Appendix

A Conceptual Framework

In this appendix, we formalize the basic ideas behind the hypotheses we tested in this paper, with the aim to gain clarity on how and why the UBI should influence workers' effort and hence their productivity. We will make use of standard economic assumptions to define the standard economic benchmark to contrast our results with.[49]

A.1 Set up

Consider a fixed number, N, of identical workers, all of whom obtain utility from consumption and disutility from exerting effort. We assume for simplicity that the utility function of the representative worker is separable and that workers are risk neutral.[50] Thus, we can write the instantaneous utility function of a worker as $U(w, e) = w - e$, where w is the wage received and e is the level of effort exerted on his job. Let us further assume, also for simplicity, that workers can provide either minimal effort ($e = 0$), or some fixed positive level, $e > 0$.[51] Each worker is in one of two states at any point in time: employed or unemployed.

When employed, the worker has to choose an effort level e. If the worker does not shirk, he receives a wage of w and looses his job with probability b. If a worker shirks, he looses his job with probability q.[52] The worker selects an effort level to maximize his expected present discounted value of utility with a discount rate $r > 0$. This involves comparing the expected lifetime utility of a non-shirker employed, V_E^N, with the expected lifetime utility of an unemployed worker, V_U. Shapiro and Stiglitz's (1984)'s "fundamental asset equation" for an employed shirker is:

$$rV_E^S = w + (b + q)(V_U - V_E^S) \tag{2}$$

V_E^S represents the "asset value", w is the "flow benefit"and the expected "capital gain"(actually a loss) is the probability of job loss for a shirker $(b + q)$ times the amount of utility lost by becoming unemployed $(V_U - V_E^S)$. The real interest rate and utility discount rate are both equal to the constant r. Likewise, the "fundamental asset equation" for an

[49] The framework we will sketch here shares similar features with Shapiro and Stiglitz (1984). However, it is essentially different in that we will exogenously impose the unemployment rate, the unemployment benefit and the wage. For that reason, we do not model the decision of the employer (wages are set exogenously in our experiment) and we only focus on the decision of the employee to exert effort while employed.

[50] The assumption of risk-neutral preferences matter in the Shapiro-Stiglitz model. However, this is not quite relevant for our purposes, since we are comparing behavioural responses to different treatments.

[51] We make this assumption in order to keep our framework as close as possible to Shapiro and Stiglitz (1984)'s seminal model. However we note that, including effort as a continuous variable would not change the qualitative results.

[52] Both b and q are assumed to be exogenous to the worker. In our experiment, however, both are decreasing in worker's relative performance and increasing in the unemployment rate of the following day. None of these factors are known to the worker. Arguably, workers may have prior beliefs about their relative performance and they could in principle update their beliefs during the period of the project. Introducing this to the model, however, would only complicate the set up without adding anything essential. Recall that our main aim is to compare behavior across treatments, and insofar as learning of these parameters is not significantly different across treatments, treating b and q as exogenous parameters in our model does not come at any cost.

employed non-shirker is:

$$rV_E^N = w - e + b(V_U - V_E^N) \tag{3}$$

Solving equations (2) and (3) for V_E^S and V_E^N yields:

$$V_E^S = \frac{w + (b+q)V_U}{r+b+q} \tag{4}$$

$$V_E^N = \frac{(w-e) + bV_U}{r+b} \tag{5}$$

The worker will choose not to shirk if and only if $V_E^N > V_E^S$, which implies

$$w > rV_U + \frac{(r+b+q)e}{q} = \widetilde{w}, \tag{6}$$

or alternatively:

$$q(V_E^S - V_U) > e \tag{7}$$

That is, unless there is a penalty associated with unemployment, everyone will shirk. Moreover, equation (7) implies that the higher the penalty (i.e. the higher q or the lower the expected utility of unemployment V_U) the lower are the incentives for a worker to shirk. The key idea of our experimental design is, precisely, to test this result, by exogenously varying V_U in a way that resembles the most used unemployment benefit systems worldwide.

A.2 Utility of the unemployed under different schemes

How does V_U differ across treatments? To answer this question, we look at the cost and benefits associated with becoming unemployed. The monetary benefits are clear. Workers assigned to an unconditional and to a conditional UBI are offered a monetary benefit $\underline{w} > 0$ for being unemployed, while those assigned to no UBI are offered $\underline{w} = 0$. Clearly, the unemployed under the unconditional and conditional schemes are not obliged to receive the money if they wish not to. However, for simplicity, we assume that everyone entitled to an unemployment benefit claims it. In terms of costs, the three unemployment systems considered in our experiment differ only in the cost of effort $\underline{e} > 0$ that the unemployed worker has to exert in order to receive the unemployment benefit $\underline{w}$ under a *Conditional* UBI.[53] Other than this, from a purely economic perspective, the three interventions are equivalent in terms of costs.

Taking into account the economic costs and benefits, the asset equation for the expected utility of an unemployed worker under each treatment is given by:

$$\textit{Conditional UBI}: \qquad rV_U = (\underline{w} - \underline{e}) + a(V_E - V_U) \tag{8}$$

$$\textit{Unconditional UBI}: \qquad rV_U = \underline{w} + a(V_E - V_U) \tag{9}$$

[53] Although the unemployed could eventually shirk also while he is performing the activity required under a *Conditional* UBI, we assume that there is an opportunity cost of staying during the time of the activity, even doing nothing. Moreover, in our experiment, at least one experimenter was present in the room, so the unemployed could not do anything else but the activity they were assigned to.

