

Unregistered work among refugees - Findings from a list experiment in Germany*

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Abstract

With the rising number of refugees seeking protection worldwide, many host countries face the challenge to integrate them into their society and labor market. Structural barriers to employment and difficult personal circumstances often complicate this task and lead to a situation in which refugees end up in informal jobs. Due to the illegal nature of unregistered work, little is known about this phenomenon, however. In this paper, we implement a list experiment in a survey of refugees to measure their exposure to unregistered work in Germany. Our results indicate that more than 30% of the respondents had worked in an unregistered job at some point since their arrival. Furthermore, we find approved asylum status and not having children as positive risk predictors and being allowed to work and better education as negative ones. These results can be used as starting point for further research and tailored measures against unregistered work.

Keywords: Unregistered work, informal employment, list experiment, refugees, Germany, survey experiment

JEL Codes: J46, J61, C83

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1 Introduction

According to the UNHCR (2019), the number of refugees worldwide is at a record high with 29.4 million people seeking protection in another country. Given past experience, many will stay in their current host countries for a long time, which poses a great challenge to these countries as they need to integrate the newcomers into their societies. An important step on this way is the successful labor market integration of refugees. This task is complicated, however, by a number of legal and formal barriers, often interacted with difficult personal circumstances and background of refugees. Thus, they need more time to find employment in significant numbers than other types of migrants (Brücker et al., 2019). At the same time, anecdotal evidence suggests that many refugees end up in unregistered work as a consequence (Deutschlandfunk, 2016; ZEIT Online, 2017), i.e., they regularly earn money for jobs that are not registered with the tax authorities or the social insurance.¹ This situation exposes the refugees to exploitative work conditions and harms society through tax evasion and benefit fraud. In order to devise effective policy instruments to reduce this problem, it is therefore important to quantify the extent of unregistered work and to identify predictive risk factors. Due to its delicate nature and possible negative consequences, affected individuals tend to remain silent about their experiences. As a result, our knowledge about the prevalence of unregistered work in general and among refugees in particular is very limited.

In this study, we aim at filling this gap by examining the phenomenon of unregistered work among refugees in Germany. Germany currently hosts the largest number of refugees (in absolute terms) in Europe (UNHCR, 2019) and thus serves as a good example of a developed country and its attempts to deal with the labor market integration of refugees. To overcome the problem of measuring undeclared work, we implement a “list experiment” in a survey of more than 1,200 refugees in the state of Baden-Württemberg. This experimental method, also known as item count technique, is designed to guarantee a high degree of anonymity and thereby elicit truthful responses to sensitive questions that reveal inappropriate behavior or illegal activities, and are related to shame and social desirability bias (Droitcour et al., 1991; Kuklinski et al., 1997).² List experiments were successfully applied in political and social science to estimate the prevalence

¹ Throughout the paper, we will use the term “unregistered work” as synonym for “illegal work”, “informal employment”, and “undeclared work”.

² For an overview of list experiments and other indirect question methods, see Blair (2015).

of illegal conduct like vote buying and drug use (e.g., Çarkoğlu and Aytaç, 2015; Wolter and Laier, 2014), as well as to investigate attitudes towards minorities (e.g., Kuklinski et al., 1997; Coffman et al., 2016) and very sensitive issues related to armed conflicts, war crimes, or female genital cutting (e.g., Blair et al., 2014; Traunmueller et al., 2019; De Cao and Lutz, 2018). Most closely related to this study, Kirchner et al. (2013) use a list experiment to examine undeclared work activities in the general population in Germany. They find a prevalence rate of roughly 6.4% for unregistered work for a company and show that this result is significantly higher than the 1.2% of the same respondents who indicated this outcome as answer to a direct question.

The mechanics of a list experiment are simple: A sample of respondents is randomly split in two groups. One is confronted with a list of non-sensitive items, in our case possible experiences on the German labor market, the other faces the same list plus the sensitive item of interest, here the experience of unregistered work since arrival. In both groups, the participants do not have to state which of the items they have experienced already, only how many in total. This way, not even the interviewers could infer from the total count which of the items were already experienced by the respondent. We then estimate the share of respondents who have experienced unregistered work from differences in the answer behavior between the two groups. Furthermore, we apply a multivariate analysis to explore correlations between the affirmation of the sensitive item and respondents' characteristics to identify possible determinants.

Overall, we find that more than 30% of refugees in our survey had experienced at least one episode of working without registration since their arrival in Germany. Thus, there were almost as many participants who had some experience with unregistered work as those who stated to be in regular employment at the time of the survey or to have had a regular job in Germany already (together 41%). With respect to possible predictors for unregistered work experience, we find higher levels among refugees with approved asylum applications and those living without children. On the other hand, possessing a work permit and having spent at least ten years in school are associated with lower levels of unregistered work experience. Furthermore, we see that some of these correlations differ significantly between men and women, with female refugees being affected more strongly by certain risk factors than male ones.

With these results, our study contributes to the literature in three ways: First, we use an innovative experimental survey method to solve the difficult methodological challenge that arises when examining the sensitive topic of unregistered work among a vulnerable group like refugees.

As unregistered work is illegal and refugees are aware of this fact and the likely consequences, controlling for misreporting is especially important when examining this politically important group. Second, we provide the first empirical evidence of the share of refugees who experienced unregistered work in Germany based on individual micro-data. Third, our approach allows us to identify correlations between characteristics of respondents and their likelihood to experience unregistered work. These findings may thus provide a starting point for further analyses and considerations of policies to address this phenomenon.

The remainder of this paper is structured as follows. In Section 2, we discuss refugees' options on the German labor market in the context of institutional regulations followed by a short summary of what we know about their current state of employment. Section 3 provides information on the data collection and sample characteristics. In Section 4, we describe the list experiment, discuss the identifying assumptions and lay out the empirical strategy. Section 5 presents the results. Section 6 concludes, discusses possible limitations of the study, and points out possible avenues for further research on this topic.

