DISCUSSION PAPER SERIES

Welfare Effects of Unemployment Benefits When Informality Is High

Hannah Liepmann
Clemente Pignatti

DISCUSSION PAPER SERIES

Welfare Effects of Unemployment Benefits When Informality Is High

Hannah Liepmann
International Labour Organization and IZA

Clemente Pignatti
International Labour Organization and University of Geneva

ABSTRACT

Welfare Effects of Unemployment Benefits When Informality Is High*

We analyze for the first time the welfare effects of unemployment benefits (UBs) in a context of high informality, exploiting matched administrative and survey data with individual-level information on UB receipt, formal and informal employment, wages and consumption. Using a difference-in-differences approach, we find that dismissal from a formal job causes a large drop in consumption, which is between three to six times larger than estimates for developed economies. This is generated by a permanent shift of UB recipients towards informal employment, where they earn substantially lower wages. We then exploit a kink in benefits and show that more generous UBs delay program exit through a substitution of formal with informal employment. However, the disincentive effects are small and short-lived. Because of the high insurance value and the low efficiency costs, welfare effects from increasing UBs are positive for a range of values of the coefficient of relative risk aversion.

Corresponding author:
Hannah Liepmann
International Labour Organization
Geneva
Switzerland
E-mail: liepmann@ilo.org

1 Introduction

Unemployment benefits (UBs) help laid-off individuals smooth consumption (insurance value), but they can also increase the duration of UB receipt and delay re-employment (efficiency costs). This tradeoff determines the welfare effects of UBs. Outside of high-income countries, the prevalence of informal employment means that individuals might receive UBs while also working informally. At the same time, the consumption drop at layoff might be higher in these contexts, especially if finding a new formal job is difficult and laid-off workers experience a high risk of poverty. Yet, while welfare effects of UBs have been studied in depth for high-income countries (Schmieder and von Wachter, 2016), fewer studies have focused on low- and middle-income countries, analyzing the insurance value (Gerard and Naritomi, 2021) or the efficiency costs of UBs (Britto, 2021; Gerard and Gonzaga, 2021).[1] In this paper, we jointly analyze the insurance value and the efficiency costs of UBs in a context of high informality, estimating for the first time overall welfare effects of UBs outside of a high-income country context.

Compared to other studies on UBs in both high- and middle-income countries, we additionally investigate how the possibility of working informally affects the insurance value and efficiency costs of UBs. A unique feature of our study is that we analyze data on employment, wages and consumption for all UB recipients independently from their re-employment status, including informal re-employment. Considering the nature of informal re-employment could be crucial for understanding how given levels of the welfare gains and losses of UBs materialize (see also Meghir et al., 2015; Ulyssea, 2018).[2] To understand why, suppose that informal jobs are relatively easy to find and are close substitutes to formal ones. In this scenario, UB recipients have high incentives to substitute formal with informal employment, while remaining eligible for UBs and maintaining adequate consumption levels. Thus, the efficiency costs of UBs would likely outweigh their insurance value, and this effect would be larger than in labor markets where informality is less relevant. The exact opposite would apply if informal jobs are not easily accessible or do not provide wages that are comparable to those of formal jobs.

Our key result is that the welfare effects of UBs are positive and comparatively large in a context with high informality and that accounting for the nature of informal jobs is decisive to explaining this finding. The positive welfare effects stem from a pronounced consumption drop at layoff, which is between three to six times larger than existing estimates from high-income countries, and relatively small efficiency costs, which are at the lower bound of elasticity

---

[1]About half of all middle-income countries have a public UB schemes (Asenjo et al., 2019; ILO, 2021).

[2]Note that data on informal employment is not strictly needed for assessing welfare effects, as the insurance value is determined by the average consumption drop at layoff among UB recipients and the efficiency costs depend on the length of UB receipt and the time until formal re-employment (Gerard and Gonzaga, 2021).

estimates from high-income countries. This is true although we document a large shift towards informal employment among UB recipients: one year after job loss, around 40 per cent of them work informally while also receiving UBs. However, the shift to informality is a response to the layoff event, while the informality response to UB generosity is relatively small. Exploiting individual-level data on informal employment is key for reconciling these findings. In particular, individuals who move to informality earn less than those who are formally re-employed and experience the largest drop in consumption. This means that informal jobs are poor substitutes of formal ones and workers seem to accept them only in the absence of better alternatives.

Our study context is the UB scheme of Mauritius, a country in the Indian Ocean with a population of 1.3 million, a level of economic development comparable to Argentina, Chile or Uruguay and 56.2 per cent of the employed population working informally. An important advantage of our setting is that it allows us to exploit a unique database, which we constructed by merging different types of individual-level administrative and survey data. The starting point are the program records of the universe of UB recipients collected at the time of job loss. We merged these with participants' full social security biographies before and after job loss. Full social security records are also available for a large sample of the Mauritian formal labor force, corresponding to around 90 per cent of those formally employed in the country. We matched each of the two panel databases (i.e. for UB recipients and the sample of the formal labor force) with the Mauritian household survey. This means that for a representative sub-sample of the two populations, namely the sub-sample interviewed in the household survey, we additionally have information on informal employment and wages as well as household transfers and consumption.

To preview our analysis in more detail, we first study the insurance value of UBs, using a difference-in-differences (DiD) approach on the matched observations to analyze the implications of losing a formal job on consumption. We center the analysis 36 months around job loss and compare UB recipients who enter unemployment after at least 36 months of tenure in a formal job (i.e. treatment group) with a sample of individuals who constantly held the same formal job for 72 months (i.e. control group). To increase comparability between the two samples, we re-weight observations based on individual characteristics. We find that expenditures for both durable and non-durable consumption drop significantly in households of dismissed workers. Three years after job loss, total household expenditures are still 31.4 per cent lower than before dismissal and consumption expenditures are still 32.8 per cent lower. These effects are substantially larger than those observed in developed economies (Schmieder and von Wachter, 2016). Individuals who find a new informal job experience the sharpest drop in expenditures (i.e. larger than the one experienced by both those that find a formal job and those who remain unemployed).

Studying the mechanisms behind the large consumption drop, as part of the DiD-analysis we also look at the effects of job-loss on employment, wages and transfers. We find that overall employment returns to pre-layoff levels two years after jobloss, but there is a permanent shift in the type of job held. Even three years after job loss, those who have lost a formal job are 38.2 per cent more likely to hold an informal job. Similarly, individual monthly wages are still 48.0 per cent below the pre-layoff level three years after job loss. This is mostly because the majority of UB recipients is re-employed in informal jobs that on average pay less than formal ones, after controlling for observable characteristics. Finally, while households of laid-off individuals experience a small and transitory increase in public transfers due to the receipt of UBs, private transfers (e.g. from relatives) are largely unaffected and other forms of public transfers fail to materialize when eligibility to UBs terminates.

Subsequently, we turn to estimating the efficiency costs of UBs. We rely on the fact that benefit levels in Mauritius are subject to upper and lower bounds and adopt a regression kink (RK) analysis as in Card et al. (2015, 2017) and Landais (2015), among others. To begin with, we build on Gerard and Gonzaga (2021) who demonstrate that estimating efficiency costs in contexts of high informality only requires knowing the effect on the length of UB receipt and time until formal re-employment (i.e. we do not need to know the effect on informal employment). This is good news, as we observe these indicators for the entire sample of UB recipients from social security records. Focusing on the upper bound, we find an elasticity of length of UB receipt with respect to benefit levels equal to 0.27 (s.e.=0.11). Higher UBs also decrease the likelihood of formal employment in the months following job loss, with an elasticity of the duration without a formal job to benefit levels being equal to 0.28 (s.e.=0.14). These estimates are below the majority of those for developed economies (Schmieder and von Wachter, 2016), despite the fact that informality rates are low in those countries.

Within the RK analysis, we then study the role that informal jobs play in determining the estimated efficiency costs. To do so, we rely on the representative sub-sample of UB recipients who are also observed in the household survey. For them, we can examine the effects of UB generosity on informal employment. Close to the kink, the number of observations in the matched sub-sample is unfortunately too small for conducting the RK analysis for informal employment. For this reason, we revert to imputing informal employment for our entire sample following the literature that predicts non-durable consumption based on food expenditure (Blundell et al., 2008; Crossley et al., 2020). Specifically, we exploit that the likelihood of being informally employed changes across time (i.e., relative to the month of job loss) and that it depends on whether an individual is formally employed (where the residual category is non-employment).

The results of the imputation show that we are able to replicate well the pattern of informal employment observed in the survey data. Applying the imputation to our RK analysis, we find that more generous UBs lead to an increase in informal employment. The effect is, however, small in magnitude and disappears one year after job loss. This is consistent with our earlier finding that informal jobs pay low wages, such that the incentives to move into informal employment when UBs are increased, are relatively small.

In the last part of the paper, we bring together the results from the DiD and RK analyses to estimate the welfare effects of increasing UB levels. To obtain the insurance value, we slightly readapt our DiD estimates. We follow Landais and Spinnewijn (2020) and Kolsrud et al. (2018) and estimate the flow drop in consumption around the unemployment event (i.e. at the monthly level) exploiting the fact that at the time of the interview in the household survey individuals will have spent a different number of months receiving UBs. This approach delivers an estimate of the average consumption drop at unemployment of 28.1 per cent. We then take the preferred elasticity estimates for the effects of UB levels from the RK analysis and obtain an average efficiency cost of 0.28 for a one-unit increase in UBs (i.e., the cost of the increase in benefit levels is equal to 1.28 once behavioral responses are taken into account). Comparing the insurance value with the efficiency cost, we find that welfare effects from increasing benefit levels are positive for any value of the coefficient of relative risk aversion above one. This finding suggests that in a labor market with high informality, having a formal job represents a key advantage and providing insurance against its loss is a sensible policy choice.

This paper builds on previous evidence on UB schemes in developing and emerging economies.[3] Two papers examine the efficiency costs of UBs. First, Gerard and Gonzaga (2021) analyze the effects of extending UB duration in Brazil and find that longer entitlements increase length of UB receipt and delay formal re-employment, but this is mostly driven by a mechanical effect (i.e. formal jobs are difficult to find) rather than a change in incentives. Second, Britto (2021) compares the labor supply responses to the generosity of severance payments with those of an extension of UBs in Brazil and shows that the latter is more detrimental to formal labor supply in the short-run, but the effects become similar over the medium-term. A third study focuses on the insurance value: Gerard and Naritomi (2021) find that individuals increase spending when they receive lump-sum severance payments despite facing a long-run drop in earnings. We

---

[3]A related literature examines the labor supply responses to the extension of social protection schemes (Azuara and Marinescu, 2013; Bergolo and Cruces, 2014; Bosch and Campos-Vazquez, 2014; Camacho et al., 2014) or the receipt of cash transfers (Bergolo and Cruces, 2021; Bosch and Schady, 2019; Galasso and Ravallion, 2004; Galiani and McEwan, 2013; Garganta and Gasparini, 2015; Skoufias and Maro, 2008), with some of these contributions looking separately at the effects on formal and informal employment. Compared to these studies, we look at an UB scheme for which we can expect that labor supply responses differ given that program eligibility is conditional on previous labor market participation and program exit is dependent on being re-employed.

contribute to this literature by examining both the insurance value and efficiency costs of UBs for the same population.[4] This allows us to compute overall welfare effects of UBs for the first time in a context of high informality.[5] Additionally, we refine the existing knowledge on welfare gains and losses, even when considered independently, by observing UB recipients not only when they are formally employed, but also when they are informally employed or non-employed.[6] This allows us to characterize how the availability of informal jobs affects UB receipt, re-employment, wages, consumption, and, ultimately, the welfare effects of UBs.

Having access to rich worker-level data on informality allows us to also contribute to a debate on the nature of the informal labor market. According to the first stylized view, workers voluntarily choose informal employment based on their comparative advantages. A second view of informality argues that workers generally prefer formal jobs, but have difficulty accessing these. These two views on informality are no longer seen as a clear dichotomy (see La Porta and Shleifer, 2014; Meghir et al., 2015; Ulyssea, 2018). Accordingly, studies analyzing workers' transitions show that workers move between formal and informal jobs, but that wages are higher in formal ones (Botelho and Ponczek (2011); Bosch and Esteban-Pretel (2012); McCaig and Pavcnik (2015); Díaz et al. (2018); see also the review by Ulyssea (2020)). We contribute to this literature by providing detailed evidence on the transitions of workers dismissed from a formal job. We find that formally displaced workers rapidly move to informal employment. However, they receive lower wages, have reduced consumption levels and do not return to formal jobs even long after the end of UB eligibility. This suggests that the transitions of displaced workers are asymmetric and that informal jobs are poor substitutes of formal ones.

Finally, the findings of the paper are informative to a few branches of the literature at the intersection between public and development economics. Previous studies on UBs in developed economies have found that the efficiency costs of benefit generosity are lower during downturns (Kroft and Notowidigno, 2016; Schmieder et al., 2012). This is consistent with our result of a low elasticity of the duration without a formal job to benefit levels in a context characterized by high informality rates. Although smaller than the literature on the efficiency costs

---

[4]Other characteristics differentiate our paper from these previous studies. Differently from Gerard and Gonzaga (2021) and Britto (2021), we examine the effects of UB levels rather than UB duration, where the labor supply responses might be different (Schmieder and von Wachter, 2016). Differently from Gerard and Naritomi (2021), we study the consumption drop around layoff for individuals who do not receive any concurrent positive income shock, which is preferable for analyzing the insurance value of UBs (Landais and Spinnewijn, 2020).

[5]González-Rozada and Ruffo (2016) study the UB scheme in Argentina and find that welfare would increase if UB levels were higher, but provided for a shorter time. However, their paper analyzes the tradeoff between UB level and duration, rather than the welfare effects of UBs (levels or duration) in general.

[6]In comparison, Gerard and Gonzaga (2021) compare survival rates out of formal employment for their sample of UB recipients with survival rates out of total employment for a sample of unemployed workers in the household survey and interpret the difference between the two as evidence of informality responses. Britto (2021) performs an imperfect matching based on clusters of observations between social security records and household survey data. Both studies do not characterize informal jobs in terms of their implications for earnings and consumption.

of UBs, different studies have also documented drops in consumption (Ganong and Noel, 2019; Gruber, 1997; Kolsrud et al., 2018; Landais and Spinnewijn, 2020) and earnings (for a review, see Couch and Placzek (2010)) at layoff. Our estimates are among the first ones from a context of high informality and they are significantly larger than those previously obtained, which can be explained by limited self-insurance and gaps in social safety nets. Finally, previous studies have investigated the cost of public policies in contexts characterized by low enforcement capacities and high levels of non-compliance (Carrillo et al., 2017; Naritomi, 2019). We contribute to this literature by measuring for the first time non-compliance in the context of an UB scheme outside developed countries and finding relatively low efficiency costs compared to the welfare gains.

The rest of the paper is organized as follows: section 2 provides the theoretical framework for the welfare analysis; section 3 describes the institutional context and the functioning of the UB scheme in Mauritius; section 4 introduces the data sources used in the analysis and presents descriptive evidence; section 5 includes the main results from the DiD analysis on changes in consumption expenditures, labor market status, wages, and transfers around job loss; section 6 presents the imputation procedure for informality and the results from the RK analysis on the effect of benefit generosity on length of UB receipt, formal and informal employment; section 7 brings together the two main parts of the analysis to estimate the welfare effects from increasing UB levels; and section 8 summarizes and concludes.

2 Theoretical framework

2.1 Insurance value and efficiency costs of UBs

Throughout the paper, we define the optimal UB level in terms of the tradeoff between the insurance value and the efficiency costs of UBs. The insurance value pertains to the role of UBs to smooth the potential drop in consumption at layoff. The efficiency costs arise due to moral hazard, whereby more generous UBs can result in delayed (formal) re-employment and longer UB receipt. We employ the job search model of Baily (1978) and Chetty (2008), as extended to labor markets with high informality by Gerard and Gonzaga (2021).

Gerard and Gonzaga (2021) show that both UB recipients who are informally re-employed and those who remain unemployed, can be treated as one group in the welfare analysis. This is based on two assumptions. First, informal workers do not pay payroll taxes and are not eligible to UBs upon layoff. Second, formally displaced workers continue receiving UBs when they

6

are informally re-employed, as this is not detected by the authorities.[7] Under these assumptions, the implications of informal re-employment are the same for welfare considerations (including public finance) as those of non-employment.[8]

Consider a representative worker i who is laid off from a formal job in discrete time at $t = 0$ and retires after T periods. When not formally employed, the worker optimizes search efforts for a formal job $(s_{i,t})$, which are normalized to equal the probability that the job search is successful. More intense job search implies a higher likelihood of finding a formal job, but also entails search costs $\psi(s_{i,t})$. The same worker receives UBs (b_t) for a maximum of $P < T$ periods. In parallel, she can work informally to additionally earn $l_{i,t}w_i$, where w_i is the informal wage and $l_{i,t} \geq 0$ is the amount of informal employment provided. $l_{i,t}$ is chosen by the worker and entails a job effort cost $\phi(l_{i,t})$ of working informally; where the case of $l_{i,t} = 0$ represents a non-employed UB recipient. The worker also chooses an optimal intertemporal consumption path, with consumption given by $c_{i,t}^u$; and has an increasing and concave utility function $u(.)$. Once the worker finds a formal job, she accepts it, up until T earns a fixed wage of w_f, no longer receives b_t, and pays taxes τ that contribute to financing the UB scheme. She decides about an optimal consumption path $(c_{i,t}^{fe})$ and has an increasing and concave utility function $v(.)$.

The social planner determines b_t and τ to maximize welfare W, which is given by workers' expected lifetime utility.[9] The planner's budget constraint equals $(T-D)\tau = Bb$, where D is the expected time spent outside of formal employment and B is the expected duration of benefit receipt. Increasing UB levels requires the social planner to account for a mechanical effect, with workers receiving higher benefits throughout the duration of B irrespective of any behavioral response. This is valued at the gap in marginal utilities between the non-formally employed and the formally employed. Additionally, the social planner considers the behavioral response stemming from the potential increase in the time until formal re-employment and the duration of UB receipt. Suppressing time and individual indices and normalizing terms, the marginal welfare effect of increasing b amounts to (see also Schmieder and von Wachter, 2016):

$$\frac{\partial W}{\partial b}\frac{1}{Bv'(c_{fe})} = \underbrace{\frac{u'(c_u) - v'(c_{fe})}{v'(c_{fe})}}_{Insurance\ Value} - \underbrace{\left(\eta_{B,b} + \eta_{D,b}\frac{D}{B}\frac{\tau}{b}\right)}_{Efficiency\ Costs} \tag{1}$$

---

[7]Both assumptions seem justified in our study context. While informal workers are in theory eligible for UBs in Mauritius, only three per cent of those losing an informal job actually enter the UB scheme (Liepmann and Pignatti, 2019). We also show in Section 4.2 below that virtually all those who are informally employed in the first year after job loss receive UBs.

[8]The presence of informal jobs can alter the incentives to work formally and thus affect both the insurance value and the efficiency costs. However, for welfare, only the margin of formal re-employment is relevant.

[9]In line with the empirical analysis, we focus on an increase in benefit levels (i.e. holding P constant).

where the normalization by $v'(c_{fe})$ expresses the marginal welfare effect in terms of a one unit increase in consumption of the formally employed, while the normalization by B expresses it in units of expected benefit duration. $\eta_{B,b}$ and $\eta_{D,b}$ are the elasticities of length of UB receipt and time until formal employment with respect to benefit levels, respectively. The first term on the right-hand side represents the insurance value as measured as the gap in marginal utilities between non-formally employed and formally employed workers. Empirically, we approximate the insurance value based on the flow drop in consumption after layoff, following Chetty (2008).[10] The second term on the right-hand side refers to the efficiency costs defined by the behavioral response to higher UBs, both in terms of length of UB receipt and delay in formal re-employment. We estimate the parameters of Equation 1 in Sections 5 and 6 and bring them together to compute overall welfare effects in Section 7.

2.2 The role of informal jobs

Whereas informal employment does not directly enter the welfare function, having data on formal and informal employment and their implications for wages and consumption allows us to investigate the influence of informal employment on the magnitudes of the insurance value and efficiency costs. This also makes it possible to shed light on the role of informal jobs as insurance mechanisms against the loss of a formal job. Given estimates of the insurance value and the efficiency costs of UBs might be consistent with a range of different labor market dynamics and even opposite views on the role of informal employment. The two stylized views of informality assume informal employment to be the result of a voluntary choice associated with decent wages (i) or, in contrast, to be chosen out of economic necessity in the absence of better-paid alternatives in the formal sector (ii) (see Gerard and Gonzaga, 2021).

To begin with, the efficiency costs of UBs can be due to workers reacting to UB generosity by moving into informal employment as opposed to remaining unemployed. This is key to understanding whether informal jobs are readily available to laid-off individuals and the extent to which informal employment represents an additional margin of behavioral response to UB generosity. Similarly, a given positive level of the insurance value of UBs could be related to workers remaining out of employment and earning a wage in addition to receiving UBs. Alternatively, significant numbers of UB recipients could move into informal employment, but the wages they earn in these informal jobs are insufficient to preserve consumption levels.

---

[10]The insurance value can be approximated by the flow drop in consumption, rescaled by the coefficient of relative risk aversion (γ). This is based on a Taylor approximation and the assumption that the third order derivatives of the utility functions are negligible. Then, $\frac{u'(c_u)-v'(c_{fe})}{v'(c_{fe})}$ can be approximated by $\gamma\frac{\Delta c}{c}$, where Δc denotes the change in consumption levels after layoff (Chetty, 2008).

The two stylized views on informality have similar implications in terms of the efficiency costs of UBs. Formally displaced workers may return to formal employment slowly, either because individuals do not value formal jobs more than informal ones (view (i) above) or because formal jobs are difficult to find (view (ii) above). However, these two views have opposite implications for the insurance value of UBs. If view (i) is correct, wages should be similar between formal and informal employment and informal re-employment should allow workers to cushion the consumption drop experienced at layoff. In contrast, under view (ii) wages in the informal sector should be lower than in the formal sector and informally re-employed individuals should experience large drops in consumption (Gerard and Gonzaga, 2021).

This means that a priori welfare effects of UBs can be either higher or lower in contexts of high informality. Additionally, a given level of the insurance value (efficiency costs) can be compatible with opposite views on the role of informal employment. In our analysis, we therefore begin by estimating the relevant terms that allow us to characterize Equation 1. Subsequently, we exploit our rich individual-level data to assess the role of informal jobs in determining welfare in line with the two main views presented above.

3   The institutional context of the Mauritian UB scheme

Mauritius is a country in the Indian Ocean with a population of 1.3 million and a median age of 36.2 years (Statistics Mauritius, 2017). The country has experienced sustained economic growth over the last decades: GDP per capita has more than doubled since the 1990s and is currently comparable to levels in Latin American countries such as Argentina, Chile and Uruguay. The Mauritian service sector accounts for the majority of employment (67.4 per cent), followed by industry and manufacturing (25 per cent) and agriculture (7.6 per cent). Despite rapid economic growth, evidence from the household survey shows that informal employment is still prevalent. An estimated 43.8 per cent of the employed population was formally employed in 2018 (i.e. employed in a job for which compulsory social security contributions to the National Pension Fund are made, as required by the relevant legislation) and this value had barely changed compared to previous years (i.e. it was equal to 44.3 in 2012, when our survey data starts).[11]

The current system of UBs is in place since 2009. Unemployed individuals are eligible

---

[11]Throughout the analysis, we follow the ILO definition and define formal employment for employees based on the presence of work-related social security contributions to the National Pension Fund (i.e. by both the employer and the worker). This is the relevant definition measuring the fiscal implications of labor supply responses to UB generosity (e.g. foregone tax revenues) and it is also the definition adopted in previous studies. We consistently observe formal employment based on this definition in the household survey (where information on these contributions is elicited) and the administrative data (which records these contributions). Later in the analysis, we will compare the estimates from the two sources.

to participate if they have been employed in a full-time job for at least six months without interruption. All reasons for job loss apply (including the expiration of a fixed-term contract), except for voluntary resignations. Eligibility is verified at the time of registration at the local Labor Office, when the dismissed worker needs to present a letter of termination of employment. Additionally, the previous employer is called upon to confirm details of the employment relation.

Conditional on meeting these criteria, laid-off individuals coming from both formal and informal jobs can enter the program. When informal workers apply, the government tries to recover social security contributions from the previous employer but fully finances the participation of the individual in the intervention if this proves impossible. In practice, however, very few informal workers apply to UBs upon layoff (i.e. they represent 20 per cent of total participants, despite accounting for roughly 70 per cent of the unemployed in the country). This is because they are both less likely to meet the program eligibility criteria and to apply conditional on being eligible, since program registration requirements (e.g. the need to present a letter of termination of employment and the requirement for the employer to confirm the dismissal) discourage their participation (Liepmann and Pignatti, 2019).