$$No\ UBI: \qquad rV_U = a(V_E - V_U) \tag{10}$$

where a is the job acquisition rate and V_E is the expected utility of an employed worker, which equals V_E^N in equilibrium. We can now solve (5), (8), (9) and (10) simultaneously for V_E, $V_U^{UnconditionalUBI}$, $V_U^{ConditionalUBI}$ and V_U^{NoUBI}:

$$rV_U^{ConditionalUBI} = \frac{a\,(w-e) + (\underline{w} - \underline{e})(r+b)}{r+a+b} \tag{11}$$

$$rV_U^{UnconditionalUBI} = \frac{a\,(w-e) + \underline{w}(r+b)}{r+a+b} \tag{12}$$

$$rV_U^{NoUBI} = \frac{a\,(w-e)}{r+a+b} \tag{13}$$

Substituting (11), (12) and (13) into (6) yields the Non-Shirking Condition for each treatment:

$$Conditional\ UBI: \qquad w > \Phi + \underline{w} - \underline{e}, \text{ and} \tag{14}$$

$$Unconditional\ UBI: \qquad w > \Phi + \underline{w} \tag{15}$$

$$No\ UBI: \qquad w > \Phi \tag{16}$$

where $\Phi = e + \frac{(r+a+b)e}{q}$. Note that, from a purely economic perspective, the (net) unemployment benefit under an *Unconditional* UBI is the highest of all the unemployment systems (i.e. $\underline{w} > (\underline{w} - \underline{e})$ and $\underline{w} > 0$). Therefore, workers under an *Unconditional* UBI should have the highest incentives to shirk. This implies the following two hypotheses:

Hypothesis 1 (Unconditional UBI vs Conditional UBI and No UBI): From a purely economic perspective, the productivity of workers under an *Unconditional* UBI should be the lowest among the three systems.

Hypothesis 2 (Conditional UBI vs No UBI): From a purely economic perspective, if the productivity of workers under a *Conditional* UBI is higher (respectively lower or equal) than the productivity of workers under no UBI, it must be because the unemployment monetary benefit $\underline{w}$ is lower (respectively higher or equal) than the cost of exerting effort $\underline{e}$ in the activity imposed by the *Conditional* UBI.

B Experimental Procedures

This appendix describes the recruitment process, the characteristics of the task, the productivity measure and the sequence of the experiment in further detail. A copy of the instructions on how to codify is available upon request to the authors.

B.1 Recruitment

For the four groups (two UBIs - unconditional and conditional, and two No UBI - comparison) we recruited students from *University of Rosario* and *University of Los Andes*, to work as research assistants. We sent an e-mail to all social sciences students announcing the opening of temporal positions to work with the *Political Studies Research Group*.[54] Applicants had to be willing to work one and a half hours, two days a week for a month. They could choose to work Mondays and Wednesdays or Tuesdays and Thursdays, in the mornings or in the afternoons. The job announcement also stated that the number of vacancies per day would vary and that their final payment would depend on their work performance and on the labor demand per day. Given this, applicants were told to expect a final payment between US$32 and US$90.[55] The announcement also made clear that availability during the whole month period of the job was a necessary condition to be eligible. For the sake of transparency here we transcribe the entire text of the open call, which was exactly the same for both UBI treatments, with the only exception of the university email and the URL address given to applicant to fill their application form online. The call read:

> Vacancies available for research assistants. The *Political Studies Research Group*, an inter-institutional group of academic researchers, are conducting a research project in Colombia. To this end, they are looking for research assistants in Bogotá. The work will last 4 weeks, from 8 August to 5 September. Selected participants will be required to work a maximum of 3 hours per week. The total compensation will be between COP 50,000 and COP 150,000, depending on the task performance of the assistant. The details of the call are below. VACANCY: Research Assistant. Research assistants are required for a project to be held in Bogotá. The task is to code news from Colombian newspapers to create a database that will be used in future research by academics. REQUIREMENTS: very good knowledge of Internet and Excel. No previous experience or knowledge of foreign languages is required. PLACE: The task will be performed at the University of Rosario/ Andes. SCHEDULE: There will be different workgroups. Applicants can choose to work Monday and Wednesday or Tuesday and Thursday in the morning or afternoon. The slots available can be found in the form that has to be completed online (see at the end of this announcement). PERIOD: Candidates selected must commit their availability on the selected days and times from August 8 to September 5. Those who are not available during

[54] This group was created in order to preserve the identity of the researchers involved in the project. These researchers were never present in any of the sessions taking place in the universities to which they are affiliated to. We did so to separate the employer-employee relationship from an eventual professor-student one.

[55] For a participant who would never lose his job and hence work for a maximum of 12 hours (1.5 hours per shift $\times$ two shifts per week $\times$ four weeks) the corresponding expected final payment was between 2 and 6 times the hourly minimum wage. The incentive for taking up the job was then relatively large.

the whole period, will receive a lower payment. PAYMENT: The total compensation will be between COP 50,000 and COP 150,000. The need for assistants will vary across days. Your final payment will depend on your performance in the task and the number of vacancies available each day. Those interested in the job must complete their resume in the form available in the following link: (Rosario) `https://www.surveymonkey.com/s/hoja_de_vida_ur`. Application deadline: Wednesday August 3, 2011 (11:00 pm).[56]

The interested candidates had to fill in a pre-formatted CV online, where they had to select the time-slots in which they were available to work. We gathered socio-demographic information such us age, gender, residential address (from which we inferred the socioeconomic stratum), GPA, current job, etc. Among all subjects available, we randomly selected and invited to the induction day approximately 100 subjects per treatment/stage.[57] For each recruited subject, we created a non-photo badge with an ID and an e-mail account using that ID as user name and password.[58] Subjects were told that we would use that e-mail account to communicate with them exclusively.[59]

B.2 Job description and productivity measurement

B.2.1 Job description

As already mentioned, the job consisted in coding news about electoral practices of local politicians from the online archives of the two most popular Colombian newspapers, *El Tiempo* and *El Espectador*. The resulting dataset was used by Fergusson et al. (2013), who needed a candidate-level dataset on media exposure to look at the electoral consequences of the media exposing Congress candidates as having ties with illegal armed groups before the elections relative to exposing them after the elections.

