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Welfare effects of unemployment benefits when informality is high[☆]

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ABSTRACT

We investigate the welfare effects of unemployment benefits (UBs) in the context of high informality, analyzing matched administrative and survey data with detailed information on consumption, transfers and informal and formal employment of UB recipients. Difference-in-differences analysis reveals a comparatively large consumption drop after the loss of a formal job, resulting from shifts towards lower-quality informal employment and an associated earnings penalty. Exploiting a UB kink, we then show that higher UBs delay program exit through a substitution of formal with informal employment. Even when conservatively estimated, however, welfare effects are positive for coefficients of risk aversion exceeding two.

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 trade-off 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.[1] At the same time, the consumption drop at layoff might be higher in these contexts, especially if finding a new formal job is difficult, earnings from informal employment are low and other forms of social protection are insufficient. While the welfare effects of UBs might therefore be either higher or lower in low- and middle-income countries compared to high-income countries, the policy debate has often emphasized the possible inefficiencies associated with widespread informality (Duval and Loungani, 2021; Robalino et al., 2009; Vodopivec, 2013). This is one of the reasons why UB schemes are less developed in low and middle-income countries than other forms of social protection (see ILO, 2021).

In this article, we jointly analyze the insurance value and the efficiency costs of UBs in Mauritius, where informal employment is widespread.[2] Our study is the first to match, at the worker level, administrative data from UB program records and social security biographies with survey data. The administrative data relate to UB receipt and formal employment outcomes, while the survey data also capture expenditures, episodes of informal employment and non-employment, and a range of related labor market outcomes. This allows us to shed new light on how the possibility of working informally affects the insurance value and efficiency costs of UBs. To understand why this is relevant, 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

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[1] In low- and middle-income countries, an estimated 69.6 percent of employment is informal. This share is smaller in high-income countries, but still amounts to 18.3 percent on average, with significant variation across and within countries (ILO, 2018).

[2] We build on the few previous contributions on UBs outside of high-income countries. These studies, which we review in more detail below, have focused on the insurance value (Gerard and Naritomi, 2021) and the efficiency costs of UBs (Britto, 2022; Gerard and Gonzaga, 2021) in the context of Brazil.

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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 (on the general role and nature of informal jobs see also Meghir et al., 2015; Ulyssea, 2018).

Based on difference-in-differences (DiD) and regression kink (RK) analysis, our key result is that the welfare effects of UBs are positive for reasonable levels of risk aversion, 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. The consumption drop counterbalances the efficiency costs, although the latter, at least when conservatively estimated, tend to exceed the median of efficiency costs identified in studies for high-income countries. Investigating underlying mechanisms, we indeed document a large shift towards informal employment among UB recipients. However, individuals who move to informality experience a much larger drop in earnings compared to those who are formally re-employed. This means that informal jobs are poor substitutes of formal ones and workers seem to accept them only in the absence of better alternatives.

To provide more details on our study context and data, Mauritius is a country in the Indian Ocean with a population of 1.3 million. It has a level of economic development comparable to Argentina, Brazil or Uruguay. 56.2 percent of Mauritian employment is informal. An important advantage of our setting is that we can exploit a unique database. Its starting point are the program records of the universe of Mauritian 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 percent of those formally employed in the country. We matched the databases for both the UB recipients and the sample of the formal labor force with the Mauritian household survey CMPHS. This means that for a representative sub-sample of the two populations, namely the sub-sample interviewed in the CMPHS, we can additionally exploit rich survey information.

We begin by adopting a DiD approach on the matched observations to analyze the implications of losing a formal job. To our knowledge, we study for the first time detailed trajectories of UB recipients after job loss in a context of high informality, especially with respect to the transitions between formal and informal employment and what these transitions entail for labor market and consumption outcomes. 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 also conduct several robustness tests in which we vary the control group and present event-study estimates with no control group.

We find that overall employment returns to pre-layoff levels two years after job loss, 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 percent more likely to hold an informal job, which is equally explained by a shift to informal dependent employment and (informal) self-employment. Additionally, individual monthly earnings are still around 50.0 percent 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. We also show that other types of public or private transfers fail to materialize when eligibility to UBs terminates, pointing to a substantial drop in disposable income after job loss. Accordingly, 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 percent lower than before dismissal and consumption expenditures are still 32.8 percent lower. Overall, the drop in consumption is substantially larger than the effects observed in developed economies (Schmieder and von Wachter, 2016), indicating that the insurance value of UBs is comparatively large in a context with high informality rates.

Subsequently, we estimate the efficiency costs of UBs. We rely on the fact that UBs in Mauritius are subject to an upper bound and conduct a RK analysis around the maximum benefit level to which individuals are eligible during the first three months of the unemployment spell, when UBs replace 90 percent of the previous wage (as in Card et al., 2015, 2017; Landais, 2015). 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. We observe these outcomes for the entire sample of UB recipients from social security records. Using a mean squared error (MSE)-based bandwidth, we find an elasticity of length of UB receipt with respect to benefit levels equal to 0.47 (s.e. = 0.14). 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 equal to 0.76 (s.e. = 0.27). These estimates fall within the range of those elasticities found for developed economies, but tend to exceed their median (Schmieder and von Wachter, 2016). At the same time, compared to other bandwidths, the MSE-based bandwidth yields relatively large efficiency costs and we interpret these as conservative estimates of the biggest possible effects that we cannot rule out. Within the RK analysis, we then study the role that informal jobs play in determining the estimated efficiency costs. In our survey data, the size of the sample, for which we observe informal employment in the proximity of the kink, becomes small. We thus impute 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., 2022). Applying the imputation to our RK analysis, we find suggestive evidence that more generous UBs may lead to a certain increase in informal employment and that this effect becomes smaller in the second year after job loss.

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.[3] To obtain the insurance value, we slightly adapt our DiD estimates. We follow Landais and Spinnewijn (2021) and Kolsrud et al. (2018) and estimate the flow drop in consumption around the unemployment event (i.e. at the monthly level) for individuals in the proximity of the upper bound of benefit levels. We exploit 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 percent. Comparing the insurance value with our conservative estimates of the efficiency costs, we find that welfare effects from increasing benefit levels are positive for any value of the coefficient of relative risk aversion above 2. While it is important to highlight the dependency of our conclusions on the imposed level of risk aversion, a recent study by Landais and Spinnewijn (2021) provides evidence for coefficients of risk aversion falling within the range of around 4 to 8. In our setting, this would imply clearly positive welfare effects. Overall, our findings thus indicate that in a labor market with high informality,

[3] It is important to note that our efficiency cost estimates are derived from a change in benefit levels taking place in the first three months of unemployment, while the welfare formula that we apply relates to an increase in benefit levels for the overall duration of UB receipt. In Section 2 we will specify the conditions needed for this theoretical framework to apply to our specific context, and in Section 6 we will provide some supportive evidence in favor of this general applicability.

having a formal job represents a key advantage and providing insurance against its loss is a sensible policy choice.

This paper builds on the few previous contributions on UB schemes in emerging economies. Two papers examine the efficiency costs of UBs. Gerard and Gonzaga (2021) analyze the effects of extending UB duration in Brazil and find that longer entitlements increase the length of UB receipt. Nevertheless, the resulting efficiency costs are low. Because formal jobs are difficult to find, the efficiency costs of extending UB duration are largely due to a mechanical effect of the policy. Complementary analyses of these authors also suggests that significant shares of UB recipients work informally, even when drawing UBs.[4] Britto (2022) compares the labor supply responses to severance payments with those of an extension of UBs in Brazil and shows that the latter is more detrimental to formal labor supply, though only in the short run. Another study focuses on the insurance value: Gerard and Naritomi (2021) find that UB recipients in São Paulo increase spending when they receive lump-sum severance payments despite facing a sizable long-run drop in spending, such that informal employment provides very imperfect means of self-insurance. Our study broadly confirms the conclusions of these papers, where it is informative that the existing results hold outside of Brazil.[5]

We also fill a gap in this literature by linking administrative and survey data at the worker level. To begin with, the matched data allow us to study the efficiency costs and insurance value – and subsequently overall welfare effects of UBs – in a unified setting.[6] In addition, we can study formal versus informal re-employment trajectories of UB recipients, without having to impose strong assumptions on the data. Finally, our matched data enable us to analyze the implications of these re-employment patterns for workers' earnings, work hours, type of jobs (i.e., by sector and dependent versus self-employment) and consumption. This analysis reveals why informal jobs are imperfect substitutes of formal ones. Combined with our finding that alternative means of insurance (from relatives and other forms of social security) are insufficient, we thus shed new light on the labor market mechanisms underlying the welfare results.

Having access to rich worker-level data on informality allows us to also contribute to another strand of literature, which focuses on the nature of the informal labor market. According to a first hypothesis, workers voluntarily choose informal employment because this is beneficial for them. Alternatively, workers may prefer formal jobs but have difficulty accessing these. These two views on informality are no longer seen as dichotomous (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 earnings, 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 two branches of the literature in public and development economics. First, although smaller than the literature on the efficiency costs of UBs, different studies have also documented drops in consumption (Ganong and Noel, 2019; Gruber, 1997; Kolsrud et al., 2018; Landais and Spinnewijn, 2021) 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 are significantly larger than those previously obtained, which can be explained by limited self-insurance and gaps in social safety nets. Second, 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 directly measuring non-compliance in the context of a UB scheme outside developed countries and finding that the welfare gains tend to nevertheless be meaningful.

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 employment and type of employment, earnings and transfers, and consumption expenditures; Section 6 presents the results from the RK analysis on the effect of benefit generosity on length of UB receipt, time until formal re-employment, and substitution effects towards 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

Throughout the paper, we define the optimal UB level in terms of the trade-off 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) and Gerard and Naritomi (2021).

In the model, both UB recipients who are informally re-employed and those who remain unemployed (i.e., they work neither formally nor informally) are 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 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. For the efficiency costs of UBs, this is because the fiscal externality is the same whether formally displaced workers remain non-employed or are informally re-employed. For the insurance value of UBs, the average drop in consumption after layoff is relevant, where again it does not directly matter whether workers are non-employed or informally re-employed (even though this can have indirect effects if earnings from informal jobs help individuals cushion the consumption drop).

[4] Gerard and Gonzaga (2021) compare survival rates out of formal employment from administrative data with survival rates out of total employment (formal and informal) from survey data. As the authors acknowledge, this comparison across data sources increases the uncertainty around the estimates. It can only be performed for workers eligible for UB (which is a larger group than actual recipients) in urban settings, and the survey data lack recall information for formally re-employed workers.

[5] For example, the majority of UB recipients in Brazil receive significant severance payments upon layoff, while severance payments play a comparatively small role in Mauritius. A priori, it is unclear whether this yields different behavioral responses, such as divergent transitions to informal employment.

[6] 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. Their paper analyzes the trade-off between UB level and duration, rather than the welfare effects of UBs.

[7] Both assumptions seem justified in our study context. While informal workers are in theory eligible for UBs in Mauritius, only three percent of those losing an informal job actually enter the UB scheme (Liepmann and Pignatti, 2019).

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 $\psi(s_{i,t})$, which are increasing and strictly convex. 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 (increasing and strictly convex) job effort costs $\phi(l_{i,t})$ of working informally; where the case of $l_{i,t} = 0$ represents a non-employed UB recipient. The worker has an increasing and strictly concave utility function $u(.)$ and chooses an optimal inter-temporal consumption path, with consumption given by $c^u_{i,t}$. $c^u_{i,t}$ cannot exceed the sum of UBs and income from informal employment ($b_t + l_{i,t}w_i$) in addition to personal assets and exogenous income from other sources. Once the worker finds a formal job, she accepts it, up until T earns a fixed wage w_f, no longer receives b_t, and pays taxes τ that contribute to financing the UB scheme. She has an increasing and strictly concave utility function $v(.)$ and decides about an optimal consumption path ($c^{fe}_{i,t}$). $c^{fe}_{i,t}$ amounts to $w_f - \tau$ plus personal assets and exogenous income from other sources.