The UB received is a function of monthly gross wages earned at the time of job loss as verified from the letter of termination of employment and the unemployment duration. The amount replaced never falls below a lower bound of 3,000 Rupees (USD 184 in PPP) and never exceeds the upper bound in place in a given year t ($UpperBound_t$). The latter is updated annually to reflect inflation and was equal to 15,000 Rupees in 2017 (USD 920 in PPP). More specifically, UB entitlements are determined as follows:

$$
UB_{itm} = \begin{cases} 3,000 & \text{if } r_m w_i \leq 3,000 \\ r_m w_i & \text{if } 3,000 < r_m w_i < UpperBound_t \\ UpperBound_t & \text{if } r_m w_i \geq UpperBound_t \end{cases} \tag{2}
$$

where w_i is the monthly wage individual i earned prior to job loss and r_m denotes the replacement ratio in month m of unemployment. In the first three months of unemployment ($m = 1, 2, 3$), the UB replaces 90 per cent of w_i ($r_m = 0.9$). This replacement ratio is reduced to 60 per cent during months 4 to 6 of unemployment ($m = 4, 5, 6$ and $r_m = 0.6$). Finally, during months 7 to 12 of unemployment, the replacement ratio further drops to 30 per cent of the initial wage ($m = 7 - 12$ and $r_m = 0.3$).

Upon entry into the program, participants choose among three available active labor market policies: job placement, training and reskilling, or start-up support. The vast majority opts for the job-placement option (85 per cent), but the type of support provided by the job

placement services is limited. The maximum duration of UB receipt is 12 months since the time of job loss. Program eligibility ends earlier if a worker finds a new job, in theory independently from its nature. However, the two processes differ significantly between formal and informal re-employment. If the participant finds a formal job, the Ministry of Social Security detects from its records that contributions are being made for a new job and automatically de-registers the UB recipient. If the new job found is instead informal, the participant should report this information to the Labor Office that would proceed with the de-registration. However, this is difficult to enforce and there are no sanctions for failing to report a new job.

4 Data and descriptive evidence

4.1 Data

For the purpose of the analysis, we have obtained access to rich administrative records that we have matched with the country's household survey. The resulting final database represents a unique source of information, allowing us to observe detailed individual and household characteristics of UB recipients independently from their re-employment status. We have combined all data sources using individual's unique national ID numbers.

The starting point is the administrative data collected by the Ministry of Labour on the universe of UB recipients. These records are elicited at the time of registration with the program and include some basic personal characteristics (i.e. age, gender and district of residence) as well as detailed information on the elapsed job-spell (i.e. wage at job loss, tenure, reason for dismissal). This information is taken from the letter of termination of employment and the previous employer needs to confirm its accuracy. When the individual leaves the program, the date of exit is also reported. For the vast majority of UB recipients who opted for the job-placement option, we have additional information from employment centers on individual and household characteristics (e.g. educational levels, marital status, previous occupation).

Second, we rely on the social security records from the Ministry of Social Security. These records contain monthly information on individuals' formal employment biographies. We have these full records (i.e. from the first month of contributions of the individual until December 2018) for two different populations of interest. The first is the universe of UB recipients, for which we can reconstruct all formal employment episodes before and after the unemployment spell. The second is a representative sample of the Mauritian formal labor force, obtained by appending the full employment records of those who were formally employed in a given month

in all the years between 2011 and 2018.[12] This second sample includes around 90 per cent of those formally employed in Mauritius during the period of analysis.[13]

In the rest of the analysis, we will jointly refer to these two data sources as the administrative database (or administrative records). For the sample of UB recipients, this will correspond to the panel of social security records from the Ministry of Social Security matched with information collected by the Ministry of Labour at the time of registration into the program. For this sample, we do not consider individuals who lose their job before 2011 (i.e. data from the Ministry of Labor for 2009 and 2010 presents inconsistencies, as program records were initially kept manually) and also restrict the analysis to those who enter the program by the end of 2017 (i.e. to observe them at least for one year in the social security records). We also focus on first-time program participants throughout the analysis. For the sample of the Mauritian formal labor force, the administrative database contains instead only the panel of social security records. For this sample, we restrict the time frame to between 2012 and 2018 to have a comparable period of analysis.

We have been able to match our administrative data with the Mauritian household survey administered by the National Institute of Statistics (the so-called Continuous Multi-Purpose Household Survey, or CMPHS). The merge was done at the individual and monthly level. The CMPHS is nationally representative and interviews every year around 30,000 individuals. It has a rotating panel structure, whereby a household can be interviewed four times 15 months apart (i.e. 2-2-2 rotating panel at the quarterly level). The survey has a standard content and reports information on a number of individual and household characteristics, including detailed labor market status (i.e. also covering informal employment). We have data on the CMPHS between 2012, when the survey questionnaire started asking individual ID numbers which we use to conduct the merging, and 2018. While giving the ID number is not compulsory, the vast majority of survey respondents provide this information (i.e. 85.1 per cent of the sample).

Table 1: Summary statistics for UB recipients, overall sample and matched versus unmatched subsamples

	Overall (1)	Unmatched (2)	Matched (3)	Difference (2)-(3)
Personal characteristics				
Male (0/1)	0.598	0.596	0.622	-0.0266**
Age (in years)	35.764	35.714	36.460	-0.7464***
Any dependents (0/1)	0.506	0.504	0.533	-0.0298**
Married (0/1)	0.526	0.524	0.554	-0.0300**
High education (0/1)	0.183	0.183	0.177	0.006
Characteristics of previous job				
Entering UBs from formal job	0.783	0.783	0.777	0.006
Wage at job loss (2018 values)	11994.166	11994.051	11995.789	-1.739
Tenure at job loss	37.840	37.745	39.166	-1.421
Managers (0/1)	0.012	0.012	0.006	0.0058**
Professionals (0/1)	0.022	0.022	0.015	0.0078**
Technicials (0/1)	0.049	0.049	0.042	0.007
Clerical support workers (0/1)	0.124	0.124	0.118	0.006
Service and sales workers (0/1)	0.158	0.158	0.160	-0.002
Agricultural workers (0/1)	0.012	0.012	0.016	-0.004
Crafts workers (0/1)	0.144	0.143	0.151	-0.008
Plant operators (0/1)	0.142	0.141	0.147	-0.006
Elementary occupations (0/1)	0.339	0.338	0.345	-0.007
Stay in program				
Months of UB receipt	10.345	10.346	10.332	0.013
Share exhausting Ubs	0.698	0.698	0.692	0.0059
N	26721	24942	1779	

Notes: Variables means are shown for the entire sample (column (1)), the sample that we did not match with the household survey (column (2)) and the sample that we matched with the household survey (column (3)). Column (4) displays the difference between columns (2) and (3) and the results from a two-sided t-test, where *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively. Except for the number of months of benefit receipt and the share exhausting UBs, all variables refer to the point in time of program entry and are taken from the administrative data of the Ministry of Labour. With regard to education, the residual category also contains persons not reporting their educational level, such that the actual share of persons with at least an upper secondary education is higher.

## 4.2 Evaluation of the matching procedure and descriptive evidence

We now discuss the quality of the matching procedure between the administrative and survey data and present some descriptive evidence. For ease of exposition, in this part of the analysis we focus on the universe of UB recipients only.[14] For this sample, we restricted the length of

---

[12]We obtained this sample by first extracting the full employment histories of those who were formally employed in July 2018 (i.e. from the date of their first contribution until December 2018). We then moved to July 2017 and added the full employment histories of those who were employed in that month and did not already appear in our database. The same process was reiterated for all the months of July until 2011. July was chosen to maximize sample coverage, as it is considered the peak of labor market participation in the country.

[13]We can check the representativeness of these records by looking at how many UB recipients (for whom we have the full sample and who should all appear in the social security records, at least during the months of UB receipt) are in this sample (which was obtained independently from UB participation). We find that 92.4 per cent of those who are in the UB database appear also in the sample of the Mauritian formal labor force. Moreover, on average there are around 250,000 individuals per month in this sample, which is in line with almost full coverage of the formal work force given that the employed population comprises around 550,000 individuals and formality rates are estimated slightly below 50 per cent.

[14]For the UB sample we have richer individual and household information from program registration, which we can use to assess the quality of the matching. However, also the administrative sample of the Mauritian formal labor force compares well with the formally employed in the country: the share of men was equal to 58.1 per

Figure 1: Formal employment shares for matched and unmatched observations, social security database
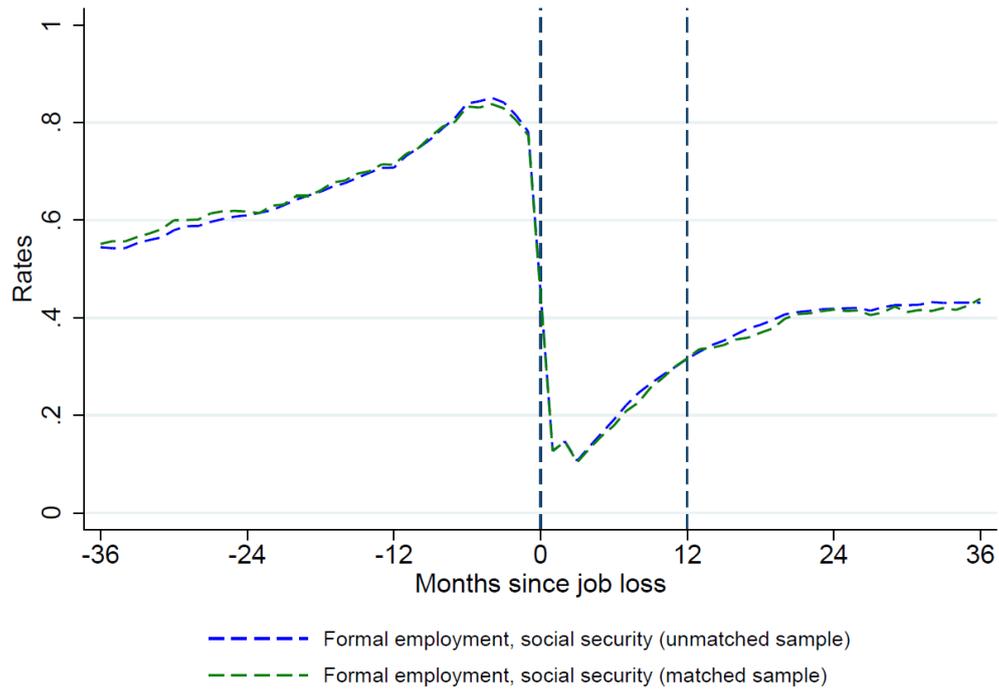
Notes: The figure reports the share of formally employed individuals (relative to the population) in the three years before and after job loss for the sample of UB recipients who has been matched between the administrative and survey data (dashed green line) and the sample of UB recipients who has not been matched (dashed blue line). Formality is measured from the social security records for both samples.

the administrative records to three years before and after job loss. This means that for almost all UB recipients, we have a panel of six years of social security records as well as cross-sectional information reported at the time of registration. We match this database with the household survey and find that 6.66 per cent of UB recipients are interviewed at least once in the CMPHS between 2012 and 2018. These individuals have not necessarily been observed in the CMPHS while receiving UBs, but rather in the three years before or after job loss.

A first possible concern is the representativeness of the matched sample with respect to the overall population of UB recipients. Random sampling of the household survey should in theory guarantee this. To verify if this is the case, we compare observable characteristics between matched and unmatched individuals. We take these variables from the administrative records at job loss, so that we have information for everybody at the same point in time. Table 1 shows that there are no statistically significant differences with respect to variables related to program participation (e.g. length of receipt of UB) and characteristics of the previous job (e.g. tenure, wages). The share of individuals formally employed in a given month, as elicited from social security records, is also very similar in the two samples both before and after job

---

cent in 2018 in our administrative sample of the Mauritian labor force and to 58.9 among the formally employed interviewed in the CMPHS in that same year. The corresponding average age was 38.9 and 39.1, respectively.

loss (Figure 1). A few differences emerge with respect to individual-level characteristics (i.e. most notably, age and gender), but these are small in magnitude. Thus, the matched sample compares well overall to the universe of UB recipients.

We observe that UB recipients (i.e. matched and unmatched observations) are disproportionately men (i.e. almost 60 per cent of participants). This is in line with the prevalence of male workers in the formal labor market in the country. The average age in the sample is almost 36 years and around half of the participants is married and has dependents. Individuals are generally low educated, with less than 20 per cent of UB recipients having attained at least upper secondary education. The monthly wage at job loss is on average slightly below 12,000 Mauritian Rupees, which corresponds to 736 USD in PPP and is marginally below the median wage in the country. Average tenure in the previous job is just above three years and the relative majority of participants (i.e. more than one third) was previously employed in elementary occupations. Finally, the average length of UB receipt is equal to 10.3 months and around 70 per cent of participants stay in the program for its entire duration.

A second possible concern relates to the comparability of the information provided between the two data sources. In the context of the present analysis, this is relevant particularly for variables capturing formal and informal employment. The definition of formality that we construct from the household survey is the same as the one that we observe in the administrative database. As mentioned above, this refers to the presence of job-related social security contributions to the National Pension Fund that are compulsory for the legislation. Measurement might nevertheless differ between the two sources, if survey respondents ignore their formality status or decide to misreport it (e.g. for fear that survey responses are used for auditing).

To address this concern and provide evidence on the quality of the matching, we exploit the fact that for the matched sample we have information on formal employment from both the administrative records and the household survey. Figure 2 plots the shares of individuals in our UB sample with a formal job before and after the time of job loss. The dotted green line corresponds to the dotted green line in Figure 1. It measures formality from the social security records for the matched sample and is therefore precisely observed at each point in time (i.e. independently from when the individual was interviewed in the household survey, we take the entire series of social security records). The continuous green line also reports the share of individuals who are formally employed for the matched sample, but this time measured from the household survey and using observations as repeated cross-sections (i.e. for each individual, we have a maximum of four observations over time such that different individuals are used to estimate formality rates across months). Sample size is small at the monthly level in the

Figure 2: Formal and informal employment shares and share of participants receiving UBs, matched sample
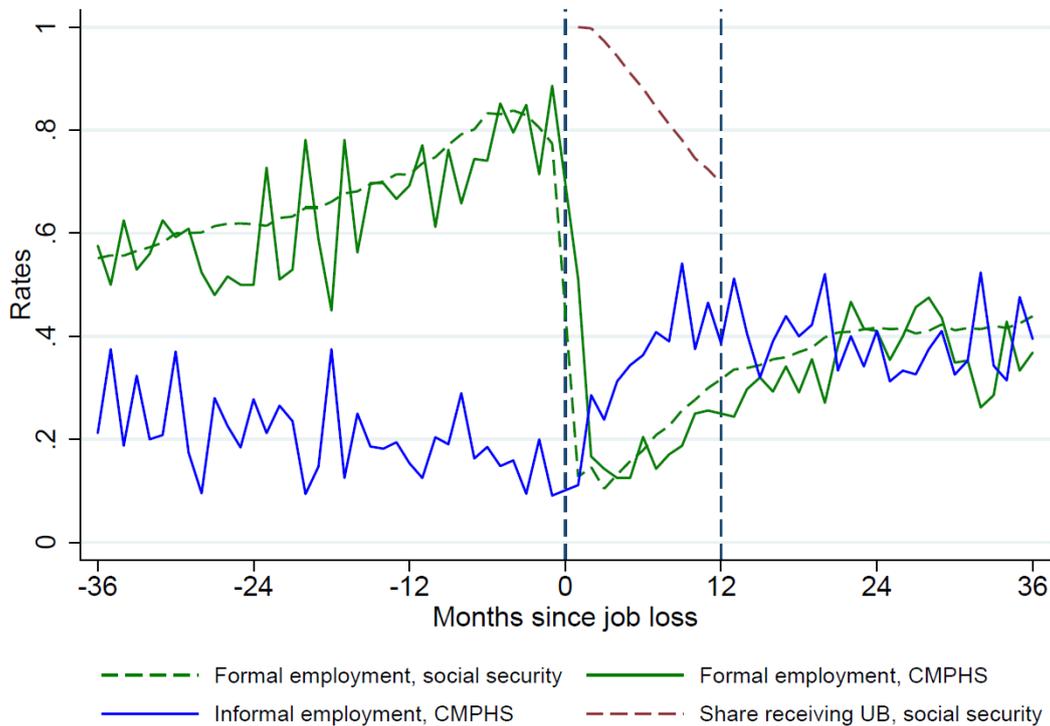
Notes: The figure reports the share of formally and informally employed individuals (i.e. relative to the population) in the three years before and after job loss as well as the share of participants receiving UBs for the sample of UB recipients who has been matched between the administrative and survey data (i.e. 6.66 per cent of total UB recipients). The dashed green line is obtained from the social security records and corresponds to population means, as it is computed based on the full panel of observations for the universe of the matched individuals (i.e. independently from when the individual was interviewed, we use the entire history of social security records). The same applies to the dashed red line reporting the share of participants receiving UBs. The continuous green and blue lines represent instead population estimates obtained by appending different cross-sections of observations depending on the time at which the individual was interviewed in the CMPHS.

household survey and this leads to some noise in the series, but the figure shows that formality shares are extremely comparable between the two sources.

This is reassuring, because it suggests that we can credibly rely on information that is only available in the household survey (including on informal employment) to enrich the analysis.[15] In Figure 2, we then plot also the informality rates for our matched sample of UB recipients (continuous blue line). A number of interesting findings emerge. First, informality is low in the period before job loss, with around 20 per cent of UB recipients holding an informal job in the year before entering the program. However, the share of individuals holding an informal job rapidly increases and settles between 40 and 50 per cent of the sample starting from six

---

[15]In theory, one might still suspect that information on informal employment is misreported in the CMPHS even if information on formal employment is adequately provided. While we cannot totally rule out this hypothesis, we note that in the six months before UB registration, when everybody should have been in a job in order to be eligible to later receive UBs, all individuals report being in a job in the CMPHS (i.e. summing the green and blue continuous line adds almost perfectly to one in all the months). Additionally, the structure of the CMPHS makes it unlikely that an informal employee strategically misreports her labor market status as questions on social security contributions are only asked when respondents are already responding to the employment module.

months after job loss. Around 70 per cent of UB recipients is therefore re-employed 12 months after job loss, 40 per cent informally and 30 per cent formally. Informality rates remain high even at the end of program exhaustion, when the incentives not to work formally end: three years after job loss, 40 per cent of UB recipients are still informally employed.

The evidence from the survey data shows that 12 months after job loss, only 30 per cent of program participants is without a job and is therefore still entitled to the benefit. However, we also see from the social security records that 70 per cent of UB recipients in the matched sample is still receiving UBs at that point in time (dashed red line in Figure 2). This means that 40 per cent of UB recipients are working while also receiving the benefit at the time of UB exhaustion. Therefore, the vast majority of those who find a new informal job do not report it to the Labor Office. Among the individuals who in our survey data appear to be informally re-employed in the first year after job loss, 92.5 per cent are still registered as receiving UBs in the administrative records in the same month of the survey interview. This is an important finding, as we provide the first estimates of the share of individuals working informally while receiving UBs. It implies that the level of non-compliance with program regulations is relatively high. De-registration from the program is instead automatic for individuals who find a formal job and the excessive UBs eventually transferred (e.g. due to administrative delays) are recovered.

5 The insurance value of UBs and underlying labor market mechanisms

We conduct a difference-in-differences (DiD) analysis centered 36 months around job loss to estimate the effects for UB recipients of losing a formal job on main outcomes of interest. The first set of outcome variables captures consumption expenditures, as this directly determines the insurance value of UBs. We then focus on a series of labor market outcomes to understand the mechanisms through which the consumption effect materializes and how the availability of informal jobs affects the insurance value of UBs.

5.1 Empirical approach

Starting from the universe of UB recipients, we restrict the sample to those who enter unemployment from a formal job, which has lasted at least 36 months before layoff as measured from the social security records.[16] As common in the literature, we define a control group of individuals

---

[16]The restriction on formality is imposed to identify a comparable control group of individuals who never lose their jobs for the entire time period (i.e. we do not observe informal workers consecutively for 72 months from the CMPHS, as the maximum length of the panel is 15 months and there might be employment gaps between interviews). The tenure requirement raises the comparability between treatment and control groups and follows

who never experience job loss (Gerard and Naritomi, 2021; Kolsrud et al., 2020; Landais and Spinnewijn, 2020). This group is taken from the sample of the Mauritian formal labor force for which we have full social security records, restricting the analysis to those who are reported in the same formal job for 72 consecutive months. We conduct the analysis only on observations in the treatment and controls groups that are matched in the household survey.

Imposing these restrictions, we end up with a final sample of 14,535 individuals (1,041 treated and 13,494 controls) who have been interviewed in the CMPHS in the three years around the (placebo) job loss. Table A1 in Appendix A shows selected descriptive statistics for this sample, as measured in the CMPHS. As expected, the treatment and control groups differ on a number of dimensions. In particular, UB recipients are more likely to be men and have lower educational attainments. The average age is instead similar between control and treated observations. The baseline equation takes this form:

$$Y_{icst} = \alpha + UB_i + \sum_{t=-12}^{11} Q_t + \sum_{t=-12}^{11} \beta * Q_t * UB_i + Year_c + Month_c + District_s + \epsilon_{icst} \quad (3)$$

where UB_i is a dummy variable taking the value of one if the individual belongs to the treatment group of UB recipients; Q_t are a set of event time dummies for each quarter before and after the (placebo) job loss at $t = 0$; $Year_c$ and $Month_c$ are calendar year and month dummies for the time in which the individual is observed in the CMPHS; and $District_s$ is a vector of dummies for the district of residence. We group observations at the quarterly level in order to have adequate sample size in each event time.

Given differences in observable characteristics between the treated and control groups, we re-weight observations to balance the first two moments of the covariate distributions (see Gerard and Naritomi, 2021; Landais and Spinnewijn, 2020). The re-weighting is based on the variables of sex, age, age squared as well as full sets of dummies for marital status, kinship relation and educational attainments. We apply the same re-weighting to all DiD results unless stated otherwise. Robustness tests will show that results are not particularly sensitive to the re-weighting procedure, which nevertheless increases the precision of the estimates. Standard errors are clustered at the individual level across CMPHS waves, but we do not add individual fixed effects given the relatively short length of the CMPHS panel. In the robustness tests, we will confirm that the inclusion of fixed effects is unlikely to change the results.

---

previous studies that have similarly focused on high-tenure workers (Jacobson et al., 1993). While the 36 month cut-off is arbitrary, modifying it does not substantially change the results.

5.2 Main results

We now present the main results of the DiD analysis. The only coefficient we report is the one of the interaction term between the event time dummies and the dummy for UB status (corresponding to $\hat{\beta}$ in Equation 3). We perform regressions in levels, but for the continuous outcomes we present results in relative changes by rescaling point estimates and confidence intervals by the mean in the treatment group in the quarter before job loss (i.e. which corresponds to the baseline event time). All graphs in this section will follow the same structure. In particular, we plot coefficients for the 12 quarters before and after the layoff event, which is denoted with a dashed blue vertical line. The period before job loss is included to check for parallel trends, where we expect the coefficient of interest to be non-significant. Wages and benefits will be presented gross of any taxes, but they are both subject to taxation.

A. Consumption expenditures

First, we analyze the effect of job loss on household expenditures (Figure 3). Total expenditures still remain constant in the first quarter after job loss (in line with the generous UB, which replaces 90 per cent of previous income in these months), but then rapidly decrease in the following quarters to reach a value between 20 and 30 per cent below the pre-layoff levels (panel A). Potentially because of sample sizes, the estimates for some quarters are imprecisely estimated. Yet, the overall pattern clearly indicates that the fall in expenditures persists during the three years following job loss. We observe a very similar pattern for total consumption expenditures (panel B). Additionally, we split consumption between durable and non-durable goods (panels C and D, respectively).[17] We see that the drop in durable goods is larger in magnitude, but non-durable expenditures (mostly food) also fall at layoff and remain below pre-treatment trends. The result on non-durables is especially important, as it confirms that the fall in expenditures leads to a fall in consumption (see Ganong and Noel, 2019).[18]

Taken together, our results indicate that the significant consumption drop implies a relatively high insurance value of UBs. Our point estimates are substantially larger than

---

[17]As in Ganong and Noel (2019), we differentiate between durable and non-durable expenditures using the standard taxonomy for survey data introduced by Lusardi (1996). Durable expenditures are limited in our data (i.e. they represent around 10 per cent of total expenditures) and include items such as education, insurance and appliances. Non-durable expenditures include utilities, transportation, medical costs and food.

