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ABSTRACT

Can Temporary Wage Incentives Increase Formal Employment? Experimental Evidence from Mexico*

Formal sector entry-level jobs in Mexico offer low starting salaries but substantial wage growth. This paper experimentally tests whether a six-months wage incentive can increase formal employment among secondary school graduates. Combining survey and high-frequency social security data, the paper shows that the incentive increases formal employment among vocational school graduates by 4.2 percentage points (14.5 percent) over the first two years driven by a 5 percentage point (25 percent) increase in permanent formal jobs. These employment gains are due to both extensive and intensive margin effects. Treatment effects are concentrated among youths with binding reservation wages who also tend to underestimate formal wage growth.

JEL Classification: J08, J24, J41, J46, J63

Keywords: youth employment, wage subsidy, formal employment, reservation wages

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1 Introduction

Governments across many developing countries try to increase formal sector employment to increase tax revenues, boost productivity and increase the share of the workforce protected by regulation and covered by social benefits programs (Hsieh and Klenow, 2009; Herrendorf et al., 2014). Most existing interventions have targeted firms by reducing bureaucratic costs (Bruhn, 2011; Bruhn and McKenzie, 2014; Rocha et al., 2018; De Mel et al., 2013) and taxes (Monteiro and Assunção, 2012) or increasing monitoring (De Andrade et al., 2016; de la Parra, 2017). These programs have either no or only modest impacts (Ulyssea, 2020). Less is known about whether and how workers can be nudged towards pursuing formal sector employment. These labor supply concerns matter as firms around the globe struggle to hire and retain workers.[1]

In Mexico, like in other low- and middle-income countries (LMICs), youths represent a large proportion of the labor force. They are disproportionately affected by high unemployment rates, high levels of turnover and informality, and disadvantaged labor conditions. When young people enter the labor market, they choose between different employment paths. In Mexico, 74.5 percent of working youths aged 17 to 20 are informally employed (ENOE 2019), which is representative of other LMICs (Chacaltana et al., 2019).These jobs may be easier to obtain and offer (short-term) benefits including relatively high starting hourly pay, shorter commuting times, and more flexibility compared to formal employment. On the other hand, informal work provides less job security, no access to social security programs, and lower wage growth. Only rarely it is a stepping stone to formal work (Levy, 2010; ILO, 2015).

A related challenge that is often overlooked is how to *retain* people in jobs once they have found formal employment (Levy, 2010). High turnover matters especially when firms struggle to fill open vacancies, face high costs of hiring and training people and rely on team production with specialized tasks (Moon et al., 2021, 2022). It also impedes on-the-job learning, which is especially important for entry level positions in low-skill sectors (Adda et al., 2013). This problem is particularly relevant for developing countries, where job switching is twice as common (Donovan et al., 2020).[2]

[1]While richer economies currently face high levels of open vacancies, supply-side concerns in lower and middle income countries are often linked to high turnover rates due to workers quitting employment, which reduces the profitability of hiring workers and thus (indirectly) lowers the number of vacancies that firms are willing to post (Moon et al., 2021; Donovan et al., 2020).

[2]For example, annual turnover rates in China are 65% in retail services, 94% for truck operators, 74% in

It is unclear to what extent the high rates of informality and turnover among youths are voluntary or the result of deeper structural factors (Perry et al., 2007). In this paper, we test whether a temporary wage incentive can encourage Mexican secondary school graduates to obtain and stay in formal employment. We first present stylized facts that motivate our intervention. Entry-level hourly wages are 9%-17% lower for young labor market entrants in the formal than in the informal sector. This pattern is in line with evidence from other LMICs showing that youths with primary and secondary education earn higher hourly wages working informally (Shehu and Nilsson, 2014). However, while informal wages stay relatively constant, formal wages in our study sample grow by about 25% (35%) over the first six (twelve) months, driven by substantial training and on-the-job skill gains. Low entry-level wages in formal jobs matter given that they are lower than the reservation wages for about one-third of school leavers. To address the initial (wage) disincentive of formal work, we design an employment incentive that pays workers the equivalent of about 20% of the average formal entry level wage for up to six months for holding a formal job. These temporary incentives may lead to long-run employment gains as they help youths to accept and retain formal jobs despite lower starting salaries.

We conduct an experiment in partnership with the Mexican government to test the effectiveness of this formal employment incentive in a sample of almost 2,000 graduating students across 13 upper secondary schools in the San Luis Potosi metropolitan region. This area has a large industrial base (including automotive production), and firms report struggling to fill vacancies, frequently ensuing from high worker turnover. About 80% of students in our sample attend schools with a vocational focus while the remaining 20% are enrolled in general schools that traditionally prepare them for tertiary education. We randomly assign half of the graduating students in each school to be eligible for the wage incentive and test its effect on employment outcomes over two years using survey and high-frequency social security data.

We offer four main results. First, the employment incentive does not lead to (short-run) changes in employment for graduates of general schools, nor for students who plan to continue their education at baseline. This is an important finding for policymakers who are concerned that programs offering (short-term) benefits to encourage formal work may have

hospitality and 41% in warehousing (Moon et al., 2021). High turnover can adversely affect firm performance. Moon et al. (2022) find that an additional percentage point in turnover among Chinese consumer mobile devices workers increases quality failure rates by 0.74%-0.79%.

unintended consequences on school leavers' decisions to pursue or complete further education. Indeed, recent evidence from the U.S. suggests that active labor market policies targeted at students may delay or even reduce graduation rates among beneficiaries (Heller and Kessler, 2021).

Second, the incentive has large employment effects for graduates of vocational schools (and those planning to enter the labor market). Over the first year, average formal employment rates increase by 4.2 p.p. (16.8%). This effect is exclusively driven by a 5.1 p.p. (30%) increase in jobs with permanent contracts. Treatment effects persist after two years with average formal employment gains of 4.2 p.p. (14.5%), driven by a 5 p.p. (25%) increase in jobs with permanent contracts. These are intent-to-treat estimates of making youths *eligible* to receive the bonus. In fact, more than half of youths who find formal employment fail to claim the incentive.

Third, we show that these results are due to employment gains on both the extensive and intensive margins. The share of vocational school graduates ever formally employed increases by 5.4 p.p. (10%) over the first two years. This average increase masks a shift between types of employment. The share ever working with a permanent contract increases by 8.6 p.p. (20.5%) while the share with temporary contracts decreases by 1.1 p.p. (4.8%). In addition, endline survey data suggests that about half of formal employment gains come from a reduction in informal employment. These shifts in employment types matter as formal jobs, especially those with permanent contracts, offer more training and social security benefits. We also find that workers in the treatment group are less likely to report adverse income shocks resulting from the COVID-19 pandemic.

The effects on retention depend on the contract type. While there is no difference between the control and treatment groups for employment spells starting with permanent contracts, the hazard rate of leaving employment is 26% lower in the treatment group for employment spells that start with temporary contracts. This increase in retention is driven by the fact that among youth starting with temporary contracts, those in the treatment group are 70% more likely to transition to a permanent contract than those in the control group. Permanent contracts provide youths with more control over their career. The main reason for leaving a permanent position is for jobs with better working conditions and pay, while those with temporary contracts most frequently state that they stopped working because their jobs were

terminated by the employer.

Improvements in job finding rates and retention translate into sizeable gains in work experience. After two years, people in the treatment group who graduated from vocational schools have spent 1.03 more months (14.8%) in formal employment, driven by a 1.23 month (25.7%) increase in experience with permanent contracts. Importantly, when people switch jobs, even involuntarily, they do not experience a drop in wages, suggesting that the skills workers learn on the job are not firm-specific (Becker, 1964). Especially in LMICs where youth turnover rates are high (Donovan et al., 2020), obtaining general skills is important for helping workers maintain positive wage growth, even as they transition between firms.

Fourth, we provide evidence for the underlying mechanisms of these treatment effects and for why not a larger share of secondary school graduates pursue formal employment. We show that high reservation wages are binding and affect graduates' career choices. The shift from informal to formal employment and transition from temporary to permanent contracts among the formally employed is driven by job seekers with reservation wages just above formal sector starting salary levels. This raises the question of why reservation wages are so high given the rapid wage growth for youth with entry-level formal jobs.

To guide our analysis of underlying mechanisms, we adapt a dynamic job search model with segmented labor markets and job seeker beliefs (Meghir et al., 2015; Mueller and Spinnewijn, 2021). We find that youths in our sample have very high discount rates, which reduces the benefit of future wage growth and dampens its negative effect on reservation wages.[3] In addition, we collect incentive-compatible data on beliefs and find that job seekers have fairly accurate beliefs about job separation rates but underestimate formal wage growth. Even two years after graduating, the median youth believes that formal wages increase by less than 10% over the first six months compared to the actual growth rate of about 25%. Mueller and Spinnewijn (2021) show that this type of misconception reduces welfare as it leads to sub-optimally high reservation wages and low search effort for formal jobs. Our wage incentive may counter the adverse effects of false beliefs by lowering reservation wages and thus nudging youths towards accepting initially lower-paying formal jobs with higher wage

[3]One explanation for high discount rates is that people are liquidity constrained (Cohen et al., 2020) and are thus forced to accept a certain type of employment (Oreopoulos et al., 2012). We do not find evidence for this explanation in our sample as discount rates are not correlated with beneficiaries' socio-economic status or other measures of being liquidity constrained.

growth. We indeed find that youths in the treatment group work in formal sector jobs with hourly wages start about 10%-15% lower than in the control group.

Our study contributes to the literature on active labor market policies (ALMPs). While the overall evidence on the effectiveness of ALMPs in developing countries is mixed (Crépon and Van Den Berg, 2016; McKenzie, 2017), a recent meta-analysis suggests that simple programs that provide clear incentives tend to be most effective (Card et al., 2018). Hence there is a renewed interest in wage subsidy programs. Importantly, most wage subsidy programs target firms rather than workers, with few positive or long-lasting results.[4] Potential reasons for this limited effectiveness include stigma effects of subsidies and firms' reluctance to file necessary paperwork and comply with other regulations. Who benefits from these programs is also unclear as firms may respond to the wage subsidy by lowering wages.[5]

Our intervention addresses these concerns by circumventing employers and paying wage subsidies directly to workers. This approach is commonly used in developed countries to increase labor force participation and supplement wages of low-income workers (e.g. Earned Income Tax Credit in the US) but has, to our knowledge, not been tried in poorer countries, possibly due to implementation challenges of compensating workers directly.[6] In Canada, Card and Hyslop (2005) find that a subsidy for full-time work offered to single parents has short-term positive effects on employment, which fade out after 18 months. Closely related to wage subsidies, employment bonus programs that pay workers for finding and keeping a job failed to increase employment in the Netherlands (Van der Klaauw and Van Ours, 2013). By contrast, programs that subsidize wages for (youth) internships tend to find more positive effects (Gelber et al., 2016; Beam and Quimbo, 2021).

In addition to testing the effect of paying incentives directly to workers, we advance the

[4]Existing studies from the U.S. find either no or limited effects (Burtless, 1985; Dubin and Rivers, 1993; Katz, 1996). More recent evidence from Argentina (Galasso et al., 2004) and South Africa (Levinsohn et al., 2014) finds similarly muted effects. One important exception is Groh et al. (2016) who find that among female college graduates in Jordan, a six-month wage subsidy leads to sizeable employment gains. However, these effects quickly disappear once the incentive expires, suggesting that the marginal productivity of workers is above the minimum wage.

[5]Unless firms increase wages, wage subsidies paid to firms would lower hiring costs. De Mel et al. (2010) find, similar to Groh et al. (2016), that a short-term hiring subsidy offered to firms in Sri Lanka leads to only a temporary increase in employment.

[6]The EITC, which is means-tested and benefits more than 20 million people, has been found to increase labor supply and employment without depressing wages (see e.g. Schanzenbach and Strain (2021)).

wage subsidy literature in several ways. First, we test the effect of a wage incentive for youths who are just graduating from secondary school.[7] At this career point, hourly wages of informal jobs are relatively attractive (Shehu and Nilsson, 2014) and youths may heavily discount and underestimate the rapid (relative) wage growth of formal jobs. Short-term wage incentives offered during the school-to-work transition are thus particularly important, not least because the first job has long-lasting impacts on career trajectories and lifetime earnings (Neumark, 2002; Kahn, 2010; Oreopoulos et al., 2012). Second, the incentive is paid monthly and is thus more attractive than bonuses paid in the more distant future, especially if beneficiaries heavily discount future payouts.[8] Finally, we link our study sample to monthly social security data, which addresses concerns of selective attrition and surveyor demand effects. It also allows us to precisely trace out employment effects over time, which is important given the high rates of worker churn, and thus estimate cumulative treatment effects.[9]

We also contribute to a nascent literature that explores the role of worker retention and on-the-job human capital accumulation in developing countries. In line with Donovan et al. (2020), our findings suggest that high turnover rates, especially in the informal sector, do not represent a reallocation of workers to more productive jobs. Donovan et al. (2020) and Bick et al. (2018) also conclude that the steeper wage-tenure profiles in developing countries are less likely to be due to human capital accumulation and more likely to result from firm selection in the presence of uncertain match quality. By contrast, our results suggest that at the very early career stage, formal employment offers substantial training and learning opportunities that are associated with wage increases. Importantly, when workers switch between formal jobs they do not experience a loss in wages, suggesting that the rapid wage increase in formal employment is at least in part due to a gain in general rather than firm-specific skills. The wage incentive thus indirectly serves as a subsidy for workers to acquire human capital.

[7] While other wage subsidy programs also focus on youths, they do not administer the program before participants graduate. This is important as most graduates find employment shortly after graduating. At the same time it raises concerns about unintended consequences, e.g. with regard to educational decisions.

[8] Indeed, one explanation for the muted effects of existing programs (e.g. Van der Klaauw and Van Ours (2013)) is that beneficiaries receive the bonus only after they hold a job for a certain period of time.

[9] Gains in statistical power from panel data are particularly large for outcomes with low autocorrelation, e.g. due to high job turnover rates (McKenzie, 2012). Our study is thus able to estimate employment effects more precisely than existing wage subsidy studies.

Last, we add to a growing literature on the role of information frictions in developing country labor markets (Abebe et al., 2021a,b; Abel et al., 2020; Bassi and Nansamba, 2021; Beam, 2016; Banerjee and Chiplunkar, 2022; Caria and Falco, 2022; Carranza et al., 2022). Specifically, we show that labor market entrants have false beliefs about formal wage growth. This is most closely related to Jensen (2010) who shows that secondary school graduates underestimate returns to education. These types of misconceptions are important since they can distort choices and reduce welfare.[10] While the reason for why people hold false beliefs and how to best correct them requires additional research, our results suggest that informing (young) people about events with distant benefits may not be effective if recipients heavily discount the future. In these settings, short-term incentives can offer a promising alternative to change behavior.

2 Background

2.1 Youth unemployment in Mexico

The Mexican labor force comprises of just over 50 million people (ENOE 2017). The unemployment rate prior to the COVID-19 pandemic was roughly 3.5% for the overall population and around 6% for youths (INEGI 2017). Among the employed population 56.5% of the overall population and 60.5% of youths work informally (INEGI 2017; IMJUVE 2017). Informal employment is the dominant entry point into the labor market for youths. Table 1 depicts transition probabilities between non-employment and formal / informal employment across quarterly panel data from the 2018 and 2019 National Labor Force Survey (ENOE) for youths aged 18 to 21. The figures estimate average transition probabilities from one quarter to the next, conditional on initial status type.

Youths without employment are almost three times as likely to be informally employed than formally employed in the following period (15.4% vs. 5.6%). At the same time, informal employment is less stable: informally employed youths are almost twice as likely to be unemployed in the following period than formally employed youth (26.4% vs. 15%). Finally, 10.6% of people transition from informal work to formal work. While this is larger than

[10] Another recent example is Alfonsi et al. (2022) who show that Ugandan job seekers hold unrealistically high wage expectations, which can be reduced by linking people to mentors with work experience. There are also examples of how misconceptions may be beneficial to workers. In a recent study, Bandiera et al. (2021) show that debiasing job seekers' optimism about employment prospects lowers their job search and employment rates.

