On-Demand Assistance: Experimental Evidence from Indonesia*

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Abstract

Given labor informality in developing countries, addressing employment shocks is challenging. We study Indonesia's new approach for on-demand online applications for temporary cash assistance and online job training. Through 20 lotteries over two years, 11 million beneficiaries were selected. In nationwide, representative surveys matched with administrative data, most beneficiaries report not receiving the assistance, with no detectable impacts on employment or consumption. In contrast, survey data from applicants with working phones reveal high reported take-up and higher incomes. The results suggest that while flexible, on-demand assistance has potential, design features that allow for flexibility could hinder receipt and, thus, impact.

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I. INTRODUCTION

To mitigate economic risks, governments aim to provide a variety of programs, including cash transfers, unemployment assistance, and job training programs. However, in many developing countries, these programs need to be designed differently than in high income environments. Consider, for example, what happens when an individual loses a job. In high income countries, the government can usually verify that a job separation occurred—and when a new job is found—through formal labor market structures and the tax system. Assistance programs can thus be tied to unemployment spells, with the government providing short-term financial support and/or job training programs until the person finds a new job. In contrast, in most developing countries, with large informal sectors (Ulyssea 2020), there is no systematic way to verify someone's employment status. As a result, the types of unemployment insurance programs that we see in developed countries tend to be limited in these settings (Gerard et al. 2025).

In this paper, we study an innovative new on-demand assistance approach introduced in Indonesia, called *Kartu Prakerja* (the *"Pre-Employment Card*"), that was launched in April 2020. During the period we study, beneficiaries received an electronic job training credit of Rp 1,000,000 (USD 70) that could be used for any approved training of their choice from a set of online training marketplaces. Upon completion of the first training course, individuals then received payments equal to IDR 2,400,000 (USD 170) – about one month's work at the minimum wage in much of Indonesia – over 4 monthly tranches – to support them financially, potentially while they looked for a new job or started a new business.

This new approach included a number of policy design innovations aimed at assisting those seeking new or improved employment — but adapted to a context of high informality (Alatas et al. 2012; Jensen 2022). First, enrollment was online, easy, and open to all in order to combat the administrative verification barriers that impede program uptake (Gupta 2017; Banerjee et al. 2021). Second, people could apply when they wanted to (if they had not received the program before): in its first year, there were 11 application windows, and there were 47 windows by October 2022. Given the limited program budget, a weighted randomized lottery was conducted for each enrollment window in each province to determine acceptance. Third, the application system provided a mechanism that could potentially screen on demand for assistance: if one lost the lottery, they could choose to enter a subsequent lottery if (and when) they decided to do so. Fourth, assistance was not conditioned on employment status, but instead used a 'soft labeling' approach

(as in Benhassine et al. 2015). Beneficiaries received four cash transfers, regardless of whether one found new employment. Instead of hard conditions, the assistance was marketed specifically to those who experienced an employment or income shock, wanted to transition to a new job, wanted to start a new business, and/or wanted to build new job skills.

Finally, the government aimed for the training access and payments to be timely and efficient. Those who won the lottery quickly received an electronic training credit that they could easily use online. Upon completing training, people then received the cash transfers via a premium e-wallet account or via a bank account. The fact that payment via e-wallets was an option made it easier for those without a bank account. In short, the program aimed to provide cash support and job training to those who needed it, when they needed it.

The roll-out of this new approach was massive: In its first two years alone, 23,434,700 people (13 percent of adults) applied over 22 regular enrollment windows, with 11,388,706 enrolled. This large scale makes this, to the best of our knowledge, one of the largest individually-randomized programs in the world (Muralidharan and Niehaus 2017). Payments were fast: the median number of days between winning, completing the training and receiving the cash transfers was 14 days. Our surveys show that 87 percent of beneficiaries received payments via e-money.

We study whether this new approach was able to reach those in need, and ultimately its impacts on both consumption and employment outcomes. We leverage the program's innovative lottery structure, measuring impacts in all 20 randomized batches from the program's first two years. To do so, we bring together three data sources. First, we have anonymized registration and lottery data for the first two years of the program – over 23 million unique individuals. Second, using national identification numbers, the government matched the *Prakerja* administrative data to the national workforce surveys (SAKERNAS) and the national household welfare surveys (SUSENAS). The match yielded very large samples: a total of 79,978 unique applicants were matched to the workforce survey and 143,586 were matched to the household welfare survey. Third, we conducted online surveys of the applicants in 2021 and 2022.

We use these data to examine two key questions. First, given the open-enrollment design, and the desire to provide rapid assistance, who on net received assistance? Second, using the lottery-design for identification, what were the impacts of the assistance on improving job market outcomes and facilitating consumption smoothing?

Our first key finding is that we observe far fewer people self-reporting receiving the

program in national sample surveys than received it according to the administrative data. For example, in the matched sample, only about 24 percent of individuals in the August 2021 national workforce survey that show up as having received the program in the administrative data report receiving the program in the survey.

What drives this mismatch? One explanation could be misreporting: people not following through on receiving the program or people forgetting that they received the program. Prima facie, it seems unlikely people would forget; the registration process required going to the *Prakerja* website and going through numerous web pages all prominently featuring "*Prakerja*". More generally, the rates of under-reporting in the household welfare survey are much higher for *Prakerja* than for other government programs; in fact, *Prakerja* beneficiaries would need to be more than *2.5 times more likely to forget* than other program beneficiaries.

Another possibility is that someone else applied using the name and identification number of the applicant, and that the person did not actually receive the program (or did not receive it in full, or gave consent for another individual to apply on their behalf and receive the funds). Among winners in the administrative data, those who do not report winning are systematically different than those who do: older, more rural, less educated, and less likely to use the internet—i.e., people who may lack the skills needed to apply online. Ironically, those who win in the administrative data, but who do not report having applied when surveyed, appear in the administrative data to complete the training and receive the cash transfer faster, despite being *less* internet literate. This is suggestive that perhaps someone else completed the training for them.

News reports suggest one possible explanation consistent with these patterns: there are widespread reports of agents – known colloquially as 'jockeys' – that emerged to help people apply.[1] These agents obtain a person's information, apply for the program, deduct a fee if they win, and in some cases offer to complete the first online training. Fees could be substantial: news reports suggest that the fee was between 7-50 percent of the cash transfer.

These jockeys may have emerged due to the bureaucratic ease introduced into the system

[1] The *Prakerja* Management Office (PMO) acknowledged that jockeys may be providing a valuable service to those who cannot navigate the website themselves; see, for example, https://www.cnbcindonesia.com/tech/20201014133307-37-194301/pmo-prakerja-joki-daftar-kartu-prakerja-bukan-kriminal. However, the PMO later clarified that it would be considered fraud if a jockey promised to guarantee program receipt, https://www.cnnindonesia.com/ekonomi/20210226183101-532-611481/joki-kartu-prakerja-bisa-dilaporkan-ke-polisi. The same article also notes that the PMO notes that jockeys emerged starting around batch 4. We provide more examples from news reports below and in Appendix Figure 1.

(e.g. online application, less cumbersome paperwork, use of e-wallet accounts). In particular, we find that relatively more recipients report receiving the program in the survey data during the first three batches than in later batches. In the first three batches, the government used a third-party facial matching program to compare the faces of the selfies uploaded with those on the identity cards and then also compared them with administrative records from the Ministry of Home Affairs. In May 2020, however, the Indonesian anti-corruption agency formally ruled that additional costs to perform facial recognition were not necessary, and instructed the program to stop doing so.[2] As a result, selfies were no longer required starting with batch 4, which is when we start to see larger rates of mismatch begin to occur. Given these issues, in the remainder of the analysis, we examine two measures of program receipt – those who are listed as receiving the program according to the administrative data, and those who report receiving *Prakerja* in survey data.

Who received assistance? We find that while the targeting of the program is progressive based on administrative data, those who actually *report* receiving the program are not systematically poorer than average, suggesting that the challenges discussed above may have dampened the progressivity of the program. However, the program does appear to assist individuals that faced industry-region consumption shocks, even conditional on per capita consumption, and even just focusing on those who report receiving assistance. The hassle of often having to re-apply multiple times before winning seems to discourage high consumption individuals from re-applying, suggesting that this innovative feature on program timing may help select the right people.

We then turn to estimating the impact of the program on household consumption and employment outcomes using the 20 rounds of program lotteries. Using the national sample surveys, we first estimate the impact of being randomly chosen to receive assistance *according to the administrative data*, since this is the relevant metric for thinking about the overall cost-effectiveness. We find no observable impact on per capita consumption. In fact, we can rule out effects on total household consumption of more than IDR 30,000 per month, even among households surveyed within 6 months of the award date (and hence during the period when cash

[2] See, for example, https://nasional.kompas.com/read/2020/06/18/16302071/program-kartu-prakerja-dianggap-bermasalah-ini-7-rekomendasi-kpk and https://www.kpk.go.id/id/berita/berita-kpk/1731-perbaiki-program-kartu-pra-kerja-sesuai-rekomendasi-kpk. See also Instagram post from *Prakerja* dated August 8, 2020: https://www.instagram.com/p/CDnY4v6M9g5/?img_index=1. The PMO re-activated selfies in batch 23, after the period we study here. Starting in batch 45, they introduced 100 percent facial recognition.

transfers were going out) – far less than the IDR 600,000 per month transferred to beneficiaries.[3] Similarly, we observe no changes in employment outcomes, including overall employment, self-employment, hours worked, hourly wages, and earnings. In fact, given our large sample sizes, we can rule out overall employment effects of more than about 1.4 percentage points (about 2 percent).

An important design feature is that individuals can apply as many times as they like until they get it. This could act like an "ordeal mechanism" (Nichols and Zeckhauser 1982; Alatas et al. 2016), ensuring the program is allocated to those who value the program the most or who have the largest treatment effect. While we did find above that poorer individuals were more likely to re-apply, suggesting a *targeting* benefit of this mechanism, we can also examine whether this led to people with higher *treatment* effects being selected (as in Haushofer et al. (2025)). Examining this, we find no differential treatment effects for those who applied multiple times before receiving the program as compared to those who got it on the first try.

Given the combination of our findings thus far, one potential explanation for the lack of detectable treatment effects is that many of the people who received the program according to the administrative data report that they did not receive it. That is, it is possible that the program had meaningful impacts for those who experienced it in full, but not for those who did not receive it.

To examine this, we turn to our online survey conducted in 2022 (batch 18-22).[4] This survey includes: a) people who had been randomly sampled for our 2021 survey, completed the 2021 survey (which we use as a baseline), but had not yet been randomly selected for the program through batch 17; and b) reapplied at least once between batch 18-22. This is a highly selected group: people with active phone numbers and those who responded to both online surveys. Unlike in the national survey data, in this group, more than 92 percent of those who receive *Prakerja* in the administrative data self-report receiving it in the 2022 survey. Thus, this survey allows us to measure impacts for a subsample of people who, if randomly selected to win, are highly likely to report having received the program. The survey was conducted about 10 months after the program; this is about 4-6 months after cash transfers would have ended, but potentially when one may expect to see job impacts.

[3] While the Permanent Income Hypothesis, of course, would suggest a relatively small monthly consumption increase in response to a one-time IDR 2.4 million transfer, the lack of a consumption response is notable. The results also contrast with results from Brazil (see Gerard and Naritomi, 2021).

[4] Our pre-analysis plan originally envisaged analyzing the 2021 survey. However, the 2021 online survey exhibits differential attrition. We focus instead on the 2022 survey since the response rate is balanced by treatment status. The 2021 survey results (including Lee bounds) show similar patterns to the 2022 survey results (see appendix).

We find that the program led to meaningful improvements in employment outcomes for this sample. Specifically, we find that the program led to a 6.3 percentage point (19 percent) increase in the share of respondents who report being self-employed or business owners. We see no change in overall employment, suggesting a shift in the type of employment. Total monthly income from all jobs increased by about IDR 215,000 (19 percent), suggesting the shift in jobs may have been profitable. While we do not see increases in consumption (perhaps because this is after the cash transfer period), we observe increases in net asset purchases (3.7 percentage points, or 27 percent). In total, net asset purchases increase by IDR 532,000 for those who won the program. The shift to self-employment combined with net asset purchases suggests that people could be using the money to buy assets or otherwise start their businesses, as in Kaboski and Townsend (2011).

Taken together, the results suggest that this type of on-demand employment assistance, consisting of cash assistance and online job training – has the potential to meaningfully help people in developing countries, primarily through encouraging a transition to self-employment. But, the program design features that allowed the program to be flexible – on-demand internet-based application, ease of payments via e-money, and fewer of the cumbersome administrative checks that impede program up-take – may have led to 70 percent of people not fully receiving the program, and thus to a lack of detectable effects across the entire population. This implies that it is important to strike the right balance between the flexibility needed to reach people and the verification to ensure that only authentic beneficiaries are receiving assistance.