2 Institutional context of the labor market integration of refugees in Germany

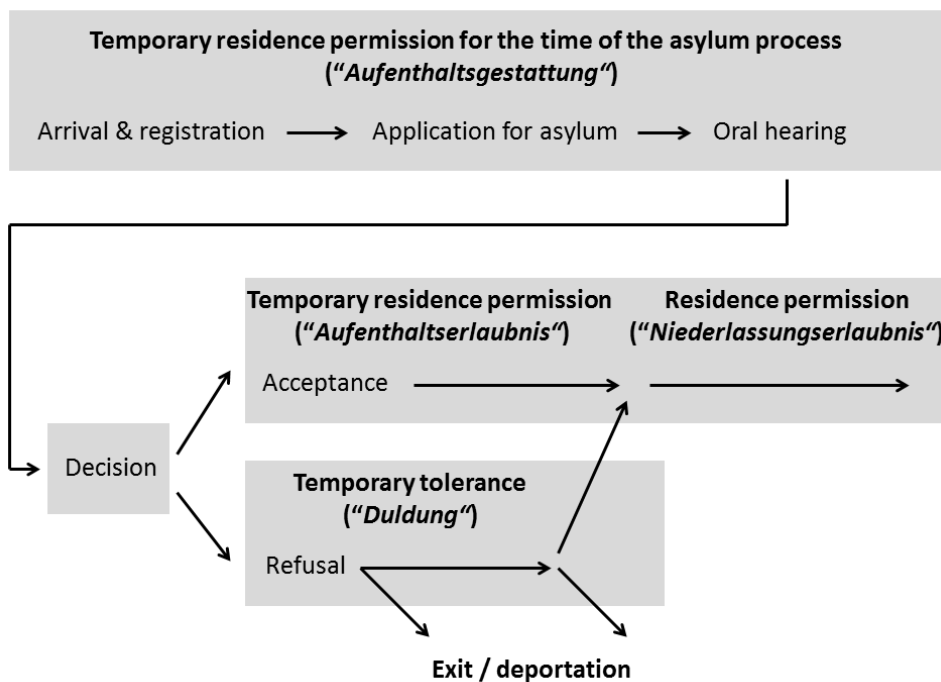
According to the economic theory of crime, individuals engage in illegal activities when their expected utility is greater than the benefits from available alternatives (Becker, 1968). In the case of unregistered work, the main alternatives are working on the regular labor market or staying out of the labor force, where the latter includes inactivity as well as preparing for a later entry by getting education or participating in some training course. In this section, we describe the institutional setting for refugees in Germany with respect to each of these options to gain an understanding of their situation and identify possible determinants for the empirical analysis.

2.1 Access to the labor market

The most important institutional aspect for the labor market integration of refugees is whether and when they are allowed to work in the host countries' regular labor market. In Germany, this depends on a combination of legal status and time since asylum application (see Figure 1).³

³ In this paragraph, we describe the legal framework for refugees in Germany enacted in 2014, which significantly reduced barriers to employment.

Figure 1: The asylum process in Germany



Note: This graphical illustration refers to information provided by BAMF (2019).

Refugees can apply for asylum directly after entering the country in order to start the formal process and obtain a residence permission for its duration (the so-called *Aufenthaltsgestattung*). During the first three months, asylum seekers are not allowed to take up employment. With the beginning of the fourth month, however, they can start to search for jobs and, with an employer's intention to hire them at hand, apply for work permission at the local foreigners' office for that specific position and employer. The officials examine whether the applicants have a right to work and if yes, forward the application to the Federal Employment Agency for an inspection of whether the conditions of the job meet common standards or are deemed exploitative.⁴ If the Federal Employment Agency does not object, the work permission is granted. In case the asylum decision is still pending after 48 months of staying in Germany⁵, the requirement to apply for a specific work permission falls away and the asylum seeker can commence any work but is not allowed to start a business.

After examination of the asylum case, the Federal Office for Migration and Refugees (*Bun-*

⁴ In some of the federal states, the Federal Employment Agency also examines whether there are equally suited German or EU nationals for the job and only approves the request if none are available.

⁵ According to Bundestag (2019), the average asylum decision in 2017 was reached after 13-14 months, but it can take much longer in complicated cases or if the asylum seeker appeals the decision in court.

desamt für Migration und Flüchtlinge, BAMF) can reach two conclusions. One is to recognize the need for protection in the form of granting asylum, refugee status, or subsidiary protection (*Asylberechtigung*, *Flüchtlingsschutz*, and *Subsidiärer Schutz*). In all three cases, the refugee obtains temporary residence permission and unrestricted labor market access for up to three years. If the conditions in the country of origin have not improved substantially and the state of integration is advanced, refugees in these legal states can apply for a permanent residence permission (*Niederlassungserlaubnis*) after three to five years. The requirements for application include among others: sufficient income to be independent of government benefits, certain proficiency in German, health insurance, accommodation, and a spotless criminal record.⁶

Alternatively, the BAMF rejects the application for asylum, leading to either the obligation to leave the country (*Ausreisepflicht*) or a temporary tolerance (*Duldung*) that lasts six months and can be renewed multiple times (Sachverständigenrat deutscher Stiftungen für Integration und Migration, 2019; BAMF, 2019). In the first case, individuals are not allowed to work since they are supposed to leave the country as soon as possible. If they are temporarily tolerated they continue in the same regime as during the asylum process, i.e., they need to apply for a work permission for each specific job and are not allowed to start a business or work as self-employed. Individuals who have lived in Germany with temporary tolerance for more than seven years can apply for a temporary residence permission and a permanent permission later, if they have acquired a certain state of integration, which mainly consists of earning their own living and possessing sufficient German proficiency and cultural knowledge.

Summing up, refugees in Germany gain access to the labor market relatively early,⁷ with the intention that formal obstacles should not hinder a quick labor market integration. The exact conditions for each legal status can be hard to understand, however, and actually starting a job requires a number of administrative steps that take some time and whose outcome may not always be transparent for the refugee.