Table 1: Transitions among young (18-21 years old) between work statuses

	No work	Informal work	Formal work	Total
No work	78.93	15.43	5.64	100
Informal work	26.37	63.07	10.56	100
Formal work	15	14.92	70.07	100
Total	53.06	29.09	17.85	100

Notes: Author's calculations using panel data from 2018 and 2019 ENOE surveys. Table presents the average probability of maintaining or changing work status from one period to the next. The rows reflect the initial status, and the columns reflect the final status. Youth are followed for four quarters (3 month periods) between 2018 and 2019 in staggered cohorts.

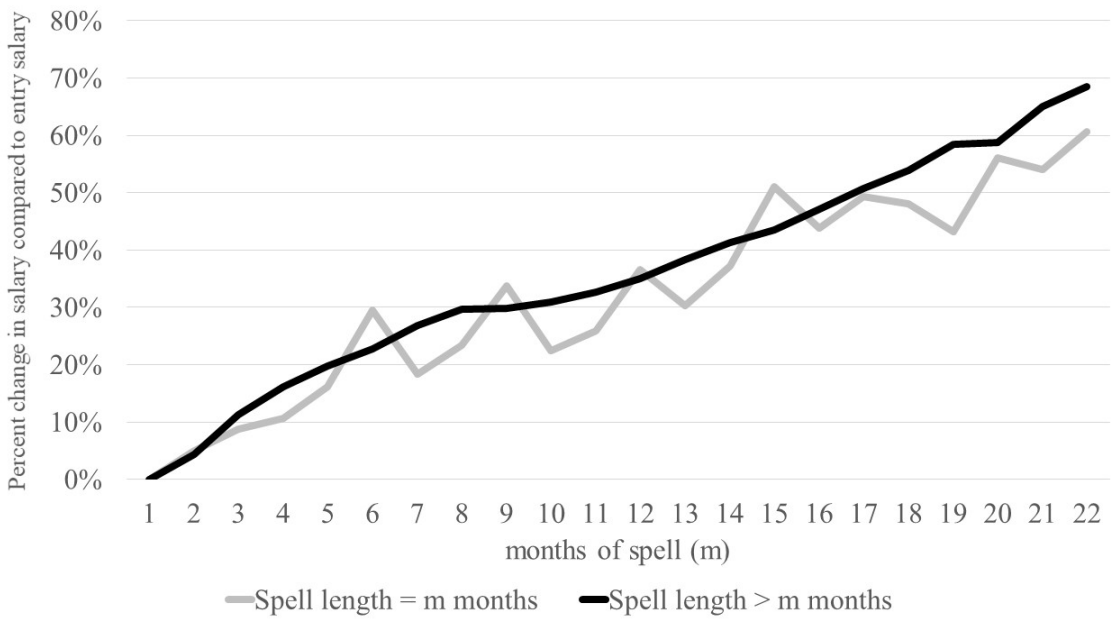
the probability of transitioning to formal work from non-employment (5.6%) it is small in magnitude and less than the likelihood of transitioning from formal to informal employment (14.9%).

In sum, the data suggest that, similar to many other LMICs, including Argentina and Brazil (ILO, 2015), informal work is not a stepping stone to formal work for youth, and that the informally employed are prone to greater employment instability. These findings are also in line with recent evidence by Donovan et al. (2020) who show that across developing countries transitions between jobs are more common and are less likely to lead to wage gains than in developed economies. Overall, these results suggest that, similar to evidence from other settings, early career choices matter and have long-lasting effects on employment prospects (Oreopoulos et al. (2012)).

2.2 Formal vs. informal sector wage growth

In addition to job stability, choices over different types of employment also affect earnings as different career paths offer different wage trajectories. Figure 1 uses social security data (IMSS) to plot the average wage growth for formally employed people in our study sample of school leavers. In order to test how much of the observed wage growth is driven by firm selection, we show wage increases separately for people who exit employment in a given month (light) versus those that retain their job (dark). The graph shows several interesting stylized facts. First, there is substantial wage growth at the beginning of school leavers' careers. After six months, wages have increased by almost 25%. While the wage growth slows down slightly afterwards, we still see a large gain of 35% after 12 months and 50% after 18 months. (The average annual inflation rate over this period was below 5%.) Second,

Figure 1: Formal sector wage growth

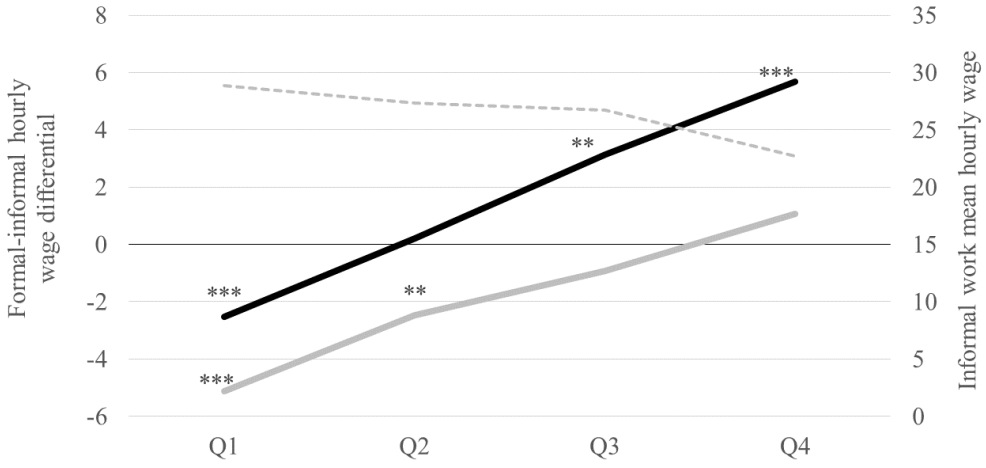


Notes: Administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021. Graph depicts mean percent salary change from formal employment over time (capturing 1005 employment spells). The grey line shows the average wage gain for workers who leave after exactly m months.

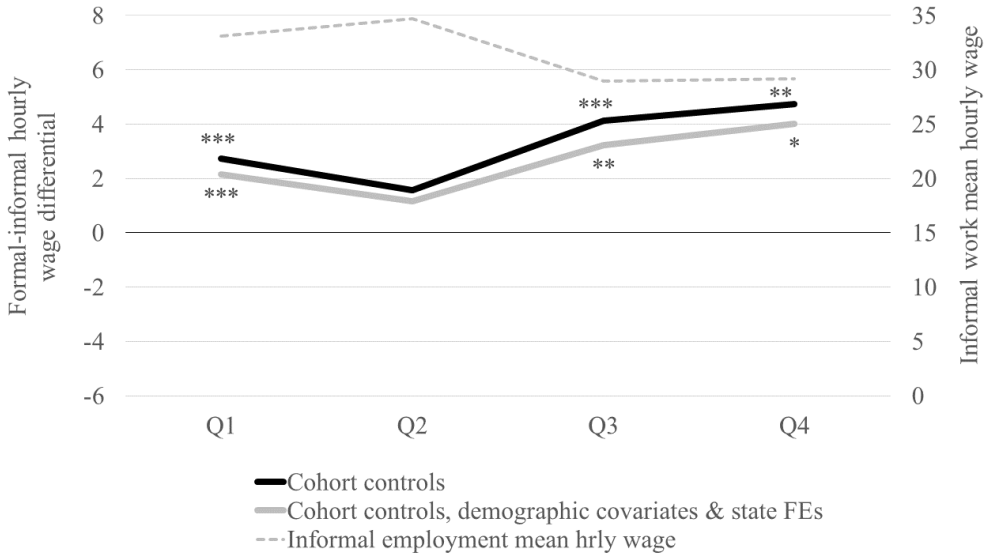
we find that for most employment spells wage growth of the marginal person leaving the job is slightly below those that stay on the job for most spells. This pattern may be explained by firms either screening hired workers on the job (Donovan et al., 2020) or workers with lower match quality selecting to exit the employment relationship. However, workers who leave jobs in a given month accrue approximately 85% of the average wage gain suggesting that the steep increase in wages in our setting is not primarily due to selection.

To compare this rapid growth to wage growth in the informal sector, we use data from the nationally representative National Survey of Occupation and Employment (ENOE). Panel A in Figure 2 shows how the difference in hourly wages between formal and informal work evolves over the first year on the job for people aged 17 to 20. The dark bar shows that starting wages are approximately 2.5 pesos (9%) lower in the formal sector. However, while the informal sector wages are relatively stagnant, formal wages increase rapidly (in line with the IMSS data). Assuming that measurement challenges are independent of the time a person is employed in a given job, the increase in the ratio of formal over informal wages is still indicative of an increase in the *relative* attractiveness of formal work over time. By quarter

Figure 2: Wage differentials between formal and informal work across employment spells



(a) Youth ages 17 to 20



(b) Youth ages 21 to 24

Notes: Source: National-level panel data from ENOE 2016-2019. Standard errors are robust, *** p<0.01, ** p<0.05, * p<0.1. Graphs depict trajectories of hourly wage differentials across youth employment spells. Solid lines depict formal-informal wage differentials over time with and without state fixed effects and demographic controls (for gender, marital status, and education level). The dotted line shows average hourly wage for youth with informal employment spells. Cohort controls refer to the rotating nature of the ENOE panel. Hourly wages are in pesos. Sample limited to youth starting a new employment spell and with positive wages.

three (four) formal jobs pay 10% (20%) more than informal jobs. The light line shows that when we control for worker characteristics (including gender and education), the same trend holds but the line is shifted downwards implying that the initial wage advantage of informal work is even larger (17%). One explanation is that workers in the formal sector tend to be more educated (Conover et al., 2022). A similar pattern holds when we limit the sample to those who are continuously employed for at least three quarters (Appendix Figure A1), confirming results from IMSS data that fast wage growth is not primarily driven by selection.

Interestingly, the pattern looks different for the cohort of people aged 21 to 24 (Panel B). Starting hourly wages are higher in the formal sector and there is less wage growth over time. One possible explanation is that this older group is less likely to accumulate human capital on the job. The wage patterns for these older cohorts is more similar to aggregate wage differentials in other settings, where formal jobs pay more even after controlling for observable characteristics (e.g. Ulyssea (2010)).[11] However, evidence disaggregated by age and education levels shows that across many LMICs, youths with primary and secondary education earn more in informal than in formal employment (Shehu and Nilsson, 2014).

2.3 Characteristics of formal vs. informal employment

What can explain this substantial wage growth for school leavers, and why is it limited to the formal sector? Table 2 compares job characteristics from employed youths in our study sample (described in more detail below) collected two years after graduating. Among these, 47% and 30% report that they received formal training on the job for formal employment with permanent and temporary contracts, respectively. By comparison, this figure is only 6.5% in the informal sector.[12] A very similar pattern holds for other forms of human capital accumulation: 8.5% of those informally employed report learning on the job compared to 45% (26%) among formally employed workers with permanent (temporary) contracts. These differences may in part reflect that the formally employed mainly work for firms (93%), while

[11] Some of this difference may be driven by unobservable differences.For example, Ulyssea (2018) finds that the formal sector wage benefit in Brazil completely disappears when controlling for firm fixed effects. We also find that for older workers the wage gap decreases (by about 30%) when we control for worker characteristics (Panel B).

[12] The average length of training workers report to receive is almost 60 hours. This excludes training received as part of the initial "on-boarding" process.

the informally employed either work for family businesses (51%) or are self-employed (24%).[13]

Receiving training on the job is strongly predictive of career advancements. In our sample, it increases the probability of receiving a wage increase by almost 50% and more than doubles the share that changes positions within the company. Overall, these findings suggest that the observed wage growth is related to productivity increases. This is consistent with recent evidence by Bobba et al. (2021) who show that on-the-job human capital accumulation is an important driver of formal sector productivity growth. Yet, another requirement for productivity gains to increase wages is that workers learn general rather than firm-specific skills (Becker, 1964).[14]

We find support for workers acquiring general skills by looking at wage changes for people who switch jobs. Transitioning between formal jobs without unemployment spells is associated with no change or even modest increases of about 5% (.6 pesos, SD 69.79) in daily wages. Wage increases tend to be larger when youths transition from permanent employment (12.8 pesos), consistent with the higher reported rates of skill acquisition in these types of jobs. Even when workers experience a gap in formal employment, they only experience a very small drop in wages (.3 peso, SD 64.03). The acquisition of general skills also helps explain why firms do not pay starting wages that are (temporarily) above workers' productivity levels as they may be poached by other firms at later points in time when firms need to pay below their marginal productivity in order to compensate initial losses (Acemoglu and Pischke, 1999).

Overall, these results suggest that a temporary wage incentive may be effective in increasing human capital accumulation and retention by making the formal sector more attractive at the early career stage when productivity and salary levels are low. In the next section, we will describe our intervention, which pays beneficiaries a wage increase of about 20% for up to six months. Figure 1 shows that after six months the average wage increase is 23% over the starting salary (median increase is 16%), thus exceeding the starting average compensation *inclusive* of the incentive.

[13] In Mexico, it is possible for formal firms to hire workers informally (Levy, 2010; Ulyssea, 2018). de la Parra (2017) finds that in Mexico 56% of the informally employed work for firms, which is considerably higher than in our sample.

[14] Alternative explanations for why firms invest in general skill training are that there are labor market frictions that prevent other firms from learning about worker productivity (Acemoglu and Pischke, 1999) or that firms use general skill training as a screening mechanism (Autor, 2001).

Table 2: Comparison of jobs

		Total	Informal (1)	Formal Temp. (2)	Formal Perm. (3)	Difference	Difference	Difference
Variable	N	Mean/SE	Mean/SE	Mean/SE	Mean/SE	(1)-(2)	(2)-(3)	(1)-(3)
Weekly hours	925	41.325	34.831	44.280	47.650	-9.449***	-3.369**	-12.880***
		[0.541]	[0.899]	[1.271]	[0.484]			
Monthly income	895	6469	5083	6850	7897.8	-1767***	-1048***	-2780***
		[124.2]	[178.2]	[359.2]	[158.7]			
Wage increase	898	0.438	0.277	0.327	0.630	-0.050	-0.303***	-0.353***
		[0.020]	[0.028]	[0.052]	[0.028]			
Work: Firm	925	0.556	0.193	0.710	0.926	-0.517***	-0.216***	-0.747***
		[0.016]	[0.019]	[0.044]	[0.013]			
Work: Governm	925	0.024	0.011	0.121	0.010	-0.111***	0.111***	0.012
		[0.005]	[0.005]	[0.032]	[0.005]			
Work: Self-Empl	925	0.125	0.243	0.047	0.010	0.197***	0.036*	0.235***
		[0.011]	[0.020]	[0.020]	[0.005]			
Work: Family	925	0.276	0.526	0.103	0.041	0.424***	0.062**	0.483***
		[0.015]	[0.023]	[0.029]	[0.010]			
Learning on job	822	0.264	0.085	0.265	0.453	-0.180***	-0.188***	-0.373***
		[0.015]	[0.014]	[0.045]	[0.026]			
Training in job	822	0.251	0.065	0.255	0.445	-0.190***	-0.190***	-0.385***
		[0.015]	[0.013]	[0.044]	[0.026]			
Change position	925	0.146	0.094	0.103	0.217	-0.009	-0.115***	-0.124***
		[0.012]	[0.014]	[0.029]	[0.021]			
Job satisfaction	925	8.185	8.088	8.056	8.348	0.032	-0.292	-0.297**
		[0.057]	[0.087]	[0.166]	[0.078]			
Plan stay in job	907	0.732	0.651	0.724	0.822	-0.073	-0.098**	-0.182***
		[0.015]	[0.023]	[0.044]	[0.019]			
Number benefits	925	6.121	1.914	7.374	10.547	-5.459***	-3.173***	-8.802***
		[0.172]	[0.126]	[0.426]	[0.170]			
F-test (F-stat)						18.504	8.132	278.972
F-test (p-value)						0.000	0.000	0.000

Notes: Data collected from endline survey for study participants in employment. The last column reports differences in the means across the groups. *, **, *** indicate t-test significance at the 10, 5, and 1 percent critical level. Standard errors are robust. Household vulnerability score is based on the national Mexican AMAI measure which considers the number of bathrooms, bedrooms, cars, and employed persons in the household, as well as internet access and head of household education level.

