

# Unemployment Insurance, Risk, and the Acquisition of Specific Skills: An Experimental Approach

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Educational and skill divisions among workers are an increasingly important political cleavage in advanced democracies. We provide the first experimental analysis of the effects of unemployment risk and unemployment insurance generosity on workers' investment in job-specific skills. Using both laboratory and online samples, we find that, even in highly permissive contractual environments, more generous unemployment insurance leads to a greater level of investment in task-specific skills that risk obsolescence. Our experiment provides evidence supporting a key part of the “Varieties of Capitalism” approach to political economy while also finding several behavioral deviations from standard human capital theory.

JEL codes: J24, J65, P16

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# 1 Introduction

Education and skill differences have re-emerged as a core political cleavage across advanced democracies in the past few decades ([Abou-Chadi and Hix, 2021](#); [Gethin, Martínez-Toledano and Piketty, 2022](#)). How these differences emerge, reproduce themselves, and feed back into the political system has occupied scholars of advanced capitalism for some time ([Iversen and Soskice, 2019](#); [Iversen and Rhem, 2022](#); [Thelen, 2004, 2012](#)). In particular, scholars in the Varieties of Capitalism (VoC) framework argue for the importance of labor market institutions in shaping the incentives individuals face to acquire particular types of skills. ([Goergen et al., 2012](#); [Hall and Soskice, 2001](#)). Consequently, understanding how institutions frame educational choices is crucial in assessing the ways in which skill cleavages emerge and play out politically across the advanced industrialized world. Yet there is uncertainty about whether labor market institutions actually play their expected role in shaping educational choice. In this paper we provide original experimental evidence supporting the key VoC assumption that generous unemployment insurance stimulates investment in specific skills.

In the Varieties of Capitalism framework, employment protection and unemployment insurance shape investment in skills. Specifically, workers will be more willing to invest in firm- or technology-specific skills when workers are better insured against the economic risks of redundancy and skill obsolescence ([Estevez-Abe et al., 2001](#); [Iversen and Soskice, 2001](#); [Mares, 2000](#); [Wasmer, 2006](#)). Consequently, firms become more willing to develop and use technologies with strong skill complementarities because firms can count on a workforce willing to acquire specific skills. And workers will, in turn, lend greater support to labor market protections and generous social insurance benefits. This virtuous circle relies on the hitherto untested assumption that workers react to more generous unemployment insurance by making risky skill investments.

This line of reasoning around skills has proven influential, in part because it implies that institutions and policies reducing economic risk and promoting relatively egalitarian outcomes can support highly productive and innovative economies

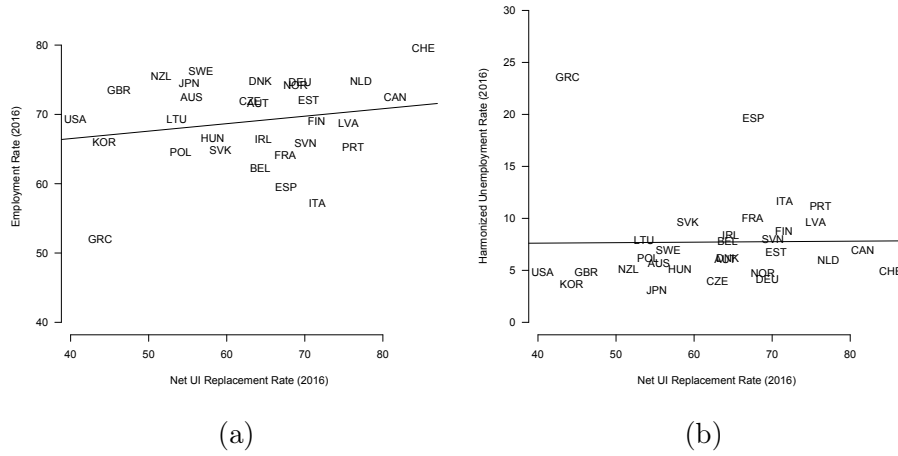


Figure 1: The 2016 employment rate (a) and Harmonized Unemployment Rate (b) against the UI net replacement rate across OECD countries. The rate is for the initial quarter of an unemployment spell for a married worker who earned the average wage with an out-of-work spouse and two children. The line is a bivariate regression. Data from [OECD \(N.d.\)](#).

([Iversen and Soskice, 2019](#)). As an illustration, Figure 1 displays the generosity of unemployment insurance (UI), as measured by the net replacement rate, against the employment and unemployment rates among a set of rich democracies. The lack of apparent correlation between UI replacement rates and (un)employment rates suggests that good labor market performance is possible under generous social insurance regimes. Rather than weakening the incentive to work by creating moral hazard, generous unemployment insurance may enable workers to make productive investments that encourage higher long-run activation in the labor market and retention by firms.

The Varieties of Capitalism framework has provoked a spate of empirical studies around social insurance and skills. Most of these have focused on evaluating the relationship between workers' skills and survey based measures of support for social insurance and redistribution ([Ahlquist, Hamman and Jones, 2017](#); [Cusak, Iversen and Rehm, 2006](#); [Gingrich and Ansell, 2012](#); [Iversen and Rhem, 2022](#); [Nickelsburg and Timmons, 2012](#); [Rehm, 2009, 2011](#); [Sjöberg, 2008](#); [Timmons and Nickelsburg,](#)

2014). These studies face a series of empirical and conceptual challenges. Defining and measuring “skills” is hard, something well-documented across the social sciences. But measuring the degree to which a skill is specific to a single firm or technology is extraordinarily difficult at scale. Existing research relying on observational data turn to proxies for skill specificity, such as industry-level indicators of training or the observed frequency of job switching. This strategy is clearly unsatisfactory since we never actually observe workers’ “skills”; specificity is inferred (Culpepper, 2007). Moreover, existing research designs have not fully addressed questions of endogeneity and reverse causation.

Beyond the problems of endogeneity and measurement, the empirical literature has not yet established that more generous job protections do in fact stimulate worker investment in specific skills, a foundational component of the VoC framework and a precondition for the hypothesized skills-preferences link. In a review, Schmieder and von Wachter (2016) state “whether UI actually leads to a crowd in or crowd out of human capital, and if so of what kind, is an interesting open question.” (567) We follow Charness and Kuhn (2011) and turn to experimental settings in order to provide evidence about whether and how this crucial VoC mechanism functions.

In this paper, we develop a model of unemployment insurance and skills investment. We then report the results of an experiment designed to mimic key aspects of the relationship between unemployment insurance and acquisition of specific skills. Our setup mirrors the three-state model of specific skills, general skills, and unemployment in Iversen and Soskice (2001).

In the experiment, participants earn resources by performing a tedious task (“sliders”) for several rounds under (exogenous) threat of unemployment and, if subsequently re-employed, working at an alternate task. Before beginning the task, we inform subjects of the risk of unemployment and the generosity of any unemployment insurance benefit. We then ask subjects whether they wish to invest some of their resources up front so that they will earn a higher per-unit wage for the slider task, but not for the alternate task. Hence, investing in the slider task mimics the acquisition of a “specific” skill.

Using both laboratory and online samples—the latter meant to minimize concerns

with external validity—we randomly vary both the incidence of unemployment and the generosity (i.e., replacement rate) of any UI scheme, examining whether participants are more or less willing to make a costly investment in a skill that only pays off when performing a particular task.