[18]This would not necessarily be the case if the expenditure categories that are reduced are those with a weak link between the time of expenditure and consumption. A possible threat to our interpretation would be that households substitute market purchased goods with home produced ones. While we do not have direct information on this, it seems a relatively remote possibility given that (i) we see a similar fall in spending across different categories, independently from their suitability for home production, and (ii) home production of consumption goods is very limited in Mauritius, given high population density and the use of most arable land for export-oriented agriculture (e.g. sugar cane) (National Research Council, 1993). For instance, in our sample only 0.06 per cent of individuals derive income from backyard-produced goods.

Figure 3: DiD results on household expenditures
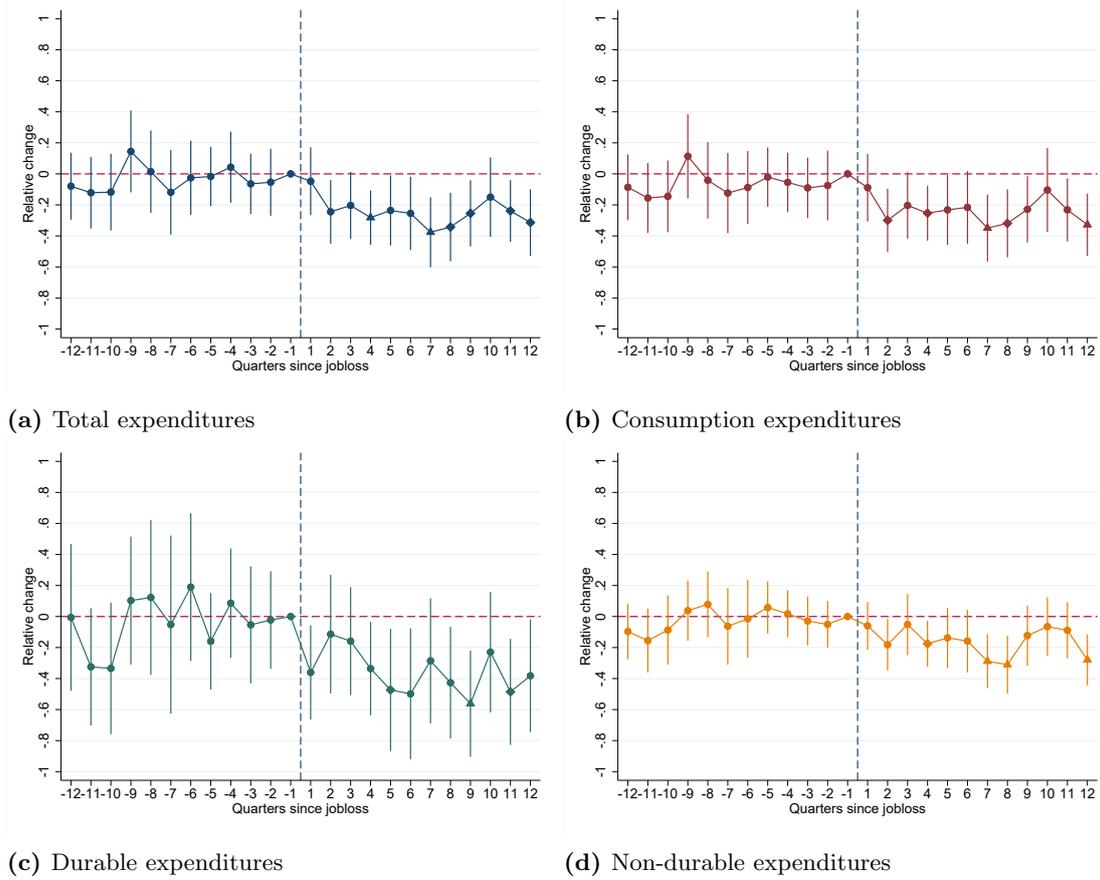
(a) Total expenditures

(b) Consumption expenditures

(c) Durable expenditures

(d) Non-durable expenditures

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. All outcomes of interest are taken from the CMPHS.

previous ones for developed economies, which are generally below 10 per cent (Ganong and Noel, 2019; Gruber, 1997; Kolsrud et al., 2018; Landais and Spinnewijn, 2020), while larger estimates have been obtained for individuals who lose their job in a recession (Schmieder and von Wachter, 2016). Our results are in line with those obtained in Brazil by Gerard and Naritomi (2021) for their sub-sample of individuals fired for cause, which is not eligible to lump-sum severance payments at layoff. Additionally, the fact that spending decreases when UB generosity falls corresponds to models where present-biased households fail to save in anticipation of a predictable income drop (Ganong and Noel, 2019; Gerard and Naritomi, 2021). At the same time, the fact that spending for non-durable also decreases confirms a consumption response to transitory income shocks (see Browning and Crossley, 2001).

To provide evidence on the role of re-employment in cushioning the drop in consumption, we look at the evolution of expenditures by re-employment status after job loss.[19] Given

---

[19]Of course, neither the timing of re-employment nor the type of job found are exogenous. However, the robustness tests will show that selection into employment and employment type is unlikely to drive our estimates.

that everybody is formally employed before layoff, this differentiation is not done for the quarters before job loss, which is why pre-treatment trends coincide and will be denoted by a unique line (Gerard and Naritomi, 2021).[20] We see that the expenditure drop is the largest among those who are informally re-employed after the initial layoff (panel A in Appendix Figure A1). In contrast, individuals who are not re-employed experience similar consumption patterns as those who are formally re-employed in the two years after job loss. This suggests that individuals who remain without a job are those who can afford longer unemployment spells, while informal jobs are taken-up by credit-constrained individuals trying to prevent an even deeper fall in consumption. This could imply that informal jobs, while being relatively available compared to formal ones, provide for poor means of self-insurance. The rest of this section will further test this hypothesis and shed light on the mechanisms behind the large consumption drop, which is key to understanding what causes the high insurance value of UBs in our context.

B. Labor market status, wages and transfers

Figure 2 above has descriptively shown a large increase in informal employment following job loss. This is confirmed in the DID results on labor market status, which are reported in Figure 4. They show that the treatment group experiences a reduction in the probability of being employed (i.e. formally or informally) following job loss. However, the drop in employment is relatively small and disappears after two years (panel A).[21] A similar trend is observed for unemployment (panel B) and inactivity (panel C). The relatively small drop in overall employment can be explained because the sharp reduction in the probability of being formally employed is compensated by a rapid increase in the probability of being in informal employment (panel D). This shift in the type of job held is permanent: even three years after job loss, displaced formal workers are 43.7 per cent less likely to hold a formal job and 38.2 per cent more likely to work informally. This is an important result, as we are the first to directly document such a long-term reallocation from formal to informal employment for UB recipients.[22]

The large shift towards informal employment may explain the consumption drop documented above, provided that informal jobs are associated with inferior job characteristics. Figure

---

[20]An alternative approach would be to split the sample based on the time-invariant characteristic of the first job found after layoff. Results would be largely in line with those presented in the text and the robustness tests provide evidence in that respect (see Figure A8 in Appendix A).

[21]In the first and second quarter after the layoff event, UB recipients are on average only 40 per cent less likely to be employed compared to the quarter before job loss. In comparison, estimates from developed economies report an initial drop in employment of around 60 per cent (Verho, 2020).

[22]The finding is, however, consistent with results in Gerard and Gonzaga (2021), who find that re-employment rates in the formal sector remain low even after individuals have terminated UB eligibility. Our findings are also consistent with implications arising from models of informality as developed by Meghir et al. (2015) and Ulyssea (2018), where workers transit between formal and informal jobs but formal ones are valued more.

Figure 4: DiD results on labor market status
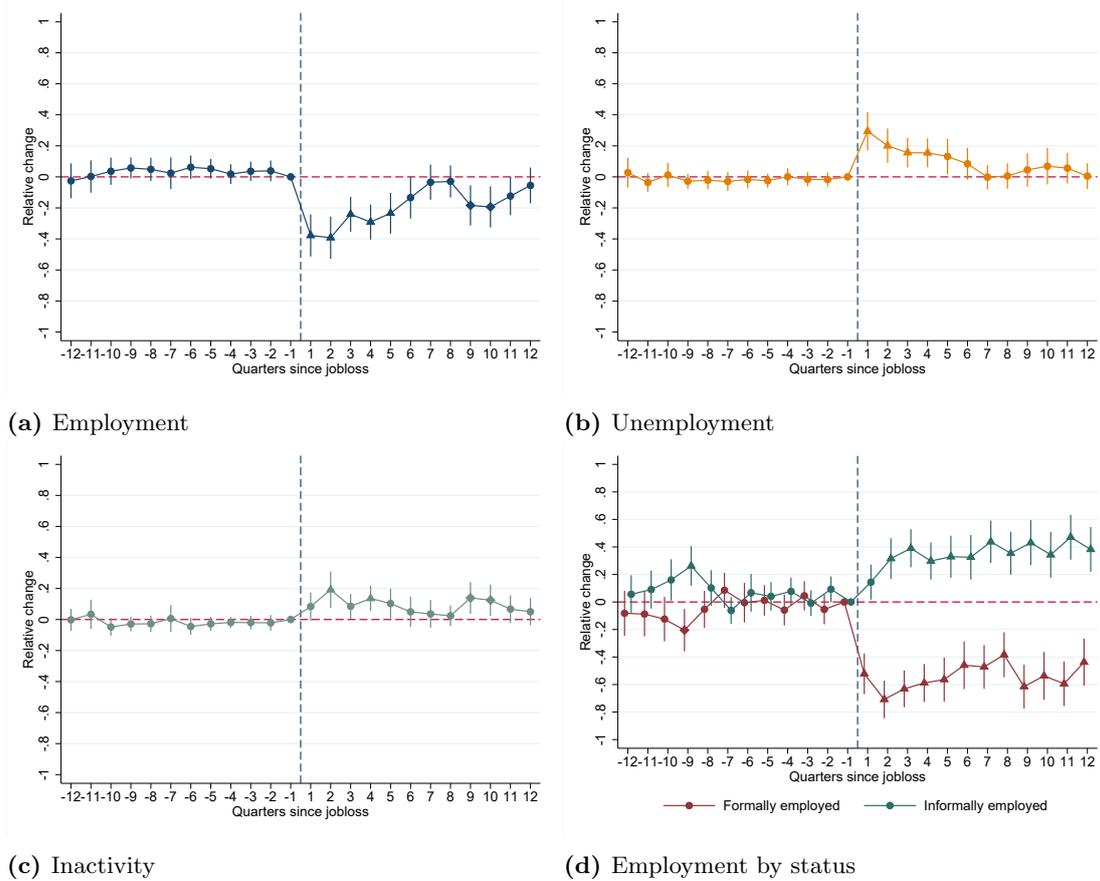
(a) Employment

(b) Unemployment

(c) Inactivity

(d) Employment by status

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. All outcomes of interest are taken from the CMPHS.

5 reports DiD results on job characteristics. We differentiate between the overall sample of UB recipients and those who find a new job after layoff and also consider the formality status of the job found. Starting with gross wages (panel A), looking at the full sample we see a decrease in monthly wages of almost 60 per cent in the year after job loss (i.e. blue line, where we assign a wage of zero to the non-employed). Wages drop also for those UB recipients who find a new job (brown line in the same figure, where the non-employed are assigned missing wages), which can be interpreted as evidence for loss of occupation, firm, or job specific human capital. The two lines converge around two years after job loss, when the majority of the laid-off individuals is back in employment. However, even three years after job loss previous UB recipients experience a wage loss of around 40 per cent.

The observed wage loss is substantially larger than previous estimates for advanced economies, which generally range between 15 and 20 per cent for prime-age workers (see the review in Couch and Placzek (2010)). As a matter of comparison, our results on wages are similar to those obtained in developed countries for displaced older workers (Chan and Stevens,

Figure 5: DiD results on job characteristics

(a) Wages

(b) Wages plus UB

(c) Wages by re-employment status
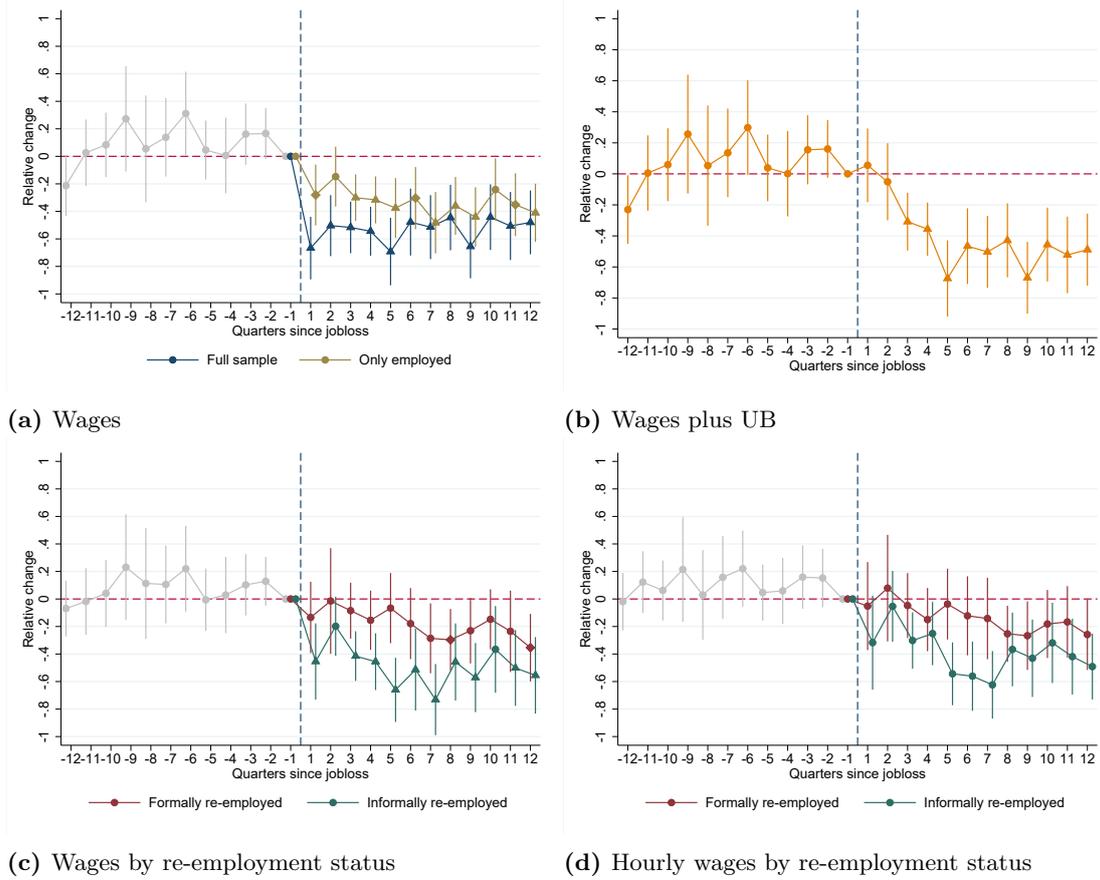
(d) Hourly wages by re-employment status

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. Figures showing results for different sub-groups according to their re-employment status include the whole sample in the period before layoff (i.e. independently from their re-employment patterns after job loss), which is why pre-treatment trends coincide and are denoted by a unique line. Hourly wages are estimated by using information on monthly wages and weekly number of hours worked. All outcomes of interest are taken from the CMPHS.

2004; Couch, 1998; Couch et al., 2009) or for individuals who lose their job in a recession (Jacobson et al., 1993). Our estimates are also larger than the few available ones for emerging and developing economies (Amarante et al., 2014), despite the fact that we also consider wages from informal work. UB receipt partially cushions the income drop at the beginning of the unemployment spell (panel B). However, benefit levels decrease sharply over time and UB eligibility ends well before wages have recovered.

As for consumption, we analyze the evolution of monthly wages for re-employed individuals by status in the new job (panel C). We find that informally re-employed individuals experience a sharper reduction in wages than those who find a new formal job (respectively, around 50 and 20 per cent wage drop on average in the three years after layoff). Differences in wages between formally and informally re-employed individuals are not driven by the fact that informal jobs require working fewer hours per week. In contrast, we see that hourly wages decline

in both formal and informal jobs, but that the magnitude of the decline is larger for informal workers (panel D). The lower hourly wage indicates that informal jobs are less productive than formal ones, and that the shift towards informal employment entails a significant wage penalty (consistent with theoretical models, see Meghir et al., 2015).

The documented wage drop is before taxes and transfers. While we do not have quarterly information on the amount of taxes paid, we can investigate whether households receive transfers and if this helps alleviate the earning drop experienced at layoff. We find a small increase in total transfers after job loss (panel A of Appendix Figure A2). This comes from an increase in social security and other transfers (panels B and D, respectively), consistent with the receipt of UBs.[23] At the same time, private transfers (e.g. from relatives, panel C) do not increase and other forms of public support fail to materialize after UB exhaustion. As a result, the increase in total transfers is overall small and short-lived. This is in contrast with evidence of program complementarity in developed countries, where studies have documented that individuals move to other forms of public support when eligibility to the initial transfer ends (Giupponi, 2019; Inderbitzin et al., 2016; Ye, 2020). Those who remain without a job (i.e. unemployed or inactive) benefit from the largest total transfers (panel B in Appendix Figure A1), although transfers tend to decline rapidly over time for all groups.

5.3   Robustness tests

We conduct a series of tests to verify the validity of the DiD results (Appendix A). For ease of exposition, we will conduct most of these tests only for selected outcomes of interest within each outcome category (i.e. total and consumption expenditures, formal and informal employment, wages and hours worked and total and social security transfers). First, we compare our DiD results with those of a pure event study approach with no control group (Figure A3). This addresses concerns on how our control group was constructed. Second, we run the DiD specification but without re-weighting observations with the propensity score (Figure A4). This shows that the weights only increase the precision of the estimates, without drastically changing point estimates. Finally, the DiD analysis relied on repeated cross sections from the CMPHS, but for formal employment we can alternatively use the social security records and perform a panel regression. The results are almost identical using the two methods (Figure A5).

A second set of tests verifies if UB recipients anticipate job loss, which would represent a threat to identification. First, we restrict the treatment group to individuals dismissed for

---

[23]The survey questionnaire does not directly ask information on UB receipt, so it is possible that respondents categorize it within "Other social security benefits" or the residual category of "Other transfers".

economic cause.[24] This is done to look at more exogenous forms of job separation, in line with studies that have focused on firm closures. Results are very similar to those obtained for the overall sample (Figure A6). We then look at the probability of being registered at the public employment services (PES). The PES provide job-search support also to employed people willing to change jobs, so we should see an increase in registrations before job loss if individuals were anticipating dismissal. Instead, PES registrations go up exactly at the time of job loss (Figure A7, panel A). Similarly, we look at patterns of work absenteeism before job loss. Individuals could anticipate or even trigger the layoff by missing days of work. However, we do not find support for this hypothesis (Figure A7, panel B).

A final set of tests checks if the results by re-employment status are driven by composition effects, given that neither the timing of re-employment nor the type of job found can be taken as exogenous. First, we exploit the fact that we know the exact date of formal employment from social security records and conduct the analysis separately by groups of workers according to the month of formal re-employment. Results in Figure A8 show that all groups experience a long-term drop in wages (panel A) and expenditures (panel B), even though those who find a new job earlier do relatively better in the short-run. Second, we investigate if differences in wages between formal and informal jobs are driven by selection bias. This is done by comparing specifications which add different sets of controls. In particular, we present results, (i) with no controls (i.e. no weights), (ii) with weights obtained as in the baseline model (i.e. baseline model), and (iii) with weights obtained adding also dummies for industry (i.e. at the one digit level), enterprise type (i.e. public, private or other types) and establishment size (i.e. less than five workers, between five and nine, ten and above) (i.e. augmented specification). If selection bias was driving differences in wages, adding controls should reduce the estimated wage gap between formal and informal jobs. However, this does not appear to be the case (Figure A9).

6 The efficiency costs of UBs and the substitution between formal and informal employment

We now turn to analyzing the efficiency costs of UBs, which are determined by the effect of UB generosity on benefit duration and time until formal re-employment (recall Equation 1 in Section 2 above). To investigate the role of informal jobs in determining the efficiency costs, we subsequently assess whether UB generosity leads to a substitution from formal to informal employment. For effect identification we employ regression kink (RK) analysis, following Card

---

[24]This has been classified as dismissals for: firm's closure, economic dismissal, financial difficulties, in receivership, redundancy, structural dismissal and technological reasons.

et al. (2015, 2017) and Landais (2015), among others.

6.1 Empirical approach

A. Sample selection and empirical specification

As explained in Section 3, the Mauritian UB schedule exhibits kinks at the upper and lower end, which we illustrate in Figure 6. Monthly UB entitlements decline rather strongly with time, from 90 percent of the monthly wage at job loss (during months 1-3 of unemployment), to 60 percent (months 4-6) and finally to 30 percent (months 7-12). The share of individuals for which the upper bound is binding is highest during the first three months of unemployment (equal to 13 percent, see Appendix Table B1). In contrast, the share of participants entitled only to the lower bound of the UBs peaks during the last six months of program participation (51.7 percent, Appendix Table B1). Given this distribution of participants and its implications for sample sizes, we focus on the kink at the upper bound of UB entitlements during the first three months of unemployment and the kink at the lower bound during the last six months. We additionally focus on benefit entitlements rather than benefits actually received. Entitlements are exogenous to participants' behaviour, whereas benefits received are determined by when individuals leave the program upon re-employment (see Section 3).

Regarding the selection of our sample, we drop a small fraction of UB participants for whom the wage at job loss is implausibly small (below 1,500 Rupees) or high (top 1 percent of the real wage distribution). We also focus on individuals who worked in a formal job in the month prior to program participation.[25] After imposing these restrictions, we obtain a sample of 17,791 individuals. Summary statistics are shown in Appendix Table B2 for the overall sample and for individuals in the proximity of the upper and lower bounds, respectively.

We employ a sharp RK design and estimate the following model for the upper bound:

$$E[Y|w] = \beta_0 + \beta_1(w - w_k) + \delta_k(w - w_k)\mathbb{1}[w \geq w_k] \tag{4}$$

and for the lower bound:

$$E[Y|w] = \beta_0 + \beta_1(w - w_k) + \delta_k(w - w_k)\mathbb{1}[w \leq w_k] \tag{5}$$

---

[25]Policy take-up is extremely low among informal workers who lose their job (while being almost complete among previously formal workers) and those who participate are not representative of informal workers in Mauritius overall (Liepmann and Pignatti, 2019). This might lead to unobserved heterogeneity, where it is not clear how this affects individuals around the kink (see Landais, 2015). Additionally and differently from formal workers entering the UB scheme, the wage information for informal workers cannot be verified by the caseworker at the time of registration, increasing the risk of measurement error of the running variable (see Section 3).

Figure 6: Unemployment benefit entitlements in Mauritian Rupees as a function of the monthly wage at job loss and month of unemployment duration
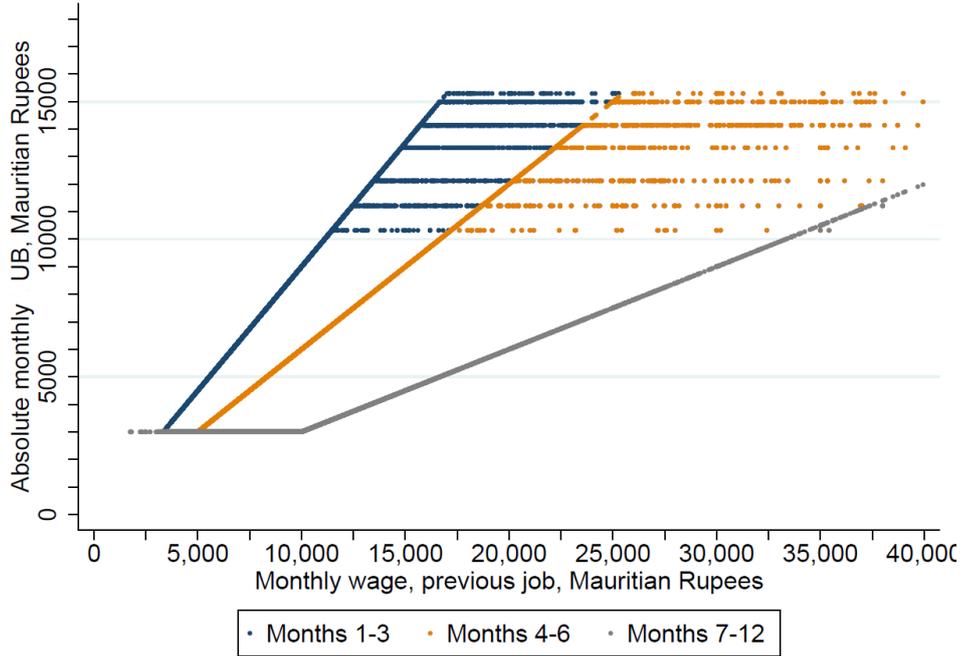
Notes: Graphical illustration of UB entitlements (see Equation 2) during months 1-3 (blue), months 4-6 (orange), and months 7-12 (gray) of unemployment duration. UB entitlements were inferred using information on monthly wages at the time of job loss for UB recipients entering the program between January 2011 and April 2018. For ease of exposition, in this figure we do not adjust for inflation.

