worked. This allows us to decompose total errors in understanding this outcome into individual errors along these three dimensions. Prior to discussing this decomposition, we examine the average overestimation for each outcome separately. Table 1.2 Panel B summarizes recall results once again. First, note that sample sizes differ across

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8 Our estimated expected expenses share is, on average, 32% of total gross earnings.

9 Note that our error in capturing variations in expenses across time partially explains the dispersion seen in this graph.

outcomes. The reason for this is a combination of trimming, that not all screenshots contain hours information, and that, as mentioned before, we do not calculate expected expenses for certain types of gig workers, such as those who use bikes or scooters.

Overestimation of weekly hours is around 6 hours a week, or 31% of actual hours worked. Gig workers underestimate expenses by, on average, about 22% or 7 percentage points. Gross hourly pay is overestimated only by a small non-statistically significant magnitude. However, this analysis is based on recalling the average hourly pay over entire weeks. Our finding does not imply that workers are able to recall or predict variation in gross hourly pay over particular hours, for instance. Finally, we find that net hourly pay is over-recalled by, on average, 20.7%.

Table 1.2 Panel A shows forecast errors for these outcomes. Weekly hours are overestimated by about 8 hours, or 63% of actual hours driven. Once again, gross hourly pay is overestimated by only a small, non-significant amount. For both recalls and forecasts, about 70 percent of gig workers overestimate (or underestimate in the case of expenses) each individual outcome, with the exception of average gross hourly pay. These shares are statistically different from 50% at the 1% significance level. Thus, we find strong evidence of significant overestimation in recalls and forecasts across most job outcomes in gig jobs.[10]

We now decompose the total error in recalling net weekly pay into three categories: average gross hourly pay, the expenses discount and weekly hours worked. Multiplying the recall for each of these components gives us the *implied* recall of net weekly pay. The average gig worker overestimates this implied measure by 53.3% of the expected actual net weekly pay. This is greater by about $50/week to what we found using another definition of net weekly pay recall (recall of gross weekly pay minus recall of weekly expenses).

In the next step, we replace each element of the implied net weekly pay recall with its correct equivalent. For instance, we replace the recall of gross hourly pay by its actual value, while keeping recalls of expenses and hours in place. A comparison between this multiplication and the original implied belief reveals the importance of gross hourly pay errors *only*.

Figure 1.5 shows the results of this exercise. In this figure, we show the average pay for four possibilities related to this decomposition of net weekly pay: (i) implied recall, (ii) correct gross hourly, (iii) correct weekly hours; and (iv) correct expenses. By replacing recalls by their actual equivalent for each individual outcome, net weekly pay drops by 57% for hours, 36% for expenses and 11% for gross hourly pay. Hence, underestimation of expenses and overestimation of hours explain most of why workers overestimate their take home weekly pay. Therefore, both hours and net pay flexibility can cause misperceptions.

[10] Appendix Figure A.3 and Appendix Figure A.4 present additional scatter plots relating beliefs to actual job outcomes. Appendix Table A.4 shows that overestimation is generally positively correlated across different job outcomes, with the exception being hourly pay and hours. Appendix Figure A.5 and Appendix Figure A.6 show a significant relationship between forecast and recall errors and forecast and recall beliefs for both gross hourly pay and weekly hours. This means that the strong ties between the two types of beliefs extend beyond weekly pay.

Additional Results and Heterogeneity Analysis

In our baseline survey, we asked people to recall either the last week or the last month in which they worked for the gig company. Appendix Table A.5 shows that overestimation is generally larger when workers are asked to recall an average of the last 4 weeks, compared with just last week. Appendix Figure A.7 shows the results of regressions of recall beliefs for the past month on the last four weeks of actual realizations of the same outcome, ordered by recency, and a constant. For gross weekly pay, we find that recent periods influence beliefs more than older ones. These findings are consistent with individuals having a more difficult time remembering events that happened further ago in the past.

Appendix C details our analysis of beliefs about the average worker at the same gig company. We elicited these beliefs in all three of our surveys for a sub-sample. Workers believe the average gig worker works more hours but earns a similar amount per hour, both before and after expenses. We find no evidence of overplacement (Healy and Moore, 2007). That is, workers' belief about their pay differential relative to the average gig worker is not higher than the actual difference in earnings between the two.

In Appendix Table A.6, we examine whether overestimation is heterogeneous by worker characteristics. Within each table, we regress our measure of recall overestimation for different job outcomes on variables summarizing key attributes. In Panel (A), we divide the sample into (i) workers who are entirely or somewhat certain of their recall beliefs and (ii) workers who are neither certain nor uncertain, somewhat uncertain, or not certain at all of their recall beliefs. We find some evidence that overestimation is larger for workers more certain of their recalls. This is problematic because this group may be less likely to obtain information about their actual gig pay and work hours.

Panel (B) examines whether errors vary by whether workers are full-time gig workers or not. Full-time gig work is defined as working at least 30 hours per week, on average. In our sample, 23% of workers satisfy this definition. The results indicate that full-time gig workers overestimate their hourly wages by more, but underestimate their weekly hours. The combination of these effects implies the same amount of overestimation of weekly pay recalls.

Panel (C) of Appendix Table A.6 focuses on financial need. Results show that workers who rely more on gig pay tend to overestimate their hours and weekly pay by more. This is significant, since these workers are likely to face more consequences from not knowing their pay. In Panel (D), we examine demographic characteristics (age, gender, race, education, and household income). Overall, we find no statistically significant correlation between these characteristics and mistakes in recalling job outcomes. In Panel (E), looking at labor market information, we show that gig workers previously employed full-time overestimate their weekly pay and hours by more.

Finally, in Table 1.3, we examine how misestimation varies with gig work experience. We find that gig workers with more than one year of experience in both rideshare and delivery overestimate their weekly pay by around $70 *more* than inexperienced ones.[11] Thus, not only is experience not associated with gig workers holding more accurate beliefs, it is correlated

[11] We provide an alternative analysis in Appendix Table A.7, by separately considering delivery and ride

with larger errors. As we discuss below, this result reflects a mix of incomplete learning and selection of biased workers over time at these jobs.

Robustness

In this section, we discuss alternative explanations for some of our findings and conduct robustness checks. We categorize possible issues into three categories: selection, measurement of beliefs, and measurement of outcomes. We address each set of potential problems in turn.

We first argue against the concern that our overestimation results come from disproportionally selecting biased workers into our sample. First, our sample is likely to be more sophisticated and more informed than the average gig worker, as we partially recruited participants from gig worker groups and gig economy newsletters. Additionally, Appendix Table A.3b indicates our sample is more educated than the median gig worker. Previous research has found that education is negatively correlated with behavioral biases (Stango and Zinman, 2020).

Next, our heterogeneity analysis in Appendix Table A.6 has shown that mistakes are widespread across a wide range of demographic and other characteristics. For this reason, selection along many common dimensions would not be enough to reproduce our findings. Finally, a pilot study conducted without monetary incentives and with no information treatment found overestimation of job outcomes of a similar magnitude to what we document here. This variation in incentives to answer our survey likely attracted different types of gig workers. Nevertheless, the fact that both of them produced similar results reinforces our conclusions that sample selection cannot explain our results.

We believe our findings are not the result of measuring recall and forecast beliefs incorrectly. First, we explicitly ask gig workers to consider all elements of pay, including tips, bonuses and platform fees. Another possibility is that workers round up their beliefs when answering our survey. Appendix Table A.8b shows that considering only workers with beliefs that are not round numbers implies qualitatively similar overestimation results.

We elicit beliefs about job outcomes only for one gig company. However, a significant number of gig workers in our sample (42%) worked for more than one platform in the previous three months. If workers do not correctly understand our survey questions, they might report total pay and hours across multiple companies. Appendix Table A.8a shows overestimation of job outcomes is still economically and statistically significant if we only consider the group who worked for only one gig company.

As a result of social desirability bias, workers may intentionally overstate their beliefs. We do not believe this is driving our results for a number of reasons. First, remember that predictions of weekly pay were incentivized with a monetary bonus for accuracy. This raises the costs of inflating beliefs and should make beliefs more accurate in the presence of a desire to impress researchers. Nevertheless, we found substantial overestimation in weekly pay forecasts. Second, work hours are less likely to be overstated in this way. Indeed, working

share experience. We also include other job outcomes. We find very similar results.

additional hours implies a lower hourly pay and does not clearly carry a self-image benefit (required for social desirability bias). We have shown, however, that workers significantly overestimate their hours worked.[12]

Finally, during the midline and endline surveys, workers are aware that we might ask them to later submit screenshots containing their job performance. As such, inflating beliefs for the sake of impressing researchers makes less sense, since participants can infer that their answers can be directly compared to reality. Appendix Table A.18 shows that, in the follow-up surveys, gig workers in the control group of the information treatment still significantly overestimate job outcomes.

We discuss robustness checks for our measurement of job outcomes, including a discussion on the distinction between online and active hours, in Appendix D. To test the robustness of our measure of expected gig work costs, we now propose an alternative formulation of errors in understanding this outcome. We first ask workers which categories of costs they consider when calculating their net pay. Results are shown in Appendix Figure A.8. Many gig workers report ignoring types of costs such as maintenance (45%), taxes (65%) and depreciation (83%).

Based on this information, we calculate an implied measure of belief about expenses. Specifically, we assume drivers use only the cost categories they self-report to take into account. We then input *our own* expenses estimates for each of these categories. In this alternate estimate, only errors resulting from ignoring some types of expenses are considered. In reality, errors also arise from wrong beliefs about the expected cost of repairs, for instance. Using this measure, we still find that gig drivers underestimate expenses by about 5 percentage points, or 15%. In other words, mistaken beliefs about expenses are not due to our estimates of actual costs of different types.

1.4 Explaining Mistaken Beliefs

In the previous section, we documented that gig workers overestimate both their prior and future earnings by significant margins. This section attempts to understand why these errors occur. The proposed mechanism should be able to explain why beliefs are biased toward overestimation, rather than being inaccurate in both directions. In addition, we found before that forecasting and recall errors are correlated. In light of this, we must also take into account the connection between inaccurate memories and wrong beliefs about the future.

We consider motivated beliefs (Bénabou and Tirole, 2002; Bénabou and Tirole, 2016) to be a likely explanation for the patterns we observe. In this framework, agents hold incorrect beliefs due to hedonic utility or as a motivational tool. For instance, a person might enjoy believing they are highly paid, smart or productive.

Motivated beliefs are developed and maintained by selective recall: favorable memories are more easily accessed than unfavorable ones. By relying on a biased memory, forward-

[12] Appendix C documents that gig workers in our sample do not believe they earn more than the average gig worker. This piece of evidence is also inconsistent with the standard formulation of the social desirability bias.

looking beliefs used in making decisions are distorted. For example, a person with selective recall may remember days when they exercised more readily than other days. By doing so, one reinforces the desirable perception of being healthy and active. However, this may taint predictions regarding future gym attendance. Mistakes will occur even if the agent has a correct function for inferring forecasts from recalls. Indeed, the key error lies in failing to appreciate that recalls are partially chosen to justify particular beliefs.

In the same way, new information does not necessarily lead to more accurate beliefs: updating is not Bayesian, but it reacts disproportionately to positive news, which have a more lasting effect on beliefs. Therefore, incorrect views can persist over time and additional experience may not make mistakes disappear. There are limits to belief manipulation in this framework. Nevertheless, these constraints can be relaxed when signals are less informative and the environment is noisier, since this allows for additional leeway in choosing unrepresentative memories.

Recent empirical literature finds extensive evidence for overly optimistic beliefs tied to selective memory. Many domains have been studied, including investment decisions (Godker et al., 2022), intelligence (Zimmermann, 2020; Moebius et al., 2022), beauty (Eil and Rao, 2011), and generosity and altruism (Saucet and Villeval, 2019; Di Tella et al., 2015; Carlson et al., 2020).

Evidence of Motivated Beliefs

We now apply the predictions of this theory to our setting. Gross weekly pay is the main outcome we consider in our analysis. Assume gig workers want to believe they are highly paid. A favorable memory, such as a period with high pay, will support this belief, while an unfavorable memory will contradict it. Unless otherwise noted, we use only data from the baseline survey.

First, we examine whether high-paying periods influence recalls more than low-paying ones. This may happen if gig workers remember high-paying weeks more easily. This analysis only includes workers asked to recall the average of the last four weeks. Our first specification regresses recalls of gross weekly pay on the maximum and the minimum of the last four actual realizations. In the absence of biases, a one-unit increase to either the maximum or the minimum should affect recalls equally. Indeed, the recall *should* be a simple average of the weekly pay in the last four weeks. We find that this is not the case. Results are shown in the first column of Table 1.4a.[13] Increasing pay in the workers' highest paying week by \$100 is associated with a rise in recall of \$57, compared to \$26 for a similar increase in the lowest paying week.

In column (3), we regress the recall of weekly pay on both the actual mean of the past 4 weeks and the maximum of these same weeks. If workers rationally form their beliefs, the coefficient on the maximum week should equal 0, since the mean is a sufficient statistic for the recall. Yet, we find that not to be the case. Indeed, the maximum week has a statistically significant coefficient that is half as large as the coefficient on the mean. Thus,

[13] Appendix Table A.11 shows the same set of results, but for forecasts.

the best periods seem to be overweighted by gig workers in belief formation, as predicted by selective recall of favorable information.

We now evaluate whether workers update their beliefs asymmetrically when presented with new information about their pay. We estimate how the weekly pay recall at the midline survey relates to the relative comparison of (1) the actual weekly pay in the week that the recall refers to, and (2) the previous recall belief, elicited in the baseline survey. We expect workers to update more strongly to positive than to negative news.

We apply this idea in a simplified way by dividing weekly pay realizations into two: those above or equal to the previous recall, and those below. We then run regressions of the midline weekly pay recall on the previous recall and two binary variables, equal to one when the actual weekly pay realization is (i) above or equal to or (ii) below the original recall belief.

Results are shown in Table 1.4b. We find that gig workers update their beliefs more strongly after a positive realization. The recall of weekly pay is estimated to increase by $208 when the actual weekly pay is above or equal to the previous belief, and to fall by only $48 when the opposite is true. This asymmetric in reacting to new information can justify the persistence of biased beliefs.

These findings are consistent with the previously discussed Table 1.3, which examines how mistaken beliefs vary with gig work experience. We found that not only is gig experience not associated with gig workers holding more accurate beliefs, it is correlated with larger errors. Our correlational estimates of experience effects are influenced by both selection and learning. Workers may become significantly better at measuring their job outcomes over time. Nonetheless, there may be a stronger countervailing effect caused by overconfident individuals who stay at gig jobs longer, precisely because they believe their pay is higher than it is.

We can test whether this is the case by examining the average levels of overestimation in our endline survey, which was conducted two to five months after our initial survey. We ask for a recall of the past 4 weeks. By comparing mistaken beliefs from this survey with errors in the baseline survey, we can estimate learning effects. Appendix Table A.18 shows the results. The average level of overestimation across job outcomes is 30% to 40% lower at the endline survey (compared to one month recalls shown in Appendix Table A.5). Thus, while there is some degree of learning, it is insufficient to counter the likely selection of overconfident gig workers at these jobs.

We now evaluate whether more variation in job outcomes, by relaxing the constraints on belief manipulation, leads to more overestimation of weekly pay. Indeed, more uncertainty in outcomes might make it easier to select unrepresentative memories and base one's beliefs on them. The same mechanism might also explain why workers in non-flexible jobs do not appear to overestimate their earnings.[14] In fact, studies comparing survey data with tax data reveal a slight *under*-reporting of labor income in surveys (Moore et al., 2000; Rothbaum, 2015).

[14] See, for instance, Appendix Figure C.4 in Cullen and Perez-Truglia (2022).

We measure noisiness in weekly pay realizations across four weeks as the coefficient of variation, which is the standard deviation divided by the mean. Results are presented in Table 1.5. We find that a higher coefficient of variation leads to more overestimation: increasing this variable by 0.1 (mean of 0.48) is associated with an increase in weekly pay recall overestimation of around \$9, which is statistically significant at 1%. This result supports the view that the flexibility of gig jobs is the key aspect behind overestimation of job outcomes.

We re-do our previous analyses for weekly hours and gross hourly pay in Appendix Table A.9 and Appendix Table A.10. Generally, the results are similar for hours. One possible interpretation is that believing one works long hours helps the belief that weekly pay is high. Our results for hourly pay are generally consistent with a lack of significant overestimation in this variable, as we don't find much evidence for the mechanisms discussed above. Alternatively, this can be explained by each realization of hourly pay in our data being only its weekly average, not allowing us to not incorporate intra-week variation.

We have not discussed the mechanisms underlying underestimation of gig job expenses. This is because we do not observe variation in actual expenses over time at the individual level. We believe it is reasonable to assume similar mechanisms than the ones just discussed are at play. For instance, when forming their beliefs, gig workers may overlook periods when their car broke down or when they had to pay taxes.

There is one last point we want to make before moving on. Overestimation of weekly pay following the patterns predicted by the theory of motivated beliefs with selective recall is another argument in favor of us identifying true mistakes. Indeed, any suggested confounder is now also required to explain why overestimation in our settings fits the predictions of this theory.

1.5 Consequences of Mistaken Beliefs

In this last section, we explore the consequences of workers consistently overestimating key job outcomes, such as gross and net pay. This is critical, as these mistakes will be less important if they do not lead to welfare-reducing labor market decisions. We derive potential implications from mistaken beliefs by developing a simple behavioral labor supply model. We then test the model's predictions using survey data as well as a randomized de-biasing information treatment, in which workers are informed of their mistakes in understanding their net wages.

A Model of Labor Supply

Consider a model in which a worker first chooses a job, consumes (c) and works (h) over two periods, and then leaves some amount of savings (s) for the future. The worker can freely borrow and save across both periods, which can be thought of as the first and the second halves of the household budget cycle.

At the Job Choice Stage, the worker decides between a gig job G and a non-gig job O, which are identical in all dimensions besides net hourly wages. The gig job has a fixed net wage of w_G, which the worker mistakenly believes to be $\tilde{w}_G = w_G \cdot (1 + \theta)$. The parameter

$\theta \geq 0$ is the degree of net hourly pay overestimation. As previous discussed, θ is around 0.2 in our sample.[15] The non-gig job has a fixed net wage of w_O, which we assume the worker correctly assess. Assume that w_O possibly includes the difference in non-wage amenities between the two jobs.

Denote the job choice by $J \in \{G, O\}$. Given our setup, the worker will choose the job with the highest perceived wage: $J = G$ if $w_G \cdot (1+\theta) \geq w_O$ and $J = O$ otherwise. Let $\tilde{w}$ be the *perceived* wage, where $\tilde{w} = \max\{w_G \cdot (1+\theta), w_O\}$, and let w be the *actual* hourly wage, such that $w = 1\{J = G\} \cdot w_G + 1\{J = O\} \cdot w_O$.

After choosing a job, in Period 1 the worker maximizes his perceived value function by choosing c_1 and h_1, while making plans for c_2, h_2 and s:

$$\max_{c_1,c_2,h_1,h_2,s} u(c_1) + u(c_2 - \bar{c}) + V(s) - c(h_1) - \delta c(h_2) \tag{1.1}$$

such that

$$c_1 + c_2 + s = \tilde{w} \cdot h_1 + \tilde{w} \cdot h_2 + M \tag{1.2}$$

Where $u(c_1)$ is a concave period 1 consumption utility function and $u(c_2 - \bar{c})$ is a Stone-Geary utility function defined over period 2 consumption. This implies a subsistence condition of $c_2 \geq \bar{c}$, where $\bar{c}$ is a minimum level of consumption needed to pay household bills in period 2. In addition, $c(h)$ is the convex cost of effort function, s is the leftover (potentially negative) savings to be used after period 2, and $V(s)$ is the continuation value of s, with $dV(s)/ds > 0$. Furthermore, M is non-labor income. For simplicity, we assume no time discounting and zero interest rates between periods 1 and 2. Represent Period 1 choices by $\tilde{c}_1^*, \tilde{h}_1^*$ and Period 1 plans by $\tilde{c}_2^p, \tilde{h}_2^p$ and s^p.

At the start of Period 2, the worker learns the *actual* amount of money leftover after Period 1 ($w \cdot \tilde{h}_1^* - \tilde{c}_1^* + M$), which is potentially negative. When $\theta > 0$ and $J = G$, the worker will be negatively surprised by this information. For simplicity and to match evidence from previous sections, we assume there is no learning of overestimation θ from this fact. After this, still in Period 2, the worker has an opportunity to revise their plans for c_2 and h_2. In particular, they maximize the perceived value function by choosing c_2, h_2 and making plans for s:

$$\max_{c_2,h_2,s} = u(c_2 - \bar{c}) + V(s) - c(h_2) \tag{1.3}$$

such that

$$c_2 + s = (M + \tilde{w} \cdot \tilde{h}_1^* - \tilde{c}_1^*) + \tilde{w} \cdot h_2 \tag{1.4}$$

Represent choices by $\tilde{c}_2^*$ and $\tilde{h}_2^*$. Finally, at the end of Period 2, the worker learns the *actual* amount of money saved (or borrowed) after Period 2, $\tilde{s}$:

$$\tilde{s} = w \cdot \tilde{h}_1^* + w \cdot \tilde{h}_2^* + M - \tilde{c}_1^* - \tilde{c}_2^* \tag{1.5}$$

This is accompanied by the continuation value $V(s)$, which is a reduced form way of considering how increased savings or borrowing affect the worker's future, which we don't explicitly

[15] We make the simplifying assumption that the worker's only bias lies in misunderstanding the net hourly pay in a gig job, where before we also shown evidence for the overestimation of work hours.

model. Variation in the shape of this function across workers can reflect, for instance, borrowing constraints.

Overestimating net pay ($\theta > 0$) can lead to three categories of mistakes for a gig worker:[16]

1. *Incorrect choice of employer.* A gig worker will not choose a higher-paying outside job when overestimation θ is enough to move the perceive gig wage $\tilde{w}_G$ from below to above the outside job wage w_O. This happens when $w_G \cdot (1+\theta) > w_O > w_G$.
2. *Under-smoothing of labor supply.* A gig worker with $\theta > 0$ believes their period 1 labor income is higher than it actually is by $\theta \cdot w_G \cdot \tilde{h}_1^*$. Realizing this fact at the start of Period 2 is equivalent to an unexpected negative wealth shock. This unexpectedly increases $u'(c_2 - \bar{c})$, the marginal utility of consumption in period 2, causing the gig worker to re-optimize by working more than originally planned ($\tilde{h}_2^* - \tilde{h}_2^p > 0$). The gig worker then inefficiently works too much in Period 2 and too little in Period 1.
3. *Incorrect choice of hours.*
   a) *Works too much.* If gig income is not essential to fulfill the household budget (M is large enough relative to $\bar{c}$), the relevant margin in deciding labor supply is the marginal benefit of consumption. Biased gig workers then work more hours than optimal.

      If $c_2 \geq \bar{c}$ (which is guaranteed for large M), gig workers decide labor supply in Period 1[17] by equating

$$c'(h_1) = c'(h_2) = \tilde{w}_G \cdot u'(c_1) = \tilde{w}_G \cdot u'(c_2 - \bar{c}) = \tilde{w}_G \cdot V'(s) \tag{1.6}$$

      As $\tilde{w}_G > w_G$ when $\theta > 0$, a biased gig worker believes the benefits of additional consumption and savings of working one additional hour are higher than they actually are. Due to the assumed convexity of $c(\cdot)$, this implies they will work too many hours relative to when $\theta = 0$.

   b) *Works too little.* If gig income is essential to fulfill the household budget (M is not large relative to $\bar{c}$ and $\bar{c}$ is sufficiently high), the relevant margin in deciding labor supply is consuming enough in Period 2 to reach the subsistence level $\bar{c}$. Biased gig workers then work less than needed to optimally satisfy the subsistence restriction. To isolate this mechanism, consider a version of our model with only Period 2 and $s = 0$. In this case, the first-order condition implies a choice of hours equal to h. However, if $\tilde{w}_G \cdot h + M < \bar{c}$, the subsistence condition is not satisfied and the optimal labor supply choice will be such that $\tilde{w}_G \cdot \tilde{h}^* + M = \bar{c}$. In this case, $\tilde{h}^*$ is decreasing on the overestimation parameter θ, such that a more biased agent works fewer hours.[18]

[16] As previously mentioned, we do not incorporate errors in understanding labor supply into our model. Two of our predictions below, incorrect job choice and wrong labor supply allocation across the pay cycle, are exacerbated if we consider this additional bias.

[17] The same logic holds for Period 2 decisions.

[18] The same logic holds for our full model for sufficiently high values of $\bar{c}$, despite countervailing adjustments in c_1 and s when the subsistence restriction does not hold.

In Figure 1.6, we solve our model numerically and further illustrate the consequences of overestimation. We do comparative statics by varying the overestimation parameter θ. Details of our parametrization and calibration are provided in the notes of this figure.

We show in Panel (A) the number of hours worked in period 1 and period 2 for different values of θ. In this graph, our model is parametrized so that the gig job is not chosen if the bias θ is low enough. As a result, workers with low θ accurately predict their pay and work the same number of hours in both periods. However, at higher values of θ, the worker chooses the gig job, committing an error in job choice. This results in a larger labor supply for period 2 than for period 1. The difference in work hours between both periods is also increasing on θ. Thus, this illustrates our second mechanism, the inefficient under-smoothing of labor supply in the presence of overestimation.

In Panel (B), we plot the difference between total labor supply of a biased and a rational ($\theta = 0$) worker as overestimation θ increases. We plot two separate lines, one for a low value of non-labor income M and another for a high value of M. For this graph, we assume the outside job is considered an inferior choice for all values of $\theta \geq 0$. As discussed above, we find that a behavioral gig worker works less than the optimal amount when non-gig labor income M is low (relative to $\bar{c}$). In contrast, if M is sufficiently high, the behavioral gig worker supplies too much labor relative to the rational benchmark.

Empirical Evidence

We begin by testing our first prediction: biased gig workers will sometimes not choose a superior outside option. Our measure of the reservation wage is equal to workers' self-reported lowest acceptable net hourly pay to keep working in their current gig job. Figure 1.7 shows scatter plots relating the reservation wage to the net hourly pay, for either its recall (left panel) or its actual expected value (right panel). We find that, as we move from the recall to the actual expected net hourly pay, many gig workers are moved under the 45 degree line. In other words, their net pay falls below their reservation wage.

This impression is confirmed in Table 1.6. First, we find that 23 percent of workers have a higher reservation wage than recall of net hourly pay. This relatively high percentage may reflect a mix of noise in beliefs and actual job outcomes, confusion over the outside option, and perhaps real plans to stop working for the gig company in the future. When comparing the reservation wage with the *actual* expected net hourly pay, however, this percentage increases substantially, to 68%.

In other words, 45 percent of gig workers move from being above to being below their reservation wage when their actual net hourly pay is considered, versus their belief of it. In our model, gig workers for whom this is true are characterized as making a mistake in job choice. We find similar qualitative results by looking at the averages (Appendix Table A.12) or the share of workers \$5 below their stated reservation wage.

Our analysis thus far contains some caveats. To this end, the other rows of Table 1.6 provide robustness checks to our results. First, we may exaggerate the magnitude of gig workers not choosing the best available job if they also overestimate their outside options. To address this, we first consider workers less likely to have another gig job – where mistaken beliefs are more likely to occur – as their outside option. Specifically, we focus on workers who did not work a gig job before or in addition to their main gig company. Next, we assume that the reservation wage is reported with an error equal to half of the error related to understanding the net hourly pay. Our results remain when using these alternate measures.

We then re-run our analysis using a different proxy for the reservation wage. We use the worker's gross hourly wages in either a previous job or in another current job. This measure is elicited in 5-dollar bins, for which we take the midpoint. The magnitude of gig workers earning below this wage is significant, even relative to their beliefs of gig pay. In any case, we still find an increase in the group positioned under this proxy of outside option of approximately 20 percentage points when using the actual expected net hourly pay.

Next, fluctuations in gig pay may temporarily place workers below their reservation wages, even if this is not the case over longer periods of time. We provide a robustness check to this by assuming the actual pay is the highest average net hourly pay over the four previous weeks. We find that our conclusions are similar when we run this analysis: the share of workers predicted to hold a sub-optimal job is 31%.

Finally, we should keep in mind that the outside option for the gig work we consider might be to work for another gig company. Consequently, workers who quit their current gig jobs after being de-biased would not necessarily leave the gig economy. On the other hand, our job choice analysis ignores errors in valuing gig work stemming from the overestimation of labor supply. Due to this channel, gig income will likely be lower than expected, lowering the value of gig work. With these caveats in mind, our findings indicate that a significant share of gig jobs are sustained by mistaken beliefs about pay.

Our model predicts that gig workers will work more hours in the second half of the household budget cycle. In our framework, this happens because workers are surprised by their gig income being less than expected in the first half of the cycle. As they need to pay bills at the end of the cycle, they work more than originally planned. Alternatively, procrastination or a failure to anticipate future bills would also predict this labor supply pattern. Having a convex cost of effort function makes this pattern welfare-reducing.

We now see whether this prediction is borne out in practice. First, we ask workers to report two days each month when they have to pay major bills (Appendix Figure A.9). As we only observe total weekly hours and not their division day-by-day, we make the simplifying assumption that hours are driven uniformly across the week. Importantly, this assumption will weaken the relationship between work hours and the budget cycle, in particular at the end or the beginning of the cycle. Following that, we calculate the number of total hours worked in intervals of seven days, based on the distance from paying a major bill.

Results are shown in Figure 1.8. Gig workers in our sample work, on average, 40 percent (or 3 hours per week) more when a major bill is close to due, compared to when it is at least 3 weeks away. Thus, gig workers work more at the end of the household budget cycle. Note that our analysis is based only on correlations. Thus, we cannot rule out that unobservable characteristics related to which days gig workers pay their bills partially explain our findings. Nevertheless, we find evidence that overestimation of pay in gig jobs is connected to under-smoothing of work hours across the household budget cycle.

As a further test of our model's predictions, we examine the effects of our randomized information treatment.[19] In this intervention, we inform gig workers in the treatment group (during our baseline survey) what their *actual* expected net hourly pay is. Then, we compare this number to their recall of net hourly pay, informing participants if their beliefs are incorrect. Appendix Figure A.12 shows an example of our information treatment. The effects of this intervention should be similar to moving our model's overestimation parameter θ towards zero. Note that we provide gig workers with an informative signal. However, that signal is not fully accurate at the individual level due to noise in measuring actual expenses.

In general, we are under-powered to identify small to moderate effects. This is primarily due to a small sample size, since we need to observe workers at our follow-up surveys to estimate treatment effects on labor market decisions. In addition, we previously documented that gig workers have incomplete learning of their job outcomes, possibly due to biased updating and selective recall. Thus, fundamentally changing workers' beliefs can be difficult, especially if they overestimate their pay. Nevertheless, we find suggestive evidence that we are able to influence beliefs about gig job outcomes, especially expenses, through our information treatment.[20]

We expect our information treatment to affect job choices and labor supply decisions when misperceptions about gig pay exist. However, the effects should be heterogeneous based on the direction of gig workers' mistaken beliefs. In other words, treatment effects should differ depending on whether we tell gig workers they overestimate (bad signal) or underestimate (good signal) their net hourly pay. Accordingly, we estimate treatment effects separately based on whether initial overestimation is positive or negative.

In Table 1.7, we estimate the effect of the information treatment on labor supply and on the probability of holding a job outside the gig company. Effects are measured at the endline survey, distributed between two to five months after the treatment. Labor supply is an average of the weekly hours worked for the gig company for all available weeks after the baseline survey. It includes zeroes for those no longer working for the gig company.

Following the predictions of our model, we estimate effects on labor supply separately by financial need, using information on whether the worker's household is struggling financially.[21] We find that treated workers who initially overestimate their net pay and are more budget constrained slightly increase their workers worked. In contrast, more financially secure workers reduce their average weekly labor supply by around 3 hours per week after our information treatment. These effects are, however, not statistically significant at standard levels.

Next, we find that gig workers in the treatment group initially overestimating their net hourly pay are 5 percentage points more likely to hold another job, which is, again, not statistically significant. In general, we find larger and statistically significant effects – in the opposite direction – among treated workers who initially underestimate their net pay. This is consistent with the mechanisms we discussed in Section 4, which imply that people incorporate positive information more readily than negative information.

19 See Appendix E for more details.

20 We discuss these results in Appendix E.

21 Struggling financially is defined as reporting to be receiving calls from collectors, contemplating bankruptcy, or struggling to pay the bills.

1.6 Conclusion

In this study, we examine whether gig workers correctly understand their job outcomes. First, we collected data on beliefs and on actual job performance. When comparing the two, we find that gig workers consistently overestimate recalls and forecasts of their gross and net weekly pay. We then document that recall errors and forecast errors are strongly correlated. In addition, overestimation of labor supply and underestimation of expenses are key drivers of aggregate mistakes.

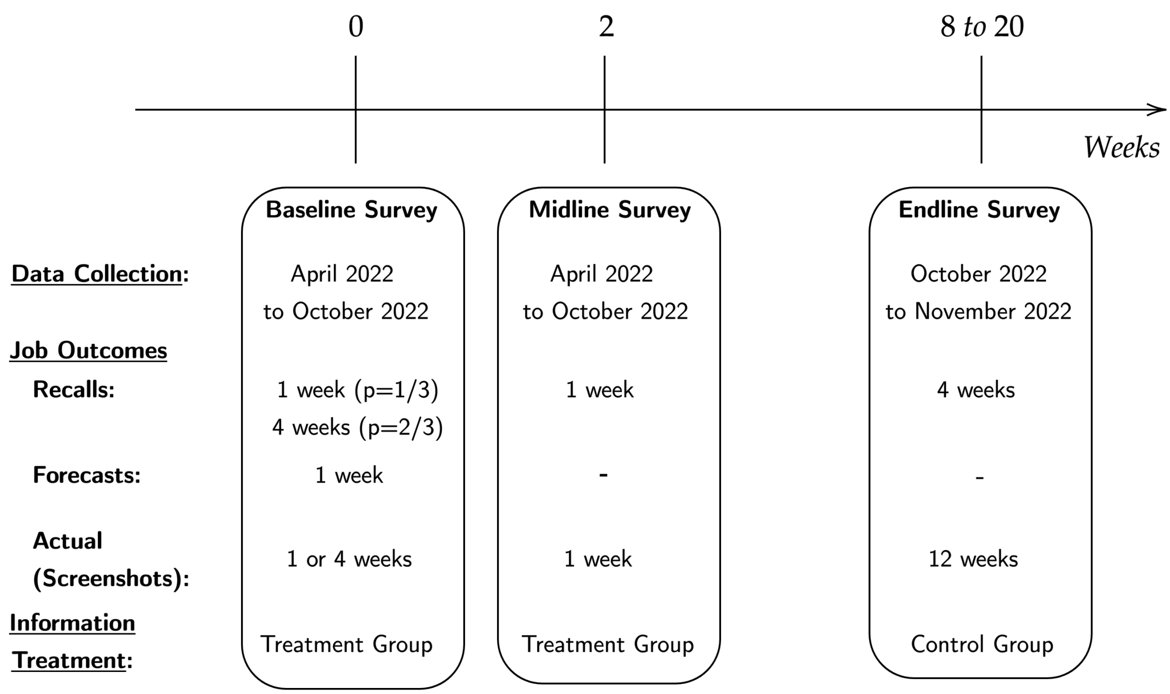
We show that our findings are consistent with motivated beliefs supported by selective recall. According to this theory, errors stem from biased formation and updating of beliefs, do not necessarily improve with experience and increase with the noisiness of job outcomes. We find evidence for these implications in our setting. Then, we develop a simple behavioral labor supply model and derive predictions of the consequences of overestimating pay in gig jobs. The magnitude of the errors we identify is enough to move a significant share of gig workers below their stated reservation wage, indicating potential mistakes in job choices. We also document that gig labor supply choices and allocation across the budget cycle are affected by overestimation of gig job outcomes, leading to sub-optimal decisions.