Our paper also contributes to the literature on emergency assistance. In such situations, program designers are often faced with tradeoffs between getting the program out to those who may desperately need it now versus having all the safeguards and mechanisms in place to reduce the chance that some of the "wrong" people receive assistance. These challenges are not just an issue in developing countries: for example, Khetan et al. (2024) note that the rapid expansion of Unemployment Insurance in the United States during COVID, coupled with changes in procedures and eligibility rules, increased the probability of a "diversion of benefits intended for eligible beneficiaries by organized criminals via identity theft," particularly in states with laxer identity verification procedures. Griffin, Kruger, and Mahajan (2023) examined the Paycheck Protection Program (PPP), which distributed about $800 billion of federally backed loans in the United States to business during COVID, and estimates $117.3 billion in suspicious lending. Griffin, Kruger,

and Mahajan show that during the crisis, FinTech lending quickly grew, as they were able to provide more lending ease than traditional bankers. However, they also find that FinTech firms were "3.23 times as likely to have at least one primary indicator of misreporting compared to traditional loans." Aman-Rana, Gingerich, and Sukhtankar (2023) also examine the Paycheck Protection Program, and find that more stringent screen rules reduced the incidence of suspicious loans by about $750 million. Our paper shows how these challenges can manifest themselves in a developing country setting, potentially limiting the overall effectiveness of program assistance.

In Section II, we discuss the setting and empirical design. Section III explores program take-up and targeting, while Section IV presents the program impacts. Section V concludes.

II. SETTING, DATA, AND EMPIRICAL DESIGN

IIA. Program and Context

Launched in April 2020, *Prakerja* aimed to fill a gap in Indonesia's social protection system. In 2019, Indonesia had begun to design *Prakerja* as a marketplace-oriented job training program, complemented with a small cash subsidy component. Due to the COVID crisis in early 2020, the design was reconceptualized, with approximately 70 percent of the assistance delivered in the form of cash transfers and about 30 percent through a voucher for online training programs.[5]

Prakerja aimed to aid those who had fallen through the gaps in the social welfare system. While the current social protection system included numerous cash transfer and subsidy programs aimed at those who are systematically poor (as determined every few years by a socio-demographics census), and social insurance for formal workers (e.g., health insurance, disability insurance) financed via payroll taxes, many individuals who are self-employed or engaged in informal work are not reached by the system. Moreover, many of Indonesia's social programs are targeted using proxy-means tests, which by their nature have some exclusion error (Alatas et al. 2012), particularly during the COVID-19 period when economic displacements meant that proxy-means tests done several years before may not have been up to date.

The program allowed individuals to apply for short-term assistance through a website when they chose to. It was targeted at those who are "currently looking for work, workers/laborers who have been laid off, or workers/laborers who need to increase their work competency, such as

[5] The 70/30 shares remained for three years, 2020-2022; in 2023, the program returned to its original design with more emphasis on skill development. (See Purbasari, Satriawan, and Sitorus 2022).

workers/laborers who have been laid off and workers who do not receive wages, including micro and small businesspeople."[6] However, given the large informal economy, these criteria were not verified. Rather, *any* Indonesian adult ages 18-64 was eligible to apply (regardless of job status), conditional on not being a student and not receiving other targeted transfer programs, such as conditional cash transfers or food assistance. Unlike other transfer programs that have infrequent intake, this program allowed flexible application timings: in its first year, there were 11 open enrollment windows spread over 7 months, with a similar number of windows spread over the second year. Applying was simple: individuals fill in their demographic information and national identification number, upload a photo of their national identity card, take a short online skills test, and answer a few background questions about their current job situation. The prioritization for those who had experienced job losses was done through a lottery weighting scheme (see below).

Given a fixed budget, the government had a set number of slots for each enrollment window. While eligibility was universal, the government sought to prioritize certain types of individuals – and not have the program just be first come, first serve – through an innovative lottery scheme. If a given enrollment window was over-subscribed (i.e., applicants exceeded the pre-defined batch quota in each province, which it always was), the government randomly chose beneficiaries from that batch by lottery, with a few exceptions that we note below. Different applicants had different lottery probability weights based on the application questions, as well as the number of times that they had previously applied. The lottery weights were not public (and also changed over time). For example, the government appears to have prioritized those who self-reported as unemployed, as well as younger applicants and those with higher test scores.[7] The fact that quotas for slots were also pre-defined by province ensured that the program was spread throughout the country. If an applicant did not win a given batch, she could enter any future lottery batches, but this was not automatic – the applicant needed to physically enter each separate lottery through the online system. (Note, however, that the full application information only needed to be filled out once a year. To enter a subsequent batch within the first year of application, one would need to go online and apply to the specific batch.)

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[6] Source: www.prakerja.go.id, which stated that a criteria for enrollment is "*Sedang mencari kerja, pekerja/buruh yang terkena PHK, atau pekerja/buruh yang membutuhkan peningkatan kompetensi kerja, seperti pekerja/buruh yang dirumahkan dan pekerja bukan penerima upah, termasuk pelaku usaha mikro & kecil.*"

[7] We have administrative data on a) the number of 'lottery tickets' received by each applicant in each batch and b) the complete set of input variables used in constructing the weights. In Appendix Table 1, we regress the lottery weights on the input variables to characterize the weights on different types of applicants.

If an individual won the lottery, she would first need to complete an online skills training program of her choice from her training fund to receive the cash transfers. This could foster skills to facilitate job search or new business development. In addition, it could act as an "ordeal mechanism" to provide the cash transfers to those who had the time and inclination to complete the training. Specifically, beneficiaries were given an online job training credit of Rp 1,000,000 (~USD 70) that could be used to purchase an online training course through one of six online training marketplaces (operated by major e-commerce platforms, such as Tokopedia and Bukalapak, as well as an Indonesian government training provider). Training institutes could apply to have their courses sold on these digital platforms, which also actively recruited training partners.[8] Trainings were mostly online, and training providers were free to set their own curriculum and course prices on the platform; the government expected that the competition within the platforms would increase the quality and lower the course prices. The trainings covered a wide range of skills, such as entrepreneurship, food and beverage, online sales and marketing, lifestyle, and IT. In the administrative data, we find that 93 percent of lottery winners completed at least one training course.

Upon completing at least one online training course, beneficiaries received cash assistance equal to IDR 2,400,000 (about USD 170), payable in 4 monthly tranches of IDR 600,000 each. There was no formal verification of employment status at this stage; all beneficiaries received the full payment schedule regardless of whether they found a job or started a business. This had the dual purpose of being able to include informal workers in the system, as well as not disincentivizing households from finding a formal job.[9]

Payments were designed to be timely and easily accessible: beneficiaries could choose whether to receive their cash through one of Indonesia's 4 major electronic-money platforms (OVO, LinkAja, GoPay, or DANA) or through bank transfer at one of the largest state-owned banks. The account was required to be a so-called 'premium account' linked to an individual's national ID number, and the government verified that the ID number on the account matched the

[8] The legal definition of a training institute was defined in Permenko No. 11 2020 (Coordinating Ministry of Economic Affairs 2020), and the application steps to have courses included were posted on the PMO website (PMO 2021a). The criterion included being owned by a public entity or a registered private business and holding "job skills and/or entrepreneurship trainings that are relevant to the needs of the labor market."

[9] Beneficiaries were also asked to complete three post-course completion surveys administered by the *Prakerja* management office, which provided an additional payment of IDR 50,000 (USD 3) per survey completed. This was simply for the Prakerja's government's evaluation and tracking purposes; since these surveys are only administered to the treatment group, not the control group, we have not obtained them and do not use them for our analysis.

ID number used to register for the program. This meant that even if a household did not have a bank account, it could still open an e-wallet account online to receive the payments. Our online survey data show that 87 percent of beneficiaries used an e-money platform to receive the transfers.

The flow of applicants through the program is shown in Appendix Figure 2. As will be described in more detail below, we examine applications, enrollment, completing the first training course, and stipend receipt in administrative data, as well as *reported* applications, enrollment, and training from survey data. Final outcomes such as consumption and employment outcomes are observed in the survey data only.

We study all randomized batches in 2020 and 2021, beginning with batch 2, which was announced on April 23, 2020, and ending with batch 22, which was announced on October 27, 2021 (see Appendix Table 2). We exclude batches 1 and 15, which were not randomized.

IIB. Data

We use three sources of data – administrative data shared by the *Prakerja* management office, national sample surveys conducted by the Indonesian Central Bureau of Statistics, and online surveys that we conducted ourselves via Qualtrics.

Administrative data. From the administrative data, for every individual that entered the online lottery for *Prakerja* in each of the study batches, we have anonymized registration data.[10] First, we have the list of 23.4 million applicants along with whether they won or lost the lottery in each batch to which they applied. Second, we have data on basic characteristics from the application form, including age, gender, education, and district; the aggregate score on an online basic skills test that the applicant needed to complete at registration; and the employment questions. Third, we observe an anonymized family identifier (derived from the individual's official family card), which allows us to link individuals within the same family, whether in the same batch or across batches. Fourth, we have the lottery weights for everyone included in each batch.

Nationally representative survey data. We combine the administrative data with multiple waves

[10] A separate set of applicants who were on official government layoff lists were designated as 'whitelisted' and allocated through a different process, rather than the lottery; these applicants are not included in our microdata. We restrict attention to the applicants who were chosen through the lottery. See https://news.detik.com/berita/d-5058758/kaji-program-prakerja-ini-4-temuan-dan-rekomendasi-kpk and https://www.kpk.go.id/id/berita/berita-kpk/1731-perbaiki-program-kartu-pra-kerja-sesuai-rekomendasi-kpk for more information on the 'whitelist.'

from two large-scale, representative cross-sectional household surveys conducted by the Indonesian Central Bureau of Statistics. First, we have data from the Indonesia's nationally representative workforce survey (SAKERNAS), which collects information on employment for the adult population (ages 15-64). The survey is run twice per year, with a large wave of about 300,000 households (about 1:250 of the population) every August and a smaller wave of about 75,000 households every February, and the questionnaire is asked to each adult household member. Starting in August 2020, the survey included separate questions about whether each household member had applied for, as well as received, *Prakerja*. To study program outcomes, we use the August 2020, February 2021, and August 2021 survey waves.

Second, we have data from Indonesia's nationally representative household welfare survey (SUSENAS), which collects information on a wide range of characteristics at the household level, including a very detailed consumption module. The survey is also run twice per year, with about 300,000 households every March and about 65,000 households every September. Starting in September 2020, the welfare survey included questions about whether anyone in the household had received *Prakerja*, although it did not ask about application status. To examine outcomes, we use the waves from September 2020, March 2021, September 2021, and March 2022.

Note that both the national workforce survey and welfare survey datasets were matched by the government to the *Prakerja* administrative data using national identification numbers; we worked with anonymized data after the matching had been completed.

Given the size of the national sample surveys, and the very high share of the country that applied for *Prakerja,* we have unusually large sample sizes for experimental analysis: on net, for batch 1-22, a total of 79,9781 unique individual *Prakerja* applicants were matched to the national workforce survey and 143,586 unique applicants were matched to the welfare survey waves to study outcomes. Importantly, the different survey waves correspond to different program batches (given the timing of each survey wave and batch), so we have detailed data on the first two years, including multiple follow-up surveys for the earlier batches. Our main specification includes everyone who applied to the program in any batch with an announcement date prior to or including the month that the survey was conducted.

Finally, to examine the targeting of the program, we also look at the March and September 2020 welfare surveys. The September 2020 wave differed from the typical welfare survey in that rather than drawing a new sampling frame, the sample was drawn from the March 2020 survey.

We use the panel nature of this survey wave that can be used to study targeting issues.

Online survey data. We conducted two rounds of online surveys of *Prakerja* applicants, one launched in August 2021 ("2021 survey") and one launched in August 2022 ("2022 survey"). These surveys included a few key outcomes that were also in government surveys (e.g., employment status, working hours), but they also covered a much wider range of outcomes, including modules on self-reported well-being, consumption smoothing, interpersonal migration, transfers, and e-money adoption and usage. The 2021 survey was conducted after batch 17 was completed, and the 2022 survey was conducted after batch 22 (the last randomized batch) was completed, as well as several subsequent non-randomized batches.

For both online surveys, respondents were contacted via WhatsApp (using the phone number that they provided during the application process) with a personalized link to a Qualtrics survey, with a series of reminders to complete the survey.[11] For those who did not have WhatsApp (which we could tell through the system), we also sent text messages, and in the 2021 survey, we additionally called 9000 people who had not responded with a reminder to fill out their survey link. Respondents were compensated at least IDR 50,000 (US$3) for their survey participation.[12]

The sampling techniques differed for the two surveys. For the 2021 survey, we randomly sampled 154,336 people from the administrative data from those who had applied in batches 7-17, stratified by batch. In this survey, we slightly oversampled families for which multiple family members applied in the same batch to allow us to study gender issues in more detail. For the 2022 survey, we sampled all respondents who had completed the 2021 survey, but who had not yet received the program as of batch 17 (i.e., had not received it as of the time of the 2021 survey), but who then reapplied to the program at least once between batches 18-22. This implies that for those in the 2022 survey, we also have baseline information from their 2021 survey responses.

In the main tables, we examine the 2022 survey, which had a high survey response rate and was balanced across treatment and control. For the 2022 survey, about 55 percent of those who

[11] Respondents received a WhatsApp message (or text message) from the University of Indonesia's Institute for Research on Economics and Management inviting them to complete a survey of *Kartu Prakerja* applicants. Given concerns about the potential skepticism of respondents that the survey was legitimate, the message included a link to an FAQ on the official *Prakerja* website explaining that the survey was an official survey of randomly selected applicants, that responses were confidential, and that it would not affect their chances of receiving the program.

[12] We say 'at least' because respondents had an incentivized choice option between payment in e-money or phone credits, to measure their demand for e-money, but all could earn at least IDR 50,000.

were sent a request completed the survey. This response rate is comparable to mail and phone surveys in other contexts (see for example, Finkelstein et al. (2012); Egger et al. (2021)). Using the same specification as in our main analysis, we show that the response rate is balanced between those who won the lottery and those who lost (Appendix Table 3, Panel A).