$\mathbb{1}[]$ is an indicator function equal to 1 whenever an individual receives UBs corresponding to the respective bound. w is a worker's monthly wage at job loss and w_k denotes the wage at the respective kink points. We express both variables in 2017 Mauritius Rupees and divide them by 1,000. The models are estimated for $|w - w_k| \leq h$, where h is the bandwidth size. In our main specifications, we use the mean squared error (MSE) optimal bandwidth of $h = 0.248$ around the normalized kink points (see Calonico et al., 2017). We vary the bandwidth in robustness tests.[26] The average treatment effect is given by $\alpha_k = \delta_k/\tau_k$, where τ_k is the change in slope in the relationship between the UB level and w. α_k identifies the average impact of an additional 1,000 Mauritius Rupees of UBs. For the upper bound, $\tau_k = -0.9$. Relative to initial wages, workers above this kink receive lower benefits than workers below the kink. In our analysis of the lower bound, $\tau_k = 0.3$.[27] While α_k is estimated locally, the investigation of two different kinks provides a more comprehensive understanding for different wage levels. As explained below, we show results for the upper bound in the main text and merely reference those for the lower

---

[26]We identify this MSE-optimal bandwidth for the upper bound and based on the outcome of UB duration, as this outcome is central for our subsequent welfare analysis. We additionally consider the coverage error rate (CER) optimal bandwidth of $h = 0.142$ and a range of values chosen without theoretical foundation.

[27]For workers below the kink, the unemployment benefit schedule is flat at the lower bound, whereas workers above the kink receive 30 percent of their initial wages. Therefore, workers below the kink receive higher unemployment benefits – relative to their initial wages – than workers above the kink.

bound, placing the corresponding results in the Appendix.

In our preferred specification, we include year and district fixed effects to account for unobservable general time trends and time-invariant regional characteristics. This is important since the upper bound changes across years and registration to the program occurs at the district level. We also investigate how the inclusion of additional covariates affects our results. These include age and its square, marital status (captured by 4 categories), the number of dependents (3 categories) and educational attainment (3 categories). These covariates should not substantially affect our estimates provided that the identifying assumptions of the RK analysis are satisfied.

### B. Imputation of informal employment

In contrast to all other outcome variables analyzed in this section, we observe informal employment only for the sub-sample of program participants we could match with the survey data, which means that sample sizes in the proximity of the kinks are very small. Based on the matched sub-sample, we thus revert to imputing the probability for individuals in the unmatched sub-sample to be informally employed in a given month. We build on the literature that started with Blundell et al. (2008) and predicts non-durable consumption based on food expenditure (see also Browning et al., 2003; Blundell et al., 2004; Attanasio et al., 2015; Kaplan et al., 2020). This methodology distinguishes between time-varying proxy variables (i.e., the main explanatory variables in the imputation regression) and variables that serve as controls. Both types of variables need to be available for the matched and unmatched sub-samples.

Our imputed outcome variable, inf_empl_{im}, is defined as the probability for an individual i to be informally employed in month $m = \{-24, ..., 23\}$ around job loss. We focus on this two-year window to show pre-treatment trends and dynamics over time. Two empirical observations guide our choice of proxy variables. First, the likelihood of being informally employed changes around job loss. It is low in the months before job loss, but then significantly increases with time spent in the program (recall the discussion of Figure 2 above). Second, the likelihood of being informally employed in a given month does not increase for those who hold a formal job, an information that we observe for our entire sample (where the residual category is non-employment). Therefore, we include as proxy variables a dummy variable $form\_empl_{im}$, capturing whether an individual worked formally in a given month according to social security records, month since job loss fixed effects γ_m, and full interactions between these two sets of variables.[28] We additionally include demographic control variables, thereby accounting for

---

[28]Because control variables need to be identical in the imputation regression and the RK-regression of interest (Crossley et al., 2020), we include year and district fixed effects and the first term of the RK-specification (i.e., $(w_i - w_k)$) in the imputation regression.

Figure 7: Share informally employed in a given month around job loss, actual versus imputed
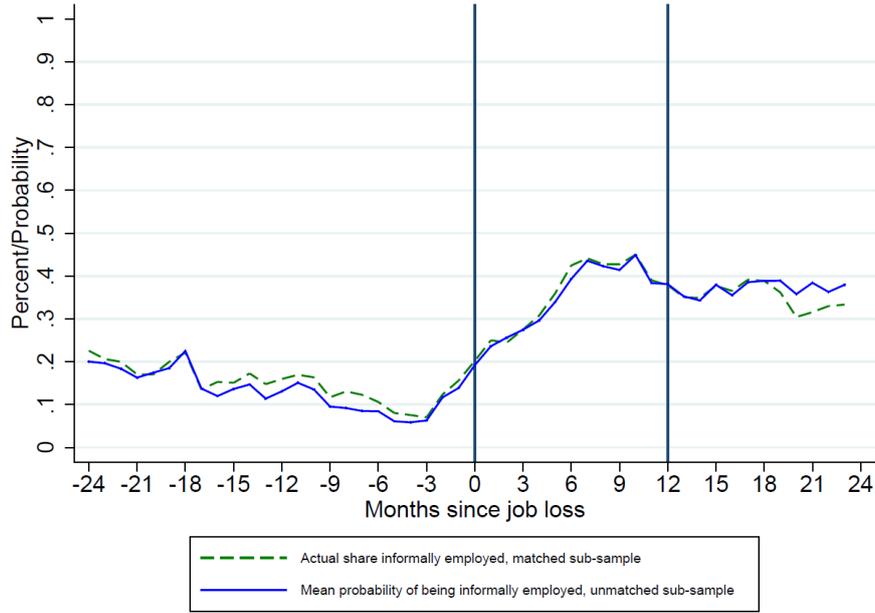
Notes: The figure shows the actual share of informally employed individuals in a given month around job loss for the matched sub-sample (dashed green line) and the imputed mean probability of being informally employed for the unmatched sub-sample (solid blue line). The imputation is based on Equation 6 in the main text and results are shown in Appendix Table B3.

any observed differences between matched and unmatched sub-samples in terms of age, gender, marital status, number of dependents or education (see Table 1 above).[29] The imputation regression is then estimated for individuals we could match with the household survey and takes the following form:

$$inf\_empl_{im} = \gamma_1 form\_empl_{im} + \gamma_m + \gamma_2 form\_empl'_{im}\gamma_m + \gamma'_3 X_i + \epsilon_{im} \qquad (6)$$

The results for this regression are shown in Appendix Table B3. Its R-squared is 0.26 and the partial R-squared pertaining to the proxy variables is 0.16, which is reasonably high given the relatively sparse set of variables. We next predict the likelihood of being informally employed in a given month for the unmatched observations. Figure 7 shows that we are able to replicate well the pattern of informal employment around job loss. However, since this prediction is naturally associated with imprecision in the imputed outcome variable, we need to account for the fact that the usual standard errors of the coefficients relating UBs to informal employment (i.e., of δ_k in equations 4 and 5) are too small. Moreover, due to non-classical, or Berkson type, measurement error in the dependent variable, these coefficients are downward biased. We

---

[29]These variables are time-constant and are measured at the time of job loss. We deliberately include them as controls, not proxies. Indeed, these controls should not yield differential predictions around the kink points of the UB schedule, given that the RK design hinges on the assumption of smooth covariate evolution.

Figure 8: Probability density function of the running variable around the upper bound
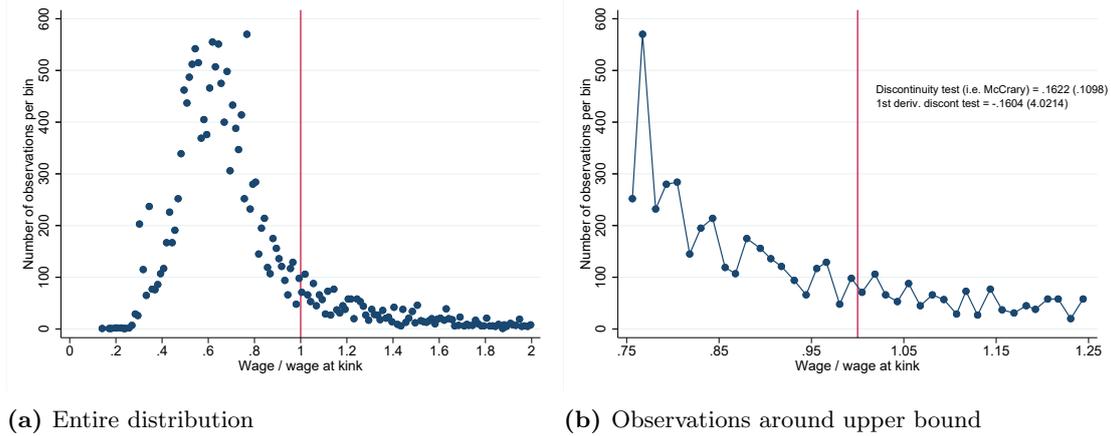
(a) Entire distribution

(b) Observations around upper bound

Notes: The figure plots the probability density function of the running variable (i.e. wage at layoff) around the upper bound. Wages are normalized by the wage at the upper bound and the bin size is equal to 0.0125. Panel A presents the entire distribution of participants, while panel B focuses on observations around the upper bound using the MSE-optimal bandwidth size. In panel B, we also report results of two tests of discontinuity. The first one is a classical McCrary test for the continuity of the probability density function at the kink. The second test follows Landais (2015) and aims to check the continuity of the partial derivative of the probability density function at the kink.

correct for both phenomena relying on the procedure and Stata routine of Crossley et al. (2020) for our RK results on informal employment.

6.2 Assessment of the identifying assumptions

The RK methodology hinges on two identifying assumptions. First, the density and the partial derivative of the density of the assignment variable are assumed to evolve smoothly around the kink. Intuitively, this rules out that observed changes in the outcomes of interest are generated by sample selection. Figure 8 displays the number of observations in each bin of the wage distribution at layoff, normalized by the wage at the upper bound for a bin size of 0.0125. Panel A shows the entire distribution of participants, while panel B focuses on observations close to the upper bound according to an MSE-optimal bandwidth. The figure shows no sign of discontinuity around the kink, which is confirmed by the McCrary (2008)-test. We also test for a possible discontinuity of the partial derivative of the density as in Landais (2015) and cannot reject the null hypothesis of no discontinuity (see Figure 8).[30]

Lack of sorting at the upper bound is coherent with the institutional setting faced by UB recipients. The upper bound has changed every year during the period of analysis and is computed as 90 per cent of the threshold at which social security contributions are capped. Identifying these values is complex and participants would also need to optimize based on the presumed schedule that will apply when they can expect to be dismissed. In contrast

---

[30]We regress the number of observations in each bin on polynomials of the running variable centered at the kink $(w - w_k)$ and the interaction term for being above the upper bound $(w - w_k)\mathbb{1}[w \geq w_k]$. The coefficient of the interaction term for the first-order polynomial is a test for the change in slope of the derivative of the density.

Figure 9: Evolution of covariates around the upper bound

(a) Gender

(b) Age

(c) Married

(d) Any dependent

(e) High education

(f) Predicted length of UB receipt
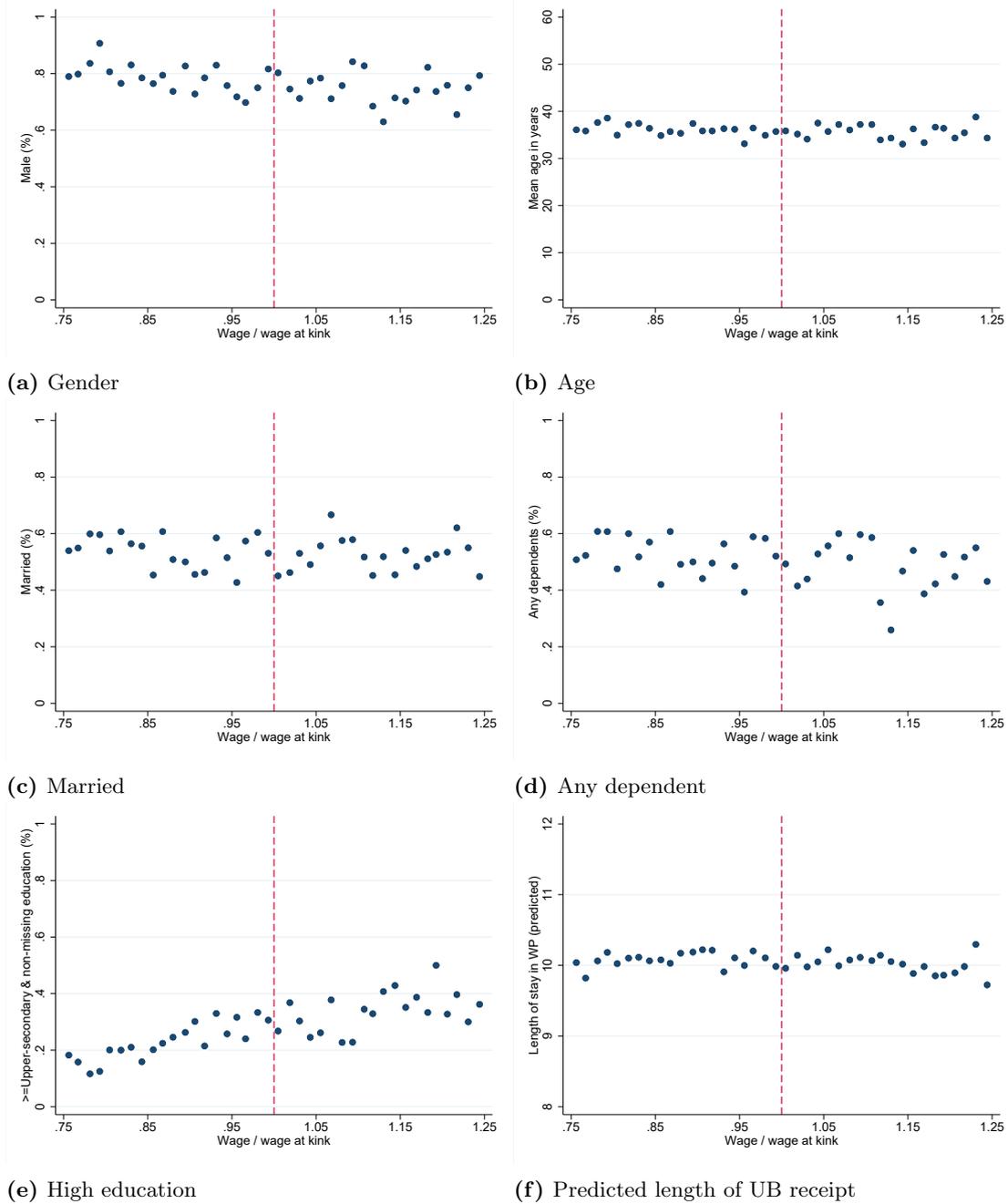
Notes: The figure plots the evolution of covariates and predicted outcomes around the upper bound of benefit levels, using a bin size of 0.0125 and an MSE-optimal bandwidth of 0.248. All variables are measured at the time of job loss, using administrative data from the Ministry of Labour. The predicted length of UB receipt is obtained with a regression using as explanatory variables: gender, age, age square, marital status, number of dependents, educational attainments, previous occupation and year and district fixed effects.

with this understanding, there is evidence of sorting at the lower bound (see Appendix Figure B1). Although we cannot directly test the reasons behind this, the lower bound was binding throughout the study period for individuals whose initial wage was equal to 10,000 Rupees. This is a round amount, where a mass of individuals is naturally concentrated. Coherently, we see similar discontinuities for placebo kinks at pre-layoff wages of 8,000 and 12,000 Rupees.

Table 2: Smoothness of covariates at the upper bound

	Age		Gender		Married		Any dependent		High education	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
δ_k (Coefficient for the interaction term)	-0.083	-0.195	0.011	0.005	0.006	0.002	-0.007	-0.012	-0.011	-0.010
	(0.215)	(0.213)	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Year and Region FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: The table shows RK-results for selected individual level characteristics measured at the time of job loss using the administrative data of the Ministry of Labour. Estimates are obtained at the upper bound with an MSE-optimal bandwidth of 0.248 around the normalized kink point. Each column refers to a separate regression, with and without year and region fixed effects. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

The second assumption needed for RK is that the marginal effect of the assignment variable on the outcomes of interest is smooth at the kink. Figure 9 shows that observable individual-level characteristics measured at the time of job loss (i.e. gender, age, marital status, presence of dependents and educational attainments) evolve smoothly around the kink at the upper bound. We confirm this result by running regressions in the form of Equation 4, but using the covariates as dependent variables (Table 2). Finally, we estimate the main outcome of interest (i.e. length of UB receipt) based on pre-determined covariates (as in Britto, 2016; Ye, 2020).[31] Panel E of Figure 9 shows that the predicted length of UB receipt evolves smoothly around the kink. Instead, the presence of bunching at the lower bound implies that some of the covariates do not evolve smoothly around that kink (i.e., gender and age, see Appendix Figure B2 and Table B4). This leads us to focus on the upper bound in the rest of the analysis and present results for the lower bound only as a matter of comparison.

6.3 Main results

A. Benefit duration and time before formal re-employment

As a first outcome, we analyze how increased UBs impact the duration of benefit receipt for individuals affected by the upper bound of the benefit schedule. The emerging pattern stands in clear contrast to the smooth evolution of covariates and the predicted outcome presented above. The scatter bin plot for length of UB receipt shows a change in the slope around the kink point (Figure 10). To the left of the kink, where benefits amount to 90 per cent of previous earnings, we observe a positive relationship between underlying wages and UB duration. That relationship becomes flat to the right of the kink, where UBs are capped at the maximum level. This suggests that replacement rates below 90 per cent induce individuals to exit the program faster and to receive UBs for a shorter time. Results are similar without controls (panel A) and

---

[31]In particular, we predict the length of UB receipt based on individual-level characteristics (i.e. gender, age, age square and educational attainments), household characteristics (i.e. dummies for marital status and number of dependents) as well as occupation in the previous job and year and district fixed effects. All of these variables are measured at the time of job loss and are therefore not affected by treatment.

Figure 10: Scatter bin plots for the duration of UB receipt (in months) at the upper bound
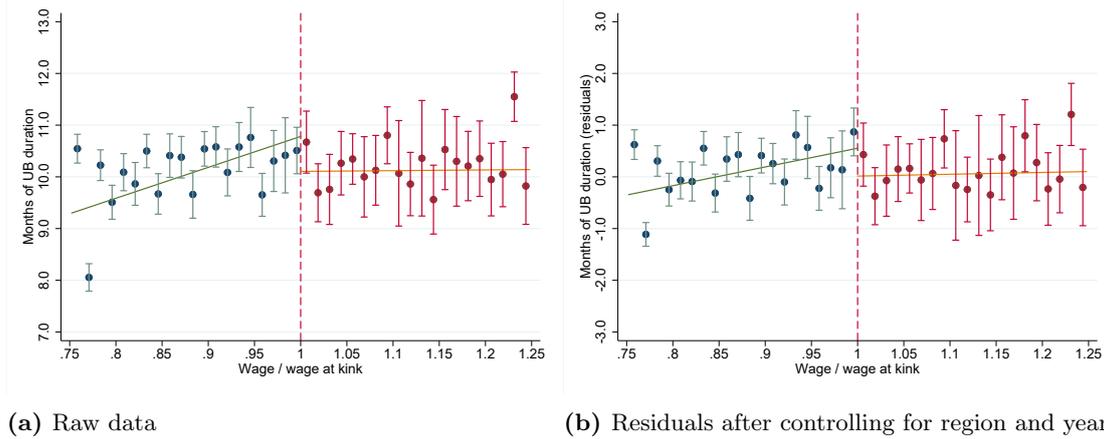
(a) Raw data

(b) Residuals after controlling for region and year

Notes: The figures show mean values and 90 per cent confidence intervals of UB receipt in months per bin of size 0.0125 around the normalized kink point, based on raw data (panel A) and after controlling for region and year fixed effects (panel B). The MSE-optimal bandwidth of 0.248 was used. Linear models were fitted on both sides and independently of the choice of bin size; i.e., all programme participants are weighted equally in the linear fits. The figure might seem to be influenced by the presence of outliers at the two extremes of the bandwidth. In reality, their exclusion would change the slope of the lines on the two sides of the cutoff while still maintaining a change in slope at the cut-off. The robustness tests will check the sensitivity of the results to bandwidth choice.

with the inclusion of year and district fixed effects (panel B, which plots regression residuals).

The RK-results in Table 3 corroborate this finding. Our preferred specification includes only district and year fixed effects and indicates that benefit receipt increases by 0.184 months due to a 1,000 Rupee (USD 61 in PPP) increase in the UB level (column 2). The effect is virtually the same when additional control variables are added (column 3). It is larger in the specification without any control variables (column 1), which demonstrates the importance of accounting for district and year. The elasticity corresponding to our preferred specification equals 0.27 (s.e. 0.11). Although they must be interpreted with some caution, we find similar effects at the lower bound (see Figure B3 and Table B5).[32] In comparison, Schmieder and von Wachter (2016) report a median elasticity of UB receipt to benefit levels of 0.30 for studies in the US and Europe (authors' calculation based on Table 2 in their review).[33] Our estimates are thus below the median of those typically found for high-income economies, despite informality being low in those countries.

UB efficiency costs also depend on the total time spent out of employment, independently from the length of benefit receipt. In this regard, we follow Gerard and Gonzaga (2021) who show that, even in a context characterized by high informality, efficiency costs arise only from the effect of UB generosity on delayed formal (but not informal) re-employment. Using our

---

[32]We again find that higher benefits increase UB duration, with an effect of 0.897 months due to a 1,000 Rupee increase and a resulting elasticity of 0.25 (s.e. 0.05) in our preferred specification. This suggests that the effects hold and are very similar in magnitude at a lower point of the income distribution.

[33]The few studies focusing on a middle-income country context (Britto, 2021; Gerard and Gonzaga, 2021) analyze benefit duration rather than levels and are thus not directly comparable.

Table 3: RK-results for the duration of receiving UBs (months), upper bound

	(1)	(2)	(3)
RK results:			
β_1 (Coefficient for $w - w_k$)	0.190***	0.092***	0.104***
	(0.034)	(0.033)	(0.032)
δ_k (Coefficient for the interaction term)	-0.275***	-0.166**	-0.163**
	(0.069)	(0.068)	(0.066)
Constant	10.297***	11.194***	10.348***
	(0.085)	(0.205)	(0.675)
Mean UB duration (D)	9.91	9.91	9.91
Treatment effect: $\alpha_k = (\delta_k/-0.9)$	0.306	0.184	0.181
Elasticity: $\eta_B = (\alpha_k * b/D)$	0.447	0.270	0.265
	(0.112)	(0.111)	(0.107)
Observations	4,463	4,463	4,463
Region and Year FEs	NO	YES	YES
Additional controls	NO	NO	YES

Notes: The table shows RK-results for the duration of receiving UBs (in months) at the upper bound and an MSE-optimal bandwidth of 0.248 around the normalized kink point. Each column refers to a separate regression. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively. Additional controls include age and its square and dummy variables capturing gender, marital status (4 categories), number of dependents (3 categories) and educational attainment (3 categories). α_k is the average treatment effect due to a 1,000 Rupee increase in UBs. Elasticities are given by α_k multiplied by the maximum benefit b in thousands (averaged across years) over the mean UB duration D at the kink point.