Our findings imply that the economic modeling of flexible jobs needs to incorporate behavioral biases in understanding pay and hours. Otherwise, welfare calculations and predictions may be inaccurate. Moreover, our work motivates policies that provide summarized job performance information to workers in flexible jobs. By doing so, mistakes in understanding job outcomes can be reduced. However, we see challenges in doing this, and we believe that changing worker beliefs may be difficult. As technological advances allow for greater flexibility across a wider range of jobs, these issues become more relevant.

Our study can be interpreted as saying that flexible gig jobs are less valuable than previously thought. In spite of this, we believe these jobs may still provide significant surpluses, especially to individuals with strong preferences for flexibility. A quantitative welfare analysis of the consequences of the mistakes we identify here is left for future research. The same is true for a study of how mistaken beliefs affect firms' decisions and their market power.

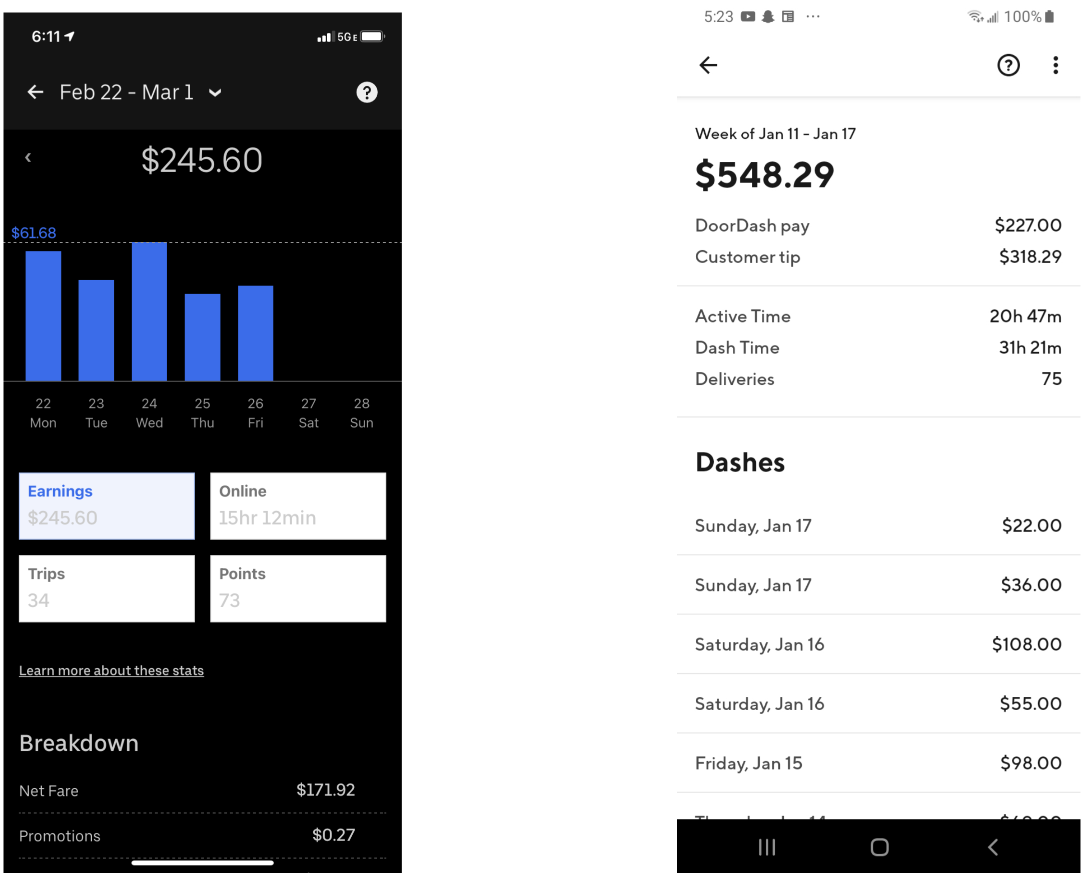
Figures

Figure 1.1: Survey timeline



Notes: We survey gig workers in three occasions: the baseline, midline and endline surveys. For 2/3 of the sample, we elicit a 1 month recall in the baseline survey, while for another 1/3 we ask about the previous week. Beliefs refer to full weeks, from Monday to Sunday. We elicit a one week recall in the midline survey and a 4 weeks recall in the endline survey. In the baseline survey, we obtain information on 1 or 4 weeks of actual job outcomes based on screenshots from the gig platform app. In the midline survey, we obtain information on 1 week of actual job outcomes based on screenshots. In the endline survey, we obtain information on up to 12 weeks of actual job outcomes based on screenshots. The information treatment group receives information, at the both the baseline and the midline surveys, about their actual net hourly pay and sees an example of how to calculate expenses. The control group is presented this information at the end of the endline survey.

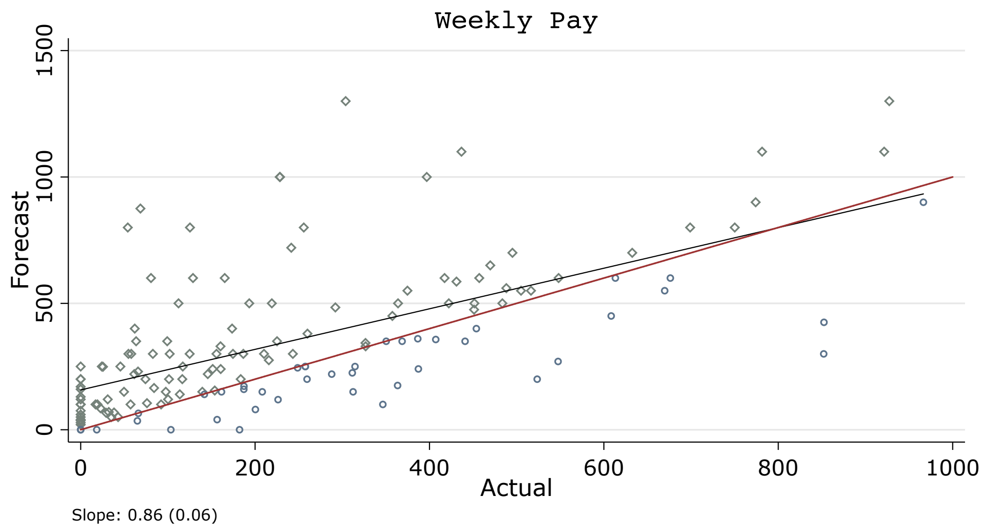
Figure 1.2: Examples of valid screenshots from gig platform apps



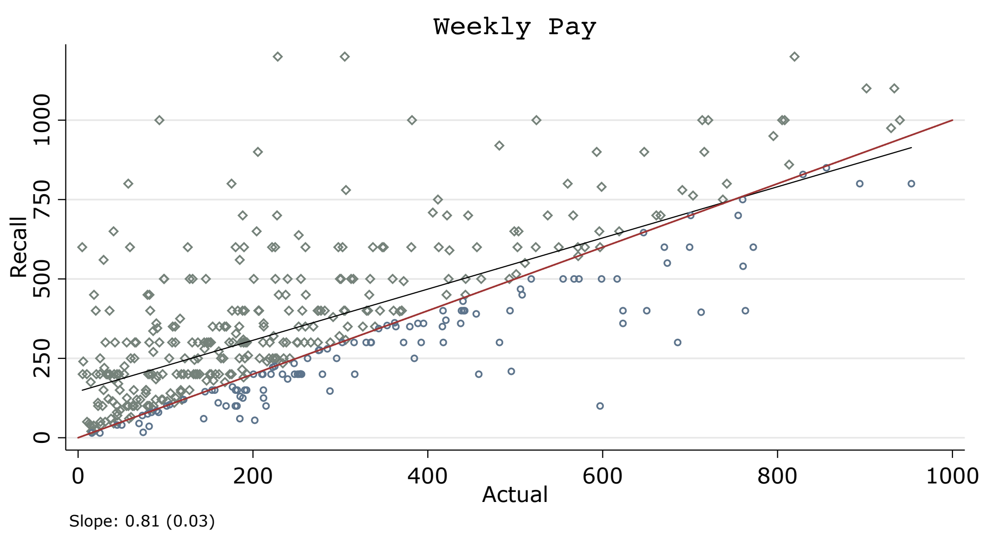
(a) Uber/Uber Eats (b) Doordash

Notes: We show examples of valid screenshots from the weekly earnings page in the gig platform app. Panel (A) is an example of an Uber or Uber Eats screenshot, while Panel (B) is an example of a DoorDash screenshot. In each screenshot, among other things, we have information on the gross weekly pay on top (including tips, bonuses and platform fees), one or two measures of work hours and information on the week that the screenshot refers to.

Figure 1.3: Scatter plots relating beliefs to actual gross weekly pay



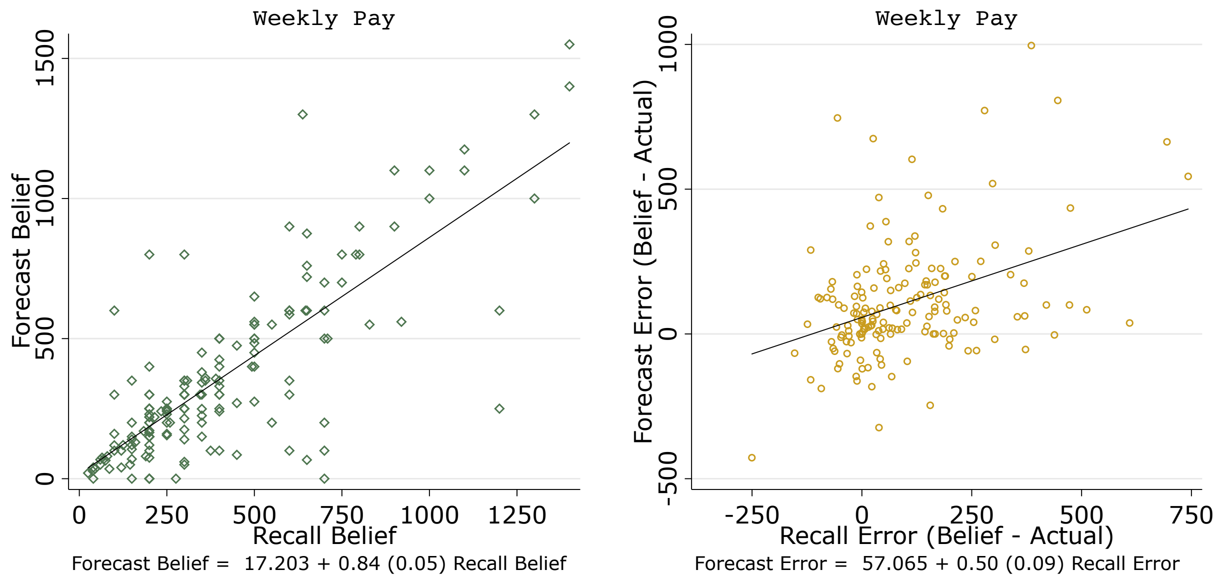
(a) Forecast



(b) Recall

Notes: In each graph, we draw a 45 degree line and the estimated slope of the regression of the belief on the actual weekly pay. Underneath each graph, we present the slope from the regression of the actual outcome versus its recall or forecast belief, with the associated standard error in parentheses. We exclude some outliers from each graph. Points above the 45 degree line indicate overestimation, whereas points below the 45 degree line indicate underestimation of weekly pay. Panel (A) shows forecasts for the first full week (starting on a Monday) after the baseline survey. In Panel (B),we pool recalls of the week and the month before the baseline survey. The actual weekly pay refers to the same time period as this forecast or recall and are obtained using information from screenshots from gig platform apps.

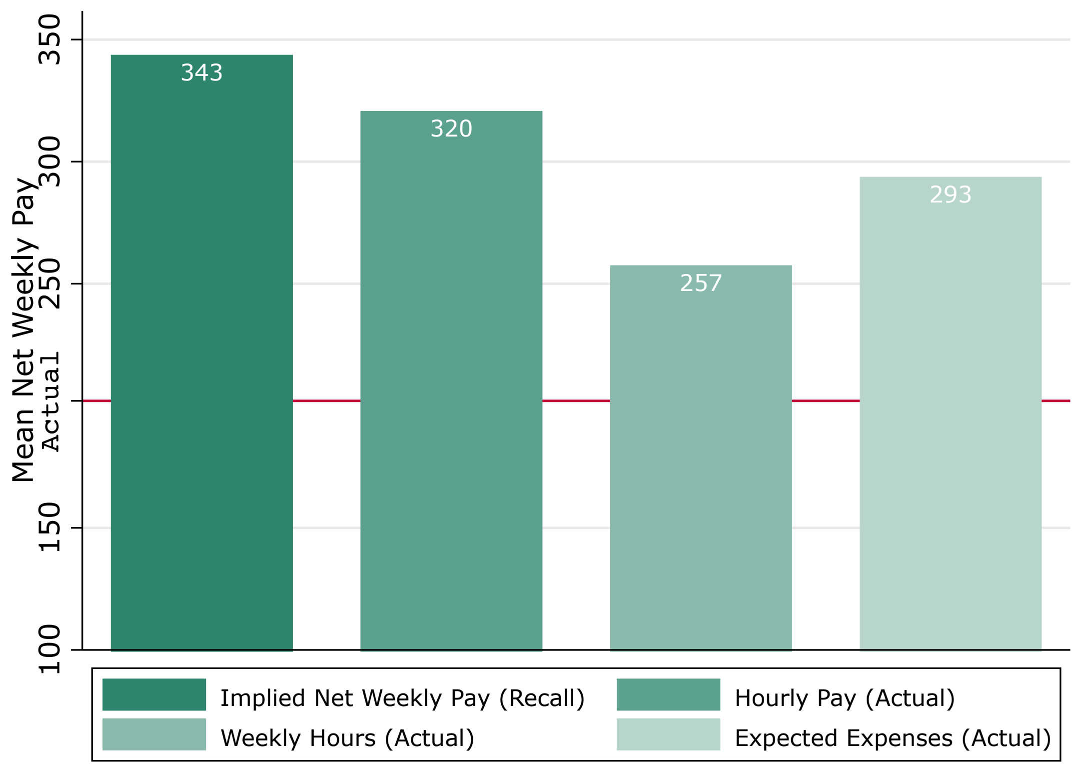
Figure 1.4: Relationship between forecasts and recalls of weekly pay



(a) Beliefs **(b)** Errors (Belief - Actual)

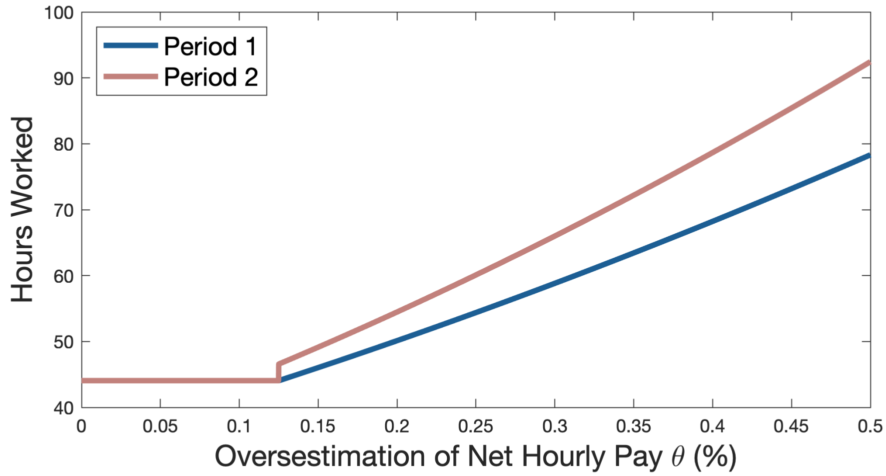
Notes: In Panel (A), we draw the fitted line of the regression relating the recall belief and the forecast belief of weekly pay. In Panel (B), we show the fitted line of the regression relating the forecast overestimation and the recall overestimation of weekly pay. The estimated equation is shown underneath each plot. Overestimation is measured as the recall or forecast minus the actual job outcome. We exclude some outliers from each plot. We pool recalls of the week and the month before the baseline survey. The forecast refers to the first full week (starting on a Monday) following the baseline survey.

Figure 1.5: Decomposing factors that explain errors in recalling net weekly pay

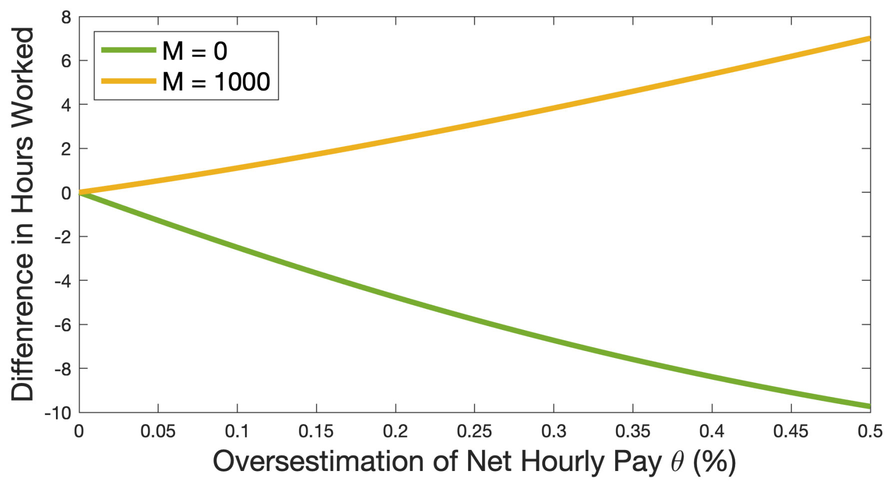


Notes: We plot the average of different measures of net weekly pay. The *Implied Net Weekly Pay (Recall)* is the product of the recall of three variables: gross hourly pay, weekly hours and one minus the expenses share. We pool recalls of the week and the month before the baseline survey. The next three variables replace one element of the implied net weekly pay recall with its correct equivalent. *Hourly Pay (Actual)* is found by replacing the recall for the actual gross hourly pay, while keeping the recalls for weekly hours and expenses. *Weekly Hours (Actual)* and *Expected Expenses (Actual)* are constructed in an analogous manner. Actual job outcomes refer to the same time period as the recalls and are obtained using both information from screenshots from gig platform apps and a calculation of expected expenses by car category and car age group. We plot the average actual expected net weekly pay as a red line on the y-axis.

Figure 1.6: Solving behavioral labor supply model while varying overestimation parameter θ



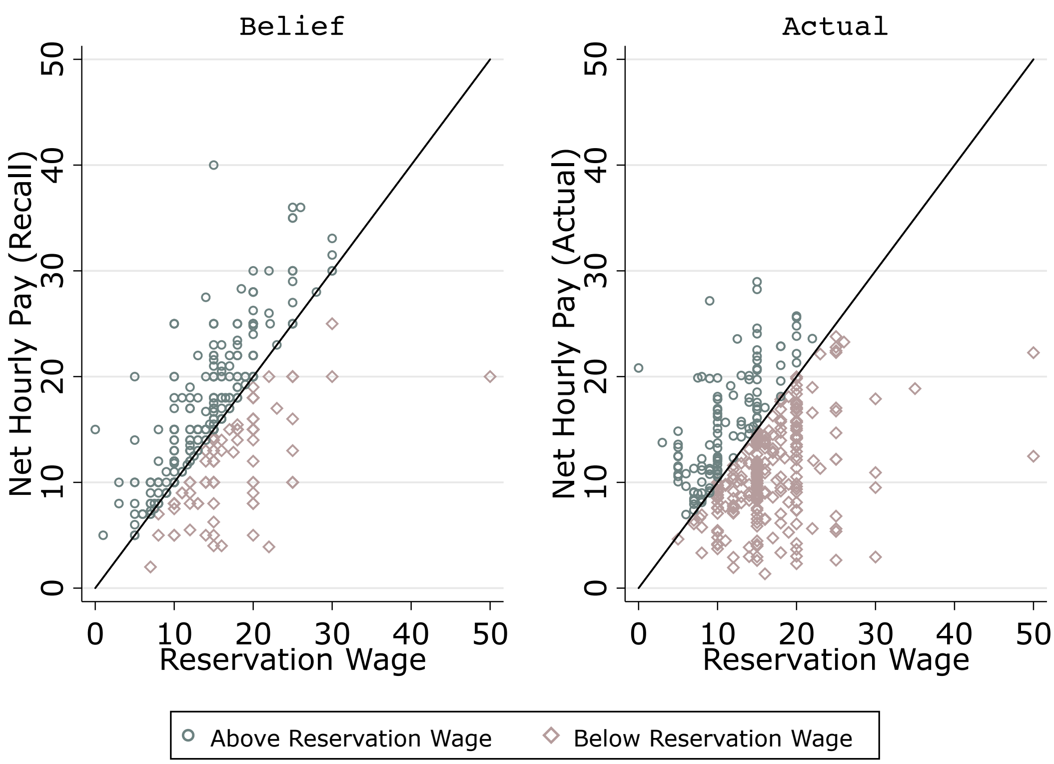
(a) Under-smoothing of labor supply and job choice



(b) Errors in total labor supply

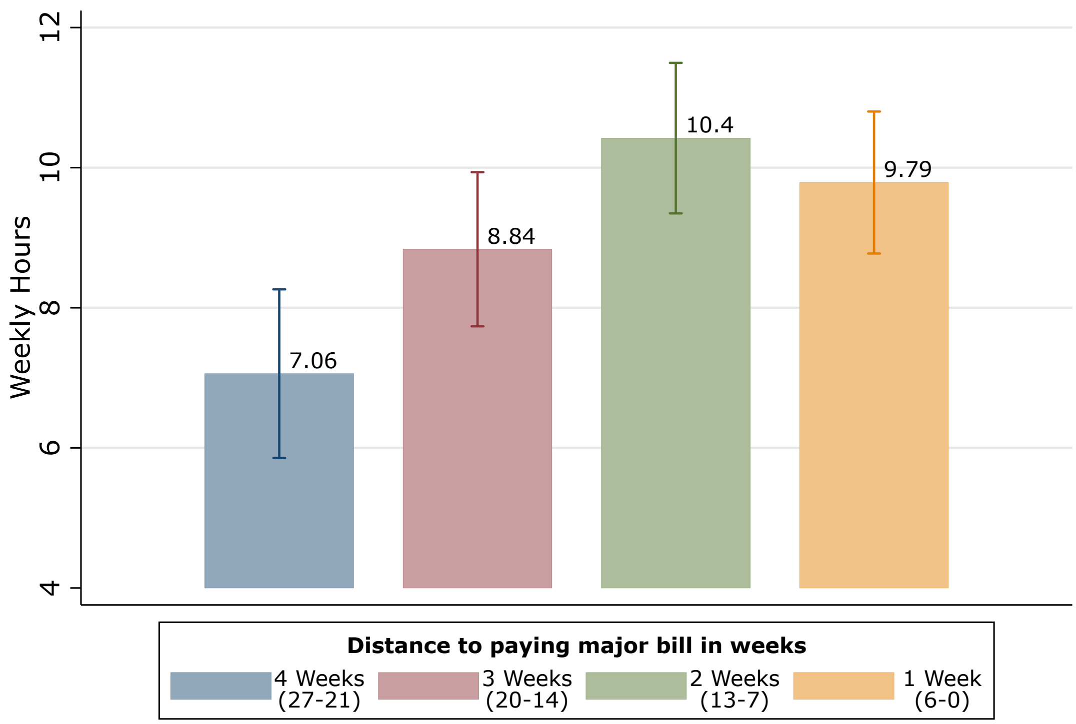
Notes: We numerically solve the model described in Section 5.1 for different values of the overestimation parameter θ, holding all other parameters fixed. In Panel (A), we plot the labor supply in each period as a function of θ. The worker in Panel (A) chooses the non-gig job for $\theta < 0.125$. In Panel (B), we plot the difference in total labor supply across both periods between a worker with the θ shown in the x-axis and one with $\theta = 0$. We plot two separate lines, one for non-labor income $M = 1000$ and one for $M = 0$. We assume that the consumption utility function is $u(c) = 0.85 \cdot c^{0.8}$, the cost of effort function is $c(h) = h^{1.2}$, the continuation value of savings function is $V(s) = 0.02 \cdot s$ and the gig hourly net pay is $w_G = 12$. In Panel (A), we assume that the net hourly pay at the outside job is $w_O = 13.5$, that the shift parameter in the Stone-Gehry utility function is equal to $\bar{c} = 0$, and that $M = 0$. In Panel (B), we assume that $w_O = 0$ and $\bar{c} = 800$.

Figure 1.7: Relationship of belief and actual net hourly pay with reservation wages



Notes: In each graph, we relate a measure of net hourly pay with workers' stated reservation wage. The reservation wage is the answer to the question: "What is the lowest hourly pay after taxes and expenses that would accept and keep working for [gig company]?" Each chart includes a 45 degree line. Points above this line represent workers for whom the net hourly pay measure is above the reservation wage, with the opposite being true for points below this line. Plotted on the y-axis on the left chart is a recall of net hourly pay, which pools the week and the month before the baseline survey. Plotted on the y-axis on the right chart is the actual expected net hourly pay, referring to the same time period as the recalls and obtained using a calculation of expected expenses and information from screenshots from gig platform apps.

Figure 1.8: Relationship between due date of major bills and labor supply



Notes: We use information on self-reported days of the month where each worker has to pay major bills (Appendix Figure A.9) to measure the average amount of work hours relative to when a bill is due, in 7 day intervals. For each interval, we calculate the total amount of hours worked. In this calculation, we assume work hours are spread evenly across the week. We use the minimum of the distance to a bill if a worker reports two dates for paying major bills. Actual work hours data is derived from screenshots from the gig platform app and includes information of job performance for the 4 previous weeks before the baseline survey.

Tables

Table 1.1: Summary statistics

Baseline Survey	**Full Sample**	
	Mean	Std. Dev.
Panel A: Demographics (%)		
Age 18-34	42.07	(49.42)
Age 35-54	47.14	(49.97)
White	72.69	(44.61)
Male	41.19	(49.27)
College Degree	38.55	(48.72)
Panel B: Financial Situation (%)		
Household Income $0-$40k	54.19	(49.88)
No Household Budget	21.59	(41.19)
Struggling Financially	42.73	(49.52)
Gig Pay is Essential	83.48	(37.18)
Panel C: Labor Market (%)		
Has Other Gig Job	35.68	(47.96)
Has Non-Gig Job	17.18	(37.76)
Employed Full-Time Prior to Gig	36.34	(48.15)
Employed Part-Time Prior to Gig	20.26	(40.24)
Unemployed Prior to Gig	10.13	(30.21)
Experience Delivery (12+ mo.)	57.49	(49.49)
Experience Rideshare (12+ mo.)	23.57	(42.49)
Panel D: Actual Outcomes		
Weekly Pay	284.2	(250.4)
Weekly Hours	17.41	(14.81)
Hourly Pay	17.94	(7.515)
Expected Expenses Share	32.19	(5.188)
Panel E: Recall Outcomes		
Weekly Pay	372.8	(265.8)
Weekly Hours	22.69	(13.63)
Hourly Pay	18.85	(6.251)
Panel F: Forecast Outcomes		
Weekly Pay	376.8	(324.9)
Weekly Hours	23.24	(14.05)
Hourly Pay	19.36	(6.798)

Notes: Sample of delivery and ride share gig workers in the United States from our baseline survey ($N = 454$). The mean and standard deviation are shown for each variable. Variables in Panels A-C are measured in percentage units. *Struggling Financially* is defined as receiving calls from collectors, contemplating bankruptcy, or struggling to pay the bills. Panel D shows actual job outcomes, collected from screenshots of the gig economy apps that workers submit. *Expenses Share* is an estimation of expected expenses by car category and car age group. Panel E presents pooled recalls of job outcomes for the previous week and month. Panel F shows information on forecasts about the first week (starting on a Monday) after the baseline survey.

Table 1.2: Forecast and recall overestimation of main job outcomes

	N	Mean of Actual	Overestimation (Belief - Actual) Mean	Mean (%)	Share
Panel A: Forecast					
Weekly Pay	155	\$260	\$113.7***	43.7%	.72***
Weekly Hours	142	13.5	8.4***	62.3%	.8***
Hourly Pay	125	\$20.2	\$.5	2.4%	.54
Panel B: Recall					
Weekly Pay	434	\$284.2	\$88.9***	31.3%	.73***
Net Weekly Pay	408	\$198.8	\$92.1***	46.3%	.74***
Expenses Share	396	32.2p.p.	-7.1p.p***	-22%	.29***
Weekly Hours	392	17.4	5.8***	33.1%	.75***
Hourly Pay	386	\$17.9	\$.7*	3.9%	.53
Net Hourly Pay	338	\$12.4	\$2.6***	20.7%	.68***

Notes: Panel A shows errors in forecasting job outcomes for the first full week (starting on a Monday) after the baseline survey. Panel B shows pooled errors in recalling the week and the month before the baseline survey. We include, for each variable, the number of observations used for calculating overestimation. This number varies across variables due to trimming and a subset of submitted screenshots having incomplete information. *Mean of Actual* is the mean of the actual job outcome. Overestimation is defined as the recall or forecast belief minus the actual job outcome. *Mean* is the mean overestimation (including negative values) for each outcome. *Mean (%)* overestimation is the ratio of the mean overestimation and the mean actual job outcome in our sample. *Share* is the share of workers for whom overestimation is positive. We test whether the average overestimation is equal to 0 and the share overestimating is equal to 50%. Stars are used to denote the statistical significance of these tests (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). *Expenses Share* recall is defined as the ratio of recalls for weekly costs and weekly pay. Actual *Expenses Share*, *Net Weekly Pay* and *Net Weekly Pay* use an estimation of expected expenses by car category and car age group.

Table 1.3: Correlation of gig work experience with overestimation of job outcomes

	Overestimation (Belief - Actual)	
	(1)	(2)
	Weekly Pay	Net Weekly Pay
Inexperienced (Less than 6 Months)	40.2*	64.3***
	(22.0)	(22.2)
Some Experience (Between 6 and 12 Months)	87.2***	93.6***
	(8.57)	(8.90)
Experienced (Over 12 Months)	132.0***	104.7***
	(18.7)	(18.4)
Observations	439	400
p-value(Some Experience = Inexperienced)	0.047	0.22
p-value(Experienced = Inexperienced)	0.0016	0.16

Notes: We regress the overestimation of weekly pay on binary variables for experience in gig work. The regressions do not have a constant term. Overestimation of each outcome is defined as the recall belief minus the actual job outcome for the same time period. We pool recalls of the week and the month before the baseline survey. *Inexperienced* is equal to 1 if a gig worker has less than six months of experience in both delivery and ride share. *Experienced* is equal to 1 if a gig worker has more than one year of experience in both delivery and ride share. *Some Experience* is equal to 1 if both *Inexperienced* and *Experienced* are equal to 0. Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Under each regression,we present the p-value for a test of whether (i) the coefficients on *Experienced* and *Inexperienced* are equal, and (ii) the coefficients on *Some Experience* and *Inexperienced* are equal.

Table 1.4: Predictions from motivated beliefs theory: selective recall and updating

	Recall Belief Weekly Pay			
	(1)	(2)	(3)	(4)
Last 4 Weeks (Actual)				
Maximum	0.57***	0.55***	0.24**	0.22*
	(0.058)	(0.059)	(0.12)	(0.12)
Minimum	0.26***	0.26***		
	(0.086)	(0.087)		
Mean			0.60***	0.59***
			(0.14)	(0.14)
Observations	320	320	320	320
Demographic Controls		✓		✓
p-value(Max=Min)	0.026	0.039		

(a) Selective Recall

	Belief$_t$ Weekly Pay	
	(1)	(2)
Belief$_{t-1}$	0.69***	0.67***
	(0.049)	(0.053)
1{Actual$_t$ $\geq$ Belief$_{t-1}$}	208.3***	166.5**
	(33.6)	(69.6)
1{Actual$_t$ $<$ Belief$_{t-1}$}	-47.5*	-81.8
	(25.9)	(68.6)
Observations	155	155
Demographic Controls		✓
p-value(Above=Below)	0.0016	0.53

(b) Belief Updating

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year. Outcome variables in Panel (A) are recalls referring to the month before the baseline survey. Panel (A) shows regressions of recalls of weekly pay on functions of the 4 previous weeks of actual weekly pay. We define the minimum, the maximum and the mean for this set of four weeks. We present the p-value for a test of whether the coefficients for the maximum and the minimum variables are the same. In Panel (B), only participants that replied to our midline survey are included. $Belief_t$ is the recall of weekly pay of the first full week (starting on a Monday) after the baseline survey. $Belief_{t-1}$ is the pooled recall for each job outcome for the week or the month before the baseline survey. $Actual_t$ refers to the actual job outcome (obtained from a screenshot submitted from the gig platform app) of the first full week after the baseline survey. Each column shows the result of the regression of $Belief_t$ on $Belief_{t-1}$ and two binary variables: a variable equal to 1 if $Actual_t$ is above or equal to $Belief_{t-1}$, and a variable equal to 1 if $Actual_t$ is below $Belief_{t-1}$. There is no constant term in the regressions in Panel (B). We present the p-value for a test of whether the coefficients for the two indicator variables are the same.

Table 1.5: Predictions from motivated beliefs theory: variance of outcomes

	Overestimation (Belief - Actual) Weekly Pay	
	(1)	(2)
Last 4 Weeks (Actual)		
Coefficient of Variation (CV)	0.85***	0.94***
	(0.30)	(0.31)
Constant	62.3***	60.3
	(17.4)	(45.7)
Observations	232	232
Demographic Controls		✓
Average CV (SD/Mean)	0.48	0.48

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Overestimation of a job outcome is defined as the recall belief minus the actual job outcomes for the same time period. We allow for negative values of this variable. We show regressions of the overestimation of weekly pay on the coefficient of variation for the 4 previous weeks of weekly pay. The coefficient of variation is the standard deviation over the mean. We normalize this variable so that a 1 unit increase is equal to an increase of 1 percentage point. We present the average coefficient of variation underneath each column. Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year.

Table 1.6: Relationship of net hourly pay with reservation wage

	Net Hourly Pay		
	Belief	Actual	Difference (SE)
	(1)	(2)	(2) - (1)
Panel A: Share with Pay below Reservation Wage			
Full Sample	23%	68%	45% (2.6)
Alternative Calculations:			
Outside Option Not Gig Work	22%	57%	35% (4.6)
Overestimated Outside Option	23%	55%	32% (2.5)
Maximum Actual Net Hourly Pay	23%	54%	31% (2.4)
Wage at Other or Previous Jobs	61%	78%	17% (3.3)
Panel B: Share with Pay $5 or More below Reservation Wage			
Full Sample	6%	29%	22% (2.2)
Alternative Calculations:			
Outside Option Not Gig Work	4%	22%	18% (3.6)
Overestimated Reservation Wage	6%	21%	15% (1.9)
Maximum Actual Net Hourly Pay	6%	22%	16% (1.9)
Wage at Other or Previous Jobs	32%	51%	20% (3.6)

Notes: Belief of net hourly pay is the pooled net hourly pay recall of the week and the month before the baseline survey. The actual net hourly pay refers to the same period. Our reservation wage proxy is the answer to the following question: "What is the lowest acceptable hourly pay after taxes and expenses that would accept to keep working for [gig company]?". In the two panels, we calculate the share of workers for whom the reservation wage proxy is above the net hourly pay by either 0 or 5 dollars. The final column shows the difference between these shares when the belief and actual net hourly pay are used. The first row includes our full sample and is our base measure. For *Outside Option Not Gig Work*, we include only workers that did not do gigs before their current gig job and that do not work for other gig companies. For *Overestimated Reservation Wage*, we assume the reservation wage is measured with half as much error as the net hourly pay. For *Wage at Other or Previous Jobs*, we use the worker's gross hourly wages in either a previous job or in another current job as the reservation wage proxy. This measure is elicited in 5-dollar bins, for which we take the midpoint. For *Maximum Actual Net Hourly Pay*, we assume the actual net hourly pay is the maximum weekly average of the net hourly pay in the past 4 weeks.