⁶ More detailed and current information on the possible outcomes of the asylum process and the respective conditions for staying and working in Germany can be found on the website of the Federal Office for Migration and Refugees at www.bamf.de.

⁷ In France and the UK, the earliest possibility to work is after six and 12 months, respectively (AIDA, 2019b,a). In the US, refugees can apply for work permits directly after arrival, but the number of permits is capped (Congressional Research Service, 2018).

2.2 Welfare and benefit system

Given that almost all refugees could theoretically start working very quickly, the next question of interest is how the respective main alternatives to unregistered work actually look like. That is, how much asylum or welfare benefits do refugees get if they stay at home and what do they achieve by taking up regular employment? For the sake of simplicity, we use a strictly monetary and static perspective to shed light on these issues, that is, we do not include non-monetary or dynamic benefits of working or leisure time into consideration.

In general, the amount of benefits refugees receive in Germany depends on their legal status, the time passed since their asylum application, and the household size. Starting with the case of an adult refugee without children, the monthly benefits for an asylum seeker (*Asylbewerberleistungen*) is 354 Euros per month (385 US dollars in 2018 exchange rates) in the first 15 months after the asylum application and 416 Euros (452 US dollars) afterwards (GGUA, 2018).⁸ For a childless couple, the same rates for the respective time periods are 636 and 748 Euros, and for a family with two children under the age of six 1,064 and 1,228 Euros. In each case, the government additionally provides an accommodation (mostly rooms in residences for asylum seekers, but also private apartments) and covers expenses for necessary medical treatment. Furthermore, the government also organizes childcare and education for children and provides vouchers for school lunch and extra-curricular activities. Asylum seekers can additionally apply for extra funding for particular needs like new furniture (see, *Asylbewerberleistungsgesetz* in the version from October 24, 2015).

The benefits for asylum seekers can thus be considered as rather generous in international comparison (comparable numbers for a single refugee in, for instance, the UK or France in 2018 are 186 and 204 Euros per month, respectively, see AIDA, 2019b,a) and there is no immediate need to work or engage in illegal activities in order to have at least some basic standard of living. This could lead to a negative incentive structures with respect to employment also known as “welfare trap” (e.g., Blank, 2002; Blundell, 2001; Lemieux and Milligan, 2008). At the same time, the system provides incentives to pursue unregistered work, as it is difficult to increase the disposable income with regular employment due to large benefit reduction rates and low

⁸ The latter number is the same level of benefits as in the general social assistance program, the means-tested unemployment benefit II (*Arbeitslosengeld II*). The incentive effects discussed in this section therefore continue to be relevant after the decision on the asylum application as well.

exemptions. This is especially the case for refugees with low education or those who lack the necessary formal certificates to work in their profession.⁹ To illustrate this point, we calculate the number of hours an individual in each of the three household constellations discussed (single, childless couple, and family with two small children) would have to work at the national minimum wage in order to be completely independent of means-tested transfers but earn a net disposable income equivalent to the one he or she obtains in the form of benefits when being inactive.

The components of this calculation for refugees within the first 15 months after their arrival are reported in Table 1. Most importantly, refugees who start to work have to pay taxes and social security contributions¹⁰ as well as the rent for their accommodation (depending on the location at least 200 Euros per month and person, but often considerably more) and the child-care costs in the case of families. As displayed in column 1, a single adult would have to earn a monthly gross income of 681 Euros to break even.¹¹ At the 2018 level of the national minimum wage of 8.84 Euros per hour, this translates into a break even employment of 77 hours a month, which is equivalent to a part-time work of around 18 hours per week. Under the assumption that only one partner in a couple finds a job, the respective bread-earner would have to work 148 hours per month (34 hours per week) at the national minimum wage in the case of a childless couple and 269 hours for a couple with two young children (62 hours per week).¹² The respective calculation for these cases can be found in columns 2 and 3.¹³

The key take-away from these examples is that asylum seekers can live from the transfers they obtain, but face a sizeable challenge if they want to become independent of government assistance in Germany. Furthermore, the incentives to obtain a regular job get smaller over time, as the benefits for asylum seekers increase to the levels of the welfare system for the general population (the means-tested *Arbeitslosengeld II*) after the first 15 months in Germany or a

⁹ Even if a refugee possesses a formal education or employment certificate, it takes a lot of time, effort and costs to get it examined and recognized by the German authorities (e.g., Bauder, 2005; Frank et al., 2012)

¹⁰ Income taxation in Germany follows a progressive system once a tax-exempted minimum of roughly 8,000 Euros in earnings is passed. The marginal tax rates start at 14% and increase to 42% at a taxable income of about 56,000 Euro a year. Additionally, employees have to pay social security contributions at a flat rate of almost 20% of gross income capped at around 78,000 Euros in 2018.

¹¹ The calculations are made with the online gross-net calculator <https://www.rechner.pro/netto-brutto-rechner> using the parameters for 2018.

¹² Note that working 62 hours a week would violate worker protection legislation in Germany (*Arbeitszeitgesetz*), which only allows for a maximum of 48 hours per week on average.

¹³ For a family with two children, we include the value of the unconditional child benefit of 194 Euro per child and month in our calculation, as the family would receive it almost automatically and without any means-test. Under certain conditions and upon application, families could also receive housing benefits. As we are focusing on a situation without any means-tested government support, we do not take them into account here.