In addition to the accumulation of human capital and subsequently wage growth, Table 2 points to other reasons for why policymakers may want to nudge workers from the informal to the formal sector. Most importantly, formal workers are much more likely to receive job benefits. When asked about twelve potential job benefits, formal workers with permanent (temporary) contracts report receiving 11.12 (9.32) compared to 1.96 among those working in the informal sector. In addition, workers in formal employment, especially those with permanent contracts, are more satisfied on the job and plan to stay in the job for longer.[15]

3 Experimental design

3.1 Sample and randomization

Mexico's upper secondary education comprising grades 10 to 12 is divided into two tracks: schools offering a general academic curriculum preparing students for university and vocational (technical and technological) schools that prepare students predominantly to work after graduating. Since the government was concerned about unintended consequences of the wage incentive for those planning to continue their education, we focused our recruitment on students planning to work (either full or part-time) after graduation. Of students in our sample, 80% thus attend vocational schools and 20% attend general schools. All schools are located within 80km of the industrial zone of San Luis Potosi, an area with a sizeable manufacturing industry located in north-central Mexico.

School officials informed parents and legal guardians in the 13 participating schools that were part of our evaluation about the study and asked them to authorize their children's participation. To avoid conflicts with exam dates and other school events, the research team scheduled activities in coordination with each school director. Activities took place in the last quarter before graduation. We asked students enrolled in 12th grade for consent to participate in the study and administered an in-person baseline survey that included modules about socio-demographic characteristics, employment trajectories, and career expectations.[16] Approximately 90% of invited students agreed to participate. Table 3 shows baseline char-

[15] In Mexico, law-mandated job benefits are the same for formal jobs regardless of the type of contract (temporary or permanent). Some firms offer additional benefits to workers, which, in our sample, seem to be more prominent for workers under a permanent contract.

[16] Within a school, we randomly selected whether the morning or afternoon shift was included in the study. Within a shift, all students were invited to participate.

acteristics of our sample: 50% of participants are female and the average age is 17.8 years. Approximately half have internet at home, and live in households with on average one car. 56% plan to work after high school and 37% had a job at baseline, predominately in the informal sector.

Figure 3: Experimental design and timeline

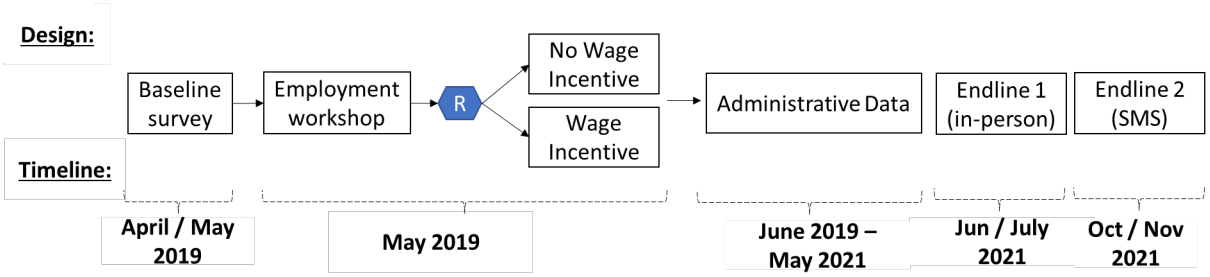


Figure 3 shows the overall experimental design. Of the 1,924 individuals for whom baseline data were collected, 970 are randomly assigned (stratified by gender and school type) to the wage bonus treatment and 954 individuals to the control group. Table 3 shows average baseline characteristics for the control and treatment group. Of the 20 variables, one difference is significant at the 1% level (grit) and one at the 10% level. We can reject that these differences are jointly significant (p-value: 0.22) suggesting that randomization was successful. However, we will report results with and without controlling for these covariates. This evaluation design provides us with a minimum detectable effect size of 0.11 standard deviations for cross sectional data, which is sufficient to detect treatments effects that are small to moderate in magnitude.

3.2 Intervention

Two to four weeks after the baseline survey, all 12th-grade students in our sample (including the control group) were invited to a "Labor Market Information Workshop" (LMLW) during regular school hours within the school premises. The three-hour workshop led by the staff of the National Employment Service covers basic tools for job search and information about the local labor market context including wages in the informal and formal sector. At baseline (prior to the workshop), only 29% of the participants had any knowledge regarding the benefits offered by a formal job. Of the 1,924 students surveyed at baseline, 1,769 (92%)

Table 3: Randomization check

	Total		Wage bonus treatment (1)		Wage bonus control (2)		Difference
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)
Female	1924	0.501	970	0.499	954	0.503	-0.004
		[0.011]		[0.016]		[0.016]	
Age	1924	17.81	970	17.78	954	17.84	-0.060*
		[0.017]		[0.023]		[0.026]	
Married	1924	0.022	970	0.026	954	0.019	0.007
		[0.003]		[0.005]		[0.004]	
Caregiver	1924	0.221	970	0.223	954	0.220	0.003
		[0.009]		[0.013]		[0.013]	
Home internet	1924	0.484	970	0.476	954	0.493	-0.016
		[0.011]		[0.016]		[0.016]	
Number bathrooms (HH)	1923	1.147	970	1.140	953	1.153	-0.013
		[0.013]		[0.018]		[0.019]	
Number bedrooms (HH)	1924	3.145	970	3.121	954	3.170	-0.049
		[0.024]		[0.034]		[0.034]	
Number cars (HH)	1924	1.031	970	1.044	954	1.018	0.027
		[0.021]		[0.031]		[0.028]	
HH SES well-being score	1924	139.6	970	139	954	140.2	-1.173
		[0.932]		[1.316]		[1.321]	
Number of earners (HH)	1914	1.990	964	1.971	950	2.008	-0.037
		[0.024]		[0.034]		[0.033]	
Grit score	1924	3.536	970	3.574	954	3.498	0.076***
		[0.012]		[0.017]		[0.016]	
Vocational school	1924	0.800	970	0.801	954	0.799	0.002
		[0.009]		[0.013]		[0.013]	
Commute time city	1924	0.759	970	0.748	954	0.771	-0.023
		[0.010]		[0.014]		[0.015]	
Plan to work	1924	0.557	970	0.553	954	0.561	-0.008
		[0.011]		[0.016]		[0.016]	
Plan to study	1924	0.435	970	0.438	954	0.432	0.006
		[0.011]		[0.016]		[0.016]	
Has a job at baseline	1924	0.372	970	0.371	954	0.372	-0.001
		[0.011]		[0.016]		[0.016]	
Has held a formal job	1924	0.182	970	0.177	954	0.188	-0.010
		[0.009]		[0.012]		[0.013]	
Reservation wage	1924	4679	970	4635	954	4723	-87.88
		[74.8]		[99.9]		[111.6]	
F-test joint significance (F-stat)							1.203
F-test joint significance (p-value)							0.216

Notes: Source: Baseline survey. The last column reports differences in the means across the groups. *, **, *** indicate t-test significance at the 10, 5, and 1 percent level. Standard errors are robust. Household vulnerability score is based on the national Mexican AMAI measure which considers the number of bathrooms, bedrooms, cars, and employed persons in the household, as well as internet access and head of household education level.

attended the workshop.[17]

At the end of the workshop, based on the random assignment, students received a package with different documents. Students assigned to the treatment group received a personalized letter informing them of their eligibility to receive financial support for employment in the formal labor market, as well as the instructions to register (Appendix Figure B7).[18] All envelopes, including for students in the control group, contain a prepaid card through which participants could receive a small monetary compensation for completing surveys. The same day in school, immediately after the LMLW, individuals in the treatment group received a 15-minute talk delivered by members of the research team to further explain details of the wage incentive, including the process for receiving the money. The 8% of students not attending the workshop received their letter and instructions at a later point in time.

The size of the wage bonus offered (900 pesos per month, around 45 USD) was equivalent to about 17% percent of the average monthly wage in a full time entry-level formal job. This bonus was paid on a monthly basis as a top-off to their salary for up to six months conditional on holding formal employment. The six months could be split between employment spells over the course of two years after graduation. Beneficiaries needed to provide identification documents and proof of formal employment in order to receive payments.[19] Importantly, the incentive was transferred directly to beneficiaries (using the prepaid card) and was thus not observable to potential or actual employers. This is a key distinction from most existing wage subsidy programs tested in developing countries (e.g. Levinsohn et al. (2014); Galasso et al. (2004)). It ensures that benefits went fully to workers and that treatment effects are not driven by a reduction in hiring costs for firms or by other factors such as stigma.

The size of the incentive was calibrated based on wage growth data. Figure 1 shows that after six months, average (median) formal sector wages have increased by about 23% (16%).

[17]Those that do not want to attend are assigned another activity by the school director (like studying, completing classwork, etc.). Among participating students, 76% report the LMLW was useful.

[18]Participants are (truthfully) informed that due to limited resources the incentive cannot be provided to all graduates and are thus allocated through a lottery.

[19]Several actions were implemented to encourage wage bonus take-up. First, participants could claim the payment of the economic incentive remotely and could electronically certify their formal employment status each month by sending a digital copy or photo of the documentation requested. The project team validate documentation using public records. Second, the project team answered information requests, provided clarifications, and sent reminders to participants through different channels such as SMS, WhatsApp, phone calls, and emails. Take-up rates can be seen in Appendix Table A4.

For the majority of beneficiaries, at or shortly after six months, the wage paid by employers exceeds the sum of the salary and incentive they accepted at the start of the job. By this time, the incentive may thus not be necessary to keep people in their job.

3.3 Data sources

For our main employment outcomes, we use monthly social security data (IMSS). In Mexico, employers are required to register contributions on behalf of formally employed workers. IMSS data are reported monthly and include information on the type of contract the worker holds (temporary or permanent) and the daily salary reported by the employer. The administrative data are comprehensive of private sector employees, but exclude public sector employees since their social security contributions are managed through a different agency. Given that the public sector is small in San Luis Potosi (the study region) and only 2% of the study sample report holding a public sector job in our endline survey, this omission is unlikely to affect our estimated program impacts. We consider the use of administrative data as an important contribution of this paper. Most existing studies from developing countries in this literature rely on self-reported employment data, which are prone to surveyor demand effects and recall biases (McKenzie, 2017). High-frequency administrative data also allow us to accurately estimate cumulative treatment effects on work experience and income required to conduct a cost-benefit analysis.

We complement administrative data with two rounds of in-person data collection: a baseline survey prior to the workshop and a follow-up survey completed in June 2021, two years after the treatment. Both surveys are administered through a professional survey firm.[20] For the endline survey, we successfully surveyed 75% of participants. The overall attrition rate is slightly lower for treatment participants: 23.2% for the treatment and 27.2% for the control group. In addition, we administered an SMS survey in October and November 2021 to collect data on people's time preferences and labor market beliefs.

[20] All the instruments are written in Spanish; they have been translated to English for IRB review purposes and fidelity of translation can be attested by the principal researchers who are bilingual. For the endline survey, we successfully surveyed 79.8% at home, 17.2% via phone and an additional 2.9% via email that we were not able to reach in-person due to COVID-19 concerns.

3.4 Empirical specifications

For our main specification, we use monthly panel data between June 2019 and May 2021, spanning the two years after participants graduate. For formal sector outcomes, including wages and contract type, we use the administrative IMSS data. For informal sector outcomes, we reconstruct the panel data from the employment history reported in the endline survey.

As our main specification, we follow Groh et al. (2016) and estimate:

$$y_{it} = \beta Treat_i + \gamma X_i + \theta_i + \lambda_t + \epsilon_i \tag{1}$$

where y_{it} is the outcome of interest in time period t for person i. $Treat_i$ is equal to one if person i received the offer of a wage bonus at baseline. θ_i is a set of stratifying covariates including gender and school type, and λ_t present month dummies. We estimate regressions with and without controlling for covariate vector X_i. Standard errors are clustered at the individual level. The coefficients of interest (β) are intent-to-treat estimates and should be interpreted as the average effect of being *eligible* for the wage incentive over all periods t.[21] We also estimate specification 1 separately for each month t to test how treatment effects evolve. We present these monthly results graphically.

Second, to estimate effects on aggregate outcomes y_i, e.g. the total number of months employed, we estimate:

$$y_i = \beta Treat_i + \gamma X_i + \theta_i + \epsilon_i \tag{2}$$

with β measuring the cumulative effect over the evaluation period.

Third, to analyze effects on job retention we apply the Anderson-Gill (AG) extension of the Cox proportional hazards regression model that accounts for recurrent events and discontinuous risk intervals associated with multiple entries into and exits from work (Andersen and Gill, 1982). In this specification, the hazard of exiting employment is:

$$\lambda_{ij}(t) = \lambda_0(t) exp(\beta X_{ijm}) \tag{3}$$

[21] Similar to Groh et al. (2016), we do not report the treatment effect on the treated since being offered the voucher may affect job search and other determinants of employment outcomes.

where $\lambda_{ij}(t)$ indicates the risk of event occurrence for individual i for the jth recurrent event of the specified outcome. $\lambda_0(t)$ is an unspecified baseline hazard, and X_{ijm} represent a series of m covariates for individual i in the jth recurrent event.

We assess the total risk of event occurrence across the two-year study period (June 2019 to May 2021) of three different recurrent events: leaving formal employment, leaving employment that started with a permanent contract, and leaving employment that started with a temporary contract. X_{ijm} includes the key independent variable of whether the youth was part of the treatment group. We also confirm that results are robust to including stratifying covariates. The data are structured to account for multiple discontinuous risk periods per individual, and standard errors are clustered at the individual level.

4 Results

We first present average treatment effects for different types of employment and how these effects evolve over the first two years after graduation. We then decompose the overall impact into effects on the extensive and intensive margin, testing whether the incentive increases the share of people who are employed and/or the employment spell conditional on finding work. Last, we investigate the effects on wages as well as aggregate income and experience.

4.1 Average employment effects

Table 4 shows the average treatment effects of the incentive over the first year (Panel A) and the first two years (Panel B). Since the randomization was stratified by school type, we show results separately for general and vocational schools. Results (not reported) are very similar with regard to magnitude and statistical significance when we divide students by whether they plan to work after graduation. Appendix Table A1 shows corresponding results for the aggregate sample.

