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The Fiscal Costs of Unemployment Insurance

RESEARCH

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ABSTRACT

This paper studies the fiscal costs of unemployment insurance (UI). It surveys alternative methods used in the literature to estimate the impact of UI on government budgets and compares them within a unified framework that incorporates behavioral responses, wage effects, and fiscal externalities. These methods are then applied to administrative data from Argentina. Combined with estimates of the consumption drop following unemployment, the paper evaluates the Marginal Value of Public Funds (MVPF) for UI and finds that, while results vary depending on the treatment of fiscal externalities, MVPF is generally above one—indicating that the social benefits of marginal increases in UI outweigh their fiscal costs.

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1 INTRODUCTION

A common problem in applied economics is the measurement of the cost of an intervention. In the first place, this implies measuring the opportunity cost of resources that are directly involved in the intervention. It is then crucial to assess the indirect costs, which are related to behavioral responses of agents affected by the intervention. Additionally, the interaction between interventions could lead to fiscal externalities.

In the measurement of costs, economists are armed with empirical techniques that provide evidence of causal effects. Experiments, natural experiments, discontinuities, kinks, and other forms of variations in the policy parameters are exploited to measure the effects on behavior and on multiple outcome variables. In fact, the "credibility revolution" has generated an accumulation of causal effect estimates. Less effort has been devoted to use these estimates for welfare analysis. A neat route to channel this flow of information is to build complete estimates of costs of the intervention, including direct costs, indirect costs, and fiscal externalities. Additionally, whenever welfare gains can be estimated, the Marginal Value of Public Funds (MVPF) can be computed and compared (Finkelstein and Hendren 2020; Hendren and Sprung-Keyser 2020).

In this paper, we focus on the costs of a transfer: of unemployment insurance (UI). This is an example of a policy that has deserved the attention of researchers, not only because of its importance, but also because the effects of these transfers have been frequently measured consistently with theory exploiting available microdata.

The literature on the impact and welfare effects of UI has frequently emphasized the behavioral effects of these transfers (see the surveys by Tatsiramos and van Ours 2014 and Schmieder and von Wachter 2016). The effects of UI on search behavior among the beneficiaries is the first and most frequently analyzed response. To measure it, empirical research has focused on the effects of UI on the non-employment duration, the extension of covered UI, the job-finding rate, and the total amount of UI paid. These variables contribute differently to provide a measure of the effects of UI on public funds.[1]

Much less attention has been given to the fact that UI could also affect re-employment wages, the quality of re-employment jobs, the expected employment duration, and other variables that could provide a measure for additional fiscal externalities of UI. A clear example of these externalities is the additional tax collected by proportional income taxes when wages rise due to UI. While evidence is scarce, some results highlight the importance of the effects of UI on the quality of re-employment jobs.[2]

The literature has also studied the effects of unemployment and UI on other sets of variables, such as health and crime. These other effects could be part of the long-term fiscal externalities; Also, it has been acknowledged that externalities could arise by the effects of UI on other programs, such as Unemployment Assistance or Disability Insurance. If these effects are large, they should be taken into account.[3]

Finally, the empirical literature has considered the possible effects of this policy on non-beneficiaries' outcome variables. There are different economic mechanisms behind these types of effects. First, UI could alter the labor market as a whole, change wages, and affect total job creation. Second, a more generous UI could reduce job-search congestion, leaving more job opportunities for non-beneficiaries. Third, a more generous UI could provide incentives to search for a job among non-beneficiaries through an eligibility effect. In this sense, this literature intends

1 There is a vast empirical literature on the effects of UI, including effects on non-employment duration (Card, Chetty, and Weber 2007; Lalive 2007; Schmieder, von Wachter, and Bender 2016; Uusitalo and Verho 2010; Lalive, Ours, and Zweimüller 2006), on job-finding rate (Meyer 1990; Katz and Meyer 1990; Bover, Arellano, and Bentolila 2002; Chetty 2008), on the total amount of UI paid (Lee et al. 2021; Campos, García-Pérez, and Reggio 2022), among others.

2 Nekoei and Weber (2017), Lalive (2007), Schmieder, von Wachter, and Bender (2016), Gangl (2002), Centeno (2004), Centeno and Álvaro Novo (2014) explore the effects of UI on re-employment jobs.

3 See for example Britto, Pinotti, and Sampaio (2022) for an analysis of the effect of UI on crime. Hendren and Sprung-Keyser (2020) provides some measures of fiscal externalities through Disability Insurance.

to quantify the macroeconomic effects of UI, extending the microeconomic effects of UI on beneficiaries to the more macroeconomic effects on unemployment rate.[4]

The welfare-improving effect of UI relates to maintaining consumption levels after a job-separation shock. Thus, this insurance value depends chiefly on the ability of individuals to smooth their own consumption, using liquid savings, assets, family transfers, or credit markets. This view has emphasized the importance of measuring the consumption drop upon unemployment, of consumption differences between employees and covered unemployed workers, or other estimates of the effects of UI on consumption smoothing.[5] These estimates are then used in sufficient statistics (Chetty 2006; Schmieder and von Wachter 2016) or MVPF formulas (Hendren and Sprung-Keyser 2020) to evaluate the welfare gains of providing larger transfers.

This paper analyzes alternative ways to measure the total fiscal effects of UI under different assumptions. We compare formulas, methods and variables; and we discuss the convenience of each approach according to the data available, the identification method, and the characteristics of the provision of UI. This comparison between methods is uncommon in the literature.

We first deal with a general description of a government budget that depends on taxes collected and benefits paid. Government income is collected through proportional payroll taxes. Using this general setup, we consider the different ways to measure the total fiscal effects of UI. We deliver these results in Section 2.

Each approach provides a particular formula that can be evaluated in the data. For this purpose, we use administrative data from Argentina spanning 2005 to 2009. In Section 3, we describe the UI rules, our data, and the methods we use to evaluate the effects of UI on government budget. We also describe the different outcome variables that we use, including non-employment duration, job-finding rate, benefit duration, total benefits paid, and re-employment wages. Our contribution here, compared to our previous work, is to estimate the impact of UI level on different variables, all of which are relevant for alternative estimations of the effects of UI on welfare (Gonzalez-Rozada and Ruffo 2026).

In Section 4, we provide the empirical results and the evaluations of the formulas, beginning with the more parsimonious to the more complete ones. In the process, we describe the alternatives and results, and we compare our findings with those in the literature.

To apply these estimates to welfare analysis, we implement an estimate of the MVPF. This measure of welfare requires considering the gains due to consumption smoothing of UI. Thus, consumption estimates are required. We first report the difference in per capita consumption, comparing employed and unemployed workers. Assuming a given risk aversion, we compute the welfare gain due to the increase in benefits per each additional unit of public funds generated by a marginal change in UI. Section 5 reports that an increase in UI would improve welfare, under reasonable assumptions, and discusses the role of fiscal externalities. Importantly, this result is consistent with our previous research, even when it is based on very different variables and methods, contributing to clarifying the welfare effects of UI on developing countries, a relatively thin literature.

We show that fiscal externalities are key to assessing UI's fiscal impact, far outweighing the role of variables or estimation choices. Overall, UI appears highly valuable in Argentina, with welfare gains exceeding those typically found in developed countries. This aligns with a setting in which households cannot easily offset formal job loss, and where UI has statistically significant but economically modest effects on behavior. We present this conclusions in Section 6.

4 Some papers estimate different macroeconomic effects, including market externalities and effects of UI over different equilibrium outcomes (Hagedorn et al. 2013; Lalive, Landais, and Zweimüller 2015; Marinescu 2017; Johnston and Mas 2018). Other types of equilibrium effects have been analyzed empirically (Landais, Michaillat, and Saez 2018a) and quantitatively (Mukoyama 2013; Landais, Michaillat, and Saez 2018b; Cirelli, Espino, and Sánchez 2021; Piguillem, Ruffo, and Trachter 2023).

5 Gruber (1997), Browning and Crossley (2001), Kroft and Notowidigdo (2016), Landais and Spinnewijn (2021). See also Ganong and Noel (2019), and Gerard and Naritomi (2021).

2 THE GOVERNMENT BUDGET

Let time $t = 1, 2, ...T$ be discrete; let $i = 1, ...N$ be the identification variable of the worker; let U_{itd} be the variable that takes a value of one if worker i is unemployed at time t with unemployment duration d and zero if not. Assume that labor income is taxed at a fixed rate, τ, and that w_{it} is the gross wage of worker i in period t, so that τw_{it} is what that individual contributes in taxes in the period. Let b_{itd} be the amount of benefits received by individual i in period t when unemployment duration is d, where $d = 1, 2, ...T$. Let P be a per period and per capita expenditure that the government incurs in other programs, beyond UI and unrelated to employment. Then, the budget satisfies:

$$\tau \sum_i \sum_t (1 - U_{it}) w_{it} - \sum_i \sum_t \sum_d U_{itd} b_{itd} - P \times T \times N = 0. \tag{1}$$

Notice that P represents the fiscal externalities through other expenditures that labor income finances. Through this variable, any change in the employment rate would have an effect on government budget.