Figure 11: Hazard rate of formal employment and share of UB participants finding a formal job in each month after job loss
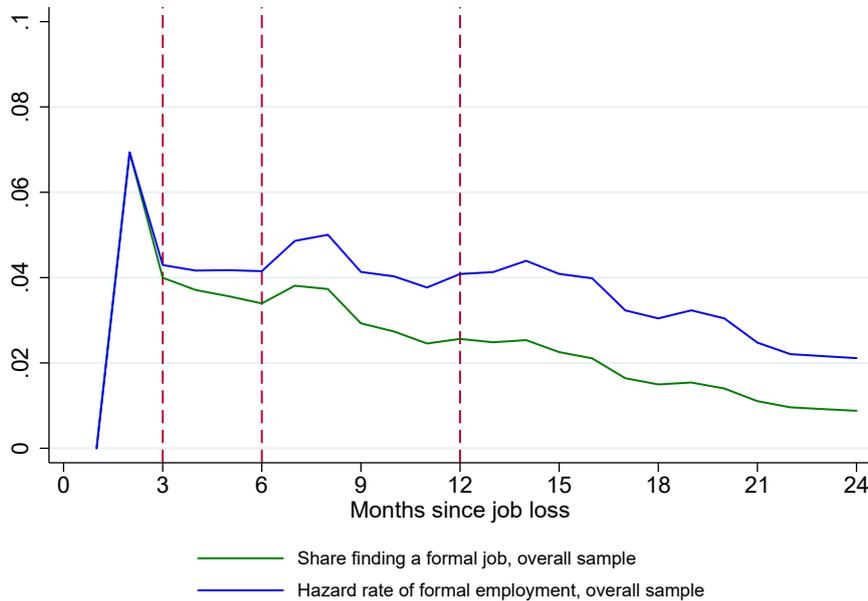
Notes: The figure shows the share of UB participants finding a formal job in any given month after job loss as well as the hazard rate of formal employment, computed as the ratio between the share formally re-employed in that month and the survival rate out of formal employment in the previous month. These variables are computed on the entire sample of UB participants with data from social security records. The three vertical lines denote when UB replacement rates vary.

preferred specification at the upper bound, we obtain elasticity estimates of the duration out of

a formal job with respect to benefit levels of 0.28 (s.e. 0.14) when we cap formal re-employment

at one year after job loss and 0.21 (statistically not-significant) when we cap it at two years. These estimates are substantially lower than those generally found in developed economies (the median estimate is equal to 0.57 in the review by Schmieder and von Wachter (2016)). This can be explained by the fact that hazard rates of formal employment remain low in our sample even after individuals lose benefit eligibility (Figure 11), while they spike right after UB exhaustion in other contexts (Card et al., 2007).[34]

B. Formal and informal employment

We next analyze formal and informal employment responses to UB generosity. To show dynamics over time and capture pre-treatment trends, we assess each month separately in a two-year window around job loss.[35] In Panels (a) and (b) of Figure 12, we compare the RK-results for formal employment at the upper bound with few and additional control variables, corresponding to specifications in columns (2) and (3) in Table 3. Here, the outcomes are dummy variables equal to one whenever an individual was formally employed in a given month as observed in the social security data. We find that higher UBs have a negative effect on formal employment in the 12 months after job loss, with seven of the twelve estimated coefficients being statistically significant at least at the ten per cent level. The effect is strongest in the fourth month after job loss, where our estimate indicates that a 1,000 Rupee increase in UBs decreases formal employment by 3.0 percentage points (the share formally employed in that month is 20 percent). The negative effect on formal employment does not persist in the second year after job loss. These findings are corroborated by the neutral pre-treatment trend and results are very similar with few or additional controls.

In Panel (c) of the same figure, we analyze informal employment. As explained in Section 6.1, the outcome variables are the imputed probabilities for individuals to be informally employed in a given month. We directly include all control variables to account for differences between the samples that we use in our imputation procedure (see the discussion in Section 6.1). We find that higher UBs increase the likelihood to be informally employed. Compared to formal employment, the effect on informality occurs with a time-lag and becomes apparent in months 4 to 12 after job loss (after correcting standard errors, eight out of these nine coefficients are statistically significant at least at the ten per cent level). The effect on informality is largest in the eighth month after job loss, where a 1,000 Rupee increase in UBs is estimated

---

[34]While the evidence for emerging economies is more limited, Gerard and Gonzaga (2021) find that in Brazil hazard rates of formal employment more than double in the month after benefit exhaustion.

[35]We directly move to the analytical results, but scatter bin plots yield the same qualitative conclusions (see Appendix Figure B4 for selected months; plots for all months are available from the authors).

Figure 12: RK-results for the probabilities of being formally and informally employed in a given month around job loss, upper bound

(a) Formal employment, Region+Year FEs

(b) Formal employment, additional controls

(c) Informal employment, additional controls

Notes: The figures show RK results for the probabilities of being formally and informally employed in a given month, for the upper bound and a bandwidth of 0.248 around the normalized kink point. Specifically, we display α_k, i.e., the average treatment effect due to a 1,000 Rupee increase in UBs. For each month, a separate regression was run. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Panel (a) shows results with sparse controls. In Panels (b) and (c), additional controls include age and its square and dummy variables capturing gender, marital status (4 categories), number of dependents (3 categories) and educational attainment (3 categories). In Panels (a) and (b), the outcomes are dummy variables equal to 1 whenever an individual was formally employed in a given month. In Panel (c), the outcomes are imputed probabilities of being informally employed in a given month, with details on the imputation provided in the text; coefficients and standard errors were adjusted using the imputation correction of Crossley et al. (2020).

to increase the likelihood of being informally employed by 7.0 percentage points (the imputed share of individuals informally employed equals 42 percent in that month). Again, the effects on informality do not persist beyond the first year after job loss and are corroborated by a neutral pre-treatment trend.

Comparing the formal and informal labor supply effects, up until the third month after job loss, we only observe the negative effect on formal employment. However, from months four to 12 we see a strong increase in informal labor supply. That is, we find that higher UBs entail a substitution from formal to informal employment. Our estimated net employment effect (i.e. formal plus informal) appears to be non-negative, although we refrain from directly comparing coefficients on formal and informal employment due to differences in the outcome

variables (i.e. dummy for formal employment and probability for informal employment). These results represent the first evidence of substitution of formal with informal employment due to an increase in UB levels in emerging and developing economies. However, they are consistent with findings from studies that have documented this type of substitution as a result of the expansion of social protection schemes (Camacho et al., 2014) or the receipt of cash transfers (Bergolo and Cruces, 2021). Similar results were also obtained in Brazil for an increase in UB duration by Britto (2021) through an indirect matching of administrative and survey data as well as by Gerard and Gonzaga (2021) using descriptive survey evidence.[36]

Finally, we replicate the results discussed in this section for the lower bound (see Appendix Figures B5 and B6). While these must be interpreted with caution, it is reassuring that after job loss they are qualitatively similar to those found at the upper bound.[37]

6.4   Robustness tests

We now present robustness tests to confirm the validity of the RK results. For ease of exposition, in the text we focus on the outcome of UB receipt and refer to our preferred specification (i.e. with only year and district dummies as controls), but results for other outcomes of interest can be consulted in the Appendix (Appendix Figure B7 and Tables B6 and B7).[38] First, we test the sensitivity of the results to the bandwidth choice. Results presented so far were obtained with the MSE-optimal bandwidth, which represents an improvement of earlier solutions (see, Imbens and Kalyanaraman (2012) and Calonico et al. (2014)). We now present results for all bandwidth from 0.05 to 0.35 (see Appendix Figure B8). The MSE-optimal bandwidth (equal to 0.248) is denoted by a long-dashed vertical line; while the Coverage Error Rate (CER) optimal bandwidth is denoted with a short-dashed red line and is equal to 0.142. In line with expectations, estimates are larger in magnitude but less precisely estimated as we get closer to the cutoff. It should also be noted that the RK design performs poorly with small samples relative to regression discontinuity methods (see Landais, 2015). Results are in any case significant at conventional

---

[36]Britto (2021) matches administrative and survey data based on a number of individual level characteristics, while not having access to individual identifiers. As a result, the same survey observation could correspond to different individuals in the administrative database. Gerard and Gonzaga (2021) compare hazard rates of total employment for unemployed in the household survey with hazard rates of formal employment for UB recipients in the administrative data, interpreting the difference between the two as evidence of informality responses.

[37]However, the pre-treatment trend is not neutral for either formal or informal employment at the lower bound. While pre-treatment trends go into the opposite direction than treatment effects (i.e. potentially suggesting that our results for the lower bound are downward biased), this leads us to have extra caution when interpreting these results. For the time after job loss, the magnitude of the effects is larger compared to the upper bound, which could be rationalized by the fact that the hypothesized 1,000 Rupee increase in UBs has a stronger economic significance for individuals entitled to only the minimum benefit level.

[38]In particular, Appendix Figure B7 shows the results on formal and informal employment by bandwidth size while Appendix Tables B6 and B7 show the sensitivity of the results on, respectively, formal and informal employment, to the choice of the polynomial order.

levels for a range of choices in the vicinity of both the MSE- and CER-optimal bandwidths.

A second concern relates to the choice of the polynomial order. Results presented so far have been obtained with a linear specification. There is a debate in the literature on how to identify the optimal model in analyses with regression discontinuities or kinks (Pei et al., 2020), generally favoring lower polynomial orders (Gelman and Imbens, 2019). We observe that the Aikake Information Criterion (AIC) marginally prefers the quadratic specification, while the Bayesian Information Criterion (BIC) strongly favors a linear model. The main results on length of UB receipt are generally confirmed in specifications that use the two preferred polynomial orders (although they partially lose significance in some of the quadratic specifications), while they change sign and lose statistical significance in the cubic specification (see Appendix Table B8). This sensitivity is not particularly worrying given the discussion in the literature, and the results of the AIC and BIC tests, and is common to a number of studies that adopt RK methods (see Böckerman et al., 2018; Manoli and Turner, 2018; Ye, 2020).

A final set of tests refers to the possible functional dependence between the forcing variable and the outcome of interest. We assume that this relationship is smooth at the kink, but it could be kinked or quadratic. First, we aim to detect non-parametrically the presence of a kink by looking at the breakpoint that minimizes the residual sum of squares. Appendix Figure B9 plots the R^2 for the model with length of UB receipt as the dependent variable for different kink points. It shows that the R^2 increases sharply in the proximity of the true kink. We also run RK estimates with a placebo forcing variable. The challenge is to identify a meaningful placebo (i.e. a proxy of previous earnings) that is at the same time not too correlated with wages at job loss. We follow Landais (2015) in using re-employment wages for formal employment. We do not find any treatment effect in this placebo test (Appendix Table B9). Finally, we exploit variations in the location of the kink in absolute terms over time (see Figure 6) in a double difference RK design (DD-RK). The intuition is to compare slope changes in the relationship between the outcome of interest and the forcing variables between years when the kink was in place at that point of the wage distribution and years in which the kink was somewhere else.[39] Appendix Table B10 shows that the DD-RK estimates confirm the results of the analysis, while being larger in magnitude.

---

[39]As stated in Landais (2015), the problem is that each yearly change in the location of the kink taken separately would not provide for enough statistical power as the bandwidths would largely overlap. At the same time, taking years further apart in time would increase the risk that the parallel trend assumption is violated. For the purpose of this exercise, we will be comparing kinks that apply in 2013 and 2016. However, the results would be confirmed also with a different choice of the years.

7 Welfare analysis

In the last part of the paper, we bring together the results from the DiD and RK analyses to derive the marginal welfare effect of increasing UB levels. We follow the sufficient statistics approach of Baily (1978) and Chetty (2006, 2008), as adapted by Gerard and Gonzaga (2021) to contexts of high informality and presented in Section 2. We have already estimated most of the terms in Equation 1 (recall that the insurance value is approximated by the flow drop in consumption, rescaled by the coefficient of relative risk aversion, Chetty (2008)).

Starting with the insurance value, we have documented in Section 5 a sharp and persistent drop in consumption following job loss. However, those results were obtained for the overall sample of UB recipients. To consistently estimate the insurance value and efficiency cost for the same underlying population, we replicate the main findings of the DiD analysis for the sub-sample in the proximity of the upper bound. Appendix Figure A10 shows that the main results continue to hold for individuals around the kink. However, we want to retrieve a higher frequency measure of the drop in consumption. To this end, we follow Landais and Spinnewijn (2020) and Kolsrud et al. (2018) and exploit the fact that those interviewed in the CMPHS in the year following job loss have spent a different number of months in unemployment at the time of the interview. Restricting the analysis to individuals around the upper bound, we augment the DiD specification (Equation 3 in Section 5.1) by including an additional term for the number of months spent in unemployment at the time of interview, to obtain an estimate of the consumption drop at the monthly level.[40] This yields an estimated drop in consumption equal to 0.281 (s.e. 0.126), which is very similar to what we discussed in Section 5.2 for the sample of all UB recipients, but again larger than most estimates from advanced economies.

Turning to the efficiency costs, we take our preferred estimate of elasticity of benefit duration (i.e. 0.27) and time for formal re-employment (i.e. 0.28). These are local estimates stemming from our analysis of the upper bound and can be directly used to estimate the efficiency costs as defined in Equation 1. B and D are observed in the sample as the mean duration in the UB program and the mean time before formal re-employment, respectively, for the population around the upper bound (equal to 9.91 and 14.93, see column (2) in Appendix Table B2). Finally, we follow Landais (2015) and Gerard and Gonzaga (2021) in approximating $\frac{\tau}{b}$ with the share of UB recipients per formal employee in the country.[41] This leads us to an estimate of 0.283

---

[40]This further requires assuming a linear relationship between months spent in unemployment and the consumption drop. Suggestive evidence in favor of this assumption is provided by the evolution of coefficient estimates in the two years after job loss (see panel C in Appendix Figure A10).

[41]This leads to an estimated value of $\frac{\tau}{b}$ equal to 0.024. This is quite low compared to previous studies (e.g. Gerard and Gonzaga (2021) use a value of 0.086) and, given the average replacement rate of 0.525 throughout

Table 4: Welfare effects from increasing UB levels

(1) Efficiency costs	(2) Average consumption drop	(3) Coefficient of risk aversion	(4) Welfare effects
0.283	0.281	1	-0.002
		2	0.279
		5	1.122

Notes: The table reports the estimated efficiency costs of UBs in column (1) and the average consumption drop in column (2) (which, rescaled by the coefficient of relative risks aversion, approximates the insurance value of the UBs). Column (4) combines these two effects to estimate the welfare effects of an increase in UB levels. These are estimated for different values of the coefficient of relative risk aversion. See equation 1 and the text for definitions and explanations.

for the behavioral cost, meaning that a one unit increase in UBs will generate a public cost of 1.283 once behavioral responses are taken into account.[42] This is lower than most estimates for advanced economies (Schmieder and von Wachter, 2016). However, our results are very similar to those obtained by Gerard and Gonzaga (2021) in Brazil for an increase in UB duration. This confirms that efficiency costs might be lower in emerging and developing economies, despite higher informality rates. This finding can be rationalized by the fact that informal jobs pay comparatively low wages and that finding a formal job is difficult.

In Table 4, we present our estimates of the welfare effects of an increase in UB levels. The results show that welfare effects are close to zero for a coefficient of relative risk aversion equal to one, and positive for any value above one. While there is debate in the literature on the value of the coefficient of risk aversion, most studies generally take one as the lower bound (Chetty and Szeidl, 2007; Schmieder and von Wachter, 2016). This implies that an increase in benefit levels would likely be welfare improving in our context. Our welfare estimates are in any case more favorable than those obtained in previous studies in advanced economies (i.e. the median estimate for $\gamma = 2$ is equal to 0.21 in the review by Schmieder and von Wachter (2016)), whereas no previous estimates exist for low- and middle-income economies.

8   Conclusions

This paper analyzes the welfare effects of UB generosity in a context of high informality, where it is a priori unclear whether the presence of informal jobs increases or decreases welfare gains. We provide one of the first estimates in low- and middle-income countries of the behavioral response to UB generosity and of the drop in consumption at layoff, while bringing together these elements for the first time. Having access to data on employment, wages and consumption

---

the year of benefit eligibility, it would be consistent with a payroll tax rate of 0.013 per cent. In comparison, Schmieder and von Wachter (2016) use a tax rate fixed at 0.03 to compute the welfare effects in their sample of studies. The alternative approach used in the literature would be to approximate $\frac{\tau}{b}$ with the unemployment rate (Chetty, 2008; Schmieder et al., 2012), which would lead to a value of 0.078 in our study. However, efficiency costs would not substantially change (see below in the text).

[42]The behavioral cost would be equal to 0.306 if we were approximating $\frac{\tau}{b}$ with the unemployment rate.

of formal and informal workers, we characterize both the efficiency cost and the insurance value of UBs in terms of the substitutability between formal and informal jobs. This is important to inform a debate on the nature and role of informal jobs.

Our results reveal that the detrimental effects of job loss are large in magnitude and persist over time, while the response to UB generosity are relatively small and rapidly disappear. As a result, we find that efficiency costs are below the median of existing estimates from high-income economies while our estimate of the insurance value of UBs is substantially larger than previous results. This can be interpreted by the fact that displaced workers move to informal employment out of necessity, having to accept lower-paying jobs. This, in turn, generates a large drop in consumption, which persists over time as individuals fail to return to formal employment. Welfare effects of UB generosity are therefore positive and comparatively large.

The findings of the paper have important policy implications, as they show that the unintended consequences of UB schemes in low- and middle-income countries might have been overstated and their benefits sometimes overlooked. UB schemes are still absent in around half of all middle-income and in most low-income countries (Asenjo et al., 2019), partly due to a general understanding that efficiency costs would be substantially higher in contexts of high informality (Duval and Loungani, 2019; Robalino et al., 2009). Our empirical results, however, suggest otherwise. In a context characterized by high informality, a formal job gives workers a key economic advantage and its loss represents an event from which it is difficult to recover. Providing UBs to formally displaced workers is therefore a sensible policy option, with the welfare gains largely outweighing the efficiency costs.

References

Amarante, V., R. Arim, and A. Dean (2014). The Effects of Being Out of the Labor Market on Subsequent Wages: Evidence for Uruguay. *Journal of Labor Research 35*, 39–62.

Asenjo, A., V. Escudero, H. Liepmann, C. Pignatti, and D. Tabasso (2019). *What Works: Promoting Pathways to Decent Work*. Geneva: Studies on Growth with Equity Series.

Attanasio, O., E. Hurst, and L. Pistaferri (2015). The Evolution of Income, Consumption, and Leisure Inequality in the United States, 1980-2010. In C. D. Carroll, T. F. Crossley, and J. Sabelhaus (Eds.), *Improving the Measurement of Consumer Expenditures*, pp. 100–140. Chicago: Studies in Income and Wealth, Vol. 74, University of Chicago Press.

Azuara, O. and I. Marinescu (2013). Informality and the expansion of social protection programs: Evidence from mexico. *Journal of Health Economics 32*(5), 938 – 950.

Baily, M. N. (1978). Some aspects of optimal unemployment insurance. *Journal of Public Economics 10*(3), 379 – 402.

Böckerman, P., O. Kanninen, and I. Suoniemi (2018). A kink that makes you sick: The effect of sick pay on absence. *Journal of Applied Econometrics 33*(4), 568–579.

Bergolo, M. and G. Cruces (2014). Work and tax evasion incentive effects of social insurance programs: Evidence from an employment-based benefit extension. *Journal of Public Economics 117*, 211 – 228.

Bergolo, M. and G. Cruces (2021). The Anatomy of Behavioral Responses to Social Assistance When Informal Employment Is High. *Journal of Public Economics 193*.

Blundell, R., L. Pistaferri, and I. Preston (2004). Imputing Consumption in the PSID using Food Demand Estimates from the CEX. Institute for Fiscal Studies, Working Paper WP04/27.

Blundell, R., L. Pistaferri, and I. Preston (2008). Consumption Inequality and Partial Insurance. *American Economic Review 98*(5), 1887–1921.

Bosch, M. and R. M. Campos-Vazquez (2014, November). The Trade-Offs of Welfare Policies in Labor Markets with Informal Jobs: The Case of the "Seguro Popular" Program in Mexico. *American Economic Journal: Economic Policy 6*(4), 71–99.

Bosch, M. and J. Esteban-Pretel (2012). Job creation and job destruction in the presence of informal markets. *Journal of Development Economics 98*, 270–286.

Bosch, M. and N. Schady (2019). The effect of welfare payments on work: Regression discontinuity evidence from Ecuador. *Journal of Development Economics 139*, 17 – 27.

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

Britto, D. (2016). Unemployment Insurance and the Duration of Employment: Theory and Evidence from a Regression Kink Design. DISCE - Working Papers del Dipartimento di Economia e Finanza def048, Università Cattolica del Sacro Cuore, Dipartimenti e Istituti di Scienze Economiche (DISCE).

Britto, D. (2021). The employment effects of lump-sum and contingent job insurance policies: Evidence from Brazil. *The Review of Economics and Statistics, forthcoming*.

Browning, M. and T. F. Crossley (2001). Unemployment insurance benefit levels and consumption changes. *Journal of Public Economics 80*(1), 1 – 23.

Browning, M., T. F. Crossley, and G. Weber (2003). Asking Consumption Questions in General Purpose Surveys. *Economic Journal 113*(491), F540–F567.

Calonico, S., M. Cattaneo, M. Farrell, and R. Titiunik (2017). rdrobust: Software for regression-discontinuity design. *The Stata Journal 17*(2), 372–404.

Calonico, S., M. D. Cattaneo, and R. Titiunik (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica 82*(6), 2295–2326.

Camacho, A., E. Conover, and A. Hoyos (2014). Effects of Colombia's social protection system on workers' choice between formal and informal employment. *World Bank Economic Review 28*(3), 446–466.

Card, D., R. Chetty, and A. Weber (2007). The spike at benefit exhaustion: Leaving the unemployment system or starting a new job? *The American Economic Review 97*(2), 113–118.

Card, D., D. Lee, Z. Pei, and A. Weber (2015). Inference on Causal Effects in a Generalized Regression Kink Design. *Econometrica 83*(6), 2453–2483.

Card, D., D. Lee, Z. Pei, and A. Weber (2017). Regression Kink Design: Theory and Practice. *Advances in Econometrics 38*, 341–382.

Carrillo, P., D. Pomeranz, and M. Singhal (2017). Dodging the taxman: Firm misreporting and limits to tax enforcement. *American Economic Journal: Applied Economics 9*(2), 144–64.

Chan, S. and A. Stevens (2004). How does job loss affect the timing of retirement? *Contributions to Economic Analysis and Policy 3*(1), 403–428.

Chetty, R. (2006). A general formula for the optimal level of social insurance. *Journal of Public Economics 90*(10), 1879 – 1901.

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

Chetty, R. and A. Szeidl (2007). Consumption Commitments and Risk Preferences. *The Quarterly Journal of Economics 122*(2), 831–877.

Couch, K. A. (1998). Late Life Job Displacement. *The Gerontologist 38*(1), 7–17.

Couch, K. A., N. A. Jolly, and D. W. Placzek (2009). Earnings losses of older displaced workers: A detailed analysis with administrative data. *Research on Aging 31*(1), 17–40.

Couch, K. A. and D. W. Placzek (2010). Earnings losses of displaced workers revisited. *American Economic Review 100*(1), 572–89.

Crossley, T. F., P. Levell, and S. Poupakis (2020). Regression with an Imputed Dependent Variable. Institute for Fiscal Studies, Working Paper W20/25.

Díaz, J. J., J. Chacaltana, J. Rigolini, and C. Ruiz (2018). Pathways to Formalization: Going beyond the Formality Dichotomy. IZA Discussion Paper No. 11750.

Duval, R. A. and P. Loungani (2019). Designing Labor Market Institutions in Emerging and Developing Economies: Evidence and Policy Options. IMF Staff Discussion Note 19/04.

Galasso, E. and M. Ravallion (2004). Social protection in a crisis: Argentina's plan Jefes y Jefas. *The World Bank Economic Review 18*(3), 367–399.

Galiani, S. and P. J. McEwan (2013). The heterogeneous impact of conditional cash transfers. *Journal of Public Economics 103*, 85 – 96.

Ganong, P. and P. Noel (2019). Consumer spending during unemployment: Positive and normative implications. *American Economic Review 109*(7), 2383–2424.

Garganta, S. and L. Gasparini (2015). The impact of a social program on labor informality: The case of AUH in Argentina. *Journal of Development Economics 115*, 99 – 110.

Gelman, A. and G. Imbens (2019). Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics 37*(3), 447–456.

Gerard, F. and G. Gonzaga (2021). Informal Labor and the Efficiency Cost of Social Programs: Evidence from the Brazilian Unemployment Insurance Program. *American Economic Journal: Economic Policy, forthcoming.*

Gerard, F. and J. Naritomi (2021). Job Displacement Insurance and (the Lack of) Consumption-Smoothing. *American Economic Review 111*(3), 899–942.

Giupponi, G. (2019). When Income Effects are Large: Labor Supply Responses and the Value of Welfare Transfers. Working Paper.

González-Rozada, M. and H. Ruffo (2016). Optimal Unemployment Benefits in the Presence of Informal Labor Markets. *Labour Economics 41*, 204–227.

Gruber, J. (1997). The consumption smoothing benefits of unemployment insurance. *The American Economic Review 87*(1), 192–205.

ILO (2021). *World Social Protection Report*. Geneva (forthcoming).