Note that while this sample is balanced on treatment and control – and hence is internally valid for estimating program treatment effects for batches 18-22 among the sampled population – those who respond to online surveys are a selected sample and thus may not be representative of the population. The 2021 survey had an overall response rate of about 31 percent – and since 2022 survey respondents are a 55 percent subsample of those respondents, in some sense the 2022 survey respondents are a selected sample of about 17 percent of the entire applicant population. We thus view the online survey results as complementary to the national surveys: they allow us to probe more deeply into many questions not in the national surveys, and estimate correct, internally valid estimates, but for a selected subset of the population (i.e., those with active phone numbers).

In addition, we can also analyze the 2021 survey cross-sectionally to examine the results for batches 7-17 (minus batch 15). Note, however, that the 2021 survey exhibits some degree of differential response: as shown in Appendix Table 3, Panel B, the response rates are 3.7 percentage points higher for those who received *Prakerja* than those who did not (Column 1); the differential attrition remains even with controls (Column 3). Given this, we report these results in the Appendix with tightened Lee bounds (Lee 2009; Semenova 2023) on the upper and lower confidence intervals of estimated effects, and we focus our analysis on the 2022 survey, which is balanced.

IIC. Multiple Randomized Lottery Design and Estimating Equations

In Section III below, we will start with an analysis of the program receipt and targeting. In Section IV, we then experimentally estimate whether the assistance improved employment and consumption outcomes using the lottery design. Importantly, if an individual applied for *Prakerja* in a given batch and was not selected in that lottery, she was allowed to re-apply in subsequent batches. Thus, while each lottery is random, when the outcome is at the person level, we need to account for the fact that an individual could have (endogenously) chosen to re-apply in subsequent batches if she lost a previous lottery. Therefore, we use a 'stack-and-cluster' instrumental variables

design, similar in spirit to Cullen, Jacob, and Levitt (2006).[13]

To see the intuition for this specification, we begin by considering estimation for a single batch b. For a given batch b, individual i, and province p, define $WinBatch_{bi}$ as whether the individual was selected for the program in batch b, and $EverWin_i$ as whether individual i was ever chosen for *Prakerja* in any batch up to the date of the survey. Both $WinBatch_{bi}$ and $EverWin_i$ are defined according to the program administrative data, and thus capture the effect of being chosen to receive the program conditional on applying. For a single batch b, we can run the instrumental variables regression:

$$y_i = \beta EverWin_i + \gamma W_{ib} + \omega X_{ib} + \epsilon_{ib} \quad (1)$$

where we instrument for ever having received *Prakerja* with whether they were selected by lottery in batch b, i.e., instrumenting for $EverWin_i$ with $WinBatch_{bi}$. W_{ib} is the win probability (computed for each number of lottery tickets and province) for individual i in batch b.[14] The variables X_{ib} are other controls that we observe in the administrative data (i.e., gender, whether the respondent is applying from a city or non-city district, and education above high school). Since $WinBatch_{bi}$ is random conditional on win probability, for any given batch, the IV regression in equation (1) is valid for estimating the effect of winning *Prakerja* in that batch and estimates the standard local average treatment effect of the program on the compliers. In this case, the compliers are those individuals who were induced to win the program because they won the lottery in batch b; there are no defiers (since if you win the lottery in batch b $EverWin_i$ is 1 by construction), but there are always-takers, since some people would continue to reapply in future rounds if they lose the lottery in batch b.

When we turn to the data, we generalize equation (1) by pooling across batches. That is, for each individual i, we create an observation for each batch b that she applies for. We include additional control variables with batch dummies, and then stack equation (1) for each batch on top of each other. We then estimate:

$$y_i = \beta EverWin_i + \alpha_b + \gamma_b \times W_{ib} + \omega_b \times X_{ib} + \epsilon_{ib} \quad (2)$$

where we instrument for $EverWin_i$ with $WinBatch_{bi} \times 1_b$; that is, we instrument with

[13] Cullen, Jacob, and Levitt (2006) investigate a set of lotteries that overlap cross-sectionally, whereas in our setting, the lotteries are sequenced over time, but the empirical approach is similar.

[14] Note that our pre-analysis plan specified two versions of this regression, one specification exactly as in (1), and one specification where we included the full set of all input variables used to construct the randomization weights instead of the weights themselves. Since we were not given the exact formula used to construct the weights, but were given the weights, controlling for the weights themselves is preferable, though results are similar.

$WinBatch_{bi}$ interacted with a dummy for the equation of interest for that particular batch. Since a given individual enters equation (2) multiple times with the same outcome variable y_i, we cluster the standard errors by individual i. The estimate of β thus captures the impact of being chosen to receive the *Prakerja* program according to the administrative data, accounting for the multiple lottery design. When we stack across survey rounds in different months/years, we also then interact the win probability and additional controls by the survey round. The estimate of β from equation (2) is an average of the local average treatment effect from equation (1) across all batches.

We assess baseline balance in several ways. First, we examine the baseline administrative variables in Appendix Table 4 for the national sample surveys and the 2022 online surveys. For each individual *i*, following our pre-analysis plan, we test balance by running equation (2) setting y_i to be the three control variables that we observe in our administrative data that do not enter the randomization weights: gender, city status, and years of school. We observe differences across treatment status for some variables, although the magnitudes are quantitatively very small; nonetheless, we control for these baseline variables in the main regressions. Second, in Appendix Table 5, we examine the 4 pre-program national workforce survey waves and the 4 pre-program national household welfare survey waves that we had additionally matched to the administrative data. We conduct balance checks for the key consumption and employment variables that we study to measure program impact. About 11 percent of the regression coefficients are significant at the 10 percent level, what we might expect from chance; a joint F test cannot reject the null of balance. Finally, in Appendix Table 6, for the 2022 online survey, we can also examine balance across a wider range of baseline variables that we collected in the 2021 survey; we find that 4 of the 27 variables are significant at the 10 percent level, what one might expect from chance; a joint F test cannot reject the null of balance.

III. WHO RECEIVED ASSISTANCE?

The rollout was enormous in scope: 23,434,700 people (13 percent of the adult population) applied for assistance over the first two years and were entered into the lotteries (22 enrollment windows), with 11,388,706 individuals chosen. This makes it roughly comparable, for example, in terms of scope (as a share of the labor force) to the United States unemployment insurance program, which provides benefits to about 11 million people each year.

III.1. How many people received assistance?

We begin by examining the rates of reporting receiving the program in both national surveys, and comparing them to what we would expect from the administrative data, in the spirit of the exercise done by Olken (2006), Muralidharan, Niehaus, and Sukhtankar (2016), and others. This is reported in Panel A of Table 1. Note that this comparison does not yet involve matching the surveys and the administrative data; instead, we are simply calculating means in the survey data and using the statistics bureau (BPS)-supplied frequency weights to get an estimate for the entire population, and then comparing it to the official administrative data counts.[15]

Reported program receipt in the surveys is substantially smaller than what would be expected given the administrative data, particularly after the first few batches. In August 2020 – which corresponds to batches 1-3 – the reported number of beneficiaries in the national workforce survey is 84 percent of the expected winners from the administrative data. This number is not *prima facie* unreasonable: some people may have forgotten that they won or were confused about the name of the program. Over time, however, these numbers drastically fall: by the August 2021 survey wave (batches 1-17), the ratio of the number of people who report winning to the number one would expect from the administrative data is only 32 percent. That is, while the program administrative data reports that 8,213,904 people received the program as of the August 2021 survey, the estimated number of people receiving the program based on those who self-report it is only about 2,600,000.[16]

We see a similar pattern in the national household welfare survey data. Note that in this dataset, we only observe if anyone in the household received *Prakerja*, not which individual, so we do the analogous calculations at the household level. In the September 2020 national household welfare survey (corresponding to batch 1-5), we find that that reported take-up is 78 percent of what we expect, which is similar to the August 2020 national workforce survey estimates. But again, the estimated number of recipients then drops in the 2021-2022 surveys, to only about 22-29 percent of what we would expect from the administrative data.

[15] In Appendix Table 7, we can also examine who reported that they *applied* in the workforce survey and compare this number to the official application totals in the administrative data. The findings are similar to Table 1: the number of applicants in the survey data looks similar to the administrative counts (and in fact larger in August 2020), but it significantly drops in later rounds – by August 2021, the number of people who report ever applying in the survey is about half of what one would expect given the administrative data.

[16] We also have data from the February 2022 workforce survey, though this is not matched to the individual administrative data, and hence not included in our main analysis. In February 2022, we find that the ratio of the number of people who report winning to what one would expect is 39 percent.

One possible explanation for these discrepancies could be sampling – i.e., perhaps the national sample surveys, even though they are supposed to be nationally representative, somehow missed *Prakerja* beneficiaries, perhaps due to systematic non-response or some other reason. Therefore, we next turn to the *matched* sample – that is, those individuals in either survey where the government was able to match individual survey respondents one-for-one with the administrative data based on the national ID number reported in the survey. This analysis is also useful for thinking about what the effective 'first stage' will be when we estimate impact below.

We first plot whether people report applying for or winning the program, over time, conditional on having *actually* applied for and/or won the program according to the administrative data. Figure 1A plots the share of matched respondents who report applying in the national workforce survey based on the batch in which the administrative data says they applied. Figures 1B and 1C plot the share of matched, winning respondents who report winning the program in the survey data separately based on the batch where they received the program (according to the administrative data). The national workforce survey graphs are at the individual level, while the welfare survey graph is at the household level (using the first batch where someone in the household won if multiple household members won). Each line in Figure 1 represents the results from a survey wave. The graphs support the national evidence presented in Table 1: Figure 1 shows that the share of applicants/winners in the administrative data who report applying/winning is higher in the earlier batches, and then further drops.

Table 2 analyzes this in regression format using equation (2) in the matched sample. In Panel A, we regress whether a respondent won in the administrative data by the survey date on whether they won in each batch they entered before that survey. This should not be 1 to 1, because many people who lose a given lottery will go on to reapply in future rounds (so in fact, the coefficient should drop over time as an individual has a chance to enter more lotteries). Panel A therefore represents what the first stage regression (of ever winning by the time of the survey on winning in batch) would be if there was perfect compliance.

In Panel B, we run the instrumental variables regression in equation (2), looking at whether the respondent *reports* being selected in the national sample surveys. Since this is an IV regression, we would expect the coefficients to be 1 (anyone who wins in a batch should report receiving the program). However, the estimates are much smaller – between 0.20 and 0.30 in the national workforce survey, and between 0.07 and 0.14 in the national household welfare survey. While

winning the lottery increases the probability of reporting winning, the magnitudes are smaller – which implies that many "winners" in the administrative data do not report receiving it in the survey. Since these results come from the matched sample, this suggests that the low rates of reported take-up are not due to survey sampling somehow missing the applicants and beneficiaries.

In short, there is a large difference between *reported* program receipt and program receipt in *the administrative data*, particularly after the first few batches when the program expanded in scale and scope. What drives these differences? Below, we examine several possible explanations.

Recall error. One possible explanation is recall error. Although we cannot test for this directly, we can test for the presence of differential recall by asking whether those who were randomly chosen as lottery winners are more likely to report ever having *applied*. This is a test of endogenous recall; since winning is random conditional on applying, households who applied in the administrative data and won should be no more likely to report applying than households who applied in the administrative data and lost. But if some households forgot about the program, winning the lottery may make them more likely to remember.

In Appendix Table 8, we see some evidence of this differential recall: those who won (according to the administrative data) were more likely to report applying in the national workforce survey than those who lost, whereas in fact these should be unrelated. However, the magnitudes are small: the IV estimate of differential recall – that is, the effect of winning the lottery on reporting ever having applied – is only about 9 percentage points, not nearly enough to explain the gaps in overall response rates we see above. And in any case, the mismatch shown in Table 2 is for people who won *Prakerja*, not those who applied but lost.

Several other factors suggest that the low overall rates of reporting receiving the program are not primarily driven by recall problems. First, we can revisit Table 1 and perform the same calculations for other programs, i.e., compare the estimated number of recipients nationally from the national household welfare survey data to the number obtained from administrative records for other government assistance programs. In Panel B of Table 1, we examine three pre-existing government programs: a food stamps program (*BPNT*); a conditional cash-transfer program (*PKH*); and a school transfer program (*PIP)*. The survey data count and administrative counts range from 73 to 104 percent across the three existing programs – far higher than the 22-29 percent rate we see for *Prakerja* using the same 2021 and 2022 national household welfare survey data. In

addition, we can also examine a special rural cash transfer program that was launched during the COVID crisis *(BLT-DD)*; this program was also launched around the same time as *Prakerja* and was also a short-run assistance program (see Hanna et al. 2024 for details). For this program, the ratio of number of recipients in national household welfare survey to the administrative data is 110 percent in September 2020, 120 percent in September 2021 and 91 percent in March 2022 – about what one would expect given normal variation.[17] It seems unlikely that households would systematically forget over time that they received *Prakerja*, but remember that they had received other programs.