[56]Translated from the original Spanish version: "Vacantes disponibles para asistentes de investigación. El *Grupo de Estudios Políticos*, un grupo inter-institucional de investigadores académicos, se encuentra realizando un trabajo de investigación en Colombia. Para este fin, requiere asistentes de investigación en Bogotá. El trabajo durará 4 semanas, en el período comprendido entre el 8 de agosto y el 5 de septiembre inclusive. Los asistentes seleccionados deberán trabajar un máximo de 3 horas semanales. La remuneración total estará entre 50.000 pesos y 150.000 pesos, dependiendo del rendimiento en las tareas del asistente. Los detalles de la convocatoria se encuentran a continuación. VACANTE: Asistente de investigación. Se requiere asistentes de investigación para un proyecto a realizarse en Bogotá. El trabajo consiste en codificar noticias de periódicos Colombianos, para crear una base de datos que será utilizada en futuras investigaciones por académicos. REQUISITOS: muy buen manejo de Internet y Excel. No se requiere experiencia previa en este tipo de trabajo ni conocimiento de idiomas extranjeros. LUGAR: Las tareas se realizarán en la Universidad del Rosario/de los Andes. HORARIO: Habrán diferentes grupos de trabajo. Los postulantes pueden elegir trabajar lunes y miércoles o martes y jueves, en horas de la mañana o la tarde. Los horarios disponibles están detallados en el formato de hoja de vida que deben completar (ver al final de esta convocatoria). PERIODO: Quienes sean seleccionados deberán comprometerse a estar disponibles, en los días y el horario seleccionado, durante todo el período comprendido entre el 8 de agosto y el 5 de septiembre inclusive. Aquellas persona que no se encuentren disponibles durante la totalidad del periodo, recibirán un pago reducido. REMUNERACION: La remuneración total estará entre 50.000 pesos y 150.000 pesos. La necesidad de asistentes irá variando con los días. Su pago final dependerá de su rendimiento en la tarea asignada y de la cantidad de vacantes disponibles cada día. Aquellos interesados en el trabajo deberán completar el formato de hoja de vida en el siguiente link: (Rosario) `https://www.surveymonkey.com/s/hoja_de_vida_ur`. Fecha límite de recepción de aplicaciones: miércoles 3 de agosto 2011 (11:00 pm)".

[57]No subject who was RA in the first year was RA in the second year.

[58]Subjects were allowed to change the password to protect their privacy.

[59]We chose this procedure in order to provide anonymity especially for the unemployed who choose to stay on the unemployment benefit. We wanted to avoid the unemployed to feel any pressure to eventually apply for a new vacancy.

Every working day, each coder was assigned a random list of politicians drawn from a big pool of politicians. We made sure that a coder did not receive the same politician twice. We used the same pool of politicians in all the four interventions (conditional, Unconditional UBIand the two comparisons). The worker had to search online for all news mentioning the names of the politicians in his list. For each news found, he had to code information on a pre-formatted excel file. Every news coded was a new row in the file. Coders had to follow a rigorous protocol which explained step by step the search criteria as well as the type of information they had to record and how to do so. At the end of each session, they had to save their excel files with all the entries they recorded and label the file with their unique ID number and the day of work. In this way, we were able to identify uniquely the output of each coder in each working day.

Each working day lasted ninety minutes in total. Seventy minutes were used for coding and the rest of the time was used to save the file, send it to us by e-mail (from the work account we specially created for each of them) and filling in some questionnaires.

B.2.2 Productivity measure

Coders' daily productivity was measured by the number of news correctly coded per day. This was determined as follows. Each coder would input the data from the newspaper's archives in a pre-formatted excel file. Once the coder finished his/her working session, he/she had to save the excel file with his/her coder ID number and send the file to us by email. We would check on the spot that the email had arrived with the correct attached file and, only then, the coder would be released from his/her job duty for the day. Then, during that afternoon, when all the day's sessions were over, we would download all the excel files produced on that day and randomly and uniformly assign files to each of the six assistant in charge of computing the productivity. The assistant would then open every file, *estimate* the number of correct entries per file and record the productivity of each coder for that day.

To estimate the number of correctly coded entries the research assistants used the following protocol: First, if the coder entered 100 news or less, the assistant would randomly select 20% of the coders' entries recorded in the file. If the coder entered over 100 news, the assistant would take 15% of entries. The subsample of entries gathered by the assistant was then subject to a detailed scrutiny. This task was done in two sequential stages. First, the assistant would check whether each entry was fake or valid, that is, whether the link that the coder had pasted next to the entry had information about the assigned candidate that was published by the assigned source on the day reported by the coder in that particular entry. In case the research assistant found fake entries, he/she would check every single entry recorded by that coder to establish whether or not there was systematic cheating.[60] Second, for the non-fake entries, the assistant would check that they were correctly coded. To that end he/she had to read the entire news piece attached to the link and re-code the information using the coding protocol given to the coders. If both entries (that of the coder and that of the research assistant) matched, the coders' entry would be considered correct. In the end, coder/day productivity scores were computed as the total number of correctly coded news according to the sample scrutinized by the assistants, extrapolated to 100% of

[60] As it turned out, there were only fake entries in the first working day and by a handful of coders. These people obtained very low productivity scores and became unemployed. On the second day of work we publicly announced that there were cases of cheating and that had led cheaters to have low scores. This was enough to never detect fake news for the rest of the intervention.

the coded news, minus the extrapolated incorrectly coded news.

With this input the research assistants created a productivity ranking of coders per group per day. These rankings were used to determine who would become unemployed (respectively re-employed), in case of negative (respectively positive) shocks. Importantly, coders did not have access to these rankings until the last day of work.

B.3 Groups and place of work

Coders were assigned to eighteen groups, according to their availability of time and their university. Subjects who indicated availability in more than one slot were randomly allocated to one of these slots. Once subjects were assigned to a group they were not allowed to change it during the entire period of the project. The distribution of groups was balanced across days, time of the day and newspaper to code. Half of the groups were allocated to Mondays and Wednesdays and half to Tuesdays and Thursdays. Within each day, half of the groups were in the morning and half in the afternoon. The average number of subjects in the first day of work per group was around 17, ranging from a minimum of 11 subjects to a maximum of 23. To avoid spillover and contagion between treated and control groups within each university, we implemented the UBIs interventions (i.e. *Unconditional* UBI and *Conditional* UBI) and the intervention without UBI (i.e. *Comparison*) with a one-year difference. See Table B.1 for a detailed description of the eighteen groups.

Each group was randomly assigned to a distinct computer room. All computer rooms had at least 25 PCs or laptops and internet connection. Each session was supervised by two research assistants randomly allocated across groups. The research assistants would make sure that the session ran smoothly. They would set the computers up, record attendance, answer questions about coding, administer questionnaires, etc.