This equation allows wages and benefits to change arbitrarily. We do not impose discounting. We think of this equation as representing a government that balances its budget on a per period basis, where t is time, and where there is a stationary distribution of employment and unemployment individuals. The same equation can be used to represent other interpretations.

Several papers have emphasized the importance of fiscal externalities by imposing that taxes contribute to an exogenous government expenditure. Examples include Lawson (2017), Schmieder and von Wachter (2016), Kroft et al. (2020) and Lawson (2023).

As another source of externality, we introduce proportional taxes instead of tax levels, generating a fiscal externality emphasized in Nekoei and Weber (2017) or in Kroft et al. (2020).

We now want to compute the effects of changes in UI on the government budget. In the process, we make different assumptions to derive the formulas typically used in the literature.

2.1 DURATIONS

Let wages be w and benefits be b (common for all workers). Let potential duration be B, so that $b_{itd} = 0 \; \forall \; d > B$. By dividing by $T \times N$, the government's budget can then be written as:

$$w\tau p_E - bp_B - P = 0$$

where p_E is the average proportion of time spent in employment and p_B is the average proportion of time spent in covered unemployment (receiving b).

Consider now that the average labor market history can be divided into S spells of employment and unemployment. Each spell then has duration $D_S = T/S = D_E + D_U$, where D_E is the average duration of employment, and D_U the average duration of unemployment. This same equation can then be approximated by:

$$w\tau \frac{D_E}{D_E + D_U} - b\frac{D_B}{D_E + D_U} - P = 0 \tag{2}$$

where $D_E/(D_E + D_U)$ is the proportion of time spent in employment and $D_B/(D_E + D_U)$ represents the proportion of time spent in covered unemployment, where D_B is mean covered non-employed duration. This equality is now interpreted in per-worker, per-period amounts.

Assume now a marginal increase in benefits b. The direct effect on the government's budget would then be

$$-\frac{D_B}{D_E + D_U} \tag{3}$$

and, the total effect can be written as:

$$-\frac{D_B}{D_E + D_U}\left[1 + \eta_B + \eta_U \frac{D_U}{D_E + D_U}\left[\frac{\tau w D_E}{bD_B} - 1\right] - \eta_w \frac{\tau w D_E}{bD_B}\right] \tag{4}$$

where η_B is the elasticity of covered duration with respect to benefits, η_U is the elasticity of non-employment duration with respect to benefits, and η_w is the elasticity of re-employment wages with respect to benefits. This formula assumes that D_E and P do not change with benefits, a typical assumption in the literature.

Using the above equations, we can conclude that the total effects on budget per each monetary unit transferred, equation (4) divided by equation (3), is:

$$1 + \eta_B + \eta_U \frac{PD_U}{bD_B} - \eta_w \left[1 + \frac{P(D_E + D_U)}{bD_B}\right] \quad (5)$$

where we assume that the budget is (initially) balanced, and where we use the budget constraint to make the expression depend on P.[6]

This equation shows that the main effect of UI on government budget is related to the change in the duration of covered UI, η_B. The effect of benefits on unemployment is relevant whenever there is some fiscal externality. If D_U is extended there are two countervailing effects: one related to lower taxes collected and the other related to fewer beneficiaries (fewer employed workers in steady state). If the budget is initially balanced and if there is no fiscal externality, these effects exactly balance, in which case, η_U has no effect on the budget. To see this, set $P = 0$ in the above formula. Additionally, if there is no effect on wages, $\eta_w = 0$, the effect is simplified to

$$1 + \eta_B.$$

This last equation is very similar to the effect measured in Shimer and Werning (2007).

A closely related formula is the one in Schmieder and von Wachter (2016) that analyzes values per spell. Using (2) and multiplying by the average duration of a spell ($D_S = D_E + D_U$), then[7]

$$w\tau(D_S - D_U) - bD_B - D_S P = 0$$

After assuming that D_S is fixed, the total over-direct effects on the budget is:

$$1 + \eta_B + w\tau\eta_U \frac{D_U}{bD_B}. \quad (6)$$

2.2 TRANSITION RATES

Government's budget can also be written in terms of transition rates:

$$w\tau \frac{\phi_U}{\delta + \phi_U} - b\frac{\delta\phi_U/\phi_B}{\delta + \phi_U} - P = 0 \quad (7)$$

where $\phi_U = 1/D_U$ is the mean transition rate from non-employment to employment, $\delta = 1/D_E$ is the mean transition rate from employment to UI.[8] Also, we define $\phi_B \equiv 1/D_B$ as the mean transition rate out of UI.

This equation, rewritten from equation (2), maintains the assumption that wages and benefits are homogeneous and constant.

The first term is easy to interpret, as $\phi_U/(\delta + \phi_U)$ is the proportion of employed workers; the ratio $\delta\phi_U/(\phi_B\phi_U + \phi_B\delta)$ is the proportion of covered unemployed workers.

6 If we considered an expression without imposing a balanced budget, τ appears in the formula: $1 + \eta_B + \eta_U \frac{PD_U}{bD_B} - \eta_w \left(\frac{w\tau D_E}{bD_B}\right)$. The imposition of a balanced budget seems obvious. Nevertheless, UI or other policies could be implemented with cross subsidies and thus the budget could be unbalanced for the particular group analyzed. As an example, if studying the effects of UI by regions, probable cross-subsidies would make that balanced budget is not satisfied within each region.

7 Notice that this is the same equation as in Schmieder and von Wachter (2016) but after rescaling the value of the per worker government expenditure by the duration of the spell.

8 These transition rates are defined as $\phi = \frac{\sum_{d=1}^{T} S_d\phi_d}{\sum_{d=1}^{T} S_d}$, where S_d is the survival rate up to duration d in the corresponding state. Thus, transition rates can vary by duration, and still, the average duration can be approximated by $1/\phi$.

The direct effect of marginally increasing UI can be computed as:

$$-\frac{\delta\phi_U/\phi_B}{\delta+\phi_U}$$

while the total over-direct effect ratio is

$$1-\eta_{\phi_B}-\eta_{\phi_U}\frac{P\phi_B}{b\phi_U}-\eta_w\left(1+\frac{P\phi_B}{b\delta}\frac{\delta+\phi_U}{\phi_U}\right) \qquad (8)$$

where we impose that the budget constraint is initially balanced.[9]

Under the assumption that $P = 0$ and $\eta_w = 0$, the above equation simplifies to

$$1-\eta_{\phi_B},$$

which is analogous to the previous case.

Several papers have studied the effects of UI on duration focusing on transition rates, instead of average durations. These include Katz and Meyer (1990), Meyer (1990), Bover, Arellano, and Bentolila (2002), Card, Chetty, and Weber (2007), and Chetty (2008).

Alternatively, other papers use linear models, typically imposing censoring to observed non-employment durations. This is the case with Schmieder, von Wachter, and Bender (2012), Card et al. (2012), and Card et al. (2015b).

The identity $\phi_U = 1/D_U$ suggests that measuring durations or transition rates should be the same. In other words, the results used by analyzing the elasticity of duration, η_U in equation (4), should equate the ones used by identifying the elasticity of transition rate, η_{ϕ_U} in equation (8). This, in fact, could be the case whenever all spells are completed and no censoring is imposed. But this is not the case in practice. The identification of changes on average duration is typically implemented through linear regressions, which identify dD_U/db. The effects on transition rates is typically identified through a duration model (typically a proportional hazard model) that can identify η_{ϕ_U}. If non-employment spells are incomplete, and are in fact censored in different durations, a duration model can use these observations in the estimation while the linear regression implementation drops these observations or imputes the spell duration (imposing censored durations).

Censoring can be also imposed to measure elasticities of non-employment duration at given durations. This is important and it has been done to implement the approximation $\eta_{\phi_B} \approx \eta_{\phi_U}$, that is, to use non-employment duration response to represent the duration of unemployment spell. For example, Chetty (2008) impose censoring to 50 weeks while Gonzalez-Rozada and Ruffo (2016b) implement censoring at 24, 12 or 8 months to provide robustness of their results.

2.3 ACTUARIALLY FAIR BUDGET

We now consider a particular case in which all workers are initially unemployed. Additionally, we consider that once employed they remain at that job. Finally, we assume constant and homogeneous benefits and wages. Let E_{it} be the variable that takes a value of one if worker i is employed at time t and zero if not. From equation (1) we can get

$$\sum_i\sum_t(1-E_{it})/Nb_t+P\times T-\tau\sum_i\sum_t E_{it}/Nw=0$$

where we drop the subscript for duration because time and duration are the same in this case, and we consider t as time and duration.

Given that the survival rate into unemployment at duration t is $S_t = \sum_i(1-E_{it})/N$, we can rewrite the above equation to get:

$$\sum_{t=1}^{B}S_t(b_t+\tau w+P)+\sum_{t=B+1}^{T}S_t(\tau w+P)-\tau wT=0. \qquad (9)$$

9 The formula that does not impose a balanced budget is $1-\eta_{\phi_B}-\eta_{\phi_U}\frac{P\phi_B}{b\phi_U}-\eta_w\left(\frac{w\tau\phi_B}{b\delta}\right)$.