Another possible source of recall error is that it is possible that people simply do not remember the name "*Prakerja,"* even if they received job training and the cash payment. This seems unlikely since they had to apply online through the government website – https://www.prakerja.go.id/. Nonetheless, in Panel C of Table 2, we examine, in the national workforce survey data, whether a respondent ever received any training certificate from any source, regardless of whether it was *Prakerja* or not. We report an IV regression using equation (2) where the endogenous variable is winning before survey according to the administrative data. Not all of the online trainings required a certificate, of course, but it is nevertheless worth noting that the effect of winning the program on receiving any type of training certificate, while positive, is only about 6.7 percentage points in the earliest wave (Column 2) and 5.0 percentage points by 2021. The control complier mean for this variable is 15 percent in 2021, so this means that on net no more than 20 percent of people who won *Prakerja* had received any kind of training certificate by 2021.

Finally, we note that *Prakerja* is an individual program, whereas the welfare survey is a household level survey. It is possible that a respondent does not know about *Prakerja* received by another household member. In the March 2021, September 2021, and March 2022 survey waves, the survey explicitly lists which household members were interviewed by the enumerator. We restrict attention to these cases and re-estimate Table 2 on this sample. The results, shown in Appendix Table 9, are virtually unchanged.[18] Combined, the evidence suggests that recall error is

[17] Note that for BLT-DD, we have administrative data on beneficiaries per calendar year, whereas the surveys have a 12 month lookback period, which overlaps two calendar years. To be conservative, for a survey in year t, we report the maximum of administrative data counts from calendar years t and t-1.

[18] We also re-estimate the national household welfare survey results from Table 5 below on this sample, with very similar results – see Appendix Table 10.

not likely to be the main reason for such low reported take-up rates.[19]

Sign-up by others. Another possibility is that someone else applied using the name and identification number of the individual, and that person did not actually know that they received the program. To investigate this, we first compare those that report and do not report winning among the administrative winners. Table 3 shows that those who fail to report winning are more likely to be older, less educated, located in rural areas, and non-internet users than those who do report they won. This suggests that those who are less technologically sophisticated were more likely not to know that they won and could instead have been signed up online by others.

By itself, this could reflect the fact that technologically less sophisticated people who received assistance are then less likely to remember the program. Other evidence is inconsistent with this idea, and instead suggests that, at least for some people, someone else completed the process for them. First, completing the training very quickly is suggestive of someone who is primarily interested in obtaining the cash transfer as quickly as possible and has the technical savvy to do so. To investigate this, in Figure 2, we graph the CDF of the difference in the number of days between winning and the first cash transfer; Figure 3 displays the PDF. Those who fail to report applying or winning in the national workforce survey received the cash transfer more quickly than those who did report applying (Panel A and B of both Figure 2 and 3), though we do not see this pattern in the national household welfare survey. Completing the training *faster* is not what one would expect if the lack of reporting was primarily driven by lack of technical sophistication, but it is consistent with the idea of someone else enrolling you.

Second, in Figure 4, we observe that those who accurately report their win/application status are more likely to have their demographics match in the survey and administrative data than those who did not. This would be consistent with the idea that someone else may have filled out the administrative data for those who did not report their status correctly. Moreover, we find that this demographic match quality falls over time (for example, it is about 84.8 percent in the workforce survey in April 2020 but falls to 72.3 percent in March 2021).[20] This mismatch

[19] Another survey-related possibility is that our match is imperfect. It is worth noting that even if true, this would not explain the *aggregate* differences that we observe in Table 1. Nonetheless, we next restrict attention to people for whom we have an exact demographic match between the administrative and survey data. Appendix Tables 11A-11C redo all our analysis for this demographically matched sample, and the results are unchanged.

[20] Appendix Table 12 provides further details on the match. As shown in Panel A, the overall discrepancy across the

increasing over time is consistent with the fact that fewer people correctly report their application/win status over time (see Figure 1 and Table 1) – suggesting that the two phenomena are related.

Importantly, these patterns – of decreasing rates of people reporting they received *Prakerja*, and of higher demographic mismatch, also line up with changes in verification procedures. In the first three batches of the program, *Prakerja* used a facial matching program to compare the faces of the selfies uploaded with those on the identity cards. In May 2020, however, the Indonesian anti-corruption agency ruled that additional costs of facial recognition were unnecessary, and instructed *Prakerja* to stop doing so.[21] As a result, selfies were no longer required starting with batch 4 (although they were re-instated in March 2022 after the period that we study). The fact that we observe declines in the report winning rates soon after facial recognition was discontinued is consistent with the idea that it may have become easier for someone else to complete the application form.

News reports suggest that agents were assisting with program sign-up. The Indonesian news media reports that agents – known as 'jockeys' – emerged to help people apply for the program, starting around batch 4 –when we start to see a fall-off in the share of people who win the program reporting winning in the survey data.[22] The news media reports that these agents obtain a person's information (including their national ID number), apply for the program, and deduct an agreed-upon fee if the person receives the program. These fees could be substantial: anecdotal news reports indicate the fee was between 7-50 percent of the cash transfer. It is possible that an agent could apply on behalf of those who gave their information, open an account to receive the transfers in the name of the respondent, and simply not tell the user they had won; though we are not aware of news reports of this, the *Prakerja* management office in the news repeatedly

datasets is driven by education. This is unsurprising, given that age, original location and gender are embedded in the NIK ID number. Panel B and C show that applicants in the administrative data who report applying in the workforce survey have a higher match rate (84 percent) than those who did not report applying (74 percent).

[21] See, for example, https://nasional.kompas.com/read/2020/06/18/16302071/program-kartu-prakerja-dianggap-bermasalah-ini-7-rekomendasi-kpk and https://www.kpk.go.id/id/berita/berita-kpk/1731-perbaiki-program-kartu-pra-kerja-sesuai-rekomendasi-kpk.

[22] See, for example, https://www.cnbcindonesia.com/tech/20201014133307-37-194301/pmo-prakerja-joki-daftar-kartu-prakerja-bukan-kriminal; https://www.cnnindonesia.com/ekonomi/20210226183101-532-611481/joki-kartu-prakerja-bisa-dilaporkan-ke-polisi; https://www.liputan6.com/news/read/4685707/journal-joki-kartu-prakerja-celah-dalam-sistem-atau-fenomena-saling-bantu?page=4; and https://news.detik.com/berita-jawa-tengah/d-5759040/menko-airlangga-dicurhati-warga-ada-joki-kartu-prakerja-di-bantul (the last of which reports how a beneficiary explained to a Minister during a public field visit how she signed up for *Prakerja* using an agent).

warned beneficiaries of fraudulent agents who may misuse personal data, and there are some examples of more systematic fraud that were prosecuted by Indonesian authorities. In particular, the *Prakerja* administration warned applicants to be aware that official registration is only via the website, but scammers often invite individuals to register offline using personal data such as ID cards, family cards, and/or tax numbers; require victims to fill out fake forms — in paper format, apps, or Google Forms; instruct victims to open specific bank accounts and make deposits; and promise guaranteed approval for account and program registration.[23] Examples of solicitations by agents reported in the news media are shown in Appendix Figure 1a-1d. Examples of the warnings given by the *Prakerja* administration are shown in Appendix Figure 1e.

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On net, these patterns suggest that some of the very features that allowed the program to be on-demand and easy to access – such as application via the internet, lack of the cumbersome paperwork verification steps that impede take-up in many other contexts, and e-wallets – may have also led to a substantial fraction of beneficiaries to be unaware that they had received the program.

III.2. Targeting

The idea of on-demand employment assistance is to match those who needed assistance with the timing of when they needed assistance. To examine this, we use data from the March 2020 welfare survey, which was conducted right before *Prakerja* was launched, thereby providing a snapshot of people's income and work status right before the program began. We examine measures of people's need: log per capita consumption at baseline, whether the individual was employed at baseline, whether the household had a family member hospitalized in the last year, and a dummy for whether people were in an industry-region that experienced negative consumption shocks between March 2020 and September 2020 in the top quartile of such shocks.

We estimate the following regression via OLS, where each observation is a single

[23] See, for example, https://www.cnnindonesia.com/ekonomi/20200930131246-532-552691/manajemen-minta-warga-waspada-penipuan-joki-kartu-prakerja; https://www.kompas.com/tren/read/2021/02/27/102500565/waspada-joki-prakerja-mulai-rp-300-ribu-ini-kata-pmo-prakerja?page=all. Two more systematic examples of fraud syndicates that used stolen national information cards to register for *Prakerja* are given in news reports at https://www.youtube.com/watch?v=NemYq6idViQ and https://regional.kompas.com/read/2021/12/07/070023178/fakta-lengkap-sindikat-hacker-kartu-prakerja-fiktif-bobol-12-juta-data-bpjs?page=all; these tried to register for 1,000 and 10,000 accounts, respectively. The Prakerja administration's warnings are discussed, for example, here https://www.idntimes.com/business/economy/hati-hati-penipuan-ini-ciri-ciri-penawaran-pendaftaran-prakerja-palsu-00-brqf5-l648k9.

individual:

$$y_{id} = \alpha_d + \beta X_i + \epsilon_{id} \quad (3)$$

where i is an individual, d is a district (and hence α_d are district fixed effects), and X_i are a vector of household characteristics. In Column 1 of Table 4, we examine whether each of these four characteristics affects the probability that individuals received assistance (as reported by the administrative data) in any of the 22 rounds across the full March 2020 dataset; in Column 2, we present coefficient estimates from a multivariate regression of the four variables (we also control for having received other programs and using internet at baseline, which also may have been important determinants of application, since internet use was *de facto* required to access the program and since those receiving other social programs were excluded from the program). Given—as we discuss above—that many households report not receiving *Prakerja*, we take advantage of the fact that the September 2020 wave (batch 1-5) is a partial panel of the random subset from the March 2020 wave. Thus, we next examine how these characteristics predict whether the respondent is an *administrative winner* of batch 1-5 in the matched sample (column 3-4) and whether the respondent is a *self-reported* winner in batch 1-5 in the matched sample (column 5-6).

Finally, we consider whether they reapplied if they lost (column 7-8). Since this regression is at the individual-batch level, we augment equation (3) to include batch fixed effects, i.e., we estimate:

$$y_{ibd} = \alpha_d + \alpha_b + \beta X_i + \epsilon_{id} \quad (4)$$

First, we find that those who receive assistance do have higher consumption than average. In some sense, this is not surprising since the program is explicitly targeted to those who are not on other social programs. Controlling for whether individuals receive other social programs, however, we find that administrative winners are poorer on net than the general population (column 2). However, we find no observable consumption difference between those who report that they won and the general population (column 6). We also find that the application ordeals seem to discourage the wealthier: higher consumption individuals are less likely reapply conditional on losing the lottery (column 8), though the magnitude here is relatively small.

Those who are employed in March 2020 are more likely to report winning (column 6). Moreover, even conditional on consumption levels and internet use, those who were employed at baseline applied earlier to the program. In addition, the ordeal of having to reapply if one loses

seems to discourage those who are employed from continuing to reapply if they lose initially.

Employment in March 2020, however, may not necessarily predict what happens over the subsequent months, and in particular, is not necessarily related to who experienced *shocks* over this period. Thus, we next explore whether the individual worked in an industry and region where people, on average, experienced a high negative consumption shock over the period between March 2020 and September 2020. Specifically, we compute the average difference in per capita consumption between March 2020 and September 2020 for each industry-region cell (where we have 27 industries, interacted with a dummy for being on Java or off Java), and define a dummy for being in the top quartile of such shocks.[24] We find that people in industry-regions with negative consumption shocks are more than twice as likely to report winning the program.

Finally, we also examine another measure of shocks: health shocks. We find that those households where someone was hospitalized in the last year are more likely to win the program based on the administrative data across the entire sample (column 2), although they are not more likely to report winning (column 6).

III.3. Summary

On net, the program's on-demand design, which aimed to create an easy and fast transfer mechanism for households in need in a low information environment, nevertheless screened in poorer households, and made the program responsive for households who had a recent economic shock. However, these same features that allowed flexibility and high uptake – an on-demand, internet-based application system, ease of payments via e-money, and relatively few of the cumbersome administrative checks that often substantially reduce take-up of other government assistance schemes – may be related to the fact that many households did not report that they ever received the program, despite having been beneficiaries in the official list.

This has several implications for our analysis. First, it is important to note that when we estimate the program impacts (i.e. using equation 2), we will be estimating the overall effect of the program for all those who received it according to the *administrative* data, even if households do not report receiving it in the surveys. This is the relevant estimate, for example, for conducting a

[24] One of the 27 industry categories is the unemployed. The 13 industry-region cells with the largest values of (average March consumption – average September consumption) are coded as 1 in the industry shock dummy, and the other 14 industry-region cells are coded as zero. In Appendix Table 13, we also include a version with the continuous shock for industry consumption rather than the dummy variable and find similar results.

cost-benefit analysis of program design, since the government incurred the costs regardless of whether households reported receiving the program. This approach also captures situations where a household really did receive the program, but failed to report it on the survey (perhaps forgetting the program name), since the effects would be captured regardless. For this analysis, we focus on the sample of those who applied (and won) as defined in the administrative data, and we analyze their impact using the full matched national sample survey data.

One may also be interested in studying the program's impact for those who actually received the program. The estimates from our survey sample – which collect highly detailed outcome data, but for the subset of people with live phone numbers and who responded to both of our surveys – may be useful for this purpose. We discuss both in turn.

IV. IMPACTS OF ASSISTANCE ON CONSUMPTION AND EMPLOYMENT

IV.A. Measuring overall impacts in the national sample survey

One goal of the program was to provide rapid cash transfers to individuals when they need it (i.e., when they have a shock), and thereby to improve both overall household consumption and consumption smoothing. A second goal, through the training component (as well as cash to support business creation and job search), was to increase their skill level, and to help them transition to new jobs or start businesses. This could affect employment, as well as consumption (if households earned more in their new endeavors). We next experimentally explore these ideas for the matched respondents in the national workforce and welfare surveys.