Imbens, G. and K. Kalyanaraman (2012). Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *Review of Economic Studies 79*(3), 933–959.

Inderbitzin, L., S. Staubli, and J. Zweimüller (2016). Extended unemployment benefits and early retirement: Program complementarity and program substitution. *American Economic Journal: Economic Policy 8*(1), 253–88.

Jacobson, L. S., R. LaLonde, and D. Sullivan (1993). Earnings losses of displaced workers. *American Economic Review 83*(4), 685–709.

Kaplan, G., K. Mitman, and G. L. Violante (2020). Non-durable Consumption and Housing Net Worth in the Great Recession: Evidence from Easily Accessible Data. *Journal of Public Economics 189*, 104176.

Kolsrud, J., C. Landais, P. Nilsson, and J. Spinnewijn (2018). The Optimal Timing of Unemployment Benefits: Theory and Evidence from Sweden. *American Economic Review 108*(4-5), 985–1033.

Kolsrud, J., C. Landais, and J. Spinnewijn (2020). The value of registry data for consumption analysis: An application to health shocks. *Journal of Public Economics 189*, 104088.

Kroft, K. and M. Notowidigno (2016). Should Unemployment Insurance Vary with the Unemployment Rate? Theory and Evidence. *Review of Economic Studies 83*, 1092–1124.

La Porta, R. and A. Shleifer (2014). Informality and Development. *Journal of Economic Perspectives 28*(3), 109–126.

Landais, C. (2015). Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design. *American Economic Journal: Economic Policy 7*(4), 243–278.

Landais, C. and J. Spinnewijn (2020). The Value of Unemployment Insurance. Working Paper.

Liepmann, H. and C. Pignatti (2019). Participation and Eligibility in Unemployment Benefit Schemes: Evidence from Mauritius. ILO Research Department Working Paper No. 50.

Lusardi, A. (1996). Permanent income, current income, and consumption: Evidence from two panel data sets. *Journal of Business & Economic Statistics 14*(1), 81–90.

Manoli, D. and N. Turner (2018). Cash-on-hand and college enrollment: Evidence from population tax data and the earned income tax credit. *American Economic Journal: Economic Policy 10*(2), 242–71.

McCaig, B. and N. Pavcnik (2015). Informal employment in a growing and globalizing low-income country. *American Economic Review: Papers and Proceedings 105*(5), 545–550.

McCrary, J. (2008). Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test. *Journal of Econometrics 142*(2), 698–714.

Meghir, C., R. Narita, and J.-M. Robin (2015). Wages and Informality in Developing Countries. *American Economic Review 105*(4), 1509–1546.

Naritomi, J. (2019). Consumers as tax auditors. *American Economic Review 109*(9), 3031–72.

National Research Council (1993). *Population and Land Use in Developing Countries: Report of a Workshop*. Washington, DC: The National Academies Press.

Pei, Z., D. S. Lee, D. Card, and A. Weber (2020). Local polynomial order in regression discontinuity designs. Working Paper 27424, National Bureau of Economic Research.

Robalino, D. A., M. Vodopivec, and A. Bodor (2009). Savings for Unemployment in Good or Bad Times: Options for Developing Countries. IZA Discussion Papers 4516, Institute of Labor Economics (IZA).

Schmieder, J. and T. von Wachter (2016). The Effects of Unemployment Insurance Benefits: New Evidence and Interpretation. *Annual Review of Economics 8*, 547–581.

Schmieder, J., T. von Wachter, and S. Bender (2012). The Effects of Extended Unemployment Insurance over the Business Cycle: Evidence from Regression Discontinuity Estimates over 20 Years. *Quarterly Journal of Economics 127*(2), 701–752.

Skoufias, E. and V. D. Maro (2008). Conditional cash transfers, adult work incentives, and poverty. *The Journal of Development Studies 44*(7), 935–960.

Statistics Mauritius (2017). Mauritius in Figure. http://statsmauritius.govmu.org/English/ Publications/Documents/MIF/MIF_2017.pdf [retrieved May 2019].

Ulyssea, G. (2018). Firms, Informality, and Development: Theory and Evidence from Brazil. *American Economic Review 108*(8), 2015–2047.

Ulyssea, G. (2020). Informality: Causes and Consequences for Development. *Annual Review of Economics 12*, 525–546.

Verho, J. (2020). Economic crises and unemployment persistence: Analysis of job losses during the finnish recession of the 1990s. *Economica 87*(345), 190–216.

Ye, H. (2020). The Effect of Pension Subsidies on the Retirement Timing of Older Women. Working paper.

Appendices

A Appendix: The insurance value of UBs and underlying labor market mechanisms

Figure A1: DiD results on total transfers and consumption expenditures by re-employment status
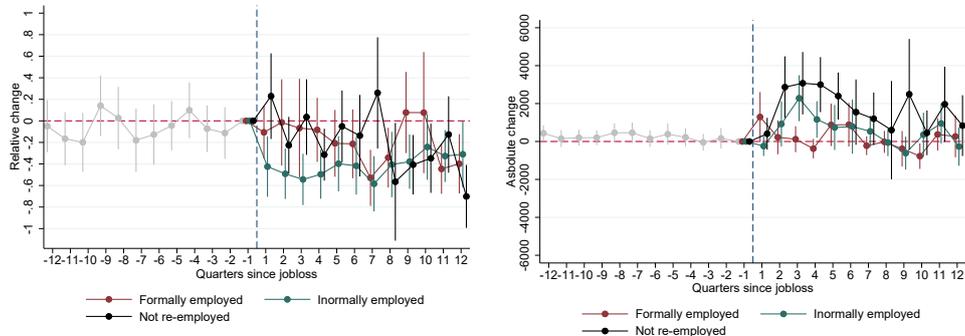
(a) Consumption expenditures by re-employment status

(b) Total transfers by re-employment status

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. Figures showing results for different sub-groups according to their re-employment status include the whole sample in the period before layoff (i.e. independently from their re-employment patterns after job loss), which is why pre-treatment trends coincide and they are denoted by a unique line. All outcomes of interest are taken from the CMPHS.

Figure A2: DiD results on household transfers
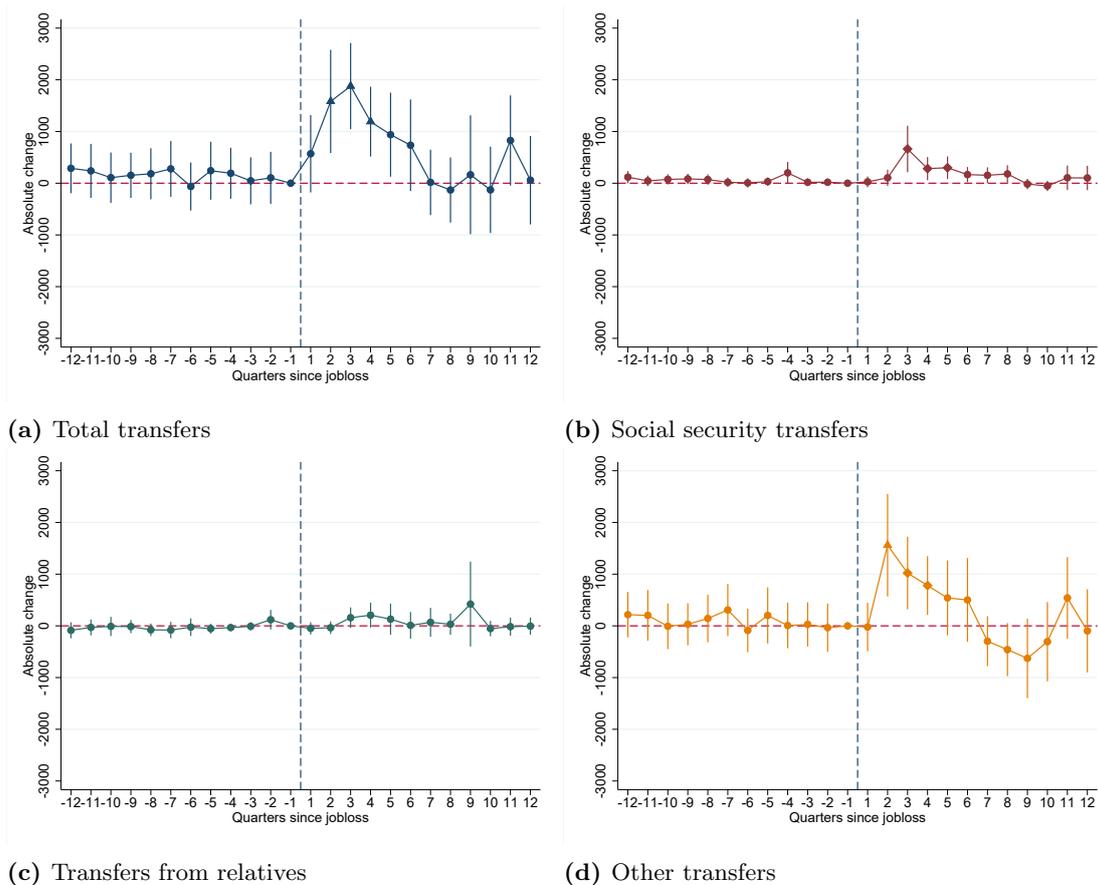
(a) Total transfers

(b) Social security transfers
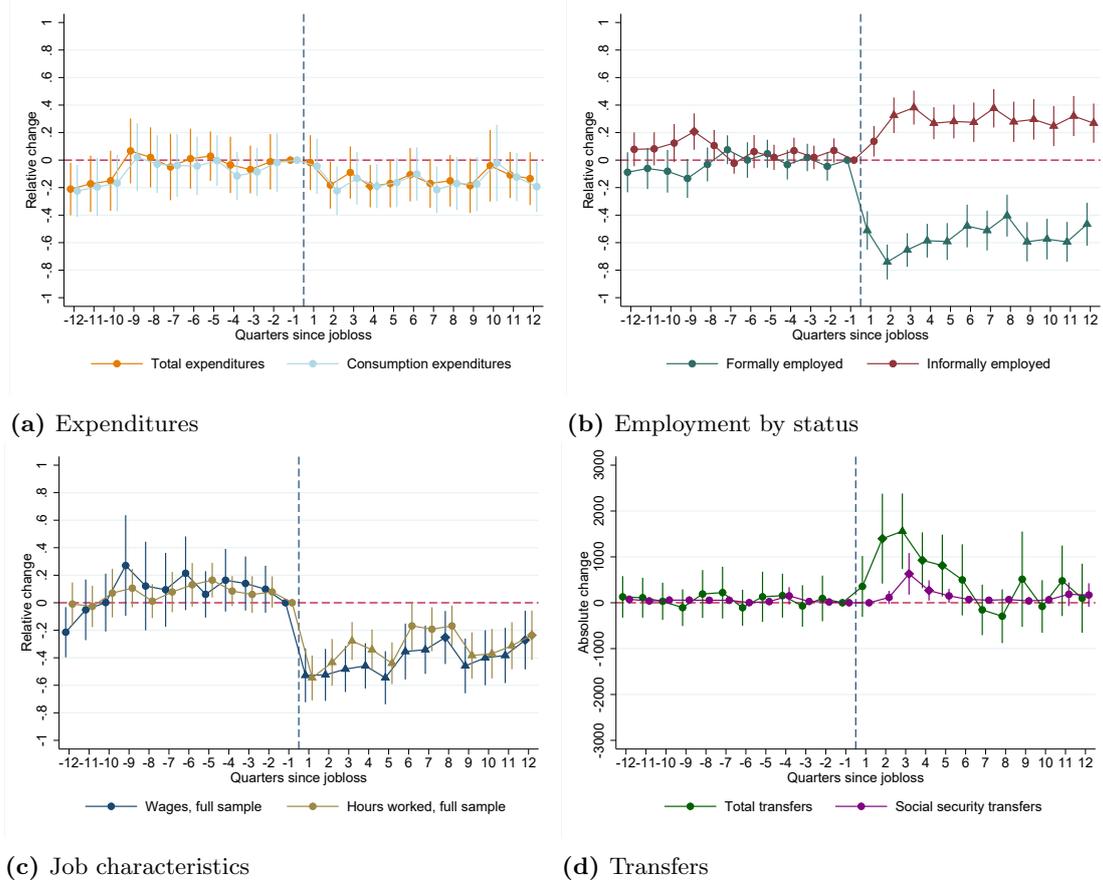
(c) Transfers from relatives

(d) Other transfers

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels and for this set of outcomes of interest they are not rescaled by dividing estimates by the mean in the treatment group in the quarter before job loss, since this mean would be equal to zero in most of the cases. All outcomes of interest are taken from the CMPHS.

Figure A3: Results using an event study approach

(a) Expenditures

(b) Employment by status

(c) Job characteristics

(d) Transfers

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. Compared to the baseline specification, these results are obtained with a pure event study approach in which we do not use any control group. All outcomes of interest are taken from the CMPHS.

Figure A4: DiD results without propensity score weights

(a) Expenditures

(b) Employment by status
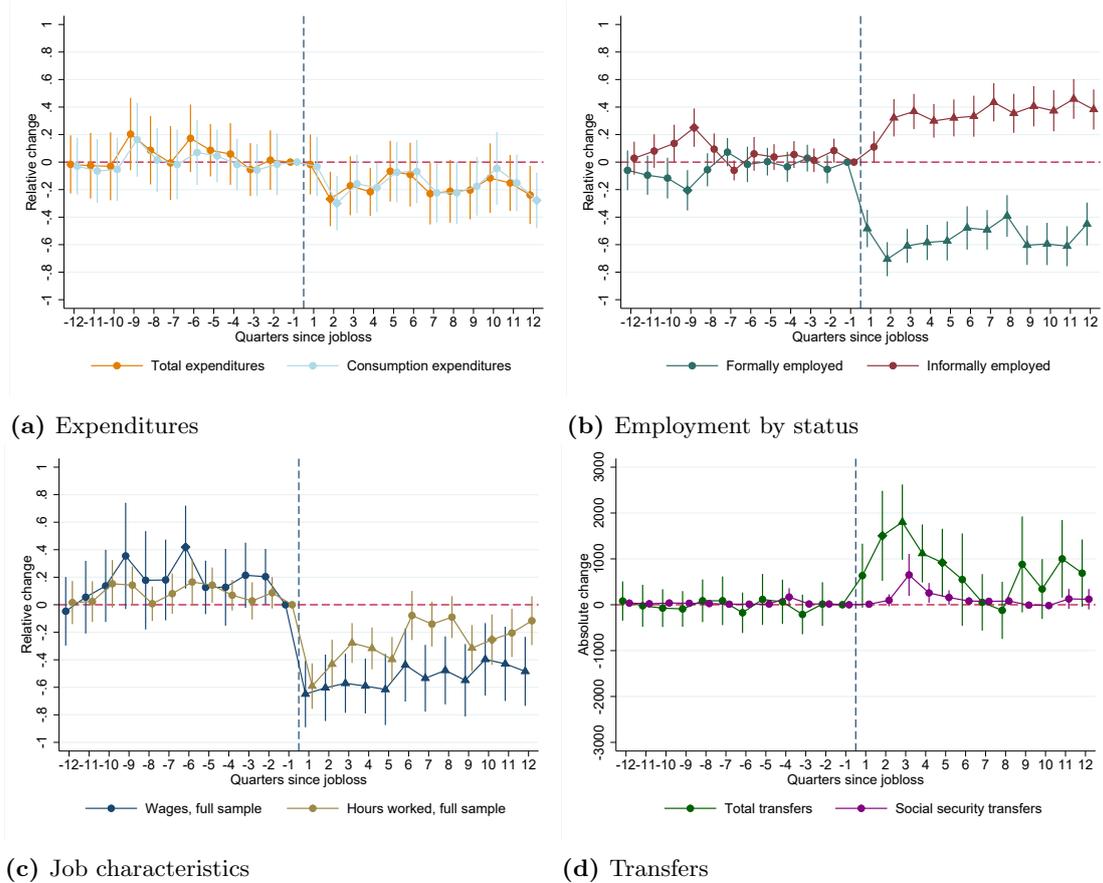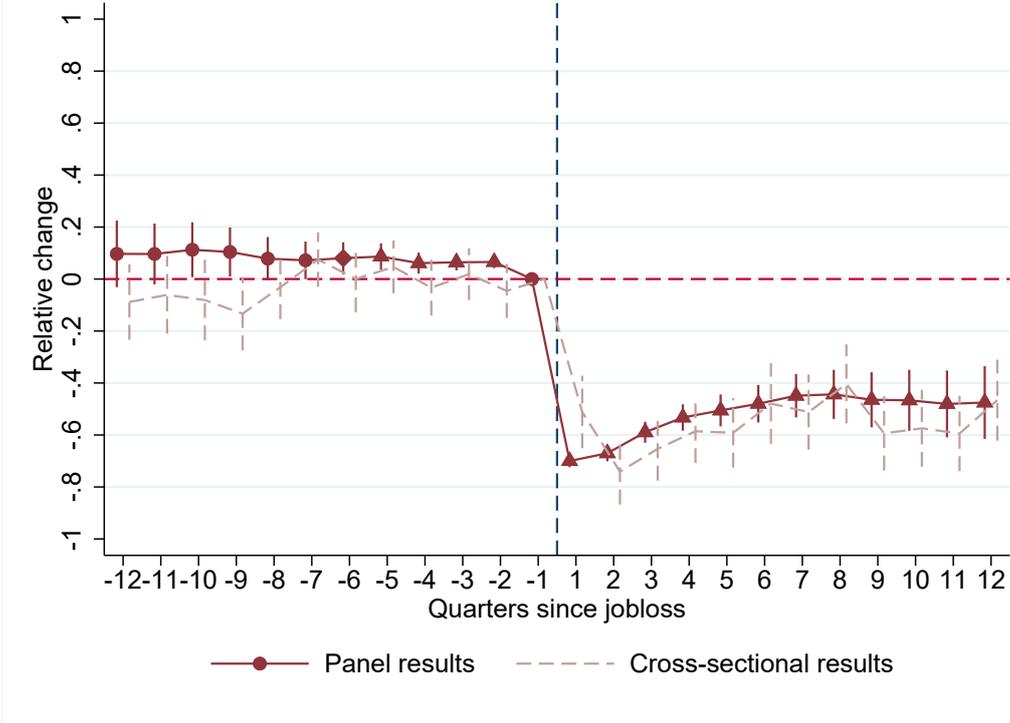
(c) Job characteristics

(d) Transfers

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. Compared to the baseline specification, these results are obtained without re-weighting observations using the propensity score. All outcomes of interest are taken from the CMPHS.

Figure A5: DiD results on formal employment from panel and cross-sectional regressions

Figure A6: DiD results on the sample of UB recipients dismissed for economic reasons

(a) Expenditures

(b) Employment by status
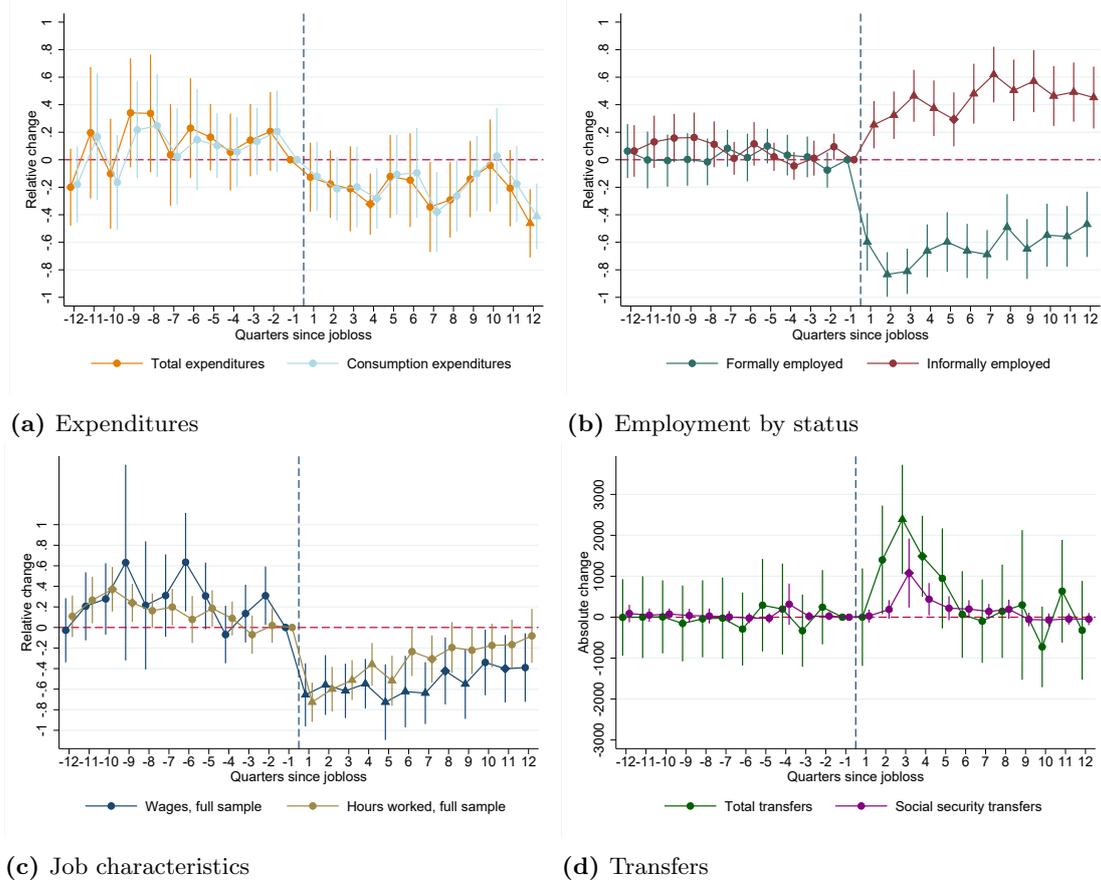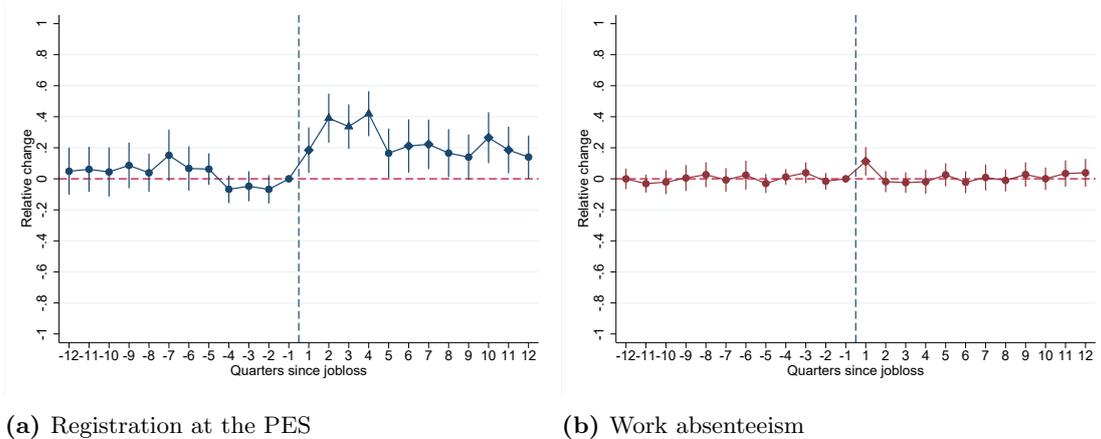

(c) Job characteristics

(d) Transfers

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. Compared to the baseline specification, these results are obtained by restricting the treatment sample to individuals who have been dismissed for economic reasons as defined in the main text. All outcomes of interest are taken from the CMPHS.

Figure A7: DiD results on PES registration and work absenteeism

(a) Registration at the PES

(b) Work absenteeism

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. This figure plots results on the probability of being registered at the PES (panel A) and the probability of having been absent at work in the reference week (panel B). All outcomes of interest are taken from the CMPHS.

Figure A8: DiD results on wages and total expenditures, by date of formal re-employment

(a) Wages

(b) Total expenditures

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. This figure reports results on overall wages (panel A) and total expenditures (panel B) for three groups: those who found a formal job (i) within six months, (ii) within 12 months, and (iii) beyond 12 months. The entire treatment group is included before job loss, which is why pre-treatment trends coincide and they are denoted by a unique line. The date of formal re-employment is taken from the social security records, while the outcomes of interests are observed in the CMPHS.