Table 1: Calculation of break-even hours of employment at the national minimum wage

	Single adult	Couple without children	Couple with two children under six
Monetary benefits	354	636	1064
Rent ¹	200	400	800
Child-related expenditures ²	-	-	200
Healthcare	Included	Included	Included
Irregular expenses ³	On demand	On demand	On demand
Equivalent net income	554	1036	2064
Equivalent gross income incl. tax and social security contributions ⁴	681	1304	2766
Child benefits ⁵	-	-	388
National minimum wage	8.84	8.84	8.84
Hours of work at the minimum wage needed to earn the same without benefits	77	148	269

Note: ¹The cost of accommodation depends on the district of residence and varies considerably. For the purpose of this calculation, we use the comparatively low value of 200 Euros/month per person. ²Child-related benefits may include the fee for childcare, subsidies for school lunch (in the case of older children), or the fees for leisure-time activities. They are paid on application and with a variable amount, depending on the individual circumstances. For the calculation, we use relatively modest 100 Euro per child and month. ³Asylum benefits also include one-time expenses like the initial equipment of an apartment with furniture and utilities. ⁴For the computation of the equivalent gross income, we used an online net-gross calculator with the relevant parameters from 2018. Source: www.rechner.pro/netto-brutto-rechner. ⁵Child benefits were 194 Euros/month for the first two children in 2018. They are part of the overall benefit package for welfare recipients, but paid out separately to all other parents.

positive asylum decision. Hence, there are incentives for refugees to engage in unregistered work to improve their financial situation, especially if they live in a partnership or larger family units.

There are two caveats of the presentation in this subsection. First, policy makers have learned from the years of discussion about incentive-compatible transfer systems and included some modest financial incentives in the formula to calculate the benefit level. 25% of each Euro earned are exempted from the deduction of benefits, up to an amount of 50% of the individual's overall asylum benefit level, i.e., 177 Euro per month for a single adult (GGUA, 2019). Second, we have not considered a number of non-monetary and forward-looking incentives for taking up regular employment in our discussion. Being regularly employed is important for most individuals, it increases the self-esteem and leads to higher social acceptance (see, e.g., Dooley and Prause, 1995; Herbst, 2013). Furthermore, being regularly employed is a requirement to obtain an apartment and, most importantly, a permanent residence permission. Thus, we expect that the participation in the official labor market leads to indirect non-monetary benefits that may unfold in the future.

2.3 Penalties for unregistered work

Apart from the existence of legal alternatives and financial incentives to work without registration, Becker’s framework features another important determinant for the expected net utility from illegal behavior: the probability to be detected and condemned, and the severity of the corresponding penalties (Becker, 1968). That is, the greater the likelihood that an individual gets caught in a criminal act and the harsher the resulting legal consequences, the lower his or her willingness to engage in it in the first place.

In Germany, the fight against illegal economic activity is spearheaded by the Financial Investigation Office for Clandestine Employment at the Federal Customs Administration. If detected, a number of different laws and directives lay out the sentences for unregistered work, with the most relevant ones for this paper stated in §8 *Schwarzarbeitsbekämpfungsgesetz* (working without permission and/or registration), §13 *Asylbewerberleistungsgesetz* (failure to report income as asylum seeker), §370 *Abgabenordnung* (tax evasion), and §263 *Strafgesetzbuch* (benefit fraud). The size of the penalty varies by case, but can reach several thousand Euros and five years in jail. Apart from direct punishment, individuals with a monetary fine of more than 90 times the net daily income or a sentence of more than three months in jail get an entry in their official criminal record. For refugees, this precludes the application for a permanent residence permission (see section 2.1), as a spotless criminal record is a prerequisite. In the case of families, this would not only affect the wrongdoers themselves, but also their partners and children.

While the consequences can be significant, the rate of detection and punishment has been very low since years, mainly due to under-staffing at the Financial Investigation Office for Clandestine Employment (see various reports in German news outlets, e.g., *Deutsche Handwerkszeitung*, 2018; *Tagesschau*, 2020). Thus, there might be a lot of opportunities to work without registration in sectors that are difficult to monitor, e.g., construction, gastronomy, and household services.

Summing up, there are competing factors influencing the probability to work unofficially among refugees in Germany. While asylum seekers are allowed to start looking for a job very quickly, the process to actually start an employment involves interaction with several government agencies and can be tedious, especially if the individual is still in the asylum process or only temporarily tolerated in the country. Similarly, the financial support for refugees is high enough to guarantee a decent standard of living, yet the associated large benefit reduction rates build a

significant entry barrier to regular jobs and an incentive to accept unregistered work. And finally, although sentences for convicted illegal workers may be severe, the probability of detection is very low in several key sectors. Given these circumstances, we expect to measure a significant amount of unregistered work among refugees in Germany.

2.4 The official employment situation of refugees in Germany in 2018

Given the uncertainties and hurdles created by the institutional environment and the fact that most refugees entered the country without any knowledge of German, it is not surprising that the labor market integration was moderate at the time when the survey was conducted in summer 2018, even though the economy had been steadily expanding and employers in many sectors reported problems to fill their job openings. More specifically, the nationally representative IAB-BAMF-SOEP refugee survey indicates that 21% of the adult refugees who arrived between 2013 and 2016 had found a job by the second half of 2017, including apprenticeships and irregular mini-jobs (Brücker et al., 2019). More recent statistics of the Federal Employment Agency from April 2019 show that this percentage has grown to 33% of all working-age individuals coming from the eight countries with the highest numbers of asylum seekers. This indicates progress over time, although the numbers are not perfectly comparable since the latter includes all individuals from these countries independent of when they immigrated and for which reason. Compared to the employment rate of 58% among EU nationals and 59% for people from the Balkans, there is still a long way to go until refugees are successfully integrated into the workforce (Brücker et al., 2019). The challenge is even greater for female refugees, who are considerably less likely to be employed than their male counterparts (6% to 27% in the second half of 2017, see Brücker et al., 2019).

In terms of the quality of positions, about half of them work in low-paid assistant jobs. The average gross earnings of working refugees are comparatively low (about 1,000 Euro in 2017). A refugee with a full-time job earns only 55% of the medium wage of a full-time employee in Germany (Brücker et al., 2019). Additionally, the employment of refugees is concentrated in precarious positions in temporary work, cleaning, gastronomy, and agriculture. Combined with the finding that less than a quarter of employed refugees have permanent contracts (Hartmann et al., 2018), this indicates that their labor market integration rests on weak foundations and is very vulnerable to any deterioration of macroeconomic conditions.