Results show a persistent increase in formal employment for graduates from vocational schools, who disproportionately plan to enter the labor market upon graduating. Over the first year, the incentive leads to an average increase in formal employment of 4.2 p.p. (16.8%) (Panel A, Col. 1). This effect persists and remains stable over the second year (Panel B, Col. 1). The gain in formal employment is exclusively driven by an increase in jobs with permanent contracts, which increase by 5.1 p.p. (30%) and 5 p.p. (25%) over 12

Table 4: Average employment effects

Panel A: After one year

	Formal		Permanent		Temporary		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	0.042**	0.008	0.051***	0.005	-0.009	0.004	-0.019	0.046
	(0.017)	(0.025)	(0.015)	(0.018)	(0.011)	(0.019)	(0.025)	(0.053)
Observations	18468	4620	18468	4620	18468	4620	14040	3252
R square	0.11	0.05	0.09	0.04	0.03	0.05	0.06	0.14
Control Mean	0.25	0.12	0.17	0.06	0.08	0.06	0.39	0.54
Std Dev	0.43	0.43	0.37	0.37	0.26	0.26	0.49	0.49
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

Panel B: After two years

	Formal		Permanent		Temporary		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	0.042**	-0.010	0.050***	-0.005	-0.008	-0.005	-0.013	0.053
	(0.017)	(0.027)	(0.016)	(0.020)	(0.011)	(0.021)	(0.025)	(0.051)
Observations	36936	9240	36936	9240	36936	9240	28080	6504
R square	0.10	0.06	0.09	0.04	0.02	0.06	0.05	0.14
Control Mean	0.29	0.16	0.20	0.08	0.09	0.07	0.39	0.54
Std Dev	0.45	0.45	0.40	0.40	0.28	0.28	0.49	0.49
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

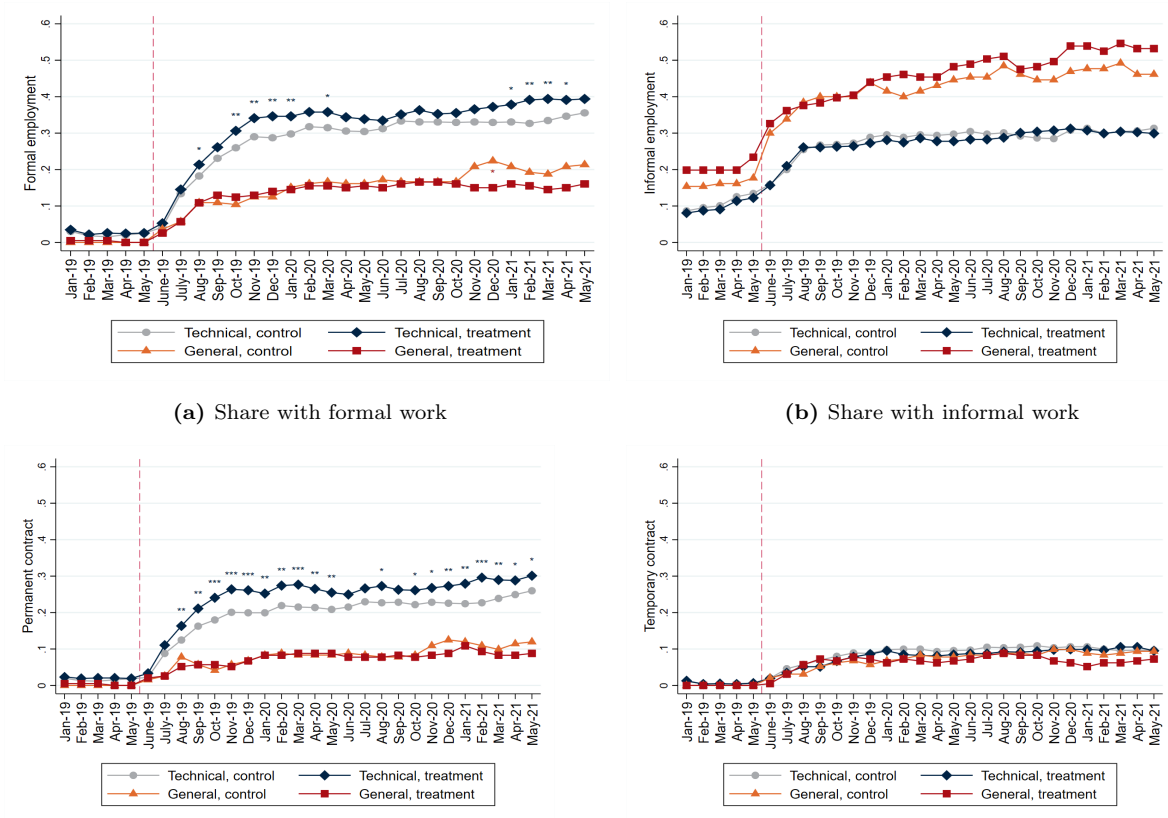
Notes: Source: IMSS administrative data for formal employment and endline survey data for informal employment, June 2019-May 2021. Observations are at the person-month level. Standard errors, clustered at individual level, are reported in parentheses. We control for demographic characteristics, socioeconomic status, work experience, time and school fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and 24 months, respectively (Col. 3). The share with temporary contracts is not affected (Col. 5). Results are very similar in magnitude and significance when we do not control for any covariates (Appendix Table A2). By contrast, we do not observe a change in formal employment or switching between employment types for graduates from general schools (Col. 2, 4, 6) or those planning to continue their education. The fact that the wage incentive does not change decisions to enter the labor market for this group assuages concerns about unintended consequences of offering short-term monetary incentives for formal work.

Our results further suggest that slightly less than half of these average formal employment gains come from a reduction in informal work (Panel A and B, Col. 7). One potential concern with the analysis of informal employment is that we rely on self-reported data from our endline survey. To test the reliability of this data, we take advantage of the fact that

we have both self-reported and administrative data for the same outcomes, namely formal employment. Appendix Figure B3 shows that formal employment rates are very similar both in terms of levels and treatment effects for the survey and admin data, which provides some reassurance about the reliability of survey responses. However, we acknowledge that the informal work estimates are less precise and not statistically significant, which limits the conclusiveness about how much of the gain in formal work is driven by a reduction in informal employment.

Figure 4: Employment impacts of wage bonus treatment, by school type



(a) Share with formal work

(b) Share with informal work

(c) Share with permanent contract

(d) Share with temporary contract

Notes: Source: Administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021. The vertical dotted line indicates the time of graduation when participants started to be eligible for the wage incentive.

Figure 4 shows how treatment effects evolve over time for the different types of employment. Among vocational school graduates, large and statistically significant increases in formal employment appear starting in October 2019, approximately four months after students

graduate, and are maintained through the beginning of 2020 (Panel a). Around April 2020, coinciding with lockdown measures implemented by the Mexican government, employment rates start to converge and treatment effects on formal employment are smaller (1-3 p.p.) and no longer significant in a month-by-month analysis. However, in the fall of 2020, treatment effects increase again and become statistically significant. This pattern is driven by corresponding changes in formal employment with permanent contracts, which show significant treatment effects for almost all post-treatment periods (Panel c). By contrast, employment outcomes for informal (Panel b) and temporary formal work (Panel d) are very similar between treatment and control group over the full study period.

4.2 Extensive margin effects

Next, we test whether the wage incentive increases the share that *ever* holds a job across different employment types. Table 5 shows the the incentive leads to an increase in the share of people graduating from vocational schools who are ever formally employed of 3.8 p.p. (8.6%) in the first year that grows to 5.4 p.p. (10.0%) after two years (Panel A and B, Col. 1). This masks large shifts in extensive margin treatment effects by type of formal employment. The share of vocational graduates with a permanent contract at some point over the first year increases by 7.6 p.p. (23.0%), while the share with temporary contracts decreases by 2.6 p.p. (15.3%) (Col. 3, 5). By contrast, the share that is ever informally employed decreases by 2.8 p.p. and 3 p.p. over the first 12 and 24 months, respectively (Panel A and B, Col. 7), suggesting that more than half of the formal extensive margin effects come from a shift from informal employment.

4.3 Intensive margin effects

While many governments try to increase the share of the labor force that obtains formal jobs, one challenge often overlooked is how to *retain* people in jobs once they find formal employment (Levy, 2010). Retention is important to both workers and firms. For workers, job stability allows them to benefit from the steep formal wage growth and provides certainty needed to make longer-term (financial) decisions. For firms, longer employment spells lead to cost reductions, e.g. in the hiring and training of workers.

Table 5: Extensive margin effects

Panel A: After one year

	Formal		Permanent		Temporary		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	0.038	-0.046	0.076***	-0.012	-0.026	-0.031	-0.028	-0.010
	(0.025)	(0.045)	(0.024)	(0.037)	(0.018)	(0.033)	(0.029)	(0.056)
Observations	1539	385	1539	385	1539	385	1170	271
R square	0.10	0.08	0.09	0.06	0.04	0.07	0.05	0.15
Control Mean	0.44	0.30	0.33	0.17	0.17	0.14	0.51	0.66
Std Dev	0.49	0.49	0.47	0.47	0.35	0.35	0.50	0.50
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

Panel B: After two years

	Formal		Permanent		Temporary		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	0.054**	-0.053	0.086***	-0.020	-0.011	-0.047	-0.030	-0.006
	(0.025)	(0.048)	(0.025)	(0.043)	(0.021)	(0.037)	(0.029)	(0.055)
Observations	1539	385	1539	385	1539	385	1170	271
R square	0.09	0.08	0.09	0.09	0.04	0.07	0.04	0.12
Control Mean	0.54	0.38	0.42	0.24	0.23	0.18	0.55	0.71
Std Dev	0.50	0.50	0.49	0.49	0.41	0.41	0.50	0.50
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

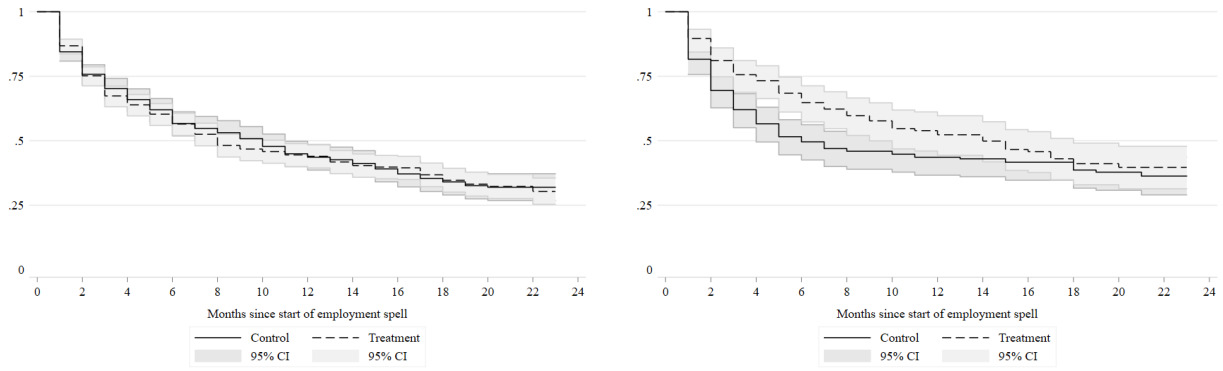
Notes: Source: IMSS administrative data for formal employment and endline survey data for informal employment, June 2019-May 2021. Robust standard errors are reported in parentheses. We control for demographic characteristics, socioeconomic status, work experience, time and school fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To analyze the effect of the wage incentive on job retention, we apply the AG extension of the Cox proportional hazards model for discrete recurrent events, such as losing employment. Figure 5 shows employment survival rates for work spells starting with permanent contracts (Panel 5a) and temporary contracts (Panel 5b). While there is no discernible difference between treatment groups for work spells beginning with permanent contracts, treatment youths who start with temporary contracts face a lower risk of exiting employment. To formally test differences in retention, Appendix Table A3 reports hazard rates separately for the first six months of employment (Panel A) and over the full employment spell (Panel B). There is no change in retention for permanent jobs during the first six months. By contrast, we observe a more than 50% reduction in turnover for jobs with temporary contracts over this period (Panel A, Col. 3). For the full employment period, retention effects decrease. However, hazard rates for temporary jobs continue to be 26.1% lower.[22] This is partly ex-

[22]It is also noteworthy that there is no discontinuity in retention when participants exhausted their wage

plained by the fact that treatment youths are 70% more likely to transition from temporary to permanent jobs (18.7 vs. 11 p.p.; p-value of 0.032).

Figure 5: Formal work stability by contract type (survival in employment)



(a) Survival in formal employment spell initiated with a permanent contract

(b) Survival in formal employment spell initiated with a temporary contract

Notes: Source: Administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021. Kaplan-Meier survival function with recurrent events. Recurrent hazard model allows for individuals to experience multiple exit events. Observations are person-spells. The sample for (a) includes youth who initiated at least one employment spell with a permanent contract during the study period (June 2019-May 2021), while the sample for (b) includes youth who initiated at least one employment spell with a temporary contract during the study period.

To further investigate the underlying reasons for high turnover rates in our study population, we collect data on reasons for why people left jobs in the endline survey. Specifically, we ask people for the main reason why their employment relationship ended. The most cited reason for workers with temporary contracts was that employers terminated the employment relationship (42%) compared to 20% who left for a job with better salary or working conditions (Appendix Figure B5). This relationship is reversed for workers with permanent contracts: 32% cite leaving for a better job while employers terminated job in 20% of cases. Being more in control of employment spells and able to leave for better jobs is another benefit of the shift from temporary to permanent jobs resulting from the wage incentive.

In sum, we find that the overall effect of the wage incentive is driven by two factors. First, youths shift into jobs with permanent contracts. Second, among treatment youths with temporary contracts we see an increase in retention, in part because these workers are able

benefits after six months on the job, suggesting that reference-dependent preferences, documented, e.g., by Kőszegi and Rabin (2006), DellaVigna et al. (2017) and DellaVigna et al. (2022) are less relevant for our setting.

to turn temporary work arrangements into jobs with permanent contracts.

4.4 Aggregate experience and income effects

This section estimates the aggregate experience and income gains resulting from the extensive and intensive employment effects previous discussed. Panel A in Table 6 shows aggregate experience gains across different employment types over two years. Treatment participants in vocational schools accrue an additional 1.03 (14.8%) more months of formal sector experience driven by a 1.23 (25.7%) increase in employment with permanent contracts (Col. 1, 3). There is no change in aggregate experience for informal or temporary work (Col. 5, 7).

Table 6: Aggregate experience and income effects

Panel A: Aggregate experience (months)

	Formal		Permanent		Temporary		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	1.033**	-0.242	1.234***	-0.116	-0.187	-0.126	-0.002	3.762*
	(0.420)	(0.660)	(0.378)	(0.480)	(0.269)	(0.503)	(0.853)	(2.114)
Observations	1539	385	1539	385	1539	385	1170	271
R square	0.13	0.08	0.12	0.06	0.03	0.08	0.08	0.25
Control Mean	6.96	3.75	4.81	1.97	2.16	1.78	9.57	14.59
Std Dev	8.44	8.44	7.39	7.39	5.24	5.24	16.07	16.07
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

Panel B: Monthly income (pesos)

	Formal		Permanent		Temporary		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	237.8**	-78.4	284.2***	-13.5	-22.4	-29.3	-2.3	413.9
	(105.9)	(152.7)	(89.2)	(99.5)	(55.2)	(99.7)	(129.1)	(292.5)
Observations	1539	385	1539	385	1539	385	1170	271
R square	0.16	0.08	0.15	0.05	0.03	0.08	0.08	0.32
Control Mean	1654	826	1089	384	446	354	1320	2065
Std Dev	2139	2139	1753	1753	1070	1070	2409	2409
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

Notes: Source: IMSS administrative data for formal employment and endline survey data for informal work. Robust standard errors are reported in parantheses. We control for demographic characteristics, socioeconomic status, work experience, time and school fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B shows treatment effects on monthly income (not conditional on the employment status). Vocational school graduates assigned to the treatment group earn on average 238 pesos (14.4%) more from formal work, driven by a 284 pesos (26.1%) increase in income

from jobs with permanent contracts. Informal income is not affected (Col. 7), implying that these are net gains in income. These income gains underestimate the benefits of the shift to (permanent) formal employment as these jobs entail eligibility to several social security programs (Table 2). Furthermore, the substantial human capital gains in these jobs are likely to lead to longer-term benefits not captured in our two-year study period.

5 Mechanisms

5.1 Framework

This section explores mechanisms for why the bonus is effective in increasing (permanent) formal employment and why not a larger share of workers pursue formal employment in the absence of our intervention. To guide our analysis, we sketch out a stylized job search model with informality as in Meghir et al. (2015), adapted to allow for wrong beliefs as in Mueller and Spinnewijn (2021).

A job seeker decides on her level of search effort e_i and reservation wage R_i (in addition to consumption level c_i) to maximize her utility. The reservation wage pins down the wage level at which a job seeker is indifferent between staying unemployed or accepting a job. Following Marinescu and Skandalis (2021), we assume that job seekers target their job search and choose optimal levels of search effort separately for the informal and formal sector s, which determines a sector specific job arrival function (λ_s).[23]

$$U(h_{i,t}) = \max_{c_{i,t}, e_{i,s,t}, R_{i,s,t}} (u(c_{i,t}, e_{i,s,t}|h_{i,t}) +$$

$$\beta(EU(h_{i,t+1}) + \hat{\lambda}_f(e_{i,f,t}|h_{i,t}) \int_{R_{i,f,t}} [EV(\hat{w_f}|h_{i,t+1}) - EU(h_{i,t+1})\, dF(\hat{w_f}|h_{i,t})]) +$$

$$\hat{\lambda}_{inf}(e_{i,inf,t}|h_{i,t}) \int_{R_{i,inf,t}} [EV(\hat{w}_{inf}|h_{i,t+1}) - EU(h_{i,t+1})\, dF(\hat{w}_{inf}|h_{i,t})])$$

[23] Abel et al. (2019) find that a shift from informal to formal search channels (resulting from a job search planning intervention) without a change in overall search time increases job offers and employment rates.