We choose values for the experimental parameters so as to create opportunities to discover whether there are any important “behavioral” deviations from the benchmark of human capital theory. A null result in this experiment would cast doubt on a core tenet of the VoC framework. A positive result puts the VoC framework on sounder behavioral foundations while also raising research and policy questions surrounding how the structure of a UI scheme would optimally relate to skill acquisition.

Our core finding is that more generous UI increases the probability that subjects give up resources to improve their productivity at a specific task, consistent with the VoC framework and providing an empirical basis for research on worker preferences. Specifically, subjects in the most generous UI regime are 26% more likely to acquire what we call “specific skills” compared to a no-insurance regime. We see little evidence that skill acquisition responds to unemployment risk, regardless of UI regime, which points to a potentially important “behavioral” component in how human capital choices respond to social insurance policies.

The experiment focuses on skills acquisition, but we also pre-registered a plan to examine whether there are any UI treatment effects on labor market frictions and task effort. For completeness, we summarize those secondary findings.<sup>1</sup> We find no evidence that more generous UI causes labor market frictions in the form of workers waiting longer to take a new job after an unemployment spell, although the waiting analysis suffers from small sample sizes. We also fail to uncover any relationship between UI and effort at the task, consistent with the [Burda, Genadek and Hamermesh \(2020\)](#) time use study.<sup>2</sup>

In addition to a direct connection to the VoC approach, this study also con-

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<sup>1</sup>Analysis and detailed results appear in Appendix sections [10](#) and [11](#).

<sup>2</sup>However, our core task involves “sliders” ([Gill and Prowse, 2011, 2012](#)), in which there may be a weak effort elasticity in response to changing monetary incentives ([Araujo et al., 2016](#)).

tributes to the empirical study of UI. Recent papers in this area, both observational (Farooq, Kugler and Muratori, 2020; Nekoei and Weber, 2017) and lab experimental (Lechthaler and Ring, 2021), focus almost exclusively on the effect of benefit *duration* on outcomes such as job search effort, match quality (measured by wages), and the length of unemployment spells. We extend the literature by looking at benefit replacement rates and explicitly focusing on skills.<sup>3</sup> In the conclusion we discuss how our findings might connect with other aspects of the empirical UI literature in future research.

In the remainder of this paper, we lay out a simple theoretical model from which we generate intuition and derive benchmarks from the perspective of human capital theory. We then describe the experiment. In section 4 we present our results while the final section concludes. Some extensions of the formal logic to account for risk aversion as well as supplementary analysis and sample description are found in the appendices.

## 2 A Model of Specific Skills, Risk, and Insurance

In this section, we analyze a simple decision-theoretic model of skill investment in order to provide benchmarks for interpreting experimental results. In both the model and the experiment we ignore equilibrium labor market dynamics, wage bargaining concerns, and the “hold up” problem associated with investment in co-specific assets. Instead we focus on worker behavior under specific circumstances. Specifically, we assume that workers confront a fixed and exogenous wage that depends on both skills and effort. Similarly, workers know the risk of unemployment and skill obsolescence. Our set up is analogous to a competitive labor market in which the worker funds skill acquisition and the firm can write wage contracts contingent on job-specific skills.

From the perspective of human capital theory, our experiments and the model here intentionally create a contracting environment that is “permissive” for skills acquisition (Acemoglu and Autor, 2009; Acemoglu and Pischke, 1999). Any worker

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<sup>3</sup>Wiedemann (2021:ch.5) also looks at benefit generosity and shows that less generous UI benefits induce Americans to take on more unsecured debt when they become unemployed.

who wishes can acquire specific skills (workers are not credit constrained); there are no contractual problems with training; wages increase proportionately with productivity; and the risk of job loss is uncorrelated with skill specificity. We abstract away from important bargaining issues as well as political-economy concerns with funding for the UI program in order to concentrate on the behavioral aspects of UI, namely whether more generous UI induces a greater willingness to acquire skills with limited applicability. We also set aside other policy areas such as employment protection and skill certification that have also received substantial attention ([Harcourt and Wood, 2007](#); [Thelen, 2004](#)).

Our interest is on how exogenously-given risk and the UI environment affect workers' decisions to give up resources now in order to improve their productivity at a particular task. Our model examines workers' optimal effort choices in a multiple period model with the risk of job loss and receipt of unemployment benefits. Given these effort schedules, we turn to the initial choice over how much to invest in acquiring specific skills.

The model is one in which we imagine there are two jobs,  $s$  (for sliders, foreshadowing the experimental task) and  $a$  (for alternative). Productivity in  $s$ -jobs depends on whether the Worker has invested in job-specific skills whereas productivity in  $a$ -jobs is assumed to require only "general" skills that are evenly distributed in the population.

To derive expectations about the decision to invest in specific skills we begin by using a simplified model including only the  $s$ -job. We then extend the simple model to incorporate  $s$ - and  $a$ -jobs, to confirm these findings in the presence of general-skills jobs and to derive predictions about waiting in the event of unemployment. To fix ideas and connect the model with the subsequent experiment, we display a schematic diagram of the experiment in [Figure 2](#). In each of the subsections below we then link the stages of play and the parameters to their experimental values in [Figure 2](#).

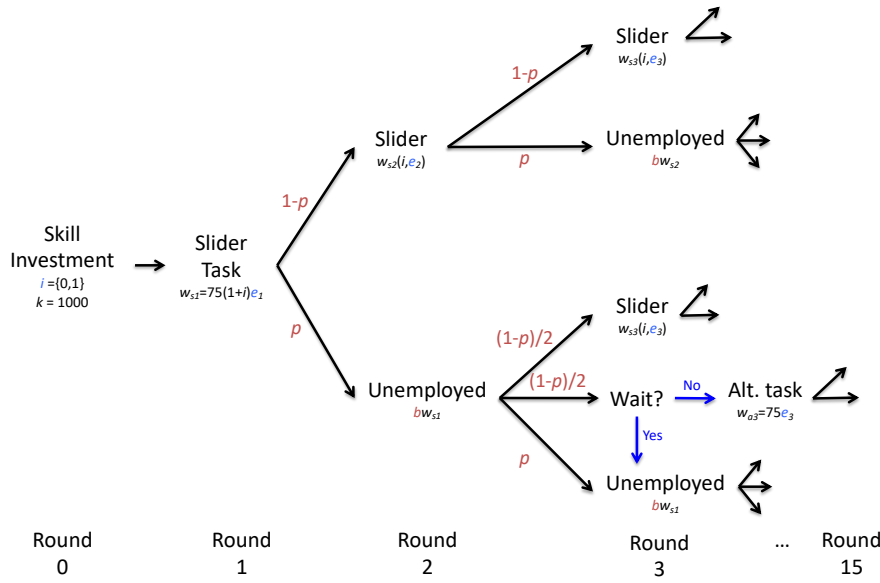


Figure 2: Schematic of the experiment. Text in **red** represents randomized quantities. Text in **blue** are choice variables for the experimental subject at that point. Values describe round-level payoffs (in ECU) associated with each branch of the tree. Compensation to subjects takes the average of three randomly chosen rounds.



## 2.1 Two Period, $s$ -Job Model

We begin by assuming a risk-neutral Worker and a single job,  $s$ . The Worker lives for two periods and there is a risk of unemployment in the second period. We describe a general model and then, at each stage, link the general model to the parameters of our experiment. To keep presentation and the experiment as stark as possible, we ignore time discounting and the funding mechanism for the unemployment insurance scheme.