Table 1.7: Effects of randomized information treatment on labor market decisions

	Other Jobs		**Weekly Hours**	
	(1)	(2)	(3)	(4)
Good Signal	-0.28**	-0.28**		
	(0.11)	(0.12)		
Bad Signal	0.037	0.055		
	(0.083)	(0.082)		
Good Signal × Less Financial Need			2.77	4.23**
			(2.87)	(2.14)
Bad Signal × Less Financial Need			-0.59	-2.99
			(2.05)	(2.02)
Good Signal × More Financial Need			1.32	-1.16
			(7.71)	(4.76)
Bad Signal × More Financial Need			0.53	1.63
			(2.95)	(3.54)
Observations	168	168	162	153
Baseline Outcome		✓		✓
Demographic Controls		✓		✓
p-value(Treatment No Effect)	0.041	0.045	0.90	0.16

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). In models (1) and (2), we estimate versions of $y_{it} = \beta_0 + \beta_1 Over_i + \beta_2 Bad\,Signal_i + \beta_3 Good\,Signal_i + X_{i0}\Gamma + \varepsilon_{it}$, where y_{it} is whether individual i at the endline survey reports having a job other than one for the main gig company. $Over_i = 1$ if initial overestimation of net hourly pay is positive and 0 otherwise. Our two variables of interest here are $Bad\,Signal_i$ and $Good\,Signal_i$: $Bad\ Signal_i$ is equal to 1 if an individual is in the treatment group and $Over_i = 1$; $Good\ Signal_i$ if an individual is in the treated group and $Over_i = 0$. Individuals in the treatment group were told whether they misestimated their actual net hourly pay (see Appendix E for more details). We test whether all treatment variables are jointly significant and provide a p-value for this test for each model. X_{i0} is the covariates matrix. Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year, in addition to the outcome variable in the baseline survey. In models (3) and (4) we replace $Over_i$, $Bad\ Signal_i$ and $Good\,Signal_i$ by their interaction with two dummies: $Less\,Financial\,Need$ and $More\,Financial\,Need$. *More Financial Need* (*Less Financial Need*) is equal to 1 if the gig worker is in a household that is (not) struggling financially and 0 otherwise. *Struggling Financially* is defined as receiving calls from collectors, contemplating bankruptcy, or struggling to pay the bills. In addition, we add a binary variable of $More\ Financial\ Need$ to our model. The dependent variable for models (3) and (4) is a simple average of the weekly hours worked for the main gig company for all available weeks prior to the endline but after the baseline survey (including zeroes).

Chapter 2

Can Laws Change Social Norms? The Formalization of Domestic Workers in Brazil

2.1 Introduction

Employers in developing countries often fail to comply with labor laws. For example, Latin America's labor market informality rate approaches 50 percent (ILO, 2022). Many factors contribute to this, including insufficient enforcement of regulations and high labor costs. Brazil has tried various approaches to boost formalization within its labor market, such as reducing business taxes and improving enforcement. One possibility has remained largely untapped as public policy: altering social norms around employing informal workers.

A change in the ethical perception of employing informal workers could lead to greater compliance with labor laws and a reduction in informality rates. Indeed, social norms are known to influence economic behavior in many ways (Young, 2015; Elster, 1989; Legros and Cislaghi, 2020). One way of leveraging this channel involves taking advantage of *the expressive value of the law* to shape social norms and attitudes. As Bursztyn and Jensen (2017) suggest, "the very act of passing a particular law sends a signal to others about a norm or prescribed attitude towards behavior." By enacting laws that emphasize the importance of following labor regulations, governments can potentially influence people's perception of formalization within the labor market. In order to be successful, this approach needs that employees and employers be sufficiently exposed to these altered norms. This is affected by public awareness, media coverage, and communication strategies.

Domestic work (DW) in Brazil involves tasks such as cleaning, cooking, laundry, and child-care. These workers may live with their employers or not. Work contracts can be full-time, part-time, or hourly. The vast majority of domestic workers in Brazil are informal, and they are predominantly Black or mixed-race women from low-income backgrounds. Considering this situation, Proposal for Constitutional Amendment (PEC) 66/12 was proposed with the intention of establishing equality of labor rights between domestic workers and other employees. Note that DW informality was already illegal before this legislation, and the PEC's aim was to extend the labor rights of DWs and to give their existing rights a constitutional basis.

Passed in April 2013, Constitutional Amendment (CA) 72/13, or *PEC das domésticas*, introduced a maximum of 8 daily and 44 weekly regular work hours, along with a 50% overtime pay bonus. CA 72 lacked enforcement mechanisms, as government agents in Brazil are barred from conducting home inspections. Thus, enforcement relied on employee-filed complaints. Complementary Law (CL) 150/15, enacted in June 2015, provided the necessary regulatory framework to establish the other labor rights foreseen under CA 72, including nighttime work bonuses, severance packages and unemployment benefits. This led to increased costs for employing formal DWs.

Thus, CA 72 had a minimal impact on formal domestic workers' labor costs between April 2013 and June 2015. During this time, additional expenses were limited to overtime pay regulations, affecting only 20% of DWs and averaging a 1% increase in total costs. In spite of this, domestic workers' pre-existing rights received intense media coverage and public interest around the passing of this legislation. Google Trends data shows a surge in searches related to DWs and their rights when the amendment was enacted (and also when CL 150/15 became law).

We claim this information shock likely raised public awareness of domestic labor legislation and changed social norms around employing informal DWs. It is in this unique period between CA 72 and CL 150 – when the law was in place but the contents were on hold – that we can isolate the social norms component of the legislation. We use a broad definition of a change in social norms. It includes the social or self-image cost of employing informal DWs and informal employee empowerment to demand better job amenities and pay. In addition, it may also be employers' perceived increased risk of fines for non-compliance.[1] Indeed, we find suggestive evidence of increased labor claims initiated by domestic workers against employers due to this legislation.

Worker formality is defined in two ways: employee registration with the government and social security contributions. We assume compliance with labor regulations depend on both formality and informality costs. Formality costs include taxes, mandated benefits, and regulations, while informality costs involve expected punishment for non-compliance, image costs, and reciprocity costs. As CA 72 and CL 150 cause higher costs for both formal and informal domestic labor, we predict a decrease in DW employment. If changing social norms are a crucial channel, formality among domestic workers may *increase* in the period after CA 72. This is despite an increase in taxes and regulations for formalized workers. In line with this, DWs' weekly hours can decrease due to a reduction in labor demand and overtime pay regulations. There may also be a rise in average wages due to formalization, gains in bargaining power and compositional effects.

We analyze the labor market consequences of DW legislation – and its effects on social norms – using the PNAD Continua dataset. Our study utilizes quarterly individual-level data from Brazil from 2012 to 2016. A simple time series analysis shows a shift towards

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[1] When this is not due to changes in government enforcement, which we believe is unchanging across this period.

formal employment in the DW sector during this period. This is coupled with decreases in informal, full-time and total number of DWs. This response to the legislation is consistent with an effect on social norms, and it shows that some share of DWs were likely hurt by CA 72 and CL 150.

We further examine the impact of CA 72/13 and CL 150/15 on domestic workers by using an event study framework with a control group. To ensure robustness, we consider four control groups based on characteristics such as race, wages, years of schooling, and gender. Furthermore, we explicitly construct these control groups to satisfy the Stable Unit Treatment Value Assumption (SUTVA).

We do not find evidence of pre-trends. As a result of CA 72, domestic workers' formality rates increased by 3 to 6 percentage points. A decrease in weekly hours and an increase in monthly wages are also identified. In addition, about 5 percentage points more domestic workers are paid the minimum wage after CA 72. These findings are robust across a variety of specifications, covariate sets, and control groups. Following CL 150/15, we observe stronger effects, which can be explained by social norm-related increases in informality costs.

In order to understand the mechanisms behind our findings, we examine what determines the social norms of informality for employers. We claim this is a function of cumulative exposure to labor legislation, employer-employee relationship closeness, and the general negative perception of informality. We claim Constitutional Amendment 72 and Complementary Law 150 intensified the negative ethical dimension of skirting labor regulations, leading to a higher formality rate for DWs. We then study how variation in some of these factors relates to the treatment effects of the legislation.

We find that DWs that live with their employers experience much larger formalization effects from CA 72 and CL 150. This suggests that the social norms mechanism, affected by the closeness of the employer-employee relationship, is important. Next, we investigate the neighborhood "peer effects" of worker exposure to other domestic and/or formal workers. We show that more DWs in a neighborhood increase the likelihood of DW formalization. In addition, the probability of becoming a formal DW after CA 72 rises by around 1 percentage point for each additional neighborhood domestic worker. We then study the impact of formalization *events*. We find larger treatment effects for DWs in neighborhoods with more workers who become formal. Exposure to formality discussions may thus enhance the social norms impact of CA 72.

Our paper contributes to the literature that employs quasi-experimental techniques to analyze the impact of labor market legislation (e.g., minimum wage, mandated health insurance, and enforcement mechanisms). For general surveys of the empirical literature in developing countries, see Boeri, Helppie, and Macis (2008) and Freeman (2010). Other relevant papers include De Barros and Corseuil (2004), Lemos (2004, 2009), MacIsaac and Rama (1997), Kugler (2004), Mondino and Montoya (2004), Theodoro and Scorzafave (2011), Cervini-Plá, Ramos, and Silva (2014), and Feld (2022). To the extent of our knowledge, ours is one of the first papers to provide quasi-experimental evidence on the expressive power of the law to change behaviors through effects on social norms.

Almeida and Carneiro (2012) examine the effects of labor regulation enforcement in Brazil. Their results show that a 21.2% increase in labor inspections leads to a 1.6% and 1.7%

increase in the proportion of formal and non-employed workers, respectively. Other papers find that disseminating information on labor rights can play an important role. Dinkelman and Ranchhod (2012) analyze the introduction of a minimum wage in South Africa's domestic worker sector, while Gindling, Mossaad, and Trejos (2015) consider a program in Costa Rica designed to increase minimum wage compliance, which included a publicity campaign. Both studies find increases in wages, formality, and compliance with mandated benefits due to information shock effects that give workers increased bargaining power. Barsbai et al. (2023) implemented a successful intervention aimed at reducing mistreatment of domestic workers in the Phillipines by their employers. The intervention involved domestic workers presenting a family photo and small gift to employers, which reduced perceived social distance between employees and employers.

Furthermore, we draw insights from literature that adds an informal sector to standard labor search models and then uses data from developing countries for calibration and simulation of different government policies. Examples include Meghir, Narita, and Robin (2015), Haanwinckel and Soares (2021), Haanwinckel (2018), Bosch and Esteban-Pretel (2012), Ulyssea (2010), and Albrecht, Navarro, and Vroman (2009). Finally, we take inspiration from works on neighbors' peer effects (Bayer et al., 2008; Hellerstein et al., 2011, 2014; Schmutte, 2015), and on social norms and social pressure (Young, 2015; Bursztyn and Jensen, 2017; Elster, 1989; Legros and Cislaghi, 2020).

We begin by discussing the legislation and its expected impact on social norms and labor market outcomes in Section 2. In Section 3, we present the data and summary statistics, providing an overview of the key variables in our analysis. In Section 4, we outline our estimation strategy and explain the process of finding a suitable control group. Section 5 presents the results of our event study analysis, examining the effects of the legislation on domestic workers' labor market outcomes. In Section 6, we delve into the mechanisms behind these effects, using variation in exposure to formalization in neighborhoods and the closeness of employer-employee relationships. Finally, we conclude in Section 7, summarizing our findings and discussing their implications for policy and future research.

2.2 Legislation and context

CA 72/13 and CL 150/15

Brazilian domestic workers (DWs) are employed by private households to handle various tasks, including cleaning, cooking, childcare, elderly care, and security. These workers may live with their employers or not. They are commonly employed by middle and upper class families. Work contracts can be full-time, part-time, or hourly. Brazilian DWs are predominantly Black or mixed-race women. They typically come from low-income backgrounds, which limits their access to education and job opportunities. DWs are prevalent in Brazil due to cultural norms and low-skill labor being affordable. Paid domestic work in Brazil has strong historical ties to the country's legacy of slavery. These factors explain the perception of these jobs as unstable, with poor working conditions and inadequate levels of formality (DIEESE, 2013; Telles, 2014).

PEC 66/12, submitted to the Brazilian Congress in December 2012, aimed to amend the Seventh Article of the Brazilian Constitution to guarantee equal labor rights between domestic workers and other employees. It is important to note that DW informality was already illegal prior to this legislation. Indeed, its purpose was to extend additional labor rights to domestic workers and to give their existing rights a constitutional basis. After several months of discussion in Congress, the PEC was approved by the Senate on March 19, 2013. The Chamber of Representatives enacted the law on April 2, 2013, as Constitutional Amendment 72/13, referred to as the *PEC das domesticas*. Humanitarian motives drove the political discussion of this law. Advocates of the amendment emphasized the need for additional labor rights and mandated benefits to provide better working conditions for domestic workers.

The Constitutional Amendment (CA) 72/13 did not foresee an enforcement mechanism to ensure employer compliance with labor laws. Additionally, Brazilian government agents are prohibited from conducting home inspections, relying on employee complaints to enforce domestic labor regulations. Under standard models for formalization decisions, this would greatly hinder the legislation's effectiveness in increasing formality rates for DWs.

Despite CA 72 establishing new labor rights, the majority of them remained unenforceable until mid-2015 due to the need for additional regulation. The Complementary Law (CL) 150/15 of June 1st, 2015 enacted the necessary legal framework for establishing all labor regulations outlined in CA 72. Table 2.1 shows changes in domestic workers' labor rights before and after Constitutional Amendment 72/13 and Complementary Law 150/15. Several labor rights, including a minimum wage, the thirteenth salary, paid vacation, maternity leave (4 months), prior notice (minimum of 30 days), and social security contributions, were established before the CA 72.

The CA 72 introduced a maximum of 8 daily and 44 weekly regular work hours, along with a 50% overtime pay bonus. Through the passage of CL 150, nighttime work bonuses, severance packages, unemployment benefits, contributions to FGTS[2], assistance for employees' children, and accident insurance on the job became valid.

Table 2.2 provides a numerical example of the labor costs implications of CA 72/13 and CL 150/15. This table outlines the different cost and salary components before and after CA 72/13, as well as after CL 150/15, and calculates the percentage change in total costs from the initial scenario. Because of CA 72/13, some salary components, such as a bonus for overtime hours, increased, resulting in marginally higher labor costs for DWs. The implementation of CL 150/15 led to larger increases in employers' total costs. This is due to new labor rights, such as family wages, severance packages, and FGTS contributions. Our example shows a cost increase of 3.3 % after CA 72/13 and 11.29% after CL 150/15.

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[2] In 1967, the Length-of-Service Guarantee Fund (FGTS) was established to increase government funds and protect workers against large income shocks. Each month, companies deposit an amount corresponding to 8% of each employee's salary into this fund. Employees may use funds from this account in the event of layoffs, health issues, or to purchase real estate.

Potential consequences for social norms

Thus, between April 2013 and June 2015, the impact of Constitutional Amendment (CA) 72 on formal domestic workers' labor costs was relatively minimal. In fact, the effects of this regulation were limited to overtime bonus pay. About 80% of domestic workers did not experience any cost impact, with an average effect of around 1% overall (calculated using hours worked before CA 72).

Thus, the CA 72 was in place but its contents were on hold. This allows us to isolate what we call the social norms component of the legislation by exploring *the expressive value of the law* to shape attitudes. We claim that changing social perceptions about informality can lead to greater compliance with domestic labor legislation and a reduction in informal employment.

Despite the modest changes in labor costs, domestic workers' rights received extensive media coverage and public attention during this period. In Figure 2.1, we present Google Trends data on search statistics related to domestic workers. The six graphics show a significant increase in related searches following the enactment of the amendment (or when CL 150/15 came into effect). This indicates that an information shock raised public awareness of labor legislation in domestic services. This strengthens our case that social norms regarding the employment of informal domestic workers changed.

A closer look at the search terms reveals a broad range of queries reflecting general interest in this topic. The terms we analyze can be divided into four subgroups: (i) labor rights valid starting in April 2013 ("overtime hours domestic worker"); (ii) labor rights valid starting in June 2015 ("FGTS domestic worker"); (iii) the formality of domestic workers ("legal domestic worker", "rights domestic worker" and "law domestic worker"); (iv) general term ("domestic worker").

We take a broad definition of social norms in our study and consider three mechanisms through which it might affect formalization decisions. First, employers who hire informal DWs may incur self-image or social image costs, since this action may be perceived as unethical. As domestic labor commonly involves a close personal relationships between employer and employee, this channel is particularly significant for this occupation. Second, the legislation and the surrounding public discourse may empower employees to ask for better job amenities and pay, changing their perceptions of their "deserved pay".

Third, employers may perceive a greater risk of being taken to labor court and paying fines for non-compliance with regulations. We also characterize this channel as a consequence of social norms. We distinguish this from government enforcement of legislation (which we believe remained more or less constant during this period). In our view, taking an employer to a labor court is a function of subjective expectations of success, which are shaped by social norms. The legislation may thus influence formalization and employment decisions in multiple ways, beyond conventional channels, through social norms.

Panel (A) of Figure 2.2 presents a line graph depicting the share of labor claims filed against employers that refer to the domestic services sector from 2009 to 2018. Legislative events are represented by two vertical red lines in the graph: respectively, CA 72/13 and CL 150/15. The graph shows a steady decline in the share of labor claims related to domestic services from 1.7% in 2009 to 1.2% in 2012, and then a fluctuating rise, reaching 2.2% in 2018. Panel (B) presents this data as *total* lawsuits against employers in this sector. The chart shows a distinct increase between 2012 and 2013 and another rise after 2015. It is difficult to confirm whether this is a direct result of the legislation. It appears, however, that they are associated with more lawsuits against employers of domestic workers.

Expected effects of CA 72 and CL 150

Our study defines worker formality in two ways. First, a job is considered formal if it is registered with the government through the employee's work card. A second definition is whether the employee contributes to social security. Assume the formality share in a sector is determined by two factors: (i) formality costs and (ii) informality costs. Formality costs include taxes, mandated benefits, and additional regulations employers must follow when hiring formal employees. Informality costs involve the expected punishment for non-compliance with labor regulations, plus image and reciprocity costs tied to social norms related to skirting labor regulations.

Table 2.3 compares labor costs for formal and informal domestic workers before and after CA 72 and CL 150. The table is divided into three periods: before CA 72 (before Q1/2013), after CA 72 but before CL 150 (Q2/2013 to Q1/2015), and after CL 150 (Q2/2015 and after). After CA 72 but before CL 150, overtime costs increased for formal domestic workers who worked more than 44 hours per week. As previously shown, this resulted in a marginal increase in labor costs. Total costs for formal domestic workers increase more after CL 150, and overtime costs remain at a higher level for work beyond 44 hours per week. For informal domestic workers, after CA 72, employers face an increase in image costs for employing informal workers. In addition, the expected punishment for non-compliance with regulations and domestic workers' bargaining power rise.

Total employment of domestic workers (DWs) is likely to decrease as both formal and informal labor costs rise. This is because employers will be less inclined to hire or keep domestic workers. During the period before CL 150 is implemented, the formality share among domestic workers should rise due to higher perceived social norm costs associated with informality, along with only small increases in formality costs.

It is common for traditional formalization models to overlook informality costs that are not associated with government inspections and fines. Consequently, they would likely predict a *decrease* in formality rates due to increased costs associated with formal employment and little change in government enforcement. Given that one of our goals is to test the importance of social norms, observing how formality is impacted is crucial.

As taxes and other costs for formal DWs increase more substantially after the CL implementation, the effect on formality rates during this period is uncertain. Employers may also exhibit anticipatory behavior before the implementation of CL 150, reducing their reliance on formal domestic workers before Q2/2015.

We expect a decline in DWs' weekly hours worked. This is due to an expected decrease in labor demand on the intensive margin. A further benefit of cutting hours is that employers avoid overtime regulation, which is now more attractive as overtime pay becomes more expensive.

We anticipate an increase in average wages for DWs. Formalized workers are more likely to receive at least the (likely higher) minimum wage. In addition, domestic workers' bargaining power may also change. Changing social norms regarding formalization could give these workers an advantage in wage negotiations, driving up average salaries. Unintended consequences of the legislation can also result in a higher average wage. Domestic workers with lower skill levels may face a higher risk of being fired or not hired at all as labor costs rise. This would skew the occupation towards higher-paid, more skilled individuals. On the other hand, a decrease in the number of formal DWs may also occur after CL 150. The combination of this reduction and, potentially, compensating differentials could result in a decline in average wages.

2.3 Data

In order to study the consequences of DW regulations and the impact they have on social norms, we primarily use the PNAD Continua. Our dataset contains quarterly individual data from Brazil between 2012 and 2016. This dataset is organized as a rotating panel, with households being interviewed for five consecutive quarters. Individual identifiers are not included in the data. Nevertheless, about 60% of the sample can be identified across periods using variables like date of birth, household identifiers, and other personal characteristics.

These shortcomings lead us to mainly use this data as repeated cross-sections. We only include individuals over the age of 15, which is the working-age population. Part-time domestic workers, or *diaristas*, are excluded from the analysis. As these workers lack legal access to most labor rights, they are less relevant to our study. Employers and self-employed individuals are also not included in our dataset.

The empirical analysis also considers variables at the UPA (neighborhood) level. A UPA typically has at least 60 households. The average size is around 250 households, a little smaller than census blocks. In each selected UPA, 14 households are interviewed quarterly in a rotating system.

Our analysis uses variables from three categories: demographics, labor market, and location. The demographic variables include the number of years of schooling, age, race, and gender. Among the labor market variables are monthly wages (adjusted for inflation), hours worked, formality, job tenure, and occupation. Formality is defined in two ways: by registration with the government through the employee's work card and by contribution to social security. These two definitions are highly correlated (r=0.85). Finally, location variables include binary variables for urban area, town type, state, and UPA leave-out means for demographic characteristics.

Our sample of domestic workers is shown in Table 2.4. Approximately 84% percent of the group are classified as general domestic service workers, followed by child caretakers (7.5%), home care workers (5.1%), cooks (1.5%), gardeners and horticulturists (0.7%), car, taxi, and truck drivers (0.7%), farmers and skilled gardeners (0.3%), housekeepers and butlers (0.2%), and security guards (0.1%). Our study includes 318,549 domestic worker-quarter of year observations.

In Table 2.5, we present a summary statistics table comparing key characteristics of domestic workers and other workers in our data. These two groups exhibit several notable differences. First, 30 to 35 percent of DWs have formal employment, compared to 70 to 75 percent of other workers. DWs work an average of 33 hours per week, while for other workers work this is equal to 40 weekly hours. Average monthly wages for domestic workers are R$518.1, whereas average salaries for other workers are R$1,172.5. On average, DWs have completed seven years of schooling, while other workers have completed ten. DWs identify as white in 33% of cases, compared to 44% of other workers. In addition, there is a clear gender disparity, as 91.5% of domestic workers are female, compared to only 36.4% of other workers. The average age of DWs is 39 years, compared to 34 years for other workers.

Panel (A) of Figure B.1 shows a histogram of weekly hours worked by both formal and informal domestic workers for the year before CA 72/13. The vast majority of domestic workers work at most 44 hours a week, with formal workers working more hours, on average. Panel (B) shows a similar histogram for monthly wages. A significant number of formal domestic workers earn exactly the minimum wage, while most informal DWs earn less than this amount.

Figure 2.3 shows the total number of domestic workers from 2012 to 2016. The two vertical red lines on each plot represent, respectively, CA 72/13 and CL 150/15, which we believe altered social norms regarding the informality of domestic workers. As compared to the period just before CA 72/13, we find that, by 2016, the number of full-time DWs decreased by 5 percent, informal DWs decreased by 12 percent, and formal DWs increased by 8 percent.

We estimate that 6.13 million domestic workers were employed in Brazil in 2012. In 2016, there were 300,000 fewer full-time domestic workers, 735,000 fewer informal domestic workers, and 490,000 more formal domestic workers. In general, these graphs indicate a shift towards more formal employment in the domestic work sector, as well as a decrease in informal and full-time DWs. This suggests that the legislation's effects are in line with what we expected, and that is creates winners and losers among DWs.

2.4 Estimation Strategy

Event study

To investigate the impact of Constitutional Amendment (CA) 72/13 and Complementary Law (CL) 150/15 on domestic workers, we use an event study framework. The starting period, t^*, is the first quarter of 2013, when the legislation gained substantial media coverage. Our empirical model explains the outcome variable, y, for each individual i, occupation o, neighborhood (UPA) s, and quarter t:

$$y_{iost} = \sum_{k=-4,\neq -1}^{14} \beta_k \mathbb{1}\{t = t^* + k\} \cdot T_o + \beta_E \mathbb{1}\{t > t^* + 14\} \cdot T_o + X_{iost} + \gamma_s + \delta_o + \eta_t + \varepsilon_{iost} \quad (2.1)$$

In this equation, we include fixed effects for occupation (δ_o), quarter (η_t), and neighborhood (UPA, γ_s) to account for unobserved heterogeneity. The treatment variable, T_o, is equal to 1 for domestic workers and 0 for occupations in the control group, detailed below. Standard errors are clustered at the occupation level. This model estimates the coefficients β_k for each quarter relative to the starting point, t^*, capturing the dynamic effects of the legislation on the labor market outcomes of domestic workers. The coefficient β_E captures the long-term effects, starting 14 quarters after t^*.

The covariate matrix, X_{iost}, incorporates individual-level and UPA-level variables. In particular, we include third-degree polynomials of years of schooling and age, as well as binary variables for female and white. The neighborhood (UPA) controls are third-degree polynomials of the leave-out mean of years of schooling, age, real monthly wages, female, and white.

Finding a comparison group

In order to causally estimate the effect of the legislation, we need a suitable comparison group that follows the Stable Unit Treatment Value Assumption (SUTVA).[3] To do this, we first find the average demographic characteristics of each occupation in 2012. Factors such as years of schooling, wages, gender, and race are taken into account. Using this information, we can create control groups that are demographically similar to domestic workers.

In the next step, we generate a variable that measures worker transitions between domestic work (DW) and other occupations (o): $trans^o_{DW}$. This variable measures the percentage of employees in occupation o at quarter t who were domestic workers in quarter $t-1$. This variable is then averaged across the 2012 quarterly data. As a result, we can measure the labor flow between domestic workers and other occupations. The 25th percentile of this variable is 0.1 percent, the median is 0.2 percent, and the 75th percentile is 0.6 percent. This says that most occupations have relatively low flows with domestic work.

We create four control groups based on objective criteria, in order to avoid biases due to hand-picking occupations. The race control group contains occupations with $trans^o_{DW}$ values at or below the 25th percentile and white worker share at or below the 75th percentile (among all occupations). The wage control group includes occupations with a $trans^o_{DW}$ value at or below the 25th percentile, and an average wage at or below the 75th percentile. The schooling control group consists of occupations with a $trans^o_{DW}$ value at or below the 25th percentile and average years of schooling at or below the 75th percentile. The gender control group consists of occupations meeting these conditions: (i) $trans^o_{DW}$ at or below the 50th percentile; (ii) over 50% female; and (iii) the individual is female. Generally, the first three control groups prioritize low-skilled men, while the last group includes many high-skilled women.

These four distinct control groups provide a robustness check to our estimates of the effects of CA and CL on the labor market outcomes of domestic workers. Table 2.6 shows that most workers in our dataset (53.1% or 434,992) belong to only one of these control

[3] As a result, occupations with high labor flows with domestic workers should not be included in the control group.

groups. A significant share (21.9% or 179,198) belongs to two control groups, while the rest belong to three or four. This implies the control groups are distinct and well-defined, reducing the chance that unobserved shocks common to domestic workers and other occupations drive our results.

The schooling control group serves as the comparison group in our main estimation. Our conclusions are robust to this choice. In Appendix Figures, we show results for the other groups. Table 2.7 shows the top occupations in the schooling control group. This includes jobs such as secretarial supervisors (5.5%), agricultural and industrial machinery mechanics (4.9%), machine tool regulators and operators (4.3%), and welders and oxicutters (4.0%). The same information is shown in Table B.1 for the other three comparison groups.

2.5 Results

Figure 2.4 shows the evolution of formality rates over time for both the treatment group and the schooling control group. Throughout the following graphs, the two vertical red lines denote, respectively, CA 72 and CL 150. Both of these legislations likely changed social norms about the acceptability of employing an informal domestic worker. For both groups, there are no pre-trends and the formality rate is stable prior to the Constitutional Amendment. After CA 72/13, however, their paths diverge. In fact, domestic workers' formalization increased significantly after CA 72, and this increase is even greater after CL 150. The control group, on the other hand, does not exhibit the same pattern.

Figure 2.5 presents our event study estimation without control variables (Panel A) and with controls and fixed effects (Panel B). We find that the legislation increased DWs' formality rates by 3 to 6 percentage points. A combination of formalization and a reduction in the number of informal domestic workers is likely to have caused this effect. As the increase in domestic workers' labor costs (due to CA 72 and CL 150) would likely lead to a *decrease* in formality rates, this result highlights the importance of changing norms.

Figure 2.6 shows the same event study analysis for weekly hours and the log of real monthly wages. These results are a little less clear, but we find that weekly hours decreased by one hour and real wages rose by 5 to 10 percent following the legislation. This is again consistent with the expected effects of the legislation in the presence of social norms.

In Figure 2.7, event study results are shown for two indicator variables: overtime work (over 44 hours per week) and being paid the minimum wage. The latter variable can be viewed as another measure of formalization. We find no clear pattern for working overtime. There is, however, a significant increase in the share of domestic workers who receive the minimum wage by about 5 percentage points, relative to the control group.

Figure 2.8 presents additional event studies aimed at unveiling the mechanisms underpinning our findings. In particular, the treatment group is divided into informal and formal domestic workers. There are several interesting findings: (i) Most of the reduction in average work hours is due to informal DWs. This indicates a reduction in labor demand rather than an overtime effect as the main channel, even though the share of overtime work done by formal (but not informal) DWs decreases. (ii) Both types of domestic workers see an increase in wages, though formal workers benefit more after CL 150. Possibly, this indicates the firing of relatively unskilled formal workers during this period. (iii) The increase in the share of DWs paid the minimum wage is concentrated among formal workers. A small effect is also seen for informal DWs after CL 150/15.

Consistent findings across specifications and sets of covariates confirms the robustness of the results. In addition, the results for our four control groups are very similar, as shown in Figure B.2, Figure B.3 and Figure B.4. Thus, our findings are not due to an arbitrary choice of control group (that might not satisfy parallel trends assumptions necessary to estimate causal effects).

We now use a difference-in-differences approach to better summarize the effects described above. The impact of CA 72 and CL 150 on Brazilian domestic workers' labor market outcomes is once again examined. The results of this analysis are presented in Table 2.8. We find an increase in the formality rate of DWs, with an increment of approximately 1 percentage point before enactment of CL 150 – but after CA 72 – and a more pronounced increase of around 5 percentage points (17%) after its implementation. Additionally, we observe a reduction in working hours, which decreases by roughly one hour per week. In addition, the percentage of domestic workers working overtime declined by 2 percentage points post-CA and pre-CL.

Moreover, the share of domestic workers receiving the minimum wage increased from 2 to 5 percentage points over time. A wage increase of approximately 4 to 7 percent accompanied this. All of these findings are statistically significant. Generally, we find stronger effects following the passage of CL 150/15. In light of the concurrent increase in labor costs, this is somewhat surprising. This can be rationalized if informality costs, related to social norms, increased at the same time, when all regulations foreseen in CA 72 actually became law.

As a robustness check, we repeat our analysis using the data in an individual panel format, instead of repeated cross-sections – as we have done so far. Our rotating panel dataset allows individuals to be observed for only five quarters. To identify them across periods, we must also match on observables. Thus, in this case, our sample size is much smaller, which makes it less likely that we will detect effects. Furthermore, composition effects are not fully considered when taking this approach. Figure B.5 shows our results, which are broadly in line with our previous analysis, although they are much noisier.

2.6 Mechanisms

Our analysis so far found that many informal domestic workers were either formalized or lost their jobs after the enactment of Constitutional Amendment (CA) 72. Additionally, more domestic workers received the minimum wage after CA 72 and Complementary Law (CL) 150 were enacted. In light of the traditional literature on informality, these findings seem counter-intuitive. Indeed, this literature would not predict such effects without changes in government enforcement, and especially not given the increased costs of hiring a formal DW.

One possible explanation for this apparent puzzle is that providing pure information about labor rights to uninformed employees and employers results in higher formality rates. This is a plausible mechanism, but we do not believe it is the leading one. As shown in

Figure B.6, previously formal workers who were, presumably, well-informed of their rights also experienced similar formalization effects. As a result, information dissemination alone is not sufficient to account for the observed changes in labor market outcomes among domestic workers.

Framework

As the key findings of our study cannot be explained by the usual mechanisms, we propose that the legislation and related discussions altered social norms in the domestic work market. We assume a simple framework for understanding formalization decisions. Let the domestic worker formality share (FS) be determined by the following reduced form function:

$$FS_{dw} = f(t, p_G \cdot F, SN_{inf}(\theta, \psi, \omega), \varepsilon) \tag{2.2}$$

Based on this framework, the domestic workers' formality share is determined by:

1. t: taxes and other costs for employing a formal worker.
2. p_G: probability of a government inspection; F: the expected fine to pay for skirting labor regulations
3. SN_{inf}: social norms' costs of employing an informal worker. This includes, among other things, social image costs, the expected cost of an employee initiating a lawsuit, and lower effort due to reciprocity effects.
4. ε: other factors that affect the formality share of domestic workers.

In this model, we expect the following relationships between the formality share and its determinants:

1. $\frac{\partial FS_{dw}}{\partial t} < 0$: An increase in taxes for employing a formal worker should lead to a decrease in the formality share.
2. $\frac{\partial FS_{dw}}{\partial (p_G \cdot F)} > 0$: An increase in the probability of government inspection and the associated expected fine for informality should lead to an increase in the formality share.
3. $\frac{\partial FS_{dw}}{\partial SN_{inf}} > 0$: An increase in the social norms' costs of employing an informal worker should lead to an increase in the formality share.

We now discuss additional assumptions surrounding the social norms' cost function, $SN_{inf}(\theta, \psi, \omega)$ and its components. Specifically, we assume that social norms' costs are increasing with respect to: θ, the cumulative exposure employers and employees have to DWs' labor rights, whether through the media or in private conversations; ψ, the degree of closeness between the employer and the employee; and ω, the negative perception of employing informality DWs.