The first summation represents all durations from initial period up to potential duration of UI, while the second represents all periods after UI exhaustion. Finally, the last term refers to tax collection.

This way of representing the budget constraint emphasizes that each period without a job is costly for the government, not only because of UI transfers but also for the foregone taxes paid. Using durations and dividing by T, we get:

$$\tau w \frac{T - D_U}{T} - b\frac{D_B}{T} - P = 0. \tag{10}$$

This case is very similar to the one described in equation (2). The difference is that now the number of periods in employment is assumed to be determined by the duration in unemployment, $D_E = T - D_U$. This assumption is a reasonable approximation whenever participation in the labor market is limited and fixed. Thus, changes in D_U would have effects on the budget even without considering fiscal externalities.

In this case, when assuming a marginal increase of the benefits b, the direct effect on the government's budget would be:

$$-\frac{D_B}{T} \tag{11}$$

whereas the indirect effect can be written as:

$$\frac{-D_B}{T}\left[1 + \eta_B + \eta_U \frac{\tau w D_U}{bD_B} - \eta_w \frac{\tau w\,(T - D_U)}{bD_B}\right] \tag{12}$$

Again, we are assuming that D_E and P do not change with benefits. The total effects on budget per monetary unit transferred, which we obtain by dividing (11) by (12), after imposing a balanced budget, is:

$$1 + \eta_B + \eta_U \frac{D_U}{T - D_U}\left[1 + \frac{PT}{bD_B}\right] - \eta_w\left[1 + \frac{PT}{bD_B}\right] \tag{13}$$

Furthermore, after imposing $P = 0$ to avoid fiscal externalities, we obtain the following result:

$$1 + \eta_B + \eta_U \frac{D_U}{T - D_U} - \eta_w. \tag{14}$$

With or without fiscal externality, equation (10) is the most frequent in the literature (Chetty 2008; Schmieder, von Wachter, and Bender 2012; Schmieder and von Wachter 2016; Liepmann and Pignatti 2024). This approach uses non-employment duration as a measure of the forgone tax collection due to longer unemployment. This idea is also in additional set of papers, including Chetty (2006).

As an example, Kolsrud et al. (2018) provide a version of equation (9), extending it to a benefit that is homogeneous but changing in duration. Shimer and Werning (2007) represent the problem of a government in a recursive definition of its budget. Afterwards, they analyze the case of a discount rate that tends to zero in order to get a similar approach to the one in here, in which equation (9) is also a representation of the actuarially fair budget without discounting.

2.4 BENEFITS PAID BY SPELL

Consider again equation (1). By dividing by $T \times N$ it can be rewritten as:

$$\tau \bar{w} p_E - \bar{b} p_B - P = 0.$$

We now allow for heterogeneity and we consider average values in $\bar{w}$ and $\bar{b}$. Additionally,

$$\tau \bar{w} D_E - R - P(D_E + D_U) = 0,$$

where now we multiply by $D_E + D_U$, considering average amounts by spell, where $R = \sum_d S_d \bar{b}_d$ is the average total UI paid in a spell of covered unemployment, where $\bar{b}_d$ is the average benefits by duration (that would simplify to $\bar{b} \times D_B$ with constant benefits), and S_d is the survival rate up to duration d.

Consider a change in UI policy. Let $\tilde{b}$ be a policy parameter, such as the initial transfer or the maximum level of transfers or other important parameter that determines the level of UI. Let $-\Delta = -\sum_d S_d \frac{\partial \tilde{b}_d}{\partial \tilde{b}}$ be the direct effect.

The total over-direct effect would be:

$$\varepsilon_R \frac{R}{\Delta} + \varepsilon_U \frac{PD_U}{\Delta} - \varepsilon_w \frac{w\tau D_E}{\Delta}$$

where ε is a semielasticity with respect to changes in $\tilde{b}$.

Imposing the budget constraint:

$$\varepsilon_R \frac{R}{\Delta} + \varepsilon_U \frac{PD_U}{\Delta} - \varepsilon_w \frac{R + P(D_E + D_U)}{\Delta}$$

the current formula simplifies further when we assume that there are no fiscal externalities ($P = 0$) and no change in wages ($\varepsilon_w = 0$):

$$\varepsilon_R \frac{R}{\Delta}.$$

For the implementation of these formulas, we can consider different cases. First, if b_d is the same for all workers and if it increases marginally in each duration, the direct effect would be $-D_B$. We can consider an alternative case, related to our empirical implementation, in which benefits are proportionally decreasing in duration so that $b_d = \gamma_d \tilde{b}$, $0 < \gamma_d \leq 1$. Also, consider that all workers begin their spell with the same level of initial benefits. The effect of marginally increasing initial benefits would imply $\Delta = \frac{R}{\tilde{b}}$. Thus, we can rewrite the formulas above to get:

$$\tilde{\eta}_R + \tilde{\eta}_U \frac{PD_U}{R} - \tilde{\eta}_w \left(1 + \frac{P(D_E + D_U)}{R}\right) \quad (15)$$

where $\tilde{\eta}_R = \frac{\partial R}{\partial \tilde{b}} \frac{\tilde{b}}{R}$ is the elasticity of R with respect to the change in the initial level of benefits.[10]

The use of the total benefits paid is not frequently considered in the literature. Some examples are Lee et al. (2021), Campos, García-Pérez, and Reggio (2022), and Gonzalez-Rozada and Ruffo (2026). Using R and its changes allows for more precise estimations of the effects of changing UI on government budgets if transfers are heterogeneous or decreasing in duration. In such cases, considering the extension of non-employment duration is not enough because the effects could vary among groups.

3 ESTIMATING THE EFFECTS

The above equations suggest different procedures to estimate the effects of UI on a government budget. We now turn to implementing them in the UI system in Argentina.

3.1 CONTEXT, DATA AND METHODS

3.1.1 UI in Argentina

Argentina provides a good setup to apply these methods because its UI system has interesting features (heterogeneity between beneficiaries, kinks and discontinuities), and administrative data that covers all beneficiaries.

UI is provided to all workers laid off from private jobs that have at least a minimum number of contributions, with few exceptions. Potential unemployment duration depends on age and employment history of the worker, as described by Table 1. For example, workers of 35 years of age with 28 months of contributions in the previous 36 months are eligible for 8 months of UI transfers.

10 Without imposing a balanced budget the formula is $\tilde{\eta}_R + \tilde{\eta}_U \frac{PD_U}{R} - \tilde{\eta}_w \left(\frac{w\tau D_E}{R}\right)$.

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This example is consistent with the average case that our sample would consider. The conditions described in the Table suggest that there are several discontinuities to evaluate the effects of UI potential duration on government budget. This paper focuses on the effect of the level of UI, and does not exploit these discontinuities.[11]

MONTHS WITH CONTRIBUTIONS TO UI DURING THE LAST 36 MONTHS	MONTHS OF UI SUPPORT	
	IF AGE < 45	IF AGE ≥ 45
6 to 11	2	8
12 to 23	4	10
24 to 35	8	14
36 or more	12	18

Table 1 Unemployment Insurance Eligibility.

Note: Before March 2006, only workers with at least 12 months of contributions during the last 36 months were eligible. The contributions can be both continuous or discontinuous. The systems of temporary contracts and of construction sector are not considered within this paper.

The initial level of UI benefits is defined as a replacement rate of 50 percent with a maximum and a minimum level. During the period covered by our data, these levels were 300 ARS and 150 ARS for the top and bottom values of initial UI level, which changed to 400 ARS and 250 ARS from March 2006 (130 USD and 80 USD of that period). Given that the maximum level is relatively low compared to the average formal wage and even to the average pre-unemployment wage of beneficiaries, the median beneficiary would be receiving the maximum benefit level.

Figure 1 shows the initial level of UI as a function of net pre-unemployment wages. The gray solid line represents the initial level of benefits before the reform, with the minimum transfer denoted as b_L^0 and the maximum as b_H^0. The black solid line shows the same schedule for each period after the reform, with thresholds b_L^1 and b_H^1. The plot displays the kinks in the schedule of initial transfers. For example, consider the case after the reform. When the pre-unemployment wage is above k_H^1, the level of benefit is constant. Immediately below that threshold, initial benefits depend linearly on wages. Our identification strategy exploits these kinks in the schedule of initial UI transfers, restricting the analysis to high kinks.

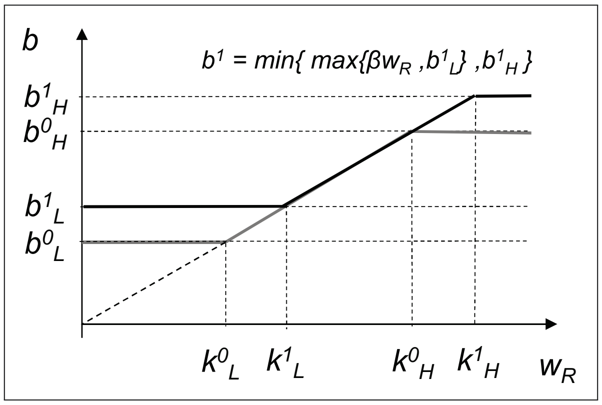

Figure 1 Schedule of initial UI transfer.