$t = 0$  The Worker decides how much to invest in job  $s$ -specific skills. We denote the skill level the Worker chooses as  $i$ ; the Worker bears a cost  $\gamma(i)$  with  $\gamma(0) = 0, \gamma'(\cdot) > 0, \gamma''(\cdot) > 0$ . In the experiment  $i \in \{0, 1\}$  and  $\gamma(1) = k = 1000\text{ECU}$ .

$t = 1$  The Worker puts in effort  $e$ , earning wage  $w_{st} = w_t(i, e_t)$  with  $w$  increasing and weakly concave in both arguments. In the experiment,  $w_{st} = (1 + i)we_t$ , where  $w = 75\text{ECU}$  (the “general skills” wage) and  $e_t$  is the number of correct responses in round  $t$ . We assume that the worker faces strictly convex cost of effort,  $c(e_t)$ .

$t = 2$  With probability  $(1 - p)$  the Worker engages in another production round as just described. With probability  $p$  the Worker loses her job and receives unemployment benefit  $bw_{s1}$  where  $b \in [0, 1]$ . In the experiment we set  $b = \{0, 0.25, 0.75\}$ .

In the last period, the Worker’s investment decision is fixed and she simply equates marginal cost and benefit, so her optimal effort,  $e_2^*$ , solves  $\frac{\partial}{\partial e_2} w_{s2}(i, e_2) = c'(e_2)$ . In the first period, the Worker’s optimal effort has implications for her payoffs in the second period in the event of unemployment, so  $e_1^*$  solves  $\frac{\partial}{\partial e_1} w_{s1}(i, e_1) + bp \frac{\partial}{\partial e_1} w_{s1}(i, e_1) = c'(e_1)$ .<sup>4</sup> Focusing on  $e_1^*$ , we see that optimal effort is increasing in both  $b$  and  $p$ . Further inspection reveals that, if the cross-partial of the wage function is positive, then

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<sup>4</sup>In this simple model,  $e_1^* \geq e_2^*$ , with this difference increasing in  $bp$ . But this is purely due to our assumption of a two period game. In the experiment, we pay based on randomly selected rounds so  $e_1^*$  is the empirically relevant quantity.

the Worker with  $i > 0$  will exert more effort than if she had chosen  $i = 0$ . In the experiment this condition holds.

**Remark 1.** *Worker effort is increasing in both  $p$  and  $b$ . Workers with job-specific skills will exert more effort in  $s$ -jobs than if they had not invested in skill acquisition. The effort responsiveness to  $bp$  will be greater for Workers who have invested in skills.*

In deciding whether to invest in job-specific skills, the Worker solves

$$\max_i (1 + bp)w_s(i, e_1^*(i)) + (1 - p)w_s(i, e_2^*(i)) - \gamma(i) \quad (1)$$

The FOC for this program is

$$[1 + bp] \left[ \frac{\partial w_s(i, e_1^*)}{\partial i} + \frac{\partial w_s(i, e_1^*)}{\partial e_1^*} \frac{\partial e_1^*}{\partial i} \right] + [1 - p] \left[ \frac{\partial w_s(i, e_2^*)}{\partial i} + \frac{\partial w_s(i, e_2^*)}{\partial e_2^*} \frac{\partial e_2^*}{\partial i} \right] = \gamma'(i) \quad (2)$$

Implicit differentiation of Equation 2 shows that the optimal investment,  $i^*$ , is increasing in  $b$ —more generous UI produces higher rates of skill investment.<sup>5</sup> The relationship between  $p$  and investment depends on  $b$  and the relative responsiveness of effort to skills in the first period versus the second. Using the experimental values, a risk neutral subject will invest in job-specific skills if  $k < w[(1 + bp)(2e_1^*(1) - e_1^*(0)) + (1 - p)(2e_2^*(1) - e_2^*(0))]$ . This also implies that the effect of unemployment insurance generosity,  $b$ , on investment in skill is increasing in the level of unemployment risk, that is,  $\partial^2 i^* / \partial b \partial p > 0$ .

**Remark 2.** *A risk-neutral Worker's investment in job-specific skills,  $i$ , is increasing in the UI replacement rate,  $b$ . If there is no UI ( $b = 0$ ), then higher risk of unemployment/skill obsolescence,  $p$ , will reduce skill investment. As the UI replacement rate grows the relationship between risk and investment turns positive. As risk*

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<sup>5</sup>In Appendix 6, we extend this model to include the case of risk-averse workers, where we show that higher levels of unemployment insurance  $b$  will produce higher levels of investment unless workers are extremely risk-averse and that higher unemployment insurance always makes existing investment more valuable. This set-up with risk-aversion more closely approximates the model of Iversen and Soskice (2001).

*increases, the positive relationship between UI generosity ( $b$ ) and investment ( $i$ ) becomes stronger.*

## 2.2 Multiple Jobs Model

We now adapt the model to examine the case where an unemployed Worker is probabilistically offered a new job, which can be either the same  $s$ -job that employed (and remunerated) their job-specific skills or an alternative  $a$ -job in which past skills investments are irrelevant. We begin by noting that this does not alter any of our conclusions from the two-period model. We then extend the model to consider the case where there are more than two periods and hence the possibility of reemployment in either the general or specific skills job after a period of unemployment. We focus here on the choice of the Worker whether to accept the general skills job if offered or whether to remain unemployed and await a potential new job offer—either specific or general skills—in the next round. Our interest is in how the unemployment benefit and risk parameters affect the incentive to wait for a preferred offer.

We begin by altering the two period model such that in the second period, workers still face a risk of unemployment, with benefits dependent on round one earnings and the benefit rate. However, we alter the nature of second round employment, such that with probability  $(1-p)(1-q)$  the Worker remains in the  $s$ -job and with probability  $(1-p)q$  they are employed in an  $a$ -job that pays the same wage rate regardless of the investment (“general skills”). Accordingly the choice of effort in the general skills job,  $e_{g2}^*$ , is independent of the level of investment. If we consider the choice over whether to invest in job-specific skills this now becomes:

$$\max_i (1 + bp)w_s(i, e_1^*(i)) + q(1-p)(w_g(e_{g2}^*)) + (1-q)(1-p)w_s(i, e_2^*(i)) - \gamma(i) \quad (3)$$

And, choosing the rate of initial investment, the FOC for this program becomes:

$$[1+bp]\left[\frac{\partial w_s(i, e_1^*)}{\partial i} + \frac{\partial w_s(1, e_1^*)}{\partial e_1^*} \frac{\partial e_1^*}{\partial i}\right] + [1-q][1-p]\left[\frac{\partial w_s(i, e_2^*)}{\partial i} + \frac{\partial w_s(1, e_2^*)}{\partial e_2^*} \frac{\partial e_2^*}{\partial i}\right] = \gamma'(i) \quad (4)$$

Note that the only difference is the term  $(1 - q)$ , which implies  $\partial i^* / \partial q < 0$ , that is, a higher chance of receiving the general skills job reduces the incentive to invest in skills, as expected. Note that  $q$  does not vary in our experiment, so testing this implication will await subsequent work.