We claim the implementation of Constitutional Amendment 72 led to an increase in ω, resulting in a higher formality share (FS_{dw}) among domestic workers. Subsequently, Complementary Law 150 resulted in an increase of t, and possibly, a further rise of ω. We now investigate differences in θ and ψ across different groups in the context of the legislation, under the hypothesis that $\frac{\partial^2 SN_{inf}}{\partial \omega \partial \theta} > 0$ and $\frac{\partial^2 SN_{inf}}{\partial \omega \partial \psi} > 0$. If this is confirmed, it strengthens our argument that the law changed social norms, since this variation in θ and ψ should not affect formalization decisions under most other mechanisms.

Living arrangements, peer effects and formalization events

Our goal in this section is to better understand the impact of variations in θ and ψ on the social norms' cost function, $SN_{inf}(\theta, \psi, \omega)$. The main focus of our analysis is on our two formality outcomes: work card registration and social security contributions. The evidence presented in this section is suggestive only.

By comparing domestic workers with different living arrangements, we examine the effects of varying ψ. We distinguish between DWs who live with their employers and those who do not. Thus, we conduct separate event studies for each subgroup of the treatment group. Figure 2.9 shows that live-in domestic workers have significantly larger formalization effects (5 to 10 percentage points) than the rest of domestic workers, with and without controls. This finding indicates that the social norms mechanism, through the employer-employee relationship closeness ψ, is relevant.

To investigate θ and its impact on formalization decisions, we compare domestic workers with different exposure to neighborhood residents who: (i) are domestic workers and/or are formalized; (ii) become formal workers over time. As mentioned before, each neighborhood, or UPA, consists of approximately 250 households, with 5 to 20 percent of residents interviewed each quarter in our dataset. Using this survey data, we construct noisy measures of neighborhood workers.

Table 2.9 shows the "peer effects" of neighborhood workers. This table uses data from before Constitutional Amendment 72/13. Despite the lack of causality in these estimates, we find that domestic workers are more likely to be formal the more DWs there are in their neighborhood. Our results indicate that the same applies for peers who are formal non-domestic workers. According to our framework, being surrounded by peers increases the likelihood of learning about formalization and holding a positive view of it. The worker may then discuss their labor rights with their employer and possibly affect their opinion.

Next, we estimate a triple differences event study estimator. This estimator compares domestic workers and the control group before and after CA 72, separated by the number of neighborhood peers. This estimation includes neighborhood leave-out means of demographic controls and neighborhood fixed effects. We thus control for time-specific peer variable effects common to both treatment and control groups.

Table 2.10 shows our estimates for the triple differences coefficients. The outcome variable for all columns is formality. The coefficients refer to the interaction of the DID variable ($Post$ x $Treat$) with different peer effect measures. They are the number of: other domestic workers, non-household domestic workers, other domestic workers across the last three quarters, formal workers, and formal domestic workers. According to the first three peer variables, the likelihood of formality increased by 0.7 to 1.2 points in the treatment group with each additional neighborhood domestic worker during the post-CA and pre-CL period. The effects are smaller and sometimes not statistically significant for the post-CL period, but they are always positive. For neighborhood formal domestic workers and non-domestic workers we find suggestive but not conclusive results.

Figure 2.10 and Figure B.7 present the same analysis graphically. We divide the treatment group by quartile of neighborhood peers and use the work card definition of formality. As in the tables, CA 72 has a greater effect on domestic workers living in neighborhoods with more DWs. Thus, domestic workers experience higher formality treatment effects when their neighborhood has more DWs. These differential effects occur primarily between the constitutional amendment and the complementary law. We find a catch-up in formalization effects by number of peers mid-2015 after CL, when formality costs increased. This could imply that new social norms became more widespread among compliers or a change occurred in the confounding skill-job-neighborhood relationship. Excluding control variables or UPA fixed effects doesn't change the results, showing their robustness.

Our next step is to look at formalization *events*, rather than the *number* of formal or domestic workers in a neighborhood. In each neighborhood, we calculate the average number of formalization events for domestic workers (DWs) and non-domestic workers over the previous two quarters. A formalization event occurs when a worker has a formal job after being informal. This information allows us to re-do our triple differences estimation in order to determine how peer formalization events affects DWs' formalization.

As before, Table 2.11 displays the "peer effects" of DWs or other workers' formalization events. We find that formalization events for other DWs and non-DWs in the same neighborhood correlate with higher formality rates for domestic workers, with statistically significant estimates. As before, neighborhood peers becoming formal can lead to information spread and changes in the acceptability of informality from the employees' perspective, which can then be transmitted to the employer.

Table 2.12 again shows the results of a triple differences estimator analyzing the effects of CA 72 and CL 150 on domestic workers' labor market outcomes. It now investigates whether DID effects vary depending on neighborhood formalization events by DWs and non-DWs. Overall, after CA 72, each additional domestic worker formal event increases formality likelihood by 4 to 8 percentage points, depending on the time period. There is a larger and more significant effect in the post CA and pre CL period. During this time, a one standard deviation increase in neighborhood domestic worker formalization events boosts the treatment effect by about 1.2 percentage points. Despite positive magnitudes for non-domestic worker formalizations, the results are not statistically significant.

Figure 2.11 presents the same analysis as an event study, dividing the treatment group by quartile of formalization events. Our previous findings are reinforced by these graphs, which use the work card definition of formality. The results indicate that domestic workers experience greater formality treatment effects when surrounded by neighborhood formalization events, aligned with a social norms mechanism that is intensified when these events occur.

2.7 Conclusion

This paper analyzes the labor market consequences of legislation on domestic workers' labor rights. For over two years, Constitutional Amendment (CA) 72/13, which aimed to give additional labor rights to domestic workers, had a relatively small impact on formal DWs' labor costs due to the need for additional regulation. Despite this, there was intense media coverage and public interest in domestic workers' rights. The unique setting during this period—where the law was in place but its contents were on hold—allows us to isolate effects on changing social norms related to DW informality. We also evaluate effects after all the elements foreseen in the amendment, with Complementary Law (CL) 150/15, came into effect.

We find a shift between 2012 and 2016 towards formal employment among domestic workers. This is coupled with a decrease in informal, full-time and the total number of DWs. This suggests a response to the legislation through the social norms channel. Using an event study with a control group, we find that domestic workers' formality rates increased by around 4 percentage points during this period. We see a decrease in weekly hours and an increase in wages for DWs. We investigate the mechanisms underlying these findings. We focus on the determinants of social norms related to employing informal workers. We show larger treatment effects for live-in domestic workers. This implies that the social norms mechanism, through variation in the closeness of the employer-employee relationship, is relevant.

Moreover, our analysis of neighborhood "peer effects" shows that domestic workers are more likely to be formalized when there are more DWs in their neighborhood. In addition, DWs experience larger formality treatment effects from CA 72 when surrounded by more domestic workers or by more formalization events in their neighborhood. Thus, it appears the social norms mechanism of the legislation is heightened on exposure to information about labor rights and regulations.

Our study illustrates how social norms affect formalization decisions in the labor market. Additionally, it shows how public discussion and legislation can influence these norms. This mechanism has implications for public policy, as it suggests an additional channel for influencing labor market decisions. Additionally, it calls for strong media coverage of labor market regulations and their implications as a possible way to reduce informality. However, we should also be cautious about negative effects on total employment, so that this mechanisms may be better applied in occupations where unemployment risks are smaller. We leave a detailed welfare discussion related to this for future work. In the future, we plan to survey domestic employers and employees about the factors involved in formalization decisions. By doing so, we will be able to better understand the mechanisms at work. As a result, we will be able to provide further insight into the factors driving formalization of workers and contribute to the development of more effective public policies.

Tables

Table 2.1: Effects of CA 72 and CL 150 on domestic workers' labor rights

Labor right	Before	CA 72/13	CL 150/15
Minimum wage	✓	✓	✓
Thirteenth salary	✓	✓	✓
Paid vacation	✓	✓	✓
Maternity leave (4 months)	✓	✓	✓
Soc. Security contribution	✓	✓	✓
Transportation vouchers	✓	✓	✓
Prior notice (minimum of 30 days)	✓	✓	✓
Maximum of 8 daily hours and 44 weekly hours	X	✓	✓
Overtime bonus of 50%	X	✓	✓
Nighttime work bonus pay	X	X	✓
Severance package	X	X	✓
Unemployment benefits	X	X	✓
Contribution to FGTS	X	X	✓
Family wage	X	X	✓
Insurance on work-related accidents	X	X	✓

Notes: The table provides an overview of effective labor rights for formal domestic workers in Brazil during three periods: before CA 72/13 (Constitutional Amendment 72/2013), between CA 72/13 and CL 150/15 (Complementary Law 150/2015), and after CL 150/15. Red checkmarks highlight the labor rights introduced or expanded by a given legislation. The FGTS contribution aims to protect workers from significant income shocks. At the beginning of each month, companies deposit an amount equal to 8% of each employee's salary toward this fund. Funds in this account can be used in cases of layoffs, health issues, or to assist in purchasing real estate for housing.

Table 2.2: Example of effects of CA 72 and CL 150 on costs of formal domestic workers

Cost	Before	CA 72/13	CL 150/15
Base salary	R$1,000.00	R$1,000.00	R$1,000.00
Overtime hours	R$81.82	R$122.73	R$122.73
Thirteenth salary	R$90.15	R$93.56	R$93.56
Paid vacation	R$33.21	R$34.46	R$34.46
Family wage	-	-	R$26.00
Transportation vouchers	R$154.00	R$154.00	R$154.00
Taxable salary	R$1,205.18	R$1,250.75	R$1,250.75
Net salary	R$1,190.45	R$1,229.64	R$1,229.64
Severance package	-	-	R$40.02
SS contribution	R$96.41	R$100.06	R$100.06
FGTS/SAT	-	-	R$110.07
Income Tax	-	-	-
Total	R$1,286.86	R$1,329.70	R$1,479.79
% Change (from Before)	-	3.3%	11.29%

Notes: The table offers a numerical example of the effective effects of CA 72/13 (Constitutional Amendment 72/2013) and CL 150/15 (Complementary Law 150/2015) on the costs associated with employing a formal domestic worker in Brazil. In these calculations, we assume that weekly regular hours equal 44, weekly overtime hours equal 4, weekly nighttime hours equal 2, the probability of being fired in each period is 10%, and job tenure is 2 years. Family wage applies to salaries up to R$1,089. The social security contribution rate used is valid for salaries up to R$1,389. Income taxes are zero for salaries up to R$1,903. FGTS contributions are explained in the notes of Table 2.1, and SAT is the insurance for work-related injuries.

Table 2.3: Consequences of CA 72 and CL 150 for formal and informal domestic workers

	Before CA 72 (before Q1/2013)	**After CA 72 Before CL 150** (Q2/2013 - Q1/2015)	**After CL 150** (after Q2/2015)
Formal Domestic Worker	-	≤44 hours - >44 hours ↑ Overtime cost	≤44 hours ↑ Taxes >44 hours ↑ Overtime cost ↑ Taxes
Informal Domestic Worker	-	↑ Image costs ↑ Expected punishment ↑ Worker bargaining power	↑ Image costs ↑ Expected punishment ↑ Worker bargaining power

Notes: The table illustrates the effects of CA 72 and CL 150 on formal and informal domestic workers. Time periods are defined as: before CA 72 (prior to Q1/2013), after CA 72 and before CL 150 (Q2/2013 - Q1/2015), and after CL 150 (post Q2/2015). Image costs are the social or self-image costs related to employing an informal domestic worker. Expected punishment represents the anticipated consequences for employers who fail to comply with labor regulations. Worker bargaining power refers to domestic workers' ability to negotiate better job amenities and pay due to increased awareness of their labor rights.

Table 2.4: Top occupations in Treated group

Occupation	Count	Share(%)
Domestic service workers in general	267,437	84.0
Child caretakers	24,019	7.5
Home care workers	16,109	5.1
Cooks	4,781	1.5
Elementary gardening and horticulture workers	2,267	0.7
Drivers of cars, taxis and trucks	2,137	0.7
Farmers and skilled workers in gardens	841	0.3
Housekeepers and domestic butlers	518	0.2
Security guards	401	0.1
Captains, cover officers and practitioners	31	0.0
Water carriers and firewood collectors	8	0.0
Total	318,549	100.0

Notes: The table displays the most common occupations in the treatment group, ranked by the number of workers in each occupation in our dataset. The data comes from the PNAD Continua rotating panel dataset spanning 2012 to 2016.

Table 2.5: Summary statistics

	Domestic Workers	Other Workers
Formality (Work Card) (%)	29.1	69.5
	(45.4)	(46.0)
Formality (Soc. Security) (%)	34.9	75.5
	(47.7)	(43.0)
Weekly Hours	33.68	40.12
	(16.63)	(12.97)
Monthly Wage	R$518.1	R$1,172.5
	(R$379.4)	(R$1,487.7)
Schooling Years	6.997	9.905
	(3.680)	(4.113)
White (%)	33.4	44.8
	(47.2)	(49.7)
Woman (%)	91.5	36.4
	(27.9)	(48.1)
Age	39.32	34.03
	(12.41)	(11.89)
Observations	64,799	495,102

Notes: The table provides descriptive statistics for domestic workers and non-domestic worker employees in Brazil prior to the implementation of CA 72/13 (Constitutional Amendment 72/2013), in 2012. The statistics include two measures of formality (work card registration with the government and employee social security contributions). Standard deviations are shown in parentheses below the mean values for each variable. The total number of observations for domestic workers and other workers is given at the bottom of the table. The data comes from the PNAD Continua rotating panel dataset spanning 2012 to 2016.

Table 2.6: Distribution of inclusion to control groups by non-domestic worker

Count of control groups inclusion	Count	Share(%)
1	434,992	53.1
2	179,198	21.9
3	194,773	23.8
4	9,763	1.2
Total	818,726	100.0

Notes: The table displays the distribution of non-domestic worker employees in our sample based on the number of control groups they qualify for. The data comes from the PNAD Continua rotating panel dataset spanning 2012 to 2016. The control groups are constructed as follows. Define $trans^o_{DW}$. This variable represents the proportion of employees in occupation o who were domestic workers in the previous quarter (quarter $t-1$). For the race control group, we include occupations at or below the 25th percentile of the $trans^o_{DW}$ variable and at or below the 75th percentile of the share of white workers among all occupations. The wage control group consists of occupations with a $trans^o_{DW}$ value at or below the 25th percentile and an average wage at or below the 75th percentile. In the schooling control group, we include occupations with a $trans^o_{DW}$ value at or below the 25th percentile and an average schooling at or below the 75th percentile. The women-focused control group includes occupations that meet the following criteria: (i) at or below the 50th percentile of the $trans^o_{DW}$ value; (ii) more than 50% female; and (iii) the individual is female.

Table 2.7: Top occupations in schooling control group

Occupation	Count	Share(%)
Secretarial supervisors	19,727	5.5
Agricultural and industrial machinery mechanics	17,513	4.9
Machine tool regulators and operators	15,490	4.3
Welders and oxicutters	14,491	4.0
Couriers, baggage handlers and parcel couriers	14,173	3.9
Military graduates and squares	13,921	3.9
Wholesale and retail managers	13,790	3.8
Trade representatives	13,623	3.8
Gas station attendants	13,561	3.8
Auto bodyworkers	11,500	3.2
Electrotechnicians	10,343	2.9
Joiners and the like	9,982	2.8
Total	359,281	100.0

The table shows the distribution of occupations in the schooling control group, ranked by the number of times they were included in this control grous. The data comes from the PNAD Continua rotating panel dataset spanning 2012 to 2016. Define $trans^o_{DW}$. This variable represents the proportion of employees in occupation o who were domestic workers in the previous quarter (quarter $t-1$). In the schooling-based control group, we include occupations with a $trans^o_{DW}$ value at or below the 25th percentile and an average schooling at or below the 75th percentile among all occupations.

Table 2.8: Difference-in-differences effects of CA 72 and CL 150 on domestic workers

	Work Card	Soc. Security	Log Real Wage	Weekly Hours
Post CA/Pre CL	0.85*	1.48***	0.039***	-1.17***
	(0.49)	(0.44)	(0.0038)	(0.087)
Post CL	3.59***	4.79***	0.068***	-1.21***
	(0.49)	(0.43)	(0.0056)	(0.28)
Observations	571303	603376	594210	603376

	Works Overtime	Wage Equal to MW
Post CA/Pre CL	-2.31***	1.71***
	(0.32)	(0.60)
Post CL	-0.76	4.50***
	(0.90)	(0.92)
Observations	604339	246140

Notes: Standard errors clustered by occupation in parentheses. Stars denote statistical significance (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). The table shows difference-in-differences (DID) coefficients for the effects of CA 72 and CL 150, which altered social norms for domestic workers. Two post periods are considered: "Post CA/Pre CL" (after CA 72 but before CL 150) and "Post CL" (after CL 150). The variables presented are an interaction with the treatment group variable; non-interacted versions are omitted. Covariates include individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white. We include fixed effects for occupation, quarter, and neighborhood. Dependent variables are: Work Card (work permit definition of formality), Soc. Security (social security contribution definition of formality), Log Real Wage (log of real monthly wage), and Weekly Hours (hours worked per week).

Table 2.9: Effect of neighborhood peers on formalization

	Work Card	Soc. Security
DW	-49.0	-56.4
	(0.23)	(0.21)
Other DWs	-0.15	-0.56
	(0.057)	(0.053)
Other DWs × DW	2.18	2.37
	(0.072)	(0.068)
Formal Non-DWs	2.31	1.65
	(0.017)	(0.016)
Formal Non-DWs × DW	-0.21	1.07
	(0.025)	(0.024)
Observations	645797	677831

Note: These regressions includes no controls. Standard errors in parentheses. Dependent variables are Work Card (work permit definition of formality) and Soc. Security (social security contribution definition of formality). The table displays the correlation between having other domestic workers (Other DWs) and formal non-domestic workers (Formal Non-DWs) in the neighborhood on formality outcomes. DW is an indicator for being a domestic worker.

Table 2.10: Triple differences estimates of peers on CA 72 and CL 150 effects

	Other Dom Workers	Non-HH Dom Workers	Mean Other DW
Post CA/Pre CL	0.91***	0.82***	1.20***
	(0.17)	(0.17)	(0.22)
Post CL	0.21	0.11	0.54**
	(0.21)	(0.20)	(0.25)
Observations	473374	473374	449753

	Other Formal Workers	Other Formal DWs
Post CA/Pre CL	-0.047	-0.42
	(0.045)	(0.92)
Post CL	-0.14	-2.71
	(0.098)	(1.88)
Observations	473374	471146

(a) Work Card Registration

	Other Dom Workers	Non-HH Dom Workers	Mean Other DW
Post CA/Pre CL	0.74***	0.65***	1.02***
	(0.16)	(0.16)	(0.22)
Post CL	0.27	0.16	0.50**
	(0.21)	(0.21)	(0.25)
Observations	480635	480635	456641

	Other Formal Workers	Other Formal DWs
Post CA/Pre CL	0.12**	2.03**
	(0.048)	(0.89)
Post CL	0.19**	1.55
	(0.081)	(1.80)
Observations	480635	478281

(b) Social Security Contributions

Notes: Standard errors clustered by occupation in parentheses. Stars denote statistical significance (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). The table shows the triple differences estimation relating the DID effects of CA 72 and CL 150 on domestic workers with peer effect variables. Two periods are considered: "Post CA/Pre CL" (after CA 72 but before CL 150) and "Post CL" (after CL 150). The presented estimates are interacted with the treatment group dummy and the peer variable; other triple differences coefficients are omitted. Covariates include individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white. Fixed effects are included for occupation, date, and neighborhood. The dependent variables are: Work Card (work permit definition of formality) in Panel A and Soc. Security (social security contribution definition of formality) in Panel B. The variables interacted with the DID estimator include: Other Dom Workers (number of other domestic workers in the neighborhood), Non-HH Dom Workers (number of non-household domestic workers in the neighborhood), Mean Other DW (mean number of other domestic workers in the neighborhood), Other Formal Workers (number of other formal workers in the neighborhood), and Other Formal DWs (number of other formal domestic workers in the neighborhood).

Table 2.11: Effect of neighborhood formalization events on formalization

	Work Card	Soc. Security
DW	-50.2	-48.6
	(0.14)	(0.14)
Formal Events	1.74	1.04
	(0.20)	(0.19)
Formal Events × DW	0.58	1.57
	(0.29)	(0.28)
DW Formal Events	6.79	4.15
	(0.56)	(0.52)
DW Formal Events × DW	5.94	9.08
	(0.75)	(0.71)
Observations	533829	560452

Note: These regressions includes no controls. Standard errors in parentheses. The dependent variables are Work Card (work permit definition of formality) and Soc. Security (social security contribution definition of formality). The table displays the correlation between having other domestic workers (DW Formal Events) and non-domestic workers (Formal Events) who became formal in the neighborhood in the past 2 quarters on formality outcomes. DW is an indicator variable for being a domestic worker.

Table 2.12: Triple differences estimates of formalization events on CA 72 and CL 150 effects

	Formal Events	DW Formal Events
Post CA/Pre CL	0.45	8.54***
	(0.70)	(1.90)
Post CL	0.40	3.58
	(0.96)	(2.18)
Observations	391895	391895

(a) Work Card Registration

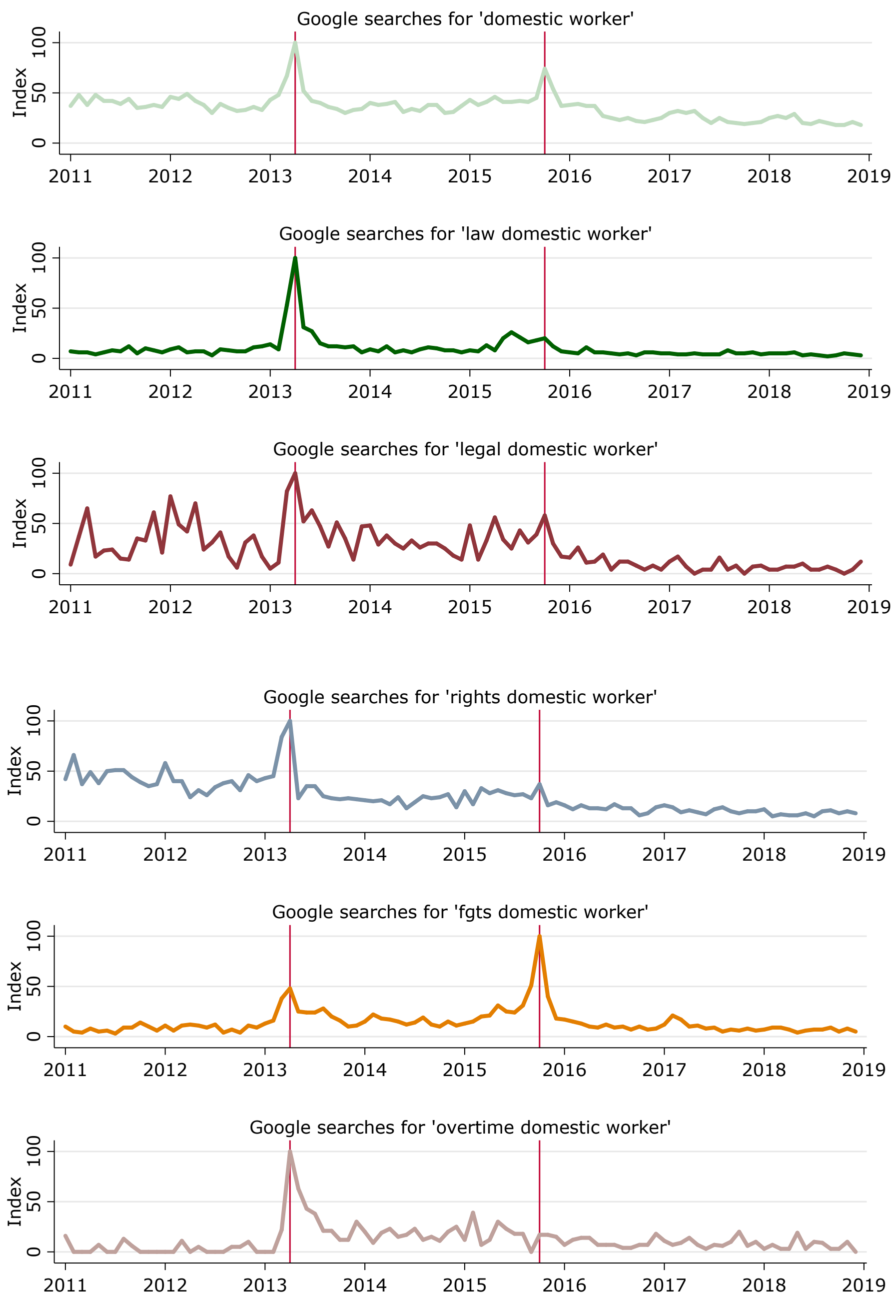
	Formal Events	DW Formal Events
Post CA/Pre CL	1.00	8.82***
	(0.61)	(2.04)
Post CL	0.49	4.26**
	(0.83)	(1.84)
Observations	397970	397970

(b) Social Security Contributions

Notes: Standard errors clustered by occupation in parentheses. Stars denote statistical significance (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). The table shows the triple differences estimation relating the DID effects of CA 72 and CL 150 on domestic workers with formalization events variables. Two periods are considered: "Post CA/Pre CL" (after CA 72 but before CL 150) and "Post CL" (after CL 150). The presented estimates are interacted with the treatment group dummy and the formalization event variable; other triple differences coefficients are omitted. Covariates include individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white. Fixed effects are included for occupation, date, and neighborhood. The dependent variables are: Work Card (work permit definition of formality) in Panel A and Soc. Security (social security contribution definition of formality) in Panel B. Variables interacted with the DID estimator include: number of other domestic workers (DW Formal Events) and non-domestic workers (Formal Events) who became formal in the neighborhood in the past 2 quarters.

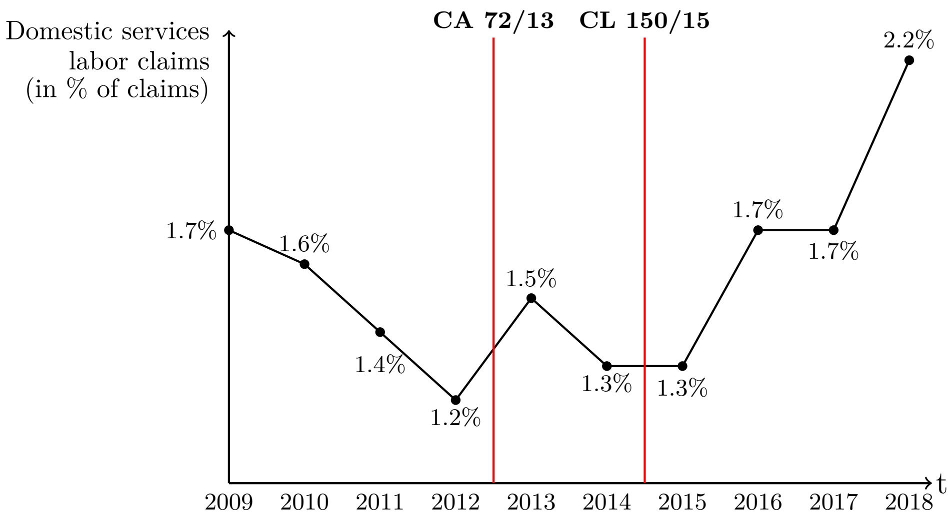
Figures

Figure 2.1: Interest on topics related to domestic workers around CA 72 and CL 150

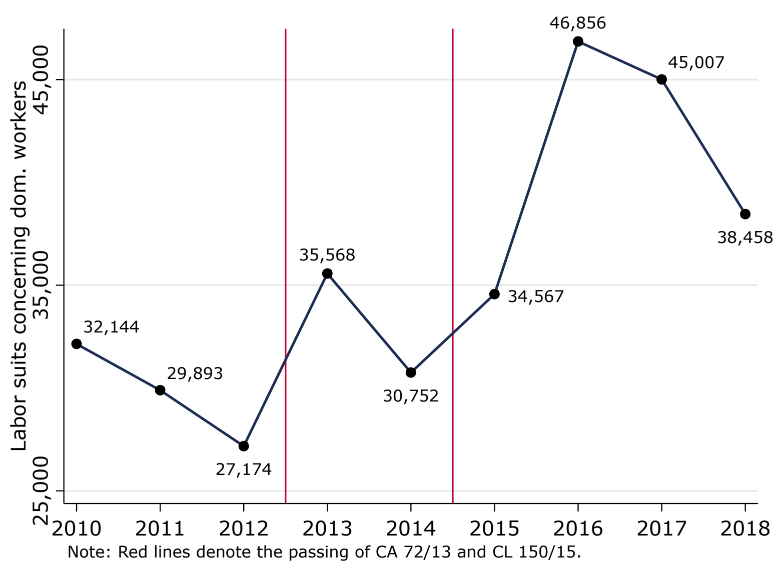


Notes: Weekly data from from Google Trends. The search index is measured relative to total searches during the same period. The first red line marks April 2013, when Constitutional Amendment 72/13 was enacted, while the second red line indicates June 2015, when Complementary Law 150/15 was implemented.

Figure 2.2: Lawsuits against employers of domestic workers around CA 72 and CL 150



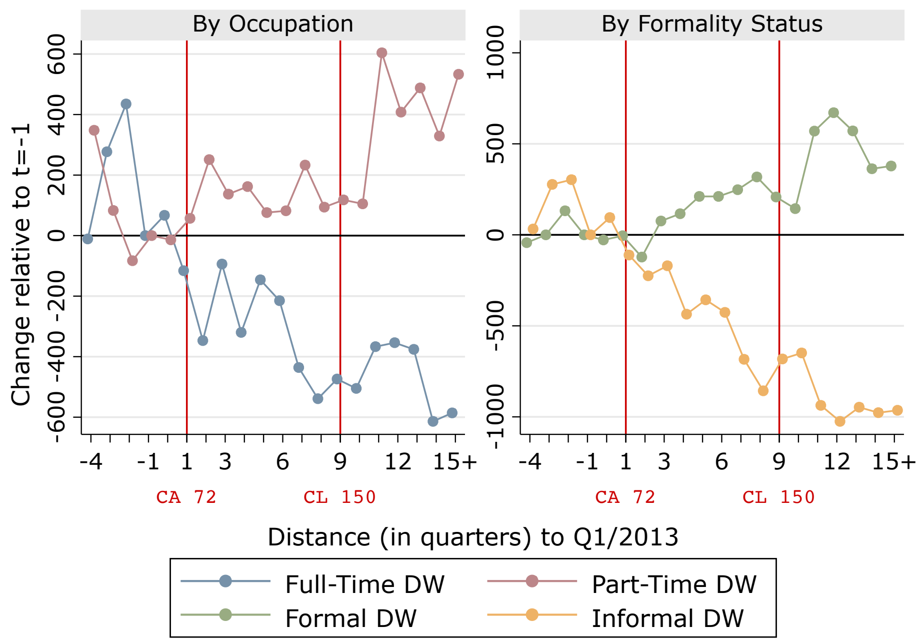
(a) Percentage of total labor claims



(b) Number of labor claims

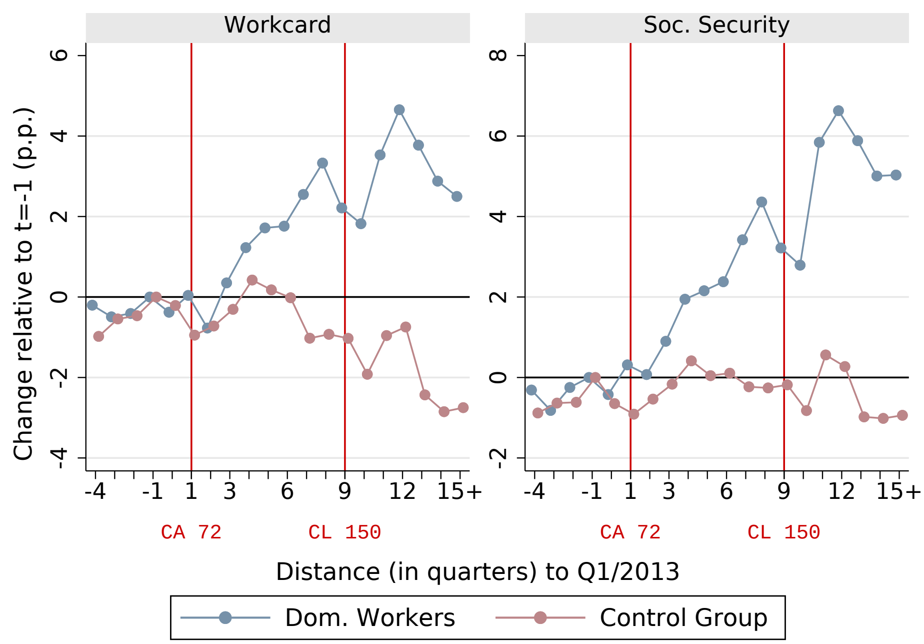
Notes: Yearly data sourced from TRT-2 (Sao Paulo's regional labor court). The red lines represent April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. The figure displays the share (Panel A) and number (Panel B) of labor claims related to domestic services from 2009 to 2018.

Figure 2.3: Time series of domestic workers around CA 72 and CL 150



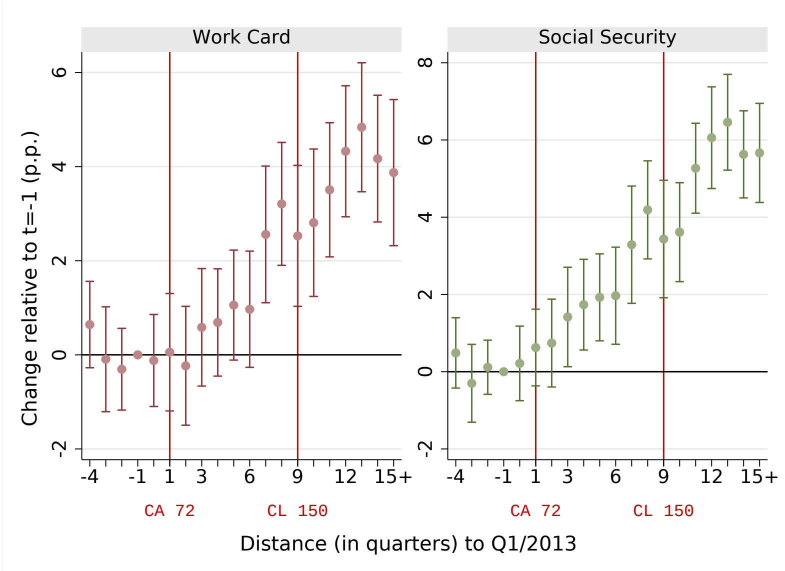
Notes: Graphs split domestic workers by formality status and full/part-time status, measuring variations in their numbers (in thousands) in quarters relative to quarter $t = -1$, just before CA 72/13. The red lines correspond to April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 was implemented.