Table B.1: Description of groups

				Coders		
Group	University	Newspaper	Treatment	Day1	Day 8	% Dropout
1	University A	El Tiempo	Unconditional UBI	16	11	31
2	University A	El Espectador	Unconditional UBI	18	9	50
3	University A	El Espectador	Unconditional UBI	14	8	43
4	University A	El Tiempo	Unconditional UBI	11	9	18
5	University B	El Tiempo	Conditional UBI	20	14	30
6	University B	El Espectador	Conditional UBI	15	8	47
7	University B	El Tiempo	Conditional UBI	23	14	39
8	University B	El Espectador	Conditional UBI	22	16	27
9	University B	El Tiempo	Conditional UBI	14	7	50
10	University B	El Espectador	Conditional UBI	13	5	62
11	University B	El Tiempo	Conditional UBI	18	12	33
12	University B	El Espectador	Conditional UBI	17	10	41
13	University A	El Tiempo	Comparison	20	6	70
14	University A	El Tiempo	Comparison	15	5	67
15	University A	El Espectador	Comparison	20	11	45
16	University B	El Tiempo	Comparison	13	4	69
17	University B	El Tiempo	Comparison	22	9	59
18	University B	El Espectador	Comparison	13	4	69

B.4 Sequence of the experiment

B.4.1 Induction day

The first day was an induction day with the purpose of providing a detailed explanation of the particularities of the working scheme. Each subject received 75% of the salary of a standard working session as a show-up compensation. During the induction, subjects were reminded that their final remuneration would depend on their performance as coders, with the minimum being US$ 32 and the maximum US$ 90 (2 and 6 times the hourly minimum wage respectively for the maximum number of hours that they could work). They also received a detailed explanation on the way their payments were going to be computed, a description of the particularities of the job as well as the instructions on how to perform the task of coding. In addition, we told them that other researchers were interested in collecting additional data, so as part of the work activities, they would be asked to answer a set of questionnaires.[61] At the end of the induction day, those subjects who accepted the terms and conditions of the job signed an informed consent and completed the first questionnaire.[62]

B.4.2 Unemployment and reemployment

At the beginning of every working day, two assistants would receive the subjects, check their coder's ID and inform them about their condition as employed or unemployed for that day. The assistants would also make sure that every session consisted of 70 minutes of coding work.

We created involuntary unemployment (shortage of vacancies) in all days except for the first and last days, when we had full employment. Unemployment increased in the second and sixth day, and decreased (i.e. vacancies were opened) during the rest of the project. Table 2 shows the unemployment rate for each working day.

On the days in which there was a negative shock (i.e. an increase in the unemployment rate), the coders ranked last according to their productivity in the previous day became unemployed. Unemployed subjects were informed via e-mail of the number of job openings for the following working day. If they wanted to be considered for the new positions, they had to reply to that email. Vacancies were filled in according to applicants' productivity on their last day employed. We informed subjects their (new) condition on the following working day.

In order to be considered as part of the labor force, all subjects had to attend their working session on time, even those who were unemployed. If failed to do so (without any justified cause, such as illness), they were considered to have withdrawn from the project.

[61] Subjects were informed that their answers to these questionnaires were going to be treated anonymously and would only be used for academic purposes. We also clarified that we were intermediaries between this group of researchers (those who needed the dataset on the politicians but also those who needed other type of data) and them, the RAs. From the debriefing and willingness to answer the psychological questionnaires, it seemed that answering some additional questions did not come as a surprise for them, as they were informed in advance about the characteristics of this project.

[62] Only one subject out of the three hundred and nighty six who came to the induction day, did not sign the informed consent. This was the only person who decided not to take the job.

B.4.3 Sequence and magnitude of the unemployment rate

We acknowledge that the unemployment rates used in our interventions are high. Rates of 35% to 60% are not typical, although rates above 30% are common in several countries in Africa and rates of about 30% are currently experienced by Spain and Greece or during major crises (e.g. the Great Depression or Argentina in 2001). Having said that, we acknowledge that it would have been preferable to use unemployment rates of levels that are mostly observed in the real world (i.e. on a range of 7% to 20%). Unfortunately, that was not feasible, mainly due to limited availability of funding and limited pool of potential experiment participants (student population of the universities). If we were to apply unemployment rates within a real-world reasonable range like the one mentioned above, we would have needed at least to quadruplicate the sample size in order to study the behavior of both, unemployed and employed people under different UBIs. Indeed, while our main aim was to compare the productivity of the employed across UBIs, we also wanted to have a fairly high number of unemployed people to better understand the channels through which UBIs affect productivity. For example, we wanted to be able to observe whether the UBI would affect the way unemployment affects self-esteem. For this reason, we needed a relative high number of both, employed and unemployed people. Clearly, if we had used lower unemployment rates, we would have needed to increase significantly the sample size, but that was not feasible given the budget and population constraints mentioned above. As a compromise, we decided to use a relatively high unemployment rate in some days. The daily exact rate took these constraints into account. We calculated a maximum of 80 coders per treatment. Moreover, for reasons of power, we did not want to have less than 30 (unemployed or employed) people per treatment/day. Hence, the maximum unemployment rate was set at 60% and the minimum at 35%. Given the number of coders in each treatment these figures would ensure a minimum number of employed/unemployed of about 30 in each treatment/day. Finally, we also wanted to allow for 0% unemployment rate in the first and last working days (day 1 and day 8). This would allow us to: i) measure and compare workers' productivity across UBIs before they actually experienced any unemployment shock (day 1), and in an environment without involuntary unemployment (day 8), and ii) compare application rates of the unemployed in an environment of no competition for vacancies (day 8).

It is also important to highlight that workers were *not* informed about the exact distribution of positive and negative shocks. Workers only knew, as early as in the open call for job applications, that the need for research assistants would vary throughout the duration of the project. Specifically, the call stated:

> The total compensation will be between COP 50,000 and COP 150,000. The need for assistants will vary day to day. Your final payment will depend on your performance in the assigned task and on the number of available vacancies each day.