We obtain more nuanced results when we move beyond a two period model and look at the incentive to wait for an  $s$ -job offer in any given period of unemployment. That is, in a period of unemployment, there is a probability  $q(1 - p)$  of receiving an offer for an  $a$ -job and probability  $(1 - q)(1 - p)$  of receiving an offer of a  $s$ -job. Assuming a steady-state choice, we set effort to a uniform level across periods and normalize it to one, hence  $e_t^* = 1$ .

In Appendix 7 we develop a full steady-state model of the choice to wait. For clarity, here we focus on the single-period choice of whether to accept an offer or wait. If we permit the Worker to choose whether to accept a given offer or to remain unemployed we can characterize this choice as comparing the benefits of waiting to not waiting when offered an  $a$ -job:

$$(1 + i)wb + \left( (1 - q)(1 - p)(1 + i)w + q(1 - p)w + p(1 + i)wb \right) \quad \textbf{Waiting} \quad (5)$$

$$w + \left( (1 - p)w + pwb \right) \quad \textbf{Taking the } a\text{-Job} \quad (6)$$

This can be simplified to choosing to wait if the following condition holds, where  $\Omega$  represents the relative value of waiting:

$$\Omega = (1 + i)b + i \left( (1 - q)(1 - p) + pb \right) > 1 \quad (7)$$

With this condition in hand we can state the following remark based on simple

comparative statics analysis.

**Remark 3.** *More generous UI and having already invested in specific skills each increases the attractiveness of rejecting an  $a$ -job and waiting for the arrival of an  $s$ -job upon experiencing a spell of unemployment. A higher probability,  $q$ , of receiving an  $a$ -job reduces the benefits of waiting. The relationship between unemployment risk and the benefits of waiting, is positive if  $b + q > 1$ .*

Making the skills investment implies that waiting is more attractive, since an  $a$ -job does not reward the investment, whereas waiting might mean being offered an  $s$ -job in the future.<sup>6</sup> Similarly, more generous UI incentivizes waiting since it improves both the current relative value of remaining unemployed versus taking the  $a$ -job and the future value of becoming unemployed.<sup>7</sup> For those who invested in  $s$ -skills, a higher probability of receiving an offer for an  $a$ -job reduces the incentive to wait because there is less chance of being employed in an  $s$ -job in the future.<sup>8</sup>

Finally, the effect of greater unemployment risk on the incentive to wait is more complicated, at least for those who make the skills investment (those who don't are always better off not waiting).<sup>9</sup> For those who make the investment, higher risk means less steady state chance of being in the  $s$ -job, and therefore an incentive accept the  $a$ -job at the first opportunity. But these Workers also benefit from having higher unemployment benefits (which are proportional to  $i$ ) and hence can endure longer unemployment spells. A higher probability of being made unemployed will increase the benefits of waiting, provided that  $b + q > 1$ .<sup>10</sup>

### 2.3 Key Benchmarks

We conclude by recapping the key implications derived from the model in terms of the choice about whether to give up resources to acquire specific skills, the probability of

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<sup>6</sup> $\partial\Omega/\partial i = b + ((1 - q)(1 - p) + pb) \geq 0$ .

<sup>7</sup> $\partial\Omega/\partial b = (1 + i) + ip > 0$

<sup>8</sup> $\partial\Omega/\partial q = -i(1 - p) \leq 0$ .

<sup>9</sup> $\partial\Omega/\partial p = i(b - (1 - q)) \geq 0$ .

<sup>10</sup>With our experimental parameters of  $q = 0.5$  and  $b \in \{0, 0.25, 0.75\}$ , this is only true for the highest level of benefit generosity.

choosing to wait if unemployed, and the effort exerted during the task. We view these implications not so much as predictions as benchmarks against which to interpret experimental results. Table 1 outlines the relevant comparative statics.

**Investing in Specific Skills:** Workers will be more likely to give up resources to acquire specific skills when the generosity of unemployment benefits are higher because benefits are proportional to incomes, which are higher with the investment. In the absence of unemployment benefits, higher unemployment risk implies a lower likelihood of skills acquisition because there is less chance of obtaining the benefits but the costs are still incurred. As unemployment benefits rise in value, the relationship between unemployment risk and likelihood of skills acquisition becomes ambiguous since some of the investment is recouped in unemployment benefits. Finally, as unemployment risk rises, the positive relationship between unemployment benefit generosity and investment in skills should intensify.

**Choosing to Wait When Unemployed:** Conditional on experiencing unemployment, Workers who have made the investment in specific skills will be more likely to reject  $a$ -jobs and wait for an offer of an  $s$ -job. Higher unemployment benefit generosity will increase the incentive to wait, as it improves the expected value of remaining unemployed. A higher risk of unemployment by contrast only increases the incentive to wait when  $b + q > 1$ ; this implies that, all else equal, waiting should be higher in the high-risk/generous UI condition compared to the low-risk, generous UI condition of our experiment.

**Task Effort:** Workers will exert more effort during either task when unemployment risk is higher because they have fewer expected rounds of work. Workers will work harder when unemployment benefit generosity is higher since benefits are proportional to earnings during periods of work. Finally, when employed in specific-skills tasks, those workers who acquired specific skills will work harder than those who did not, since their return to effort will be higher in such tasks.

Table 1: Summary of Comparative Statics

	Choice Variables		
	Investment ( $i$ )	Choice to Wait ( $\Omega$ )	Effort ( $e_t^*$ )
<b>Parameters</b>			
Unemployment Risk ( $p$ )	$\uparrow\downarrow$	$\uparrow\downarrow$	$\uparrow$
UI Generosity ( $b$ )	$\uparrow$	$\uparrow$	$\uparrow$
Prob. Alternate Job ( $q$ )	$\downarrow$	$\downarrow$	$\downarrow$

### 3 Experimental Design

#### 3.1 Subject recruitment

As part of our pre-declared experimental design, we ran experimental sessions in both lab and online environments. For the lab-based sessions, we recruited subjects from Oxford’s Centre for Experimental Social Sciences (CESS) subject pool. About half of the subjects in the pool are Oxford undergraduates with the other half hailing from the Oxford, UK region. These subjects engaged with the experiment in a controlled laboratory environment via personal computer.

We also recruited subjects from a pool of mTurk workers. External validity and cost considerations led us to recruit subjects from the mTurk channel. Specifically, the task and contracting environment on the mTurk online labor platform is similar to the contracting and work environments we present to our subjects, reducing concerns about an otherwise abstract experimental protocol. All mTurk subjects were over 18 years of age, located in the USA, and had a history of good performance at previous mTurk tasks.<sup>11</sup>

To verify that subjects reported no problems with the experimental protocol while also maintaining closer experimental control, we conducted all in-person lab sessions before taking the experiment to mTurk. No data summaries or analysis

<sup>11</sup>Specifically they had completed at least 500 mTurk tasks with a 95% or better approval rate.

was conducted prior to completing the mTurk sessions. Following our pre-declared research design, we will estimate models that include indicator variables for the lab or mTurk context as well as cluster standard errors by treatment and context. We break out key findings by context in the supplemental materials in Appendix 9.3.

Table 2 displays the distribution of subjects across treatment conditions and status. The unequal numbers of subjects across treatment conditions and contexts (lab v. online) is due to the distribution of subjects who appeared in the lab as well as the randomization across mTurkers.