Figure 2.4: Time series of treatment and control groups around CA 72 and CL 150

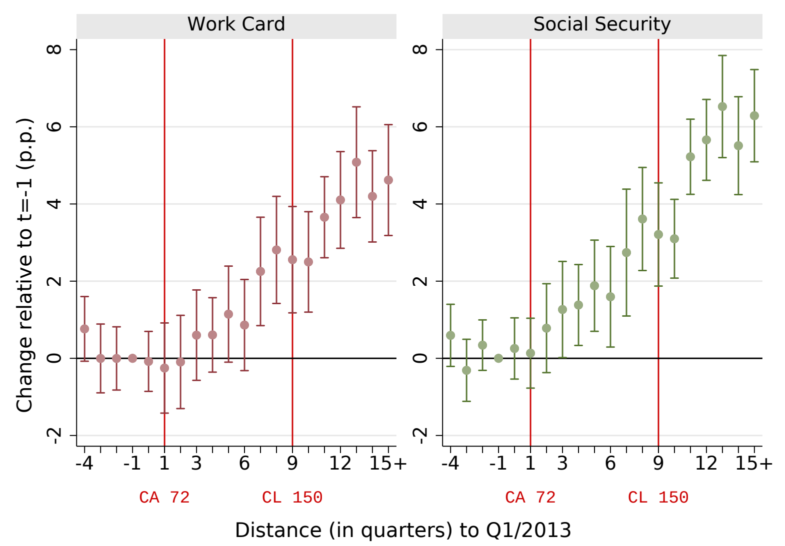


Notes: The graphs display the evolution (in quarters) of formality rates for the treatment group (domestic workers) and the schooling control group (see Section III for details), relative to quarter $t = -1$, just before CA 72. The red lines mark April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. No control variables are included in this model.

Figure 2.5: Event study effects of CA 72 and CL 150 on formality of domestic workers



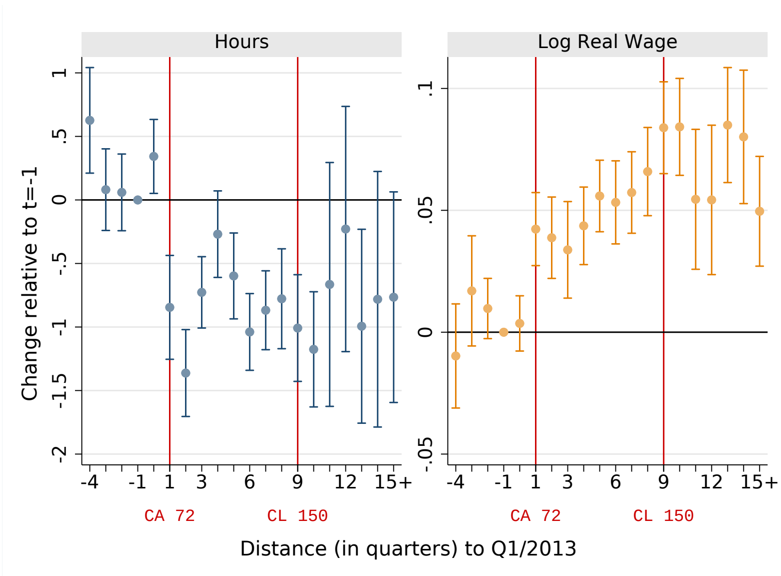
(a) Simple event study



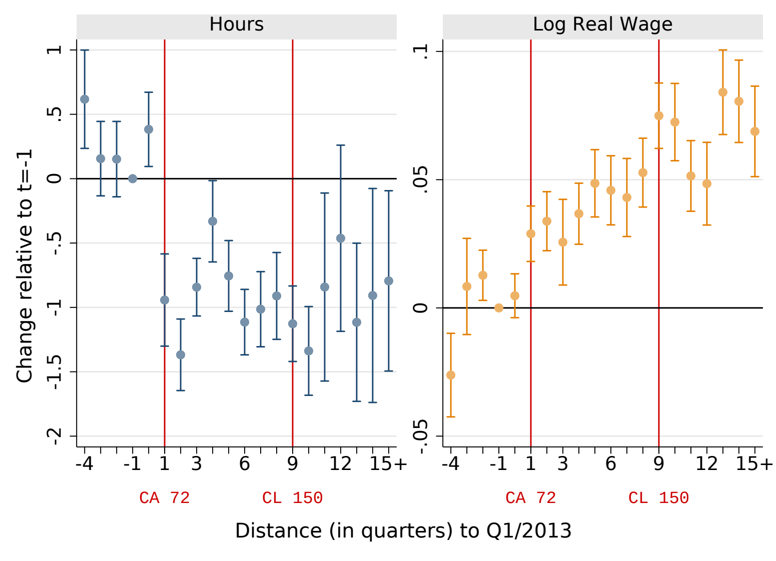
(b) All controls and fixed effects

Notes: Event study analysis of the effects of CA 72 and CL 150 on domestic workers' outcomes, compared to the control group. The red lines mark CA 72 and CL 150. The base period is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used. The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Panel A presents a simple event study, while Panel B includes third degree polynomials of schooling and age, plus variables for female and white, in addition to polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure 2.6: Event study effects of CA 72 and CL 150 on wages and hours of domestic workers



(a) Simple event study



(b) All controls and fixed effects

Notes: Event study analysis of the effects of CA 72 and CL 150 on domestic workers' outcomes, compared to the control group. The red lines mark CA 72 and CL 150. The base period is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used. The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Panel A presents a simple event study, while Panel B includes third degree polynomials of schooling and age, plus variables for female and white, in addition to polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

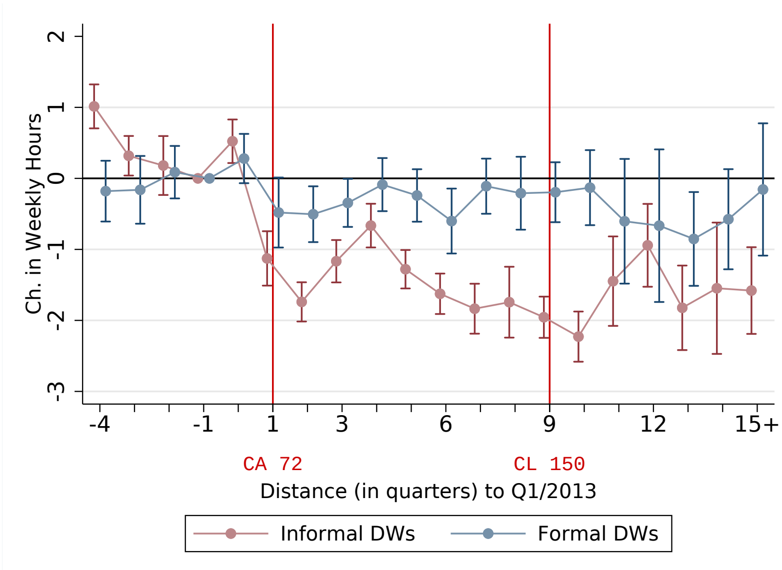
Figure 2.7: Event study effects of CA 72 and CL 150 on other outcomes of domestic workers

(a) Simple event study

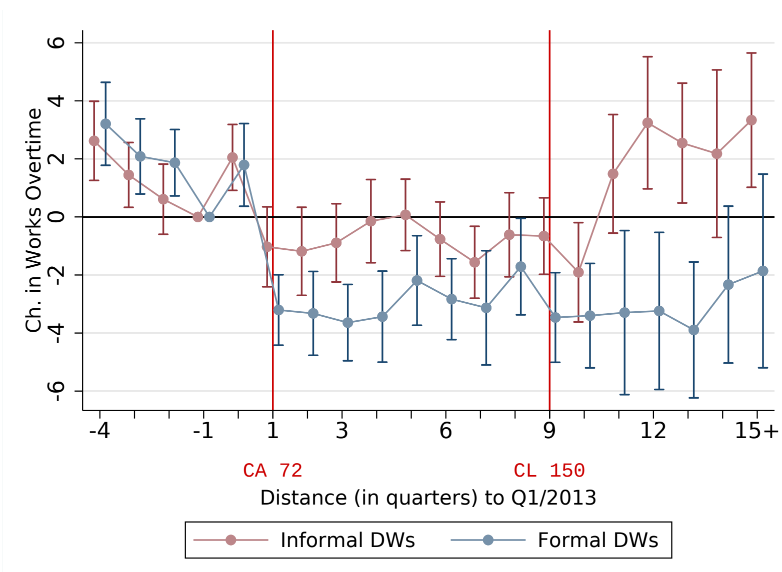
(b) All controls and fixed effects

Notes: Event study analysis of the effects of CA 72 and CL 150 on domestic workers' outcomes, compared to the control group. The red lines mark CA 72 and CL 150. The base period is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used. The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Panel A presents a simple event study, while Panel B includes third degree polynomials of schooling and age, plus variables for female and white, in addition to polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure 2.8: Event study results by formality status of domestic workers

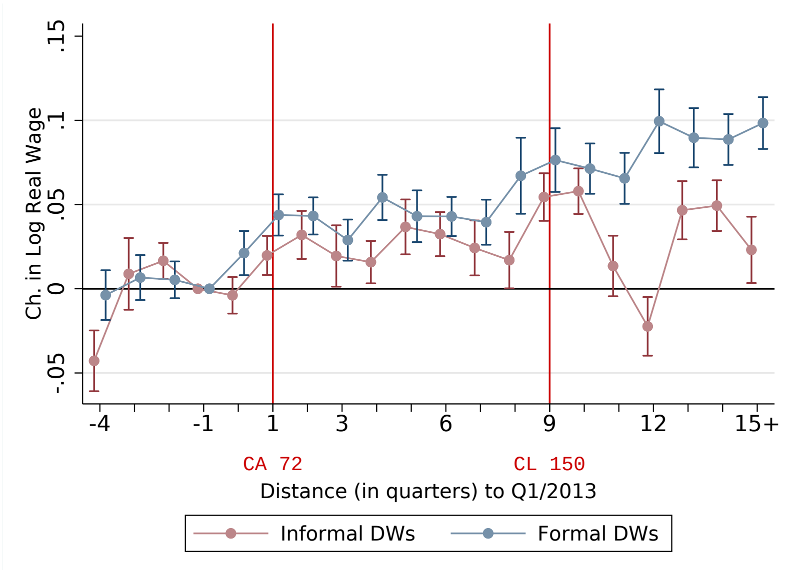


(a) Weekly hours

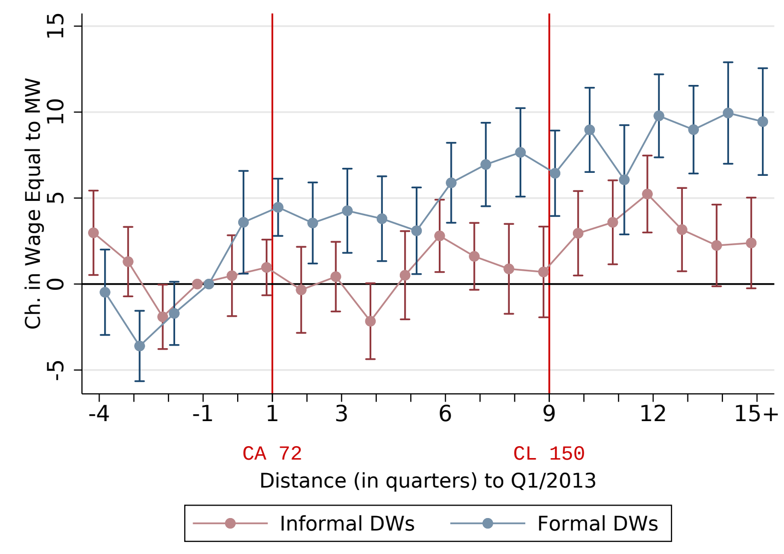


(b) Works overtime

Figure 2.8: Event study results by formality status of domestic workers (cont.)



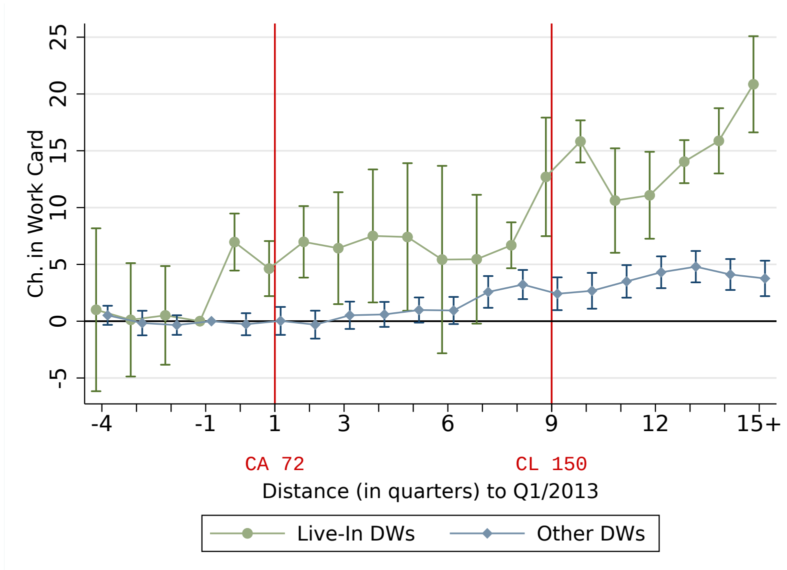
(c) Log of real monthly wages



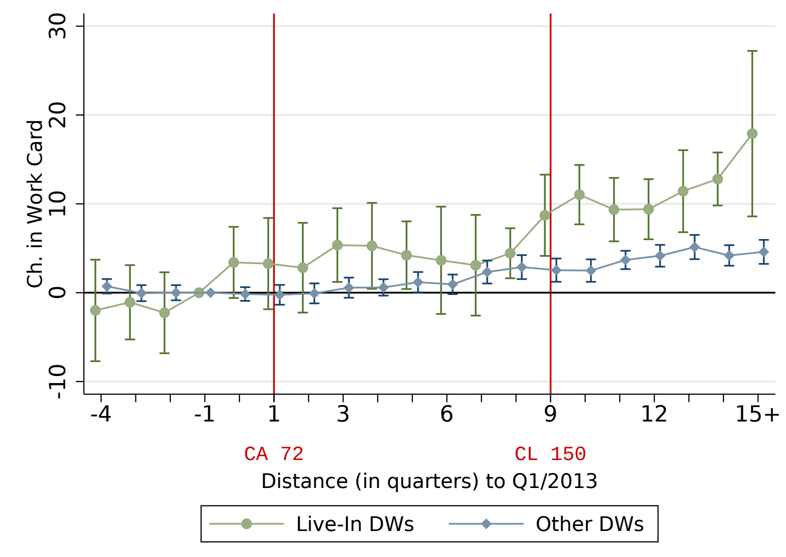
(d) Paid the minimum wage

Notes: Event study analysis of the effects of CA 72 and CL 150 on domestic workers' outcomes, separately by formality status, compared to the control group. The red lines mark CA 72 and CL 150. The base period is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used. The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Panel A presents a simple event study, while Panel B includes our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure 2.9: Event study effects of CA 72 and CL 150 on formality of live-in DWs



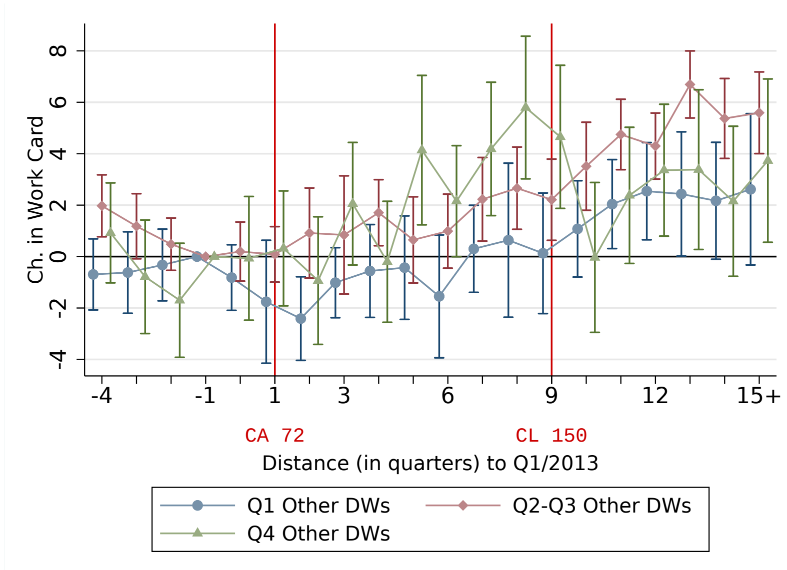
(a) Simple event study



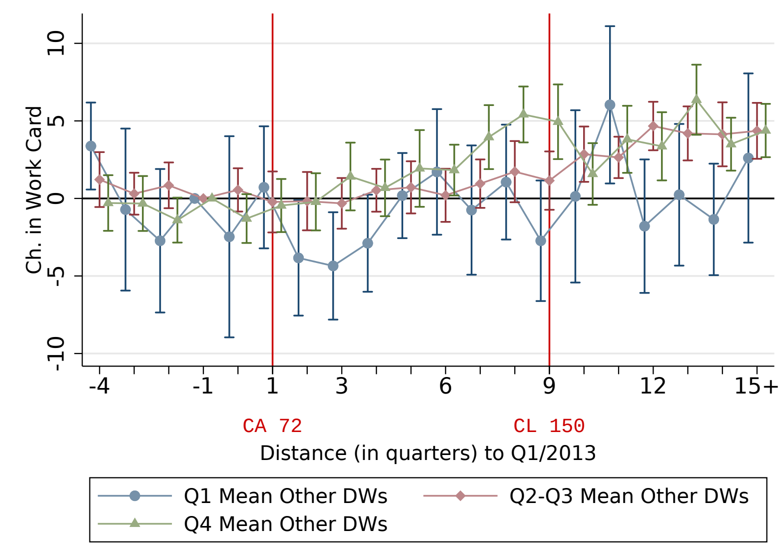
(b) All controls and fixed effects

Notes: Event study analysis of the effects of CA 72 and CL 150 on domestic workers' outcomes, separately by living arrangements, compared to the control group. The red lines mark CA 72 and CL 150. The base period is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used. The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Panel A presents a simple event study, while Panel B includes our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure 2.10: Effects of CA 72 and CL 150 on DWs' formality by peers quartile



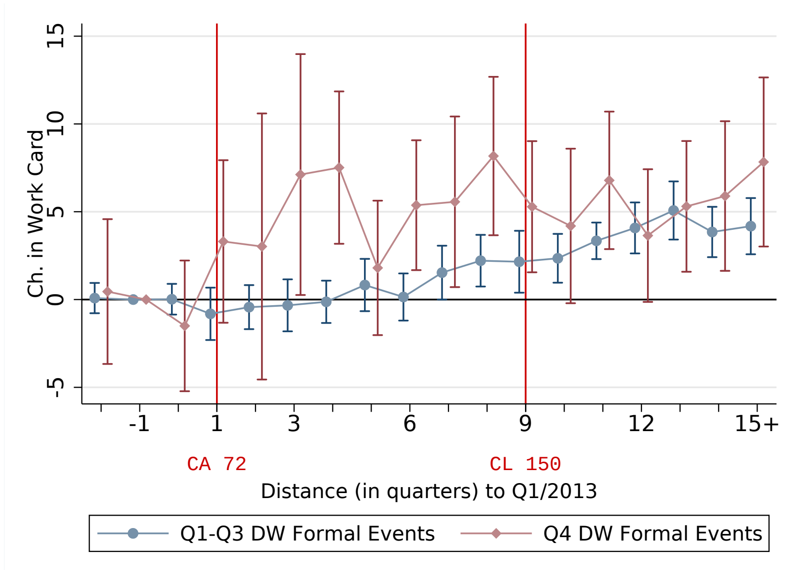
(a) Number of other domestic workers



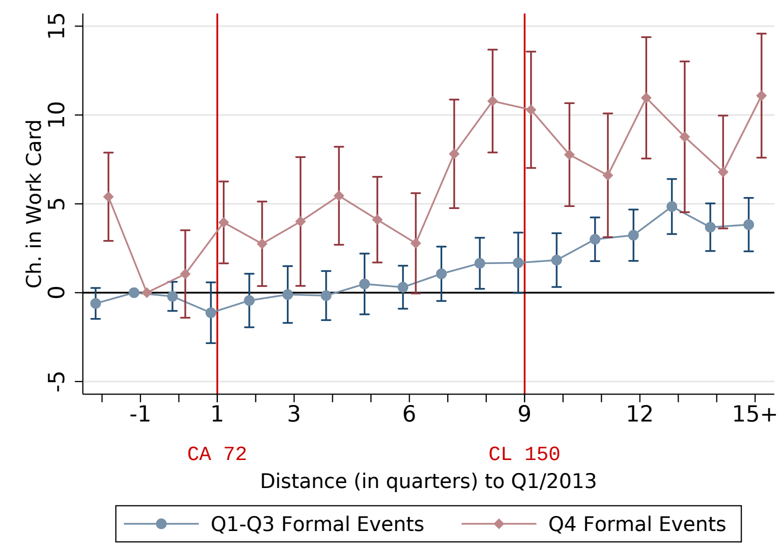
(b) Average number of other domestic workers (3 quarters)

Notes: Event study analysis of the effects of CA 72 and CL 150 on domestic workers' outcomes, compared to the control group. The treatment group is separated by quartile of neighborhood peer group. The red lines mark CA 72 and CL 150. The base period is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used. The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure 2.11: Effects of CA 72 and CL 150 on DWs' formality by formality events



(a) Number of domestic worker formalizations



(b) Number of non-domestic worker formalizations

Notes: Event study analysis of the effects of CA 72 and CL 150 on domestic workers' outcomes, compared to the control group. The treatment group is separated by quartile of neighborhood formalization events. The red lines mark CA 72 and CL 150. The base period is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used. The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

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Appendix A

Appendix to Chapter 1

A.1 Additional Tables

Table A.1: Selective attrition on midline survey

Answered Midline survey?	**No**			**Yes**			
	N	Mean	SD	N	Mean	SD	**Diff.**
Age 18-34	244	0.46	0.50	210	0.38	0.49	-0.083*
Age 35-54	244	0.43	0.50	210	0.52	0.50	0.098**
White	244	0.75	0.44	210	0.70	0.46	-0.041
Male	244	0.43	0.50	210	0.39	0.49	-0.040
College Degree	244	0.36	0.48	210	0.41	0.49	0.054
HHold Income 0−40k	244	0.59	0.49	210	0.49	0.50	-0.104**
No Household Budget	244	0.24	0.43	210	0.19	0.39	-0.056
Struggling Financially	244	0.47	0.50	210	0.38	0.49	-0.086*
Experience Delivery (12+ mo.)	244	0.52	0.50	210	0.63	0.48	0.109**
Experience Rideshare (12+ mo.)	244	0.21	0.41	210	0.26	0.44	0.049
Gig Pay is Essential	244	0.84	0.37	210	0.83	0.37	-0.003
Employed Full-Time Prior to Gig	244	0.38	0.49	210	0.35	0.48	-0.029
Employed Part-Time Prior to Gig	244	0.18	0.39	210	0.23	0.42	0.048
Unemployed Prior to Gig	244	0.10	0.30	210	0.10	0.30	-0.002
Has Other Gig Job	244	0.36	0.48	210	0.35	0.48	-0.008
Has Non-Gig Job	244	0.17	0.37	210	0.18	0.38	0.008
Hourly Pay	210	17.57	7.40	190	18.33	7.64	0.761
Hourly Net Pay	193	12.19	5.32	167	12.61	5.20	0.424
Weekly Hours	213	17.87	16.19	191	16.90	13.12	-0.975
Weekly Pay	240	271.04	227.91	205	299.60	274.24	28.559
Hourly Pay (Recall)	236	18.61	6.34	200	19.14	6.14	0.523
Net Hourly Pay (Recall)	219	15.48	6.04	193	15.43	6.61	-0.044
Weekly Pay (Recall)	236	362.15	240.15	203	385.23	292.92	23.074
Weekly Hours (Recall)	238	22.58	14.00	202	22.82	13.21	0.239

Notes: We present the number of observations, the mean and standard deviation of observable characteristics for the group of individuals who either did not (*No*) or did (*Yes*) reply to the midline survey. The data in these table is collected in our baseline survey. *Struggling Financially* is defined as receiving calls from collectors, contemplating bankruptcy, or struggling to pay the bills. The four final rows present pooled recalls of job outcomes for the previous week and the previous month. The four rows before that show actual job outcomes, collected from screenshots of gig economy apps that workers submit. The last column shows the difference in means between the two groups for each variable. Stars are used to denote the statistical significance of this difference (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table A.2: Selective attrition on endline survey

Answered Endline survey?	**No**			**Yes**			
	N	Mean	SD	N	Mean	SD	**Diff.**
Age 18-34	218	0.42	0.50	190	0.42	0.50	-0.001
Age 35-54	218	0.44	0.50	190	0.51	0.50	0.065
White	218	0.70	0.46	190	0.73	0.45	0.024
Male	218	0.39	0.49	190	0.43	0.50	0.046
College Degree	218	0.36	0.48	190	0.40	0.49	0.038
HHold Income 0−40k	218	0.57	0.50	190	0.48	0.50	-0.085*
No Household Budget	218	0.22	0.41	190	0.22	0.41	0.000
Struggling Financially	218	0.43	0.50	190	0.41	0.49	-0.021
Experience Delivery (12+ mo.)	218	0.56	0.50	190	0.61	0.49	0.055
Experience Rideshare (12+ mo.)	218	0.27	0.44	190	0.22	0.42	-0.045
Gig Pay is Essential	218	0.83	0.37	190	0.82	0.38	-0.014
Employed Full-Time Prior to Gig	218	0.33	0.47	190	0.41	0.49	0.080*
Employed Part-Time Prior to Gig	218	0.18	0.38	190	0.23	0.42	0.047
Unemployed Prior to Gig	218	0.11	0.31	190	0.09	0.29	-0.016
Has Other Gig Job	218	0.35	0.48	190	0.34	0.48	-0.011
Has Non-Gig Job	218	0.13	0.34	190	0.22	0.41	0.087**
Hourly Pay	194	17.48	7.87	168	18.47	7.12	0.987
Hourly Net Pay	175	12.30	5.55	151	12.47	4.84	0.175
Weekly Hours	192	18.27	16.87	173	16.62	12.81	-1.657
Weekly Pay	216	279.26	251.44	184	282.62	238.69	3.364
Hourly Pay (Recall)	207	18.87	6.46	184	19.06	6.20	0.183
Net Hourly Pay (Recall)	194	15.74	6.49	178	15.48	5.98	-0.251
Weekly Pay (Recall)	208	369.80	256.66	185	376.68	273.98	6.881
Weekly Hours (Recall)	208	22.63	14.29	187	22.71	13.13	0.081

Notes: We present the number of observations, the mean and standard deviation of observable characteristics for the group of individuals who either did not (*No*) or did (*Yes*) reply to the endline survey. The data in these table is collected in our baseline survey. *Struggling Financially* is defined as receiving calls from collectors, contemplating bankruptcy, or struggling to pay the bills. The four final rows present pooled recalls of job outcomes for the previous week and the previous month. The four rows before that show actual job outcomes, collected from screenshots of gig economy apps that workers submit. The last column shows the difference in means between the two groups for each variable. Stars are used to denote the statistical significance of this difference (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table A.3: Comparing our sample with other studies and the United States gig market

	Mean of: Worked for Company		
	Sample (percent)	US (Millions)	US (Sample)
DoorDash	64.76	2.00	2.00
Postmates	5.29	0.10	0.15
Grubhub	16.96	0.45	0.53
Uber Eats	35.02	0.80	1.09
Instacart	32.16	0.60	1.03
Uber	16.08	1.50	0.50
Lyft	10.79	1.00	0.34

(a) Company distribution

	Summary Stats Across Studies			
	Sample	Pew (2021)	Parrott-Reich (2020)	Doordash (2021)
Hourly Pay	17.94		23.23	25.00
10+ Hours	0.61	0.37		0.10
20+ Hours	0.34		0.56	
Age 18-34	0.42		0.37	
Age 35-54	0.47		0.53	
White	0.73	0.51	0.45	0.62
Male	0.42	0.44	0.83	0.53
Has Another Job	0.50	0.69	0.59	0.54
College Degree	0.38	0.22		

(b) Sample comparison

Notes: In Panel (A), we show how the distribution of companies in our sample compares to the distribution of gig workers for each gig company in the United States. The data from our sample is the answer to the question: "For which gig companies did you work in the past 3 months?". We obtain information on contracting for gig companies in the United States from multiple sources referring to 2020 or 2021. We use market share information and our own calculations to reach these figures. In the third column of Panel (A), we normalize the values from the first column so that the number of workers working for DoorDash in our sample matches the number of DoorDash workers in the United States (2 million). Comparing these numbers with the second column says whether other companies in our sample are over or underrepresented relative to DoorDash. In Panel (B), we compare mean characteristics in our sample to previous studies of the gig economy. Parrott and Reich (2020) survey both Uber and Lyft drivers in Seattle, Doordash (2021) is a corporate DoorDash survey and Pew (2021) is a study from Pew Research Center of all gig work on online platforms in the United States. In Doordash (2021), hourly pay is based only on time spent actively on gigs. In addition, zero work hour weeks are included in their calculations. For all studies besides ours and Pew (2021), *White* excludes Hispanic white. We define binary variables for 10+ and 20+ hours of work that equal 1 if an individual works at least that amount on average. The statistics presented here for other studies are partially derived from the author's calculations.

Table A.4: Correlation of overestimation in forecasts and recalls across job outcomes

	Overestimation (Forecast - Actual)		
	Weekly Pay	Weekly Hours	Hourly Pay
Weekly Pay	1.00		
Weekly Hours	0.65***	1.00	
Hourly Pay	0.04	-0.31***	1.00

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(a) Forecast

	Overestimation (Recall - Actual)				
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Weekly Pay	1.00				
Net Weekly Pay	0.77***	1.00			
Weekly Hours	0.54***	0.42***	1.00		
Hourly Pay	0.29***	0.16***	-0.14**	1.00	
Net Hourly Pay	0.16***	0.13**	-0.18***	0.70***	1.00

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) Recall

Notes: We present the correlation matrix of overestimation of forecasts – Panel (A) – or recalls – Panel (B) – of job outcomes. Overestimation may be negative and is measured as the recall or forecast belief minus the actual job outcome for the same time period. Recall variables pool recalls of the week and the month before the baseline survey. Forecast variables refer to the first full week (starting on a Monday) following the baseline survey. We use an estimate of expected expenses to calculate net weekly pay and net hourly pay. Stars are used to denote statistical significance (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table A.5: Overestimation of job outcome by recall period

	Overestimation (Recall - Actual)				
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Last Week	66.8***	71.5***	3.33***	0.81	2.97***
	(13.0)	(13.2)	(0.95)	(0.62)	(0.60)
Last Month	99.4***	102.1***	7.05***	0.64	2.35***
	(8.96)	(9.15)	(0.69)	(0.45)	(0.45)
Observations	439	400	397	393	345

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). The regressions include no constant term. Overestimation of each outcome may be negative is defined as the recall belief minus the actual job outcome for the same time period. *Last Week* (*Last Month*) is a binary variable equal to 1 if the worker was asked to recall the last week (last month) in the baseline survey. We use an estimate of expected expenses to calculate net weekly pay and net hourly pay.

Table A.6: Heterogeneity analysis for overestimation of recalls of job outcomes

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Certain of Beliefs	22.5	18.5	2.68*	0.85	1.59*
	(19.9)	(19.9)	(1.48)	(0.95)	(0.93)
Excluded Group	70.2***	76.8***	3.55***	0.0021	1.26
	(18.2)	(18.0)	(1.34)	(0.86)	(0.84)
Observations	439	400	397	393	345

(a) Certainty of beliefs

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Full-Time Driver	-2.32	33.1*	-11.8***	2.56**	2.26**
	(17.7)	(17.7)	(1.49)	(0.99)	(0.98)
Excluded Group	89.4***	84.3***	7.48***	0.30	2.22***
	(8.44)	(8.61)	(0.57)	(0.39)	(0.39)
Observations	439	400	397	393	345

(b) Full-time workers

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
No Household Budget	30.7*	19.4	4.18***	0.22	-0.14
	(17.9)	(18.0)	(1.35)	(0.88)	(0.86)
Struggling Financially	9.00	-1.81	1.93*	-0.28	-1.06
	(15.2)	(15.5)	(1.15)	(0.75)	(0.74)
Pay is Essential	61.9***	54.9**	2.71*	0.27	1.19
	(20.8)	(21.2)	(1.57)	(1.03)	(1.02)
Excluded Group	25.9	42.1**	1.69	0.55	2.06**
	(19.5)	(19.9)	(1.48)	(0.97)	(0.96)
Observations	439	400	397	393	345

(c) Financial need

Table A.6: Heterogeneity analysis for overestimation of recalls of job outcomes (cont.)

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Age 18-34	4.91	-26.1	-0.043	2.26*	0.33
	(27.4)	(27.6)	(2.09)	(1.35)	(1.31)
Age 35-54	24.3	21.6	2.40	1.86	0.18
	(26.8)	(26.9)	(2.04)	(1.32)	(1.29)
White	3.22	7.75	-0.62	-0.80	-0.69
	(17.4)	(17.5)	(1.32)	(0.86)	(0.86)
Male	12.3	25.4	1.06	-0.68	0.74
	(16.1)	(16.3)	(1.20)	(0.77)	(0.77)
College Degree	-10.5	-10.3	-0.74	0.25	0.42
	(15.4)	(15.7)	(1.18)	(0.75)	(0.76)
HHold Income \$0-\$40k	10.4	13.4	2.08*	0.055	0.54
	(15.5)	(15.8)	(1.18)	(0.76)	(0.76)
Excluded Group	66.2**	73.5**	3.76	-0.40	2.08
	(32.6)	(32.8)	(2.49)	(1.61)	(1.58)
Observations	439	400	397	393	345

(d) Demographics

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Was Employed Full-Time	48.6***	61.8***	3.26**	0.54	0.53
	(17.5)	(17.9)	(1.34)	(0.86)	(0.86)
Was Employed Part-Time	-6.72	-1.17	1.31	-1.50	-0.86
	(20.6)	(20.7)	(1.58)	(1.02)	(1.00)
Was Self-Employed	54.6*	61.5*	2.03	-2.06	-1.42
	(32.9)	(32.5)	(2.62)	(1.64)	(1.64)
Has Other Gig Job	8.03	12.2	1.14	0.82	0.61
	(15.7)	(16.0)	(1.21)	(0.77)	(0.77)
Has Non-Gig Job	-43.4**	-50.7**	-2.32	-0.50	-0.65
	(20.3)	(20.4)	(1.53)	(0.99)	(0.98)
Excluded Group	74.1***	70.9***	4.20***	0.72	2.54***
	(13.1)	(13.2)	(1.00)	(0.65)	(0.64)
Observations	439	400	397	393	345

(e) Labor market

Notes: We regress overestimation of job outcome for recalls against worker characteristics. Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Overestimation of each outcome may be negative and is defined as the recall belief minus the actual job outcome for the same time period. We pool recalls of the week and the month before the baseline survey. *Certain of Beliefs* is a binary variable equal to 1 if a worker is totally or somewhat certain about their recalls. *Full-Time Worker* is equal to 1 if a gig worker, on average, works for at least 30 hours a week and is 0 otherwise. *Struggling Financially* is a binary variable equal to 1 if a worker reports either to struggle to pay the bills, get calls from collectors or is considering bankruptcy. *Gig Pay is Essential* is a binary variable equal to 1 if gig income is used primarily for purchasing essential goods such as food and housing. On Panel (E), we define binary variables relating to employment previous to working for the gig company (prefixed by "*Was*") and other employment currently (prefixed by "*Has*").