Utility in period t is determined by her consumption levels and search effort conditional on a vector of state variables h_i, which measures individual-specific characteristics including a job seeker's employment and search history. Utility in period $t+1$ is the sum of the value from state $h_{i,t+1}$ and the expected value from finding a job in the informal and formal sector, adjusted by discount factor β. The former is the product of the job finding rate λ_f and the expected utility gain given the perceived wage distribution $dF(\hat{w}_f|h_{i,t})$. Parameter $\hat{w}_s$ denotes the present value of a wage stream for a job in sector s, which takes into account sector-specific wage growth and benefits as well as separation risks. Given that we observe frequent job transitions among people that are employed (Table 1), we further assume that job search continues on the job as in Pissarides (1994).[24]

While informal and formal jobs can differ along many dimensions including travel distance and other amenities, the only parameter that our intervention affects in this optimization problem is that the wage incentive increases the value of formal employment from $EV(\hat{w}_f|h_{i,t+1})$ to $EV(\hat{w}_f + inc|h_{i,t+1})$ with inc denoting the present value of the incentive. In response to being offered the incentive, job seekers adjust their optimal search strategy by increasing search effort ($e_{i,f,t}$) and lowering reservation wages ($R_{i,f,t}$) for formal work (Mueller and Spinnewijn, 2021). One implication is that the largest treatment effect should be observed for job seekers with baseline reservation wages $R_{i,f,t}$ (just) above typical formal sector starting wages. We will explore this hypothesis next.

5.2 Reservation wages

Having unrealistically high reservation wages may present an important barrier for labor market entrants without extensive experience (Babcock et al., 2012; Abebe et al., 2017; Alfonsi et al., 2022).[25] This may help explain why youths do not pursue careers that are more stable and lucrative in the long-run but offer low starting wages. We find supportive evidence for this hypothesis in our study population. At baseline, about one third of our sample holds reservation wages that are above the average starting wage of formal sector jobs. Youths

[24] This assumption is supported by recent evidence showing that search on the job is pervasive, especially for entry level positions (Faberman et al., 2022). It simplifies the framework as it implies that there is no additional continuation value of remaining unemployed.

[25] Furthermore, Krueger and Mueller (2016) show that even over longer unemployment spells, job seekers in the U.S. do not adjust their reservation wages downward by much.

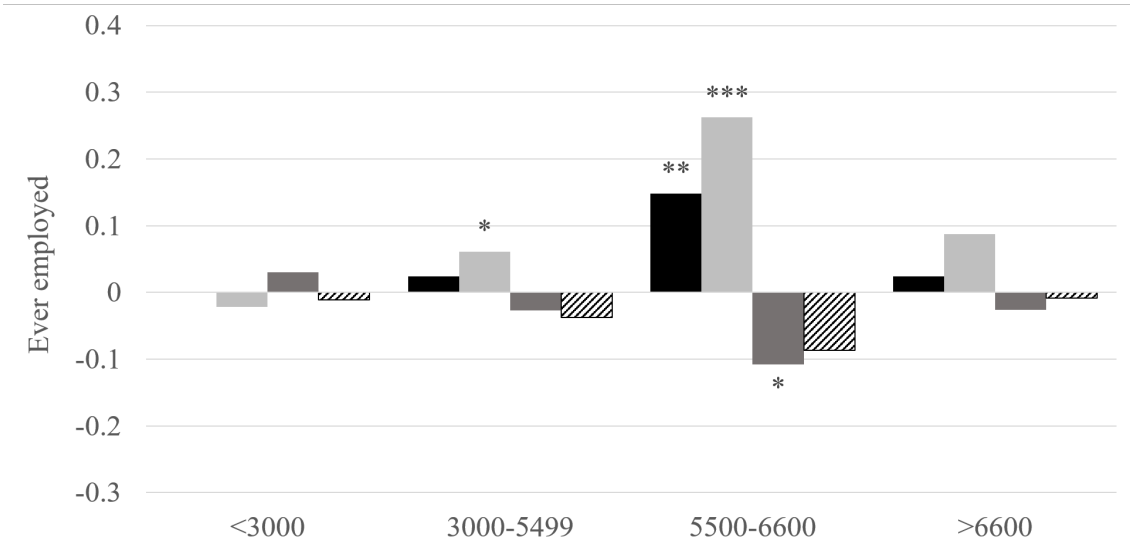
may thus decide to either stay unemployed or accept jobs in the informal sector, which tends to initially offer higher hourly wages and other benefits such as lower costs of commuting to work.Job seekers may also decide to pursue formal sector work with temporary contracts, which offer starting wages that are slightly higher, but provide less job stability and fewer benefits to workers.

The job search framework posits that a formal sector wage incentive lowers job seekers' reservation wages and increases job search for formal employment. The exact magnitude of the respective change in effort and reservation wages depends on the job finding function ($\lambda_f(e_{i,f,t}|h_{i,t})$). However, we can use the insight by Card and Hyslop (2005) that if job seekers do not change search effort - for which we find supportive evidence in our data[26] - the reduction of reservation wages is equal to the size of the incentive. This gives us a conservative upper bound for the drop in reservation wages to empirically test for the importance of wage expectations.

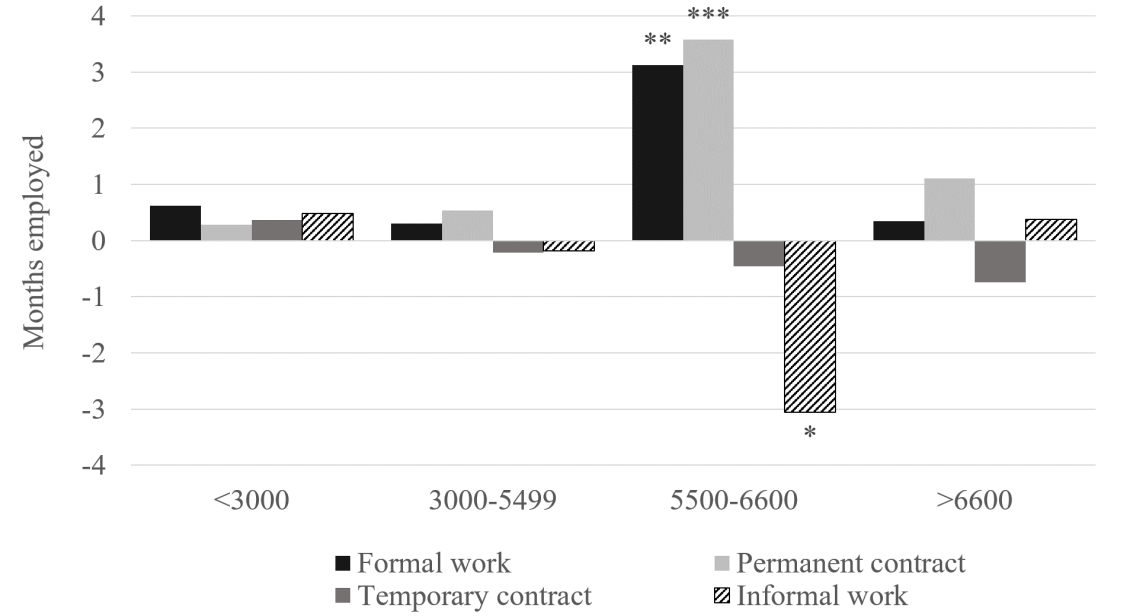
We find that reservation wages play an important role for employment choices. Figure 6 shows how treatment effects on the extensive (Panel A) and intensive (Panel B) margin differ when we divide our sample by participants' baseline reservation wages. The group of youths with reservation wage under 3,000 pesos shows no aggregate treatment effect nor a switching between jobs with temporary and permanent contracts. For the second group with reservation wages between 3,000 and the average starting salary of 5,500 pesos we also do not find an aggregate effect on the extensive or intensive margin. However, there is an increase of 6 p.p. in the share with a permanent contract. The third group includes participants with reservation wages of up to 20% above the average starting salary level; this group should be most affected by the incentive if reservation wages are binding. We indeed find a large aggregate employment effect for this group. The share with formal employment increases by 15 p.p. (Panel A) and the aggregate time formally employed goes up by more than 3 months (Panel B). This sharp increase is exclusively driven by an increase in work with permanent contracts, while the share with temporary work and informal work slightly decreases (Panel A). Strikingly, the intensive margin increase in (permanent) formal employment comes almost entirely from a reduction in informal work (Panel B), illustrating the importance of reservation wages for youths' career choices. By contrast, we do not ob-

[26]Specifically, we find that in the endline survey that the treatment group does not report higher level of job search activities. One limitation of this evidence is that we only collect job search data for the endogenously determined subgroup of people without work in our sample.

Figure 6: Incentive impacts by baseline reservation wage



(a) Extensive margin



(b) Intensive margin

Notes: Sources: Endline surveys and administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021. Graphs depict treatment coefficients for youth by subgroup of self-reported reservation wage at baseline; no control variables included in regression. Reservation wages are monthly in pesos. N-sizes for baseline-reported reservation wages are as follow: 542 youth reported below $3000, 823 youth reported $3000-$5499, 200 youth reported $5500-$6000, 359 youth reported over $6600. Standard errors are robust, *** p<0.01, ** p<0.05, * p<0.1.

serve significant changes for people with higher reservation wages (above 6,600 pesos). While the pattern of treatment effects is similar, the magnitude is much smaller and not significant.

The last piece of evidence in support of reservation wages as an underlying mechanism for the treatment effects comes from hourly wage data collected in the endline survey. Appendix Figure A2 shows how hourly wages evolve over the employment spell for participants with formal sector jobs. Compared to the control group, youths eligible for the wage incentive accept jobs that pay around 5 pesos (12%) less in the first few months (suggesting that they lowered reservation wages).

These results consistently suggest that reservation wages are binding for job seekers, which is in line with evidence from the U.S. by Krueger and Mueller (2016). It raises the question of why youths in our setting hold these high reservation wages in light of rapid skill and wage growth they would experience in formal employment. Next, we will explore the role of time preferences, false beliefs and liquidity constraints.

5.3 Discount factors

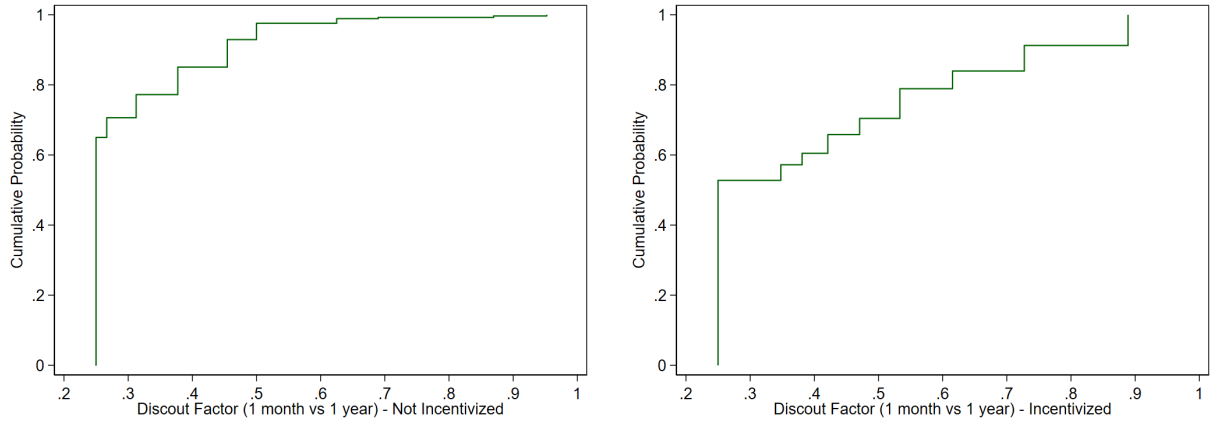
One explanation for why job seekers set high reservation wages and search little for jobs in the formal sector relative to the informal sector is that they heavily discount future benefits of formal employment including strong wage growth and being covered by social security programs.[27] We elicit participants' discount rates in two ways. First, as part of the in-person endline survey we use the traditional method of asking hypothetical choices between payments in one month or one year.[28] Second, we implement an incentive-compatible way to elicit time preferences for 665 respondents in our second endline survey. Specifically, respondents are invited to participate in a lottery. Their payout, if they win, is determined by their choices between receiving 2,000 pesos in one month or a larger amount in one year. We elicit time preferences through a set of binary choice questions and compute individual discount factors by dividing 2,000 by the amount at which they are switching to the payout

[27] Importantly, DellaVigna and Paserman (2005) show that in a search model with only one sector, higher time discount rates are associated with lower reservation wages since job seekers assign a lower value of future benefits of search. In our setting with two sectors, high discount rates lead job seekers to set high reservation wages for the formal relative to the informal sector since they discount the value of stronger future wage growth.

[28] By giving choices between one month and one year we elicit the longer term discount factor δ rather than the short time discount factor β. Letting participants choose between two future payouts also assuages concerns that choices are influenced by respondents' mistrust about whether future transfers will take place.

in one year (in steps of 500 pesos capped at 5,000 pesos).

Figure 7: Discount factors



(a) Discount Factor (Not Incentivized)

(b) Discount Factor (Incentivized)

Notes: Source: Endline Surveys. The graph shows the cumulative probability distribution of the discount factors collected in the in-person endline (panel a) and the incentivized SMS survey (panel b). It measures the factor that people are willing to discount payments in one year compared to in one month.

Figure 7 plots the cumulative distribution of discount factors. Figure 7a shows that for the non-incentivized elicitation, participants report very low discount factors. About 65% of respondents in our sample have a discount factor of about 0.25 or lower (the lowest bound) implying that they discount payouts in one year by at least 75% compared to payouts in one month. Discount factors are higher but still low for the incentivized elicitation with 52% reporting a discount factor of 0.25 or less.[29] These results may be surprising given that structural models typically assume (delta) discount parameters closer to 1 (Angeletos et al., 2001; Laibson et al., 2015). However, evidence suggests that young people (Harrison et al., 2002) and respondents in lower-income countries (Falk et al., 2018; Bartoš et al., 2021; John and Orkin, 2022) tend to discount future payouts by much more. One potential explanation for why people may heavily discount future payouts in these settings is that they are liquidity constrained (Cohen et al., 2020). This is unlikely to explain our results as discount rates are not correlated with respondents' socio-economic strata or their stated ability to raise funds for a risk-free profitable investment.

[29]These two measures are positively correlated (coefficient 0.23, p-value < 0.001).

Regardless of the underlying reason for *why* youths heavily discount future payouts, the implication is that jobs with lower starting wages but high future wage growth are relatively less attractive.[30] Table 7 tests how discount rates are correlated with participants' reservation wages. For the non-incentivized measure, we indeed find a strong negative relationship. Participants who discount the future by 10 percentage points more (i.e. their discount factor is lower) have reservation wages that are about 4.2 percent higher (Column 2). However, this correlation is small and not significant in the smaller sample for which we elicited incentivized discount factors (Column 3).

5.4 Beliefs about wage growth

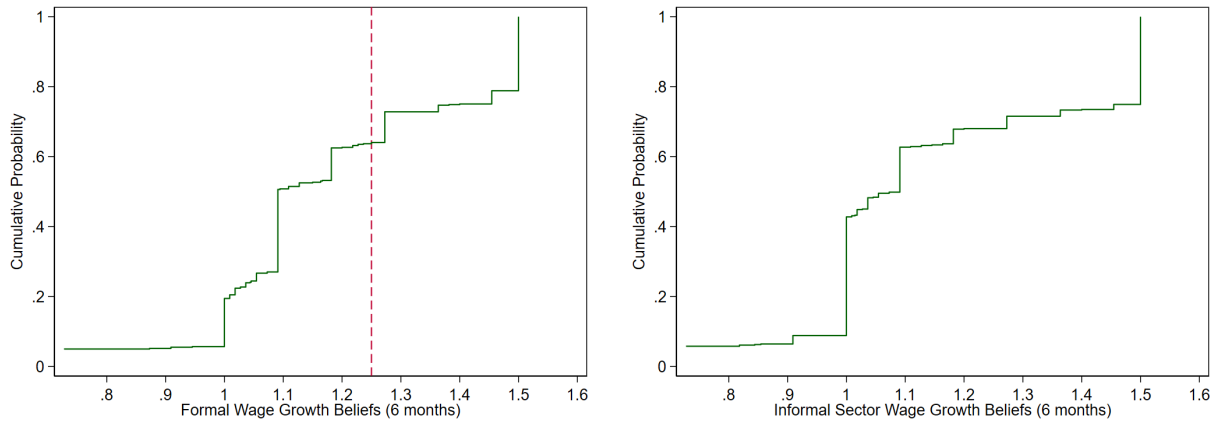
The job search framework posits that beliefs about wage growth affect job seekers' reservation wages and search effort. We elicit these beliefs in an incentive compatible way in the endline survey. Specifically, we present participants with a scenario of a person starting a formal job that pays 5,500 pesos per month. We ask them to estimate how much that person is earning in six months if she is still employed separately for jobs in the formal and informal sector. For each guess that is within 100 pesos of the correct answer, they receive a bonus of 30 pesos.