Table 2:  $3 \times 2$  Experimental Design with  $N$  from each subject pool

	No UI ( $b = 0$ )	Minimal ( $b = .25$ )	Generous ( $b = .75$ )
Low unemployment ( $p = 10\%$ )	65 (mTurk) 44 (CESS)	77 (mTurk) 35 (CESS)	58 (mTurk) 40 (CESS)
High unemployment ( $p = 25\%$ )	86 (mTurk) 43 (CESS)	77 (mTurk) 46 (CESS)	91 (mTurk) 32 (CESS)

The basic structure of our experiment was a consent process followed by an introduction to the experiment and comprehension check.<sup>12</sup> Subjects then participated in 15 rounds of the experiment described below. Each round lasted 60 seconds. Following the experimental rounds, subjects completed a basic demographic and opinion questionnaire, including a gamble to elicit risk aversion preferences, and then they were paid. We provided all participants with the IRB-approved study description; all subjects indicated their consent before proceeding. We paid subjects a participation fee as well as any additional earnings accumulated during the experiment.<sup>13</sup> We informed subjects that they would be doing a series of tasks and that their earnings in

<sup>12</sup>Before starting the experimental portion of the session, subjects face a quiz asking them (a) the size of their endowment, (b) the probability they will be ineligible to play a given round, (c) the percentage of their last round’s earnings they will earn if ineligible, (d) and the multiple of earnings they will receive in the slider task should they choose to invest their account. Regardless of lab or mTurk format, subjects must answer the quiz correctly to proceed. All experimental instructions are collected in the supplemental materials.

<sup>13</sup>The show up fee for the lab was £5, payable regardless of whether the subject completed the experiment. mTurkers received \$1, paid on completion.



these tasks are denominated in an artificial currency, Experimental Currency Units (ECU). The conversion rate of ECU into British pounds and US dollars was noted at the start of the experiment.<sup>14</sup> Our study involves no deception.

### 3.2 *Real effort tasks*

We employ two “real effort” tasks to induce the psychological experience of working at an unpleasant task for pay while also creating a direct link between earnings and the experienced productivity at a specific task. Although we use tasks that require effort, our primary interest is *not* worker productivity at the task.

At the core of the experiment are 15 one-minute rounds in which a subject undertakes either the “slider” or the “alternate” task. In the slider task subjects saw a screen with up to 60 sliders.<sup>15</sup> Sliders do not display some of the gender and cultural biases of other experimental real effort tasks, such as math or spelling problems. According to [Charness, Gneezy and Henderson \(2018\)](#), the slider task shows little evidence of pre-existing heterogeneity in skill or ability. Sliders also present little scope for in-experiment learning, mitigating any concern with intrinsic motivation or the inherent pleasure connected with skill acquisition or learning that might confound our interpretation of skill acquisition ([Charness, Gneezy and Henderson, 2018](#)). Nevertheless there is evidence that sliders are weak tools for examining how effort responds to incentives, especially when subjects are forced to do nothing when “unemployed” ([Araujo et al., 2016](#)). In our study, effort is of only secondary interest and subjects were able to engage in the alternate task when not employed at the sliders task.

Figure 3 displays a screen shot of the slider task. Each slider is initially positioned at a random position between 0 and 100 and the screen indicates the target for each slider. Subjects use the mouse to reposition each slider as many times as they wish. A subject’s effort in round  $t$ , denoted  $e_t$ , will be the number of sliders on that page positioned at the target values at the end of one minute.

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<sup>14</sup>300ECU:£1 in the lab and 600ECU:\$1 on mTurk.

<sup>15</sup>On the slider task, see [Gill and Prowse \(2011, 2012\)](#). See [Ahlquist, Hamman and Jones \(2017\)](#) for a recent application of this real effort task in the context of social insurance programs.

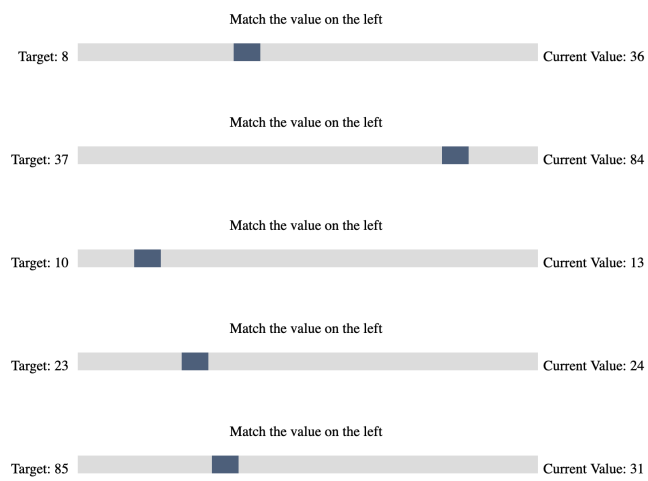


Figure 3: A screen shot of the slider task.

We incorporate the effects of skills by directly altering the per-slider wage. Subjects earn wages at the rate of 75ECU for every correct slider. Those who invest in the slider task-specific skill ( $i = 1$ ) will see their piece rate wage doubled to 150ECU per correctly placed slider. “Skilled” subjects are therefore twice as productive as the “unskilled,” but only in the slider task.<sup>16</sup> Note that in our set up, the worker’s wage directly reflects her skill investment with no uncertainty. In other words, we assume an environment in which the “hold up problem”—often used to justify UI in the VoC literature—is absent, as is any problem in verifying a worker’s skills.

The alternate task, for which there is no possible “skill” in our experiment, presents subjects with a list of twelve random digits and asks subjects to report the number of times a particular (randomly chosen) digit appears. A response is correct if the subject correctly reports the number of times the digit appears in that string. Workers earn 75ECU for every correct value in the alternate task. For all subjects, experimental earnings are an average of earnings from three randomly selected rounds.

### 3.3 *Experimental protocol*

Recall that Figure 2 in Section 2 displays the experimental protocol in schematic form. The blue text identifies choice variables for the subjects. The red text identifies randomized quantities. We randomized two variables at the individual subject level: unemployment risk ( $p$ ) and the generosity (replacement rate) of the unemployment insurance scheme, denoted  $b$ . The risk of unemployment is the fixed and known probability that a subject will lose an earnings round at any point in time. At the beginning of the experiment, we set  $p$  randomly with equal probability to one of two values: low (10%) and high (25%). Although we do not specifically calibrate these values to real-world aggregates, we note that the low value corresponds to the

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<sup>16</sup>We explored defining a subject’s score on the slider task as the proportion of sliders correctly placed, with skill investment reducing the denominator, increasing productivity. This approach proved to be a substantially more difficult programming implementation while also running the risk of imposing an upper bound on highly productive “skilled” subjects. In any event, halving the number of sliders on the page and paying on the basis of percentage correct is equivalent to doubling the wage per correct slider.

approximate pre-COVID unemployment rate in the Eurozone and the high value is approximately the rate in Greece during 2012-2016. We note (and emphasized to subjects) that the risk of unemployment is independent of task effort and any past unemployment spells.