Table A.7: Correlation of gig work experience with overestimation of job outcomes

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Experience Delivery (6-12 mo.)	17.1	-16.2	0.19	2.64**	0.10
	(23.6)	(24.6)	(1.86)	(1.17)	(1.16)
Experience Delivery (12+ mo.)	72.5***	53.5***	2.71**	3.19***	2.26***
	(16.9)	(17.2)	(1.31)	(0.83)	(0.83)
Experience Rideshare (6-12 mo.)	-15.3	34.0	1.77	-0.18	0.61
	(30.6)	(31.1)	(2.49)	(1.60)	(1.58)
Experience Rideshare (12+ mo.)	9.32	-0.96	0.31	-0.76	-0.0060
	(17.8)	(17.9)	(1.37)	(0.87)	(0.87)
Excluded Group	43.2***	61.3***	3.98***	-1.36*	1.21*
	(14.8)	(15.0)	(1.14)	(0.72)	(0.71)
Observations	439	400	397	393	345

Notes: We regress overestimation of weekly pay on binary variables for experience in ride share and delivery gig work. The excluded group has less than 6 months of experience in both ride share and delivery. Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Overestimation of each outcome is defined as the recall belief minus the actual job outcome for the same time period. We pool recalls of the week and the month before the baseline survey.

Table A.8: Robustness checks for over-recalling of job outcomes

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
One Company	-27.2*	-16.3	-0.96	-1.58**	-1.23*
	(14.8)	(15.1)	(1.13)	(0.72)	(0.71)
Excluded Group	102.1***	100.0***	6.23***	1.48***	3.17***
	(10.3)	(10.5)	(0.79)	(0.51)	(0.50)
Observations	439	400	397	393	345

(a) Number of companies

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Belief is a Round Number	68.2***	22.3	5.21***	0.91	0.65
	(16.1)	(15.5)	(1.25)	(0.72)	(0.70)
Excluded Group	43.0***	83.6***	2.06*	0.26	2.54***
	(13.5)	(9.59)	(1.06)	(0.52)	(0.48)
Observations	432	400	392	387	337

(b) Rounding up

Notes: We regress overestimation of job outcomes recalls against covariables. Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Overestimation of each outcome can be negative and is defined as the recall belief minus the actual job outcomes for the same time period. We pool recalls of the week and the month before the baseline survey. *One Company* is a binary variable equal to 1 if a worker has only worked for one gig company in the past 3 months. *Belief is a Round Number* is equal to 1 if a worker's recall belief is a multiple of 5 (for weekly hours,hourly wage and net hourly wage) or a multiple of 50 (for gross and net weekly pay) and 0 otherwise.

Table A.9: Predictions from motivated beliefs theory: selective recall and updating (other)

	Recall Belief					
	Weekly Hours			Hourly Pay		
	(1)	(2)	(3)	(4)	(5)	(6)
Maximum	0.38***	0.34***	0.13	0.22***	0.20***	0.081
	(0.081)	(0.081)	(0.15)	(0.056)	(0.056)	(0.092)
Minimum	0.22**	0.22**		0.18***	0.18***	
	(0.10)	(0.10)		(0.067)	(0.068)	
Mean			0.47***			0.33***
			(0.18)			(0.11)
Observations	293	293	293	289	289	289
Demographic Controls		✓			✓	
p-value(Max=Min)	0.37	0.49		0.76	0.86	

(a) Selective Recall

	Belief_t			
	Weekly Hours		Hourly Pay	
	(1)	(2)	(3)	(4)
Belief_{t-1}	0.66***	0.62***	0.79***	0.79***
	(0.064)	(0.072)	(0.090)	(0.097)
$1\{\text{Actual}_t \geq \text{Belief}_{t-1}\}$	10.8***	10.2**	8.55***	9.46***
	(2.11)	(3.90)	(1.84)	(3.17)
$1\{\text{Actual}_t < \text{Belief}_{t-1}\}$	-2.25	-2.28	2.49	3.45
	(1.80)	(3.72)	(2.01)	(3.30)
Observations	149	149	140	140
Demographic Controls		✓		✓
p-value(Above=Below)	0.014	0.29	0.0033	0.045

(b) Belief Updating

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year. Outcome variables in Panel (A) are recalls referring to the month before the baseline survey. Panel (A) shows regressions of recalls of job outcomes on functions of the 4 previous weeks of the actual job outcome. We define the minimum, the maximum and the mean for this set of four weeks. We present the p-value for a test of whether the coefficients for the maximum and the minimum variables are the same. In Panel (B), only participants that replied to our midline survey are included. $Belief_t$ is the recall of a job outcome of the first full week (starting on a Monday) after the baseline survey. $Belief_{t-1}$ is the pooled recall for each job outcome for the week or the month before the baseline survey. $Actual_t$ refers to the actual job outcome (obtained from a screenshot submitted from the gig platform app) of the first full week after the baseline survey. Each column shows the result of the regression of $Belief_t$ on $Belief_{t-1}$ and two binary variables: a variable equal to 1 if $Actual_t$ is above or equal to $Belief_{t-1}$, and a variable equal to 1 if $Actual_t$ is below $Belief_{t-1}$. There is no constant term in the regressions in Panel (B). We present the p-value for a test of whether the coefficients for the two indicator variables are the same.

Table A.10: Predictions from motivated beliefs theory: variance of outcomes (other)

	Overestimation (Belief - Actual)			
	Weekly Hours		**Hourly Pay**	
	(1)	(2)	(3)	(4)
Last 4 Weeks (Actual)				
Coefficient of Variation (CV)	0.072^{**}	0.077^{***}	0.10^{***}	0.10^{***}
	(0.028)	(0.029)	(0.029)	(0.030)
Constant	4.03^{***}	1.23	-1.47^{**}	-1.91
	(1.36)	(3.67)	(0.72)	(2.32)
Observations	201	201	202	202
Demographic Controls		✓		✓
Average CV (SD/Mean)	0.41	0.41	0.19	0.19

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Overestimation of a job outcome may be negative and is defined as the recall belief minus the actual job outcomes for the same time period. We show regressions of the overestimation of a job outcome against the coefficient of variation for the 4 previous weeks of that outcome. The coefficient of variation is the standard deviation over the mean. We normalize this variable so that a 1 unit increase is equal to an increase of 1 percentage point. We present the average coefficient of variation underneath each column. Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year.

Table A.11: Predictions from motivated beliefs theory: selective recall (forecasts)

	Forecast Belief								
	Weekly Pay			Weekly Hours			Hourly Pay		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Last 4 Weeks									
Maximum	0.65***	0.63***	0.48***	0.42***	0.38***	0.27	0.17**	0.16**	0.0068
	(0.076)	(0.079)	(0.16)	(0.11)	(0.11)	(0.20)	(0.067)	(0.067)	(0.12)
Minimum	0.22**	0.23**		0.099	0.13		0.22***	0.23***	
	(0.10)	(0.11)		(0.14)	(0.14)		(0.079)	(0.080)	
Mean			0.37**			0.26			0.39***
			(0.19)			(0.23)			(0.13)
Observations	262	262	262	235	235	235	229	229	229
Control Variables		✓			✓			✓	
p-value(Max=Min)	0.015	0.023		0.17	0.29		0.73	0.62	

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Forecast beliefs refer to the week after the baseline survey. Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year. We shows regressions of forecast of job outcomes on functions of the 4 previous weeks of the same job outcome. We define the minimum, the maximum and the mean for this set of four weeks. We present the p-value for a test of whether the coefficients for the maximum and the minimum variables are the same.

Table A.12: Relationship of net hourly pay with reservation wage (means)

	Net Hourly Pay	
Mean	Belief	Actual
Panel A: Full Sample		
Reservation Wage (w_R)	14.91	14.91
Net Hourly Pay (w)	15.46	12.38
$w - w_R$	.55	-2.53
Panel B: Outside Option Less Likely to be Gig		
Reservation Wage (w_R)	14.15	14.15
Net Hourly Pay (w)	14.84	12.3
$w - w_R$	.69	-1.85
Panel C: Reservation Wage is Overestimated		
Reservation Wage (w_R)	14.91	13.24
Net Hourly Pay (w)	15.46	12.38
$w - w_R$	.55	-.85
Panel D: Wage at Other or Previous Jobs		
Wage in Other Jobs (w_R)	17.76	17.76
Net Hourly Pay (w)	15.46	12.38
$w - w_R$	-2.30	-5.38
Panel E: Maximum Weekly Net Hourly Pay		
Reservation Wage (w_R)	14.91	14.91
Net Hourly Pay (w)	15.46	14.3
$w - w_R$	.55	-.61

Notes: Belief w is the pooled net hourly pay recall of the week and the month before the baseline survey. Actual w is the actual expected net hourly pay referring to the same period. Reservation wage proxy w_R is the answer to the following question: "What is the lowest acceptable hourly pay after taxes and expenses that would accept to keep working for [gig company]?". In the first two rows of each panel, we calculate the mean of w_R and w. In the final row of each panel, we calculate the difference between w and w_R. Panel A is our full sample and base measure. In Panel B, we include only workers that did not do gigs before their current gig job and that do not work for other gig companies. In Panel C, we assume w_R is measured with half as much error as w by gig workers. In Panel D, we use the worker's gross hourly wages in either a previous job or in another current job. This measure is elicited in 5-dollar bins, for which we take the midpoint. In Panel E, we assume the actual w is the maximum weekly average of the net hourly pay in the past 4 weeks.

A.2 Additional Figures

Figure A.1: Baseline survey: Recall of job outcomes

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We will now ask you questions about your pay and amount of work you do for **DoorDash**.

Please answer to the best of your ability. Your answers will be aggregated with those of other participants to create insights about working for online gig platforms.

Think about the **last month** you worked for DoorDash.

Please answer the following questions considering **only DoorDash**, and do not use commas in your numerical answers.

Consider tips, bonuses, promotions and platform fees when thinking about your pay.

How many hours do you work per week?

_____ hours per week

How much do you get paid **per hour** on average, **before expenses and taxes**?

$ _____ /hour

How much do you get paid **per week**, **before expenses and taxes**?

$ _____ /week

Out of your total pay **before expenses and taxes**, what percentage comes from bonuses and tips?

_____ %

For this question, consider only the time in which you are either picking up deliveries or passengers or bringing them to their destination.

How much do you get paid **per hour actively working on gigs** on average, **before expenses and taxes**?

$ _____ /hour

How much do you spend on **gas** per week?

$ _____ /week

How much do you spend on **expenses and taxes** per week?

$ _____ /week

How much do you get paid **per hour** on average, **after expenses and taxes**?

$ _____ /hour

Figure A.2: Baseline survey: Forecast of job outcomes

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We now ask you to estimate your weekly pay driving for DoorDash for the week starting on next Monday 12:01AM (Monday, October 10, 2022) and ending on the following Sunday at 11:59PM (Sunday, October 16, 2022)

As a reward for predicting accurately, you will receive a bonus. This reward is in addition to the $10 you receive by completing this survey.

For example, **if you make exactly the weekly earnings you predict, you will get an extra $5 Amazon gift card.** For each dollar you're off, the reward will go down, with larger reductions the further you are off.

If you're off by $100, you get an extra $3 Amazon gift card. And **if you're off by $160 or more, you get no bonus reward.**

This might sound complicated, but this system has been used in other research, and is specially designed so that you maximize your reward by stating your true beliefs.

We will email you in a few weeks with more details on your performance.

If you want to know more, select this option below.

I want more details

I do not want more details

Consider tips, bonuses, promotions and platform fees when thinking about your pay.

How much do you think your total weekly pay, **before expenses and taxes**, will be?

$ ______ /week

Keep considering the work you will do for DoorDash in **the week starting on next Monday 12:01AM (Monday, October 10, 2022) and ending on the following Sunday at 11:59PM (Sunday, October 16, 2022)**

You will not be paid for your accuracy in the following questions.

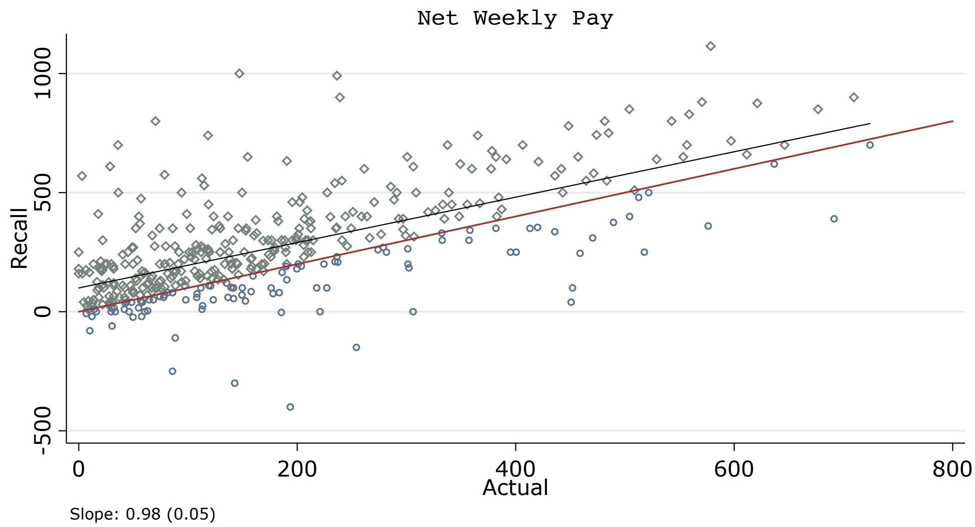
How much do you think your average pay per hour, **before expenses and taxes**, will be?

$ ______ /hour

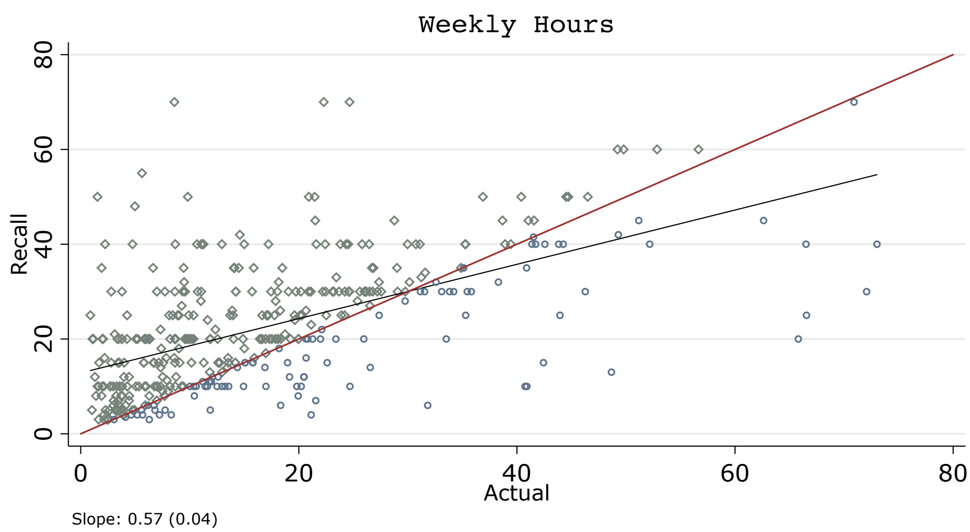
How many hours do you think you will work?

______ hours per week

Figure A.3: Scatter plots relating recalls and actual job outcomes

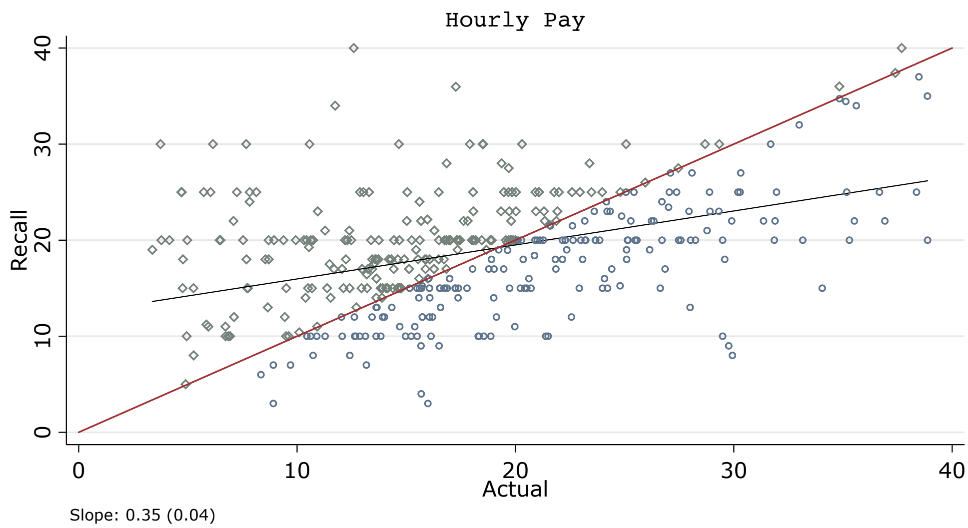


(a) Net Weekly Pay

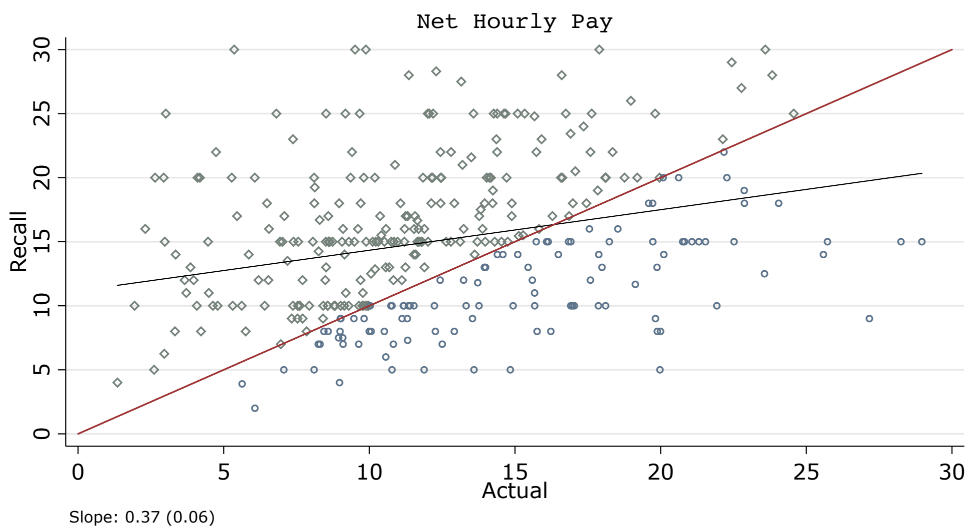


(b) Weekly Hours

Figure A.3: Scatter plots relating recalls and actual job outcomes (cont.)



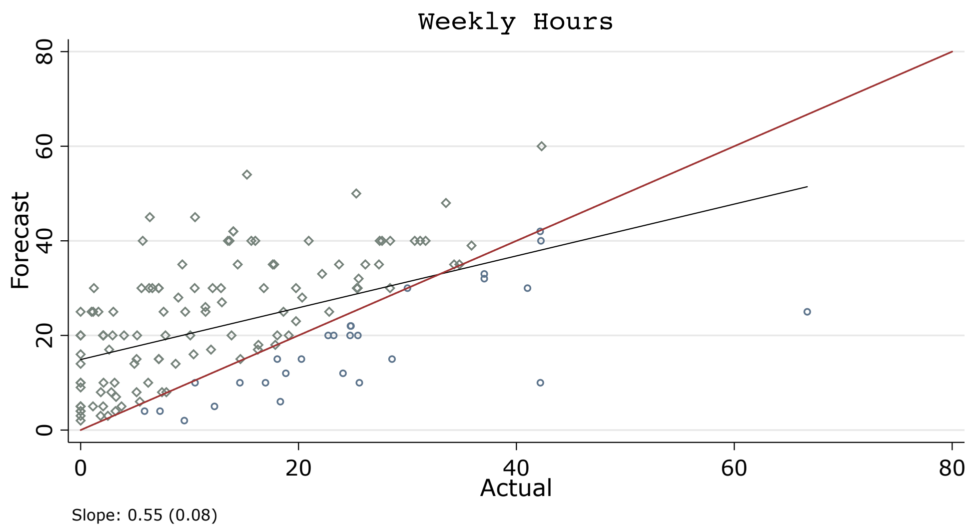
(c) Hourly Pay



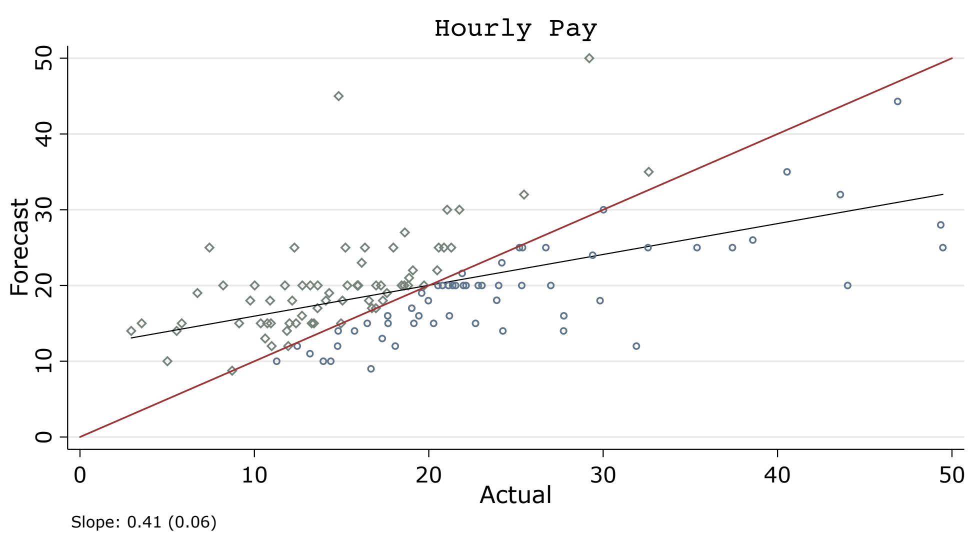
(d) Net Hourly Pay

Notes: In each graph, we draw a 45 degree line and the estimated slope of the regression of the belief on the actual the job outcome. Underneath each graph, we present the slope from the regression of the actual outcome versus its recall belief, with the associated standard error in parentheses. We exclude some outliers from each graph. Points above the 45 degree line indicate overestimation, whereas points below the 45 degree line indicate underestimation of the job outcome. We pool recalls of the week and the month before the baseline survey. The actual job outcomes pay refers to the same time period as the recalls and are obtained using information from screenshots from gig platform apps.

Figure A.4: Scatter plots relating forecasts and actual job outcomes



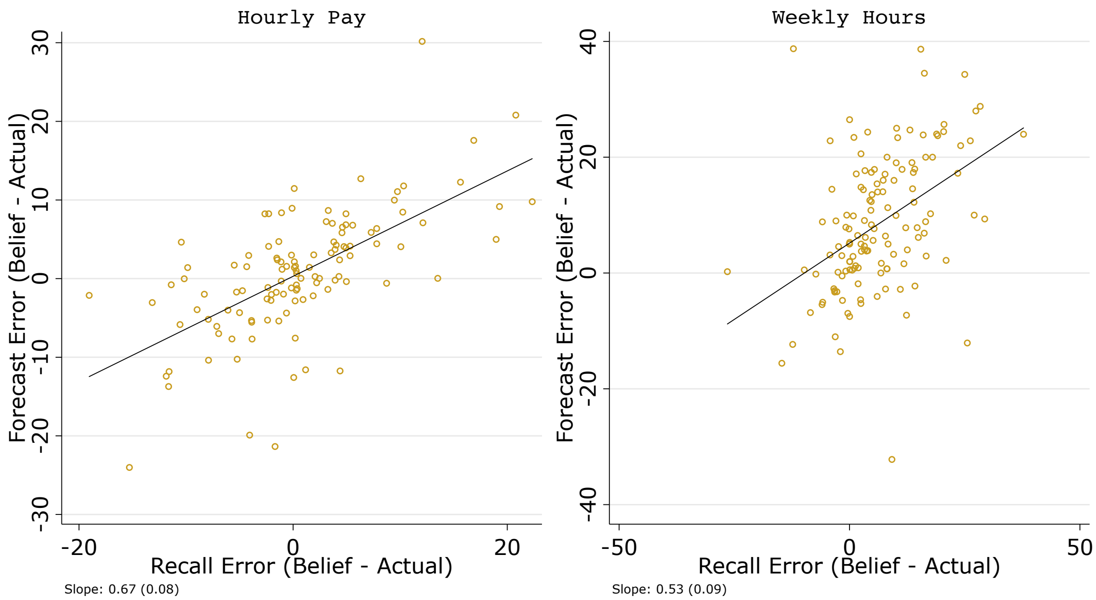
(a) Weekly Hours



(b) Hourly Pay

Notes: In each graph, we draw a 45 degree line and the estimated slope of the regression of the belief on the actual the job outcome. Underneath each graph, we present the slope from the regression of the actual outcome versus its forecast belief, with the associated standard error in parentheses. We exclude some outliers from each graph. Points above the 45 degree line indicate overestimation, whereas points below the 45 degree line indicate underestimation of the job outcome. Forecast refer to the week following the baseline survey. The actual job outcomes pay refers to the same time period as the forecasts and are obtained using information from screenshots from gig platform apps.

Figure A.5: Relationship between forecast and recall overestimation of job outcomes

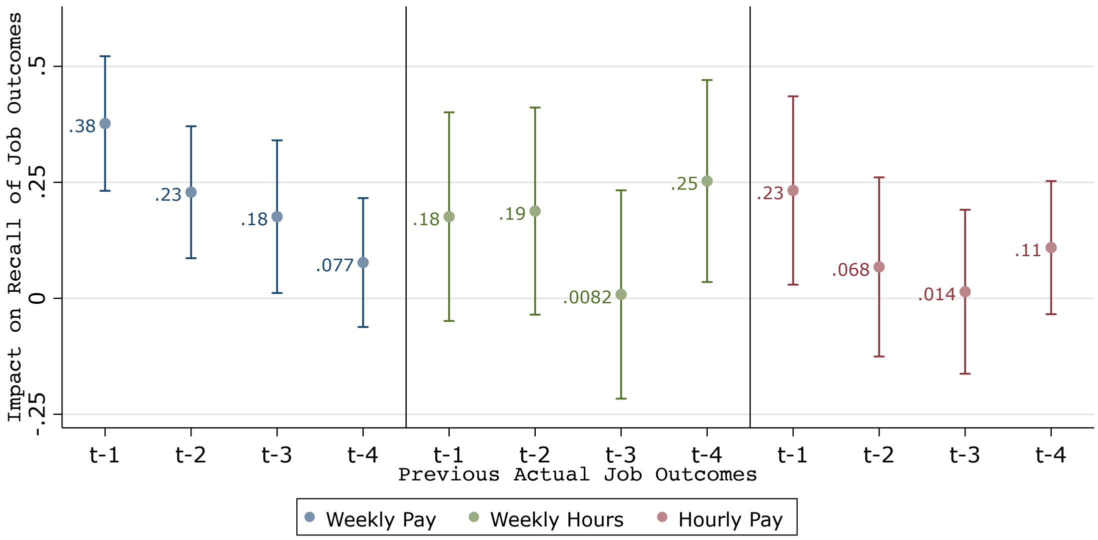


Notes: We draw the fitted line of the regression relating the forecast overestimation and the recall overestimation of job outcomes. The slope and its standard error are shown underneath. We exclude some outliers from this graph. We pool recalls of the week and the month before the baseline survey. The forecast refers to the first full week (starting on a Monday) following the baseline survey. Overestimation is measured as the recall or forecast minus the actual job outcome.

Figure A.6: Relationship between forecast and recall beliefs

Notes: In each graph, we draw the fitted line of the regression relating the recall belief and the forecast belief of a job outcome. The slope and its standard error are shown underneath each plot. We exclude some outliers from each graph. We pool recalls of the week and the month before the baseline survey. The forecast refers to the first full week (starting on a Monday) following the baseline survey.

Figure A.7: Estimated recall belief weights by recency of previous job outcomes



Notes: We plot the coefficients from regressions of recall beliefs for the past month on the last four weeks of actual realizations of the same outcome, ordered by recency, and a constant (omitted from the graph). Only recalls of the last month and workers who submitted four weeks of screenshots containing data on actual job performance are included. The notation for weeks on the x-axis ($t-1$, $t-2$, $t-3$, $t-4$) do not necessarily refer to four previous consecutive weeks, but only indicate the relative ordering of the four weeks.

Figure A.8: Categories of costs considered in expenses calculation by gig workers

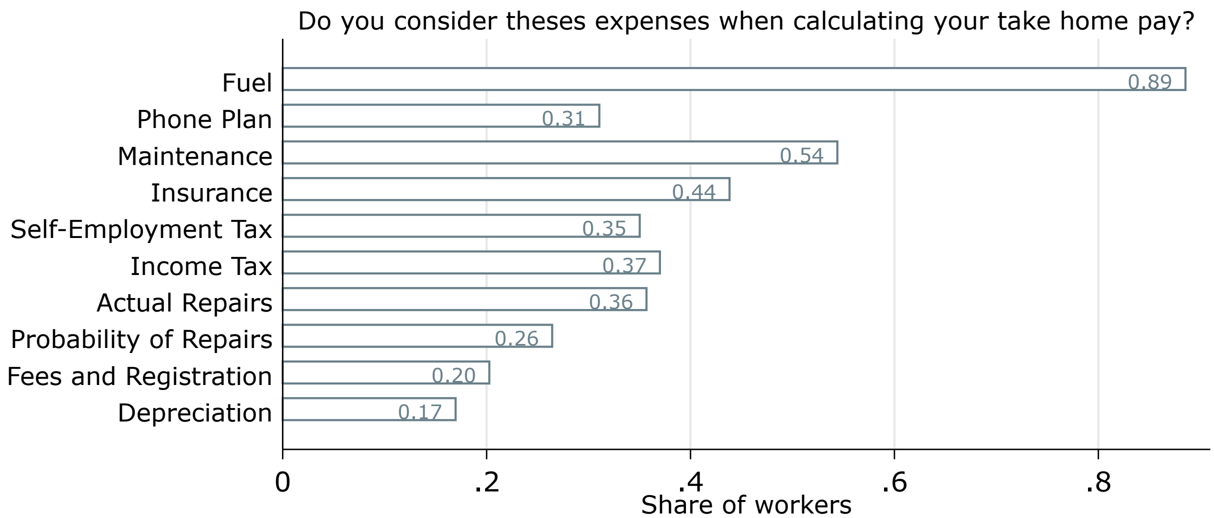
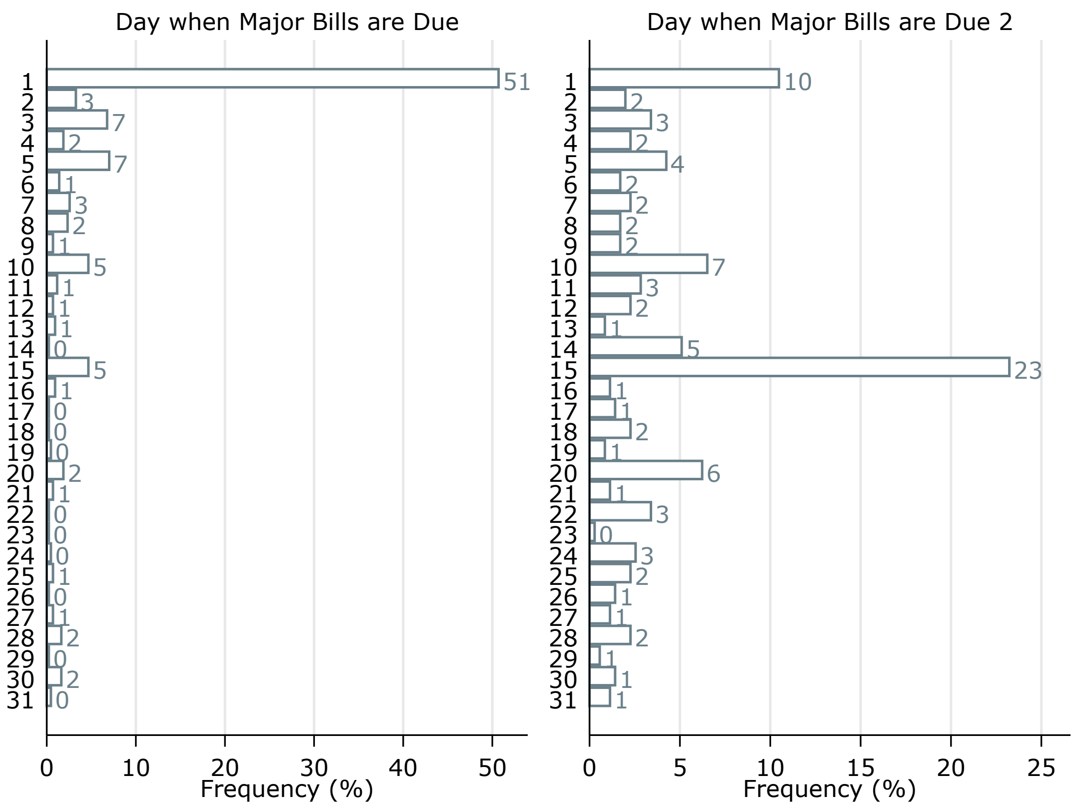


Figure A.9: Days of the month where workers pay major bills



A.3 Details of Expenses Calculation

We now provide details of our calculations of expected expenses associated with gig work, which we use to estimate the actual expected net hourly and weekly pay. Only variable costs are considered: fuel, maintenance and repair, variable depreciation, self-employment taxes, and federal income taxes. We obtain estimates of operating costs from the AAA Your Driving Costs 2022 booklet. This guide has been published since 1950 and uses a proprietary methodology to calculate the costs of owning and operating a *new* car in the United States over five years.

This source has estimates of per mile operating costs for 9 different car categories: small sedan, medium sedan, subcompact SUV, compact SUV, medium SUV, midsize pickup, half-ton pickup truck, hybrid, and electric. We use expected maintenance and repair costs from this guide. We only use its estimates of fuel costs for hybrid and electric cars. For other categories, we average the AAA's gas price for the three months before the baseline survey. This is combined with information on fuel efficiency (miles per gallon) taken from the guide. Only the variable part of depreciation, resulting from driving additional miles, is considered. We calculate this cost by taking the increase, estimated in the guide, in total depreciation costs from driving 15,000 to 20,000 miles and dividing that by 5 thousand. In this way, we obtain an estimate of variable depreciation per mile for each car category.

To account for variation in costs with car age, we adjust the reported costs in the Your Driving Costs guide based on information from CarEdge. We use three car age groups: between 0 and 5 years, 6 and 10 years, and 10 years and above. We then calculate how (i) maintenance and repair costs and (ii) variable depreciation costs increase with car age. For each car category, we take an average of this variation for the top five car models.[1] On average across car categories, and relative to the costs for a car that is between 0 and 5 years old, depreciation costs are estimated to be 84% (43%) of the initial ones from years 6 to 10 (after year 10). According to our estimates, repair and maintenance costs for cars between 6 and 10 years old are, on average, 2.75 times higher than those for cars from 0 to 5 years old. This number is 3.76 for cars older than 10 years. We then multiply these factors by the estimates of depreciation and maintenance and repair from the AAA guide. Fuel expenses are assumed to be the same regardless of the age of the car.