We estimate impacts using equation (2), and report instrumental variables estimates that account for the fact that losers from one lottery can re-apply in subsequent lotteries. In particular, as described above, we instrument for having received the program prior to the survey date *according to the administrative data*, stacking all lottery batches prior to the survey date. The estimate thus captures the average program treatment effect for everyone who received the program according to the administrative data, regardless of whether they report receiving the program or not.[25] This estimate is useful for conducting cost-benefit analysis. For each survey, we

[25] Our main specification uses as the key variable whether they have won *Prakerja* according to the administrative data before the survey date. As an alternative, we have redone all the results where we instead define the key endogenous variable as having won *Prakerja*, completed a training, and received the first incentive payment prior to the survey date. This excludes, for example, a small percentage of households who never completed the training. The results, shown in Appendix Tables 14 through 18, are qualitatively very similar to the main results.

report a 'stacked' effect which captures the average effect pooling across all survey waves, as well as the individual survey wave-by-wave estimates.[26] Table 5 reports these results.[27]

First, we observe no statistically detectable effects on total household consumption (Panel A). Specifically, stacking all national welfare survey waves, we can rule out consumption effects larger than IDR 30,000 (USD 2.0 or about 0.6 percent). This is true even in early waves, such as September 2020 and March 2021, when the respondents were much closer in time to when they would have received the four monthly IDR 600,000 transfers. For example, in March 2021, we can rule out an effect on total consumption larger than IDR 31,000 (USD 2.1).[28] To compare this to the size of the incentives, note that if the total IDR 2.4 million received was spent evenly over say a 6-month period, and all participating households received the full transfer amount, one may have expected an impact on consumption on the order of IDR 400,000 per capita per month; we can clearly reject impacts this large in the data. Of course, if households followed the permanent income hypothesis (e.g., Friedman 1957), they would spend substantially less than IDR 400,000 per month out of a one-time lump-sum transfer, but nevertheless the lack of any detectable consumption impacts is notable, especially in light of the findings in Section III.1 that many households who won in the administrative data may not have received the transfers.

Second, we find no systematic impacts on employment outcomes (Panel B). Looking at the stacked results (Column 1 for national workforce survey; Column 5 for national welfare survey), we find no detectable impacts overall on the key employment variables: employment, self-employment, hourly wages, hours worked, job search, or activity preparing new businesses in both datasets. Pooling across all datasets yields small standard errors, so we can rule out reasonably small treatment effects. For example, we can statistically reject an effect on employment larger

[26] Appendix Table 19 presents the OLS results, Appendix Table 20 provides the IV estimates where reported receipt of *Kartu Prakerja* is the endogenous variable rather than administrative receipt, and Appendix Table 21 examines the results where winning *Prakerja* is defined at the household level rather than the individual level. All qualitative conclusions are the same as in Table 5.

[27] The primary outcome variables for the analysis of both the national sample surveys and online surveys are pre-specified in the pre-analysis plan available at https://www.socialscienceregistry.org/trials/8051, which was filed before we ran any treatment/control comparisons. Not all outcome variables are available in all datasets.

[28] If consumption responds to cash on hand, one may expect the largest impacts while households were still receiving the transfers. To investigate this timing issue more systematically, Appendix Table 22 splits all respondents and surveys into those where the survey was within 6 months of the batch announcement date, or greater than 6 months. Given the timing of the program (after winning and completing the first training course, beneficiaries receive monthly transfers for each of the next 4 months), the 6 month window from the batch announcement date to the survey date thus captures the period when almost all winners would be receiving transfers. Restricting to all respondents who were surveyed within 6 months of the survey date, we can reject increases of more than IDR 23,000 per month in total household consumption (see Appendix Table 22).

than 1.3 percentage points (about 2 percent), an effect on hourly wages greater than 2.8 percent, and an effect on household earnings greater than IDR 18,000 (USD 1.2, or 1.8 percent).

An important design feature is that individuals are not automatically re-entered if they lose the lottery. Instead, they can apply as many times as they like until they get the program. This could act like an ordeal mechanism (Nichols and Zeckhauser 1982; Alatas et al. 2016), ensuring the program is allocated to those who value the program most or to those who have the largest treatment effect. We examine this in the data as pre-specified; specifically, in Appendix Table 23, we compare the treatment effects in the first batch they applied vs. (for those who lost the first time around) in subsequent batches in which they applied. We find no systematic patterns in the data, suggesting that the feature that you can apply multiple times was not acting as a large enough ordeal to generate treatment effects.

Finally, we examine heterogeneity along other pre-specified characteristics that may affect one's labor market experience (gender, age, education, and region) in Appendix Table 24A and 24B. We also find no systematic patterns here.

IV.B. Impacts on those with high rates of reported program receipt: online survey evidence

It is possible that the low reported rates of program receipt could dampen any program impacts. Therefore, we next turn to the results from the online survey that we conducted. These surveys were initially designed to provide rapid responses (i.e., prior to the national sample surveys being collected, processed, matched, and released), and to allow us to include some additional questions that were of interest, even if they were not in the national sample survey. They also have an additional benefit: for people to have answered the survey, they needed to have real and working phone numbers registered with the *Prakerja* website months after applying (which would be unlikely for those who did not apply on their own).

As we discuss in Section II.B, we focus on the 2022 survey, which corresponds to batches 18-22, using the 2021 survey responses as a baseline.[29] Given the balance between treatment and control, the estimates are internally valid to this sample. However, they represent a *highly selected sample*: one can think of the 2022 survey as selecting about a non-random 17 percent of the original target population. These types of response rates are not particularly surprising for an online survey,

[29] Results for the 2021 survey results with Lee bounds to account for the differential attrition are provided in Appendix Tables 25 and 26. Although these results cover earlier batches, the conclusions are largely similar to the 2022 surveys.

and could reflect the usual challenges in getting people to fill out surveys online. In that world, the characteristics of those who respond should mirror the normal population. However, we find that respondents are younger, more educated, and more urban than those who did not respond (Appendix Table 27). This mirrors our findings in Table 3; i.e., those who answered our online surveys are more likely to exhibit the same selection patterns as the winners who reported winning.

The respondents are also more likely to self-report winning the program, more so than in the national surveys. Unlike the national surveys, here the estimates are very high: Table 6 shows that winning between batches 18-22 increases the likelihood of reporting winning by 79 percentage points. Since the control mean is 13 percent (some people who do not win in batches 18-22 report winning anyway, most likely because they won in subsequent batches after batch 23), this implies that those who win *Prakerja* in the administrative data are more than 92 percent likely to report winning it in the survey. This suggests that – unlike in the national data – virtually all the survey respondents who we expect to receive the program based on the administrative data do, in fact, report receiving the program.[30]

In addition to reporting that they receive the program at high rates, they also report high rates of ever having received any types of job training, though it is still far from 100 percent: we find that the program led to a 32.9 percentage point increase (164.5 percent, over a mean of 20 percentage points) in those who report having completed any type of training course. Recall this is much larger than those who report receiving any training certificate in the national survey samples (Table 2). As shown in Appendix Table 29, Food & Beverage was the most common type of training courses (25.30 percent), with Sales and Marketing second (13.65 percent).

In short, the sample of respondents to the online survey appear to have been individuals who were more likely to have gotten the program, or gotten it in full. We turn to the estimated impacts for this sample. Table 7 presents the results, estimated using equation (2).[31] Column 1 reports the results including strata and baseline demographic controls, while Column 2 adds the baseline variable of the outcome as an additional control.

We begin by examining consumption outcomes in Panel A of Table 7. In addition to total consumption (as we have in the national household welfare survey), we have a wider range of

[30] Since both the national surveys and our online surveys are independent random samples, the number of people who are in both surveys is very small. For this small overlap set, however, we can find that those who respond to our online survey are more likely to report winning *Prakerja* in the national sample surveys (Appendix Table 28).
[31] Appendix Table 30 reports the results using OLS rather than IV. The conclusions are the same.

consumption smoothing outcomes. Similar to our findings in the national sample surveys, we find no detectable effects on total monthly consumption. The standard errors are larger in this sample, though, and we cannot reject an increase of about 4 percent in consumption. It is worth noting that the survey was conducted about 10 months after the lotteries (and hence about 4-6 months after they would have finished receiving the cash transfers); if the transfers were spent immediately rather than being smoothed over time, they would not necessarily be picked up months later.

However, we find effects on the other measures of consumption and consumption smoothing. For example, households are more likely to report purchasing durable assets: we find that households are about 3.7 percentage points (27 percent) more likely to have purchased any durable asset in the last 15 months. It is possible that these asset purchases could be complementary to business formation, though we do not have sufficient statistical power to tease out impacts among multiple types of assets. If so, this could also explain the lack of consumption response – e.g., as in Kaboski and Townsend (2011), where households who receive a positive credit shock actually reduce consumption to co-finance investment.

Similarly, the point estimates suggest that households are also 3.4 percentage points *less* likely to sell assets, though this is not statistically significant. Interestingly, when we zoom in on asset sales (see Appendix Table 31), we find that households are 4.7 percentage points (21 percent from a mean of 23 percent) less likely to have sold gold in the past 15 months, suggesting that the program had an impact on consumption smoothing. Combining asset purchases and sales, we find that *net* asset purchases increase by IDR 532,000 (or about IDR 26,000 per month).

To capture the overall effect on utility, we also ask a subjective well-being question. Specifically, we ask respondents to imagine a ladder where the highest step (10) represents the best overall living conditions and 0 represents the lowest, and then to respond which step they view themselves as being on. This 'subjective ladder' welfare measure increases by about 0.3 (6 percent, from a mean of 4.6). We also find a reduction in self-reported depression of 0.03 on a normalized 0-1 scale (5 percent, from a mean of 0.57); see Appendix Table 32.[32]

We also find that the program seems to have affected labor market outcomes, primarily through an increase in self-employment. Specifically, we find that the program led to a 6.3 percentage point (19 percent) increase in the share of respondents who report being self-employed

[32] We used a two-question depression screen, the PHQ-2 (Kroenke et al. 2003), and a 2 question self-efficacy module, adapted from Schwarzer and Jerusalem (2010). We find no measurable impacts on self-efficacy.

or business owners. Interestingly, self-employment was the only outcome where we observed a positive and statistically significant treatment effect in the early national data (the August 2020 workforce survey; see Table 5 column 2), which was the period in batches 2 and 3 before we saw the large mismatch issues arise in the national data. We see no change in overall employment, but we do see a substantial (9.7 percentage points, or 22 percent) decline in those who are searching for work. The fact that we see increases in self-employment, without changes in overall employment, suggests that the *Prakerja* program led to changes in employment mainly through self-employment among those who already had a job, but may have been looking to improve their work environment by starting their own business.

We also see increases in incomes: total monthly income from all jobs increased by about IDR 215,000 (19 percent). Although not statistically significant, the point estimates suggest that this came from earning more per hour (increase of 36 percent, p-value=0.17), rather than working more hours. The fact that incomes go up, but consumption does not, suggests that savings may explain the difference. Indeed, as described above, we observe measurable increases in net asset purchases. While the observed net asset purchases do not fully explain the difference between the reported income increases and the lack of a reported consumption increase, it is worth noting that we only asked about four common asset types (motorcycles, electronics, livestock, and gold/jewelry), so it is possible that there are other unmeasured asset types that make up the difference. Finally, in addition to increases in income, self-reported job satisfaction (measured on a 1-6 scale, normalized to range from 0-1) also increased by about 6.4 percent (p-value = 0.014).

Finally, by allowing recipients to receive benefits through e-money, the government may have also promoted adoption of this new financial technology, similar to the effect of government-issued debit cards on financial use in Mexico documented by Higgins (2022). Indeed, *Prakerja* was the first Indonesian government program to allow the use of e-money platforms for payments, and as discussed above, 87 percent of beneficiaries opted to receive payments via e-money. We therefore examine whether exposure to the program changed respondents' preferences about using e-money more generally. Specifically, as a revealed preference measure of respondents' comfort with e-money, we offered respondents the choice of receiving their compensation for completing the survey (IDR 50,000, about USD 3) as mobile phone credits or an e-money transfer. Appendix Table 32 shows that those who won *Prakerja* are 6 percentage points (9 percent) more likely to choose payments via e-wallets rather than phone credits – suggesting that, indeed, the program did

increase e-money usage.[33] Respondents also self-report they are 9 percentage points (21 percent) more likely to have made an e-wallet purchase over the previous month. Combined, these results suggest that the use of e-money as a government-to-person payment platform helped improve adoption of this new financial technology.

IV.C. Implications and Marginal Value of Public Funds

We can use our estimates to compute the marginal value of public funds (MVPF) for Prakerja. Following Hendren and Sprung-Keyser (2020), the MVPF is defined as the ratio between the recipient's willingness to pay for the program divided by the net cost to the government, defined as the program cost less any additional fiscal revenues that result from the program.