UI generosity is the fixed and known proportion of a subject’s last earnings round that she receives in any round in which she finds herself unemployed. We set this randomly with equal probability to one of “none” ( $b = 0$ ), “minimal” ( $b = 0.25$ ) or “generous” ( $b = 0.75$ ). Neither the structure of the UI program nor the replacement rates are calibrated to exactly reflect specific, existing policies. Rather we structure the policy to make it simple to understand and set parameter values so as to fall within the ballpark of real world quantities. The minimal UI value approximates what many OECD countries deliver over five years of unemployment. The generous UI value approximates what the most generous systems deliver early in an unemployment spell. For the purposes of simplicity in this experiment, we assume that workers do not directly contribute to funding insurance.<sup>17</sup>

The task a subject does may change during the experiment, but all subjects begin with sliders. To change tasks, a worker must pass through a spell of unemployment in which they lose an opportunity to work but earn the UI benefit. When offered a new job, the worker is offered the alternate task with probability  $q = 1/2$  and the slider task with probability  $(1 - q) = 1/2$ .

Accordingly, upon experiencing a spell of unemployment, the worker confronts different situations: with probability  $p$  the subject remains unemployed and loses the next round (but continues to earn UI benefits); with probability  $(1 - p)/2$  subject is again employed in the slider task (where their previous skill investment, if any, persists); and with probability  $(1 - p)/2$  the subject is offered a chance to work at the alternate task. When presented with the alternate task, subjects are told that this task involves solving simple problems; that the payment scheme remains the same; and that their skill investment does not apply in the alternate task. When offered

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<sup>17</sup>We think of this as analogous to a system in which UI is funded through employer contributions or general government revenue. In future work we may incorporate a payroll tax on workers to fund insurance.

the alternate task, a subject can either accept the opportunity or they can choose to *wait*, in which case they forfeit the round (but earn UI benefits). This process repeats for 15 rounds.

Before beginning production rounds but subsequent to a practice round on the slider task, subjects have the choice of whether to give up their initial 1000ECU endowment; this initial investment cost is denoted  $k$ . Given the chosen values of  $b$  and  $p$ , we fixed  $k$  at approximately the level where a risk-neutral participant facing low unemployment would be roughly indifferent between investing or not, assuming an average effort level of 15 sliders.<sup>18</sup> We inform subjects that if they invest their endowment they will see their per-correct-slider wage doubled. They are then informed how much they would have earned in the practice round had they made the investment. We tell subjects that they must choose whether to make the investment before starting round 1 and that their choice will be fixed for the entire experiment. Those not making the investment retain their 1000ECU endowment and see it added to their earnings from the experimental rounds.

### 3.4 *Measurement and variables*

#### 3.4.1 Outcome variables

We examine two policy-relevant outcome variables in the main text: whether subjects invest in the task-specific skill and whether subjects who are subjected to unemployment voluntarily extend their unemployment (“waiting”).<sup>19</sup>

Subjects’ choices over skill investment is the primary outcome of interest. We measure this as a binary individual-level variable indicating whether subject  $i$  chose to give up their initial endowment for better “skills” at the slider task. We measure waiting in two ways. First, as our preferred measure, we use a binary indicator

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<sup>18</sup>Among experimental subjects, we observed mean and median slider task effort levels of 12.9 and 13.3, respectively. The maximum number of correct sliders in any one round was 42.

<sup>19</sup>We also study observed task effort, as proposed in our registered research design and highlighted in the theoretical model. For space considerations and because slider tasks may be underpowered when examining effort (Araujo et al., 2016), we report analysis of effort in the supplemental materials section 11.

taking the value of 1 if a subject ever voluntarily extends any of her unemployment spells and 0 for those who always choose to end their unemployment spell at the first opportunity. Second, we use the count of subject-specific unemployment spells in which the subject chooses to prolong their unemployment duration. Both variables are undefined for any subject not experiencing any spells of unemployment.

### 3.4.2 Covariates

We administered a survey to all subjects upon completion of the experimental session. We collected a variety of covariates that we use to test for failure in the randomization procedure, reduce the variance of estimated treatment effects, and characterize any interesting environmental predictors of behavior in the experiment. Figure 5 in the supplemental materials reports covariate balance across treatments. We have no reason to believe there were any problems in treatment randomization.

In the analysis below, we report models including only randomized quantities as well as those that adjust for pre-declared covariates.<sup>20</sup> age, gender (identifies as female or not), education (has a college degree or not), race (white or not), whether the subject is unemployed in “real life”, whether the subject lives in an urban area, household income (in quintiles), and risk aversion<sup>21</sup>

## 4 Analysis and Results

We begin with our core outcome—whether to invest in the task-specific skills—before turning to a brief discussion of voluntary extensions of unemployment spells and a task effort. In the main text we report results pooling our laboratory and mTurk

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<sup>20</sup>In our pre-analysis plan we also stated that we would condition on the respondent’s frequency of video game play, as those who frequently play video games might be better at the slider task. This question was not included in the standard survey so this covariate is not included.

<sup>21</sup>We measure risk aversion using incentivized gambles administered in the post-experiment survey. We map the choices into the risk aversion parameter under a CRRA utility function. The outcomes of these gambles alter the final payouts to the subjects but gamble outcomes are not drawn from experimental earnings nor were subjects’ total compensation allowed to fall below the CESS-established floor.

data and then including relevant indicators for context in models that adjust for covariates. In the supplemental materials (9.3) we report results for the investment decision separately for the lab and mTurk samples.

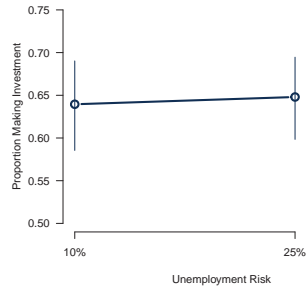
#### 4.1 *Acquisition of Specific Skills*

Recall that our factorial design splits out two key dimensions: the risk of unemployment (10 or 25 percent) and the level of unemployment insurance (zero, 25, or 75 percent of the previous employed round’s earnings). Before turning to combinations of these treatments (that is the full  $2 \times 3$  factorial design) it is instructive to examine the effect of unemployment risk and UI generosity in turn.

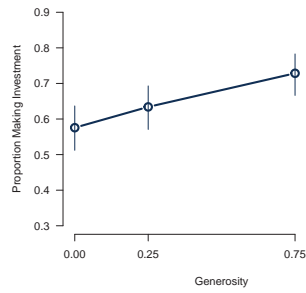
Figure 4a demonstrates that unemployment risk alone does not appear to alter participants’ investment choices. Regardless of unemployment risk, around 64% of our participants chose to make the investment, with the confidence intervals around these estimates far larger than the one percent point difference in investment probabilities. Thus, there is no indication that risk alone drives investment behavior.

However, when we turn to unemployment *insurance* we do find evidence that more generous UI benefits induce higher rates of skill investment. Figure 4b displays the proportion of subjects investing in skills at the three UI generosity levels. We find a clear positive relationship, with the investment proportions rising from 58% (under no UI) to 73% under the generous scheme. In the Appendix, several detailed regression analyses in Section 9.2.1 show that the generosity of UI benefits has a consistently statistically significant positive effect on the choice to invest.