Likewise, in the induction day, each worker received the following information:

> The need for assistants can vary from one day to the other. Every day we report the work done to the senior researchers, and they decide the needs of data for the next working day. So it is possible that in the next days the need for assistants decreases. But it can also happen that the need for assistants increases.[63]

[63] We acknowledge that the fluctuations in labour demand may have appeared to be artificial for the

Table B.2 shows the daily sample composition by treatment, distinguishing between subjects who are employed, who are unemployed and who drop out by not showing up. This table complements Table 3 by showing the day by day dropout ultimately resulting in the overall attrition rate discussed in Section 3.1.

Table B.2: Day by day sample composition

	UBIs		Comparison	
	University B *Conditional UBI*	University A *Unconditional UBI*	University B *No UBI*	University A *No UBI*
Day 1				
Average Productivity	28.8	29.9	18.3	18.1
	(16.2)	(17.5)	(13.7)	(10.7)
Employed	143	60	48	55
Day 2				
Employed	56	24	20	22
Unemployed	84	34	27	33
Dropout	3	2	1	0
Day 3				
Employed	65	28	23	24
Unemployed	64	29	15	19
Dropout	11	1	9	12
Cum. dropout	14	3	10	12
Day 4				
Employed	77	34	22	26
Unemployed	45	20	12	11
Dropout	7	3	4	6
Cum. dropout	21	6	14	18
Day 5				
Employed	91	39	23	28
Unemployed	28	14	8	7
Dropout	3	1	3	2
Cum. dropout	24	7	17	20
Day 6				
Employed	51	22	14	16
Unemployed	67	31	15	16
Dropout	1	0	2	3
Cum. dropout	25	7	19	23
Day 7				
Employed	60	26	16	18
Unemployed	54	27	10	12
Dropout	4	0	3	2
Cum. dropout	29	7	22	25
Day 8				
Employed	87	37	22	24
Unemployed	27	14	2	6
Dropout	0	2	2	0
Cum. dropout	29	9	24	25

workers. However, we tried to minimize this risk by recreating a scenario in which such fluctuations were defined by researchers' daily needs of data. The job setting was one in which the researchers had to analyze the data collected on a daily basis, and there were going to be times in which they could not keep up with the pace of data production. Although we recognize this is not entirely natural, it is definitively not an implausible scenario.

B.5 Remuneration scheme

The remuneration per 90 minutes of work was the market for research assistants in the universities where the intervention took place. We paid subjects COP$ 14,250 (≈ US$ 8). This salary was over four times the minimum wage in Colombia for such working shift.

In addition, there was a bonus of COP$ 40,000 (≈ US$ 22), payable at the end of the experiment, for the subject in each group who correctly coded the highest number of news in a session.

The maximum possible remuneration was US$ 90, earned by subjects who came to the induction day (US$ 6), were employed during the eight working days (US$ 8 × 8 days = US$ 64) and won the bonus for best coder of his/her group (US$ 20). The minimum possible remuneration of someone who participated in the project over the whole period was US$ 32 if the person was assigned to a treatment with an UBI, and US$ 14 is he/she was assigned to the *Comparison*. To receive the minimum amount, the coder had to come to the induction day (US$ 6) and worked the first day of work (US$ 8), but remained unemployed and active since the first day of unemployment. If the coder was assigned to an UBI, then he/she would receive additional US$ 18 (a US$ 3 unemployment compensation × 6 days).

In order to minimize attrition, we paid the last day of the project. Additionally, subjects who withdraw from the project without a justified reason were entitled to request only half of the money earned by the last day they showed up to work.

B.6 Interventions

As mentioned, we implemented three different experimental interventions. The *Comparison* treatment, without any UBI, and two treatments designed to mimic the two main existing UBIs (*Unconditional* UBI and *Conditional* UBI).

The amount of the unemployment benefit was identical in the two UBI treatments and equivalent to a third of the daily shift salary of the employed. This is of course an abstraction of the real world UBIs, in which unemployment compensations vary, even within countries, according to several characteristics of the unemployed like the spell of unemployment. While we do not think that abstracting from this variation is relevant for our experiment, we did make an effort to to replicate other features of the actual implementation of UBIs. For instance, when applying for the benefit in reality, unemployed individuals have to fulfill some requirements, including signing up, filling in paperwork, and showing up regularly at some administrative office (usually to fill in additional paperwork). We had a similar procedure in our two treatments: The unemployed who wanted to receive the benefit had to show up every working day at the time and place of work. If assigned to the *Unconditional* UBI treatment the unemployed could leave immediately after filling in some short questionnaires and receiving the compensation.[64] If assigned to the *Conditional* UBI treatment, the unemployed could choose whether to complete the ancillary task and get the compensation or to leave without getting paid.[65] In any case the unemployed had

[64] We used the questionnaires to measure various psychological traits that we use when testing the mechanisms behind the main result.

[65] The task, which lasted 30 minutes, was to place letters in envelopes and paste on them randomly assigned (real) postal addresses. We manipulated the content of the letter in two sub-treatments. A *Charity* sub-treatment used letters that asked for donations to a social NGO. A *Placebo* sub-treatment used letters to publicize the business of a local private firm. While the existence of these sub-treatments within the *Conditional* UBI treatment does not affect the incentives exploited for this paper, these data will be used in subsequent companion papers.

to show and sign an attendance sheet. Failing to do so would be equivalent to quit the job altogether and stop being considered part of the labor force (and not being able to re-apply for a position when new vacants were opened).

For comparability, unemployed coders in the *Comparison* treatment were also required to show up every working day at the time and place of work if they wanted to be considered part of the labor force. Of course, they were invited to leave immediately after filling some short questionnaires.