The social planner determines b_t and τ to maximize welfare W, which is given by workers' expected lifetime utility.[8] 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):

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

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. The first term on the right-hand side represents the insurance value as measured as the gap in average marginal utilities associated with a one-unit change in consumption between all UB recipients who would mechanically receive more generous transfers ($u'(c_u)$), and formally employed workers who would be paying the increased UB taxes ($v'(c_{fe})$). Empirically, we approximate the insurance value based on the flow drop in consumption after layoff, following Chetty (2008).[9] 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 Eq. (1) in Sections 5 and 6 and bring them together to compute overall welfare effects in Section 7.[10]

In this relatively simple model, changes in b affect informal work, but any change in informal work only has second-order effects on welfare due to the envelope condition (cf. Chetty, 2006). Since the worker is assumed to have already optimized her choices, increases in b merely translate into higher c_u and, because of this, into less search effort for a formal job. However, the prevalence of informal employment indirectly affects both the insurance value and the efficiency costs of UBs, because income from informal employment lw directly increases c_u and, therefore, also affects formal re-employment. In this context, given estimates of the insurance value and the efficiency costs are consistent with opposing views on informality (Gerard and Gonzaga, 2021).

According to a first stylized view, informal employment is readily available and the result of a voluntary choice associated with decent wages. This would imply that UB recipients prefer to work informally and that they earn relatively high wages (lw is high) in addition to receiving benefits. Even in the absence of UBs, these workers would return slowly to formal employment, which would decrease the efficiency costs of UBs.[11] However, informal employment would be an adequate insurance mechanism against the loss of a formal job, which would decrease the insurance value of UBs.

In contrast, it is also possible that informal jobs are difficult to enter and/or are associated with lower wages and inferior working conditions compared to formal jobs. UB recipients would have difficulty finding informal employment and/or choose it only in the absence of better-paid formal alternatives. These workers would earn relatively low wages (lw is low) in addition to receiving benefits. Similarly to the first stylized view, UB recipients could return slowly to formal employment even in the absence of UBs, which would again mechanically decrease the efficiency costs of UBs. However, in this context, UB recipients would experience larger consumption drops and this would increase the insurance value of UBs.

Therefore, in addition to estimating the parameters of Eq. (1), we empirically investigate the role of informal employment. This is important for understanding the specific mechanisms that determine the welfare effects of UBs in labor markets with high informality. Shedding light on how and why the insurance value and efficiency costs of UBs differ across environments with different degrees on informality, may also provide guidance to other settings with high informality – where available data are more limited – to inform debates about policy design.

3. The institutional context of the Mauritian UB scheme

Mauritius is a middle-income 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

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

[9] 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).

[10] In practice, our efficiency cost estimates will be obtained by looking at the effects of a benefit change that applies in the first three months of UB receipt, while the welfare formula introduced above is for the case of a change in benefit levels during the entire program duration. We thus apply this formula to our specific context, assuming that the elasticity estimates that we obtain are similar to those that would be obtained for a change in benefit levels that applies over the entire twelve months period. We will provide suggestive evidence in favor of this hypothesis in Section 6, by estimating the efficiency costs exploiting a different kink in benefits, which is binding in the last six months of benefit receipt.

[11] This means that the mechanical cost of raising UBs would be high (as most individuals would draw the additional UBs), but the behavioral response small. Given that the efficiency costs of UBs correspond to the ratio between behavioral and mechanical effects, the efficiency costs of increases in UBs would be small (Gerard and Gonzaga, 2021).

since the 1990s and is currently comparable to levels in Latin American countries such as Argentina, Brazil, and Uruguay. The Mauritian service sector accounts for the majority of employment (67.4 percent), followed by industry and manufacturing (25 percent) and agriculture (7.6 percent; Statistics Mauritius, 2017). Despite rapid economic growth, evidence from the household survey shows that informal employment is still prevalent. An estimated 43.8 percent 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).[12] Annual GDP growth and unemployment rates have also been stable during the years we focus on.[13]

The current system of UBs is in place since 2009 (see also Asenjo et al., 2019). Unemployed individuals are eligible to participate if they have been employed in a full-time, private-sector job for at least six months without interruption. As is a common feature of many UB schemes (Asenjo and Pignatti, 2019), part-time workers, public sector employees, and self-employed individuals are excluded. Among eligible individuals, 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 asked 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, if this proves impossible, fully finances the participation of the individual in the intervention. In practice, however, very few informal workers apply to UBs upon layoff (i.e. they represent 20 percent of total participants, despite accounting for roughly 70 percent 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) appear to discourage their participation (Liepmann and Pignatti, 2019).

In addition to receiving UBs, dismissed individuals in Mauritius are eligible to severance payments, as in most developed and emerging economies (Asenjo and Pignatti, 2019). However, severance payments are accessible only under certain conditions, which are more restrictive than those for UBs. Specifically, receipt of severance payments occurs only if the employment spell has lasted at least 12 months and if the dismissal was unjustified.[14] Dismissed individuals in Mauritius can join the UB scheme and in parallel file a lawsuit for unjustified dismissal. While these individuals will immediately receive UBs, severance payments are paid, if at all, with a time lag. The number of UB recipients obtaining severance payments is likely to be very small. In our initial sample (i.e. before imposing any sample restriction relevant to either the RKD or DiD analyses, see below for details), only 2 percent of individuals report a tenure of at least 12 months *and* that their dismissal was unjustified.[15]

Mauritian UBs are co-financed by workers' and employers' contributions, which is common in many countries. 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 percent of w_i ($r_m = 0.9$) and this is the main kink that we exploit to estimate the efficiency costs. This replacement ratio is reduced to 60 percent 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 percent 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 (see also Asenjo et al., 2019). The vast majority opts for the job-placement option (85 percent), 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 observes that contributions are 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. 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.

[12] 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.

[13] Mauritius shares common features with Brazil, which has been the focus of the other studies examining UB schemes outside of advanced economies. In addition to comparable per-capita GDP levels, the sectoral employment distribution is similar between the two countries. However, informality is more prevalent in Mauritius: in 2018, roughly two out of five workers in Brazil were informally employed (ILO, 2022).

[14] While most developed and emerging economies attach similar conditions to the receipt of severance payments, these are slightly more restrictive in Mauritius (Asenjo and Pignatti, 2019). Note also that the Mauritian UB scheme is different from individual savings accounts, which exist in some emerging economies as an alternative or complement to UBs and entail, under certain conditions, severance pay upon job loss.

[15] The majority of UB recipients report that their dismissal was due to economic difficulties of the firm (49 percent). Other frequent reasons for job loss include disciplinary dismissals (21 percent), unsatisfactory workers' performances (12 percent) and the end of the temporary contract (12 percent).

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) and 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, such as educational levels, marital status, and 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. 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 combining the full employment records of those who were formally employed in the month of July in any year between 2011 and 2018.[16] This second sample includes around 90 percent of those formally employed in Mauritius during the period of analysis.[17]

In the rest of the analysis, we will 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 restrict the analysis to those who enter the program by the end of 2017, 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 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. This also covers informal employment. We have CMPHS data from 2012, when the survey questionnaire started asking individual ID numbers which we use to conduct the merging, through 2018. While reporting the ID number is not compulsory, with 85.1 percent the vast majority of survey respondents provide this information.

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.[18] For this sample, we restrict the length of 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 percent 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 UB receipt) and characteristics of the previous job (e.g. rate of formal employment, tenure, wages). A few differences emerge with respect to individual-level characteristics, which are most notable for age and gender but small in magnitude. Overall, the matched sample compares well with the universe of UB recipients.

Looking at matched and unmatched UB recipients combined, we observe that 60 percent are men. 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. Around half of the participants is married and has dependents. Individuals are generally low educated, with less than 20 percent 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 more than one third of the participants was previously employed in elementary occupations. Finally, the average length of UB receipt is equal to 10.3 months and around 70 percent of participants stay in the program for its entire duration.

For some variables available from the social security records, we can investigate the comparability of the matched and unmatched samples also over time. Fig. 1 thus plots the shares of individuals in our UB sample with a formal job before and after job loss, where the dashed green and blue lines refer to the matched and unmatched samples, respectively. Thanks to the completeness of the social security biographies, both lines are precisely observed at the monthly level. We note that the formal employment rate increases before job-loss (remember that UB eligibility requires at least six months of tenure). After job

[16] July was chosen to maximize sample coverage, as it is considered the peak of labor market participation in the country. We started by including those formally employed in July 2018 and then successively added individuals not yet included but formally employed in July of the preceding years. We extract employment histories from the date of the first contribution until December 2018.

[17] 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 percent 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 percent.

[18] For the UB sample we have richer individual and household information from program registration, which we 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: in 2018, the share of men was equal to 58.1 percent in our administrative sample and to 58.9 among the formally employed interviewed in the CMPHS. The corresponding average age was 38.9 and 39.1, respectively.

Table 1
Summary statistics for UB recipients, overall sample and matched versus unmatched sub-samples.

	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 (0/1)	0.783	0.783	0.777	0.006
Wage at job loss (2018 Rupees)	11 994.166	11 994.051	11 995.789	−1.739
Tenure at job loss (in months)	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	26,721	24,942	1,779	

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.

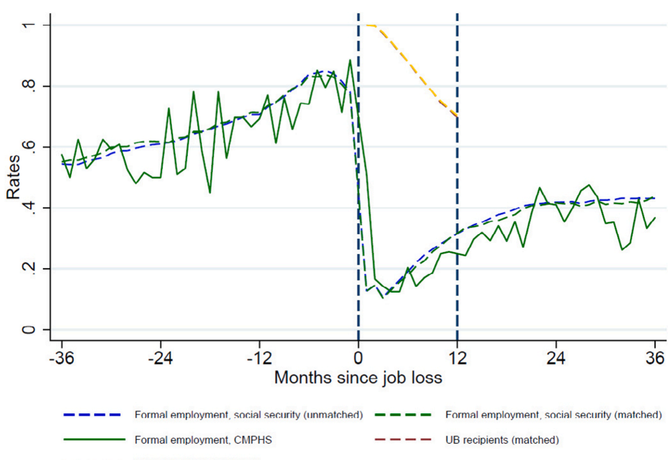


Fig. 1. Evolution of the monthly shares of formally employed workers and those receiving UBs for the matched and unmatched samples of UB recipients.
Notes: The figure reports the share of formally employed individuals in the three years before and after job loss among the samples of UB recipients who have and who have not been matched with the survey data (dashed green and blue line, respectively). Similarly, the figure reports the share of individuals who entered the UB scheme and drew UBs in each month during the year following job loss, for the matched sample (dashed maroon line) and the unmatched sample (dashed yellow line). All of these dashed lines are obtained from the fully available social security records and thus are precisely estimated. Finally, the continuous green line reports the share of formal employment for the matched sample according to the CMPHS survey. This line was obtained by combining cross-sectional observations, depending on the time at which the individuals were interviewed.

loss, formal employment declines sharply, although it does not go down to zero, and then it starts to increase again, albeit only slowly. Importantly, we do not find any large discontinuity in the series when UB generosity changes (at months 3 and 6 after job loss) or when UB eligibility expires (after 12 months). This can be also shown by looking at the share of UB recipients finding a formal job and at the hazard rates of formal employment (Online Appendix Figure B1; our appendices can be accessed as part of the link to "supplementary materials" included at the end of the article). Regarding the comparability across samples, Fig. 1 confirms that the matched and unmatched samples have very similar rates of formal employment over time. The same is true when looking at the share of individuals drawing UBs in the twelve months after job loss in the matched and unmatched samples (see the maroon and yellow dashed lines in Fig. 1).