We assume workers drive, on average, for 10 miles a hour. This allows us to turn per mile costs from the Your Driving Costs guide into per hour costs. We calculate self-employment taxes by first subtracting gross earnings per hour by the IRS mileage rate

[1] For each respective car category, these are: small sedan (Honda Civic, Hyundai Elantra, Nissan Sentra, Toyota Corolla, Volkswagen Jetta), medium sedan (Chevrolet Malibu, Honda Accord, Hyundai Sonata, Nissan Altima, Toyota Camry), subcompact SUV (Chevrolet Trax, Honda HR-V, Hyundai Kona, Jeep Compass, Subaru Crosstrek), compact SUV (Chevrolet Equinox, Ford Escape, Honda CR-V, Nissan Rogue, Toyota RAV4), medium SUV (Chevrolet Traverse, Ford Explorer, Subaru Outback, Jeep Grand Cherokee, Toyota Highlander), midsize pickup (Chevrolet Colorado, Ford Ranger, Honda Ridgeline, Jeep Gladiator, Toyota Tacoma), half-ton pickup truck (Chevrolet Silverado, Ford F-150, Nissan Titan, Ram 1500, Toyota Tundra), hybrid vehicle (Ford Explorer, Honda CR-V, Hyundai Ioniq, Toyota Prius Liftback, Toyota RAV4) and electric car (BMW i3, Chevrolet Bolt, Hyundai Kona Electric, Nissan Leaf, Tesla Model 3)

deduction (\$.585/mile x 10 miles) and, following IRS procedure, multiplying the result by 0.925 and then by 15.3%.

Income taxes per hour are estimated by using the reported yearly household income, calculating how much of that income comes from gig work and then applying the standard deduction for 2022 for single filers. We assume gig workers work in half of the weeks of the year and make their average weekly earnings in each of these weeks. We divide the result by an estimate of total miles driven per year, still assuming that 10 miles are driven per hour. In our calculations, income taxes are, on average, 1% of total expected costs.

To determine how much of the driver's total pay is going to expected expenses, we subtract fuel, maintenance and repairs, depreciation, self-employment tax, and income taxes from their gross earnings per hour. Using this method, we calculate our post-expenses and taxes earnings per hour and convert it into a share of total gross pay. Appendix Table A.13 provides an example of how this calculation is done, considering a driver of a 2022 Honda Accord who earns \$20/hour and works 20 hours per week. Using this estimated expenses share, we calculate the actual expected net hourly and weekly pay. We do not calculate expected expenses for workers who rent a car to do gig work or who use a bike or scooter.

Table A.13: Example of expenses calculation

Category	Calculation	Value
Gross Earnings (1)		\$20/hour
Fuel (2)		-\$1.57/hour
Maintenance and Repair (3)		-\$1.04/hour
Variable Depreciation (4)		-\$0.52/hour
Pre-Tax Net Earnings (5)	(1) - (2) - (3) - (4)	\$16.86/hour
Expenses Deductions (6)		- \$5.85/hour
Self-Employment Tax (7)	[(1) - (6)]*0.925*15.3%	-\$2.00/hour
Federal Income Tax (8)		-\$0.19/hour
Post-Tax Net Earnings (9)	(5) - (7) - (8)	\$14.66/hour
Share of Expenses (10)	[(1) - (9)] / (1)	26.6%

Notes: Example of calculation used to estimate the expected expenses share out of total pay. We consider a driver of a 2022 Honda Accord who makes \$20/hour, works 20 hours a week and drives, on average, for 10 miles per hour. Our cost measures for maintenance, repair and variable depreciation are based on the AAA Your Driving Costs 2022 guide. Fuel costs are the average of gas prices from AAA in the three months before the baseline survey. We adjust for variation of maintenance, repair and depreciation costs over a car's lifespan by using information from CarEdge. We apply the IRS mileage rate deduction in 2022 (\$.585/mile) to calculate self-employment taxes and we estimate federal income taxes by combining reported yearly household income, an estimate of gig income over the year and by applying the standard deduction for 2022.

A.4 Beliefs about Other Workers

One important aspect of optimistic beliefs is overplacement (Healy and Moore, 2007): the belief that one is more skilled relative to others than one actually is. To analyze this phenomenon in our setting, we ask, for a sub-sample, beliefs about job outcomes for other workers. In particular, for half of the individuals in the control group of the information treatment (or one quarter of the full sample), we elicit beliefs about job outcomes for the average worker for the same gig company in the baseline survey.[2] See Appendix Figure A.10 for an example of the questions we ask.

Our first set of results are shown in Appendix Figure A.11. We compare, divided by job outcome, the average (i) actual outcome, (ii) recall belief for oneself and (iii) recall belief for the average worker of the same gig company. We find that workers believe the average worker works longer hours and has a higher weekly pay. This effect is very large, and implies a belief of the average worker working full-time for the gig company, which is not correct for our sample. In addition, workers believe the average worker has a slightly higher gross hourly pay and a similar net hourly pay to themselves.

We then subtract each worker's belief for other workers from the recall belief about their own job performance. We next calculate leave-out means of the actual job outcomes. Then, we subtract each worker's actual job outcome by this leave-out mean. Finally, we compare these two differences to test for overplacement, which exists if workers believe their job outcome is higher relative to others than it actually is.

Results are shown in Appendix Table A.14. We find no evidence of overplacement on hourly pay and find evidence of underplacement for weekly hours. Finally, we ask workers whether they believe other workers in the same gig company misunderstand their pay: 72% of workers believe it's likely others overestimate pay, versus 43% for others underestimating pay.

[2] These beliefs are elicited for everyone in the midline and endline surveys.

Figure A.10: Questions on beliefs about the average gig worker

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For the next four questions, consider **the group of people who worked for DoorDash in the last month** and who will reply to this survey.

Please answer the following questions considering only the work they do for **DoorDash**, and do not use commas in your numerical answers.

Consider tips, bonuses, promotions and platform fees when thinking about pay.

How much do you think **this group** gets paid **per week**, **before expenses and taxes**?

$ ______ /week

How much do you think **this group** gets paid **per hour** on average, **after expenses and taxes**?

$ ______ /hour

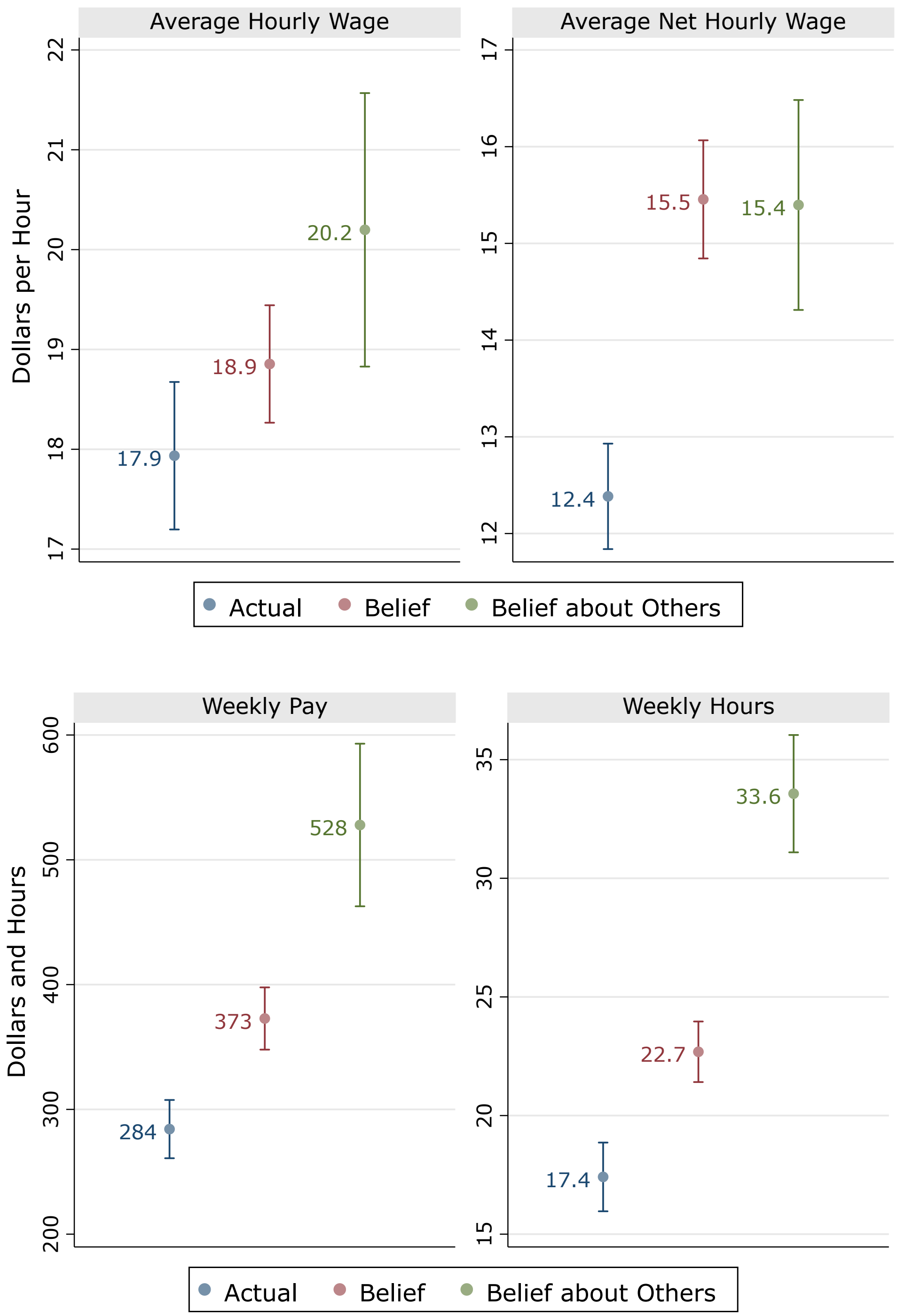
How many hours do you think **this group** works per week on average?

______ hours per week

How much do you think **this group** gets paid **per hour** on average, **before expenses and taxes**?

$ ______ /hour

Figure A.11: Comparison of actual job outcomes with self beliefs and beliefs about others



Notes: We plot the mean and the 95% confidence intervals of three measures pooled for the week and the month before the baseline survey: the actual job outcome, the recall belief of the job outcome for oneself and the belief of the job outcome for the average gig worker at the same company.

Table A.14: Beliefs of job outcomes for self versus other workers

Pay Relative to Others (Belief)	**Summary Statistics**					
	Mean	P25	Median	P75	Std. Dev.	N
Hourly Wage	-1.34	-4.75	0.00	2.00	7.07	108
Net Hourly Wage	0.21	-2.00	0.00	3.00	4.91	99
Weekly Pay	-184.91	-300.00	-150.00	0.00	289.76	110
Weekly Hours	-11.92	-20.00	-10.00	-3.00	14.89	113

(a) Belief

Pay Relative to Others (Belief - Actual)	**Summary Statistics**					
	Mean	P25	Median	P75	Std. Dev.	N
Hourly Wage	-1.52	-7.23	-0.73	4.12	9.85	98
Net Hourly Wage	0.27	-2.72	0.95	4.47	6.47	77
Weekly Pay	-147.35	-292.57	-108.03	65.94	341.45	108
Weekly Hours	-9.47	-17.69	-9.43	-0.04	14.13	99

(b) Belief - Actual

Notes: We present summary statistics of variables comparing outcomes for oneself and for other workers. Recalls are pooled for the week and the month before the baseline survey. In Panel (A), we compare recall beliefs for oneself versus for the average worker. A negative value means that a workers believes the job outcomes for the average worker is higher than for themselves. On Panel (B), we do a double difference: we subtract the recall comparison of self and others from Panel (A) with the difference of actual job outcomes for oneself minus the leave-out mean of the same outcome for other workers.

A.5 Additional Robustness Checks

We now provide some robustness checks to how we measure the job performance of gig workers in our sample. Because gross weekly pay is salient and is reported to workers with bonuses, tips and fees included, we do not believe it is subject to credible concerns on this front. In contrast, measuring work hours is not as straightforward. We measure work hours as *online* hours, which is the total time a gig worker has the platform app turned on and is available for gigs. We believe his measure captures the nature of a standard job in which only part of the time is spent actively working.

Yet, gig workers may believe active hours, which includes only the time spent actively working on gigs, is the correct measure of labor supply. In this case, there will be a discrepancy between our definition and theirs. We will *underestimate* the overestimation of weekly hours but overstate the overestimation of hourly pay, as labor supply appears in the denominator of this variable.

To empirically access how much the definition of work hours matters, we provide a robustness check in Appendix Table A.15. Our regressions include binary variables for behaviors that might lead to inaccurate measures of actual work hours, such (i) multi-tapping, or having more than one gig platform app active at the same time; (ii) being online on the app but with no intent to actually do gigs; (iii) not considering the time spent waiting for gigs as work or (iv) thinking commuting is work. We find that overestimation of recalls of job outcomes is robust to excluding gig workers who report doing any of these behaviors. In other words, our overestimation results are not the result of a mismatch between ours' and the gig workers' definition of work hours.

Our measure of actual expenses is based on expected costs calculated at the car category and age level. This measure is therefore less reliable than job outcomes derived directly from screenshots. We now detail a few ways in which we may be underestimating the expenses involved in gig work. First, our calculations ignore fixed costs such as insurance, registration fees, and non-variable depreciation (that is, depreciation not due to driving more miles). This means we likely underestimate costs for drivers who buy cars primarily for doing gig work. In addition, we ignore state income taxes and assume drivers can deduct miles driven when calculating self-employment taxes. This deduction is known to reduce self-employment taxes. Yet, this deduction is not available to 47% of gig drivers in our sample, since they report not recording their miles.

Table A.15: Robustness checks for overestimation considering definition of work hours

	Overestimation (Belief - Actual)				
	(1)	(2)	(3)	(4)	(5)
	Weekly Pay	Net Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Online but Not Working	28.8	23.6	3.97***	-0.24	-0.65
	(19.1)	(19.8)	(1.46)	(0.94)	(0.92)
Multiple Apps	56.5***	32.3*	1.31	3.20***	2.41***
	(16.1)	(16.6)	(1.26)	(0.79)	(0.78)
Considers Wait Not Work	-10.9	9.50	-0.15	0.076	0.65
	(16.2)	(16.6)	(1.24)	(0.79)	(0.78)
Considers Commute Work	-0.014	-5.23	-0.19	-0.12	0.64
	(15.8)	(16.3)	(1.21)	(0.77)	(0.76)
Excluded Group	70.6***	77.0***	4.79***	-0.17	1.51**
	(12.9)	(13.1)	(0.99)	(0.63)	(0.62)
Observations	439	400	397	393	345

Notes: We regress overestimation of job outcomes recalls against covariables. Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Overestimation of each outcome can be negative and is defined as the recall belief minus the actual job outcomes for the same time period. We pool recalls of the week and the month before the baseline survey. *Online but Not Working* is a binary variable equal to 1 if a worker, either all the time or frequently, is online in the gig platform app with no intention of accepting gigs. *Multiple Apps* is a binary variable equal to 1 if a worker, either all the time or frequently, is online in more than one gig platform app at the same time. *Considers Wait Not Work* is equal to 1 if a worker considers little or none of the time spent waiting for rides as work hours and 0 otherwise. *Considers Commute Work* is equal to 1 if a worker considers all or most of the time spent commuting before and after a shift as work hours and 0 otherwise.

A.6 Details of the Information Treatment

We now detail our randomized information treatment. Our treatment is inspired by previous work aiming to change beliefs and behaviors through de-biasing interventions (Cullen and Perez-Truglia, 2022; Card et al., 2012; Bottan and Perez-Truglia, 2020). In the baseline survey, workers are randomly assigned to the treatment group with a 50% probability.[3] In that case, workers are required to manually input the weekly pay and weekly hours information from all screenshots they submit. This was done so we could provide feedback on their beliefs during the baseline survey. This also has the added effect of forcing them to face the information contained in the screenshots.

Then, on a single page, we explain the following: (i) how we calculate their gross hourly pay and its value; (ii) show how we calculate their expected expenses share, given their car; (iii) calculate their actual net hourly pay based on this information; (iv) compare the actual net hourly pay with their recall, informing them if they are under or overestimating it. We provide gig workers with an informative signal. Despite this, the signal is not fully accurate at the individual level due to noise in outcomes and in measuring expenses.

The next page informs them that gig workers in our sample often overestimate their job outcomes. We then provide them with a brief explanation of the concept of overconfidence. We find that 72% of gig workers say they plan to use in practice the information we provide, and that 81% say they agree partially or entirely with the information presented to them. Appendix Figure A.12 shows an example of our information treatment. During the midline survey, we give the treatment group the same information pages presented at baseline. The purpose of this is to reinforce the information initially presented to them. Control group gig workers receive the same information, but only at the end of the endline survey.

If a worker does not use a car in his gig work, or if he rents a car to do gigs, we provide an alternative version of the information treatment, as we believe our measure of expected expenses would then be inadequate. In particular, we show these workers similar information, but concerning their gross weekly pay. This happens for less than 5% of our sample. This group is excluded from all results regarding the information treatment.

Estimation Strategy

Our treatment will have heterogeneous effects depending on which direction we correct gig workers' beliefs. In other words, treatment effects should differ based on whether we tell gig workers they overestimate (*Bad Signal*) or underestimate (*Good Signal*) their net hourly pay. As a result, we estimate treatment effects separately based on whether the initial overestimation of net hourly pay is positive or negative. Our main specification is:

$$y_{it} = \beta_0 + \beta_1 Over_i + \beta_2 Bad\ Signal_i + \beta_3 Good\ Signal_i + X_{i0}\Gamma + \varepsilon_{it} \tag{A.1}$$

where y_{it} are belief or labor market outcomes for individual i at period t. Period t a post-information treatment period. $Over_i = 1$ if initial overestimation of net hourly pay is

[3] Appendix Table A.16 shows a randomizing balancing test.

positive and 0 if it is negative. Our two variables of interest here are $Bad\ Signal_i$ and $Good\ Signal_i$: $Bad\ Signal_i$ is equal to 1 if an individual is in the treatment group and $Over_i = 1$; $Good\ Signal_i$ if an individual is in the treated group and $Over_i = 0$. Finally, X_{i0} is the covariates matrix, composed of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income at most \$40k/year. In addition, it includes the pre-treatment outcome y. The variable ε_{it} is the regression error. We use robust standard errors in all of our regressions.

In the above specification, the treatment effect is identified by comparing all workers who overestimate (underestimate) their net pay in the treatment group with all workers who overestimate (underestimate) it in the control group. By doing so, we ignore the magnitude of the mistakes gig workers make. In order to take that into account, we use the following alternative specification:

$$y_{it} = \beta_0 + \beta_1 Mis_i + \beta_2 Treat_i + \beta_3 Treat_i \cdot Mis_i + X_{i0}\Gamma + \varepsilon_{it} \tag{A.2}$$

where $Treat_i$ is the treatment binary variable, equal to 1 if an individual is in the treatment group, Mis_i is the initial overestimation (recall belief minus actual) in net hourly pay. In equation (8), β_2 identifies the intercept of the treatment effect, while β_3 identifies its slope: it measures how the treatment effect varies depending on the value of the initial overestimation of net hourly pay.

Effect on Beliefs

We now analyze the effects of our randomized information treatment on beliefs about gig job outcomes. After the information treatment, all of the beliefs we elicit about job outcomes are recalls and concern periods following the treatment. Thus, effects on beliefs are also dependent on how job market outcomes change as a result of our treatment. For instance, our treatment may affect a worker's choice of hours. As a result, actual hourly pay can change in ways that should alter beliefs about net hourly pay. Many settings for information treatments, in contrast, do not allow the subject to influence the actual outcome after knowing its value. The following results should be analyzed with this in mind.

After the information treatment, workers in the treatment group can review their incentivized forecast of weekly pay for the next week. We expect workers who are told they overestimate their net hourly pay to lower their forecast. This is because they might decide to work fewer hours in the future and can also lower their belief of their hourly pay. The opposite should happen for workers initially underestimating their pay.

Appendix Table A.17 confirms this. In column (1), we regress the change in weekly pay forecast against the initial net hourly pay overestimation. On average, workers who review their pay forecast decrease it by \$10. In addition, a raise in initial overestimation by \$1/hour is associated with a decrease in the forecast of around \$1.6, which is significant at 10%. Using column (2), we test whether workers are more likely to review their forecasts when told they are making larger mistakes (in absolute value). It appears that this is the case.

In Appendix Table A.18, we show the average overestimation of job outcomes at our midline and endline surveys, separately by the treatment and the control groups. Compared with the control group, the treatment group overestimates recall of job outcomes less after one week at the midline (one week recall) and after two to five months at the endline (one month recall). It is worth noting that our midline survey has a lower level of misestimation. We believe this is due to us (unlike in the baseline survey) requesting a recall of a specific week, explicitly stating the beginning and the end dates. Thus, it is likely that a share of workers looked at their actual job outcomes in that week and input them. We do not observe this pattern at the endline survey, where we elicit one month recalls.

We also examine how the information treatment affects beliefs when the initial level of overestimation is taken into account. Results are shown in Appendix Table A.19 The first finding is that workers' recall beliefs for the expenses share rise when they initially underestimate net hourly pay (and the opposite when they overestimate). Our treatment does not seem to affect recall beliefs of net hourly pay in the expected way. This might partially be due to changes in behaviors (in response to our intervention) happening at the same time as our information provision. Alternatively, our information treatment might change beliefs immediately, but this effect may fade away over time.

On the middle two columns of both panels, we find some evidence that belief for the *average worker*'s net hourly pay does react in the expected way: increasing for those underestimating and decreasing for those told they are overestimating. This is however, not statistically significant. Possibly, beliefs about others are less affected by labor market decisions and are more easily changeable. Appendix Table A.20 presents an alternative specification for measuring effects on beliefs. We use a continuous measure of net hourly pay overestimation. Our findings are similar.

Effects on Labor Market Outcomes

We discussed the main effects of our information treatment on job market outcomes in Section 5.2 and in Table 1.7. In Appendix Table A.21, we estimate an alternative specification for our analysis of effects of labor supply and other jobs. In particular, we use a continuous measure of initial net hourly pay overestimation. Following the predictions of our model, we estimate effects on labor supply separately by financial need, using information on whether the worker's household is struggling financially.[4] We find similar effects to those reported in Table 1.7: labor supply is affected more by the information treatment when there is less financial need.

[4] Struggling financially is defined as reporting to be receiving calls from collectors, contemplating bankruptcy, or struggling to pay the bills.

Figure A.12: Example of information treatment

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We now compare your perceptions of pay to the information in the screenshots you submitted.

Please read carefully, as you might find the following information valuable to your decision making.

Earlier in the survey, you said that you believe that your average pay per hour, *after* expenses and taxes, is $18. We will now try to estimate what is your actual post-expenses hourly pay.

According to information from the AAA Driving Costs brochure and our calculations, an average driver of a Midsize pickup from 2019 needs to **subtract around 29% of their earnings to account for expenses and taxes, *such as fuel, car maintenance and repair, depreciation, self-employment and income taxes.***

If you wish to see further details on how this number was reached, select this option below.

I want more details

I do not want more details

Consider the following example for a part-time driver for DoorDash who makes $20/hour, works 20 hours a week and drives, on average, for 10 miles per hour in a Midsize pickup from 2019.

We base this calculation on *expected costs* over a year, taken from the AAA Driving Costs brochure. For instance, mechanical problems are unpredictable but will occur at some point. Using *expected* costs takes into account this possibility and how likely it is to happen. We only include variable costs, and so we assume that a car was already available to do gigs.

If a car was bought expressively to do gigs, these expenses can be considerably higher.

Gross Earnings: $20/hour
Fuel: -$2.35/hour
Maintenance and Repair: -$0.99/hour
Depreciation (Drop in Resale Value): -$0.85/hour
Pre-Tax Net Earnings: $20/hour - $2.35/hour - $0.99/hour - $0.85/hour = $15.81/hour
Self-Employment Tax: -$2.00/hour
Federal Income Tax: -$0.19/hour
Post-Tax Net Earnings: $15.81 - $2.00-$0.19 = $13.62/hour
Share of Expenses: ($20-$13.62)/$20 = **28.8%**

The calculations are an approximation and will likely not perfectly describe anyone's condition. Click on the link below for more information and sources.

Figure A.12: Example of information treatment (Cont.)

In the **screenshots you submitted for last week**, you worked approximately **9.2 hours** for DoorDash. Your total pay in this time were **$155.12**.

Assuming you were working for DoorDash the whole time you were online on the app, your **actual** average pay per hour, *before* expenses and taxes, is found by dividing your total pay by the time you worked during these weeks. This is equal to **$16.86 per hour**.

Using the expenses share of 28.8%, we can estimate that your **actual** average take home pay (or hourly pay *after* expenses and taxes) to be around **$12**.

This is 33% lower than your assessment ($18).

This might indicate that you are *overestimating your take home pay.*

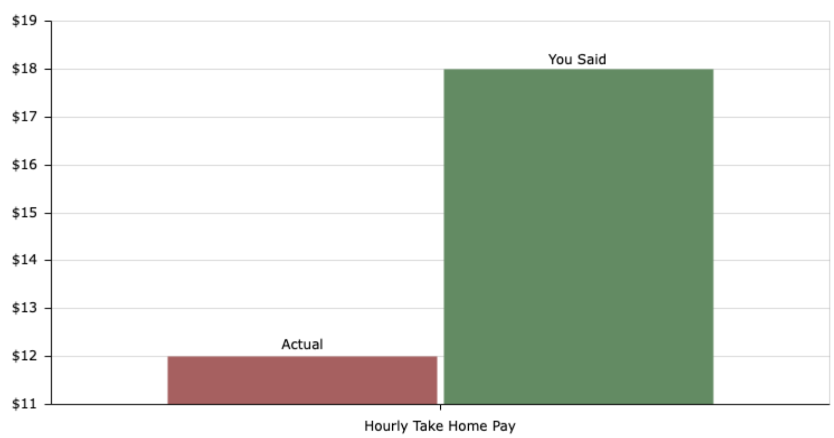

Figure A.12: Example of information treatment (Cont.)

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We will now to share some of the preliminary results we have obtained from our study.

Following similar calculations as the ones we just presented, we found that previous respondents to our survey have, on average, **overestimated** their pay and **underestimated** their expenses.

According to our data, drivers, on average, overestimate their average hourly pay by **8.3%**, their average hourly pay after expenses by **23.8%** and their weekly pay by **23.9%.**

While this might seem weird, as it can imply that drivers do not always have a good understanding of how much money they make on gig economy jobs, **we believe they can be explained by the psychological concept of *overconfidence*.**

Overconfidence means that, in many situations, people have a tendency to be too optimistic and too certain about important aspects of their lives. For instance, three-quarters of US drivers consider themselves better-than-average drivers.

These concepts matter more in situations where understanding something is a hard task. In our view, that is exactly the case for calculating pay in gig economy jobs, as pay tends to vary a lot depending on time of day, expenses and market conditions each day.

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Recall that we asked you to predict your weekly pay driving for DoorDash for the week starting on next Monday 12:01AM (Monday, October 10, 2022) and ending on the following Sunday at 11:59PM (Sunday, October 16, 2022). As a reward for predicting accurately, you will receive a bonus.

Before, you predicted a total weekly pay equal to $ **before expenses and taxes**.

If you want to change your prediction, please write it down in the box below.
$ /week

Table A.16: Balancing table for randomized information treatment

Baseline Survey	Full Sample			Control		Treatment		
	N	Mean	SD	Mean	SD	Mean	SD	**Diff.**
Age 18-34	454	42.07	49.42	41.95	49.45	42.20	49.50	0.3
Age 35-54	454	47.14	49.97	48.73	50.09	45.41	49.90	-3.3
White	454	72.69	44.61	75.42	43.15	69.72	46.05	-5.7
Male	454	41.19	49.27	40.25	49.15	42.20	49.50	1.9
College Degree	454	38.55	48.72	41.10	49.31	35.78	48.05	-5.3
HHold Income $0-40$k	454	54.19	49.88	54.66	49.89	53.67	49.98	-1.0
No Household Budget	454	21.59	41.19	18.22	38.68	25.23	43.53	7.0*
Struggling Financially	454	42.73	49.52	41.53	49.38	44.04	49.76	2.5
Gig Pay is Essential	454	83.48	37.18	83.47	37.22	83.49	37.22	0.0
Has Other Gig Job	454	35.68	47.96	36.44	48.23	34.86	47.76	-1.6
Has Non-Gig Job	454	17.18	37.76	17.80	38.33	16.51	37.22	-1.3
Employed Full-Time Prior to Gig	454	36.34	48.15	38.98	48.87	33.49	47.30	-5.5
Employed Part-Time Prior to Gig	454	20.26	40.24	18.64	39.03	22.02	41.53	3.4
Unemployed Prior to Gig	454	10.13	30.21	8.47	27.91	11.93	32.48	3.5
Experience Delivery (12+ mo.)	454	57.49	49.49	56.78	49.64	58.26	49.43	1.5
Experience Rideshare (12+ mo.)	454	23.57	42.49	23.31	42.37	23.85	42.72	0.5

(a) Covariates

Baseline Survey	Full Sample			Control		Treatment		
	N	Mean	SD	Mean	SD	Mean	SD	**Diff.**
Hourly Pay	400	17.94	7.52	18.36	7.24	17.47	7.80	-0.9
Hourly Net Pay	360	12.38	5.26	12.50	4.85	12.27	5.65	-0.2
Weekly Pay	445	284.20	250.44	261.02	217.48	308.76	279.61	47.7**
Weekly Hours	404	17.41	14.81	16.70	13.95	18.17	15.66	1.5
Expenses Share	408	32.19	5.19	32.42	5.17	31.95	5.21	-0.5
Hourly Pay (Recall)	436	18.85	6.25	18.98	6.31	18.72	6.19	-0.3
Net Hourly Pay (Recall)	412	15.46	6.31	15.73	6.21	15.15	6.42	-0.6
Weekly Pay (Recall)	439	372.82	265.80	371.20	259.91	374.57	272.62	3.4
Weekly Hours (Recall)	440	22.69	13.63	22.68	13.57	22.69	13.73	0.0
Hourly Pay (Forecast)	324	19.36	6.80	18.87	6.32	19.92	7.31	1.1
Weekly Pay (Forecast)	344	376.79	324.95	354.20	313.60	402.77	336.65	48.6
Weekly Hours (Forecast)	333	23.24	14.05	22.19	13.55	24.47	14.56	2.3

(b) Outcomes

Notes: Data in this table is collected in our baseline survey. We first show the full sample. We then divide it into the control and the treatment groups for our information treatment. The treatment group receives detailed information on whether they overestimate or underestimate their net hourly pay. The last column shows the difference in means between the control and the treatment groups for each variable. Stars are used to denote the statistical significance of this difference (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table A.17: Effect of information treatment on reviewing incentivized forecast of weekly pay

	Reviews Weekly Pay Forecast	
	(1)	(2)
	Change in Forecast	Has Reviewed Forecast
Overestimation Net Hourly Pay	-1.60*	
	(0.83)	
Abs(Overestimation Net Hourly Pay)		0.014*
		(0.0075)
Constant	-10.9*	0.17***
	(6.06)	(0.057)
Observations	147	149

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Only individuals in the information treatment group are included in the regressions, as they were the only ones allowed to review their forecast of weekly pay for following full week. *Overestimation Net Hourly Pay* is the overestimation in net hourly pay, which is defined as the recall belief minus the actual net hourly pay. Recall of net hourly pay is pooled for the week and the month before the baseline survey, with the actual job outcomes referring to the same time period for each worker. *Abs(Overestimation Net Hourly Pay)* is the absolute value of the overestimation in net hourly pay. *Change in Pay Forecast* measures the within-individual difference in the weekly pay forecast before and after receiving the information treatment. *Has Reviewed Pay Forecast* is a binary variable equal to 1 if the individual has chosen to review their weekly pay forecast immediately after the information treatment.

Table A.18: Effect of information treatment on overestimation of job outcomes

	Overestimation (Recall - Actual)			
	(1)	(2)	(3)	(4)
	Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Treatment	-4.66	0.21	0.35	2.21***
	(13.4)	(0.91)	(0.84)	(0.67)
Control	17.0	1.62*	2.12***	2.39***
	(12.2)	(0.83)	(0.74)	(0.64)
Observations	172	162	161	131

(a) Two Weeks After Treatment

	Overestimation (Recall - Actual)			
	(1)	(2)	(3)	(4)
	Weekly Pay	Weekly Hours	Hourly Pay	Net Hourly Pay
Treatment	43.7**	3.64***	-1.07	1.62**
	(17.6)	(1.13)	(0.77)	(0.69)
Control	63.3***	4.27***	0.029	2.05***
	(16.4)	(1.06)	(0.70)	(0.66)
Observations	159	152	147	126

(b) Two to Five Months After Treatment

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We regress the overestimation of recalls of job outcomes on two binary variables: one for the treatment group and one for the control group in the information treatment. We omit the constant term of these regressions. *Overestimation* is the recall belief minus the actual job outcome. Thus, positive (negative) overestimation implies overestimation (underestimation) of a job outcome. The recalls are for the last week in Panel (A) – measured in our midline survey – and for the last month in Panel (B), measured in our endline survey.

Table A.19: Effect of information treatment on beliefs of job outcomes

	Belief Net Hourly Pay		Belief Other Net Hourly Pay		Belief Expenses Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Good Signal	2.41	1.07	2.91	2.37	-9.40**	-9.76*
	(1.90)	(1.91)	(1.90)	(1.79)	(4.72)	(4.94)
Bad Signal	0.072	0.81	-1.31	-1.31	4.45	3.84
	(1.30)	(1.26)	(1.16)	(1.24)	(3.14)	(3.53)
Observations	136	136	141	141	145	145
Baseline Outcome		✓		✓		✓
Demographic Controls		✓		✓		✓
p-value(Treatment No Effect)	0.45	0.69	0.17	0.23	0.054	0.080

(a) Two Weeks After Treatment

	Belief Net Hourly Pay		Belief Other Net Hourly Pay		Belief Expenses Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Good Signal	2.22	1.23	-0.68	-0.95	-2.05	0.74
	(1.72)	(1.58)	(1.37)	(1.33)	(4.11)	(4.53)
Bad Signal	1.23	1.85*	-0.77	-0.85	-4.57	-5.54**
	(1.20)	(1.06)	(0.82)	(0.88)	(2.83)	(2.73)
Observations	129	128	132	132	136	136
Baseline Outcome		✓		✓		✓
Demographic Controls		✓		✓		✓
p-value(Treatment No Effect)	0.26	0.17	0.57	0.49	0.24	0.13

(b) Two to Five Months After Treatment

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We estimate versions of $y_{it} = \beta_0 + \beta_1 Over_i + \beta_2 BadSignal_i + \beta_3 Good\,Signal_i + X_{i0}\Gamma + \varepsilon_{it}$, where y_{it} is the outcome variable for individual i at period t. $Over_i = 1$ if initial overestimation of net hourly pay is positive and 0 otherwise. Our two variables of interest here are $Bad\,Signal_i$ and $Good\,Signal_i$: $Bad\,Signal_i$ is equal to 1 if an individual is in the treatment group and $Over_i = 1$; $Good\,Signal_i$ if an individual is in the treated group and $Over_i = 0$. Individuals in the treatment group were told whether they misestimated their actual net hourly pay. X_{i0} is the covariates matrix. Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year, in addition to the outcome variable in the baseline survey. We test whether all treatment variables (*Good Signal* and *Bad Signal*) are jointly significant and provide a p-value for this test for each model. *Beliefs* dependent variables refer to the recall for net hourly pay (models (1) and (2)), the recall for expenses share (models (5) and (6)) and the belief of hourly net pay for the average worker in the same gig company (models (3) and (4)). The recalls are for the last week in Panel (A) – measured in our midline survey – and for the last month in Panel (B), measured in our endline survey.