3 Data collection and sample statistics

3.1 The ifm Refugee Survey 2018

This study is based on data collected in the ifm Refugee Survey, a cross-sectional survey of 1,279 refugees living in publicly provided, mostly centralized residences in the state of Baden-Württemberg, the third largest of Germany’s federal states with over 10 million inhabitants. It was conducted between mid-April and mid-July 2018 with the aim to obtain a comprehensive overview on the labor market integration of refugees at that point in time. The data collection was financially supported by the state government and assisted by local authorities which shared aggregate information about the residences in their respective districts and the composition of individuals living there. For logistical reasons, we used a clustered sampling approach and selected two to three districts from each of the states’ four main regions. In each district, the interviews took place in almost all of the medium- to large-sized residences (20 to 200 inhabitants).¹⁴ Using this approach and given the fact that refugees are almost randomly distributed to the different states and districts in Germany according to the national distribution quota “Königsteiner Schlüssel” (which is based on tax income and population levels), the resulting sample should be relatively similar to the national population of refugees living in publicly provided residences.

Each visit to a residence took place in the afternoon and early evening (usually between 3 and 7 pm) to ensure that all inhabitants had a chance to participate in the voluntary interviews, even if they worked or went to language or integration courses. The schedules were arranged in accordance with the responsible local social workers or residence managers and advertised to the inhabitants in advance by means of multi-language posters hung up in community areas. Most of the interviewers came from Middle Eastern or African countries themselves to facilitate the contact. They worked in mixed-gender teams of three to five persons depending on the size of the targeted residence. The interviewers actively recruited participants by approaching individuals in public spaces and community rooms or knocking at doors and explaining the study objectives. Although we did not offer monetary incentives or in-kind gifts for participation, the interviewers estimated that half of the contacted people agreed to answer the questions.

¹⁴ The main exceptions were residences in which large changes in inhabitants had recently taken place or were about to happen as well as few residences with predominantly African-origin individuals in the immediate aftermath of highly publicized quarrels between refugees from this ethnic group and the police during that time.

The questionnaire was available in the languages of the main countries of origin of asylum seekers in Germany at the time when the survey took place: English, Arabic, and Persian.¹⁵ Combined, about 70-80% of refugees come from countries in which one of these three languages is an official language. The questions asked a whole range of items, starting with demographic information and the migration and asylum process, as well as education and professional experience in the home country and self-assessed skills and preferences. The core was a large section about the current state of labor market integration, job search activities and limitations, followed by questions on German language proficiency and the willingness to invest in education. The interviews were conducted as computer-assisted personal interviews (CAPI) and lasted between 25 and 60 minutes.

3.2 Sample statistics

Out of the 1,279 interviews, 1,259 respondents completed the questionnaire. In Table 2, we report the mean values and standard deviations of individual characteristics for all respondents in the sample (columns 1 and 2, respectively). Women represent roughly a quarter of the observations and the average age of respondents is 31 years. 44% are married or live in a partnership, the average number of children is 1.2 (about 2 for those with at least one child), and participants went to school for nine and a half years on average. The main countries of origin are Syria (23%), Afghanistan (16%), Iraq (14%), Gambia (13%), Nigeria (10%), and Iran (6%). At the time of the interviews, the refugees had spent two years and four months in Germany on average. One fifth still waited for the outcome of their asylum application, whereas 42% had been assigned some protection status and the remaining 37% were rejected (most of them living in Germany with a temporary suspension of deportation, which needs to be renewed every six months). With respect to their housing situation, hardly any respondents lived in private apartments, which is not surprising given that we targeted refugee residences for interview recruitment. Finally, more than a quarter of the participants reported to be engaged in some kind of work activity which includes full- and part-time employment, mini jobs, publicly-sponsored 1-Euro jobs, apprenticeships, internships, and participation in a labor market related training course.

In order to assess how this sample of refugees compares to the population of recent refugees in Germany, we also report the numbers for the respective characteristics from the 2018 wave of

¹⁵ The different language versions of the questionnaire are available upon request.

Table 2: Sample characteristics and representativeness

	ifm Refugee sample		IAB-BAMF-SOEP		IAB-BAMF-SOEP	
	Baden-Württemberg		Baden-Württemberg		Germany	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Personal characteristics						
Female	0.242	(0.429)	0.250	(0.433)	0.298	(0.457)
Age in years	31.3	(9.6)	31.0	(9.3)	31.8	(10.7)
Married/Partnership	0.436	(0.496)	0.415	(0.493)	0.475	(0.499)
No. of children	1.162	(1.559)	1.220	(1.796)	1.278	(1.848)
Years of schooling	9.6	(3.2)	9.6	(2.8)	9.9	(3.1)
Country of origin						
Syria	0.225	(0.418)	0.407	(0.492)	0.426	(0.495)
Afghanistan	0.163	(0.369)	0.151	(0.358)	0.151	(0.358)
Iraq	0.138	(0.345)	0.096	(0.295)	0.102	(0.303)
Gambia	0.129	(0.335)	0.064	(0.245)	0.010	(0.100)
Nigeria	0.104	(0.305)	0.015	(0.121)	0.019	(0.136)
Iran	0.060	(0.238)	0.009	(0.097)	0.026	(0.160)
Other African countries	0.115	(0.319)	0.102	(0.302)	0.132	(0.339)
Other countries	0.066	(0.248)	0.156	(0.364)	0.134	(0.340)
Situation in Germany						
Years since arrival	2.3	(0.9)	3.0	(0.8)	3.0	(0.8)
Asylum application						
Pending	0.206	(0.404)	0.252	(0.434)	0.156	(0.363)
Approved	0.422	(0.494)	0.675	(0.469)	0.750	(0.433)
Rejected	0.373	(0.484)	0.073	(0.260)	0.093	(0.291)
Private apartment	0.031	(0.174)	0.625	(0.485)	0.743	(0.437)
Some work in last 7 days	0.260	(0.439)	0.460	(0.499)	0.354	(0.478)
Number of observations	1,259		457		4,184	
Weighted			71,636		669,183	