Note: The graph shows the schedule of the initial benefits, b_1, based on the pre-unemployment reference wage, w_R, up to a maximum and minimum level of transfer, b_H and b_L. The initial benefit schedule changed in March 2006, increasing the maximum and minimum levels by 100 ARS.

In Argentina, benefits are paid on a monthly basis and decline according to the duration of unemployment. In the fifth month, the transfer represents 85 percent of the initial level. At the ninth transfer, the level of benefit represents 70percent of the initial transfer.

To fix ideas, we will summarize a case that is close to the median beneficiary in the period after the reform. This would be a male worker laid off after 28 months of tenure on the job and who would be eligible for eight transfers. This beneficiary would receive the maximum level of benefits, 400 ARS, for four consecutive months, and would be eligible to receive 340 ARS from the fifth to the eighth months of unemployment.

11 See Gonzalez-Rozada, Ronconi, and Ruffo (2011) for a general description of UI system in Argentina, and Gonzalez-Rozada and Ruffo (2016b) for estimations exploiting discontinuities in potential duration.

3.1.2 Administrative data

We use administrative data to construct UI spells and to follow workers backward (in their pre-unemployment working history) and forward (capturing re-employment job). We also collect covariates (see Appendix B). Our data covers all beneficiaries of UI from 2005 through 2007, covering a period of rather stable growth.

Column (1) of Table 2 summarizes the main characteristics of the observations in the database, restricting the sample to workers with reference wage between 75 and 4800 ARS. The workers in our database are predominantly young males who earn relatively low wages compared with the average wages of formal workers. About 50 percent of the beneficiaries have a dependent spouse and the average number of dependent children is .77. On average, the workers contributed about 28 months out of the last 36. Workers are eligible for approximately 9 months of UI. Many exhaust the funds they are entitled to, and on average eligible workers collect a total transfer of 2200 ARS in 7 months, with an average of 312 ARS per month.

In columns (2) to (4) the table shows UI beneficiaries in our pre-reform sample, and divides them into three groups that consider if the initial benefits are at the minimum, between the minimum and the maximum, or at the maximum level. The reference wages used to determine these groups are reported in the first line. Columns (5) to (7) report the analogous division for the post-reform sample. The different groups defined by the level of reference wage show some differences in the predetermined variables, including age, tenure, contributions, proportion of males, and others. The average potential duration of UI also changes for these groups, increasing for those with a higher reference wage. They also differ in outcomes: non-employment duration tends to decrease with the reference wage and average UI duration tend to increase (less than UI eligibility, though).

	TOTAL	JAN.2005 TO MAR.2006			APR.2006 TO DEC.2007		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Minimum reference wage	75	75	361	722	75	602	963
Maximum reference wage	4800	361	722	3600	602	963	4800
Reference wage	1186.45	306.70	596.82	1115.04	494.32	811.35	1525.36
UI initial benefit	338.84	150.10	244.13	299.60	250.13	332.99	399.88
UI potential duration	9.45	9.09	9.43	10.56	8.66	9.08	10.29
Age	35.43	34.54	34.35	36.02	34.68	34.48	35.37
Male	0.69	0.49	0.65	0.75	0.45	0.60	0.73
Dependent spouse	0.51	0.45	0.49	0.56	0.44	0.48	0.55
Dependent children	0.77	0.78	0.85	0.88	0.67	0.73	0.80
Tenure in pre-un. job	38.86	35.28	38.05	46.96	32.99	35.45	45.57
Number of contrib.	28.09	27.14	28.17	30.44	26.33	27.47	30.15
Non-emp. dur.	21.13	31.54	27.04	23.12	24.20	21.48	18.70
UI duration	7.09	7.06	7.20	7.54	7.14	7.22	7.55
Total UI transfers	2213.38	1194.80	1690.87	2178.32	1785.66	2192.74	2663.79
Num.of obs	114914	2687	16565	25369	9406	17792	70662

Table 2 Descriptive statistics by period and range of reference wage.

Notes: The table divides the sample according to the initial period of UI and the reference wage. The reported reference wage is the highest gross wage of the last six. Bottom kink is at 361 ARS before March 2006 and 602 afterwards; top kink is at 722 ARS before March 2006 and 963 afterwards. Columns (2) and (5) include workers below the bottom kink; columns (4) and (7) workers above the top kink; columns (3) and (6) include workers in between the kinks. The schedule of initial benefits depends on net reference wage, while we observe gross reference wage. Number of contributions reported are the average number of contributions in the last 36 months. Tenure and durations are in months; monetary variables are in ARS.

We study the effect of the level of benefits exploiting the high kinks and define for that purpose a sample that consists of workers with a reference wage higher than the bottom kink plus 5 percent. In all cases we restrict the analysis to workers displaced from work with more than 12 contributions in the 36 months prior to unemployment.[12] In our results, we apply Regression Kink Design (RKD) by using observations of all periods available, and, thus, considering periods both before and after the reform.

12 We do this to avoid the possible change in eligibility criteria in 2006 (see Table 1).

3.1.3 Methods

We now turn to show how we exploit the kink in the determination of the initial level of benefits to identify the effect of UI on several outcomes. Figure 1 shows the initial level of benefits, considering the periods both before and after the 2006 reform.

Consider the solid black line in Figure 1. After the reform, an unemployed worker initially receives a monthly benefit of b_L^1 if their reference wage, W_R, is such that half this wage is less than b_L^1. However, if half of their reference wage is greater than b_L^1, then their benefit is a linear function of pre-unemployment wage until a maximum threshold of b_H^1. When half of their reference wage is greater than b_H^1, the benefit received remains constant at b_H^1. The UI formula after the reform as a function of previous earnings therefore has two kink points, at k_L^1 and k_H^1.

We use these kink points in the schedule of UI transfers to identify the causal effects of the level of benefits on outcome variables. We concentrate on the high kinks. There are several reasons for this. First, the high kinks are closer to the median of the reference wage distribution, implying that they affect many workers and that there are more observations for any local estimation. Second, the high kinks affect the benefits paid during all the UI spells. On the contrary, the low kinks k_L^t would affect only the first four months of benefits; after that duration, the fall in benefits would become binding and workers at both sides of the kink would have the same benefit level. For these reasons, we focus our estimations on the high kinks and we continue the description of our method concentrating on this case.

The basic idea for identification is to look for an induced kink in the mapping between the assignment variable and the outcome variable that coincides with the kink in the policy rule, and to compare the relative magnitude of the two kinks. This is called a regression kink design (RKD).

Consider the following constant-effect additive model,

$$Y = \alpha b + g(W_R) + \epsilon. \tag{16}$$

In our setting, Y is the outcome of interest (total benefits paid, re-employment wages), b is the initial UI transfer, W_R is the running variable (pre-unemployment wage), and α is the parameter of interest. It shows the effect of the benefit transfer on the outcome variable. Since UI benefit is a piecewise linear function of pre-unemployment wage, there is no variation in b conditional on W and the model is not identified in (16). Nevertheless, it may be possible to exploit the kink in the benefit rule to identify the causal effect of b on Y. The idea is that if b exerts a causal effect on Y, and there is a kink in the deterministic relation between b and W_R at the kink point, k, then we should expect to see an induced kink in the relationship between Y and W_R at k.

Nielsen, Sørensen, and Taber (2010) made precise the assumptions needed to identify the causal effects in (16). They showed that if $g(W_R)$ and $E[\epsilon|W_R = k]$ have derivatives that are continuous in W_R at the kink point, then

$$\alpha = \frac{\lim_{v_0 \to 0^+} \frac{dE(Y|V=v)}{dv}|_{v=v_0} - \lim_{v_0 \to 0^-} \frac{dE(Y|V=v)}{dv}|_{v=v_0}}{\lim_{v_0 \to 0^+} b(v_0) - \lim_{v_0 \to 0^-} b(v_0)}, \tag{17}$$

where $V = w - k$ is to normalize the kink point around zero. The expression on the right-hand side of equation (17) is simply the change in slope of the conditional expectation function $E[Y|V = v]$ at the kink point ($v = 0$), divided by the change in the slope of the deterministic assignment function $b(\cdot)$ at zero. Equation (17) is referred to in the literature as the Sharp RKD estimand.