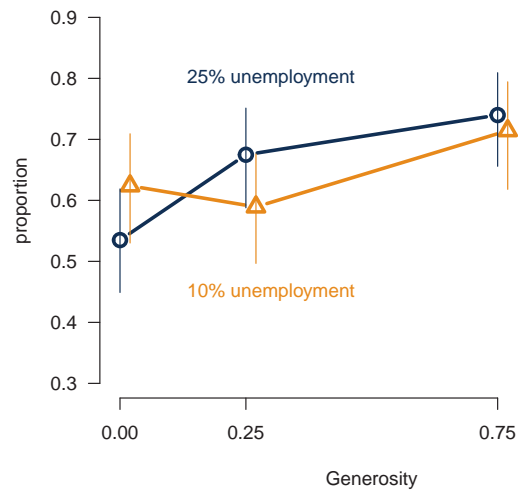
We now turn to the full  $3 \times 2$  factorial design. Figure 4c shows the proportions investing across all six treatment combinations, with the higher unemployment risk treatments in dark blue and the lower unemployment risk treatments in orange. Generous UI conditions (Low Generous and High Generous) have the highest proportion of subjects investing. The lowest investment rates occurred when unemployment risk is high but there is no UI (High None). These treatments closely match expectations in one respect: generous UI encourages acquisition of task-specific skills. Although we see that skills investment is least desirable when the risks of job loss are high



(a) pooled across risk



(b) pooled across UI levels



(c) Investment decision

Figure 4: The proportion of subjects giving up 1000ECU to acquire slider-specific skill, as a function of experimental treatments. Vertical bars represent 95% binomial confidence intervals. There is no effect of unemployment risk and an approximately linear treatment effect of UI generosity.



and there is no UI, there does not appear to be a detectable mean-shift between risk treatments. Nor is there clear evidence of an interactive relationship between risk and generosity. We turn to a regression approach for estimating and reporting treatment effects. In Appendix section 9.1, we report standard  $p$ -values for all pairwise differences in proportions as well as  $p$ -values controlling the false-discovery rate given that these are multiple comparisons. All inference and  $p$ -values reflect two-tailed tests.

Following our pre-analysis plan, we estimate both linear probability and logistic regression models on an indicator for whether a subject chose to invest their initial endowment. For ease of presentation and interpretation, we report just the LPM results here.<sup>22</sup> Logit results appear in Appendix section 9.2.2. Again following the pre-specified analysis, we report standard errors clustered by treatment-context.<sup>23</sup>

Table 3 reports LPM coefficients. The first model includes only the experimental treatments, entered categorically with “low-none” as the reference. In the second model we enter each treatment condition as the relevant numerical quantity since these values have a meaningful interpretations and we have evidence that treatment effects are approximately linear in generosity (Figure 4b). The third model looks at whether there is any interaction between the treatments and the fourth model looks at whether the experimental context (a dummy for mTurk) had any moderating effects on the treatments. The fifth model includes the battery of pre-specified, pre-treatment covariates.<sup>24</sup>

In the regression analysis framework we see that more generous UI increases the chances of skill acquisition. It is difficult to discern any effect of unemployment risk treatment. By way of effect sizes, we find that subjects in the generous UI setting are about 15 percentage points (26%) more likely to acquire specific skills compared to subjects in the no UI treatment, holding fixed the risk environment.

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<sup>22</sup>In this case, the LPM produced well-calibrated predicted probabilities and estimated treatment effects are nearly identical between the two estimation strategies

<sup>23</sup>In Appendix 9.2.1 we report LPM with unclustered but HC3 standard errors as well as logit models with classical standard errors. This supplemental analysis was not pre-registered.

<sup>24</sup>Two subjects failed to answer the income question and one did not complete the risk aversion questions, leading to the exclusion of three observations.

Table 3: Linear Regression for Investment Choice

	base (categorical)	base (linear)	interacted	context	covariates
low-minimal	-0.03 (0.06)				
low-generous	0.09+ (0.05)				
high-none	-0.09* (0.04)				
high-minimal	0.05 (0.04)				
high-generous	0.12*** (0.03)				
unemployment rate		0.04 (0.23)	-0.21 (0.31)	-0.07 (0.27)	0.07 (0.22)
UI generosity		0.20*** (0.04)	0.06 (0.12)	0.28*** (0.04)	0.19*** (0.05)
generosity $\times$ rate			0.78 (0.54)		
mTurk				-0.03 (0.09)	-0.03 (0.04)
rate $\times$ mTurk				0.21 (0.40)	
generosity $\times$ mTurk				-0.12 (0.07)	
$n$	694	694	694	694	691
BIC	976.9	961.5	967.0	979.3	999.3

standard errors clustered by context-treatment in parentheses.

+  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

When experimental treatments are entered as categorical variables, we see some evidence that subjects in the high unemployment/no UI treatment were less likely to invest in skills than those in the low/none treatment. Other comparisons between high and low unemployment conditions for the same UI level showed no difference distinguishable from zero at conventional thresholds.<sup>25</sup> When combined with the differences-in-proportions evidence in figure 4b and the appendix, we conclude that there is little evidence that unemployment risk affected investment behavior.

Looking at the interactive models, we similarly find little evidence for more complex relationships. Echoing findings above and contrary to the implication in Remark 2, there is no evidence of an interaction effect between the two treatments and the inclusion of this interaction term does not improve the model’s ability to describe the experimental data. When we allow for systematic differences in investment and treatment effects based on experimental context (lab vs. mTurk), we see that UI generosity effect among the mTurkers was about 60% that of lab subjects. But the uncertainty around this estimate is considerable and the BIC as well as  $F$ -tests lead us to prefer the simpler model without the interaction. We conclude that the experimental context did not systematically moderate the UI treatment effect.<sup>26</sup>

Among the covariates (complete estimates available in Appendix table 5) we see that those with a college degree are more likely to acquire specific skills in this experiment while the more risk averse are less likely to do so.<sup>27</sup> Other covariates show no discernible association with our subjects’ choices to acquire skills.

## 4.2 Labor market “frictions”

In our study preregistration and pre-analysis plan, we highlighted that our experimental context allows us to speak to whether unemployment risk and UI generosity affects the duration of unemployment spells and work effort. Although these are not the primary outcomes of interest, we briefly discuss our findings for complete-

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<sup>25</sup>Using a Wald  $\chi^2$  test for equal coefficients based on the clustered covariance matrix.

<sup>26</sup>We report analysis broken out by context in Appendix 9.3. Note that this supplemental analysis was not included in the pre-analysis plan.

<sup>27</sup>There is also no evidence of an interactive effect between unemployment risk and risk aversion.

ness. Detailed results are relegated to the appendix (10 and 11). In discussing these issues, there are several important caveats about our experiment relative to common concerns in the UI literature: in our experiment relative wages do not change; benefit duration is not an issue; the arrival of new job offers is exogenous; and (un)employment is unrelated to task effort.

#### 4.2.1 Unemployment duration

All participants probabilistically received new “job offers” after a round spent unemployed. The offer may require work at a task where earlier skill investments are not valuable. Participants can refuse the offer and “wait”, i.e., sit out for another round. We measure waiting in two ways: the number of offers rejected (an integer variable) and whether they ever reject an offer (a binary variable).

We confront two limitations when looking at waiting behavior. First, due to a programming oversight, the data needed to observe waiting were *not* recorded for the mTurk sample, so we rely exclusively on the lab subjects. Second, it is only possible to observe waiting among subjects who actually experience a spell of unemployment before the final round. We therefore exclude the 38 lab respondents who never experienced unemployment. Both of these restrictions limit our sample size and therefore the precision of our estimates and the strength of the conclusions we can draw.

It is also important to note that subjects assigned to the high unemployment risk category will probabilistically have more *opportunities* to wait.<sup>28</sup> For this reason we expect to observe more waiting in the high unemployment treatments compared to the low unemployment conditions, even though risk averse subjects should be less likely to wait. To account for this, we condition on the observed number of employment offers as well as the unemployment risk treatment itself. Although the number of offers is clearly a post-treatment variable, adjusting for it allows us to distinguish between the effects of being in a high risk *environment* (the high risk treatment) versus actually experiencing adverse events.