Another possible concern relates to the comparability of the information provided between the administrative and survey data. In the context of the present analysis, this is particularly relevant for the formality status of employed workers. The definition of formality that we construct from the household survey is the same as the one that we observe in the administrative data. As mentioned above, this refers to the presence of job-related social security contributions to the National Pension Fund, which are a legal requirement. Measurement might nevertheless differ between the two sources, if survey respondents ignore their formality status or decide to misreport it, for example out of the fear that survey responses are used for auditing. To partially check whether individuals misreport their formality status, the continuous green line in Fig. 1 plots the share of individuals who are formally employed according to the household survey. This information is available only for the matched sample, and we use observations as repeated cross-sections given that for each individual, we have a maximum of four observations over time when they are interviewed. The related sample size is small at the monthly level 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 rely on information that is only available in the household survey (including on informal employment) for our analysis.[19]

[19] One might still suspect that informally employed individuals state that they do not work, even if information on formal employment is adequately

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. We begin by considering a series of labor market outcomes to shed new light on the employment implications of formal job loss, then look at other means of insurance, and finally investigate consumption expenditures.

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.[20] As is common in the literature, we define a control group of individuals who never experience job loss (Gerard and Naritomi, 2021; Kolsrud et al., 2020; Landais and Spinnewijn, 2021). This group is taken from the sample of the Mauritian formal labor force for which we have full social security records, where we restrict the analysis to those who have the same formal job for 72 consecutive months. We conduct the analysis only for observations in the treatment and control groups that are matched with 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 survey 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 equal to 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 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, 2021). 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.

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 Eq. (3)). All graphs in this section will follow the same structure, plotting 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.

We present results for our full sample of UB recipients, as well as for different sub-samples based on re-employment status. In particular, whenever possible, we separately focus on individuals who are still outside of formal employment (the so-called survival sample). This sub-sample is informative when considering UBs, as it contains individuals who are still eligible to UBs until benefit exhaustion.[21] Additionally, for certain outcomes of interest, we differentiate the treated sample by detailed employment status (e.g. formal employment, informal employment or non-employment), relying on the richness of our survey data. This is done to understand whether specific re-employment conditions are associated with differences in outcomes of interest (e.g. earnings or consumption). Given that everybody is formally employed before layoff, there is no differentiation of the treated sample for the quarters before job loss, which is why pre-treatment trends coincide and will be denoted by a unique line.

A final note concerns the rescaling procedure. We perform regressions in levels, but for the continuous outcomes 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, which corresponds to the baseline event time. When analyzing different sub-groups, this requires rescaling point estimates using the mean of the respective sub-group (Gerard and Naritomi, 2021).[22] We can conduct this rescaling when analyzing earnings, for which we have information provided at the time of registration in the program. However, we cannot perform the correct re-scaling by sub-groups for consumption and transfers in the absence of adequate panel data from the CMPHS. Therefore, for these outcomes we will present results on consumption and transfers for the overall sample in the main text and refer to the Online Appendix for additional sub-group analysis.

A. Employment and type of employment

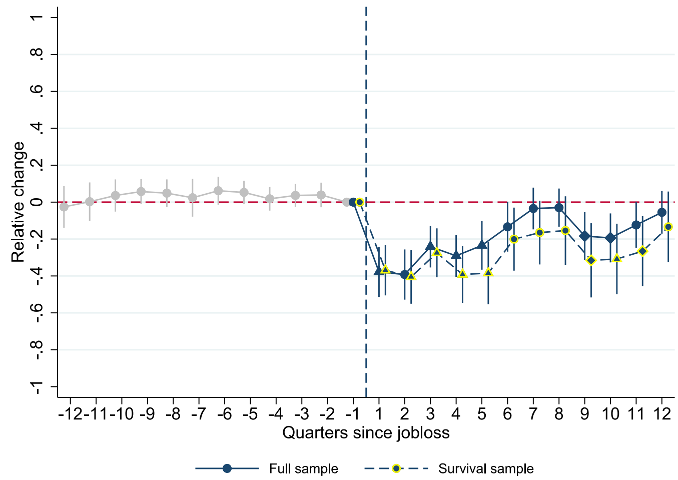
We now investigate the labor market trajectories of laid-off individuals, overall and by formality status, type of employment (i.e. dependent employment or self-employment) as well as by sector of economic

provided. While we cannot totally rule this out, in the six months before UB registration, when everybody should have been either formally or informally employed to be eligible to later receive UBs, monthly informal and formal employment rates almost perfectly sum up to one. Additionally, the survey structure makes it unlikely that an informal employee strategically misreports her labor market status as questions on social security contributions are only asked when respondents have already stated that they are employed. Finally, in the results section, we will show that there is no discontinuity in the self-reported probability of being informally employed when UB eligibility ends and individuals no longer have incentives to hide an informal job.

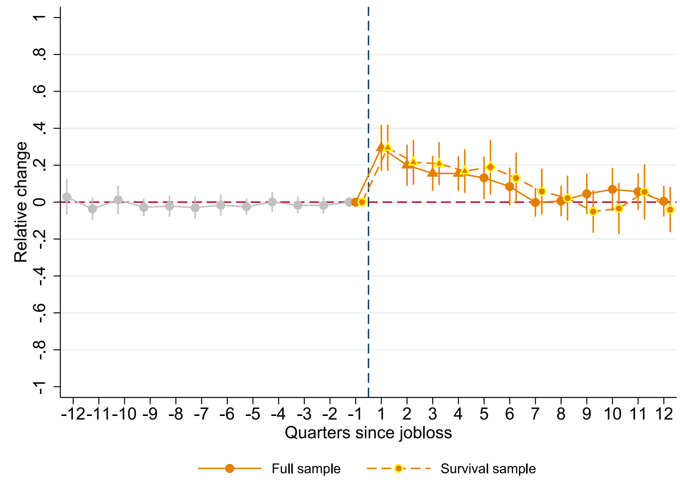
[20] The restriction on formality is imposed to identify a comparable control group of individuals who never lose their jobs for the entire time period (i.e. we do not observe informal workers consecutively for 72 months from the CMPHS, as the maximum length of the panel is 15 months and there might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

[21] Of course, this differentiation between the overall and survival samples will not be presented in cases when the definition of the outcome of interest would make it meaningless (e.g. for formal employment).

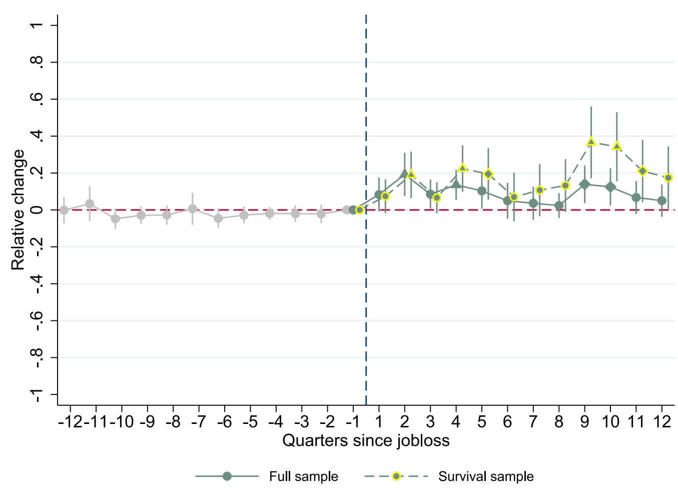
[22] For instance, when analyzing effects on earnings for the survival sample, we rescale coefficients differently in each quarter after job loss, based on the average earnings at job loss in the sub-group of individuals who are still outside of formal employment in that quarter.



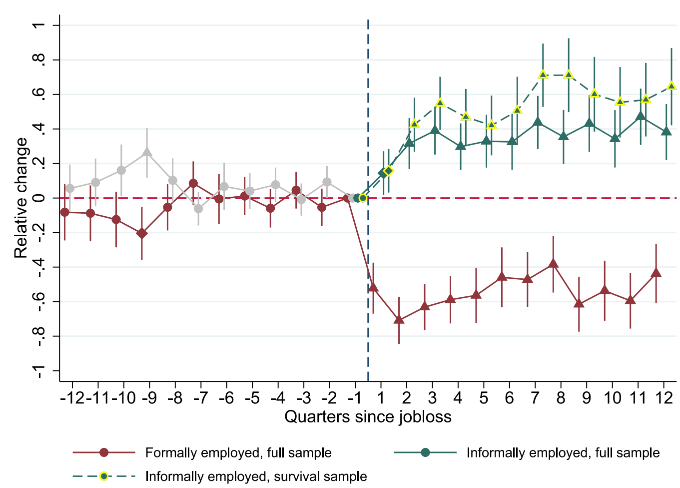
(a) Employment



(b) Unemployment



(c) Inactivity



(d) Employment by formality status

Fig. 2. DiD results on labor market status.
Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Eq. (3). 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. All outcomes of interest are taken from the CMPHS survey.

activity. This analysis is interesting because no detailed evidence exists to date on the labor market trajectories of individuals dismissed from a formal job in labor markets with high informality rates, particularly with respect to their transitions from formal to informal employment. Additionally, these results will serve to interpret findings presented below on consumption, which is the outcome determining the insurance value of UBs.

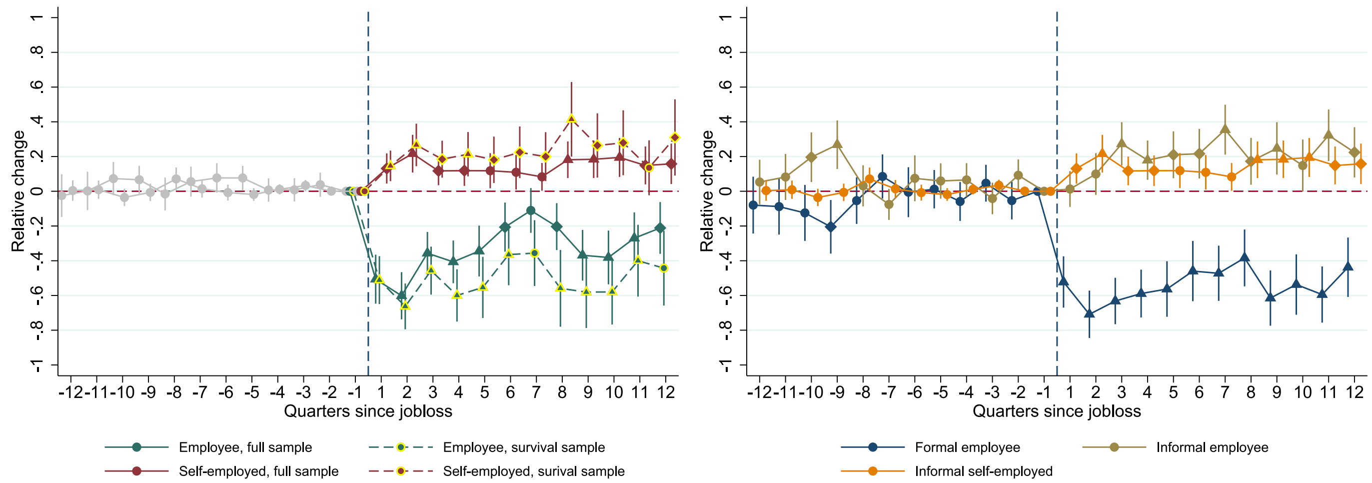
Fig. 1 above has descriptively shown a large decrease in formal employment following job loss. This is confirmed in the DiD results on labor market status in Fig. 2. They show that the treatment group experiences a reduction in the probability of being employed (i.e. formally or informally) following job loss. By construction, this effect is larger for the survival sample, which excludes individuals as soon as they are formally re-employed. However, the overall drop in employment is relatively small and almost disappears after two years (panel A).[23] Corresponding trends are observed for the increase in 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). While the drop in total employment was only of temporary nature, the shift in the type of job held is permanent: even three years after job loss, displaced formal workers are on average 43.7 percent less likely to hold a formal job and 38.2 percent more likely to work informally. To our knowledge, we are the first to directly document such a reallocation from formal to informal employment for UB recipients.[24]

Our data also enable us to present novel results on whether the shift of employment by formality status (i.e. formal and informal) is associated with a change in employment status (i.e. dependent employment and self-employment).[25] This allows to obtain a better sense of the long-term implications of the shift towards informality, as self-employment

[23] In the first and second quarter after the layoff event, UB recipients are on average only 40 percent 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 percent (Verho, 2020).