Figure A9: DiD results on formal and informal wages, different specifications
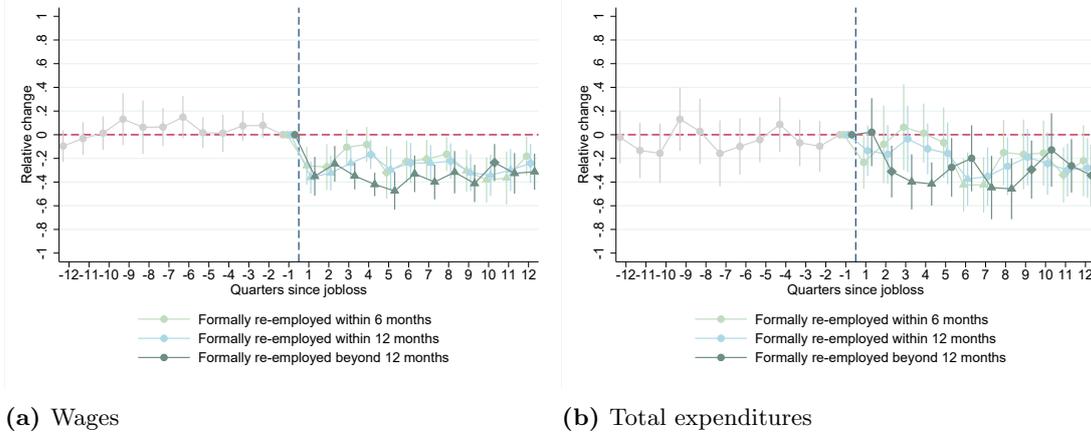

(a) Formal wages

(b) Informal wages

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. This figure plots results on formal wages (panel A) and informal wages (panel B) using three different specifications: (i) with no weights, (ii) with the weights used in the baseline results, and (iii) with weights obtained with an augmented specification (see text for details). The entire treatment group is included before job loss, which is why pre-treatment trends coincide and they are denoted by a unique line. All outcomes of interest are taken from the CMPHS.

52

Figure A10: DiD results on the sample of UB recipients around the upper bound

(a) Expenditures

(b) Employment by status
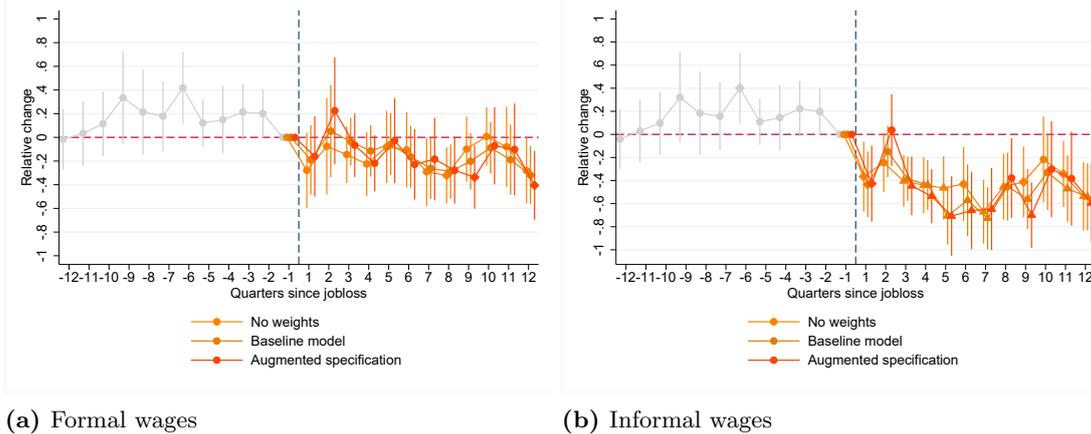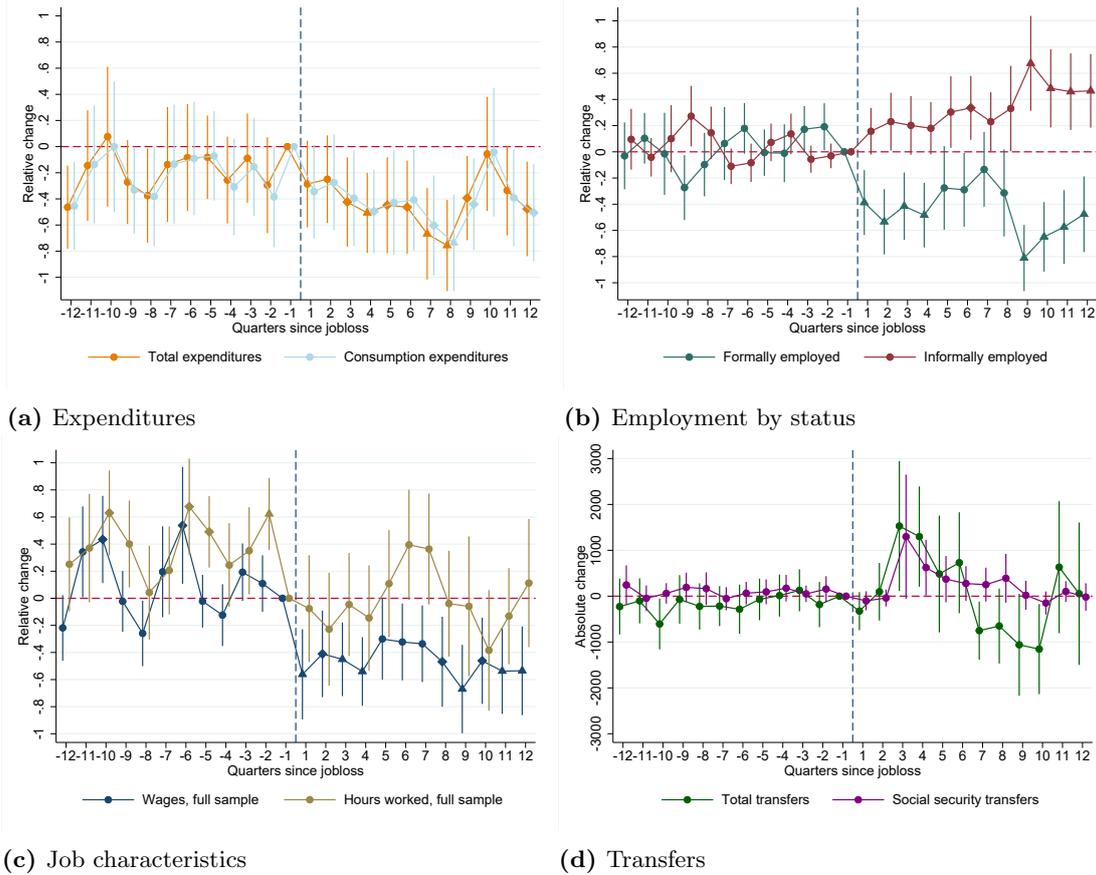
(c) Job characteristics

(d) Transfers

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Equation 3. Confidence intervals refer to the 90 per cent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Regressions are run in levels, but to facilitate the interpretation results are presented in relative changes by dividing the estimates by the mean in the outcome of interest in the treatment group in the quarter before job loss. Compared to the baseline specification, these results are obtained by restricting the treatment sample to individuals who are around the upper bound based on the MSE-optimal bandwidth used in the RK analysis. All outcomes of interest are taken from the CMPHS.

Table A1: Summary statistics for the DiD analysis: treated and control groups

	Treated (1)	Control (2)	Difference (2)-(1)
Male (0/1)	0.723	0.636	-0.0877***
Age (in years)	41.332	41.975	0.6426**
Marital status: Married	0.703	0.743	0.0398***
Marital status: Single	0.200	0.185	-0.0152
Marital status: Separated	0.097	0.072	-0.0246***
Kinship status: Head	0.526	0.506	-0.0197
Kinship status: Spouse	0.184	0.224	0.0391***
Kinship status: Child	0.256	0.248	-0.0077
Kinship status: Grand child	0.003	0.002	-0.0006
Kinship status: Other	0.032	0.021	-0.0112**
Educational attainment: Primary	0.445	0.270	-0.1752***
Educational attainment: Secondary	0.485	0.550	0.0649***
Educational attainment: Tertiary	0.045	0.172	0.1269***
Educational attainment: Other	0.025	0.008	-0.0166***
N	1041	13494	

Notes: Variables means are shown for the treatment group of UB recipients (column (1)), and the control group of individuals who never lose their job according to social security records (column (2)). Column (3) displays the difference between columns (2) and (1) and the results from a two-sided t-test, where *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively. All variables are measured from the CMPHS and refer to the time of the survey interview, which might take place in the three years around the (placebo) layoff.

B Appendix: The efficiency costs of UBs and the substitution between formal and informal employment

Figure B1: Probability density function of the running variable around the lower bound
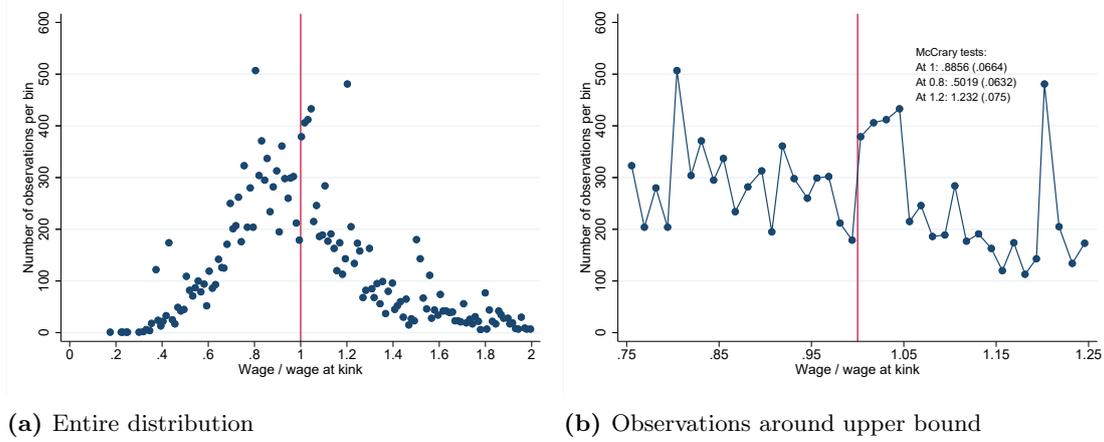
(a) Entire distribution

(b) Observations around upper bound

Notes: The figure plots the probability density function of the running variable (i.e. wage at layoff) around the lower bound. Wages are normalized by the wage at the lower bound and the bin size is equal to 0.0125. Panel A presents the entire distribution of participants, while panel B focuses on observations around the lower bound using the MSE-optimal bandwidth size. In panel B, we also report results of McCrary tests for the continuity of the probability density function at the true kink (i.e. $w/w_k = 1$) as well as at for two additional placebo kinks (i.e. $w/w_k = 0.8; 1.2$).

Figure B2: Evolution of covariates around the lower bound

(a) Gender

(b) Age

(c) Married

(d) Any dependent

(e) High education

(f) Predicted length of UB receipt
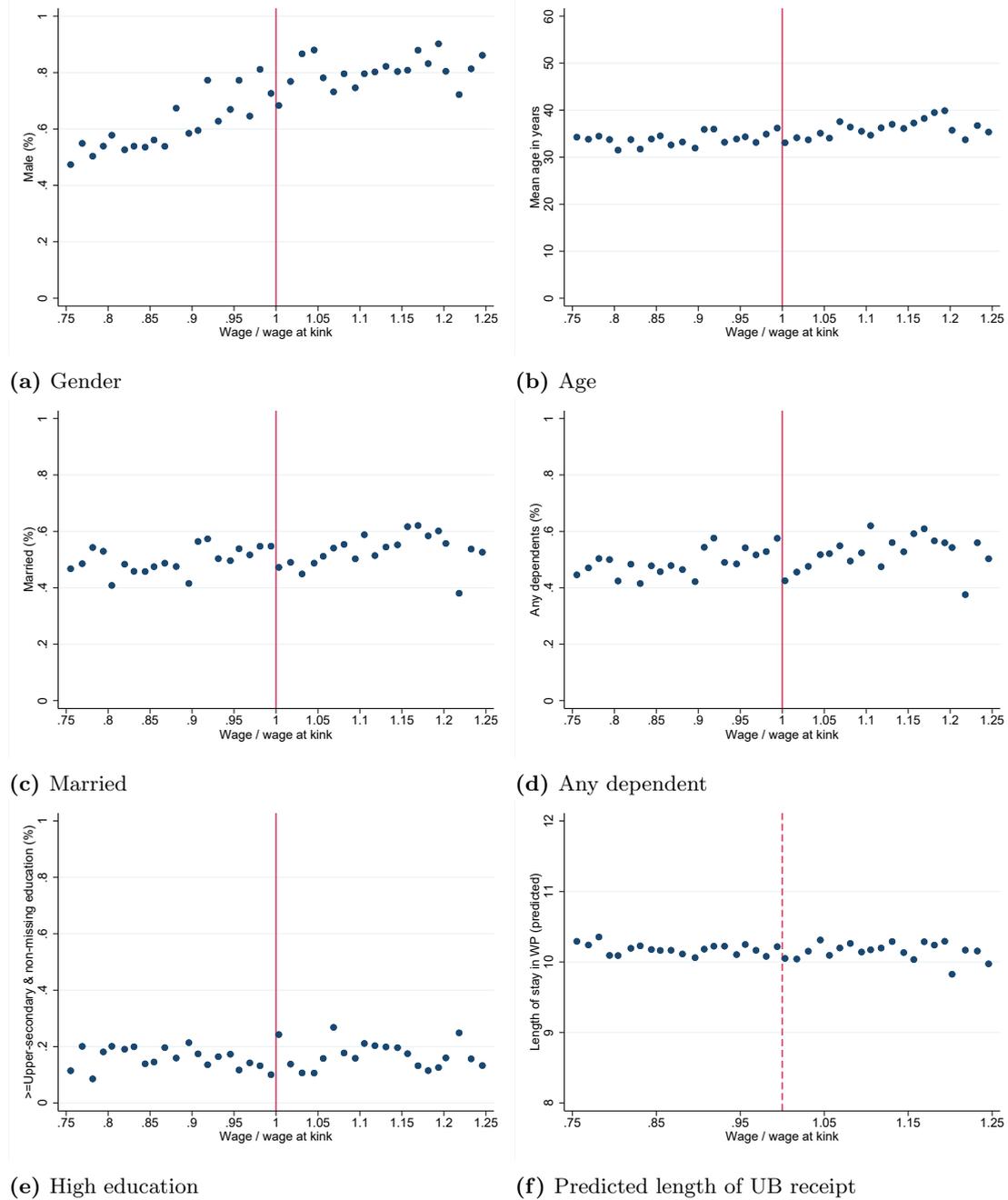
Notes: The figure plots the evolution of covariates around the lower bound of benefit levels, using a bin size of 0.0125 and an MSE-optimal bandwidth of 0.248. All variables are measured at the time of job loss, using administrative data from the Ministry of Labour. The predicted length of UB receipt is obtained with a regression using as explanatory variables: gender, age, age square, marital status, number of dependents, educational attainments, previous occupation and year and district fixed effects.

Figure B3: Scatter bin plot for the duration of UB receipt (in months) at the lower bound
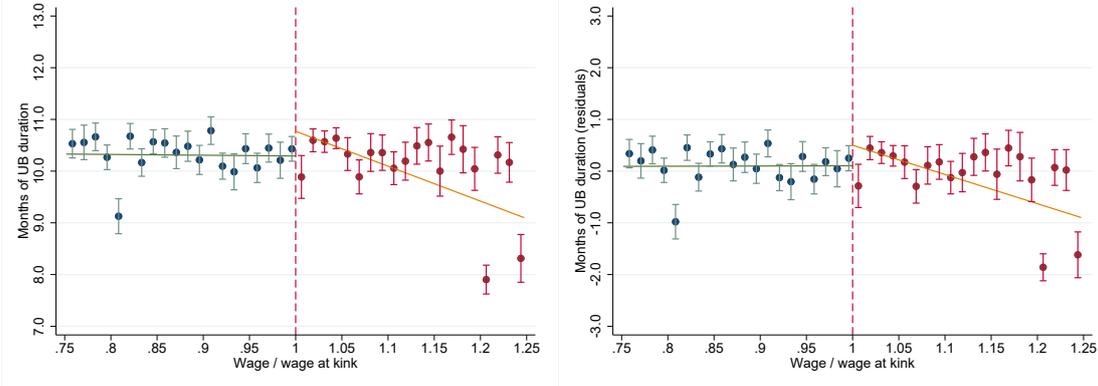
(a) Raw data

(b) Residuals after controlling for region and year

Notes: The figures show mean values and 90 per cent confidence intervals of UB receipt in months per bin of size 0.0125 around the normalized kink point, based on raw data (panel A) and after controlling for region and year fixed effects (panel B). A bandwidth of 0.248 was used. Linear models were fitted on both sides and independently of the choice of bin size; i.e., all programme participants are weighted equally in the linear fits.

Figure B4: Scatter bin plots for formal and informal employment in selected months around job loss (upper bound)

(a) Formal employment

(b) Informal employment

Notes: The figures show mean values and 90 per cent confidence intervals per bins of size 0.0125 around the normalized kink point, for the likelihoods of being formally employed (panel A) and informally employed (panel B) in selected months before and after job loss. The bandwidth of 0.248 was used. Linear models were fitted on both sides and independently of the choice of bin size; i.e., all programme participants are weighted equally in the linear fits. In panel A, the outcome is a dummy variable equal to 1 whenever an individual was formally employed in a given month. In panel B, the outcome is the imputed probability of being informally employed in a given month, with details on the imputation provided in the text. Note that the imputation of informality results in an underestimation of the dispersion of outcomes, which we correct for in the RKD-analysis. The figures are based on raw data, where no control variables have been included. Graphs for other months are available from the authors.

Figure B5: Scatter bin plot for the formal and informal employment in selected months around job loss (lower bound)

(a) Formal employment

(b) Informal employment

Notes: The figures show mean values and 90 per cent confidence intervals per bins of size 0.0125 around the normalized kink point, for the likelihoods of being formally employed (panel A) and informally employed (panel B) in selected months before and after job loss. The bandwidth of 0.248 was used. Linear models were fitted on both sides and independently of the choice of bin size; i.e., all programme participants are weighted equally in the linear fits. In panel A, the outcome is a dummy variable equal to 1 whenever an individual was formally employed in a given month. In panel B, the outcome is the imputed probability of being informally employed in a given month, with details on the imputation provided in the text. Note that the imputation of informality results in an underestimation of the dispersion of outcomes, which we correct for in the RKD-analysis. The figures are based on raw data, where no control variables have been included. Graphs for other months are available from the authors.

Figure B6: RK-results for the probabilities of being formally and informally employed in a given month around job loss, lower bound
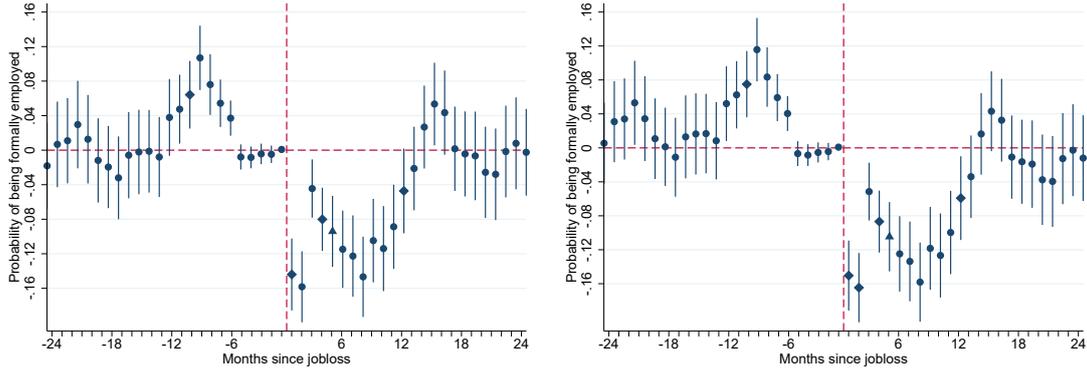
(a) Formal employment, Region+Year FEs

(b) Formal employment, additional controls
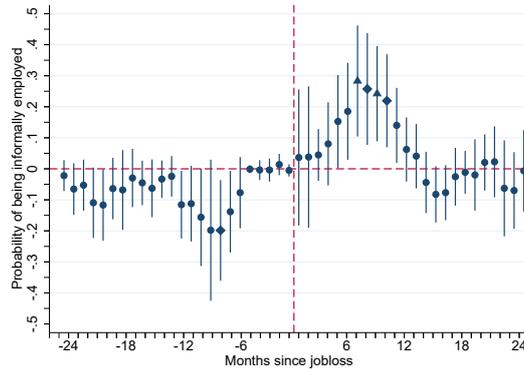
(c) Informal employment, additional controls

Notes: The figures show RK-results for the probabilities of being formally and informally employed in a given month, for the lower bound and a bandwidth of 0.248 around the normalized kink point. Specifically, we display α_k, i.e., the average treatment effect due to a 1,000 Rupee increase in UBs. For each month, a separate regression was run. Confidence intervals refer to the 90 percent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. Panel (a) shows results with sparse controls. In Panels (b) and (c), additional controls include age and its square and dummy variables capturing gender, marital status (4 categories), number of dependents (3 categories) and educational attainment (3 categories). In Panels (a) and (b), the outcomes are dummy variables equal to 1 whenever an individual was formally employed in a given month. In Panel (c), the outcomes are imputed probabilities of being informally employed in a given month, with details on the imputation provided in the text; coefficients and standard errors were adjusted using the imputation correction of Crossley et al. (2020).

Figure B7: RK-results for the probabilities of being formally and informally employed in selected months after job loss, upper bound, by bandwidth
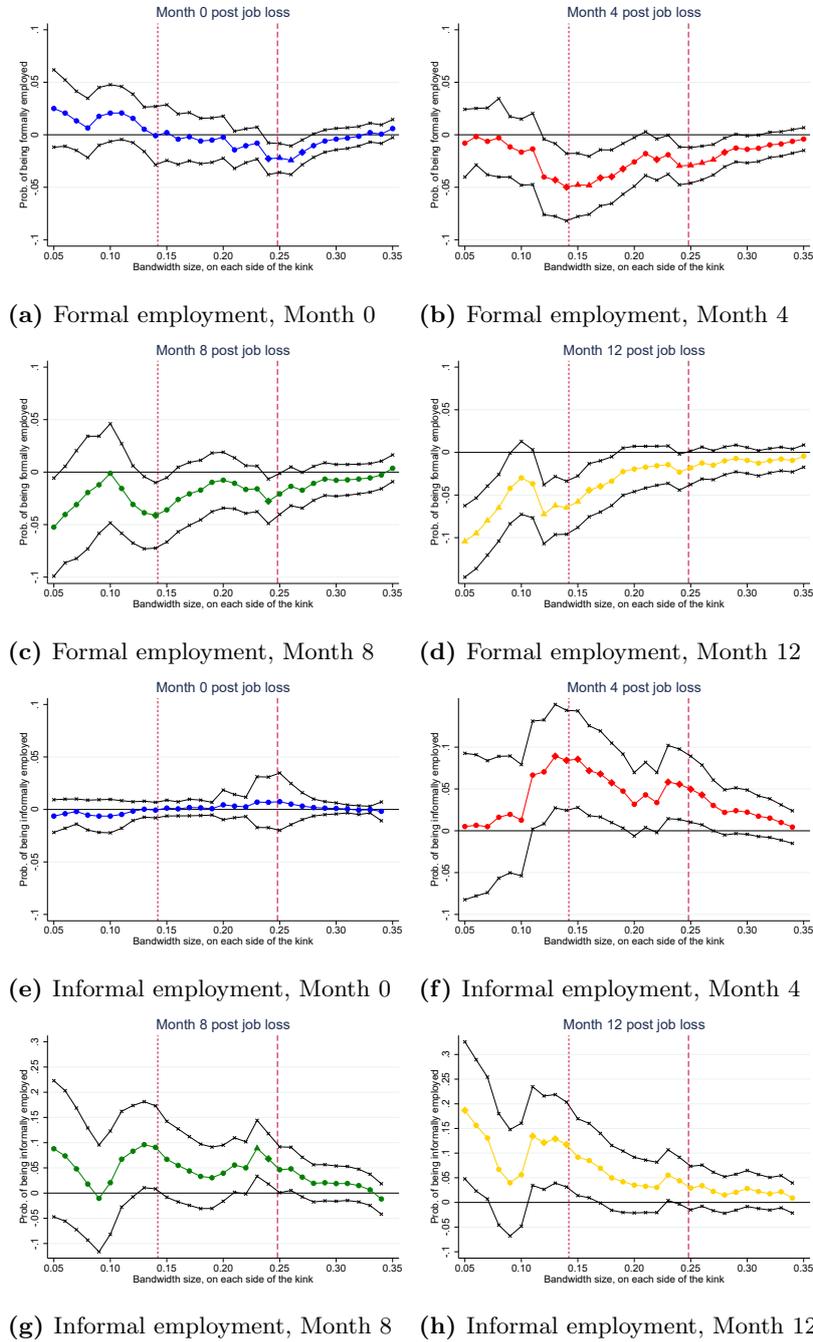
(a) Formal employment, Month 0   **(b)** Formal employment, Month 4

(c) Formal employment, Month 8   **(d)** Formal employment, Month 12

(e) Informal employment, Month 0   **(f)** Informal employment, Month 4

(g) Informal employment, Month 8   **(h)** Informal employment, Month 12

Notes: The figures show RK-results for the probabilities of being formally and informally employed in selected months after job loss, referring to the upper bound. Specifically, we display α_k, i.e., the average treatment effect due to a 1,000 Rupee increase in UBs. For each bandwidth, a separate regression was run. Confidence intervals refer to the 90 percent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. The specification follows Figure 12, Panel (a). For informality, the outcomes are imputed probabilities of being informally employed in a given month, with details on the imputation provided in the text; coefficients and standard errors were adjusted using the imputation correction of Crossley et al. (2020). For comparison, we highlight the bandwidths of 0.248 and 0.142 as in the previous figure.