However, when the policy rule of interest depends on unobserved individual characteristics or is implemented with error then the error ϵ can be correlated with W_R and equation (17) can give a non-consistent estimation. Card et al. (2012) show that to get a consistent estimation of the treatment effect on the outcome variable, one replaces the known change in slope of the assignment rule at the kink with an estimate based on the observed data:

$$\alpha = \frac{\lim_{v_0 \to 0^+} \frac{dE(Y|V=v)}{dv}|_{v=v_0} - \lim_{v_0 \to 0^-} \frac{dE(Y|V=v)}{dv}|_{v=v_0}}{\lim_{v_0 \to 0^+} \frac{dE(b|V=v)}{dv}|_{v=v_0} - \lim_{v_0 \to 0^-} \frac{dE(b|V=v)}{dv}|_{v=v_0}}. \tag{18}$$

They called this a "fuzzy regression kink design". Dong (2011) shows that the fuzzy RKD estimate can be recovered from an instrumental variable (IV) procedure.

3.2 EMPIRICAL STRATEGY

We use this general equation that relates the output variable with the running variable:

$$E[Y|W_R = w] = \mu + \gamma_1 (w - k) + \nu_1 (w - k) D + X'\beta, \quad (19)$$

for a bandwidth $|w - k| \leq h$ around the kink point, and where $D = 1\{w \geq k\}$ is an indicator variable for those observations above the kink point, X is a vector of controls,[13] μ, γ_1, ν_1 and β are the parameters of the model. We are interested in the parameter ν_1, which captures the change in the slope of the relationship between the outcome variable and the running variable at the kink point. The sharp RKD estimated effect would then be $\hat{\alpha} = \frac{\hat{\nu}_1}{\pi_1}$, where $\hat{\nu}_1$ is an OLS estimation of the previous equation, and π_1 is the deterministic change in slope of b at the kink. If necessary, an elasticity could be computed as

$$\eta_{Y,b} = \hat{\alpha}\frac{b_H^1}{\bar{Y}} \quad (20)$$

where $\bar{Y}$ is the mean of the outcome variable in the sample.

If the schedule for b is not deterministic, a fuzzy RKD has to be implemented. For this case we instrument b with $(w - k) \times D$, and the IV estimate follows from the following reduced form and first stage equations:

$$E[Y|W_R = w] = \mu + \gamma_1 (w - k) + \nu_1 (w - k) D + X'\beta \quad (21)$$

$$E[b|W_R = w] = \lambda_0 + \phi_1 (w - k) + \pi_1 (w - k) \times D + X'\theta$$

for a bandwidth $|w - k| \leq h$, and where the fuzzy RKD effect would be equivalent to $\hat{\alpha}^{RKD} = \frac{\hat{\nu}_1}{\hat{\pi}_1}$.

In both equations above it is assumed a linear effect of the running variable W_R. In our results, we prefer the Fan and Gijbels (1996) (FG) selection algorithm for the computation of the bandwidth. This method provides a wider bandwidth compared with the alternatives considered and provides more stable results. Our choice is in line with the results in Card et al. (2012). They show that the FG bandwidth provides the lowest root mean square error in a Monte Carlo exercise.

Several assumptions allow for the identification of the effect of b by equation (17). The first assumption states that the treatment variable, b, changes deterministically according to the rule stated above. This assumption can be tested by simple observation of the corresponding function in the data. It can also be lifted, as discussed above, by the fuzzy method, but in this case the relevance of the rule should be tested. The second assumption relates to continuity. In particular, it is assumed that (i) the effect of the assignment variable on the outcome is smooth, (ii) any other determinant of the outcome change smoothly close to the kink, and (iii) the marginal effect of the treatment variable on the outcome (if heterogeneous) is continuous close to the kink. The third assumption is that there is no manipulation of the assignment variable or the treatment variable. In Appendix A, we provide important evidence about these three assumptions.

4 EMPIRICAL RESULTS: EFFECTS OF UI

4.1 RKD ESTIMATES

Table 3 reports the result of implementing the RKD method on benefits duration, non-employment duration, total UI paid, and reemployment wages. We also include the log of non-employment

13 In our application, controls include age, sex, dependent spouse, number of dependent children, tenure in the last job, severance pay received (which is not observed in the data, but imputed through tenure, wages, and regulations), the number of contributions, and a set of fixed effects including eligibility for potential duration, as well as region, industry, year, and month fixed effects. For wages, we add a set of dummies for non-employment duration (less than 5 months, 5 to 8, etc.). These are intended to capture any difference in the level of re-employment wage due to different unemployment duration.

duration and the log of reemployment wages. In this case, the implementation is based on a linear specification of the policy variable (initial UI level) the assignment variable (pre-unemployment wage), and a sample defined by a bandwidth determined by FG. This bandwidth is sufficiently wide to provide a large sample and a relatively precise estimate.

	(1) BENEFITS DURATION	(2) NON-EMPLOYMENT DURATION	(3) NON-EMPLOYMENT DURATION (IN LOGS)	(4) TOTAL UI PAID	(5) WAGES	(6) WAGES (IN LOGS)
Coef	0.0009***	0.0116***	0.0004***	5.92***	0.2771**	0.0008***
	(.0003)	(.0008)	(.0001)	(0.1544)	(0.1233)	(.0002)
Mean of Y	7.47	20.98	2.69	2395.46	1350.5	6.96
Elasticity	0.05	0.22	0.16	0.99	0.08	0.30
Bandwidth	1579.6	1016.0	1216.8	1040.8	878.8	1343.0
N	92856	86636	86337	87490	45914	54149

Table 3 Effects of a marginal increase in initial UI level - RKD estimates.

Note: The coefficients are the result of implementing IV methods to estimate the effect of benefit level. It reports fuzzy RKD estimates as in equation (21) using all available periods. Controls include age, age squared, gender, number of children, presence of spouse, pre-unemployment tenure and its square, number of pre-unemployment contributions, imputed severance pay level, and identification variables for the eligibility UI duration, region, industry of pre-unemployment job, year, and month; for the log of re-employment wages, controls include a set of dummies identifying groups of observations by four months of non-employment duration (less than 5, 5 to 8, etc.). Robust standard errors for the estimates are in parentheses. Statistical significance:
* significant at the 10% level;
** significant at the 5% level;
*** significant at the 1% level.

First, the table reports a very small change in benefit duration. The value of the coefficient suggests that the duration of benefits would be extended in about one day if the level of benefits were increased by 40 ARS (by 10 percent). Considering that the average benefit duration of the particular sample used is 7.5, the elasticity is 0.05.

Columns (2) and (3) of the table deal with different estimates of the effect of UI on non-employment duration. The first case, in which this duration is linear, is associated with a relatively precise estimate of the elasticity of 0.22. When duration is logged, the elasticity falls to 0.16.[14]

These estimates on the effects on duration provide evidence of a very low effect on behavior (on job-finding rate) of the increase in the initial level of UI. Another piece of evidence in the same direction is the estimate related to the total UI paid. The value of the coefficient suggests that each additional monetary unit of initial UI implies almost 6 ARS of increase in total UI paid. Given that the average potential duration of UI is longer than 7 months, this value shows that changes in behavior are very small, implying that most of the increase of UI is a direct cost with no additional distortionary effect. To convert this estimate into an elasticity, we use the average UI paid within the spell, which is about 2400 ARS, to approximate an elasticity to one. This estimate provides evidence of both the direct and indirect cost of one additional monetary unit on the government's budget. Thus, an elasticity of one is comparable to the above-mentioned very low elasticities of benefit duration or censored non-employment duration.

On the other hand, UI level has a significant positive effect on wages. Column (5) shows that reemployment wages would increase by 0.28 cents for an additional 1 ARS of initial UI, which implies an elasticity of 0.08.[15] Column (6) shows the result of using log wages. The very significant coefficient implies that wages would rise about 0.1 percent per one additional ARS of initial UI, implying an elasticity of 0.3.

These results show that UI level has a small effect on finding rate, a small behavioral effect on duration but a sizeable impact on wages. Considering the linear specification, the elasticity of benefits duration of 0.05 is lower than the median in Schmieder and von Wachter (2016) of 0.3, which considers developed countries, and also lower than the one in Liepmann and Pignatti (2024), of 0.27, for Mauritius. Our elasticity of non-employment duration, considering only formal jobs, is 0.22. In comparison, the corresponding elasticity in developed countries is around 0.57 (Schmieder and von Wachter 2016), while in Liepmann and Pignatti (2024) it is 0.28. Compared to

14 In these tables we use non-employment duration censored at 24 months, to avoid the sensitivity to long durations. If we censored duration at 8 months, the elasticity would be even lower, with a precisely estimated but insignificant elasticity of .01. This last estimate would be exactly the same as the effect on benefit duration if all potential UI durations were set to 8 months.

15 For this result, we avoid the lowest and highest 5 percent of reemployment wages, in order to avoid sensitivity to outliers. When we use log wages, we do not trim the sample.

	(1) LOG OF BENEFITS DURATION	(2) LOG OF NON-EMP. DURATION	(3) LOG OF TOTAL UI PAID	(4) LOG OF WAGES
Coef	0.2241***	0.1588***	1.2693***	0.1412*
	(0.0250)	(.0191)	(0.0374)	(0.0750)
Mean of *Y*	1.7033	2.6894	7.5267	6.9682
Elasticity	0.22	0.16	1.27	0.14
Bandwidth	1.0357	1.7765	0.5210	1.0328
N	95053	94242	78703	56597

Table 4 Effects of a marginal increase in initial UI level - RKD estimates - Running variable and policy variable in logs.