Figure 8a shows the cumulative distribution of wage growth beliefs for *formal* employment. The dotted line marks the actual wage growth of about 25% based on social security data. We find that the majority of participants substantially underestimate formal sector wage growth: 20% believe that there is no wage growth and the median belief is less than 10%. Only about one third either has correct beliefs or overestimate wage growth. Beliefs about *informal* sector wage growth appear to be more accurate. Figure 8b shows that more than 40% believe there is no wage growth and two thirds of youths believe that informal wages grow by less than 10%. While we do not have administrative data for informal wages, ENOE survey data suggests that the actual increase is between zero and 10%.[31]

[30] A related question is whether treatment effects vary across people's discount rates. Results (not reported) show that the interaction of the treatment indicator and people's discount factor is positive, but modest in magnitude and not statistically significant. There are at least two factors that make this test inconclusive: first, we only elicit discount rates at the endline (although discount rates are not significantly correlated with the treatment assignment). Second, we only collect discount rates for a subset of participants, which limits the statistical power of subgroup analyses.

[31] The hourly wage for informal work remains flat over the first half year while the monthly income increases by 10% suggesting that workers increase the number of hours over time. In our endline survey, respondents working informally are also less than half as likely to report that they received a wage increase compared to those working in the formal sector.

Figure 8: Wage growth beliefs



(a) Beliefs *formal* wage growth

(b) Beliefs *informal* wage growth

Notes: Source: Endline Survey. The graph shows the cumulative probability distribution of the perceived growth in formal (panel a) and informal (panel b) jobs over a six months period. The dotted line shows the actual growth rate computed with IMSS data.

Misconceptions of wage growth can reduce welfare (Mueller and Spinnewijn, 2021). According to the search framework, job seekers who underestimate formal sector wage growth and thus the present value of a formal sector job ($\hat{w}_f$) exert too little effort searching for formal work and have reservation wages that are too high compared to the optimal level pinned down by their preferences.[32] We indeed find that perceptions of wage growth and reservation wages are negatively correlated. Believing that wages grow by ten percentage points more (over a six-month period) is associated with having reservation wages that are 0.31 percent lower (Table 7, Column 1). While these findings are correlational, they reinforce concerns that misconceptions of wage growth may reduce welfare by inflating reservation wages. By contrast, the welfare implications of high discount rates are less clear since they present the preferences of job seekers.

[32] An alternative reason for why people may have misconceptions of the present value of formal employment is that they over or underestimate the separation rate. However, incentivized outcomes we collect suggest that these beliefs are fairly accurate. When asked about the chance of being employed in an informal and formal job, job seekers report 40% and 60% respectively, which is close to the correct numbers suggesting that these beliefs do not lead to suboptimal search behavior.

Table 7: Reservation wage correlates

	Reservation Wage (Log)			
	(1)	(2)	(3)	(4)
Wage growth belief	-0.031*			
	(0.018)			
Discount factor		-0.417**		
		(0.169)		
Discount factor (Inc.)			-0.004	
			(0.109)	
Liquidity Constrained				-0.103*
				(0.055)
Observations	533	1362	619	552
R square	0.01	0.00	0.00	0.01

Notes: Source: Endline survey. The dependent variable is the log of reservation wages reported at endline. *Wage growth belief* measures the perceived percentage increase over a six months spell. *Discount factor* measures how much people discount future payments, collected in the in-person edline. *Discount factor (Inc.)* captures the incentivized measure of how much people discount future payments, collected in the SMS edline. *Liquidity constrained* is a dummy measuring whether people are not able to raise funds for a risk free profitable investment. Robust standard errors are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.5 Other explanations

In addition to underestimating wage growth, youths may also under-value coverage by social security programs, either because they are not aware of these programs and / or heavily discount these future benefits. In the endline survey we ask participants whether they know what constitutes a formal job: 31% of the control group did not known, suggesting that a lack of knowledge may be a partial explanation for low rates of formal work. The share is reduced to 17% in the treatment group, indicating that the wage incentive helps to raise awareness. We also ask how much of their salary participants would be willing to have deducted in order to be covered by social security programs. The average (median) response is 17% (14%) of their wage.[33] This is considerably lower than the legally mandated social security contributions, which stand at around 26% of formal wage rates (Levy, 2010).[34] However, it is unclear whether this means that job seekers underestimate the *marginal* benefit

[33]In the treatment group the willingness to pay is 55 pesos (5.7%) higher. However this difference is not significant (p-value: 0.136).

[34]This gap can help explain why formal firms in Mexico often hire workers informally (de la Parra, 2017).

of social security coverage given that they already have access to some services including publicly provided health insurance (Bosch and Campos-Vazquez, 2014).

Second, we test for the importance of liquidity constraints, which may drive up reservation wages if people are in urgent need of money and cannot borrow (Cohen et al., 2020). While it is theoretically plausible that our incentive alleviates liquidity constraints and thus allows beneficiaries to accept formal employment, we do not find supportive evidence in our data. We form a baseline index of participants' socio-economic strata (a common proxy for being liquidity constrained) and find that treatment effects are largest for the middle SES tercile (Appendix Figure B1). We also collect a direct measure of being liquidity constrained at endline by asking participants *"Imagine there is a risk-free very profitable investment opportunity. Could you raise 5,000 pesos to invest in it?"* We find that treatment effects for the 60% who cannot raise the funds is, if at all, smaller than for those who say they can. Liquidity constrained participants also report reservation wages that are approximately 10% *lower* (Table 7, Column 6).

Last, it is theoretically possible that the treatment affects firm screening. One of the key rationales for wage incentive programs for youths is that it incentivizes firms to hire candidates with little work experience, which can increase overall labor market efficiency (Pallais, 2014). While this is a priori less likely to be important for incentives paid to workers (as they do not reduce hiring costs), it is still possible that the incentive affects firm selection by nudging different types of job seekers to apply for work (Abebe et al., 2021b). To test this hypothesis, we analyze whether formal sector hires in the treatment and control group differ by their secondary school grades. Appendix Table B3 shows results from a regression in which we interact school grades with a treatment dummy, effectively testing whether those hired in the treatment group performed better in school. The interaction coefficient is close to zero and insignificant suggesting that the incentive does not affect firm selection in our setting.

6 Cost-benefit comparison

We calculate several variations of cost-benefits. We consider both a narrow cost measure that only includes payments made to beneficiaries as well as a broader measure that also

includes administrative costs of running the program (e.g. verifying employment status, transfer fees). We additionally consider benefits both on the extensive and intensive margin. One factor that decreases the cost of our intervention is that a substantial share of eligible youths do not complete the paperwork required to receive the incentive. Despite receiving SMS reminders and replacing lost debit cards, only 46.2% of youths with formal employment completed the registration and 38.3% submitted pay stubs (Appendix Table A4). While these figures are substantially higher than those of many related programs that pay wage subsidies to firms (e.g. Galasso et al. (2004); Levinsohn et al. (2014)), it raises the question of how the process can be simplified to increase program take-up. When we ask eligible beneficiaries in the endline survey why they did not complete the paperwork, the most frequent responses include that the registration process was too complicated (23%), people forgetting about it (21%), and that they lost the payment card or contact information (20%).

Focusing on vocational school graduates, we find a narrow (broad) cost-benefit measure of US$1,177 (US$1,404) per *additional* person with formal sector work experience compared to the control group over the first two years. The corresponding figure for permanent formal sector experience is US$739 (US$882) per person. These figures compare favorably to alternative ALMPs such as vocational training, which tend to cost at least $10,000 per additional person with formal work experience. Other interventions such as search and matching assistance tend to be cheaper but in most cases fail to lead to significant employment gains (see McKenzie (2017) for a review).

Turning to our cumulative results, we find that the program costs US$63 (US$75) per additional month with formal sector work experience and US$5.3 (US$6.4) per additional dollar earned from formal work. While the extensive margin effects are stable between the first and second year, one limitation of these cumulative measures is that treatment effects likely continue to increase past our two year study period, leading us to underestimate the cost-effectiveness of our intervention.

7 Concluding remarks

We study the effect of a temporary wage incentive in a population of secondary school graduates. We find substantial increases in formal employment for graduates of vocational schools

driven by gains in jobs with permanent contracts. The overall effect is due to both an increase on the extensive margin, partly resulting from youths switching from temporary to permanent jobs, and an increase in retention for youths starting on a temporary contract. Additionally, we find that formal employment leads to accumulation of human capital that benefits workers. Workers do not experience a drop in wages when they switch jobs, and income gains from (formal) employment are large at around 15% for vocational school graduates exclusive of additional coverage social security programs.

Our wage incentive experiment effectively increases workers' return to accepting formal employment – thus providing insights into an important debate about how the tax implicit in the design of social security and social protection systems affects formal sector labor supply (Packard et al., 2019). Social security deductions act like a tax on formal employment while social protection programs act like a subsidy to informal employment, widening the gap between the marginal product of formal and informal labor. It has been argued that this wedge may contribute to an oversized informal sector, which workers enter in part by choice (Levy and Cruces, 2021; Perry et al., 2007). For low-skilled workers in Mexico the implicit tax-cum-subsidy is estimated to be in the order of 34% (Levy, 2010).[35] Our experimental results suggest that a 20% increase in (starting) wages paid to workers increases formal employment by about 4 p.p. or 10%, which implies a labor supply elasticity of around 0.5 at the level of formal sector starting wages. For permanent formal employment, the elasticity is around 1.2, suggesting that a reduction in this tax-cum-subsidy would have sizeable effects for low-skilled youths in Mexico.

Together, these findings suggest that low-skilled youths with higher reservation wages may be opting out of formal employment – foregoing higher wages in the future, and some may even be opting out of the labor force. Moreover, youths who would not enter formal employment in the absence of our intervention are comparable in terms of quality to others hired formally, bearing out on previous work documenting a misallocation of labor in Mexico (Levy and López-Calva, 2020). Our study thus dovetails with evidence that factors that interfere with equalization of marginal products distort the allocation of resources from their most efficient use in less developed economies (Restuccia and Rogerson, 2017), and with a

[35]The implicit tax on formal salaried labor is 26% of the formal wage rate and the subsidy to non-salaried informal labor is 8% of the informal wage rate. Overall, subsidies paid to informal labor in Mexico amount to 2% of GDP, of which 0.65% of GDP are subsidies to salaried workers hired illegally, while de facto taxes paid on formal labor amount to 2.4% of GDP (Levy, 2010).

larger literature showing that misallocation can hurt productivity and growth (Restuccia and Rogerson, 2013; Hopenhayn, 2014).

One caveat is that our estimates present partial equilibrium effects. While we cannot formally test the magnitude of displacement effects, it is noteworthy that many formal sector employers, including in our study area, have open vacancies, often resulting from high turnover rates (Moon et al., 2022). In tight labor markets where employers struggle to fill positions, displacement effects tend to be less pronounced (Crépon et al., 2013). With this caveat in mind, our results suggest that a one-time temporary wage incentive can be a relatively cost-effective intervention with lasting benefits.

A final question is why one would need a wage incentive rather than simpler and cheaper interventions such as providing job seekers with information, which could nudge job seekers to lower their reservation wages. While more evidence is needed to conclusively test for these mechanisms, our results suggest that only providing information about wage growth is not sufficient if youths heavily discount these future benefits. In addition, participants may not fully update their beliefs either because they pay limited attention to information or do not find it credible or relevant.[36] In these contexts, programs that offer temporary, immediate benefits may be more effective.

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[36] Indeed, other studies show that students do not change beliefs when presented with information about wages (Conlon, 2021).

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A Appendix

Table A1: Average employment effects (pooled sample)

Panel A: One year

	Formal	Permanent	Temporary	Informal
	(1)	(2)	(3)	(4)
Incentive	0.036**	0.042***	-0.006	-0.006
	(0.014)	(0.013)	(0.009)	(0.023)
Observations	23088	23088	23088	17292
R square	0.11	0.10	0.03	0.08
Control Mean	0.22	0.15	0.07	0.42
Std Dev	0.43	0.37	0.26	0.49
Sample	Pooled	Pooled	Pooled	Pooled

Panel B: Two years

	Formal	Permanent	Temporary	Informal
	(1)	(2)	(3)	(4)
Incentive	0.032**	0.040***	-0.008	0.001
	(0.015)	(0.013)	(0.010)	(0.023)
Observations	46176	46176	46176	34584
R square	0.11	0.10	0.02	0.08
Control Mean	0.26	0.18	0.09	0.41
Std Dev	0.45	0.40	0.28	0.49
Sample	Pooled	Pooled	Pooled	Pooled

Notes: Source: IMSS administrative data for formal employment and endline survey data for informal employment, June 2019-May 2021. Observations are at the person-month level. Standard errors, clustered at individual level, are reported in parentheses. We control for demographic characteristics, socioeconomic status, work experience, time and school fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Average employment effects, no covariates

Panel A: After first year

	Formal		Permanent		Temporary		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	0.043**	-0.000	0.052***	-0.001	-0.008	0.000	-0.024	0.051
	(0.017)	(0.026)	(0.015)	(0.018)	(0.011)	(0.019)	(0.026)	(0.056)
Observations	18468	4620	18468	4620	18468	4620	14040	3252
R square	0.09	0.02	0.07	0.01	0.02	0.01	0.03	0.02
Control Mean	0.25	0.12	0.17	0.06	0.08	0.06	0.39	0.54
Std Dev	0.43	0.43	0.37	0.37	0.26	0.26	0.49	0.49
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

Panel B: After second year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	0.045**	-0.017	0.053***	-0.008	-0.007	-0.009	-0.018	0.058
	(0.018)	(0.028)	(0.016)	(0.020)	(0.011)	(0.021)	(0.025)	(0.055)
Observations	36936	9240	36936	9240	36936	9240	28080	6504
R square	0.08	0.02	0.07	0.02	0.01	0.01	0.03	0.02
Control Mean	0.29	0.16	0.20	0.08	0.09	0.07	0.39	0.54
Std Dev	0.45	0.45	0.40	0.40	0.28	0.28	0.49	0.49
Sample	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.	Techn.	Gener.