Table A.20: Effect of information treatment on beliefs of job outcomes (other specification)

	Belief Net Hourly Pay		Belief Other Net Hourly Pay		Belief Expenses Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1.53	0.59	0.70	0.53	-2.19	-2.51
	(1.13)	(1.08)	(1.13)	(1.13)	(3.03)	(3.28)
Treatment · Overestimation	-0.26*	0.0025	-0.24	-0.23	0.65	0.61
	(0.16)	(0.15)	(0.17)	(0.16)	(0.40)	(0.40)
Observations	136	136	141	141	145	145
Baseline Outcome		✓		✓		✓
Demographic Controls		✓		✓		✓
p-value(Treatment No Effect)	0.14	0.85	0.37	0.36	0.28	0.32

(a) Two Weeks After Treatment

	Belief Net Hourly Pay		Belief Other Net Hourly Pay		Belief Expenses Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1.97*	1.16	-0.75	-1.01	-4.78*	-3.96
	(1.08)	(0.99)	(0.82)	(0.82)	(2.57)	(2.57)
Treatment · Overestimation	-0.17	0.069	-0.019	0.019	0.22	-0.018
	(0.14)	(0.13)	(0.12)	(0.12)	(0.39)	(0.41)
Observations	129	128	132	132	136	136
Baseline Outcome		✓		✓		✓
Demographic Controls		✓		✓		✓
p-value(Treatment No Effect)	0.17	0.27	0.49	0.42	0.17	0.22

(b) Two to Five Months After Treatment

Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We estimate equation (8) defined in Appendix E. We include in our regressions but do not report the constant term and *Overestimation*. *Overestimation* is defined as the recall belief of net hourly pay minus the actual net hourly pay, pooled for one week and one month before the baseline survey. *Treatment* is a binary variable equal to 1 if the worker was assigned to our information treatment at the baseline survey. Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year, in addition to the outcome variable in the baseline survey. We test whether all treatment variables (*Treat and Treat · Overestimation*) are jointly significant and provide a p-value for this test for each model. *Beliefs* dependent variables refer to the recall for net hourly pay (models (1) and (2)), the recall for expenses share (models (5) and (6)) and the belief of hourly net pay for the average worker in the same gig company (models (3) and (4)). The recalls are for the last week in Panel (A) – measured in our midline survey – and for the last month in Panel (B), measured in our endline survey.

Table A.21: Effect of information treatment on labor market decisions (other specification)

	Other Jobs		**Weekly Hours**	
	(1)	(2)	(3)	(4)
Treatment	-0.12*	-0.11		
	(0.071)	(0.070)		
Treatment · Overestimation	0.022**	0.024**		
	(0.011)	(0.011)		
Treatment × Less Need			0.89	0.081
			(1.77)	(1.52)
Treatment × Need			0.77	-1.13
			(3.92)	(3.01)
Treatment · Overestimation × Need			-0.19	0.17
			(0.43)	(0.53)
Treatment · Overestimation × Less Need			-0.20	-0.36*
			(0.25)	(0.20)
Observations	168	168	162	153
Baseline Outcome		✓		✓
Demographic Controls		✓		✓
p-value(Treatment No Effect)	0.080	0.056	0.92	0.47

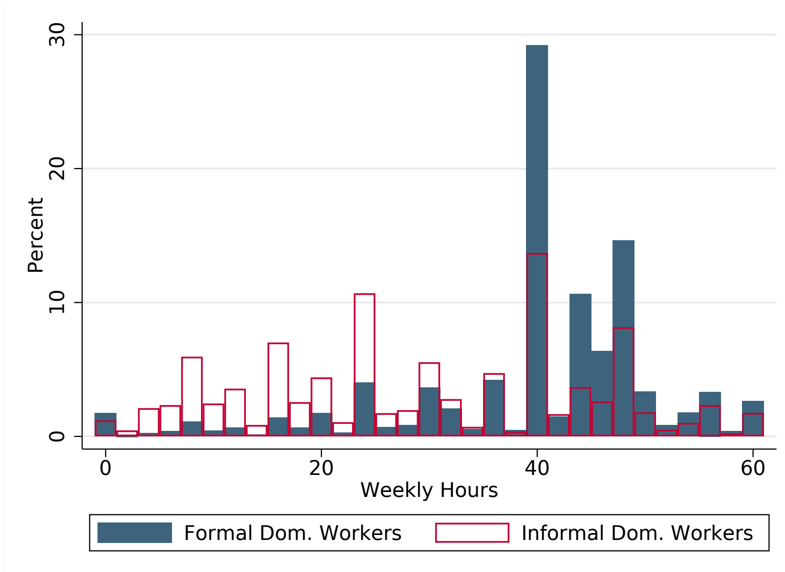
Notes: Robust standard errors in parentheses. Stars are used to denote the statistical significance of each coefficient (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We estimate equation (8) defined in Appendix E. We include in our regressions but do not report the constant term and *Overestimation*. *Overestimation* is defined as the recall belief of net hourly pay minus the actual net hourly pay, pooled for one week and one month before the baseline survey. *Treatment* is a binary variable equal to 1 if the worker was assigned to our information treatment at the baseline survey. Our set of demographic controls consists of binary variables for male, white, age between 18-34, age between 35-54, at least a college degree and household income below $40,000/year, in addition to the outcome variable in the baseline survey. We test whether all treatment variables are jointly significant and provide a p-value for this test for each model. *Need* (*Less Need*) is equal to 1 if the gig worker is in a household that is (not) struggling financially and 0 otherwise. *Struggling Financially* is defined as receiving calls from collectors, contemplating bankruptcy, or struggling to pay the bills. In addition, we add but do not a binary variable of *Need* to our model. The dependent variable for models (3) and (4) is the weekly hours worked for the main gig company for in the week following the baseline survey (including zeroes). *Other Jobs* is a binary variable equal to 1 if the worker has another gig or non gig job.

Appendix B

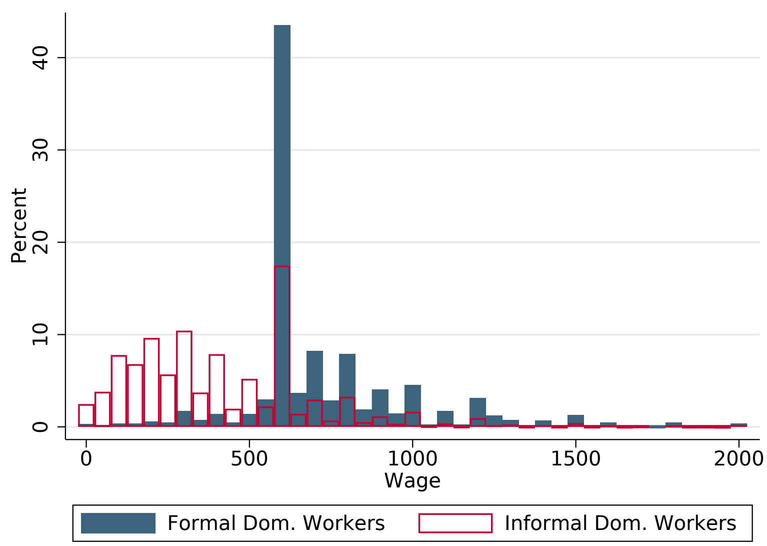
Appendix to Chapter 2

B.1 Additional Figures

Figure B.1: Histogram for domestic workers' hours and wages before CA 72/13



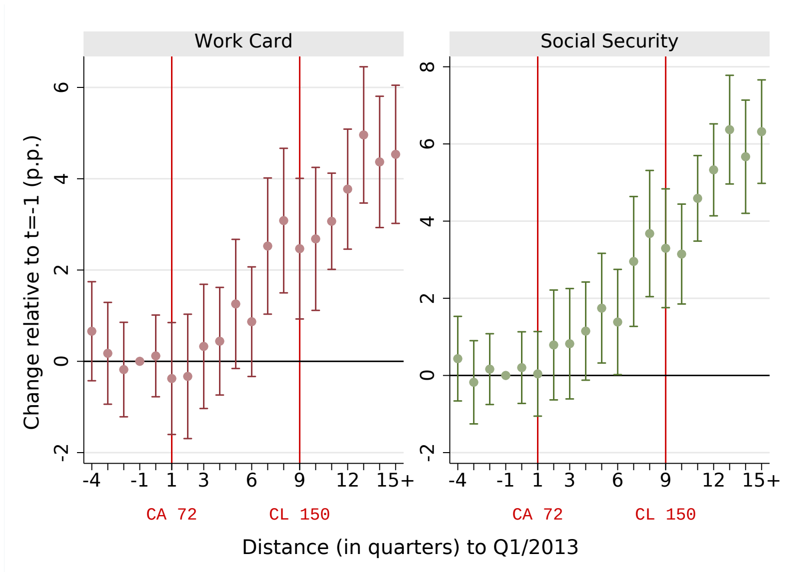
(a) Weekly hours worked



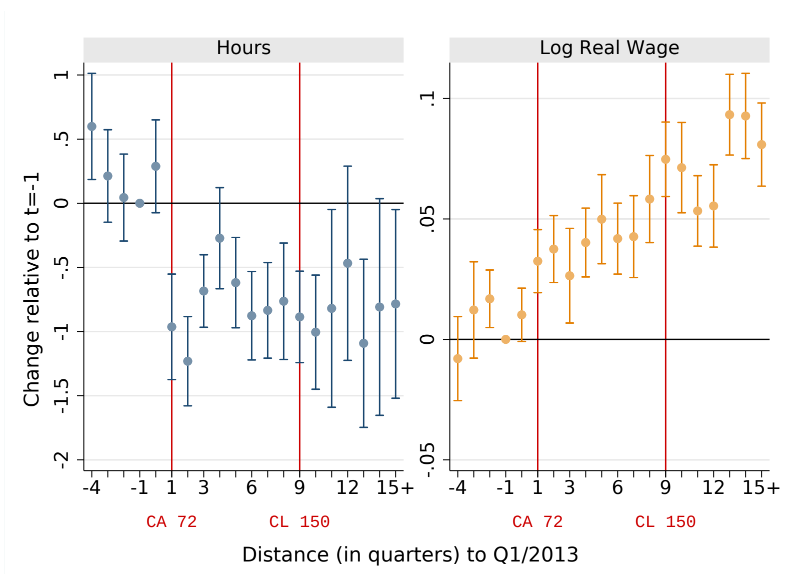
(b) Monthly wages

Notes: The graphs show the average of quarterly data from 2012 using the rotating panel dataset PNAD Contínua. The first histogram illustrates the distribution of weekly hours worked by domestic workers in 2012, before the implementation of CA 72/13 (Constitutional Amendment 72/2013). The second histogram displays the distribution of real monthly wages during the same period. Both histograms are divided into formal and informal domestic worker categories. Formal workers have a work card registration or social security contributions, while informal workers do not. The maximum wage is capped at R$2000,00 in the second graph.

Figure B.2: Effects of CA 72 and CL 150 on domestic workers (wage control group)

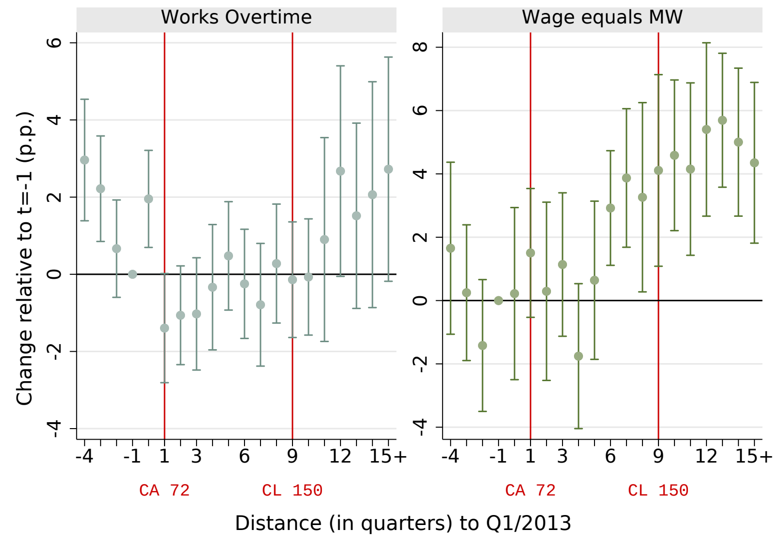


(a) Formality



(b) Weekly hours and real wages

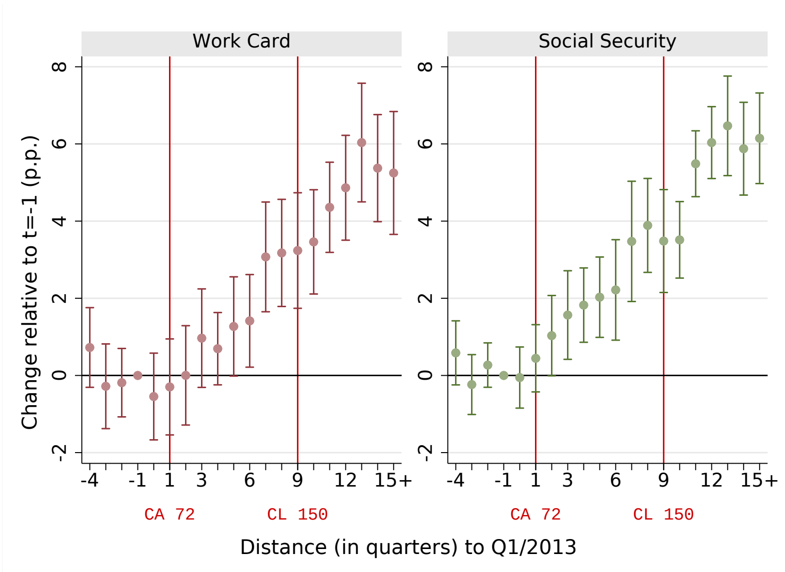
Figure B.2: Effects of CA 72 and CL 150 on domestic workers (wage control group) (cont.)



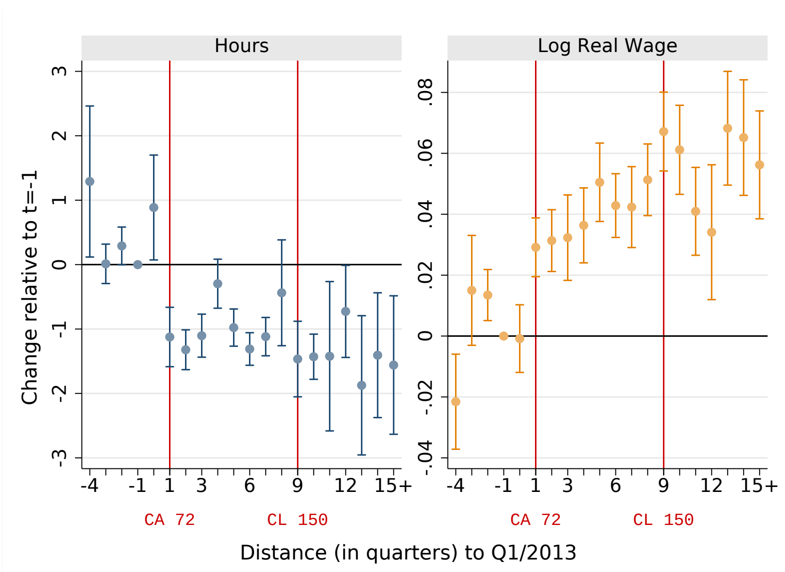
(c) Works overtime and is paid the mininum wage

Notes: The graphs display event study analyses examining the effects of CA 72 (Constitutional Amendment 72) and CL 150 (Complementary Law 150) on domestic workers' labor market outcomes, compared to the control group. The red lines mark April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. The base period for the event study is t=-1, the last period before the CA was discussed heavily in the media. The wage control group is used in this analysis (see Section 3 for details). The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood..

Figure B.3: Effects of CA 72 and CL 150 on domestic workers (race control group)

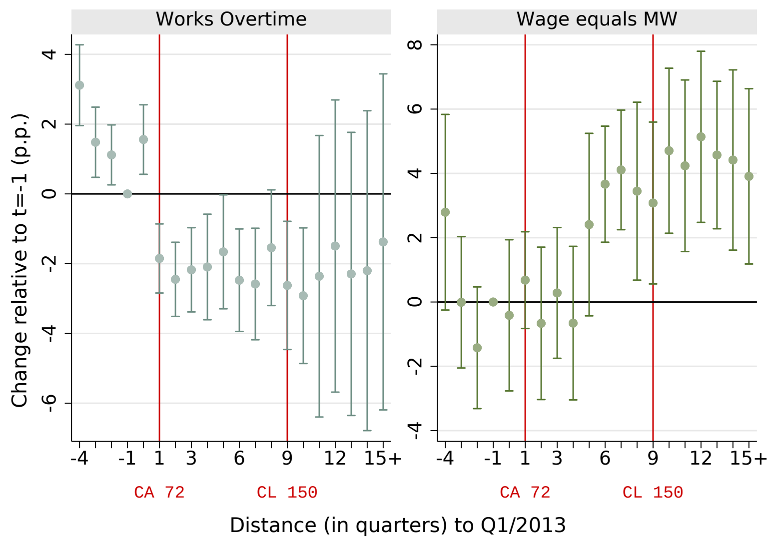


(a) Formality



(b) Weekly hours and real wages

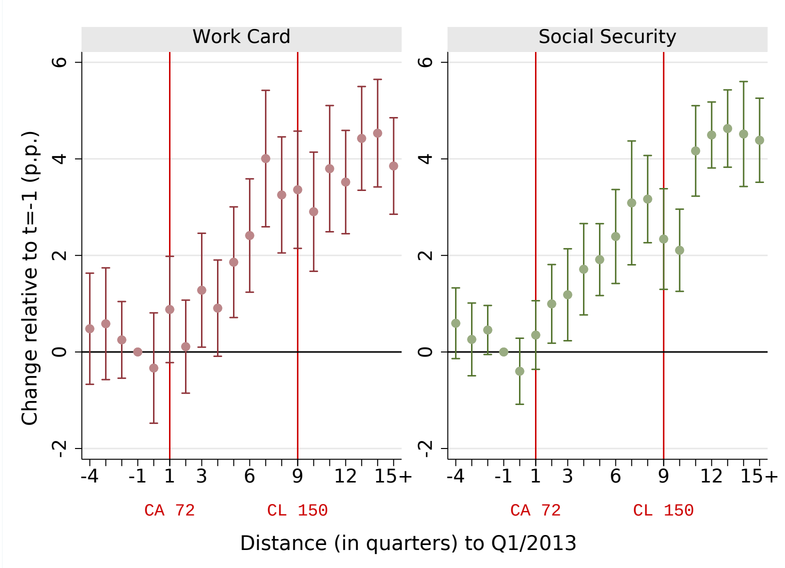
Figure B.3: Effects of CA 72 and CL 150 on domestic workers (race control group) (cont.)



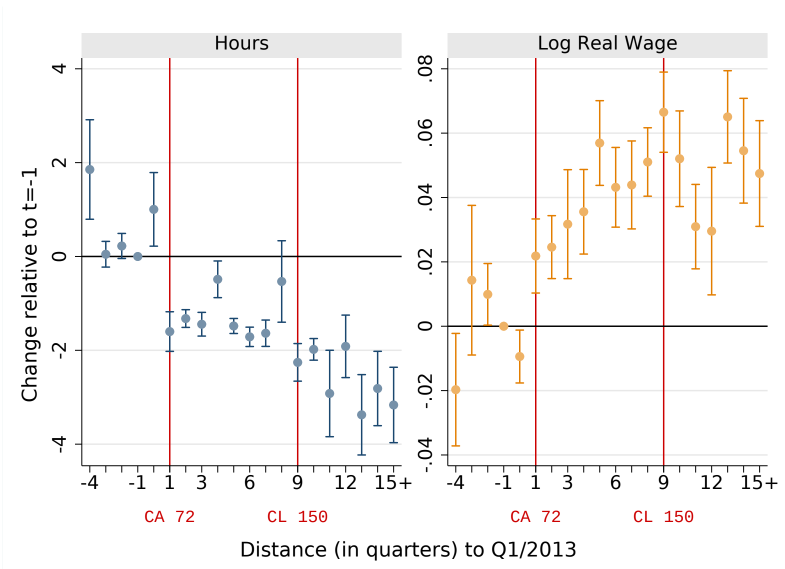
(c) Works overtime and is paid the mininum wage

Notes: The graphs display event study analyses examining the effects of CA 72 (Constitutional Amendment 72) and CL 150 (Complementary Law 150) on domestic workers' labor market outcomes, compared to the control group. The red lines mark April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. The base period for the event study is t=-1, the last period before the CA was discussed heavily in the media. The race control group is used in this analysis (see Section 3 for details). The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure B.4: Effects of CA 72 and CL 150 on domestic workers (gender control group)



(a) Formality



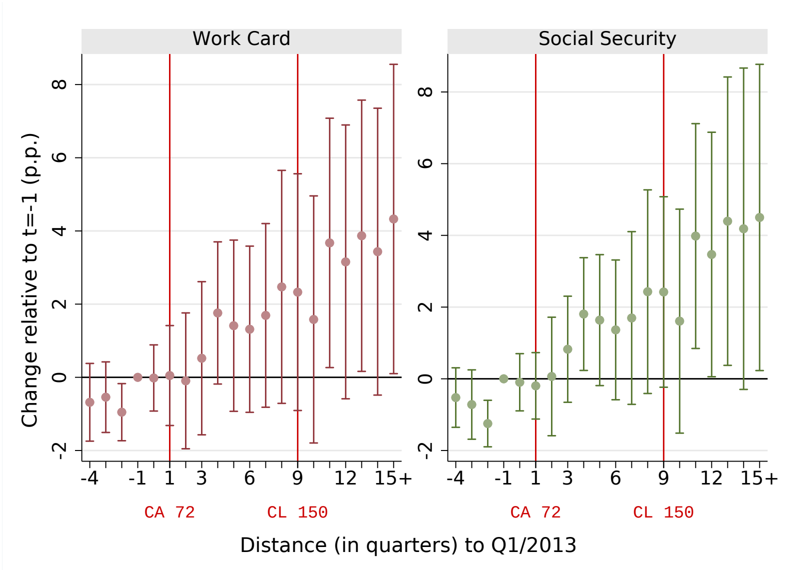
(b) Weekly hours and real wages

Figure B.4: Effects of CA 72 and CL 150 on domestic workers (gender control group) (cont.)

(c) Works overtime and is paid the mininum wage

Notes: The graphs display event study analyses examining the effects of CA 72 (Constitutional Amendment 72) and CL 150 (Complementary Law 150) on domestic workers' labor market outcomes, compared to the control group. The red lines mark April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. The base period for the event study is t=-1, the last period before the CA was discussed heavily in the media. The gender control group is used in this analysis (see Section 3 for details). The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure B.5: Effects of CA 72 and CL 150 on domestic workers (individual fixed effects)



(a) Formality

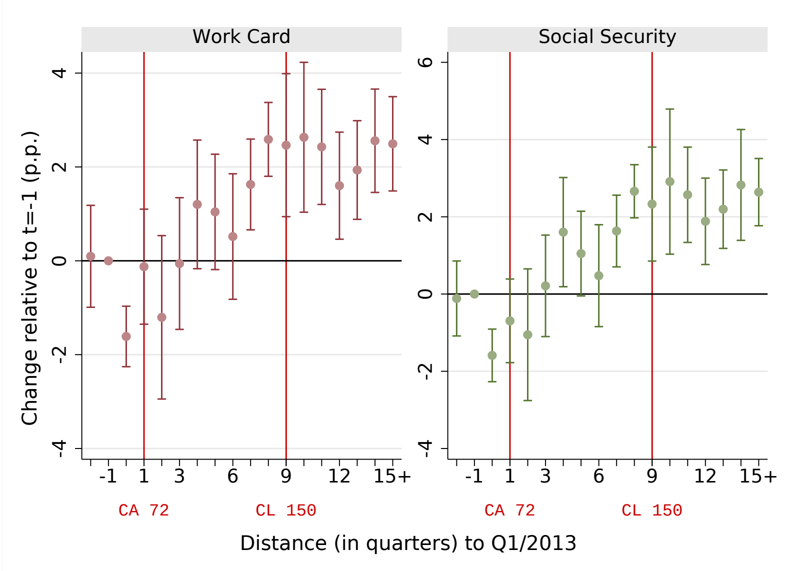
(b) Weekly hours and real wages

Figure B.5: Effects of CA 72 and CL 150 on domestic workers (individual fixed effects) (cont.)

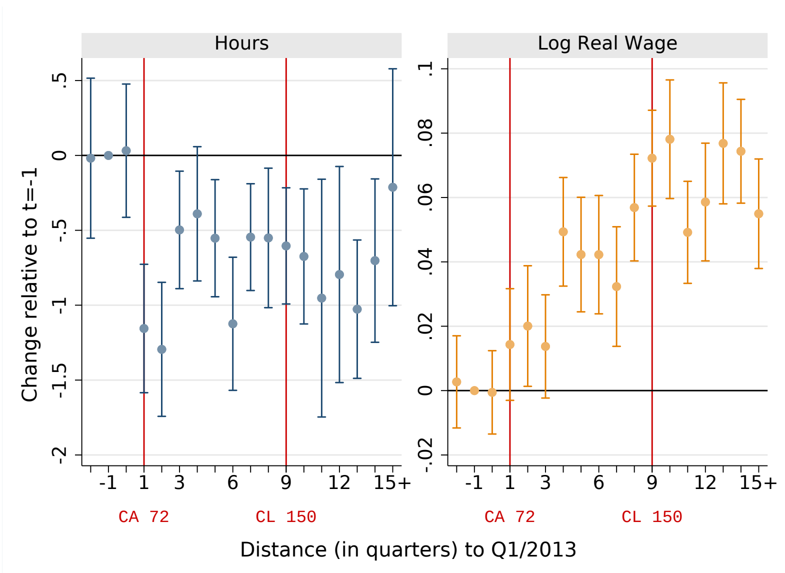
(c) Works overtime and is paid the mininum wage

Notes: The graphs display event study analyses examining the effects of CA 72 (Constitutional Amendment 72) and CL 150 (Complementary Law 150) on domestic workers' labor market outcomes, compared to the control group. The red lines mark April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. The base period for the event study is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used in this analysis (see Section 3 for details). The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure B.6: Effects of CA 72 and CL 150 on domestic workers who were previously formal

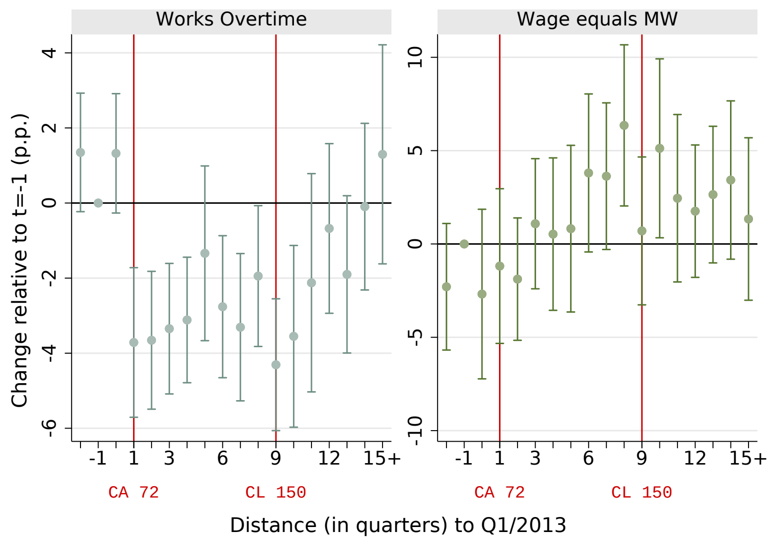


(a) Formality



(b) Weekly hours and real wages

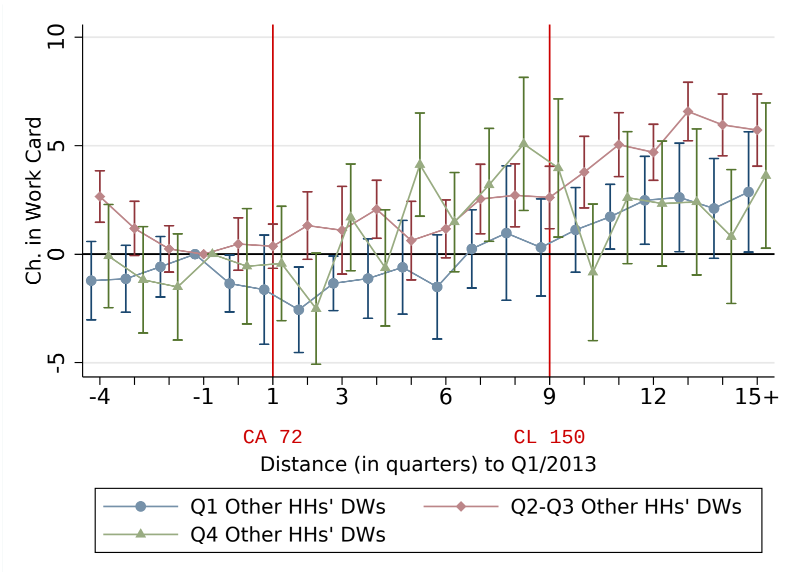
Figure B.6: Effects of CA 72 and CL 150 on domestic workers who were previously formal (cont.)



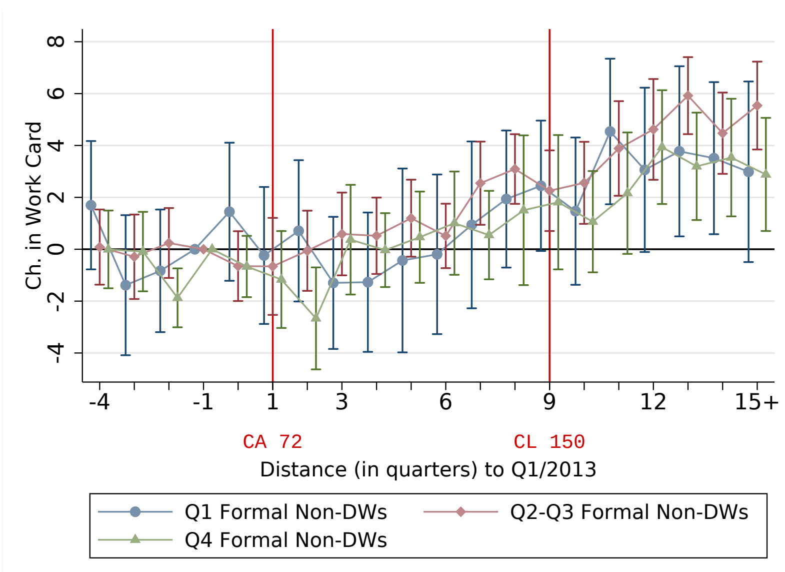
(c) Works overtime and is paid the mininum wage

Notes: The graphs display event study analyses examining the effects of CA 72 (Constitutional Amendment 72) and CL 150 (Complementary Law 150) on domestic workers' labor market outcomes, compared to the control group. Only workers who were previously formal are included in the treatment group. The red lines mark April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. The base period for the event study is t=-1, the last period before the CA was discussed heavily in the media. The schooling control group is used in this analysis (see Section 3 for details). The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

Figure B.7: Effects of CA 72 and CL 150 on domestic workers' formality by peers quartile

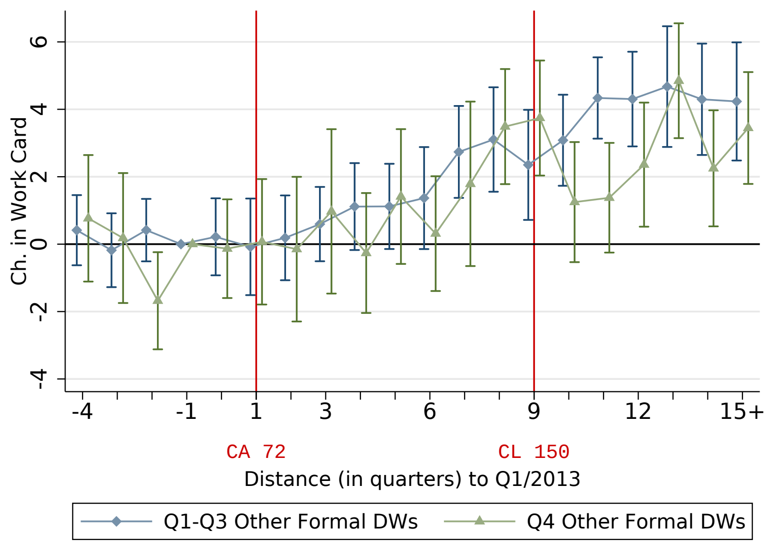


(a) Number of other DWs outside household



(b) Number of formal non-DWs

Figure B.7: Effects of CA 72 and CL 150 on domestic workers' formality by peers quartile (cont.)



(c) Number of formal DWs

Notes: The graphs present event study analyses, examining the effects of CA 72 (Constitutional Amendment 72) and CL 150 (Complementary Law 150) on domestic workers' formality (work card registration), compared to the control group. The treatment group is separated by quartile of neighborhood peer group. The red lines mark April 2013, when Constitutional Amendment 72/13 was enacted, and June 2015, when Complementary Law 150/15 took effect. The base period for the event study is t=-1, the last period before the CA was discussed heavily in the media.The schooling control group is used in this analysis (see Section 3 for details). The 95% confidence intervals are shown by the lines around point estimates, with standard errors clustered by occupation. Results include our covariates for individual-level variables: third degree polynomials of schooling and age, plus variables for female and white. In addition, it includes third-degree polynomials of the leave-out mean (at the UPA level) of years of schooling, age, real wages, female, and white., and fixed effects for occupation, date, and neighborhood.

B.2 Additional Tables

Table B.1: Top occupations in alternative control groups

Occupation	Count	Share(%)
Elementary school teachers	89,041	21.7
Agricultural and industrial machinery mechanics	17,513	4.3
Machine tool regulators and operators	15,490	3.8
Welders and oxicutters	14,491	3.5
Military police graduates	14,484	3.5
Couriers, baggage couriers	14,173	3.5
Military graduates	13,921	3.4
Gas station attendants	13,561	3.3
Auto bodyworker	11,500	2.8
Electrotechnicians	10,343	2.5
Welders and similar	9,982	2.4
Public transport inspectors and collectors	9,383	2.3
Total	409,812	100.0

(a) Race

Occupation	Count	Share(%)
Machine tool regulators and operators	15,490	6.5
Welders and oxicutters	14,491	6.1
Couriers, baggage couriers	14,173	6.0
Gas station attendants	13,561	5.7
Auto bodyworkers	11,500	4.9
Joiners and the like	9,982	4.2
Public transport inspectors and collectors	9,383	4.0
Electrical workers and the like	8,193	3.5
Forklift operators	7,687	3.2
Land movement machine operators	7,449	3.1
Teachers' helpers	6,686	2.8
Polishers	6,516	2.8
Total	236,697	100.0

(b) Wage

Table B.1: Top occupations in alternative control groups (cont.)

Occupation	Count	Share(%)
General clerks	95,254	23.6
Elementary school teachers	74,423	18.4
Preschool teachers	25,938	6.4
High school teachers	20,819	5.1
Community health workers	19,407	4.8
Specialists in pedagogical methods	15,504	3.8
Executive and administrative secretaries	12,279	3.0
Nursing professionals	11,669	2.9
Mid-level legal professionals	8,176	2.0
Accounting and costing workers	7,458	1.8
University and higher education teachers	7,008	1.7
Beauty treatment specialists	6,896	1.7
Total	404,398	100.0

(c) Gender

Notes: The tables shows the distribution of top occupations in our alternative control groups, ranked by the number of times each occupation is included in each group. The data comes from the PNAD Continua rotating panel dataset from 2012 to 2016. See Section 3 for more information on the construction of these groups.