Note: The coefficients are the result of implementing IV methods to estimate the effect of benefit level. It reports fuzzy RKD estimates as in equation (21) using all available periods. Controls include age, age squared, gender, number of children, presence of spouse, pre-unemployment tenure and its square, number of pre-unemployment contributions, imputed severance pay level, and identification variables for the eligibility UI duration, region, industry of pre-unemployment job, year, and month. Robust standard errors for the estimates are in parentheses. Statistical significance:
* significant at the 10% level;
** significant at the 5% level;
*** significant at the 1% level.

this last paper, UI duration in Argentina is much shorter and heterogeneous (a median of 8 months compared to 12 months), and the evaluated level of benefits is also much lower (the upper bound over average pre-unemployment wage of beneficiaries was about 25 percent in our case and 125 percent in Mauritius).[16] This partially explains our low behavioral effects. Nekoei and Weber (2017) found a positive and significant effect on reemployment wages, quantitatively similar to ours, and they argue that low behavioral responses in non-employment duration is related to more positive wage effects. Our results are in line with that observation.

Table 4 reports the results of implementing RKD using the log of the policy variable (log of initial UI level) and the log of the assignment variable (log of pre-unemployment wages). Column (1) reports the effects of UI on the log of benefit duration. The coefficient is a significant 0.2, and can be directly interpreted as an elasticity. Column (2) reports the coefficient when the outcome variable is the log of non-employment duration. The implied elasticity is a significant 0.16.[17] Column (4) shows a wage elasticity of 0.14, which is marginally significant. These results confirm that behavioral effects are significant but modest compared to the literature, and that the effects on wages could be relevant.

	(1) BENEFITS DURATION	(2) BENEFITS DURATION	(3) NON-EMPLOYMENT DURATION	(4) NON-EMPLOYMENT DURATION
		log. run. var.		log. run. var.
Coef	0.0002***	0.2408***	0.0001***	0.3831***
	(0.0000)	(0.0193)	(0.0001)	(0.0238)
Effect	−0.0005***	−0.2408***	−0.0002***	−0.3831***
	(0.0001)	(0.0193)	(0.0001)	(0.0238)
Elasticity	−0.20	−0.21	−0.09	−0.32
Bandwidth	831.59	6.72	1187.93	7.08
N	83101	85663	85663	85663

Table 5 Effects of a marginal increase in initial UI level - RKD estimates using Proportional Hazard models.

Note: The table reports coefficients and sharp RKD effects. The coefficients are the result of implementing Proportional Hazard models with exponential distribution. Controls include age, age squared, gender, number of children, presence of spouse, pre-unemployment tenure and its square, identification variables on the number of pre-unemployment contributions, imputed severance pay level, year and month fixed effects. Robust standard errors for the estimates are in parentheses. Statistical significance:
* significant at the 10% level;
** significant at the 5% level;
*** significant at the 1% level.

Up to now we have used linear models and applied a fuzzy RKD to estimate effects on duration. We now consider proportional hazard models. Table 5 shows results from a proportional hazard model assuming exponential distribution, where we interpret the results as in a sharp RKD. The first column applies the proportional hazard model to benefits duration, and finds that the elasticity is −0.2. When the policy and the assignment variables are measured in logs, the elasticity is almost identical. Both elasticities are similar to the one estimated in the previous

16 Ndiaye et al. (2025), applying a very different method based on a survey in Senegal, found even lower elasticities that the ones we report. See also Gerard and Gonzaga (2021) and Sehnbruch, Navarrete, and Guajardo (2022) for additional empirical evidence in developing countries.

17 When censoring duration to the first eight months, the elasticity falls to the point estimate of 0.05, which is precisely estimated, but insignificant.

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table (using logged variables). Column (3) reports the results of a proportional hazard model applied to non-employment duration. Given that these models deal with censoring, we report results without imposing censoring at a particular period (all censored observations are those that are not in a job in December 2009, our last period in the sample). Results are consistent with an elasticity of –0.09. When we use a logged policy variable, we find a stronger effect, an elasticity of –0.32.

4.2 THE EFFECT OF UI ON THE GOVERNMENT'S BUDGET

We turn now to provide different approximations to the effects of the UI level on the government's budget, using selected point estimates as inputs of evaluating the formulas in Section 2.

Table 6 reports the evaluation of these formulas. The first panel uses the elasticities provided by RKD. From all estimates, we select the results that identify the effects through the linear relationship between the outcome variable and the policy variable, discarding the ones with the logged variables. We also use the results from the proportional hazard models when calibrating the responses of transition rates.

The table also reports the estimated parameters of the formula, such as average values of durations or transition rates, as well as P, b, τ_P, and τ. We set $P = 300$, which is close to the value of the basic pension, we set $b = 400$, and we set taxes τ_P to roughly satisfy the budget constraint, which gives a tax rate of 40 percent, a reasonable value, in line with the average tax

	(A)	(B)	(C)	(D)
Elasticities with respect to benefits				
D_B	0.05		0.05	
D_U	0.22		0.22	0.22
ϕ_B		–0.20		
ϕ_U		–0.09		
R				0.99
w	0.08	0.08	0.08	0.08
Average level of variables				
D_B	7.48		7.48	
D_U	19.26		19.26	19.26
D_E	45.00			45.00
T			64.26	
ϕ_B		0.13		
ϕ_U		0.05		
δ		0.02		
w	1200	1200		1200
P	300	300	300	300
b	400	400	400	400
R				2586
Evaluation of the formula				
Formula	0.87	1.06	1.14	0.79
No wage change	1.48	1.36	1.75	1.48
No fiscal externality	0.97	1.12	1.06	0.91
No wage ch. or ext.	1.05	1.20	1.14	0.99

Table 6 Effect of UI on the government's budget.

Note: The table evaluates the effects of UI on government's budget in panel *III* using elasticities of panel *I* and other parameters of panel *II*. Column (A) evaluates equation (5), column (B) equation (8), column (C) equation (13), and column (D) equation (15). Line *i* of panel *III* uses the main formula, line *ii* assumes no wage change ($\eta_W = 0$), line *iii* assumes no fiscal externality ($P = 0$), and line *iv* assumes neither wage change nor externality.

rate.[18] When we set $P = 0$ and abstract from fiscal externality, the tax rate becomes 3 percent. We set average wages to the average pre-unemployment wages of the beneficiaries in our sample. For calibrating durations we use the average time spent in the state for a comparable population, as reported in the tables,[19] and we compute transition rates as the inverse of the average duration. Finally, we assume a duration in employment of 45 months, roughly in line with the observed tenure in the last job, and implying a layoff rate of 2.2 percent.

With these estimates and parameters, we calibrate the formulas presented above. In particular, we base on equations (5), (8), (13), and (15). The first line shows the formula including wage and fiscal externalities. We find that the total effect on budget is lower than the direct effect for columns (A) and (D), implying that the wage increases compensate for the already relatively low behavioral response. In columns (B) and (C), the values higher than one imply a stronger behavioral effect.

The second line dispenses from the wage externality, setting that elasticity to zero. These are the largest estimated costs, because they set the possible positive externalities (the increase in wages)to zero and the negative fiscal externalities to the highest level. We find that total costs are 48 percent higher than direct costs in the case of column (A) and column (D). Column (C) shows the highest costs because, in that formula, the effect on job finding rate affects not only the number of benefits paid (through η_B) but also the time paying taxes (through η_U), and the latter effect becomes even more important with a relevant fiscal externality. Column (B) shows the lowest costs, because, in this column, the elasticity of job-finding rate is relatively low compared to other estimates.

The third line dismisses fiscal externalities and incorporates wage externality. In formulas for columns (A), (B), and (D), the absence of fiscal externality makes the elasticity of non-employment duration irrelevant: when the duration of employment is fixed, the effect of non-employment duration is only important through fiscal externalities. Through this effect, costs should be lower than in the baseline formula, but they are not. This is because fiscal externality also increases the role of wage effects: a higher fiscal externality is also a higher tax rate and a larger effect of a change in reemployment wages. Column (C) includes a case in which non-employment duration reduces time in employment and is thus important even without fiscal externalities. But fiscal externality amplifies the importance of non-employment duration, which generates the effect that, in the absence of fiscal externality, costs fall moderately. In all, effects are very similar to the baseline, with total effect on budget ranging from 0.9 to 1.12.

The last line evaluates the simplest formulas, which eliminates all externalities. In these cases, the total effect of the increase in UI is between 1.05 when we use duration, and 1.20 when we use the results of the proportional hazard models. In both, only benefits duration is important. The effect on budget is 1.14 in the case of the actuarially fair formula. Finally, in column (D), where we use total UI paid as the sole indicator of the effect on government's budget, the effect is 0.99.