Notes: Source: IMSS administrative data for formal employment and endline survey data for informal employment, June 2019-May 2021. Observations are at the person-month level. Standard errors, clustered at individual level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Take-up of the incentive in the treatment group

	Wage bonus treatment group		Wage bonus treatment group and baseline plans to work after high school		Wage bonus treatment group and had formal employment (IMSS)	
Take-up by subgroups	No.	%	No.	%	No.	%
Initiated wage bonus registration by Aug 2021	366	37.7	265	49.4	287	55.2
Completed wage bonus registration by Aug 2021	274	28.2	207	38.6	240	46.2
Sent at least one paystub for wage bonus by Aug 2021	213	22.0	167	31.2	199	38.3
Received all 6 wage bonus payments by Aug 2021	147	15.2	114	21.3	137	26.3
Total	970	100	536	100	520	100

Notes: Table presents different measures of take-up depending on the universe considered eligible: (1) youth offered the wage bonus, (2) youth offered the wage bonus who had plans to work after graduation, or (3) youth offered the wage bonus who actually were eligible to claim it because they found a formal job at some point during the study period

Table A3: Wage bonus treatment effect on survival in employment

Panel A. Discrete recurrent hazard model, spells capped at 6 months

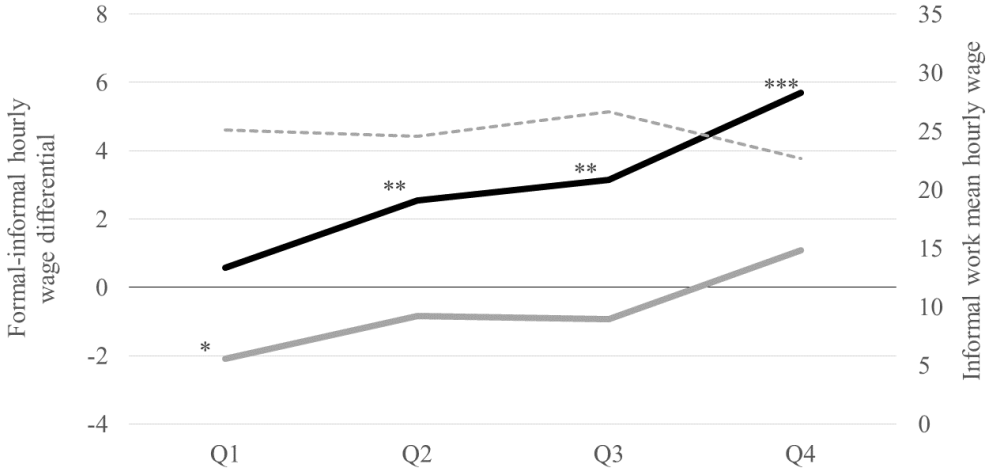
	Exited formal employment (1)	Exited formal employment with **permanent** contract (2)	Exited formal employment with **temporary** contract (3)
Incentive	−0.135	0.011	−0.505***
	(0.088)	(0.103)	(0.163)
	0.126	0.915	0.002
Observations (person-spells)	1413	1008	405

Panel B. Discrete recurrent hazard model, spells not capped

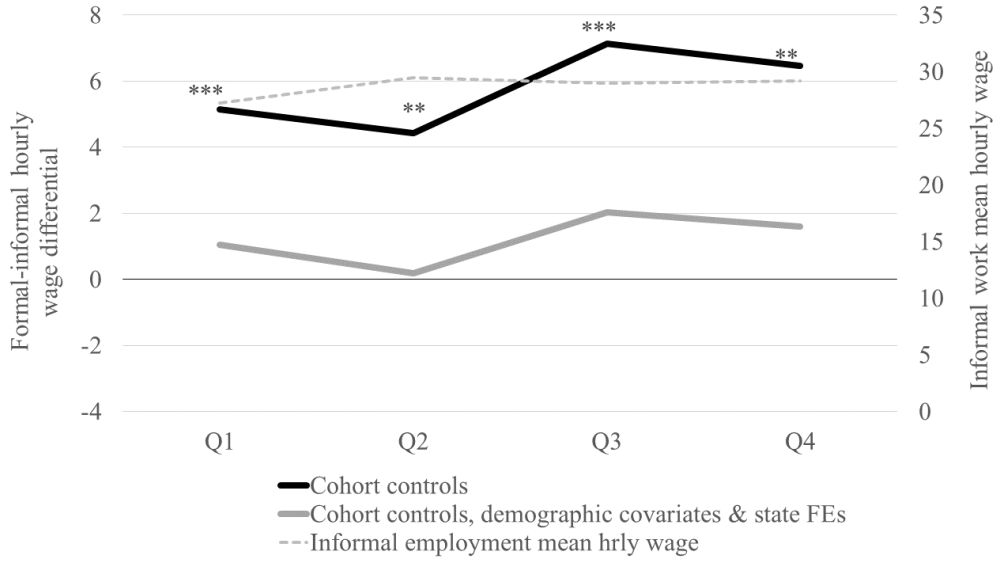
	Exited formal employment (1)	Exited formal employment with **permanent** contract (2)	Exited formal employment with **temporary** contract (3)
Incentive	−0.054	0.018	−0.261*
	(0.075)	(0.088)	(0.137)
	0.475	0.839	0.057
Observations (person-spells)	1413	1008	405

Notes: Source: Administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021. Standard errors clustered by ID are in parentheses, with p-values below (*** p<0.01, ** p<0.05, * p<0.1.). Job types are categorized based on their intial contract type. Spells are defined as continuous employment, regardless of changes in employer. For Panel A, spells are truncated at month 7, allowing for 6 months of tenure after the initial month – the length of time for incentive eligibility; for Panel B, spells are not truncated. Regressions do not include control variables. Results are robust to inclusion of stratifying covariates (gender and school type).

Figure A1: Wage differentials between formal and informal work across employment spells lasting at least three quarters



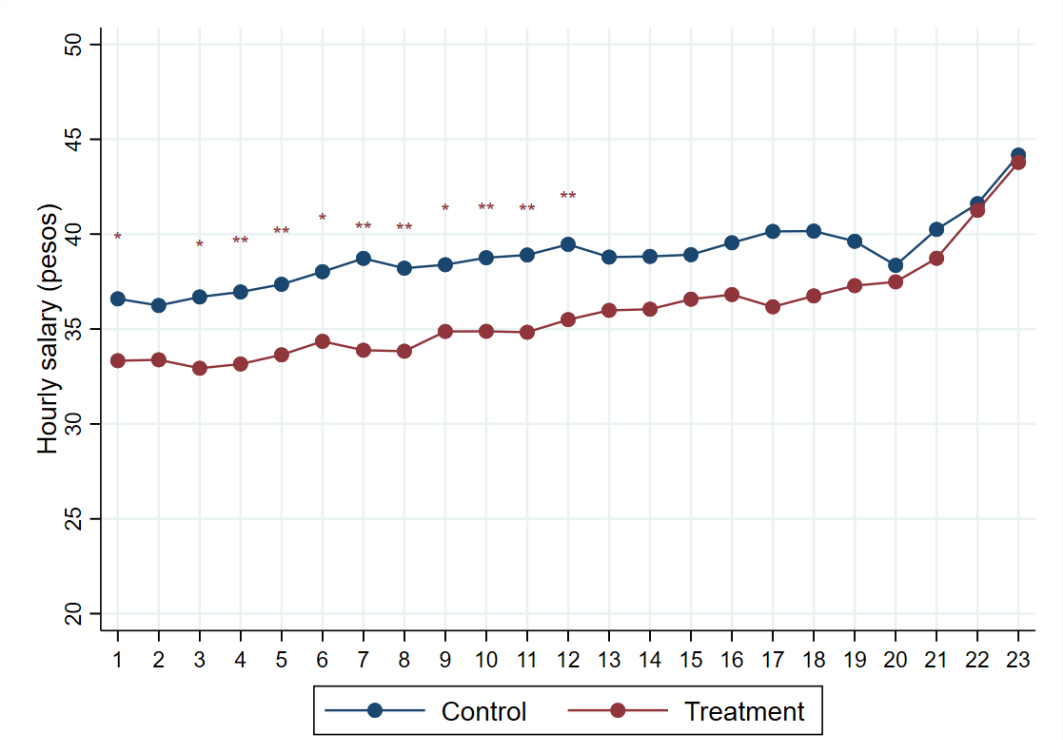
(a) Youth ages 17 to 20



(b) Youth ages 21 to 24

Notes: Source: National-level panel data from ENOE 2016-2019. Standard errors are robust, *** p<0.01, ** p<0.05, * p<0.1. Graphs depict trajectories of hourly wage differentials across youth employment spells. Solid lines depict formal-informal wage differentials over time with and without state fixed effects and demographic controls (for gender, marital status, and education level). The dotted line shows average hourly wage for youth with informal employment spells. Cohort controls refer to the rotating nature of the ENOE panel. Hourly wages are in pesos. Sample limited to youth starting a new employment spell, with positive wages and with at least three quarters of continuous employment.

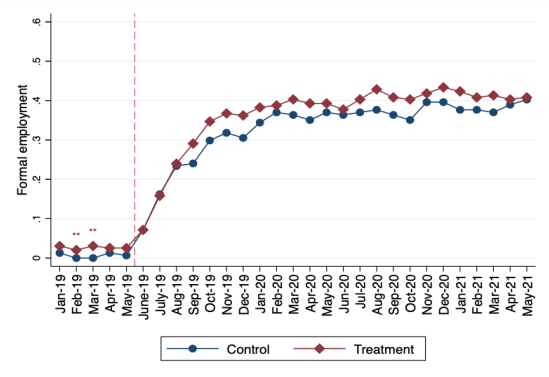
Figure A2: Hourly Wages (Formal Sector)



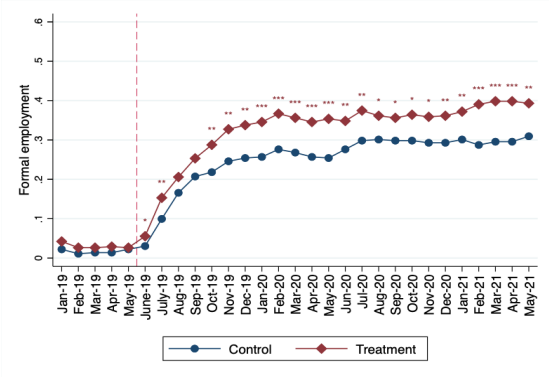
Notes: Source: Endline Survey. The graph shows how hourly wages evolve over formal employment spells. Wages are winsorized at the 95% level. *** p<0.01, ** p<0.05, * p<0.1.

B Online Appendix

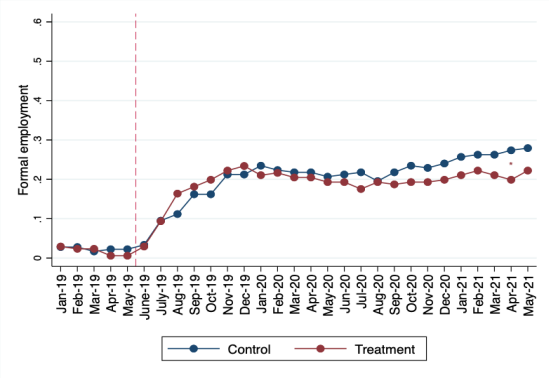
Figure B1: Employment impacts of wage bonus treatment by socioeconomic strata



(a) Lower socioeconomic strata



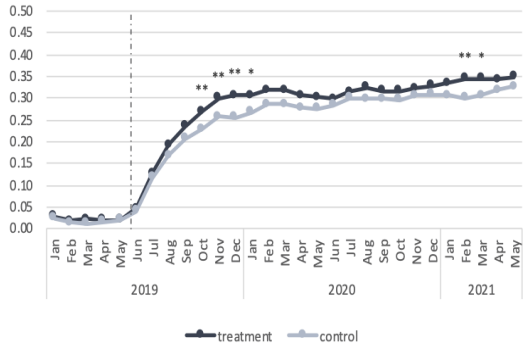
(b) Medium socioeconomic strata



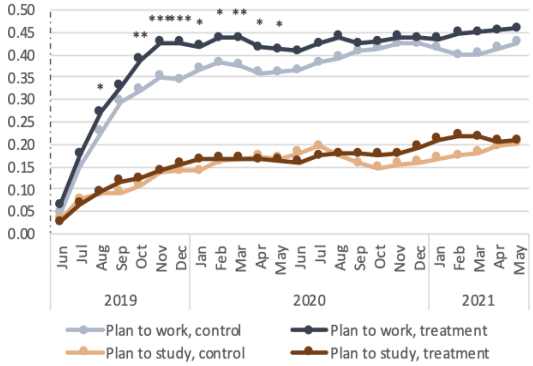
(c) Higher socioeconomic strata

Notes: Source: Administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021.

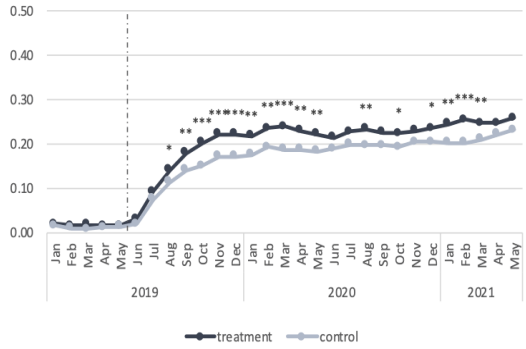
Figure B2: Employment impacts of wage bonus treatment, overall and by baseline plans



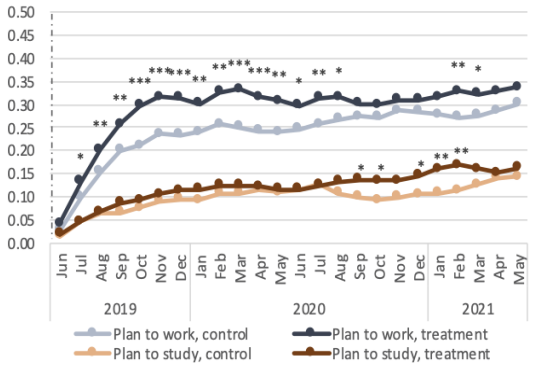
(a) Share with formal work by treatment



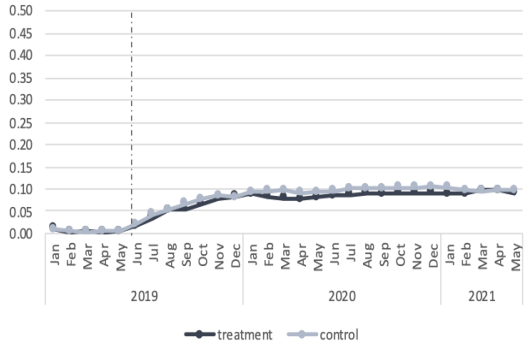
(b) Share with formal work by treatment and baseline plans



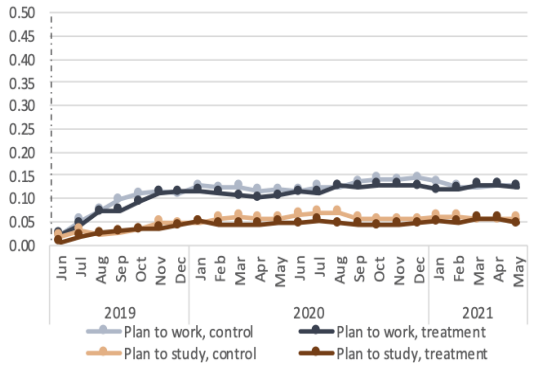
(c) Share with permanent contract by treatment



(d) Share with permanent contract by treatment and baseline plans



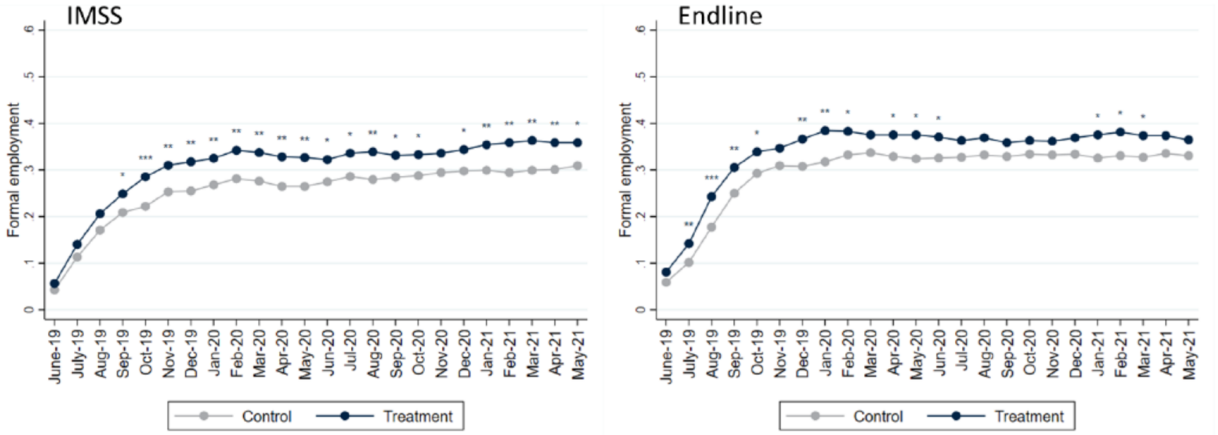
(e) Share with temporary contract by treatment



(f) Share with temporary contract by treatment and baseline plans

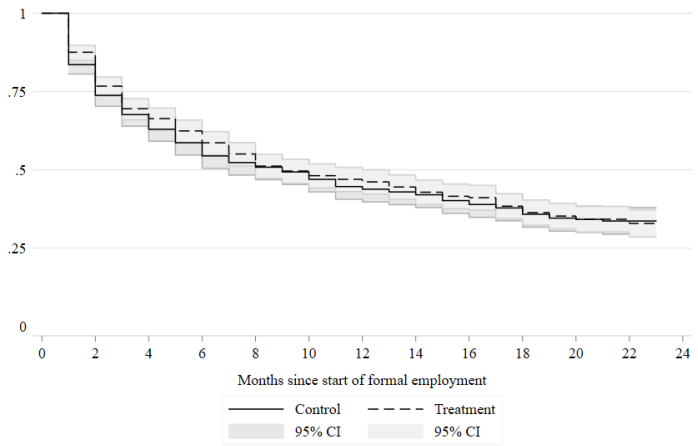
Notes: Source: Administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021. Universe includes all treatment and control youth who had formal work for at least one month during the study period. Note that while the IMSS covers most most workers with formal employment, it does not include public sector workers or workers in the XX industries. Baseline plans to work refer to those who at baseline planned to work or work and study after June 2019. Baseline plans to study refer to those who at baseline planned to only study. The vertical dotted line in panels (a), (c), and (e) indicates the start of the wage bonus intervention.