Notes: In both samples, we only include adult individuals without German citizenship (to exclude German partners in the same household) who have arrived in Germany after 2012. The number of children refers to children under 18 in the case of the ifm Refugee sample BW and to all given births for the IAB-BAMF-SOEP sample. Population weights are applied for the IAB-BAMF-SOEP (2018) sample.

the IAB-BAMF-SOEP refugee sample in Table 2. This add-on to the German Socio-Economic Panel started in 2016 and targets households of refugees who entered Germany between 2013 and 2016, using administrative records about foreigners in Germany (the *Ausländerzentralregister*) as sampling frame.¹⁶ In columns 3-4, we show the weighted averages and standard deviations for those individuals in the IAB-BAMF-SOEP refugee sample who lived in the state of Baden-Württemberg in 2018 as the most relevant and direct reference group to our sample. In columns 5-6, we additionally report the respective numbers for the whole of Germany.

¹⁶ More information about the IAB-BAMF-SOEP refugee sample and how it can be accessed can be found at www.diw.de.

The ifm refugee sample is very similar to the state and federal numbers in terms of gender and age composition, the fraction of individuals in a partnership, and the average number of children. It also resembles the average years of education and the percentage of people who are still waiting for their asylum decision. There are some notable differences between our sample and the IAB-BAMF-SOEP, however, most of which reflect our approach of recruiting study participants in refugee residences. In particular, the respondents in our sample had spent less time in Germany already, lived under less stable legal conditions (about 37% vs. less than 10% of participants had received rejections to their asylum application and lived mostly under temporary suspension of deportation status), and were less advanced in terms of their labor market integration (26% reported some kind of work-related activity vs. 46% in BW and 35% in the German-wide sample). In line with these numbers, the ifm Refugee sample contains less Syrians, but more individuals from Iraq, Gambia, Nigeria, and Iran compared to the state of Baden-Württemberg or Germany. This seems reasonable as the large majority of Syrian refugees entered Germany in 2015, i.e., three years before the interviews, and almost automatically obtained refugee protection status, which means that they were less likely to still live in central residences. In contrast, individuals from Iraq and Iran had lower admission rates, and those from Gambia and Nigeria are hardly accepted at all (see BAMF, 2020). Obtaining private housing is therefore very difficult for these groups.

Our sample is not representative for the whole population of recent refugees in Germany and has a tilt towards more difficult cases. Conducting a subgroup analysis will therefore be important to gauge the direction to which the aggregate findings for the incidence of unregistered work may be biased. If we find more experience with unregistered work among refugees with lower education and less stable legal status, for instance, then the results should be interpreted as an upper bound for the corresponding numbers in the general population of refugees.

4 Methodology and estimation

Obtaining reliable evidence about unregistered work is a difficult task due to its illegal nature. Administrative data contain only the number of breaches detected by customs authorities and therefore depict a lower bound of the prevalence of unregistered work (Enste, 2017). Survey data likely suffer from problems of non-response and underreporting because participants may hesitate to reveal information about such a sensitive topic. We overcome this challenge by

employing a list experiment on unregistered work. In this section, we describe the idea and setup of the list experiment, discuss the identifying assumptions that are necessary to obtain the estimate of interest, and present the empirical strategy to analyze respondents' answers.

4.1 List experiment

The survey participants faced the list experiment about two thirds through the questionnaire, at the end of the section about their labor market integration in Germany. The sample was randomly split in two groups. The control group was confronted with a list of five non-sensitive items, in our case, non-sensitive experiences on the German labor market they may have made already, and were asked to answer the following question:

If you think about your experiences on the German labor market so far: How many of the following situations have you experienced? (Provide a number between 0 and 5)

- (1) I have/had difficulties to get my academic degree recognized.
- (2) I have/had difficulties to get my professional education recognized.
- (3) It is easy to find a job here.
- (4) The local employment agency is very helpful in the job search.
- (5) I work/have worked in a job which corresponds exactly to my education.

The treatment group faced the same list of non-sensitive items plus the sensitive item of interest, here, the experience of unregistered work. They were asked the same question, but should provide a number between 0 and 6.

- (6) I work/have worked in a job in which I was not officially registered

In this setup, the participants did not have to provide an answer to any individual item, they only had to indicate the number of experiences already made. Thus, not even the interviewers could infer which of the items were already experienced by the respondent. This provides a high degree of anonymity (see Droitcour et al., 1991; Wolter and Laier, 2014), which is very important when asking sensitive questions in general, but may be absolutely crucial for the sample of refugees who face the risk of losing their work or residence permission in case of illegal behavior. To strengthen this aspect even more, we instructed the interviewers to only read out and explain the task of this question, but then hand the tablets to the respondents which

then typed in the corresponding number themselves. This added an extra layer of individual confidentiality and protection, as the interviewers did not even see the number of experiences.

4.2 Identifying assumptions

To discuss the identifying assumptions for the parameters of interest in the list experiment and whether they are fulfilled in our case, we use the notation of Imai (2011) and Blair and Imai (2012). We consider T as the treatment indicator, with $t = 0$ for the control group and $t = 1$ for the treatment group. We indicate the number of control items as J . Individuals in the control group face a list of five items $J = 5$ with $j = 1, \dots, 5$ whereas the members of the treatment group see the same list plus the sensitive item, $j = J + 1$. Furthermore, we denote the answer of respondent i to each item j as binary indicator $Z_{ij}(t)$ for $j = 1, \dots, 5$ and $t = 0, 1$. For example, $Z_{i3}(1) = 1$ means that respondent i experienced that it is easy to find a job in Germany given that he or she was part of the treatment group. $Z_{i3}(0) = 1$ is the counterfactual outcome for respondent i in the case of assignment to the control group. As the control group did not encounter the sensitive item, the experience of unregistered work, $Z_{i,J+1}(t)$ is only observable for the treatment group $Z_{i6}(1)$ but not for the control group $Z_{i6}(0)$. Finally, we denote the sum of items that were experienced by the respondents as $Y_i(0) = \sum_{j=1}^J Z_{ij}(0)$ in the control group and $Y_i(1) = \sum_{j=1}^{J+1} Z_{ij}(1)$ in the treatment group.