[24] The finding is, however, consistent with results in Gerard and Gonzaga (2021), who show that re-employment rates in the formal sector remain low even after individuals have terminated UB eligibility.

[25] While self-employed individuals are not eligible to UBs in Mauritius (see Section 3), it is possible for UB recipients to become self-employed after layoff.



(a) Employment status

(b) Employment status and formality

Fig. 3. DiD results on employment status and formal nature of the job.
Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Eq. (3). 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. All outcomes of interest are taken from the CMPHS survey. In panel B, formal self-employment is not plotted as the fourth possible group as its share in total employment is close to zero (see Online Appendix Figure A1, panel A).

is likely associated with different working conditions compared to both formal and informal dependent employment, such as a higher earnings volatility. Additionally, self-employed individuals may be less likely to return to formal dependent employment, compared to informal employees. A shift towards informal self-employment might therefore explain the long-term effects on informality rates documented above.

We find that self-employment increases after job loss (panel A of Fig. 3), but not as much as overall informality rate. As shown in panel B of the same figure, this is because the increase in informality is equally explained by an increase in informal dependent employment and (informal) self-employment. This effect is not driven by any structural change in employment distribution within the Mauritian labor market during the period of the analysis (Online Appendix Figure A1, panel A).[26] For our sample of UB recipients, however, there is a drastic reduction in formal dependent employment after layoff, while informal dependent employment and self-employment increase (Online Appendix Figure A1, panel B).

Similarly, we study transitions of laid-off individuals by overlapping their broad sector of economic activity (i.e. agriculture, manufacturing and services) with their formality status. We find that, before job loss, most individuals are employed in manufacturing and services. Employment then drops the most in the manufacturing sector, followed by services, while the already low share of employment in agriculture remains constant (Fig. 4, panel A). Within each of the three sectors, formal employment decreases and informal employment increases (Fig. 4 panels B, C, and D).

B. Earnings and transfers

Our data also allows us to explore the consequences of job loss for selected additional job characteristics, most notably for earnings. This is done to complement the results presented above on labor market status. In particular, the implications of the shift towards informality would be different if informal jobs are associated with favorable or less favorable working conditions, relative to formal jobs. In the extreme, if informal jobs were relatively easy to find post job loss (as documented above) and paid relatively high wages, there would be little need for publicly provided unemployment insurance. We also analyze whether other forms of transfers compensate for the loss in labor income at job loss, to have a complete picture of the evolution of disposable income.

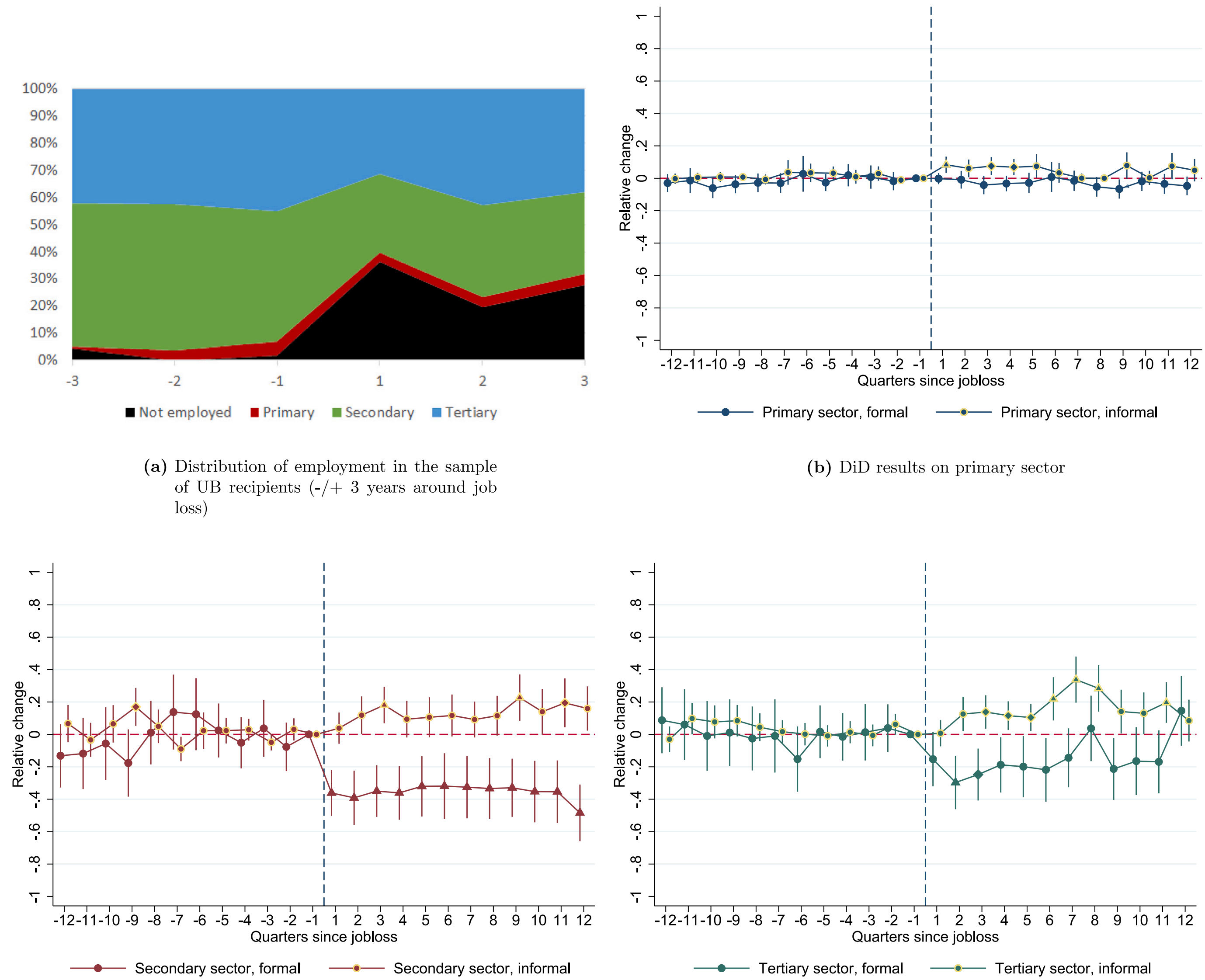
Starting with gross total earnings (Fig. 5, panel A) and looking at the full sample, we see a decrease in monthly earnings of around 60 percent in the year after job loss (i.e. blue line with no outside marker). Earnings drop also for those UB recipients who find a new job (blue line with red markers). The fall in earnings is even larger for the survival sample (blue line with yellow markers), suggesting that informally re-employed individuals earn lower wages compared to those who find a new formal job. In any case, the drop in earnings is persistent over time: in the overall sample, even three years after job loss, previous UB recipients experience an earnings loss of around 50 percent.

The observed earnings loss is substantially larger than previous estimates for advanced economies, which generally range between 15 and 20 percent for prime-age workers (see the review in Couch and Placzek (2010)). Our results are similar to those obtained in developed countries for displaced older workers (Chan and Stevens, 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 income from informal work. UB receipt partially cushions the income drop at the beginning of the unemployment spell (Fig. 5, panel B). However, benefit levels decrease sharply over time and UB eligibility ends well before earnings have recovered. Additionally, we see that hours worked return to their pre-layoff levels relatively rapidly (following a pattern similar to overall employment, panel C of Fig. 5). This suggests that the drop in monthly earnings cannot be explained by a shift towards more part-time employment.

We next analyze the evolution of monthly earnings for re-employed individuals by status in the new job (Fig. 5, panel D).[27] We find that informally re-employed individuals experience a sharper reduction in earnings than those who find a new formal job (respectively, around

[26] Among the employed in Mauritius, formal self-employment is practically non-existent. During the period we analyze, more than 40 percent are formal employees, slightly fewer are informal employees, and around 20 percent are informally self-employed workers.

[27] Neither the timing of re-employment nor the type of job found are exogenous. This implies that results by employment status should be interpreted with caution. However, the robustness tests will show that selection into employment and employment type is unlikely to drive our estimates.



(a) Distribution of employment in the sample of UB recipients (-/+ 3 years around job loss)

(b) DiD results on primary sector

(c) DiD results on secondary sector

(d) DiD results on tertiary sector

Fig. 4. Employment by sector.

Notes: Panel A presents the distribution of employment by broad economic sector for our sample of UB recipients, three years before and after job loss. The primary sector corresponds to agriculture, the secondary sector to manufacturing and the tertiary sector to services. Panels B, C and D report instead regression results, corresponding to the estimates and confidence intervals for the coefficient β, as presented in Eq. (3) in the main text. 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. All outcomes of interest are taken from the CMPHS survey.

50 and 20 percent earning drop on average in the three years after layoff). We see that informal workers tend to work slightly fewer hours per week (Online Appendix Figure A2, panel A), but differences in earnings remain also when these are estimated at the hourly rate (Online Appendix Figure A2, panel B). This indicates that the shift towards informal employment entails a significant earnings penalty, which also persists over time (consistent with theoretical models, see Meghir et al., 2015). Given that UB receipt does not compensate for the earnings differential between formal and informal jobs, we interpret the shift to informal employment as a forced – rather than strategic – decision of formally dismissed individuals, which they make following considerations of economic necessity.

The documented earnings drop is before taxes and transfers. While we do not have information on the amount of taxes paid, these are unlikely to compensate the wage gap between formally and informally re-employed individuals documented above, given that countries like Mauritius have low income and payroll tax rates compared to advanced economies. Additionally, we can investigate whether households receive transfers and if this helps alleviate the earnings drop experienced at layoff.[28] We find a small increase in total transfers after job loss (Fig. 6, panel A). This comes from an increase in social security and other transfers (panels B and D, respectively), consistent with the receipt of UBs.[29] At the same time, private transfers (e.g. from relatives, panel C) do not increase and other forms of public support

[28] Unfortunately, we do not have information on other sources of income except for labor income, transfers and income from properties (the latter being available only for very few individuals in the sample).

[29] The survey 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”.