5 MARGINAL VALUE OF PUBLIC FUNDS

The MVPF compares the fiscal costs of the transfer to its welfare gains. In general, the welfare gains are measured through willingness to pay. In the case of UI transfers, the willingness to pay for one additional unit is measured through the consumption drop upon job loss (Landais and Spinnewijn 2021; Hendren and Sprung-Keyser 2020). We then turn to the estimation of the effects of job loss on income and on consumption.

18 The average difference between labor costs and net wages is approximately 40 percent for formal workers. We use basic pensions of 2005 to approximate P since this amount gives a reasonable approximation for this level of taxes, but also because after that year a noncontributory system was implemented in practice (Gonzalez-Rozada and Ruffo 2016a). This means that even without any contribution, workers are entitled to the basic pension after reaching the retirement age.

19 For example, in the line "Mean of Y" in Table 3.

5.1 INCOME LOSSES

To measure income losses after displacement we analyze household survey data, exploiting the longitudinal dimension of the Permanent Household Survey, from 2004 to 2013 (INDEC 2013). This source covers urban population, and is composed of rotating panels that follow the same household for more than a year. Households are interviewed for two consecutive quarters, are not in the sample for the next two quarters, and then are interviewed for two more consecutive quarters. This implies that when we follow an individual, we would have at most four quarters of observations, and also that we can observe the same individual five calendar quarters after being initially observed.

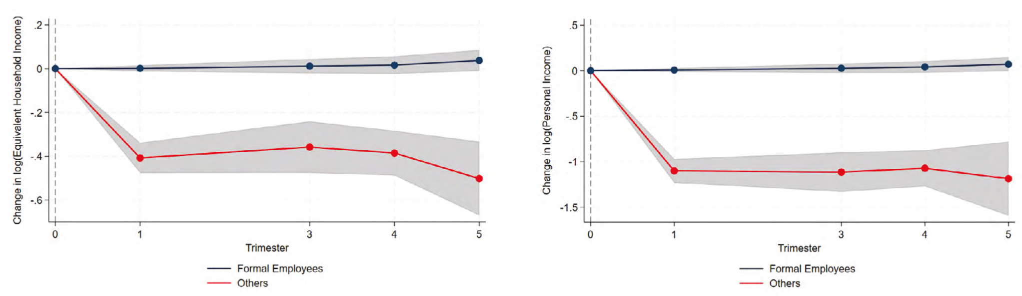
We identify heads of households between 21 to 60 years of age with formal jobs, that are eligible for UI (they have been in a formal job for at least six months, excluding public administration, construction sector and domestic work), and we follow them in time. The sample includes workers that begin as formally employed and that can continue in the same job, that become unemployed (both covered or uncovered), and who are out of the labor force or with an informal job. We do not include a formal re-employment in the sample. For each formal job loss, we follow the worker through the eligibility period, according to the rules described in Table 1.

We then analyze changes in log income by using the following regression equation:

$$y_{it} = \alpha_i + \beta_{e,s}\mathbf{1}\{\text{period} = s \times \text{status}_i = e\} + \mu_t + \epsilon_{it}, \tag{22}$$

where y_{it} is the log of income corresponding to the i head of household, in quarter t, α_i stands for the fixed effect by individual, μ_t are common fixed effects by calendar time, and $\beta_{e,s}$ are coefficients for employment status e and period s, in which the base group are the continuers (those that keep the same formal job). In this case, the period is defined for those laid off as the number of quarters since the last time the worker was observed as being formally employed. Additionally, the same variable is the number of quarters since the first quarter as when formally employed for those who do not lose their jobs. This allows for the comparison between workers who are not displaced and for those who are. We estimate the same equation for two income definitions: the adult equivalent household income, and the total personal income. Additionally, we estimate the equation with different definitions of worker status.

Figure 2 displays the estimated coefficients and their 95 percent confidence intervals for formal jobs (dashed blue lines), and those separated in quarters after job loss (red lines). This groups all cases, including unemployed, those with an informal job, and others. Households income drops by about 0.40 log points (33 percent) the first quarter after layoff. They then recover mildly to around 0.36 log points (a reduction of 30 percent). Panel (b) of the figure plots the same results when using personal income. We find a reduction of 67 percent the first quarter and even a year after job loss.



(a) Household income

(b) Personal income

Figure 2 Income Losses After Formal Job Loss.

Note: The figure plots the point estimates and 95% confidence intervals of β coefficients that are the result of implementing regression equation (22) to the panel of head of households in prime age in the Permanent Household Survey. The red lines represent the income drop in log points, comparing to the income when the head of household had a formal job. The difference between the red and the blue lines represents the income loss from displacement.

These results are comparable to the ones presented in Liepmann and Pignatti (2024). They find that labor income decreases by 60 percent in the first year after job loss.

These estimates are the result of the composition of workers in each quarter. In other words, they are the average of all cases, some of which are more mildly affected. For example, those employed

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informally see their household income reduced by about 20 percent, those covered unemployed (UI beneficiaries) face a reduction in household income as large as 70 percent.

Table 7 shows the same result after using the interaction that identifies workers with informal jobs, self-employed, UI-covered unemployed, unemployed with social assistance, unemployed without transfers, and out of the labor force. Column (1) reports the interaction of the dummy variables per status with the dummy that takes a value of one after displacement. In this case, income drops by 70 percent for those unemployed, by 55 percent in the case of workers out of the labor force, by 21 percent for those with an informal income and by 21 percent for those self-employed. Column (2) performs the same estimation but it further decomposes the unemployed workers. Those unemployed who are UI beneficiaries are affected by a 51 percent reduction in household income, while those who receive other types of public assistance face a reduction of 32 percent of their income. Column (3) groups all statuses different than formally employed in one dummy to find that those displaced are affected, on average, by a 33 percent drop in income.

Columns (4) and (5) show the result of the same regressions in columns (1) and (3) but in a cross-section, i.e. without individual fixed effects. In this cross-section regression we find a difference of 37 percent in household income comparing formal workers with all other employment status, and we find a difference of 35 percent if we add controls for demographic characteristics. Interestingly, the lack of fixed effects or even of controls offer comparable results, which suggests that we can still use cross-section evidence to approximate income losses upon unemployment.

From these results it seems that informal wages and income from self-employment are an effective way to smooth consumption, possibly more effective than UI. It is important to note that both sources of income could be combined, because informal income is unobservable for the government. In fact, about half of workers collecting UI benefits are also employed as informal workers or as self-employed. Notably, labor income from these sources almost double the UI transfer (see Table 8). In all, while these sources of income seem to reduce the liquidity relevance of UI, the household income drop that we observe for UI beneficiaries is still very strong.

Table 7 Household income and consumption differences.

Note: All results include fixed effects by quarter. Controls refer to demographics, including fixed effects by gender, age groups, educational level, marital status, and region. Columns (8) and (9) restricts the sample to unemployed workers with labor income in the previous six months. Column (10) reweights all observations to represent the same proportion of workers that are separated from a formal job after three quarters. Robust standard errors in parentheses.
Statistical significance:
* significant at the 10% level;
** significant at the 5% level;
*** significant at the 1% level.

	HOUSEHOLD INCOME						HOUSEHOLD CONSUMPTION			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unemployed	−1.203*** (0.137)	−1.364*** (0.163)		−1.536*** (0.169)			−0.442*** (0.0224)		−0.440*** (0.0387)	
Unemp. w/UI		−0.721*** (0.210)		−0.825*** (0.180)					−0.160 (0.0996)	
Unemp. w/assist.		−0.389 (0.539)		−0.667 (0.575)						
Inactive	−1.004*** (0.120)	−1.003*** (0.120)		−0.947*** (0.108)			−0.270*** (0.0157)			
Informal	−0.232*** (0.0309)	−0.230*** (0.0307)		−0.394*** (0.0334)			−0.265*** (0.0103)			
Self-employed	−0.232*** (0.0303)	−0.179*** (0.0347)		−0.115*** (0.0336)			−0.0776*** (0.00988)			
All other			−0.404*** (0.0293)		−0.460*** (0.0272)	−0.434*** (0.0261)		−0.418*** (0.0367)		−0.229*** (0.00818)
Individual f.e.	YES	YES	YES	NO	NO	NO	NO	NO	NO	NO
Controls						YES	YES	YES	YES	YES
R^2	0.113	0.118	0.090	0.480	0.471	0.569	0.360	0.403	0.403	0.351
Obs	146,845	144,201	144,201	144,201	144,201	144,201	51,682	21,205	21,205	51,682
Individuals	52,259	52,259	52,259							

VARIABLE	UI + NON-EMPLOYMENT	UI + EMPLOYMENT	TOTAL
% of Sample	51.4%	48.6%	100%
Average Unemployment Income	434.16	436.94	435.51
Average Labor Income	0	748.05	362.11
Average Other Income	377.57	196.14	289.40
Average Total Family Income	1876.64	2549.79	2203.79

Table 8 Income Descriptive Statistics. (Weighted).

This seems similar to the results in Liepmann and Pignatti (2024). They find that those that are quickly re-employed informally are those who suffer a high income drop, due to liquidity constraints, and that even adding wages and UI transfers, their income drops by about 35 percent in the third quarter after job loss.