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<sup>28</sup>Descriptive information about waiting counts appear in Appendix figure 7.

In the supplemental materials section 10, we report detailed analysis of waiting behavior. We show that, holding the number of offers at the median among those experiencing unemployment (2), subjects in the low risk condition were over 2.5 times more likely to have waited and had twice as many waiting events compared to a subject in the high risk environment. There is no evidence that UI generosity affects waiting, *contra* Remark 3. We also investigate the claim in Remark 3 that higher unemployment risk increases waiting if  $b + q > 1$ . Practically speaking, this implies that waiting should be higher in the high risk/generous UI treatment than in the low risk/generous UI treatment. We do indeed find that the difference in the proportion of subjects who waited at least once is greatest when comparing high risk/generous UI to low risk/generous UI. But this difference does not cross conventional thresholds for significance and it reverses after conditioning on the number of job offers.

#### 4.2.2 Task effort

We measure task effort as the number of accurate responses in a round, averaged over all employed rounds.<sup>29</sup> In the supplemental materials Section 11 we find that task effort was significantly lower in the high-risk treatment than under lower unemployment risk, consistent with Burda, Genadek and Hamermesh (2020) but *contra* Remark 1. Subjects in the high risk setting averaged 6 fewer correct sliders, about 1.5 standard deviations. We do find higher effort exerted by those who made the investment, consistent with Remark 1. Finally, we find no relationship between UI generosity and task effort and no evidence of any interaction effect between the risk and generosity treatments, again *contra* Remark 1, which anticipated a positive relationship between generosity and effort (especially under high unemployment risk). While these findings only offer limited support for our expectations they should be interpreted with caution, as slider tasks may be underpowered when examining the effort elasticity of incentive payments between subjects (Araujo et al., 2016).

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<sup>29</sup>Our measure of effort deviates from the pre-analysis plan (PAP). In the PAP we proposed two measures of effort, but the second proposed measure (number of sliders attempted) was not tracked in our software implementation.

### 4.2.3 Skill acquisition as mediator

Although we find no total effect of UI generosity on waiting or effort, it may be that acquiring specific skills moderates the effect of risk and UI generosity, as described in Remark 1 and 3. For example, more generous UI might induce waiting among those who invested in skills while decreasing waiting among those who did not. Labor market frictions could be severe: the most skilled workers sit out longer while less skilled workers are relatively more available.

First, we focus on the generous UI condition for the waiting outcome. Consider a subject who acquired slider-specific skills under the generous UI treatment. For her, it pays to wait whenever offered the alternate task.<sup>30</sup> In contrast, we should observe no waiting among any of the other subjects. This implies that those who invested in skills in the generous UI condition should wait about 1.31 times in the high risk setting and about 0.63 times in the low risk setting.<sup>31</sup> In fact, we observe an average number of waiting incidents of 0.4 among the skilled subjects in both the high and low risk treatments. This is *below* the 1.1 waiting instances we see among those who did not acquire specific skills in the generous UI-high risk setting. Those who invested in skills are waiting at lower than expected levels while those who did not are waiting at higher-than-expected levels.

In the supplemental materials (12), we report results from a formal causal mediation analysis (Imai, Keele and Tingley, 2010) for both waiting and task effort. The key assumption required is “sequential ignorability”, which, in this case, means that the choice to acquire skills is independent of waiting or effort outcomes, conditional on treatment and any other covariates. We find that unemployment risk produces a large and significant negative direct effect on both waiting and effort as reported above. There is no evidence that skill acquisition mediates this relationship. Interestingly, we find that, although UI generosity has a null total effect on both waiting and effort, acquiring skills produces a small but significant *negative* ACME on waiting

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<sup>30</sup>Specifically, so long as the number of correct sliders in the last employed round exceeds 2/3 of the number of (expected) correct answers at the alternate task, a subject who invested in skills in the generous UI condition should always wait when offered an alternate task job.

<sup>31</sup>Calculated as  $14p(1 - p)/2$

and a positive ACME on effort.

To sum up our most important result, we consistently find evidence that UI generosity produces an increased willingness to acquire task-specific “skills.” Although we do not make strong claims in this regard, we do note that in our experimental setting we find little evidence that worker effort or unemployment spells suffer as a result of more generous UI. Nevertheless, we also find several deviations from the benchmarks we derived from our simple decision theoretic model. These deviations highlight the presence of important “behavioral” factors at play that may be related to artificial experimental context or the nature of work, productivity and risk.

## 5 Conclusion

Does unemployment insurance affect workers’ willingness to give up money to acquire firm- or technology-specific skills? This question is difficult to answer because skill specificity is a tricky concept to measure at scale. We therefore construct a stylized experiment, implemented in both the lab and with online workers.

In our experimental set up, workers could invest resources in becoming more productive at one particular task. This increased productivity was reflected in a higher wage for those acquiring this specific “skill”. But workers could also become unemployed and then find themselves confronted with a different task that does not use their skills and pays a lower, “general skills” wage. We randomly varied the replacement rate of the UI program and the underlying risk of job loss and looked at whether subjects acquired specific skills. We found that more generous UI significantly increased the acquisition of specific skills, consistent with VoC claims, but had no discernible effects on waiting or on task effort.

Our findings provide some needed empirical support for key parts of the Varieties of Capitalism framework and skill-based theories of political economy. But we also failed to find some behaviors that would also be consistent with the broader human capital approach that underpins the VoC claims, as depicted in our simple decision theoretic model. These mixed findings then point to limitations in our approach that require further research in several areas to more fully test the VoC conjecture.

The first area was our conception of benefit generosity, which we implemented as a replacement rate on last period wages. Benefit duration is another important policy parameter associated with extensive research on job search and optimal UI design ([Farooq, Kugler and Muratori, 2020](#); [Lechthaler and Ring, 2021](#); [Nekoei and Weber, 2017](#); [Schmieder and von Wachter, 2016](#)). There is some evidence that longer benefit duration reduces job search effort but increases the quality of matches. Altering our experimental framework to incorporate endogenous job search and variable benefit duration along side skills investments could disentangle which aspect of benefit generosity shows the largest behavioral response. In the endogenous search context we would also be interested in varying the prevalence of high-skill jobs.

Another important policy parameter that bears experimental investigation is the maximum benefit rate. Our experiment placed no limit here, but real-world UI programs typically cap the UI benefit at some maximum. A third important extension involves the funding structure and visibility for the UI program. If benefits are funded through some visible tax on earnings then we may observe different behavior or different levels of support for UI. Fourth, access to credit may be viewed as a substitute for social insurance benefits that can also be used to fund human capital investments ([Ahlquist and Ansell, 2017](#); [Ansell, 2014](#); [Wiedemann, 2021](#)). Additional experimental work could pin down whether and how borrowing constraints affect skill acquisition in the face of different levels of UI generosity.

Finally, unemployment insurance is only one part of a broader institutional matrix that includes education systems, employment protection policies, wage bargaining, etc. We focussed on UI because it is a widespread and well-defined policy that can arguably stand on its own. But future behavioral work should examine whether and how changing UI policies may affect skill formation differently depending on how the broader political economy manages innovation, employment, and social solidarity.



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