Figure B3: Self-reported vs. survey data



Notes: Source: IMSS administrative data and endline survey data, June 2019-May 2021. The graph compares monthly formal employment rates between administrative data and endline survey responses, holding the sample constant.

Figure B4: Employment stability (hazard rates for formal employment)

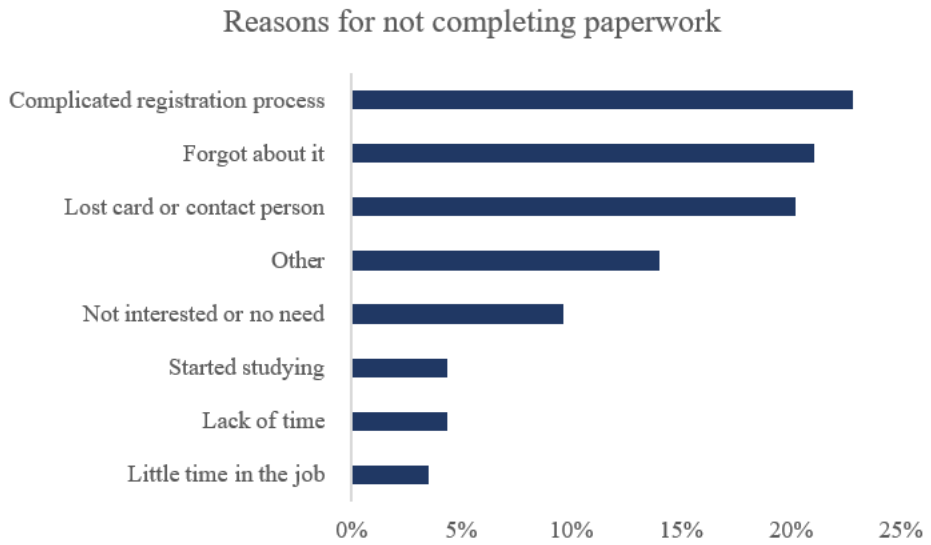


Notes: Source: Administrative data from the Instituto Mexicano del Seguro Social (IMSS). Kaplan-Meier survival function with discrete discontinuous recurrent events. Discrete discontinuous recurrent hazard model allows for individuals to experience multiple discrete events (i.e. a youth may have several discrete employment spells ending in an exit from employment). Observations person-spells. The graph depicts survival in a continuous spell of formal employment. The universe includes 924 youth who had formal work for at least one month during the study period (June 2019-Dec 2020). Total youth sample is 1,924. Note that while the IMSS covers most most workers with formal employment, it does not include public sector workers.

Figure B5: Reasons for leaving jobs

Notes: Source: Endline survey.

Figure B6: Reasons for not completing paperwork



Notes: Source: Endline survey. Answers from 114 youth selected for treatment that did not register to receive the incentive.

Figure B7: Letter for treatment group

¡Felicidades!

Queremos contarte que resultaste seleccionad@ en el sorteo para recibir un apoyo económico de **$900 al mes** por un periodo de hasta 6 meses, siempre y cuando:

1. Encuentres un empleo formal
   y,
2. Permanezcas en la misma empresa donde te registres.

Puedes trabajar en la empresa que tú quieras, lo importante es que sea un trabajo formal y te quedes un tiempo para darte cuenta si te gusta o no.

¡Puedes reclamar hasta $5,400 adicionales ($900 mensuales) a tu salario por tener un empleo formal!

Tu folio de selección:

Una vez que consigas y compruebes que cuentas con un empleo formal, tu apoyo económico será depositado mensualmente en la tarjeta Sí Vale que recibiste junto con esta carta.

Al ser una prueba piloto, contamos con recursos limitados y únicamente podemos ofrecer cierto número de apoyos. La selección para recibir este apoyo se hizo mediante una rifa donde todos los jóvenes que respondieron nuestra encuesta en abril de 2019 contaron con la misma probabilidad de ser elegidos.

Para cualquier duda o aclaración sobre este apoyo, no dudes en comunicarte:

→ Por teléfono o WhatsApp: **55 7459 7195**
→ Por correo electrónico: info@futuroconempleo.mx

Además, no olvides responder nuestros mensajes SMS para ir acumulando puntos intercambiables por dinero que será depositado a tu tarjeta Sí Vale. Estas dinámicas serán independientes a tu condición laboral.

Figure B8: Letter for control group

¡Lo sentimos!

Queremos contarte que lamentablemente no resultaste seleccionad@ en el sorteo para recibir un apoyo económico de $900 al mes por hasta 6 meses por vincularte a un empleo formal.

Al ser una prueba piloto, contamos con recursos limitados y únicamente podemos ofrecer cierto número de apoyos. La selección para recibir este apoyo se hizo mediante una rifa donde todos los jóvenes que respondieron nuestra encuesta en abril de 2019 contaron con la misma probabilidad de ser elegidos.

Sin embargo, puedes participar en las dinámicas de nuestros mensajes SMS para ir acumulando puntos intercambiables por dinero en la tarjeta Sí Vale que recibiste con esta carta. Estas dinámicas serán independientes a tu condición laboral.

Para cualquier duda o aclaración, no dudes en comunicarte:

→ Por teléfono o WhatsApp: **55 7459 7195**
→ Por correo electrónico: info@futuroconempleo.mx

Atentamente,

El equipo de Futuro con Empleo

Table B1: Randomization check, general schools

	Total		Wage bonus treatment (1)		Wage bonus control (2)		Difference
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)
Female	385	0.577	193	0.580	192	0.573	0.007
		[0.025]		[0.036]		[0.036]	
Age (mean)	385	17.694	193	17.684	192	17.703	-0.019
		[0.035]		[0.053]		[0.046]	
Married	385	0.036	193	0.041	192	0.031	0.010
		[0.010]		[0.014]		[0.013]	
Caregiver	385	0.208	193	0.202	192	0.214	-0.011
		[0.021]		[0.029]		[0.030]	
Home internet	385	0.252	193	0.249	192	0.255	-0.007
		[0.022]		[0.031]		[0.032]	
Number bathrooms (HH)	385	1.023	193	1.005	192	1.042	-0.036
		[0.030]		[0.041]		[0.043]	
Number bedrooms (HH)	385	3.306	193	3.264	192	3.349	-0.085
		[0.056]		[0.082]		[0.075]	
Number cars (HH)	385	1.216	193	1.238	192	1.193	0.046
		[0.055]		[0.084]		[0.071]	
HH SES well-being score (mean)	385	127.719	193	126.591	192	128.854	-2.263
		[2.199]		[2.926]		[3.291]	
Number of earners (HH)	383	1.843	192	1.833	191	1.853	-0.020
		[0.056]		[0.079]		[0.079]	
Grit score	385	3.456	193	3.497	192	3.415	0.082*
		[0.025]		[0.034]		[0.036]	
Vocational school	385	0.000	193	0.000	192	0.000	N/A
		[0.000]		[0.000]		[0.000]	
General school	385	1.000	193	1.000	192	1.000	N/A
		[0.000]		[0.000]		[0.000]	
Commute time home-downtown	385	0.958	193	0.990	192	0.926	0.064
		[0.023]		[0.033]		[0.033]	
Plans to work after high-school graduation	385	0.405	193	0.363	192	0.448	-0.085*
		[0.025]		[0.035]		[0.036]	
Plans to study only after high-school	385	0.571	193	0.606	192	0.536	0.070
		[0.025]		[0.035]		[0.036]	
Has a job at baseline	385	0.257	193	0.238	192	0.276	-0.038
		[0.022]		[0.031]		[0.032]	
Has held a formal job	385	0.047	193	0.041	192	0.052	-0.011
		[0.011]		[0.014]		[0.016]	
Reservation wage (mean)	385	4075.657	193	4053.368	192	4098.063	-44.695
		[120.045]		[176.081]		[163.590]	
F-test of joint significance (F-stat)							0.672
F-test of joint significance (p-value)							0.806

Notes: Source: Baseline survey. The value displayed the Difference column are differences in the means across the groups. *, **, *** indicate t-test significance at the 10, 5, and 1 percent critical level. The value displayed for F-tests are the F-statistics. Standard errors are robust. Household vulnerability score is based on the national Mexican AMAI measure which considers the number of bathrooms, bedrooms, cars, and employed persons in the household, as well as internet access and head of household education level. Lower scores indicate greater vulnerability

Table B2: Randomization check, vocational schools

	Total		Wage bonus treatment (1)		Wage bonus control (2)		Difference
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)
Female	1539	0.482	777	0.479	762	0.486	-0.007
		[0.013]		[0.018]		[0.018]	
Age (mean)	1539	17.841	777	17.806	762	17.877	-0.071*
		[0.019]		[0.025]		[0.030]	
Married	1539	0.019	777	0.022	762	0.016	0.006
		[0.003]		[0.005]		[0.005]	
Caregiver	1539	0.225	777	0.228	762	0.222	0.006
		[0.011]		[0.015]		[0.015]	
Home internet	1539	0.543	777	0.533	762	0.552	-0.020
		[0.013]		[0.018]		[0.018]	
Number bathrooms (HH)	1538	1.178	777	1.174	761	1.181	-0.008
		[0.014]		[0.020]		[0.020]	
Number bedrooms (HH)	1539	3.105	777	3.085	762	3.125	-0.040
		[0.027]		[0.038]		[0.038]	
Number cars (HH)	1539	0.985	777	0.996	762	0.974	0.022
		[0.022]		[0.032]		[0.030]	
HH SES well-being score (mean)	1539	142.579	777	142.112	762	143.055	-0.943
		[1.013]		[1.453]		[1.413]	
Number of earners (HH)	1531	2.026	772	2.005	759	2.047	-0.042
		[0.026]		[0.037]		[0.036]	
Grit score	1539	3.556	777	3.593	762	3.519	0.074***
		[0.013]		[0.019]		[0.018]	
Vocational school	1539	1.000	777	1.000	762	1.000	N/A
		[0.000]		[0.000]		[0.000]	
General school	1539	0.000	777	0.000	762	0.000	N/A
		[0.000]		[0.000]		[0.000]	
Commute time home-downtown	1539	0.709	777	0.688	762	0.732	-0.044**
		[0.011]		[0.015]		[0.016]	
Plans to work after high-school graduation	1539	0.595	777	0.600	762	0.589	0.011
		[0.013]		[0.018]		[0.018]	
Plans to study only after high-school	1539	0.401	777	0.396	762	0.406	-0.009
		[0.012]		[0.018]		[0.018]	
Has a job at baseline	1539	0.400	777	0.404	762	0.396	0.008
		[0.012]		[0.018]		[0.018]	
Has held a formal job	1539	0.216	777	0.211	762	0.222	-0.011
		[0.010]		[0.015]		[0.015]	
Reservation wage (mean)	1539	4829.697	777	4779.768	762	4880.608	-100.839
		[88.179]		[116.258]		[132.960]	
F-test of joint significance (F-stat)							1.410
F-test of joint significance (p-value)							0.130

Notes: Source: Baseline survey. The value displayed the Difference column are differences in the means across the groups. *, **, *** indicate t-test significance at the 10, 5, and 1 percent critical level. The value displayed for F-tests are the F-statistics. Standard errors are robust. Household vulnerability score is based on the national Mexican AMAI measure which considers the number of bathrooms, bedrooms, cars, and employed persons in the household, as well as internet access and head of household education level. Lower scores indicate greater vulnerability

Table B3: Firm selection by worker ability

Outcomes	Ever had formal work		Ever had permanent contract		Ever had temporary contract	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive	0.028	−0.154	0.062***	0.067	−0.020	−0.265
	(0.023)	(0.267)	(0.022)	(0.266)	(0.019)	(0.229)
	0.224	0.563	0.006	0.802	0.278	0.249
High school grade		−0.069***		−0.056**		−0.055**
		(0.024)		(0.024)		(0.021)
		0.005		0.019		0.011
Incentive * High school grade		0.024		0.002		0.029
		(0.033)		(0.033)		(0.028)
		0.465		0.945		0.297
Constant	0.508***	1.111***	0.388***	0.871***	0.218***	0.686***
	(0.016)	(0.196)	(0.016)	(0.193)	(0.013)	(0.176)
	0.000	0.000	0.000	0.000	0.000	0.000
Observations	1924	1357	1924	1357	1924	1357
R-squared	0.001	0.011	0.004	0.016	0.001	0.007

Notes: High school grade measures participants' performance during their final year of schooling. Standard errors are robust. Incentive refers to the wage bonus treatment offer * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B4: Average effects, extensive margin (Pooled sample)

Panel A: After first year

	Formal	Permanent	Temporary	Informal
	(1)	(2)	(3)	(4)
incentive	0.023	0.049***	-0.020*	-0.008
	(0.017)	(0.015)	(0.012)	(0.022)
Observations	23088	23088	23088	17292
R square	0.14	0.12	0.05	0.10
Control Mean	0.30	0.21	0.11	0.35
Std Dev	0.46	0.41	0.32	0.48

Panel B: After second year

	(1)	(2)	(3)	(4)
incentive	0.024	0.056***	-0.022	-0.009
	(0.018)	(0.017)	(0.014)	(0.022)
Observations	46176	46176	46176	34584
R square	0.14	0.13	0.05	0.10
Control Mean	0.39	0.29	0.16	0.20
Std Dev	0.49	0.45	0.37	0.40

Notes: Source: IMSS administrative data for formal employment and endline survey data for informal employment, June 2019-May 2021. P-values in parantheses. Standard errors clustered at individual level. We include covariates regarding demographics, socioeconomic status, work experience, time and school fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B5: Aggregate experience and income effects (pooled sample)

Panel A: Difference in experience (months)

	Formal	Permanent	Temporary	Informal
	(1)	(2)	(3)	(4)
incentive	0.697*	0.889***	-0.180	0.762
	(0.362)	(0.318)	(0.238)	(0.799)
Observations	1924	1924	1924	1441
R square	0.14	0.14	0.03	0.13
Control Mean	6.31	4.24	2.08	10.51
Std Dev	8.31	7.13	5.43	15.58

Panel B: Difference in monthly income (pesos)

	(1)	(2)	(3)	(4)
incentive	156.3	238.6***	-79.7	62.0
	(99.9)	(85.4)	(66.8)	(140.7)
Observations	1924	1924	1924	1924
R square	0.16	0.16	0.03	0.09
Control Mean	1553.60	1004.04	552.48	1264.80
Std Dev	2312.02	1882.25	1589.03	3162.01

Notes: Source: IMSS administrative data for formal employment and endline survey data for informal employment, May 2021. P-values in parantheses. Standard errors clustered at individual level. We include covariates regarding demographics, socioeconomic status, work experience, time and school fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B6: Wage bonus treatment effect on survival in employment

Panel A. Discrete recurrent hazard model, spells capped at 6 months

	Exited formal employment (1)	Exited formal employment with **permanent** contract (2)	Exited formal employment with **temporary** contract (3)
Incentive	−0.132	−0.010	−0.501***
	(0.089)	(0.105)	(0.179)
	0.141	0.924	0.005
Observations (person-spells)	1413	1008	405

Panel B. Discrete recurrent hazard model, spells not capped

	Exited formal employment (1)	Exited formal employment with **permanent** contract (2)	Exited formal employment with **temporary** contract (3)
Incentive	−0.029	0.036	−0.182
	(0.077)	(0.090)	(0.146)
	0.703	0.688	0.213
Observations (person-spells)	1413	1008	405

Notes: Source: Administrative data from the Instituto Mexicano del Seguro Social (IMSS), June 2019-May 2021. Standard errors clustered by ID are in parentheses, with p-values below (*** p<0.01, ** p<0.05, * p<0.1.). Job types are categorized based on their initial contract type. Spells are defined as continuous employment, regardless of changes in employer. For Panel A, spells are truncated at month 7, allowing for 6 months of tenure after the initial month – the length of time for incentive eligibility; for Panel B, spells are not truncated. Results include a full set of covariates and school fixed effects.