C Potential selection caused by dropouts

Table C.1: Baseline sample differences across universities and treatments – *Dropouts*

	Within University A			Within University B			Across universities	
	Unconditional UBI	*Comp.*	Difference	*Conditional UBI*	*Comp.*	Difference	UBI	*Comp.*
	(1)	(2)	(1)−(2)	(3)	(4)	(3)−(4)	(1)−(3)	(2)−(4)
Panel A: *Individual characteristics*								
Gender (1=female)[b]	0.78	0.56	0.22	0.59	0.38	0.21	0.19	0.19
	(0.15)	(0.10)	(0.19)	(0.09)	(0.10)	(0.14)	(0.19)	(0.14)
Age	22.78	21.12	1.66**	21.28	20.5	0.78	1.5*	0.62
	(0.68)	(0.37)	(0.74)	(0.38)	(0.30)	(0.50)	(0.78)	(0.48)
Socioeconomic status[a]	3.44	3.56	-0.12	4.52	4.48	0.04	-1.07**	-0.92***
	(0.18)	(0.18)	(0.33)	(0.17)	(0.24)	(0.29)	(0.32)	(0.30)
GPA (1 to 5 scale)	4.01	3.93	0.08	3.97	3.98	-0.01	0.03	-0.06
	(0.09)	(0.07)	(0.12)	(0.07)	(0.06)	(0.09)	(0.14)	(0.09)
Currently working (1=yes)[b]	0.33	0	0.33***	0.14	0.17	-0.03	0.20	-0.17**
	(0.17)	(0)	(0.10)	(0.07)	(0.08)	(0.10)	(0.15)	(0.08)
Major (=1 econ. related)[b]	0.44	0.44	0	0.62	0.46	0.16	-0.18	-0.02
	(0.18)	(0.10)	(0.20)	(0.09)	(0.10)	(0.14)	(0.19)	(0.15)
Panel B: *Baseline psychological characteristics*								
Self-esteem (day 0)	35.78	34.88	0.90	36	36.09	-0.09	-0.22	-1.21
	(1.12)	(0.69)	(1.32)	(0.66)	(0.53)	(0.87)	(1.33)	(0.87)
Job satisfaction (day 1)	24.44	23.83	0.61	23.76	24.27	-0.51	0.69	-0.44
	(1.88)	(1.04)	(2.05)	(0.76)	(0.77)	(1.10)	(1.71)	(1.32)
Panel C: *Initial productivity (day 1)*								
Mean	17.22	12.12	5.10	22.96	14.5	8.46**	-5.74	-2.38
	(3.80)	(1.27)	(3.08)	(1.81)	(3.16)	(3.49)	(3.87)	(3.35)
Std. dev.[c]	11.41	6.33	W_0 =8.20***	9.74	15.48	W_0 =0.32	W_0 =0.91	W_0 =2.90*

Notes: Definition of variables: **Gender**, **Age** and **GPA** are self-explanatory. **Socioeconomic status**: See section 3, in particular footnote 29. **Currently working**: Answer to the question: *Are you currently working?* (1=Yes 0=No). **Major**: Answer to the question: *What is your major o study?* (1=Economics, Business, Finance 0=Rest of social sciences). **Self-esteem**: See section 3, in particular footnote 30. **Job satisfaction**: See section 3, in particular footnote 31. **Productivity**: See appendix B.2. [a] test of difference in means using Fisher's exact test because variable is categorical. [b] test of difference in means using χ (Pearson-Chi2) test because variable is dichotomous. [c] test of difference in variance using the statistic of Levene's robust test (statistic denoted by W_0). The rest of the mean differences are checked using the `t-test`. *** significant at 1%, ** significant at 5%, * significant at 10%.

D Multiple Hypothesis Testing

Table D.1: The effect of unemployment benefit institutions on the productivity of workers correcting for multiple hypothesis testing

List et al. (2015) correction for multiple hypothesis testing			
	Mean Dif.	p-values	
		Unadjusted	MHT-adjusted
Outcome & pairwise comp.	(1)	(2)	(3)
Productivity			
Conditional UBI vs. *Comparison*	26.9	0.0003***	0.0003***
Unconditional UBI vs. *Comparison*	43.1	0.0003***	0.0003***
Unconditional UBI vs. *Conditional UBI*	16.2	0.0017***	0.003***
Log Productivity			
Conditional UBI vs. *Comparison*	0.818	0.0003***	0.0003***
Unconditional UBI vs. *Comparison*	0.997	0.0003***	0.0003***
Unconditional UBI vs. *Conditional UBI*	0.179	0.0273**	0.0273**

Notes: List et al. (2015) correction procedure for MHT. Column 1 reports simple mean differences, with no controls. Columns 2 and 3 report respectively unadjusted MHT-adjusted p-values of each pairwise comparison. *** significant at 1%, ** significant at 5%.

Table D.2: The effect of unemployment benefit institutions on the productivity of workers correcting for multiple hypothesis testing
(Subsamples of returners and always employed)

List et al. (2015) correction for multiple hypothesis testing			
Dependent Variable: *Productivity*			
	Mean Dif.	p-values	
		Unadjusted	MHT-adjusted
Subgroup & pairwise comp.	(1)	(2)	(3)
Returners			
Conditional UBI vs. *Comparison*	8.57	0.224	0.389
Unconditional UBI vs. *Comparison*	11.73	0.146	0.305
Unconditional UBI vs. *Conditional UBI*	3.16	0.567	0.579
Always employed			
Conditional UBI vs. *Comparison*	34.43	0.0003***	0.0003***
Unconditional UBI vs. *Comparison*	57.73	0.0003***	0.0003***
Unconditional UBI vs. *Conditional UBI*	23.30	0.004***	0.0143**

Notes: List et al. (2015) correction procedure for MHT. Column 1 reports simple mean differences, with no controls. Columns 2 and 3 report respectively unadjusted MHT-adjusted p-values of each pairwise comparison. *** significant at 1%.