5.2 CONSUMPTION LOSSES

After observing a substantial household income loss, we now intend to estimate the consumption losses upon layoff. For that purpose, we exploit the cross-section of the Household Expenditure Survey (INDEC 2006). Importantly, this survey includes information about current and past jobs so that we can approximate a similar sample compared to the one used above. We use the 2004–2005 survey. We restrict the sample to household heads within 21 and 60 years of age.

First we consider the difference in log per capita consumption of formally employed compared to the rest of employment states, including unemployed, informal workers, self-employed workers, and out of the labor force. In particular, consider the following regression equation:

$$y_i = \alpha + \beta_e \mathbf{1}\{\text{status}_i = e\} + X_i\gamma + u_i \tag{23}$$

where y is the log of the equivalent adult household expenditure on non-durable goods, X is a vector of controls (including a set of dummies by gender, age groups, educational level, marital status, and region), and where β_e is a set of coefficients of interest that relate to the employment status of the head of household.

In all cases, we compare the formally employed head of households with other employment statuses. We first consider the following groups: informally employed, self-employed, unemployed, and out of the labor force.

Columns (7) to (10) of Table 7 show the results of estimating the above regression in the cross-section of household heads. Column (7) shows the β_e coefficients, omitting the formally employed status. Being unemployed or out of the labor force is associated with 36 percent and 23 percent lower consumption levels. At the same time, consumption is 24 percent and 7 percent lower for informally employed or self-employed, respectively.

Columns (8) and (9) restrict the sample to workers that are unemployed but received labor income during the last six months (versus the formally employed). Consumption is 34 percent lower for these workers, while the consumption of an unemployed that received UI or other public transfers during the last six months is 15 percent lower than for a formally employed worker.

Finally, column (10) uses the same sample of column (7), but considers one dummy for all the other employment statuses different to a formal job. Additionally, in this case observations are re-weighted to represent the proportion of workers by status using the first nine months after being separated from formal employment, computing these proportions from the Permanent Household Survey data (INDEC 2013). The difference in consumption, in this case, is 20 percent.

Our results show that our measures of consumption loss at unemployment is consistent with a context with liquidity-constrained households. Estimates for other developing countries also suggest a larger consumption loss: Liepmann and Pignatti (2024) estimate a consumption loss of 28 percent. These estimations are in the upper end of the estimates for the US, that range from

6 percent to 27 percent (Schmieder and von Wachter 2016). The most common point estimate for the US is 8 percent (Hendren and Sprung-Keyser 2020) lower than our estimation.

5.3 MVPF

Armed with these alternative estimates, we evaluate possible welfare implications of a marginal increase in UI.

In particular, following Hendren and Sprung-Keyser (2020), we use $MVPF = \frac{1+\gamma\frac{\Delta c}{c}}{1+F}$, where γ is the risk aversion coefficient, $\frac{\Delta c}{c}$ is the relative consumption difference between employed and UI beneficiaries and F is the indirect fiscal cost. We use a relative risk aversion coefficient of 2.

We use four alternative measures of willingness to pay, including changes in household income after separation from a formal job, household income losses that consider only households with workers receiving UI, consumption losses after separation, and consumption losses for head of households receiving UI. Panel I of Table 9 reports these alternative measures of income and consumption drops upon separation. These come from selected values in Table 7.

Panel II of Table 9 reproduces the total costs estimates from the last lines of Table 6. These are alternative values for the denominator of the MVPF. Finally, panel III reports the alternative measures of MVPF.

I. INCOME AND CONSUMPTION DIFFERENCES				
(1) Household income losses	0.332			
(2) Household income losses covered UI	0.514			
(3) Consumption losses	0.343			
(4) Consumption losses covered UI	0.148			
II. FISCAL COSTS, TOTAL OVER DIRECT	**(A) DURATION**	**(B) TRANSITION RATES**	**(C) ACTUARIALLY FAIR**	**(D) UI PAID**
(a) Basic without externalities	1.05	1.20	1.14	0.99
(b) Including fiscal externality only	1.48	1.36	1.75	1.48
(c) Including wage externality only	0.97	1.12	1.06	0.91
(d) Including fiscal & wage externalities	0.87	1.06	1.14	0.79
III. MVPF				
Using (1) and (a)	1.59	1.39	1.45	1.68
Using (2) and (b)	1.37	1.49	1.16	1.37
Using (3) and (c)	1.74	1.51	1.59	1.86
Using (4) and (d)	1.50	1.23	1.14	1.64

Table 9 Marginal Value of Public Funds (MVPF).

Note: The table evaluates the effects of UI as $MVPF = \frac{1+\gamma\frac{\Delta c}{c}}{1+F}$. Panel *I* provide different estimates of proportional differences in income or consumption $\left(\frac{\Delta c}{c}\right)$ using selected results from Table 7. Panel *II* recovers selected estimates from the effects of UI on the government's budget (Panel *III* of Table 6, as estimates of $1 + F$). Panel *III* provides estimates of the MVPF using different estimates of consumption or income losses, assuming $\gamma = 2$.

Consider the first line, which uses household income losses to approximate willingness to pay and costs without externalities. This line shows that the social value of UI is relatively high, ranging from 1.39 to 1.68 of workers' welfare per monetary unit of government spending. This means that each additional monetary unit invested in UI is valued more than one monetary unit, suggesting that the level of UI should increase. These results are well above the median MVPF calculated in Hendren and Sprung-Keyser (2020) for UI, or other interventions, and even larger than job training programs.

The second line uses household income losses of UI-covered workers and costs that include the fiscal externality. The values now range from 1.16 to 1.50. The third line uses estimates of per capita consumption losses while the costs include wage externalities only. In these cases, estimates of MVPF are the highest among the alternatives that we compute, ranging from 1.51 to 1.86.

Finally, the last line estimates the willingness to pay using the per capita consumption losses when the head of household is receiving UI transfers, and the denominator of the MVPF includes wage changes and fiscal externalities. The results in this line, our preferred estimate of MVPF, are the ones with the largest variation between cases, ranging from 1.14 to 1.64.

While the differences between our own estimates of MVPF are substantial, they are all above one, implying that the welfare gains, measured through the willingness to pay of one additional monetary unit, are higher than government costs generated by that additional expenditure. This suggests that UI should be increased. Our estimates seem high when compared to other estimates of MVPF that evaluate the welfare effects of the level of UI. For example, estimates for the US range from 0.68 to 0.84 (Hendren and Sprung-Keyser 2020).[20] At the same time, our results are comparable to those in Liepmann and Pignatti (2024), that study the UI of Mauritius, a developing country with high informality, and that report results that are consistent with an MVPF of 1.23.[21]

In all, our preferred estimate, the last line in Table 9, consistently shows the positive welfare effects of increasing benefits, even when using very different ways to measure the costs of UI, both using linear duration models, proportional hazard models or the effect of UI level on the amount of UI paid.

6 CONCLUSIONS

In this paper, we provide different estimates of the social costs of the level of UI transfers in Argentina. We find that results vary with the methods used but that the range of the effects on social costs depend more on whether some externalities are considered than if we measure the effects through the elasticity of non-employment duration, through the duration in eligibility, or through the average UI paid. In all, we find a relatively mild effect of UI on the budget, meaning that behavioral responses seem to be rather small when the level of UI is increased. This result is in line with previous evidence and is also related to the fact that the level of UI was relatively small (the average transfer was about 25 percent of the average monthly formal wage) in the periods analyzed.

On the other hand, we provide estimates of welfare gains through several sources, including changes in household income for a sample of workers separated from their formal jobs, and through differences in consumption levels comparing households heads with a formal job or with UI transfers. We find that household income drops substantially after formal job separation, and this drop is very strong among beneficiaries of UI. We then find that the difference in household consumption between formal workers and unemployed people receiving UI is also significant but milder, possibly suggesting that workers can partly self-smooth consumption through other means different from UI. In any case, these estimates suggest that UI still have a role for consumption smoothing given that household consumption differs by about 20 percent.

In all, we use these point estimates to construct an MVPF indicator. We find that the provision of UI is valuable for beneficiaries, and this conclusion is robust to many alternative interpretations of social costs (including externalities) and welfare gains (measured both through changes in income or consumption). Our point estimates of MVPF are relatively high, compared to other estimates in the literature for developed countries.

Our results show that UI can be welfare-improving. In particular, workers are willing to pay for more than one monetary unit for each unit of government expenditure. That is, there are welfare gains from increasing UI according to most of our measures.

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20 We consider in this comparison the more related papers by Card et al. (2015a) and Landais (2015); they both exploit kinks in the benefit schedule for states of the US.

21 This number arises after using the simplest case of equation (5), with no externalities, to their results of a relative consumption drop of 0.281 and an elasticity of benefit duration of 0.27.

ADDITIONAL FILE

The additional file for this article can be found as follows:

- **Appendix.** Appendix A and B. DOI: https://doi.org/10.31389/eco.533.s1

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COMPETING INTERESTS

The authors have no competing interests to declare.

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