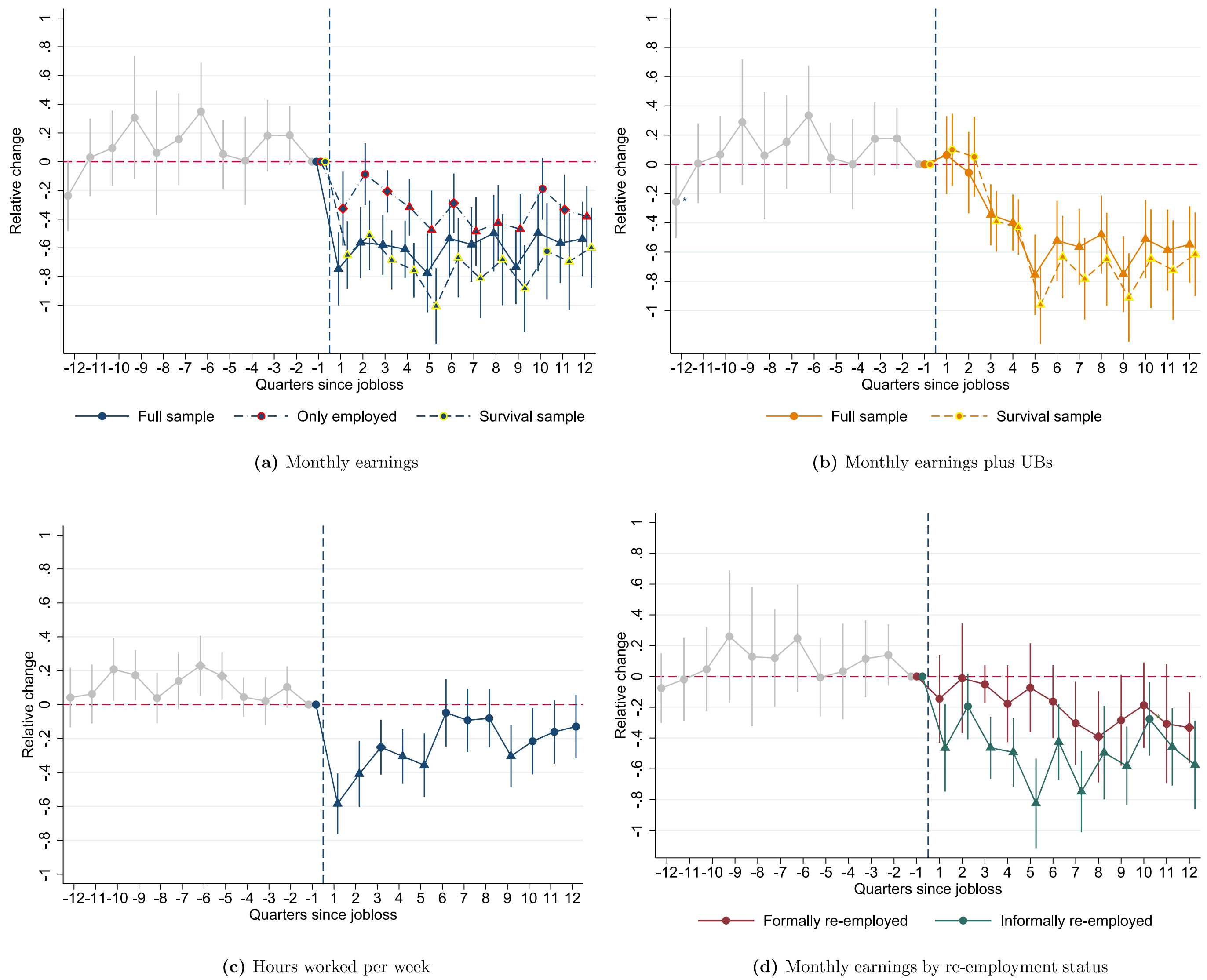


Fig. 5. DiD results on additional job characteristics.
Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Eq. (3). Confidence intervals refer to the 90 percent level; significance at the 5 and 1 percent levels are illustrated by markers shaped in diamond and triangle form, respectively. 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. When results are presented separately for different sub-groups (i.e only employed, survival sample or formally and informally re-employed), the rescaling is conducted using the mean in the respective sub-group in the quarter before job loss (see the text above for details). This is implemented using wage information collected at the time of program registration. All figures instead include the whole sample in the period before layoff (i.e. independently from the re-employment patterns after job loss, as all workers are formally employed before job loss), which is why pre-treatment trends coincide and are denoted by a unique line. Earnings refer to the wages from dependent employment and income from self-employment, and are set to zero for individuals who are not employed. All outcomes of interest are taken from the CMPHS survey.

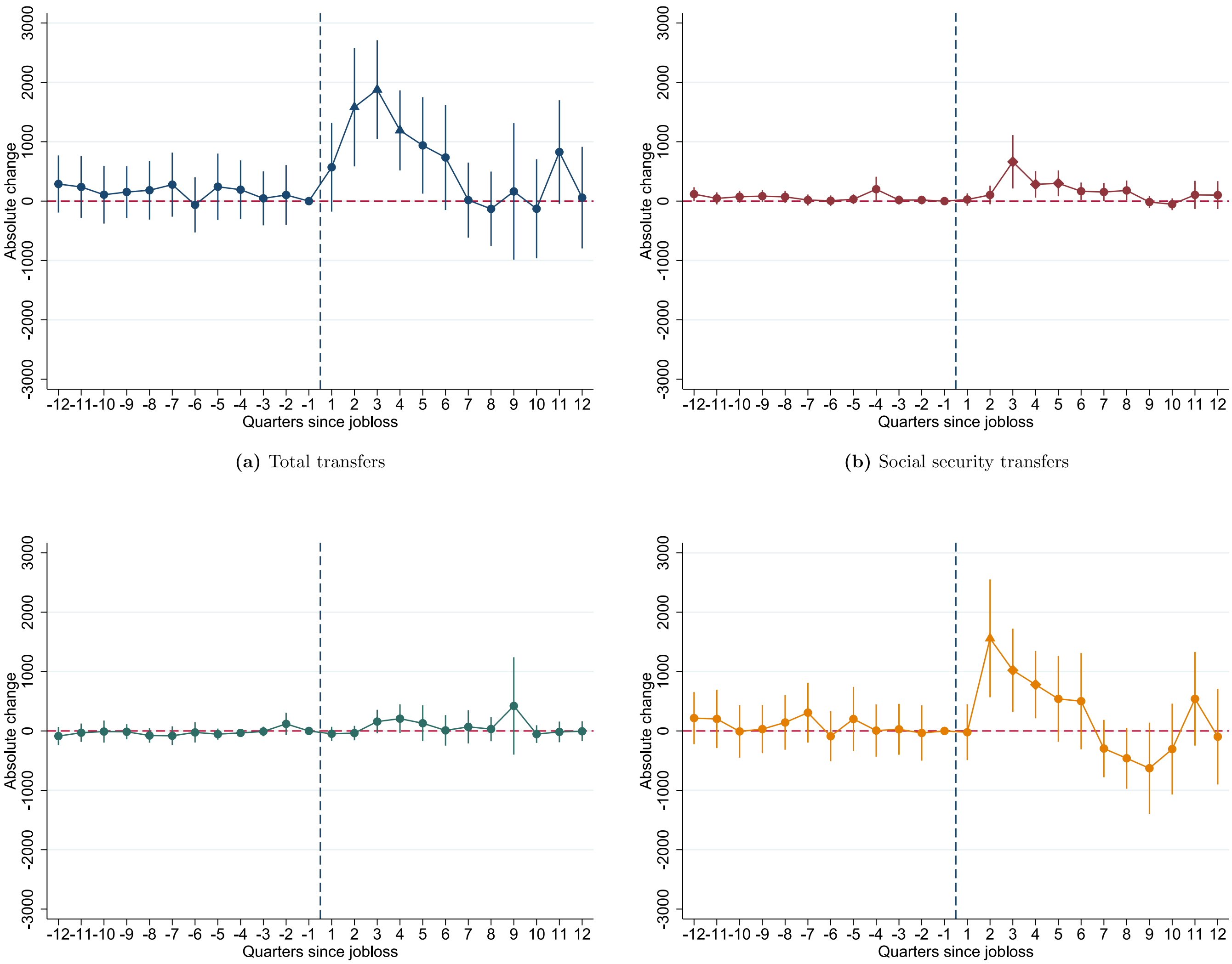
fail to materialize after UB exhaustion. As a result, the increase in total transfers is overall small and short-lived. This is in contrast with evidence of program complementarity in developed countries, where studies have documented that individuals move to other forms of public support when eligibility to the initial transfer ends (Giupponi, 2019; Inderbitzin et al., 2016; Ye, 2022).

C. Consumption expenditures

The evidence presented so far has shown that UB recipients often move into informal employment, earn lower wages upon re-employment and do not have access to other types of transfers. All of these effects are likely to impact consumption, which is the key outcome of interest in the estimation of the insurance value of UBs. We present results on consumption for the overall sample, and refer the reader to the Online Appendix for results for different sub-samples.[30]

Fig. 7 displays the results of the effect of job loss on household expenditures. Total expenditures still remain constant in the first quarter after job loss, potentially due to the presence of consumption commitments and our focus on consumption expenditures, rather than consumption itself. However, total expenditures rapidly decrease in the

[30] Presenting results on the consumption drop at layoff for the entire sample of formally displaced individuals is already an interesting exercise, with the only available estimates outside of developed economies presented in Gerard and Naritomi (2021). These authors use a different source of consumption data (i.e. data on transactions registered as part of a policy experiment to reduce VAT misreporting) and it is informative to compare their results to ours, given the more limited coverage of the registered transaction data but potentially their more accurate reporting (i.e. information not being self-reported).



(a) Total transfers **(b)** Social security transfers

(c) Transfers from relatives **(d)** Other transfers

Fig. 6. DiD results on household transfers.

Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Eq. (3). 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. 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 overall 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 survey.

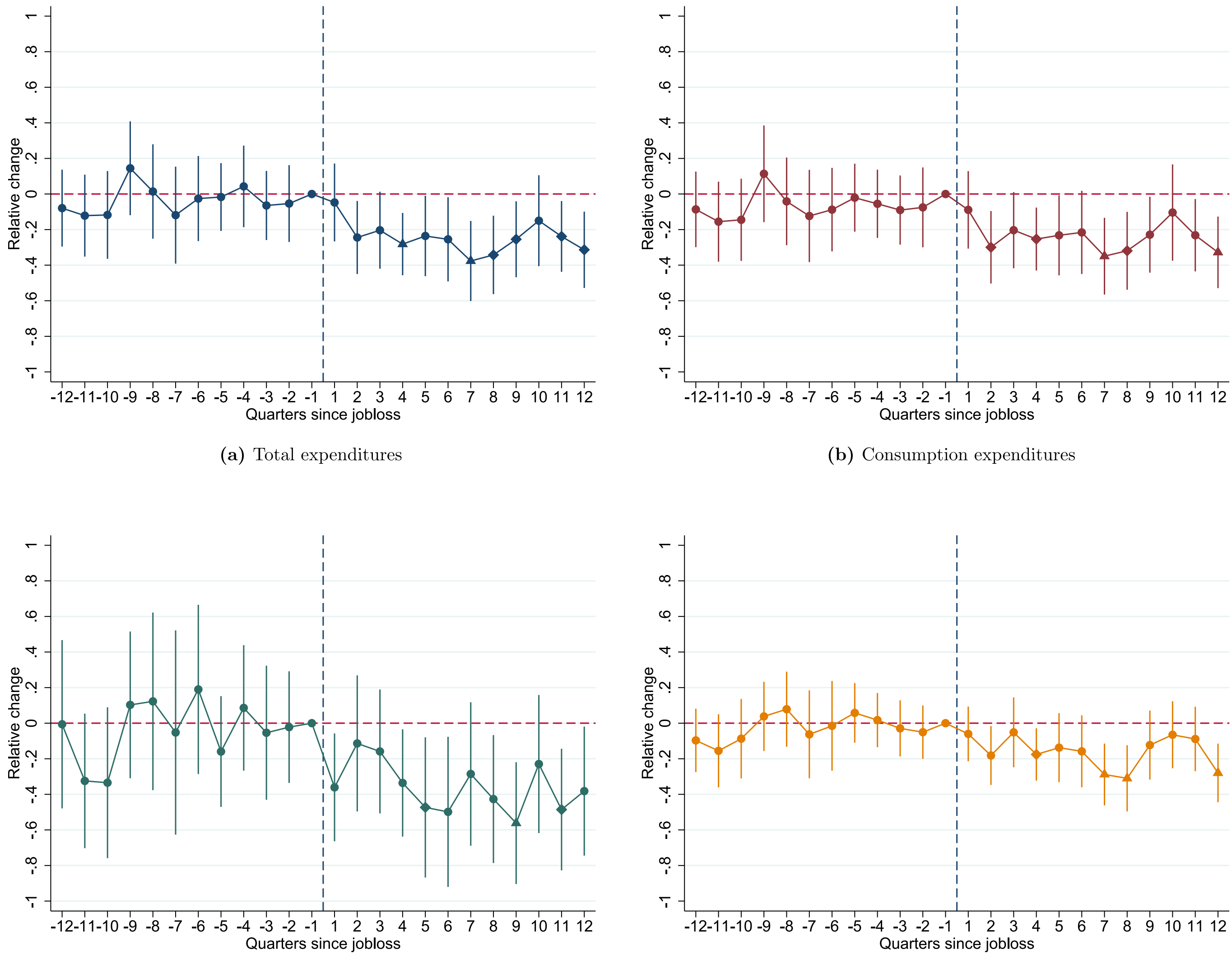
following quarters to reach a value between 20 and 30 percent below the pre-layoff levels (panel A). We observe a very similar pattern for total consumption expenditures (Fig. 7, panel B).[31] Additionally, we split consumption between durable and non-durable goods (panels C and D, respectively).[32] 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 levels. This confirms that the fall in expenditures leads to a fall in consumption (see Ganong and Noel, 2019).[33] Results for some quarters are imprecisely estimated for some outcomes, which is due to small sample sizes. In contrast, point estimates remain largely unchanged in magnitude, but gain precision, when the analysis is conducted at the yearly level (Online Appendix Figure A3).