Then, we can identify the share of respondents for whom the sensitive item is true (in our case, the share of refugees who experienced unregistered work in Germany) under three assumptions (see Imai, 2011; Blair and Imai, 2012):

Assumption 1 (*Randomized treatment*). Respondents $i = 1, \dots, N$ are randomly allocated to either treatment or control group.

If this assumption holds, we can treat the two groups as reasonable counterfactuals to each other. Otherwise, differences in the observed answers may not be caused by the sensitive item alone, but also by differences in observable or unobservable characteristics. In our case, this assumption is satisfied by the experimental design, as we had full control over the assignment procedure and non-compliance was technically impossible. In Table A.1 in Appendix A, we show

that this led to very similar comparison groups in almost all observable dimensions.¹⁷ We can therefore plausibly assume that the same holds for the unobservable characteristics as well.

Assumption 2 (*No design effect*). Including the sensitive item does not change the answer behavior of respondents to the control items, that is: $\sum_{j=1}^J Z_{ij}(0) = \sum_{j=1}^J Z_{ij}(1)$.

If respondents changed their answers to the control items due to the presence of the sensitive item, any difference in the stated total count of experiences between the two groups may be caused either by the actual experience of unregistered work or by individuals in the treatment group adjusting their answer to the control items (or any combination of the two effects). In such a scenario, we would not be able to disentangle the importance of each component and it would be impossible to correctly identify the magnitude of experience with unregistered work. To test whether this assumption holds here, we apply the test developed by Blair and Imai (2012) to detect violations of the no design effect assumption. Appendix B.2 provides more information about this procedure and the results. We do not find evidence for a design effect in our data.

Assumption 3 (*No liars*). The answer to the sensitive item represents a truthful response, that is, $Z_{i,J+1}(1) = Z_{i,J+1}^*$.

If individuals in the treatment group shy away from indicating their experience with unregistered work even in a set-up as anonymous and protective as a list experiment, the results will be downward biased and represent a lower bound estimate for the phenomenon of interest. Assumption 3 may be violated, for instance, if respondents would have to indicate that they experienced all items when answering honestly (i.e., the total count would be $J + 1$) and thus choose to state only J to avoid an exact statement on the sensitive item (*ceiling effect*). A similar reaction could happen if respondents have not experienced any of the control items and believe that indicating a ‘1’ could reveal too much about their work behavior (*floor effect*).¹⁸

¹⁷ This includes several measures for personal and family characteristics, education and skill levels, labor market status, migration history, and the regional distribution of the interviews. Together, we only find a statistically significant difference between the two groups on the 5% level in two out of 36 comparisons (5.6%), which is exactly what we would expect from the design of the statistical test. Furthermore, the test for joint significance yielded a p-value of 0.331, confirming again that the assignment procedure was not systematic in any way.

¹⁸ To reduce this risk of ceiling or floor effects, we followed the recommendations in the literature and chose control items that show thematic coherence by restricting all items to experiences on the German labor market

In the present study, the occurrence of ceiling effects is unlikely, because most refugees had limited labor market experience in Germany. For the same reason, floor effects are an issue. When we apply the statistical tools to detect possible liars provided by Blair and Imai (2012), the result indicates to floor effects in the data. More specifically, the conditional probability that we observe individuals underreporting their experience with unregistered work amounts to 18%. This large share serves shows how carefully refugees treat the topic of unregistered work, even in a setup that provides such a high degree of anonymity and security as the list experiment. It also complicates the analysis, as we need to control for this bias if we want to avoid a serious underestimation of the experience with unregistered work among the interviewed refugees.

To this end, we apply the correction procedure for floor effects provided by Blair and Imai (2012) throughout the empirical analysis. They impose an additional assumption that allows to directly model floor effects. This assumption can be formalized as

Assumption 4 (*Conditional independence*)

$$Pr(Y_i(0) = y | Z_{i,J+1}^* = 1, X_i = x) = Pr(Y_i(0) = y | Z_{i,J+1}^* = 0, X_i = x).$$

It implies that respondents answers to the control items are independent of their truthful answer to their sensitive item given pretreatment covariates X_i . This is similar to a conditional independence assumption in observational studies. We follow the recommendation of Blair and Imai (2012) and condition on pretreatment covariates that have high predictive power in explaining respondents answer behavior to the control item. Since the control items in our application largely focus on experiences during job search, we control for gender, indicators of vocational degree, and an indicator for current or already successful job search when we implement the correction procedure. We argue that given these covariates assumption A4 is likely to hold. Then, the proportion of respondents for which the sensitive item is true can be identified from the data even if floor effects exist. Further details of the testing and correction procedure are presented in Appendix B.

(Droitcour et al., 1991). This means that asking about experience with unregistered work does not come out of the blue, but reasonably fits to the overall topic. Furthermore, none of our items should create a very strong resonance compared to the control items which would lead to a bias called contrast effects (Glynn, 2013; Kuklinski et al., 1997). Additionally, we tried to avoid many low-variance items, that is, experiences which occur very frequently or hardly ever (Glynn, 2013). To this end, we selected items that are likely negatively correlated, e.g., items (2) and (5). As the descriptive statistic in Table 3 shows, we seem to have failed on that front, however.