E Potential explanations

E.1 Sample imbalance

Table E.1: The effect of unemployment benefit institutions on the productivity of workers
Balanced samples on covariates originally unbalanced

Ordinary Least Squares						
Dependent Variable: *Productivity*						
	(1)	(2)	(3)	(4)	(5)	(6)
University A	-3.261	-5.586	-3.282	-4.492	0.391	-3.015
(*Unconditional UBI* and *Comparison*)	(3.093)	(3.365)	(3.141)	(3.281)	(3.291)	(4.323)
UBI stage	18.71***	19.36***	18.72***	15.81**	22.39***	19.58***
(*Unconditional UBI* in Uni A & *Conditional UBI* in Uni B)	(5.263)	(5.388)	(5.267)	(6.173)	(5.430)	(6.383)
University A × *UBI stage*	24.10***	25.41***	24.11***	27.29***	20.09***	26.052***
(*Unconditional UBI*)	(6.438)	(8.400)	(6.483)	(6.698)	(6.694)	(8.627)
Constant	69.56**	63.39**	69.78**	74.89*	73.36***	73.95*
	(27.63)	(28.66)	(27.78)	(40.25)	(28.04)	(44.49)
Individual controls	✓	✓	✓	✓	✓	✓
Balanced characteristic						
Gender		✓				✓
Age			✓			✓
Socioeconomic status				✓		✓
Currently working					✓	✓
Observations	1,223	1,085	1,220	793	1,200	653

Notes: Robust standard errors clustered at the group level in parentheses. Column (1) reproduces column (2) of Table (4) and is used as a benchmark. Columns (2) to (6) report the results from estimating the same regression model on different subsamples that are balanced across the individual characteristics originally unbalanced. The subsample used in the results reported in column (2) is balanced on gender (see first row of table E.2). The subsample used in the results reported in column (3) is balanced on age (see second row of table E.2). The subsample used in the results reported in column (4) is balanced on socioeconomic status (see third row of table E.2). The subsample used in the results reported in column (5) is balanced on whether subjects had a second job at the time of the experiment (see fourth row of table E.2). The subsample used in the results reported in column (6) is balanced on the fourth individual characteristics simultaneously. To compute each one of the subsamples used in the results reported in columns (2) to (6) we followed the following simulation procedure: i) We randomly drop just as many subjects per university as necessary in order to get a balanced sample according to a specific individual characteristic both the in treatment and in the comparison stage (e.g. to get a gender-balanced sample we need to drop female subjects from University A in both the treatment and the comparison stage –see first row of Table 1). ii) We run the baseline regression model with the balanced sample and get the new estimated treatment effects. iii) We repeat steps i and ii 100 times, each time extracting a different random sub-sample of coders. iv) We average the treatment effects (and standard errors) across the 100 estimated coefficients. Control variables in all specifications include gender, age, whether the subject has another job, the size of the group faced by each worker every working day, socioeconomic stratum fixed effects and day fixed effects. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table E.2: Baseline sample differences across universities and treatments

	Within University A			Within University B			Across universities	
	Unconditional UBI	*Comp.*	Difference	*Conditional UBI*	*Comp.*	Difference	UBI	*Comp.*
	(1)	(2)	(1)−(2)	(3)	(4)	(3)−(4)	(1)−(3)	(2)−(4)
Panel A: *Individual characteristics*								
Gender (1=female)[b]	0.45	0.39	0.06	0.45	0.40	0.06	0.00	0.00
	(0.08)	(0.08)	(0.11)	(0.04)	(0.07)	(0.08)	(0.09)	(0.11)
Age	21.30	21.07	0.23	20.94	20.56	0.38	0.36	0.51
	(0.25)	(0.23)	(0.34)	(0.15)	(0.23)	(0.29)	(0.28)	(0.33)
Socioeconomic status[a]	3.70	4.11	-0.41	3.70	4.11	-0.41	0.00	0.00
	(0.12)	(0.13)	(0.19)	(0.12)	(0.13)	(0.19)	(0.17)	(0.19)
Currently working (1=yes)[b]	0.08	0.05	0.03	0.09	0.05	0.05	-0.01	0.01
	(0.04)	(0.03)	(0.05)	(0.02)	(0.03)	(0.05)	(0.04)	(0.05)

Notes: Definition of variables: **Gender** and **Age** are self-explanatory. **Socioeconomic status**: See section 3, in particular footnote 29. **Currently working**: Answer to the question: *Are you currently working?* (1=Yes 0=No). [a] test of difference in means using Fisher's exact test because variable is categorical. [b] test of difference in means using χ (Pearson-Chi2) test because variable is dichotomous. The rest of the mean differences are checked using the `t-test`. *** significant at 1%, ** significant at 5%, * significant at 10%.

E.2 Attrition

Table E.3: Determinants of attrition and differential attrition across treatments

	Ordinary Least Squares						
Dependent Variable: *Decision of dropping out (days 3 to 8)*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
University A	-0.00554	-0.00462	-0.00469	-0.00398	-0.00402	0.00935	0.00935
(*Unconditional UBI* and *Comparison*)	(0.0310)	(0.0327)	(0.0320)	(0.0314)	(0.0316)	(0.0270)	(0.0295)
UBI stage	-0.0801***	-0.0826***	-0.0813***	-0.0803***	-0.0822***	-0.0620***	-0.0620***
(*Unconditional UBI* in Uni A & *Conditional UBI* in Uni B)	(0.0150)	(0.0167)	(0.0159)	(0.0155)	(0.0158)	(0.0178)	(0.0200)
University A × *UBI stage*	-0.0104	-0.0124	-0.0117	-0.0129	-0.0131	-0.0307	-0.0307
(*Unconditional UBI*)	(0.0336)	(0.0358)	(0.0346)	(0.0340)	(0.0342)	(0.0249)	(0.0291)
Unemployed day before		0.104***	0.126***	0.130***	0.132***	0.102***	0.102***
		(0.0229)	(0.0321)	(0.0333)	(0.0344)	(0.0316)	(0.0301)
Unemployment spell			-0.00933	-0.00851	-0.00820	0.00416	0.00416
			(0.00741)	(0.00763)	(0.00766)	(0.00787)	(0.00809)
Application rejected day before				-0.0258	-0.0259	-0.0204	-0.0204
				(0.0323)	(0.0325)	(0.0309)	(0.0295)
Productivity on day 1					0.000182	0.000224	0.000224
					(0.000412)	(0.000443)	(0.000471)
Constant	0.118***	0.0715***	0.0704***	0.0697***	0.0647***	0.387***	0.387***
	(0.0110)	(0.0148)	(0.0144)	(0.0144)	(0.0202)	(0.0792)	(0.0845)
Individual controls						✓	✓
Bootstrapped SE							✓
Observations	1,427	1,427	1,427	1,427	1,427	1,427	1,427
R-squared	0.027	0.078	0.079	0.080	0.081	0.098	0.098

Notes: Robust standard errors clustered at the group level in parentheses. Column (7) uses bootstrap (1,000 repetitions) to compute the standard errors. Determinants of attrition include whether the subject was unemployed on the day before the observation (Column 2 and thereafter); the length of he unemployment spell experienced (Column 3 and thereafter); whether the subject applied to a vacant on the day before and was rejected (Column 4 and thereafter); and the subject's initial productivity (Column 5 and thereafter). Additional control variables include gender, age, whether the subject has another job, the size of the group faced by each worker every working day, socioeconomic stratum fixed effects and day fixed effects. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table E.4: Does experiencing unemployment affect attrition differentially across treatments?