Our point estimates for consumption are substantially larger than previous ones for developed economies, which are generally below 10 percent (Ganong and Noel, 2019; Gruber, 1997; Kolsrud et al., 2018; Landais and Spinnewijn, 2021), 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

[31] Consumption expenditures is obtained by summing consumption expenditures in a pre-selected series of items that is provided in the interview. Total expenditures also includes expenditures in items that are not part of this list.

[32] 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. around 10 percent of total expenditures) and include items such as education, insurance and appliances. Non-durable expenditures include utilities, transportation, medical costs and food.

[33] 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). In our sample, only 0.06 percent of individuals derive income from backyard-produced goods.



(a) Total expenditures

(b) Consumption expenditures

(c) Durable expenditures

(d) Non-durable expenditures

Fig. 7. DiD results on household expenditures.
Notes: The figure reports estimates and confidence intervals for the coefficient β, as presented in Eq. (3). 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. 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 overall treatment group in the quarter before job loss. All outcomes of interest are taken from the CMPHS survey.

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. This is informative given the more limited data coverage of the Brazilian data. 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).

The large consumption drop can be explained by the fact that individuals anticipate a slow return to the formal labor market and lower wages upon re-employment (recall that earnings are still 50 percent below the pre-layoff levels three years after dismissal, see Fig. 5 above). For these reasons, individuals already reduce consumption even while receiving a relatively generous UB (i.e. in the second quarter after layoff, when the replacement rate is equal to 60 percent). 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. The expenditure drop is the largest among those who are informally re-employed after the initial layoff (panel A in Online Appendix Figure A4). 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. Finally, when looking at the evolution of transfers by re-employment status (Online Appendix Figure A4, panel B), those who remain without a job (i.e. unemployed or inactive) benefit from the largest total transfers.

5.3. Robustness tests

We conduct a series of tests to verify the validity of the DiD results (these are included in Online Appendix A). For ease of exposition, we conduct most of these tests only for selected outcomes of interest within each outcome category (i.e. formal and informal employment, earnings and hours worked, total and social security transfers, and total and consumption expenditures) and for the overall sample. First, we compare our DiD results with those of a pure event study approach with no control group (Figure A5). This addresses concerns on how our control group was constructed. Second, we change the control group to include individuals with three years of employment at the time in which treated individuals lose their job, but who could be in any labor market status thereafter (Figure A6). Similarly, we change our tenure requirement for the treatment group to equal 12 months at the time of job loss (from 36 months used in the main analysis) and adjust the control group accordingly (i.e. placebo job loss after one year in the job). We see that results remain mostly unchanged, even though with the latter definition of the control and treatment groups, we do not see a full return to employment within three years after job loss (Figure A7).[34] Additionally, we run the DiD specification but without re-weighting observations with the propensity score (Figure A8). 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 with individual-level fixed effects. The results are almost identical using the two methods (Figure A9).

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 economic cause.[35] This is done to look at more exogenous forms of job separation that are less likely to be triggered by the worker, in line with studies that have focused on firm closures. Results are very similar to those obtained for the overall sample (Figure A10). 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 A11, 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 A11, 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 is exogenous. First, we exploit 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 A12 show that all groups experience a long-term drop in earnings (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 earnings between formal and informal jobs are driven by selection bias. We compare 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 dummy variables for industry 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 earnings, adding controls should reduce the estimated earnings gap between formal and informal jobs. However, this does not appear to be the case and leads us to conclude that the large earnings drop at layoff is in large part a result of the shift to informal employment (Figure A13).

[34] This might be because the treatment group now also includes individuals with a lower attachment to the labor force; these might decide to leave the labor market upon job loss.

[35] This has been classified as dismissals due to firm closure, economic dismissal, financial difficulties, being in receivership, redundancy, structural dismissal and technological reasons.

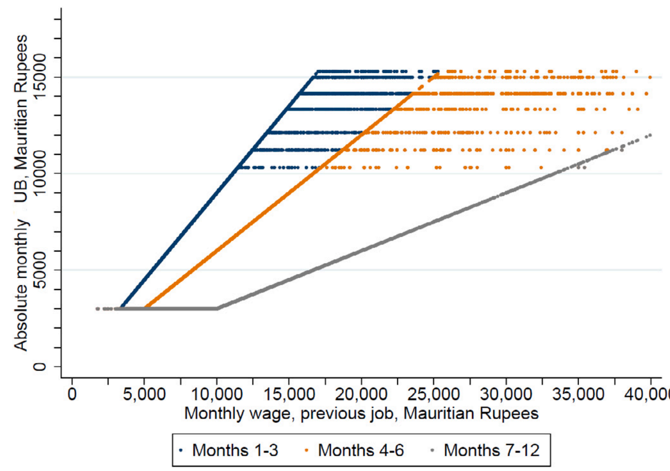


Fig. 8. UB entitlements in Mauritian Rupees as a function of the monthly wage at job loss and month of unemployment duration.
Notes: The figure shows UB entitlements (see Eq. (2)) during months 1–3 (blue), months 4–6 (orange), and months 7–12 (gray) of unemployment duration. UB entitlements were inferred 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.

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

6.1. Empirical approach

As explained in Section 3, the Mauritian UB schedule exhibits kinks at the upper and lower end, which we illustrate in Fig. 8. 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 Online 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, Online 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' behavior, whereas benefits received are determined by when individuals leave the program upon re-employment.

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.[36] After imposing these

[36] Policy take-up is extremely low among informal workers who lose their job (while being almost complete among previously formal workers) and

restrictions, we obtain a sample of 17,791 individuals. Summary statistics are shown in Online Appendix Table B2 (columns (1)–(3)) for the overall sample and for individuals in the proximity of the upper and lower bounds, respectively. As expected, individuals at the upper bound are on average older, more educated, and more likely to be men than their counterparts at the lower bound. While they also re-enter formal employment faster, informal employment rates instead play a similar role across the two bounds. Overall, the differences between the two bounds confirm the usefulness of investigating them both. At the same time, it is worth noting that even the upper bound is negatively selected compared to the overall employed population in terms of education and earnings (Online Appendix Table B2, column (4)).[37]

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:

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

$\mathbb{1}[]$ 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 Mauritian 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 $h = 0.248$ and the coverage error rate (CER) of $h = 0.142$ around the normalized kink points (see Calonico et al., 2017).[38] 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 Mauritian 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, $\tau_k = 0.3$.[39] 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 bound, placing the corresponding results in Online Appendix B.

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.

6.2. Assessment of the identifying assumptions

The RK methodology is based on the assumption that the density and the partial derivative of the density of the assignment variable evolve smoothly around the kink. Intuitively, this rules out that observed changes in the outcomes of interest are generated by sample selection. Fig. 9 displays the number of observations in each bin of the wage distribution at layoff, normalized by the wage at the upper bound.[40] Using 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 and cannot reject the null hypothesis of no discontinuity (see Fig. 9).[41]

Lack of sorting at the upper bound is coherent with the institutional setting. The upper bound has changed every year during the period of analysis and is computed as 90 percent 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 expect dismissal. In contrast, there is evidence of sorting at the lower bound (see Online Appendix Figure B2). The lower bound was binding throughout the study period for individuals who earned 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. This means that, while there is sorting at the lower bound, this does not seem to be driven by strategic behavior of individuals.

The second assumption underlying a RK analysis is that the marginal effect of the assignment variable on the outcomes of interest is smooth at the kink. Online Appendix Figure B3 and Table B3 show that observable individual-level characteristics measured at the time of job loss (i.e., gender, age, marital status, presence of dependents and educational attainment) evolve smoothly around the kink at the upper bound. 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 Online Appendix Figure B4 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. Indeed, these additional results are useful to have estimates of the efficiency costs from a kink that is binding at the end, rather than at the beginning, of the unemployment spell (see Section 7 for details).

6.3. Main results on 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. For the MSE-optimal bandwidth, the emerging pattern stands in contrast to the smooth evolution of covariates discussed above. The graphical illustration of the RK-estimates for length of UB receipt suggests a change in the slope around the kink point (Fig. 10, Panel A).[42] To the left of the kink, where benefits amount to 90 percent of previous earnings, we observe a positive relationship between underlying wages and UB duration. That relationship becomes

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).

[37] Men are over-represented among UB recipients as they are more likely to work formally and meet the eligibility criteria, such as having worked full time (Liepmann and Pignatti, 2019).

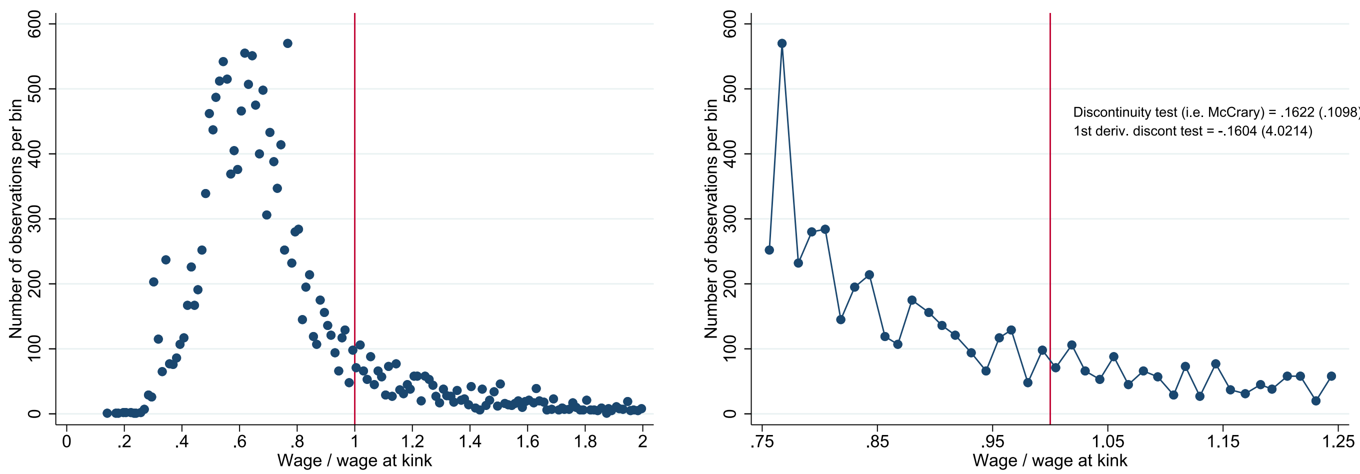
[38] We identify the optimal bandwidths for the upper bound and based on the outcome of UB duration, as this outcome is central for our subsequent welfare analysis. We additionally consider a range of values chosen without theoretical foundation.