To compute this in our setting, we need several parameters. The program had two components – cash (IDR 2.4 million) and training (IDR 1 million). For those that received the program, we assume the willingness to pay for \$1 in cash is \$1. To assess the willingness-to-pay for the training (denoted $WTPT$), in the 2022 survey, we included a series of hypothetical questions asking respondents what they would prefer – Rp. 1,000,000 in training or Rp. 800,000 in cash, Rp. 1,000,000 in training or Rp. 600,000 in cash, and so on. Focusing on the control group, i.e. those who never received the program, we compute an average willingness-to-pay for the training of 0.265. Following the analysis in Section III.1, we assume a fraction α of the participants who received the program in full and get WTP of \$1 for the cash and \$0.265 for the training; for the $1-\alpha$ who did not report receiving the program, we assume WTP of \$0. We use the August 2021 workforce survey's estimate of $\alpha = 0.32$ (Table 1).

Finally, we need to make two additional assumptions about taxation. The estimates in Table 7 suggest that, at least for those who answered the online survey and received the program in full, incomes increased by IDR 215,000 / month. In general, all households earning less than IDR 4.5 million / month are exempt from income tax, and all households who are in the informal sector do not pay income tax. We expect that this additional income would likely not be taxed (the mean reported income in our data is only about IDR 1,100,000 per month). If this income is consumed, the government would obtain the VAT tax of 12 percent on this consumption. We use $\tau = 12$ percent as our baseline tax rate assumption, but also report an alternative with $\tau = 0$. Finally, we

[33] Specifically, we randomly varied the price paid, i.e., respondents could choose between IDR 50,000-75,000 of phone credit or IDR 50,000 of e-money, to estimate a willingness to pay for e-money.

need to make an assumption about how long the income gains last y years). Our baseline assumption is they last for 1 year, but we calculate a MVPF under different assumptions. The total cost of the program to the government is IDR 3.4 million (IDR 2.4 million for the cash transfers and IDR 1 million for the training.)

The MVPF is therefore given by $MVPF = \frac{\alpha(2.4+WTPT*1)}{3.4-\tau y 0.215*12}$. With our baseline case of $\tau = 0.12$ and $y = 1$, this yields an MVPF of 0.28. On the low end, if the government collects no tax revenue (i.e. $\tau = 0$, if people do not purchase VAT-taxed goods), the MVPF would be 0.25. On the higher end, even if the income gains last 3 years, the MVPF only rises to 0.34. For those who received the program in full (i.e. for those people for whom $\alpha = 1$), the MVPF exceeds 1 if the income gains persist for at least 2.4 years. But, because so many people did not report receiving the program, the overall MVPF for the program remains about 0.3. [34]

Since we are able to measure willingness-to-pay for the training directly for program applicants who never received the program (i.e. the people who would be the compliers had they been randomly chosen to win the project), this "ex-ante" MVPF measure is likely the most accurate measure to use. However, in the presence of optimization frictions or information challenges (e.g. if people did not know their incomes would increase due to the training), one could alternatively use the earnings impacts from Table 7 to calculate the "ex-post" MVPF, that is, one could use the ex-post earnings impacts as the valuation of the training. This assumes that the earnings impacts are a pure wage effect, that they are caused by the training alone and not the cash (to avoid double-counting), and that there are no unobserved costs associated with those increased earnings (i.e. more hours worked, higher effort, costs to start a small business, which there likely were since indeed businesses were started). For all these reasons, the "ex-post" approach therefore reflects a lower bound, but it is useful to calculate it as an alternative. Denote by ω the monthly impact on earnings from Table 7, converted to millions of Rupiah. The "ex-post" MVPF is therefore given by $MVPF_{expost} = \frac{\alpha(2.4+12\omega y)}{3.4-\tau y 0.215*12}$. Using $\omega = 0.215$ and $y = 1$ yields an ex-post MVPF of 0.52.

[34] It is important to note that this analysis values any transfers not received by beneficiaries at 0. Of course, for those who did not receive the program, the money was presumably diverted somewhere. An alternative would be to value these diverted cash transfers at β and to continue to value the training for these recipients at 0, since presumably no one was actually trained. Doing so would yield an $MVPF = \frac{\alpha(2.4+WTPT*1)+(1-\alpha)(2.4\beta+0*1)}{3.4-\tau y 0.215*12}$. If we use $\beta = 0.5$, for example, so that transfers to diverted beneficiaries have a social value half of those to actual beneficiaries, we would obtain an MVPF of 0.54.

VI. CONCLUSION

Supporting those who experience labor market shocks in lower-income countries requires a different approach than in high income countries, due to high levels of informality and self-employment, and the government's resulting lack of information on employment status. We study a new program in Indonesia designed to provide a combination of this short-run assistance to those who need it, when they need it, along with skills training. Indonesia provided this on-demand employment assistance *at scale*: more than 11 million people were selected to enter the program the first two years. In doing so, Indonesia embraced a variety of new and innovative technologies—online application systems to simplify enrollment, online training platforms for customized job training, and fast e-wallets sign-ups to facilitate payments for those who are unbanked.

For those who likely received the program, our online survey suggests that the program seems to have improved consumption smoothing and increased employment outcomes. These results suggest that this type of approach has potential *if* it can be delivered in full to everyone. However, the first-order fact is that about 70 percent of people who received the program in the administrative data do not report receiving it at all in the independent national surveys. Some of this may have been due to people forgetting that they received the program or otherwise failing to report it in surveys. However, the suggestive evidence implies that much of this may have been due, in part, to the emergence of agents that registered people for the program, both bypassing the training for beneficiaries and reducing cash payments made. Perhaps, as a result, on net, we find no overall observable treatment impacts on either the consumption or employment of beneficiaries.

The results suggest that the innovative design features that allowed this program to respond flexibly to shocks in a high informality environment may have also led to its challenges. For example, people who did not have internet access – and hence would have needed help applying – are more likely to fail to report receiving the program in government surveys despite being marked as receiving it in the administrative data. As digital social protection programs continue to expand globally, our findings offer guidance on how to maximize inclusion, integrity, and impact in complex labor market settings. Ensuring that programs remain accessible while strengthening accountability and delivery fidelity is key to realizing the full potential of such innovations.

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Tables

Table 1: Comparing Take-Up in Administrative and Survey Data

	Workforce Survey Wave (Individual Level)			Welfare Survey Wave (HH Level)			
	Aug '20	Feb '21	Aug '21	Sep '20	Mar '21	Sep '21	Mar '22
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Kartu Prakerja Recipient Counts							
Win in Administrative Data	537,003	5,419,525	8,213,904	1,657,837	4,456,168	7,111,666	8,624,841
Report Selected in Survey Data	448,955	2,532,916	2,619,337	1,296,844	978,122	1,780,610	2,475,215
Survey Winners/Admin Winners (%)	84	47	32	78	22	25	29
Panel B: Social Program Recipient Counts							
BPNT Recipient Count							
Admin Data				16,647,037	15,149,570	13,897,886	18,408,804
Welfare Survey Data				14,632,533	11,844,216	10,927,532	14,428,715
(Welfare Survey/Admin) Data (%)				88	78	79	78
PKH Recipient Count							
Admin Data				9,545,093	9,545,093	9,552,714	9,804,292
Welfare Survey Data				7,018,475	9,074,683	9,953,896	9,851,002
(Welfare Survey/Admin) Data (%)				74	95	104	100
PIP Recipient Count (Individual)							
Admin Data					11,492,260	11,492,260	12,358,370
Welfare Survey Data					9,482,047	9,109,733	8,996,831
(Welfare Survey/Admin) Data (%)					83	79	73
BLT-DD Recipient Count							
Admin Data				8,040,000		8,040,000	7,500,000
Welfare Survey Data				8,854,359		9,665,715	6,825,468
(Welfare Survey/Admin) Data (%)				110		120	91
Batches	1-3	1-11	1-17	1-5	1-11	1-18	1-22

Notes: Panel A compares the number of Prakerja winners in the National Sample Surveys (weighted by population frequency weights) with the winners reported in the administrative data for batches before each survey round was conducted. Panel B compares recipients reported in the survey data with each program's respective administrative data.

Table 2: Program and Training Take-Up, National Surveys

	Workforce Survey Wave				Welfare Survey Wave				
	Stacked	Aug '20	Feb '21	Aug '21	Stacked	Sep '20	Mar '21	Sep '21	Mar '22
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Win in administrative data									
Win In Batch	0.630***	0.946***	0.728***	0.595***	0.606***	0.756***	0.735***	0.593***	0.554***
	(0.003)	(0.004)	(0.007)	(0.003)	(0.002)	(0.016)	(0.003)	(0.006)	(0.003)
Observations	156,812	6,640	18,300	131,872	290,474	2,478	73,762	33,671	180,563
Mean (Control)	0.232	0.035	0.147	0.253	0.300	0.239	0.141	0.308	0.362
Panel B: Report selected in survey data									
Ever Win	0.223***	0.254***	0.296***	0.206***	0.124***	0.123***	0.107***	0.075***	0.142***
	(0.005)	(0.014)	(0.011)	(0.005)	(0.003)	(0.028)	(0.005)	(0.011)	(0.004)
Observations	156,812	6,640	18,300	131,872	290,474	2,478	73,762	33,671	180,563
Mean (Control)	0.010	0.031	0.009	0.009	0.038	0.056	0.062	0.085	0.016
Panel C: Ever Received Training Certificate									
Ever Win	0.048***	0.067***	0.053***	0.045***					
	(0.005)	(0.016)	(0.012)	(0.005)					
Observations	156,812	6,640	18,300	131,872					
Mean (Control)	0.152	0.297	0.203	0.135					
Batches	2-14, 16-17	2-3	2-11	2-14, 16-17	2-14, 16-22	2-5	2-11	2-14, 16-18	2-14, 16-22

Notes: In this table, we examine Kartu Prakerja take-up. The sample includes individuals matched between each respective national sample survey and the administrative data for the batches that closed prior to the survey. An observation is a person-batch for each survey round; it is person-batch-survey for the stacked analysis. Each cell represents a coefficient estimate from a separate regression estimated using Equation (2). Panel A provides OLS estimates. In Panels B and C, we instrument "treated before survey" in the administrative data with treatment status in batch. All regressions include controls for stratum P(win) × batch, as well as demographics (education, gender, and urban) × batch; the controls in the stacked regressions are also interacted with survey wave. SEs are clustered by individual. $* \ p < 0.1$, $* * \ p < 0.05$, $* * * \ p < 0.01$.

Table 3: Summary Statistics by Report Won Prakerja

Variable	Workforce Survey			Welfare Survey		
	Report Won (1)	Report Didn't Win (2)	Difference (3)	Report Won (4)	Report Didn't Win (5)	Difference (6)
Age	32.92	35.60	2.544***	33.84	35.38	1.481***
	(10.16)	(12.47)	(0.147)	(10.49)	(11.94)	(0.104)
Female	0.48	0.48	0.003	0.49	0.49	-0.006
	(0.50)	(0.50)	(0.007)	(0.50)	(0.50)	(0.005)
Urban (5 yrs ago)	0.40	0.36	-0.048***	0.31	0.30	-0.009*
	(0.49)	(0.48)	(0.013)	(0.46)	(0.46)	(0.005)
Java (5 yrs ago)	0.24	0.28	0.039***	0.19	0.23	0.044***
	(0.43)	(0.45)	(0.012)	(0.39)	(0.42)	(0.004)
Years of School	11.48	10.42	-0.917***	11.50	11.07	-0.426***
	(3.24)	(3.38)	(0.045)	(3.37)	(3.43)	(0.033)
Severe Disability	0.00	0.00	0.002***	0.01	0.01	0.001
	(0.04)	(0.06)	(0.001)	(0.10)	(0.10)	(0.001)
HH Size	4.35	4.31	-0.038*	4.35	4.32	-0.033**
	(1.66)	(1.67)	(0.023)	(1.64)	(1.68)	(0.016)
Num Applications	1.86	1.92	0.032*	2.00	1.99	-0.025*
	(1.38)	(1.49)	(0.018)	(1.61)	(1.63)	(0.015)
Num Children Under 5 in HH	0.42	0.33	-0.094***			
	(0.62)	(0.57)	(0.008)			
Weekly Hours Worked Pre-Covid	11.76	9.77	-2.746***			
	(21.33)	(19.70)	(0.365)			
Num Children Under 18 in HH				1.36	1.28	-0.081***
				(1.09)	(1.14)	(0.011)
Uses Internet				0.85	0.79	-0.060***
				(0.36)	(0.41)	(0.004)
Observations	8,893	26,234	35,127	13,880	60,964	74,844
Batches	2-14, 16-17	2-14, 16-17	2-14, 16-17	2-14, 16-22	2-14, 16-22	2-14, 16-22

Notes: In this table, we compare the mean demographic characteristics of those who report winning in the national sample surveys (Columns 1 and 4) with those who do not report winning (Columns 2 and 5) in the surveys among individuals who won in the administrative data; Columns 3 and 6 present the differences across groups, controlling for survey wave. $* \ p < 0.1$, $* * \ p < 0.05$, $* * * \ p < 0.01$.