Figure B8: RK results for the duration of receiving UBs (months), upper bound, by bandwidth
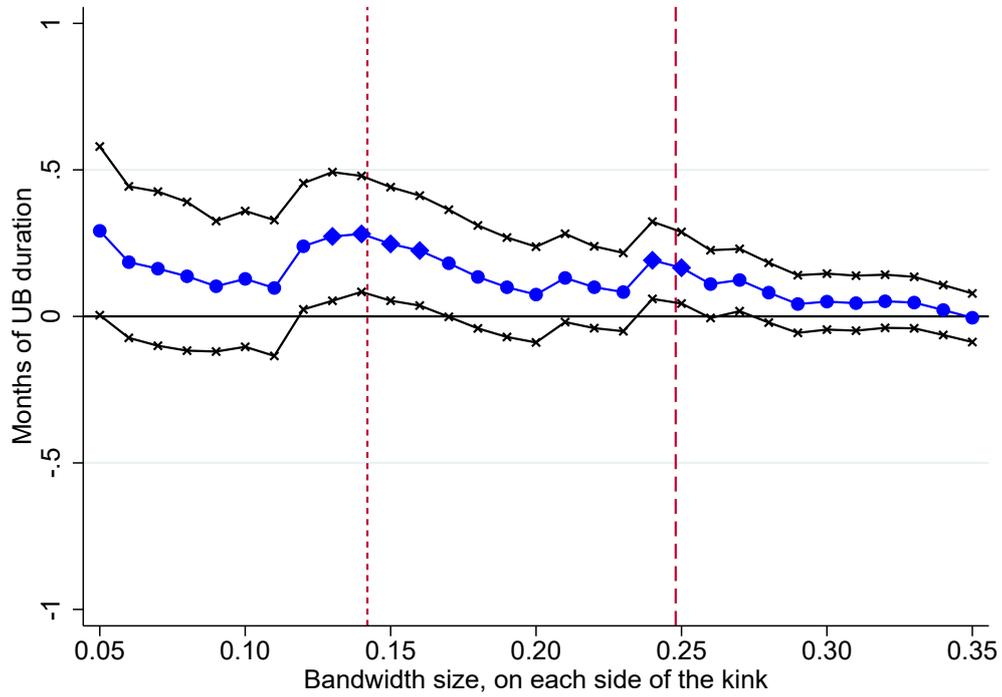
Notes: The figure plots RK-results and 90 per cent confidence intervals for the effect of a 1,000 Rupee increase in UBs on the duration of UB receipt (in months); significance at the 5 percent level is illustrated by markers shaped in diamond form. The specification is the same as in Table 3, column (2). Each estimate stems from a separate regression, obtained by varying the bandwidth size from 0.05 to 0.35 on each side of the cut-off. The long-dashed vertical line corresponds to the MSE-optimal bandwidth (0.248), while the short-dashed vertical line to the CER-optimal bandwidth (0.142).

Figure B9: Detection of actual kink

(a) With no controls

(b) With year and region FEs
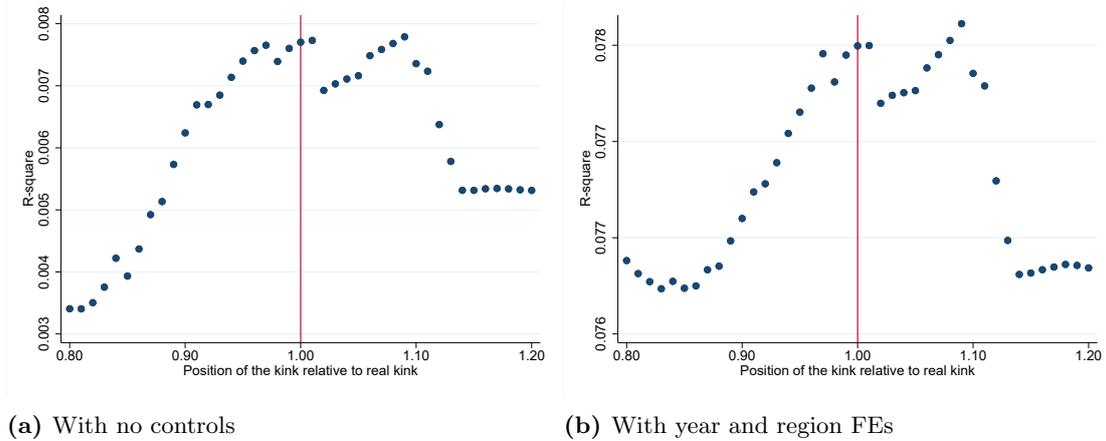
Notes: The figure plots the R^2 of different specifications using length of UB receipt as the outcome variable without controls (panel A) or with year and region fixed effects (panel B). The different R^2 are obtained by varying the location of the kink over the support of the running variable. The continuous vertical line denotes the true location of the kink.

Table B1: Distribution of participants with UB entitlements at the lower and upper bounds, by year of participation and month of unemployment

Year		Lower bound is binding			Upper bound is binding			N
		Mth 1-3	Mth 4-6	Mth 7-12	Mth 1-3	Mth 4-6	Mth 7-12	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
2011	n	2	88	789	162	50	0	1,051
	%	0.002	0.084	0.751	0.154	0.048	0.000	
2012	n	5	379	1,454	238	67	0	1,897
	%	0.003	0.200	0.766	0.125	0.035	0.000	
2013	n	2	73	1,400	312	102	0	2,282
	%	0.001	0.032	0.613	0.137	0.045	0.000	
2014	n	1	52	1,676	430	101	0	2,890
	%	0.000	0.018	0.580	0.149	0.035	0.000	
2015	n	1	21	1,514	481	164	0	3,733
	%	0.000	0.006	0.406	0.129	0.044	0.000	
2016	n	0	7	1,621	442	133	0	3,780
	%	0.000	0.002	0.429	0.117	0.035	0.000	
2017	n	1	4	746	243	67	0	2,158
	%	0.000	0.002	0.346	0.113	0.031	0.000	
Total	n	12	624	9,200	2,308	684	0	17,791
	%	0.001	0.035	0.517	0.130	0.038	0.000	

Notes: The table reports the number and shares of individuals for which the upper and lower bound is binding at different points in time during the twelve months of the program, for each year separately and in the overall period of analysis. The sample is defined as explained in Section 6.1.

Table B2: Summary statistics for the RK analysis: overall sample and individuals in the proximity of the upper and lower bounds, respectively

	(1) All	(2) Proximity Upper Bound	(3) Proximity Lower Bound
Months of UB receipt	10.13	9.91	10.18
Months until reemployment (capped at 1 year)	9.69	9.40	9.72
Months until reemployment (capped at 2 years)	15.58	14.93	15.43
Male (0/1)	0.6395	0.7847	0.6968
Age (years)	35.22	36.00	34.51
Any dependents (0/1)	0.5000	0.5160	0.4986
Married (0/1)	0.5159	0.5376	0.5056
High education (0/1)	0.1866	0.2358	0.1642
N	17,791	4,585	10,484

Notes: The table shows summary statistics for the RKD analysis, where the proximity of the two bounds is defined in terms of the MSE-optimal bandwidth underlying our main specification.

Table B3: Imputation regression for the likelihood of being informally employed in a given month around job loss

	(1) Coeff.	(2) St. error		(3) Coeff.	(4) St. error
Formally employed	-0.039	(0.099)			
FEs for Mths around job loss:			*Interaction terms:*		
Mth -24	0.148*	(0.079)	Mth -24 & form. empl.	-0.111	(0.129)
Mth -23	0.199**	(0.079)	Mth -23 & form. empl.	-0.197	(0.128)
Mth -22	0.129	(0.082)	Mth -22 & form. empl.	-0.110	(0.131)
Mth -21	0.173**	(0.085)	Mth -21 & form. empl.	-0.207	(0.132)
Mth -20	0.242***	(0.086)	Mth -20 & form. empl.	-0.293**	(0.132)
Mth -19	0.262***	(0.083)	Mth -19 & form. empl.	-0.304**	(0.131)
Mth -18	0.388***	(0.088)	Mth -18 & form. empl.	-0.429***	(0.133)
Mth -17	0.222**	(0.091)	Mth -17 & form. empl.	-0.309**	(0.135)
Mth -16	0.108	(0.087)	Mth -16 & form. empl.	-0.173	(0.132)
Mth -15	0.202**	(0.090)	Mth -15 & form. empl.	-0.278**	(0.134)
Mth -14	0.110	(0.088)	Mth -14 & form. empl.	-0.137	(0.132)
Mth -13	0.132	(0.086)	Mth -13 & form. empl.	-0.207	(0.130)
Mth -12	0.178**	(0.086)	Mth -12 & form. empl.	-0.247*	(0.130)
Mth -11	0.185*	(0.104)	Mth -11 & form. empl.	-0.224	(0.142)
Mth -10	0.180*	(0.107)	Mth -10 & form. empl.	-0.234	(0.145)
Mth -9	0.126	(0.118)	Mth -9 & form. empl.	-0.209	(0.153)
Mth -8	0.206*	(0.107)	Mth -8 & form. empl.	-0.298**	(0.145)
Mth -7	0.179	(0.113)	Mth -7 & form. empl.	-0.268*	(0.149)
Mth -6	0.151	(0.161)	Mth -6 & form. empl.	-0.227	(0.188)
Mth -5	-0.161	(0.161)	Mth -5 & form. empl.	0.075	(0.187)
Mth -4	-0.209	(0.272)	Mth -4 & form. empl.	0.120	(0.288)
Mth -3	-0.223	(0.381)	Mth -3 & form. empl.	0.137	(0.393)
Mth -2	0.342	(0.271)	Mth -2 & form. empl.	-0.385	(0.288)
Mth -1	0.980**	(0.382)	Mth -1 & form. empl.	-0.999**	(0.394)
Mth +1	0.041	(0.060)	Mth +1 & form. empl.	0.003	(0.134)
Mth +2	0.069	(0.059)	Mth +2 & form. empl.	-0.100	(0.143)
Mth +3	0.091	(0.058)	Mth +3 & form. empl.	-0.111	(0.143)
Mth +4	0.132**	(0.059)	Mth +4 & form. empl.	-0.211	(0.138)
Mth +5	0.184***	(0.058)	Mth +5 & form. empl.	-0.237*	(0.134)
Mth +6	0.274***	(0.058)	Mth +6 & form. empl.	-0.365***	(0.134)
Mth +7	0.300***	(0.060)	Mth +7 & form. empl.	-0.265**	(0.128)
Mth +8	0.320***	(0.059)	Mth +8 & form. empl.	-0.365***	(0.126)
Mth +9	0.301***	(0.059)	Mth +9 & form. empl.	-0.307**	(0.126)
Mth +10	0.328***	(0.062)	Mth +10 & form. empl.	-0.274**	(0.129)
Mth +11	0.283***	(0.062)	Mth +11 & form. empl.	-0.321**	(0.129)
Mth +12	0.288***	(0.065)	Mth +12 & form. empl.	-0.334***	(0.128)
Mth +13	0.257***	(0.066)	Mth +13 & form. empl.	-0.316**	(0.126)
Mth +14	0.215***	(0.063)	Mth +14 & form. empl.	-0.223*	(0.125)
Mth +15	0.256***	(0.065)	Mth +15 & form. empl.	-0.228*	(0.124)
Mth +16	0.262***	(0.062)	Mth +16 & form. empl.	-0.299**	(0.123)
Mth +17	0.260***	(0.061)	Mth +17 & form. empl.	-0.210*	(0.122)
Mth +18	0.333***	(0.063)	Mth +18 & form. empl.	-0.376***	(0.124)
Mth +19	0.300***	(0.067)	Mth +19 & form. empl.	-0.286**	(0.124)
Mth +20	0.306***	(0.071)	Mth +20 & form. empl.	-0.372***	(0.127)
Mth +21	0.393***	(0.071)	Mth +21 & form. empl.	-0.514***	(0.126)
Mth +22	0.323***	(0.068)	Mth +22 & form. empl.	-0.395***	(0.125)
Mth +23	0.355***	(0.070)	Mth +23 & form. empl.	-0.433***	(0.124)

Control variables:	
w-w_k	YES
Region and Year FEs	YES
Demographic Controls	YES
Observations	5,031
R-squared	0.260
Partial R-squared	0.159

Notes: The table shows the results of the imputation regression for informal employment (equation 6), estimated for the matched sub-sample (i.e., individuals are included whenever survey data is available for them in a given month in a two-year window around job loss). The dependent variable is a dummy variable capturing whether an individual was informally employed in a given month. All coefficients refer to a single regression. Robust standard errors are shown in parenthesis, where ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively. Proxy variables for informality include a dummy variable for being formally employed in a given month, fixed effects capturing the months around job loss (the omitted category is the month of job loss itself) and interaction terms between these two sets of variables. The partial R-squared refers to the variation that is attributable to the proxy variables. It was calculated by regressing the dependent variable on the proxy variables, after having partialled out the effect of the control variables in a previous regression. It is an important parameter for the corrections of standard errors and coefficients in the later RKD-regressions (see Crossley et al., 2020). Demographic control variables capture age and its square, gender, marital status, dependents, and education.

Table B4: Smoothness of covariates at the lower bound

	Age		Gender		Married		Any dependent		High education	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
δ_k (coefficient of the interaction term)	-0.769***	-0.761***	0.044***	0.043***	-0.009	-0.010	-0.005	-0.008	-0.003	-0.004
	(0.193)	(0.192)	(0.010)	(0.010)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Year and Region FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: The table shows RK-results for selected individual level characteristics measured at the time of job loss using the administrative data of the Ministry of Labour. Estimates are obtained at the lower bound with an MSE-optimal bandwidth of 0.248 around the normalized kink point. Each column refers to a separate regression, with and without year and region fixed effects. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B5: RK-results for the duration of receiving UBs (months), lower bound

	(1)	(2)	(3)
RK results:			
β_1 (Coefficient for $w - w_k$)	-0.341***	-0.295***	-0.296***
	(0.038)	(0.035)	(0.035)
δ_k (Coefficient for the interaction term)	0.313***	0.269***	0.302***
	(0.057)	(0.055)	(0.055)
Constant	10.361***	11.087***	10.941***
	(0.045)	(0.147)	(0.411)
Mean UB duration (D)	10.18	10.18	10.18
Treatment effect: $\alpha_k = (\delta_k/-0.3)$	1.043	0.897	1.007
Elasticity: $\eta_B = (\alpha_k * b/D)$	0.290	0.249	0.280
	(0.053)	(0.051)	(0.051)
Observations	10,097	10,097	10,097
Region and Year FEs	NO	YES	YES
Additional controls	NO	NO	YES

Notes: The table shows RK-results for the duration of receiving UBs (in months) at the lower bound and a bandwidth of 0.248 around the normalized kink point. Each column refers to a separate regression. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively. Additional controls include age and its square and dummy variables capturing gender, marital status (4 categories), number of dependents (3 categories) and educational attainment (3 categories). α_k is the average treatment effect due to a 1,000 Rupee increase in UBs. Elasticities are given by α_k multiplied by the minimum benefit $b = 3,000$ in thousands over the mean UB duration D at the kink point.

Table B6: Sensitivity of results to choice of the polynomial order, focusing on the probability of being formally employed in months 0, 4, 8, and 12 after job loss (upper bound)

	No controls			Few controls			All controls		
	1st order	2nd order	3rd order	1st order	2nd order	3rd order	1st order	2nd order	3rd order
	Formal employment month 0								
δ_k (coefficient of the interaction term)	0.032***	0.028***	0.005	0.019**	0.013	-0.003	0.019**	0.013	0.001
	(0.008)	(0.010)	(0.019)	(0.008)	(0.010)	(0.020)	(0.007)	(0.010)	(0.019)
AIC	4069,6	4056,0	4051,3	3752,3	3747,9	3749,8	3722,9	3720,1	3722,0
BIC	4088,8	4088,2	4096,4	3874,5	3882,9	3897,7	3922,2	3932,3	3947,1
	Formal employment month 4								
δ_k (coefficient of the interaction term)	0.034***	0.028**	-0.020	0.027***	0.021*	-0.021	0.027***	0.020	-0.019
	(0.009)	(0.012)	(0.025)	(0.009)	(0.013)	(0.026)	(0.009)	(0.012)	(0.026)
AIC	4542,7	4539,6	4533,4	4375,1	4375,4	4373,4	4353,3	4353,4	4352,1
BIC	4562,0	4571,8	4578,4	4497,2	4510,5	4521,3	4552,6	4565,6	4577,2
	Formal employment month 8								
δ_k (coefficient of the interaction term)	0.029***	0.021	-0.029	0.020*	0.011	-0.030	0.019*	0.010	-0.028
	(0.011)	(0.015)	(0.028)	(0.011)	(0.015)	(0.028)	(0.011)	(0.014)	(0.028)
AIC	6017,4	6006,2	5998,9	5829,8	5824,9	5823,1	5777,6	5772,5	5771,7
BIC	6036,7	6038,3	6043,9	5952,0	5960,0	5971,0	5977,0	5984,8	5996,7
	Formal employment month 12								
δ_k (coefficient of the interaction term)	0.023**	0.024	-0.015	0.017	0.019	-0.013	0.017	0.018	-0.011
	(0.011)	(0.015)	(0.029)	(0.011)	(0.015)	(0.029)	(0.011)	(0.015)	(0.028)
AIC	6386,1	6386,2	6386,1	6317,3	6319,1	6320,9	6222,9	6224,6	6226,9
BIC	6405,4	6418,4	6431,1	6439,5	6454,2	6468,8	6422,3	6436,8	6452,0
Region and year FE	NO	NO	NO	YES	YES	YES	YES	YES	YES
Additional controls	NO	NO	NO	NO	NO	NO	YES	YES	YES

Notes: The table shows RK-results for the effects of UB generosity on the probability of being formally employed in selected months after job loss. The table reports three types of specifications (i.e. with no controls, with only year and region dummies and with additional individual level controls) and three types of models (i.e. linear, quadratic and cubic) as well as the results of the AIC and BIC tests. Robust standard errors are given in parentheses, where ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B7: Sensitivity of results to choice of the polynomial order, focusing on the imputed probability of being informally employed in months 0, 4, 8, and 12 after job loss (upper bound)

	No controls			Few controls			All controls		
	1st order (1)	2nd order (2)	3rd order (3)	1st order (4)	2nd order (5)	3rd order (6)	1st order (7)	2nd order (8)	3rd order (9)
	Informal employment month 0								
δ_k (coefficient of the interaction term)	-0.009	-0.010	-0.002	-0.006	-0.005	0.001	-0.005	-0.003	-0.000
	(0.019)	(0.017)	(0.008)	(0.013)	(0.009)	(0.009)	(0.011)	(0.008)	(0.007)
AIC	-24080,4	-22040,4	-22072,7	-24486,4	-22764,6	-22130,7	-26016,6	-25713,9	-24222,8
BIC	-24061,1	-22008,3	-22027,7	-24364,2	-22629,6	-21982,8	-25817,2	-25501,7	-23997,7
	Informal employment month 4								
δ_k (coefficient of the interaction term)	-0.061**	-0.050*	0.029	-0.053**	-0.039	0.031	-0.042**	-0.030	0.023
	(0.025)	(0.027)	(0.044)	(0.024)	(0.026)	(0.045)	(0.020)	(0.021)	(0.037)
AIC	-6166,2	-6090,5	-6091,3	-6912,8	-6810,4	-6806,4	-8296,8	-8288,4	-8282,7
BIC	-6146,9	-6058,3	-6046,3	-6784,2	-6669,0	-6652,1	-8091,0	-8069,7	-8051,2
	Informal employment month 8								
δ_k (coefficient of the interaction term)	-0.076***	-0.068**	0.032	-0.065**	-0.051	0.044	-0.063**	-0.047	0.040
	(0.027)	(0.033)	(0.057)	(0.028)	(0.035)	(0.065)	(0.027)	(0.035)	(0.064)
AIC	-3076,9	-3178,1	-3176,5	-2879,6	-2966,3	-3014,4	-2485,5	-2541,5	-2647,3
BIC	-3057,6	-3145,9	-3131,5	-2750,9	-2824,9	-2860,1	-2279,8	-2322,8	-2415,8
	Informal employment month 12								
δ_k (coefficient of the interaction term)	-0.058**	-0.062*	0.062	-0.043	-0.045	0.066	-0.040	-0.040	0.059
	(0.027)	(0.035)	(0.064)	(0.026)	(0.035)	(0.067)	(0.024)	(0.033)	(0.062)
AIC	-1927,2	-2002,7	-2006,2	-2668,5	-2725,5	-2730,8	-2815,1	-2848,4	-2869,1
BIC	-1907,9	-1970,5	-1961,2	-2546,3	-2590,5	-2582,9	-2615,8	-2636,2	-2644,0
Region and year FE	NO	NO	NO	YES	YES	YES	YES	YES	YES
Additional controls	NO	NO	NO	NO	NO	NO	YES	YES	YES

Notes: The table shows RK-results for the effects of UB generosity on the probability of being informally employed in selected months after job loss. Details on the imputation are provided in the text and coefficients and standard errors were adjusted using the imputation correction of Crossley et al. (2020). The table reports three types of specifications (i.e. with no controls, with only year and region dummies and with additional individual level controls) and three types of models (i.e. linear, quadratic and cubic) as well as the results of the AIC and BIC tests. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B8: Sensitivity of results to choice of the polynomial order, using the duration of receiving UBs (months) as the outcome variable (upper bound)

	No controls			Few controls			All controls		
	1st order (1)	2nd order (2)	3rd order (3)	1st order (4)	2nd order (5)	3rd order (6)	1st order (7)	2nd order (8)	3rd order (9)
δ_k (coefficient of the interaction term)	-0.275***	-0.214**	0.072	-0.166**	-0.090	0.161	-0.163**	-0.081	0.144
	(0.069)	(0.094)	(0.174)	(0.068)	(0.094)	(0.175)	(0.066)	(0.091)	(0.171)
AIC	87386.25	87384.51	87385.64	86833.34	86831.68	86834.25	86532.37	86530.35	86532.17
BIC	87409.51	87423.28	87439.92	86980.67	86994.52	87012.6	86772.76	86786.25	86803.57
Region and year FE	NO	NO	NO	YES	YES	YES	YES	YES	YES
Additional controls	NO	NO	NO	NO	NO	NO	YES	YES	YES

Notes: The table shows RK-results for the effects of UB generosity on length of UB receipt, focusing on the upper bound. The table reports three types of specifications (i.e. with no controls, with only year and region dummies and with additional individual level controls) and three types of models (i.e. linear, quadratic and cubic) as well as the results of the AIC and BIC tests. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B9: RK-results for length of UB receipt using placebo running variable (upper bound)

	(1)	(2)	(3)
δ_k (coefficient of the interaction term)	-0.010	-0.015	-0.013
	(0.019)	(0.021)	(0.021)
Region and year FE	NO	YES	YES
Additional controls	NO	NO	YES

Notes: The table shows RK results for the effects of UB generosity on length of UB receipt using as placebo running variable the wage upon re-employment. These are observed from the social security data. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B10: Double difference RK-estimates (upper bound)

	2013			2016		
	(1)	(2)	(3)	(1)	(2)	(3)
δ_k (coefficient of the interaction term)	-2.060**	-1.394*	-1.413*	-0.882**	-0.993**	-0.939**
	(0.855)	(0.818)	(0.806)	(0.405)	(0.407)	(0.410)
Region and year FE	NO	YES	YES	NO	YES	YES
Additional controls	NO	NO	YES	NO	NO	YES

Notes: The table shows RK-results for the effects of UB generosity on length of UB receipt using a double difference RK design. Robust standard errors are given in parentheses, where *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.