[39] For workers below the kink, the UB 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.

[40] Following Landais (2015), we use this normalization when choosing the bandwidth, to account for the multiple changes of the upper bound between 2011 and 2018.

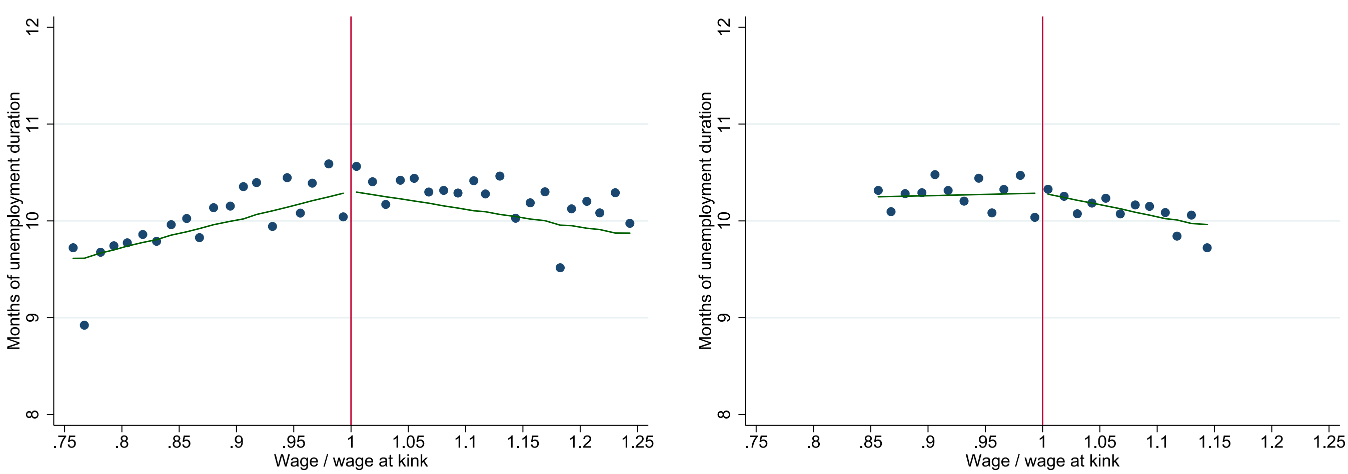
[41] We regress the number of observations in each bin on polynomials of the running variable centered at the kink $(w - w_k)$ and the interaction term for being above the upper bound $(w - w_k)\mathbb{1}[w \geq w_k]$. The coefficient of the interaction term for the first-order polynomial is a test for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

[42] Our conclusions are the same when replacing the y-axis by $w - w_k$, as we essentially normalize by a constant, w_k (albeit one that increased over time).



(a) Entire distribution (b) Observations around upper bound

Fig. 9. Probability density function of the running variable around the 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.



(a) MSE bandwidth (b) CER bandwidth

Fig. 10. RK-model fit for the duration of UB receipt (in months) at the upper bound.
Notes: For two bandwidth choices, respectively, the figures are based on RK-estimations including region (as defined by Mauritan districts) and year fixed effects, and following the specification shown in Eq. (4), which imposes smoothness around the kink. The scatter bin points are predicted values from those regression for bins of size 0.0125 around the normalized kink point. The lines display the RK-model fit, holding region and year constant at sample averages. The MSE-optimal bandwidth of 0.248 was used in Panel A, while the CER-optimal bandwidth of 0.124 was used in Panel B.

negative to the right of the kink, where UBs are capped at the maximum level. This seems to indicate that replacement rates below 90 percent induce individuals to exit the program faster and receive UBs for a shorter time.

The RK-results in column (1) of Table 2 corroborate this finding. When including only district and year fixed effects, the findings indicate that benefit receipt increases by 0.324 months due to a 1,000 Rupee (USD 61 in PPP) increase in the UB level. The corresponding elasticity equals 0.47 (s.e. 0.14). The conclusions are almost the same when additional control variables are added (column 2). In comparison, for the United States and Europe, Schmieder and von Wachter (2016) report elasticities of UB receipt with respect to benefit levels ranging from 0.07 to 0.78, with a median of 0.30 (see Table 2 in their review). Our MSE bandwidth-based estimates are thus above the median of those typically found for high-income economies. The few studies focusing on a middle-income country context (Britto, 2022; Gerard and Gonzaga, 2021) analyze benefit duration rather than levels and are not directly comparable.

At the same time, these RK-estimates need to be interpreted with some caution as they are sensitive to the chosen bandwidth.[43] They

[43] While we are not aware of an economic reason for this, we observe that – when looking from left to right – the second bin in Fig. 10, panel A represents

Table 2
RK-results for the duration of receiving UBs (months), upper bound.

	(1)	(2)	(3)	(4)
RK results:				
β_1 (Coefficient for $w - w_k$)	0.179***	0.194***	0.017	0.039
	(0.039)	(0.039)	(0.102)	(0.100)
δ_k (Coefficient for the interaction term)	−0.292***	−0.291***	−0.165	−0.186
	(0.086)	(0.085)	(0.214)	(0.210)
Constant	11.343***	10.617***	10.652***	8.490***
	(0.211)	(0.678)	(0.313)	(0.970)
Mean UB duration (B)	9.91	9.91	10.22	10.22
Treatment effect: $\alpha_k = (\delta_k / -0.9)$	0.324	0.323	0.183	0.207
Elasticity: $\eta_B = (\alpha_k * b/B)$	0.474	0.473	0.260	0.293
	(0.140)	(0.138)	(0.337)	(0.331)
Observations	4,463	4,463	1,962	1,962
Bandwidth criterion	MSE	MSE	CER	CER
Region and Year FEs	YES	YES	YES	YES
Additional controls	NO	YES	NO	YES

Notes: The table shows RK results for the duration of receiving UBs (in months) at the upper bound, according to the specification shown in Eq. (4). Columns (1) and (2) refer to the MSE-optimal bandwidth of 0.248 around the normalized kink point; while columns (3) and (4) are based on the CRE-optimal bandwidth of 0.142. 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 receipt duration B at the kink point.

tend to be smaller and imprecisely estimated outside the proximity of the MSE-optimal bandwidth (see Online Appendix Figure B5). To make this explicit, in Panel (B) of Fig. 10 and columns (3) and (4) of Table 2, we also display results for the CER-optimal bandwidth. The graphical representation suggests a minor change in slope, and the RK results, when including only region and year fixed effects, are associated with an imprecisely elasticity of 0.26 (s.e. 0.34). We therefore interpret the MSE bandwidth-based estimates as conservative results for the biggest possible effect that we cannot rule out. In our welfare analysis, we benchmark these results against the more representative effect indicated by the CER-optimal bandwidth. Finally, we note that the results for the lower bound follow similar patterns (see Online Appendix Figure B6 and Table B5).

UB efficiency costs also depend on the total time spent out of employment, independently from the length of benefit receipt. Here, we follow Gerard and Gonzaga (2021) who show that efficiency costs arise only from the effect of UB generosity on delayed formal (but not informal) re-employment. We obtain elasticity estimates of the duration until formal re-employment with respect to benefit levels of 0.76 (s.e. 0.27) when we cap formal re-employment at two years after job loss for the MSE-based specification with region and year fixed effects. The same specification yields an elasticity of 1.45 (s.e. 0.65) for the CER-based bandwidth (see Online Appendix Table B6).[44] For developed economies, Kolsrud et al. (2018) estimate the same elasticity at 1.5 using Swedish data. The review by Schmieder and von Wachter (2016) discusses elasticities in the range between 0.10 and 2.00, with a median of 0.53 (see Table 2 of their study). Overall, our estimates thus fall within the range of these elasticities but tend to exceed the median elasticity identified for developed economies. Potentially, this is because higher UBs are associated with a certain substitution towards informal employment, as suggested by the next sub-section.

6.4. Additional results on the substitution from formal to informal employment

As part of the RK analysis, in Online Appendix D we also 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., 2022). 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).

Applying the imputation to our RK analysis, our findings suggest that more generous UBs may lead to a substitution from formal to informal employment, but also that this effect tends to become smaller in the second year after job loss. Of course, the imputation requires us to assume that the probability of working informally in a given month, among those not formally employed, is constant across workers (accounting for observable characteristics).[45] Replicating our analysis in another context with larger sample sizes, where no such assumptions need to be imposed, could thus be an important direction for future work.

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

an outlier. An associated issue is that, when conducting a placebo test for the presence of a jump around the kink point for the MSE bandwidth, we find that the main RK-coefficients (β_1 and δ_k) remain qualitatively unchanged, but also that the added intercept shifter is statistically significant (it is insignificant for the CER-bandwidth). Please refer to Online Appendix C for robustness analyses concerning the choice of the polynomial order and a possible functional dependence between the forcing variable and the outcome of interest.

[44] Note that we focus on the first two years following job loss, as after this point, we no longer observe differential survival rates out of formal employment for individuals above and below the kink, respectively.

[45] In other words, we need to assume that this probability is the same for the average worker and the marginal worker, who responds to the variation in UBs.

(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 Eq. (1) of that section. As explained before, 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 first replicate our main DiD results for the sub-sample in the proximity of the upper bound (Online Appendix Figure A14). The results confirm the main conclusions of the previous DiD analysis, even though the coefficients are less precisely estimated for the smaller sub-population around the upper bound.

To estimate the flow drop in consumption at unemployment, we then follow Landais and Spinnewijn (2021) and Kolsrud et al. (2018).[46] In line with these authors' empirical implementation, we estimate a DiD model for consumption as in Eq. (3) (see Section 5.1) but at the annual rather than quarterly level, where we restrict the treatment group again to individuals in the proximity of the upper bound. We also exploit that those UB recipients, who were interviewed in the CMPHS survey in the year following job loss, have spent a different number of months in unemployment at the time of the interview; and include this number as an additional explanatory variable. Multiplying the corresponding coefficient by 12 and dividing by consumption in the year prior to job loss, we obtain an estimated drop in consumption equal to 0.281 (s.e. 0.126) when using the population around the upper bound as defined by the MSE bandwidth. For the CER bandwidth, the estimated drop is equal to 0.282 (s.e. 0.297) (see Online Appendix Table A2).[47] These estimates are similar to those we discussed in Section 5.2 for the sample of all UB recipients, but are again larger than most estimates from advanced economies.

Turning to the efficiency costs, we take our preferred estimates of the elasticities of benefit duration and time until formal re-employment using both the MSE and CER bandwidths. For the MSE bandwidth, this delivers an elasticity of 0.474 for benefit duration and 0.762 for time until formal re-employment, while the elasticities are equal to 0.260 and 1.452, respectively, for the CER bandwidth. 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 Eq. (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 for the MSE population, and equal to 10.22 and 15.42 for the CER population).