Table 4: Program Targeting on Shocks and Demographics

	Win by Batch 22		Win by Batch 5		Report Win		Reapply	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Consumption per Capita	0.0042*	-0.0081***	0.0013**	-0.0017**	0.0020***	-0.00049	-0.013***	-0.011***
	(0.002)	(0.003)	(0.0005)	(0.0007)	(0.0006)	(0.0007)	(0.004)	(0.004)
Employed	0.0018	-0.0049	0.0013	-0.00030	0.0028***	0.0015*	-0.014**	-0.016***
	(0.0065)	(0.0073)	(0.00091)	(0.00086)	(0.00089)	(0.00085)	(0.0056)	(0.0053)
Hospitalized Last Yr (HH)	0.0060***	0.0042**	0.0015**	0.00092	0.00031	-0.00014	0.0075	0.0092
	(0.0018)	(0.0017)	(0.00069)	(0.00073)	(0.00070)	(0.00077)	(0.0069)	(0.0072)
Negative Consumption Shock	0.022**	0.010	0.0086***	0.0054**	0.0082***	0.0047***	-0.0020	0.0084
	(0.010)	(0.012)	(0.0021)	(0.0022)	(0.0013)	(0.0013)	(0.013)	(0.012)
Use Internet		0.041***		0.011***		0.0087***		-0.018**
		(0.0043)		(0.00095)		(0.00094)		(0.0071)
Receive Other Social Program		-0.038***		-0.0032***		-0.0023***		-0.084***
		(0.0054)		(0.00092)		(0.00047)		(0.027)
Observations	154,664	154,664	154,664	154,664	154,664	154,664	27,534	27,534
DV Mean	0.053	0.053	0.0074	0.0074	0.0074	0.0074	0.79	0.79
Multivariate	N	Y	N	Y	N	Y	N	Y
Batches	1-22	1-22	1-5	1-5	1-5	1-5	1-21	1-21

Notes: This table examines Kartu Prakerja's targeting using the March 2020 Welfare survey. In Columns 1-4, we examine how the characteristics affect the probability that an individual is an administrative winner; in columns 5-6, we examine how they affect whether one reports winning. In Columns 7-8, we examine how they affect the probability of re-applying after losing the lottery. Columns 1-6 include respondents to the March-Sep 2020 Welfare survey panel. Columns 7-8 include Prakerja applicants who matched between the March-Sep 2020 Welfare survey panel and the administrative data. Each column presents estimates from a separate regression. Columns 1–6 are estimated at the person level using Equation (3), while Columns 7–8 are estimated at the person-batch level using Equation (4). All columns include district FEs. Columns 7-8 also include batch FEs. SEs clustered at the Industry × Java level. $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$.

Table 5: Experimental Impacts of Kartu Prakerja in National Sample Surveys, IV Estimation

	Workforce Survey Wave				Welfare Survey Wave				
	Stacked	Aug '20	Feb '21	Aug '21	Stacked	Sep '20	Mar '21	Sep '21	Mar '22
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Consumption									
Total Consumption					-32.993	371.050	-63.032	2.267	-32.576
					(31.753)	(274.801)	(48.133)	(90.542)	(41.069)
					[5161.268]	[4808.161]	[5245.282]	[5204.079]	[5130.644]
Panel B: Employment									
Employed	0.001	0.011	0.002	0.000	-0.002	-0.014	-0.012*	-0.001	0.003
	(0.006)	(0.018)	(0.014)	(0.007)	(0.004)	(0.034)	(0.007)	(0.012)	(0.006)
	[0.623]	[0.572]	[0.620]	[0.625]	[0.641]	[0.689]	[0.658]	[0.652]	[0.630]
Self Employed/Business Owner	0.000	0.029*	0.008	-0.004	-0.005	0.005	-0.011*	0.008	-0.004
	(0.006)	(0.015)	(0.013)	(0.006)	(0.004)	(0.029)	(0.006)	(0.012)	(0.006)
	[0.271]	[0.201]	[0.261]	[0.277]	[0.283]	[0.214]	[0.263]	[0.275]	[0.291]
Hours Worked	-0.032	-0.210	0.423	-0.099	-0.116		-0.669*	-0.227	0.203
	(0.312)	(0.866)	(0.710)	(0.328)	(0.234)		(0.360)	(0.638)	(0.305)
	[23.798]	[22.076]	[24.781]	[23.726]	[28.592]		[29.628]	[27.491]	[28.331]
Hourly Wage	-0.098	0.427	-0.569	-0.066					
	(0.148)	(0.389)	(0.372)	(0.158)					
	[6.775]	[6.035]	[7.338]	[6.737]					
Earnings	-17.939	40.252	-17.560	-24.067					
	(18.115)	(50.447)	(40.489)	(19.137)					
	[1014.048]	[945.231]	[1013.935]	[1020.425]					
Looking for Job	0.002	0.015	0.010	-0.001					
	(0.004)	(0.016)	(0.010)	(0.004)					
	[0.105]	[0.239]	[0.137]	[0.099]					
Prepping New Business	-0.001	-0.009	-0.002	0.000					
	(0.002)	(0.006)	(0.004)	(0.002)					
	[0.015]	[0.036]	[0.017]	[0.013]					
Observations	156,812	6,640	18,300	131,872	290,474	2,478	73,762	33,671	180,563
Batches	2-14, 16-17	2-3	2-11	2-14, 16-17	2-14, 16-22	2-5	2-11	2-14, 16-18	2-14, 16-22

Notes: In this table, we examine the effect of winning Kartu Prakerja on consumption and employment outcomes in seven rounds of the nationally representative sample surveys. The sample in each column includes individuals matched between each respective national sample survey and the administrative data for the batches that closed prior to the survey. An observation is a person-batch for each survey round; it is a person-batch-survey for the stacked analysis. Each cell represents a coefficient estimate from a separate regression estimated using Equation (2). We instrument "treated before survey" in the administrative data with treatment status in batch. All regressions include controls for stratum P(win) × batch, as well as demographics (education, gender, and urban) × batch; the controls in the stacked regressions are also interacted with survey wave. SEs are clustered by individual. The control complier mean is provided in brackets. $* \ p < 0.1$, $* * \ p < 0.05$, $* * * \ p < 0.01$.

Table 6: Program and Training Take-Up, 2022 Online Survey

Panel A: First Stage	
Win in Administrative Data	0.510***
	(0.011)
	[0.490]
Panel B: IV	
Report Selected in Survey data	0.790***
	(0.016)
	[0.134]
Took a course	0.329***
	(0.025)
	[0.200]
Hours of Training	6.110***
	(1.698)
	[7.121]
Observations	7,234

Notes: This table examines the effect of winning Prakerja on program take-up and training outcomes in the 2022 online survey. The sample includes individuals in batches 18-22. Observations are at the individual-batch level. Panel A presents the first-stage regression. Each cell in Panel B represents a coefficient estimate from a separate regression estimated using Equation (2), where we instrument whether an individual was ever treated by batch 47 in the administrative data with treatment status in batch. All regressions include controls for stratum P(win) $\times$ batch, as well as demographics (education, gender, and urban) $\times$ batch. SEs are clustered by individual. The control complier mean is provided in brackets. $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 7: Experimental Impacts of Kartu Prakerja in 2022 Online Survey, IV Estimation

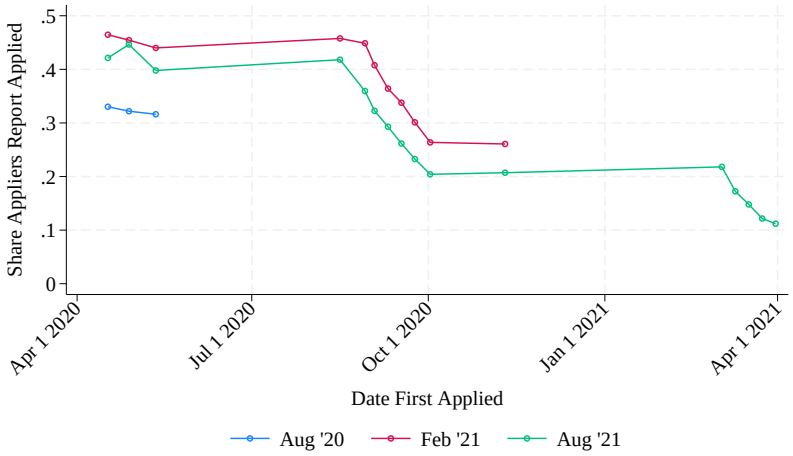
	No Baseline Controls (1)	Includes Baseline Controls (2)
Panel A: Cons. & Cons. Smoothing		
Total Consumption	-32.274	-86.736
	(113.267)	(109.580)
	[3090.066]	[3144.528]
Life Satisfaction	0.303***	0.288***
	(0.088)	(0.085)
	[4.619]	[4.634]
Buy Any Asset	0.039**	0.037*
	(0.019)	(0.019)
	[0.133]	[0.135]
Sell Any Asset	-0.025	-0.034
	(0.026)	(0.026)
	[0.423]	[0.432]
Net Asset Purchases	397.917	532.063*
	(287.407)	(285.085)
	[-440.029]	[-574.175]
Taken Additional Loans	-0.001	-0.003
	(0.026)	(0.024)
	[0.357]	[0.359]
Received gifts	-0.007	-0.010
	(0.021)	(0.020)
	[0.198]	[0.201]
Gave gifts	0.024*	0.021
	(0.015)	(0.014)
	[0.068]	[0.071]
Moved	0.005	0.005
	(0.004)	(0.004)
	[0.002]	[0.002]
Panel B: Employment		
Employed	0.001	0.003
	(0.026)	(0.026)
	[0.597]	[0.595]
Self Employed/Business Owner	0.066**	0.063**
	(0.031)	(0.030)
	[0.334]	[0.337]
Hourly Wage	2.056	1.918
	(1.347)	(1.384)
	[5.247]	[5.385]
Total Earnings	215.329**	214.696**
	(97.444)	(96.220)
	[1139.666]	[1140.299]
Searching for Job (Last Week)	-0.098***	-0.097***
	(0.026)	(0.026)
	[0.438]	[0.436]
HH Prepping New Business	0.030	0.031
	(0.020)	(0.023)
	[0.140]	[0.139]
Job Satisfaction	0.048***	0.039**
	(0.015)	(0.016)
	[0.598]	[0.608]
Business Ability Confidence	-0.024	-0.039
	(0.039)	(0.037)
	[2.989]	[3.004]
Observations	7,228	7,222

Notes: This table examines the effect of winning Prakerja on consumption and employment outcomes in the 2022 online survey. The sample includes individuals in batches 18-22. Observations are at the individual-batch level. Each cell represents a coefficient estimate from a separate regression estimated using Equation (2). We instrument whether an individual was ever treated by batch 47 in the administrative data with treatment status in batch. All regressions include controls for stratum P(win) × batch, as well as demographics (education, gender, and urban) × batch. SEs are clustered by individual. The control complier mean is provided in brackets. $* \ p < 0.1$, $* * \ p < 0.05$, $* * * \ p < 0.01$.

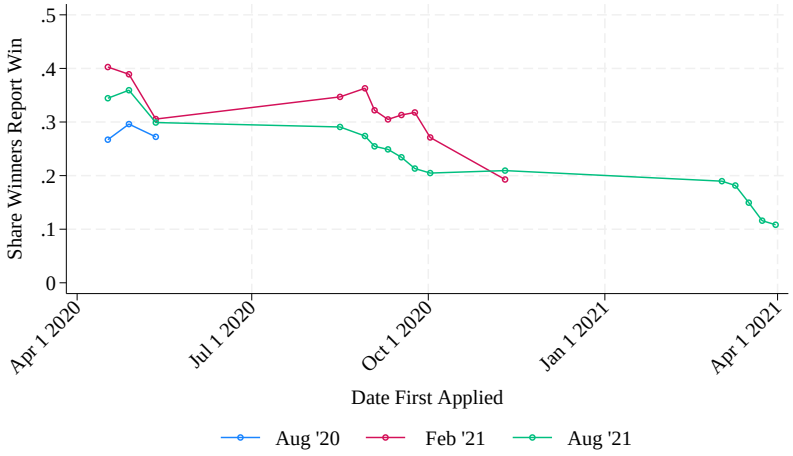
Figures

Figure 1: Report Apply/Win in National Surveys, by Date First Applied and Survey Wave

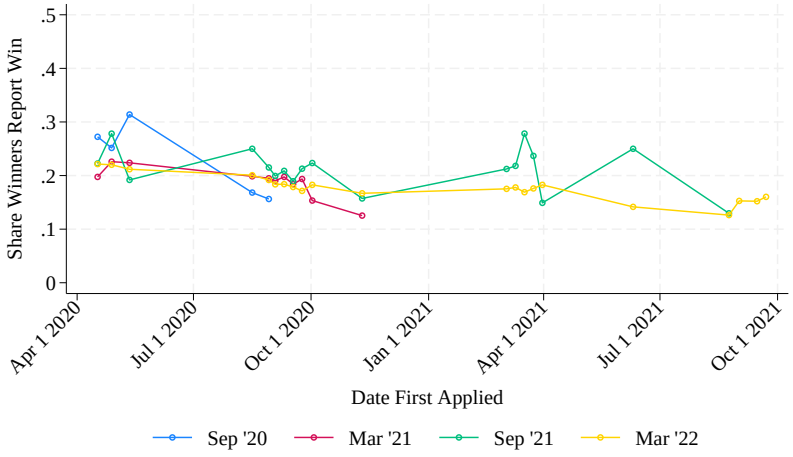
Panel A: Workforce Survey, Report Applied