Ordinary Least Squares					
Dependent Variable: *Decision of dropping out (days 3 to 8)*					
				Subsample of Unemp. day before	
				Yes	No
	(1)	(2)	(3)	(4)	(5)
University A	-0.0209**	-0.0134	-0.0134	0.0559	-0.0123
(*Unconditional UBI* and *Comparison*)	(0.00791)	(0.0102)	(0.0122)	(0.0729)	(0.0130)
UBI stage	-0.0234***	-0.00210	-0.00210	-0.108***	-0.0146
(*Unconditional UBI* in Uni A & *Conditional UBI* in Uni B)	(0.00660)	(0.00848)	(0.0124)	(0.0371)	(0.0160)
University A × *UBI stage*	0.0214*	0.00595	0.00595	-0.0904	0.0139
(*Unconditional UBI*)	(0.0108)	(0.0154)	(0.0180)	(0.0652)	(0.0176)
Unemployed day before	0.200***	0.191***	0.191***		
	(0.0328)	(0.0309)	(0.0306)		
University A × *Unemployed day before*	0.0394	0.0508	0.0508		
	(0.0708)	(0.0723)	(0.0784)		
UBI stage × *Unemployed day before*	-0.131***	-0.126***	-0.126***		
	(0.0382)	(0.0364)	(0.0382)		
University A × *UBI stage* × *Unemployed day before*	-0.0753	-0.0809	-0.0809		
	(0.0751)	(0.0741)	(0.0834)		
Constant	0.0288***	0.287***	0.287***	0.444***	0.0469
	(0.00397)	(0.0881)	(0.0917)	(0.141)	(0.0292)
Individual controls		✓	✓	✓	✓
Bootstrapped SE			✓	✓	✓
Observations	1,427	1,427	1,427	659	768
R-squared	0.105	0.124	0.124	0.100	0.036

Notes: Robust standard errors clustered at the group level in parentheses. Columns (3) to (5) use bootstrap (1,000 repetitions) to compute the standard errors. Column (4) focuses on the subsample of individuals who, at any given working day, had been unemployed on the day before. Column (5) focuses on the subsample of individuals who, at any given working day, had not been unemployed on the day before. Control variables include gender, age, whether the subject has another job, the size of the group faced by each worker every working day, socioeconomic stratum fixed effects and day fixed effects. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table E.5: The effect of unemployment benefit institutions on the productivity of workers (Subsample of finalizers)

Ordinary Least Squares			
Dependent Variable: *Productivity*			
	(1)	(2)	(3)
University A	-9.176*	-4.454	-4.454
(*Unconditional UBI* and *Comparison*)	(5.203)	(3.940)	(5.813)
UBI stage	20.93***	18.70***	18.70**
(*Unconditional UBI* in Uni A & *Conditional UBI* in Uni B)	(4.238)	(6.092)	(7.334)
University A × *UBI stage*	24.34***	25.51***	25.51***
(*Unconditional UBI*)	(7.013)	(7.128)	(8.728)
Constant	46.75***	76.26*	83.19**
	(1.668)	(37.10)	(38.64)
Individual controls		✓	✓
Bootstrapped SE			✓
Observations	1,115	1,115	1,115
R-squared	0.077	0.272	0.272

Notes: Finalizers are those subjects who remained in the project until the last day. Robust standard errors clustered at the group level in parentheses. Column (3) uses bootstrap (1,000 repetitions) to compute the standard errors. Control variables include gender, age, whether the subject has another job, the size of the group faced by each worker every working day, socioeconomic stratum fixed effects and day fixed effects. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table E.6: The effect of treatments on selection into subsamples

Ordinary Least Squares						
Dependent Variable: *Productivity on day 1*						
	Returners			Always employed		
	(1)	(2)	(3)	(4)	(5)	(6)
University A	2.030***	4.376***	4.376	-1.673	-1.206	-1.206
(*Unconditional UBI* and *Comparison*)	(0.498)	(1.272)	(2.713)	(3.734)	(3.727)	(5.742)
UBI stage	7.475***	8.768***	8.768***	15.07***	11.77**	11.77*
(*Unconditional UBI* in Uni A & *Conditional UBI* in Uni B)	(2.101)	(2.257)	(3.338)	(4.097)	(4.596)	(6.322)
University A × *UBI stage*	-1.823	-3.611	-3.611	5.554	7.699	7.699
(*Unconditional UBI*)	(2.927)	(3.490)	(4.841)	(6.017)	(7.240)	(9.649)
Constant	17.64***	11.89	19.25	29.4***	20.87	21.73
	(0.244)	(14.46)	(16.38)	(2.170)	(24.35)	(29.80)
Individual controls		✓	✓		✓	✓
Bootstrapped SE			✓			✓
Observations	97	97	97	77	77	77
R-squared	0.060	0.135	0.135	0.245	0.317	0.317

Notes: Robust standard errors clustered at the group level in parentheses. Columns (3) and (6) use bootstrap (1,000 repetitions) to compute the standard errors. Columns (1) to (3) focus on the sample of subjects who experienced unemployment but were re-hired. Columns (4) to (6) focus on the sample of workers who never experienced unemployment. Control variables include gender, age, whether the subject has another job, the size of the group faced by each worker every working day, socioeconomic stratum fixed effects and day fixed effects. *** significant at 1%, ** significant at 5%, * significant at 10%.