It is important to note that the welfare formula introduced in Section 2 refers to a general setting where the change in benefit level applies during the entire program duration. Instead, we have estimated the efficiency costs arising from a kink that applies only in the first three months after unemployment. The extent to which this general formula can be applied to our specific context depends on whether the elasticity estimates that we obtain are similar to those that would be obtained from a benefit change that applies throughout the entire length of benefit receipt. While we cannot directly test for this, we have provided suggestive evidence in the previous section that our efficiency cost results may hold more generally, by exploiting another kink that is binding in the last six months of benefit receipt and finding that point estimates are similar.

Another decision concerns the choice of $\frac{\tau}{b}$, where two approaches are possible. As a first approach, we directly impose a value for $\frac{\tau}{b}$. Here, we follow Landais (2015) and Gerard and Gonzaga (2021) and proxy $\frac{\tau}{b}$ with the number of UB recipients per formal employee (0.024 in Mauritius). Given the large rate of informal employment in Mauritius, this is a better proxy for $\frac{\tau}{b}$ than the unemployment rate. The second possible approach starts from an assumption on the appropriate tax rate to then obtain a value for $\frac{\tau}{b}$. We use this approach to benchmark our results against those from developed economies and thus impose a fixed tax rate at 0.03 as in Schmieder and von Wachter (2016). Since the average replacement rate over the year of benefit eligibility equals 0.525 in Mauritius, this delivers a value for $\frac{\tau}{b}$ equal to 0.057.

In Fig. 11, we present our estimates of the welfare effects of an increase in UB levels, using the parameter estimates obtained for the population around the upper bound as defined by the MSE bandwidth (panel A) and the CER bandwidth (panel B). In each panel, we present welfare estimates using the two different approximations of $\frac{\tau}{b}$ presented above, but note that results are not very sensitive to the choice of this parameter. Instead, welfare estimates strongly depend on the choice we make of the coefficient of relative risk aversion (γ), where we follow the literature and consider a range of values for γ from 1 up until 5 (see Kolsrud et al. (2018), Schmieder and von Wachter (2016), and also the discussion in Chetty and Finkelstein (2013)). In particular, when we approximate $\frac{\tau}{b}$ with 0.024, welfare effects from more generous UBs are positive for any value of γ above 1.7 using the MSE bandwidth, and 1.1 using the CER bandwidth. When we instead approximate $\frac{\tau}{b}$ with 0.057, welfare estimates become positive when γ is above 1.9 (MSE bandwidth) or 1.3 (CER bandwidth).[48]

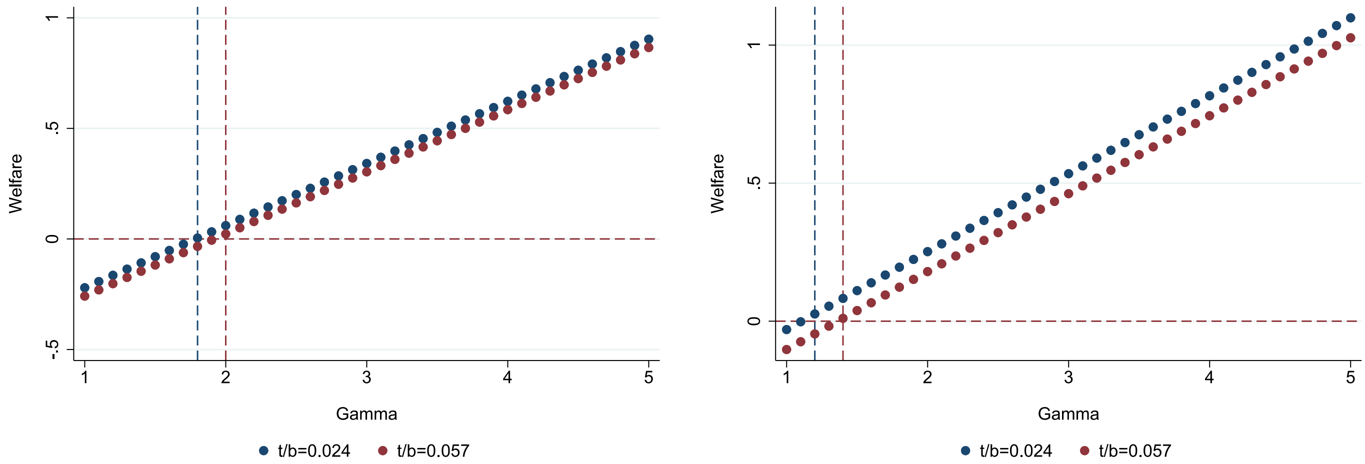
We additionally compute the Marginal Value of Public Funds (MVPF) following the approach by Hendren and Sprung-Keyser (2020). We derive our estimates using the MSE bandwidth. To compare our results with those of previous studies, we use a value of γ equal to 2 and impose a fixed tax rate of 0.03 (resulting in a value of $\frac{\tau}{b}$ equal to 0.057, as explained above). We present our results in Table 3, where we also include estimates from previous studies as reported in Hendren and Sprung-Keyser (2020).[49] Our estimate of the MVPF is equal to 1.01, meaning that the willingness to pay for the policy is marginally higher than its cost for the government, considering both the direct costs from higher benefits and the behavioral responses. As a matter of comparison, previous MVPF estimates of UB programs from the United

[46] More specifically, they rely on a parametric model that assumes that consumption patterns after layoff can be summarized by a discontinuous jump at layoff and a linear slope afterward (see Landais and Spinnewijn (2021), pp. 3059 ff). Results in Online Appendix Figure A14 (panel D) confirm that this is a reasonable assumption in the present setting.

[47] Note that these estimates correspond to the average monthly drop in consumption observed in the entire year after job loss, while, in principle, these should correspond to the onset of job loss. This is a non-trivial difference, because results in Fig. 7 suggest that consumption remains roughly stable in the first three months after job loss, while dropping significantly afterwards (even though results in Panel D of Online Appendix Figure A14 for the sub-sample of individuals around the upper bound are less conclusive in this respect). We nevertheless use the monthly consumption drop estimated over the entire year after job loss as our preferred measure. The exact timing of the consumption drop is difficult to capture in our data, given that (i) the CMPHS survey reports information on consumption expenditures (rather than consumption), (ii) we are not accounting for consumption commitments, and (iii) we might not be perfectly measuring the timing of job loss. As such, there is a risk that we would underestimate the drop in consumption, if we were to measure it only in the first three months after job loss. Finally, the large and persistent consumption drop suggests that the welfare effects of UBs increase at later unemployment duration points, assuming that the efficiency distortions are similar to what we observe for the first three months.

[48] While it is important to make transparent how our results depend on γ, a recent study by Landais and Spinnewijn (2021) provides estimates of γ in the range of around 4 to 8, which are consistent with comparatively high levels of risk aversion and, in our setting, would imply clearly positive welfare effects.

[49] The only change that we make is to present the efficiency costs using a tax rate of 0.03, rather than the US labor tax wedge, as that would be unrealistic in the present context. Consistently, we adapt the estimates presented in Table D.II of Hendren and Sprung-Keyser (2020) using the efficiency cost estimates provided in the second to last column of Table 2 in Schmieder and von Wachter (2016).



(a) MSE bandwidth (b) CER bandwidth

Fig. 11. Welfare estimates for different values of γ and $\frac{\tau}{b}$.
Notes: The figure reports the estimates of the welfare effects obtained by varying the value of the coefficient of relative risk aversion (γ) and/or the ratio between the tax rate and benefit generosity (τ/b). The dashed vertical lines indicate the values of γ at which welfare estimates equal zero, depending on the choice made for τ/b. The values for γ denote an increase in risk aversion as γ increases. For τ/b, we follow Landais (2015) and proxy it with the number of UB recipients per formal employee (delivering an estimate of 0.024 in our context). Alternatively, we impose a fixed tax rate of 0.03 as in Schmieder and von Wachter (2016) (next to last column of their Table 1, delivering an estimate of 0.057 in our context). Welfare estimates are derived for the population at the upper bound of benefit levels, as defined by the MSE optimal bandwidth (panel A) or the CER optimal bandwidth (panel B). For these two populations, we derive separate estimates of the consumption drop at layoff ($\Delta C/C$) as well as of the elasticities with respect to length of UB receipt ($\eta_{B,b}$) and time until formal re-employment ($\eta_{D,b}$). Additionally, we compute separately the values of B and D.

Table 3
Marginal value of public funds (MVPF), estimates from different contexts.

	WTP	FE	1+FE	MVPF
Card et al. (2015), Expansion	1.17	0.38	1.38	0.85
Card et al. (2015), Recession	1.17	0.95	1.95	0.60
Chetty (2008)	1.17	0.16	1.16	1.01
Katz and Meyer (1990)	1.17	0.29	1.29	0.91
Kroft and Notowidigno (2016)	1.17	0.23	1.23	0.95
Landais (2015)	1.17	0.14	1.14	1.03
Meyer and Mok (2007)	1.17	0.41	1.41	0.83
Solon (1985)	1.17	0.08	1.08	1.08
This study	1.56	0.54	1.54	1.01

Notes: The table reports the estimates of the marginal value of public funds (MVPF) for the present paper as well as from previous studies. WTP stands for "willigness to pay", while "FE" stands for fiscal externality. See Hendren and Sprung-Keyser (2020) for details on the methodology and definitions used. Estimates from previous studies are obtained from Hendren and Sprung-Keyser (2020), but using a fixed tax rate equal to 0.03, rather than the US tax wedge on labor (as retrieved from Schmieder and von Wachter (2016)). All estimates on the WTP from previous studies assume a common consumption drop at layoff, as computed in Hendren (2017), and use a value of γ equal to 2. For our own estimates, we instead use the consumption drop at layoff as computed for the population around the upper bound of benefit levels, using the sample as defined by the MSE optimal bandwidth.

States have found slightly lower MVPF estimates (i.e. ranging from 0.60 to 1.08, with a median value of 0.93).[50] Similar MVPF estimates are found for other public policies targeting adults, such as expansions of health insurance (with estimates from 0.40 to 1.63), disability insurance (0.74–0.96) and the earned income tax credit (1.12–1.20). Much larger MVPF estimates are instead found for policies targeting children (Hendren and Sprung-Keyser, 2020).

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, and bring these together in a unified setting. Having access to matched administrative and survey data on employment, wages and consumption of formal and informal workers, we are able to characterize underlying labor market mechanisms. This is important to inform a debate on the nature of informal jobs, and the role they play in shaping welfare effects of UBs.

Our results reveal that the detrimental welfare effects of job loss are large in magnitude and persist over time. We also find that efficiency costs fall within the range, but are above the median, of existing estimates for high-income countries. Yet, our estimates of the insurance value of UBs are substantially larger than results from high-income countries and tend to counterbalance the efficiency costs. This is because displaced workers move to informal employment out of economic 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 higher-quality formal employment. Even when conservatively estimated, welfare effects of UBs are therefore positive for coefficients of risk aversion exceeding two.

These findings have important policy implications, as they show that the unintended consequences of UB schemes in contexts with high rates of informal employment might have been overstated and their benefits at times 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 dominate in contexts of high informality (Duval and Loungani, 2019; Robalino et al., 2009). Our empirical results suggest otherwise. In contexts 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 insurance through UBs, is thus a sensible policy choice.

Declaration of competing interest

None.

[50] Note, however, that previous estimates were obtained by comparing the efficiency costs estimated separately in each paper with a common estimate of the willingness to pay as derived in Hendren (2017).

Data availability

The authors do not have permission to share data.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1916/j-jpubeco.2023.105032. It contains Online Appendices A through D, which are referenced in the main text.

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