Panel B: Workforce Survey, Report Win



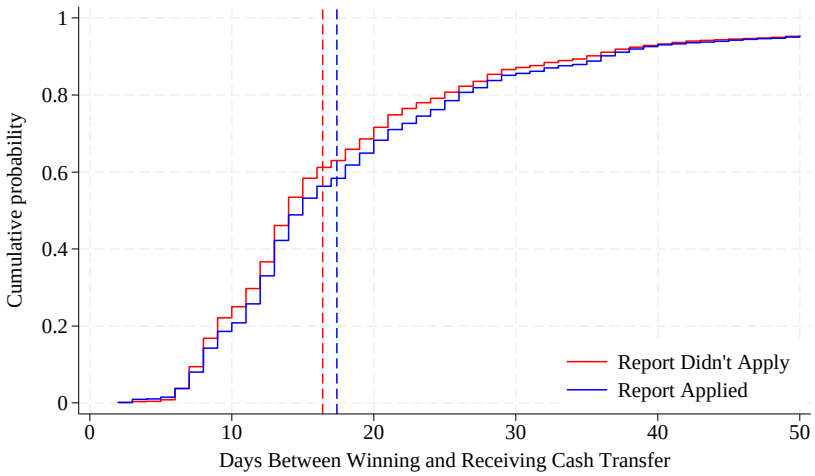
Panel C: Welfare Survey, Report Win



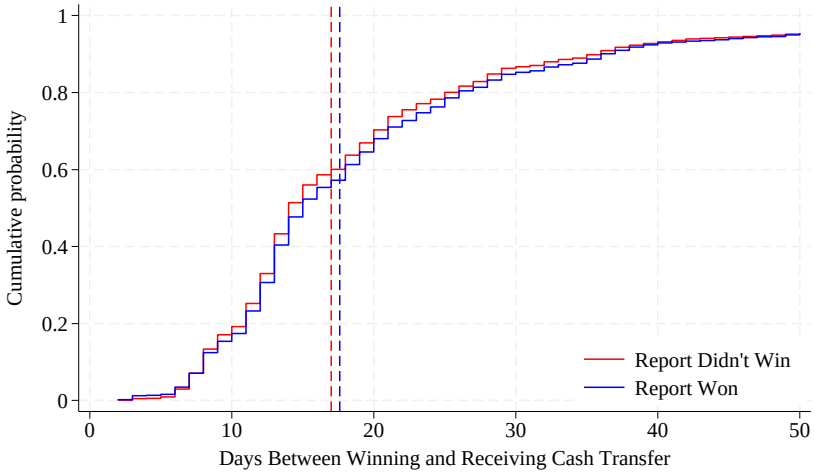
Notes: Panel A graphs the fraction of individuals who applied in the administrative data who report applying for Kartu Prakerja in each Workforce Survey wave, by the date they first applied. Panel B (C) graphs the fraction who report winning in each Workforce Survey (Welfare Survey) wave among those who report winning in the administrative data. We drop batches 17 and 22 for Panels B and C, respectively, because of low observation counts.

Figure 2: CDF of Days between Winning and Receiving Cash, by Report Apply/Win

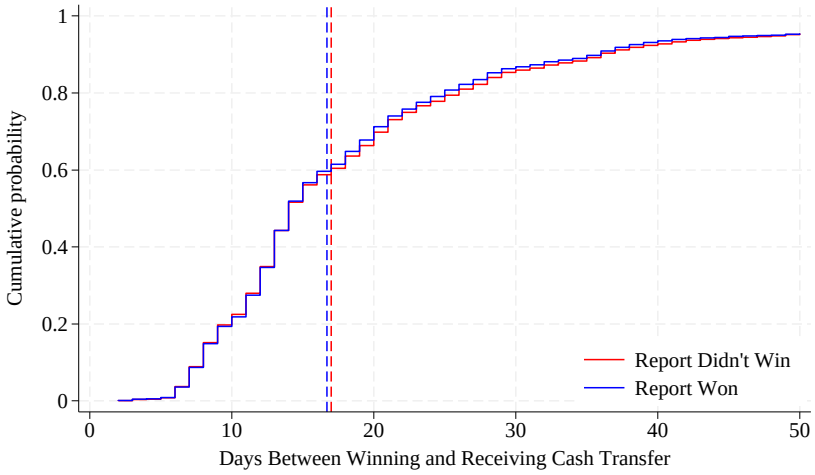
Panel A: Workforce Survey, Report Applied



Panel B: Workforce Survey, Report Won



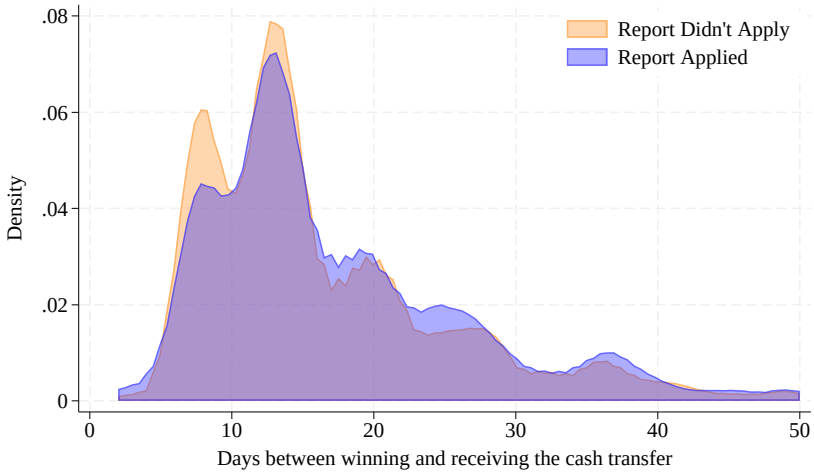
Panel C: Welfare Survey, Report Won



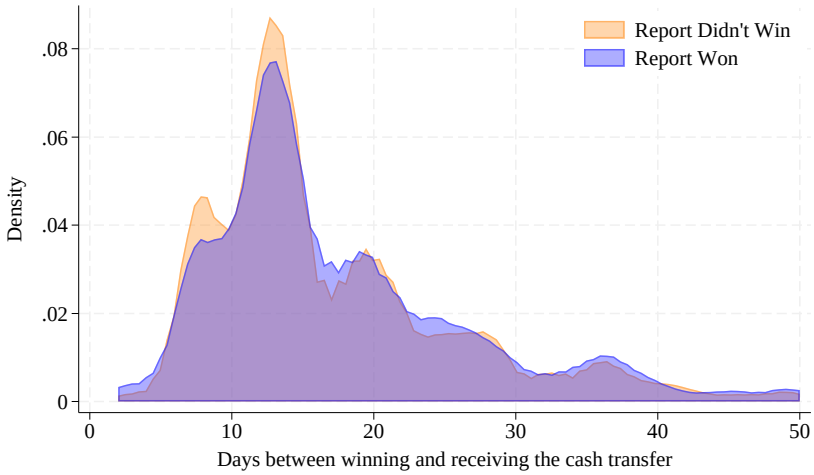
Notes: Panel A plots the CDF of the number of days between winning and receiving the first cash payment, by whether individuals report applying or not (Panel A), for those who we observe winning in the administrative data. Panel B and C plot this CDF by whether or not individuals report winning.

Figure 3: PDF of Days between Winning and Receiving Cash, by Report Apply/Win

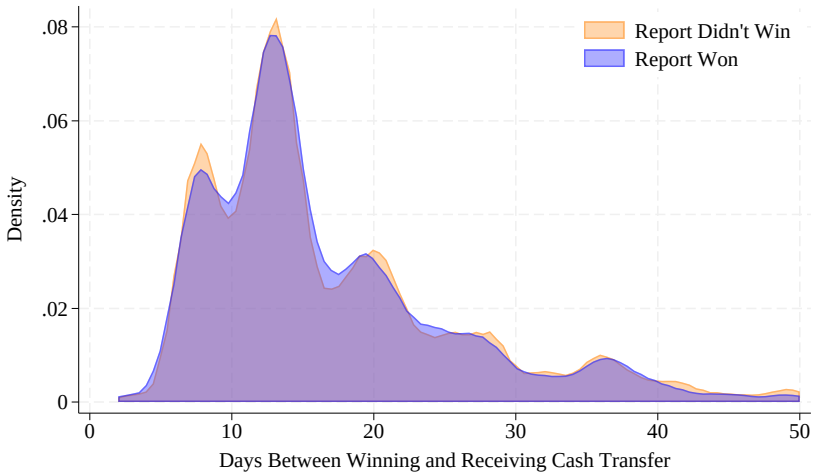
Panel A: Workforce Survey, Report Applied



Panel B: Workforce Survey, Report Won



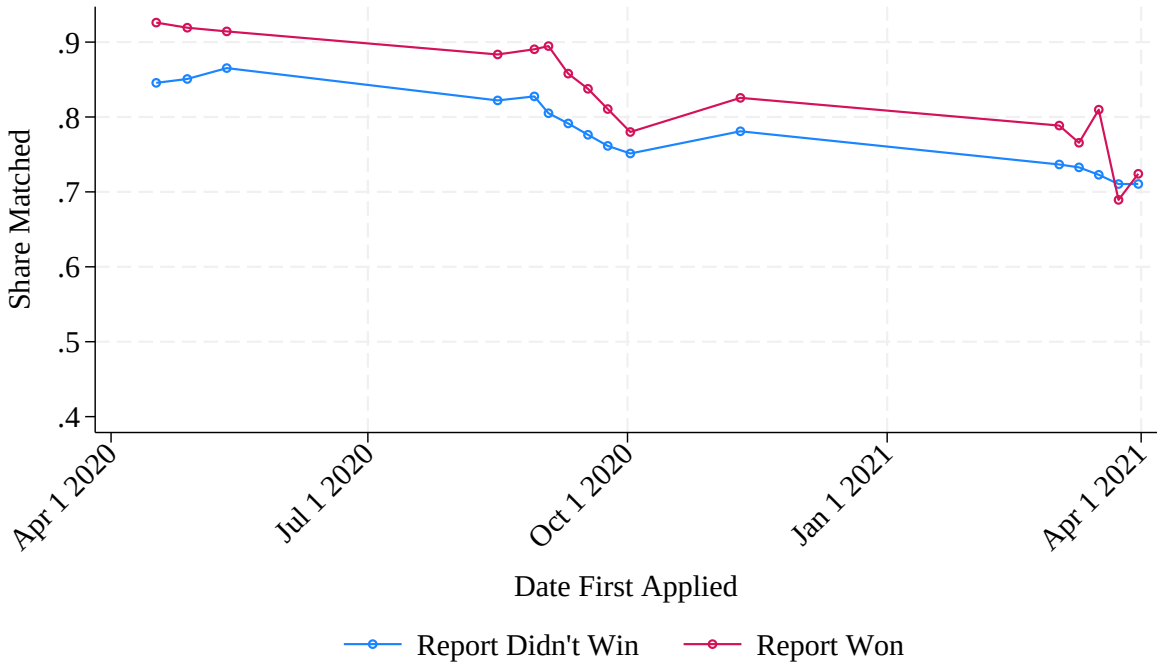
Panel C: Welfare Survey, Report Won



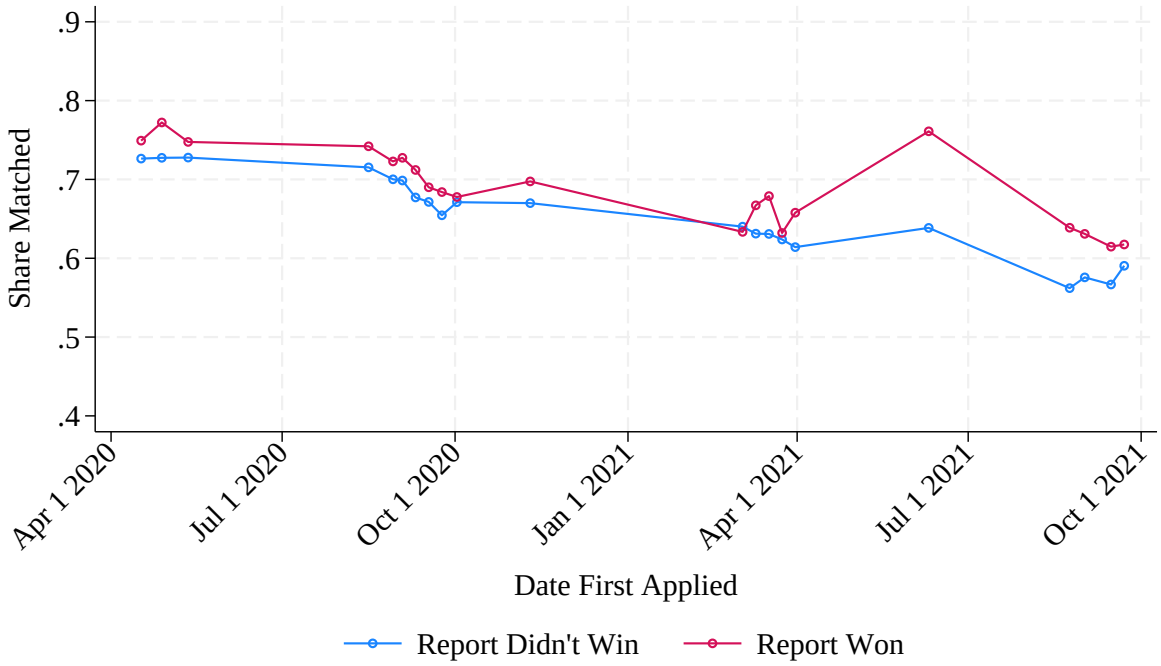
Notes: This figure plots the fraction of administrative winners whose demographic characteristics match between the survey and administrative data, by whether individuals report winning in the survey and by the dates they first applied.

Figure 4: Demographic Match in National Surveys and Admin, by Date First Applied and Report Won

Panel A: Workforce Survey



Panel B: Welfare Survey



Notes: Samples includes all individuals in the Workforce Survey or Welfare Survey who match with the administrative data. Date first applied is the date the results of the batch-lottery were announced.