4.3 Estimation strategy

Under assumptions (1) to (3), the proportion of refugees who experienced unregistered work is identified as $Z_{i,J+1}(1) = Y_i(1) - Y_i(0)$, and can be easily estimated using a difference-in-means (DiM) estimator of the form

$$\hat{\tau}_{DiM} = \frac{1}{N_1} \sum_{i=1}^N T_i Y_i - \frac{1}{N_0} \sum_{i=1}^N (1 - T_i) Y_i.$$

Here, N_1 represents the number of observations in the treatment group and N_0 those in the control group. $\hat{\tau}_{DiM}$ provides an unbiased estimate of the parameter of interest, $E(\hat{\tau}_{DiM}) = Pr(Z_{i,J+1}(1) = 1)$. In the empirical analysis, we first start by running a linear regression (OLS) without covariates to provide a benchmark estimate that is numerically equivalent to the share of refugees with unregistered work experience obtained by DiM. Second, we apply the Maximum-Likelihood estimation (MLE) procedure that uses the information from the joint distribution of answers to improve statistical efficiency. Details on the identification of the joint are presented in Appendix B.1. Third, as we find evidence for the violation of Assumption 3, we apply the Blair and Imai (2012) correction for floor effects in the empirical analysis. Details of this procedure are outlined in Appendix B.4. In short, this procedure adjusts the likelihood function for the probability of incorrect reporting and estimates the resulting model with MLE. All results are obtained in R by using the codes implemented in the programming package ‘list’ by (Blair et al., 2014). The reported standard errors in each case are robust to the presence of heterogeneity.

5 Prevalence of unregistered work among refugees

In this section, we start the presentation of results with a descriptive summary of the responses to the list experiment, before we discuss the estimates of the share of refugees who experienced unregistered work in Germany. In particular, we consider the impact of floor effects and present adjusted estimates. Then, we run a multivariate analysis with additional covariates and investigate correlations between respondents’ characteristics and their experience with unregistered work to identify predictive risk factors. Finally, we show some complementary descriptive results on the possible motives behind unregistered work. To this end, we present the answers to direct questions about why employers and refugees may engage in this activity in general.

5.1 Experience with unregistered work

In Table 3, we present the responses to the list experiment by showing the non-response rates as well as absolute and relative frequencies of the total counts for each experimental group. The first thing to note is the low percentage of non-responses, with 6% and 5% in the control and treatment group, respectively. This indicates that the way the question was asked and administered did not lead to a significant amount of respondents unable or unwilling to answer. Next, we observe similar distributions of answers in both groups, each strongly skewed to the right. More specifically, almost 60% of respondents reported count ‘0’ as their answer. This is a first indicator for the existence of floor effects in the data. Finally, the mean number of reported items amounts to 0.547 in the control group and to 0.669 in the treatment group.

Table 3: Summary of responses to the list experiment

	N	% non-response	Item count								Mean
			0	1	2	3	4	5	6		
Control group	637	6.44	n	395	108	71	15	4	3	-	0.547
			%	62.01	16.95	11.15	2.35	0.63	0.47	-	
Treatment group	622	5.31	n	366	125	52	30	8	5	3	0.669
			%	58.84	20.10	8.36	4.82	1.29	0.80	0.48	

With these numbers, the simple difference in means yields a share of 12% of refugees who had experienced unregistered work in Germany already. Table 4 depicts this result and the corresponding standard error estimated by OLS in column 1. As the estimate is statistically significant, it provides first empirical evidence that unregistered work is a relevant experience for a sizeable portion of refugees. In comparison, the list experiment of Kirchner et al. (2013) found only 6.4% of the German population working without registration throughout the year 2010. In column 2 of Table 4, we apply the binomial logistic regression estimated with MLE as proposed by Imai (2011). The outcome is very similar (a share of 11% of respondents with experience with unregistered work) and the smaller standard errors show that using MLE leads to a higher precision in the estimates as it takes the whole joint distribution of counts into consideration rather than only the means.

In the presence of floor effects, these OLS and MLE estimates represent the lower bound of the share of refugees who experienced unregistered work. Column 3 therefore shows the estimated share when we account for floor effects by using the Blair and Imai (2012) correction

Table 4: Results of the list experiment

	(1)		(2)		(3)	
	OLS		MLE		MLE	
Share experienced unregistered work	0.122	(0.058)***	0.114	(0.024)***	0.331	(0.010)***
Num. of observations	1185		1185		1185	
Adjust for floor effects	no		no		yes	

Note: The dependent variable is the response to the list experiment question. It could be 0, 1, 2, 3, 4, or 5 for respondents in the control group. It could be 0, 1, 2, 3, 4, 5, or 6 for respondents assigned to the treatment group. Column (1) present the coefficient of the treatment indicator from linear regression (OLS) which is equivalent to difference-in-means (DiM). In column (2) and (3), we implement the binary logistic regression estimated by Maximum-Likelihood estimation (MLE). The original coefficient is reported in log-odds and transformed into a probability using $p(x) = 1/(1 + e^{-x})$. In column (3), we additionally adjust for floor effects. We do not use control variables in these estimations. Heteroskedasticity-consistent robust standard errors are presented in parentheses.

approach. In this scenario, the share of respondents with unregistered work experience increases remarkably to 33%, suggesting that every third refugee had already worked without registration since having arrived in Germany. Compared to the 41% of respondents who worked at the time of the interview or indicated that they have had a job at some point in Germany already, this suggests that the experience with unregistered work is almost as common among refugees as the experience with registered employment.

Although the estimate appears large at first glance, it seems plausible if we recall the information provided in Section 2. In particular, the complexity of whether a refugee is allowed to work at the beginning of the asylum process and in which specific jobs may be difficult to understand and drive people away from seeking regular jobs during that period. Likewise, the benefit system for refugees provides little financial incentive to start registered employment, especially for individuals with partners and children. Last but not least, the sectors offering the kind of low skill jobs that are the easiest to obtain for refugees at the beginning are the same as those typically associated with unregistered work, like household services, construction, and gastronomy. Coupled with the facts that more difficult integration cases are likely overrepresented in our sample and that we asked about the overall cumulative experience with unregistered work throughout the time in Germany so far instead of in any given year, finding 33% of refugees having experienced unregistered work does not seem surprising.

5.2 Potential determinants of unregistered work among refugees

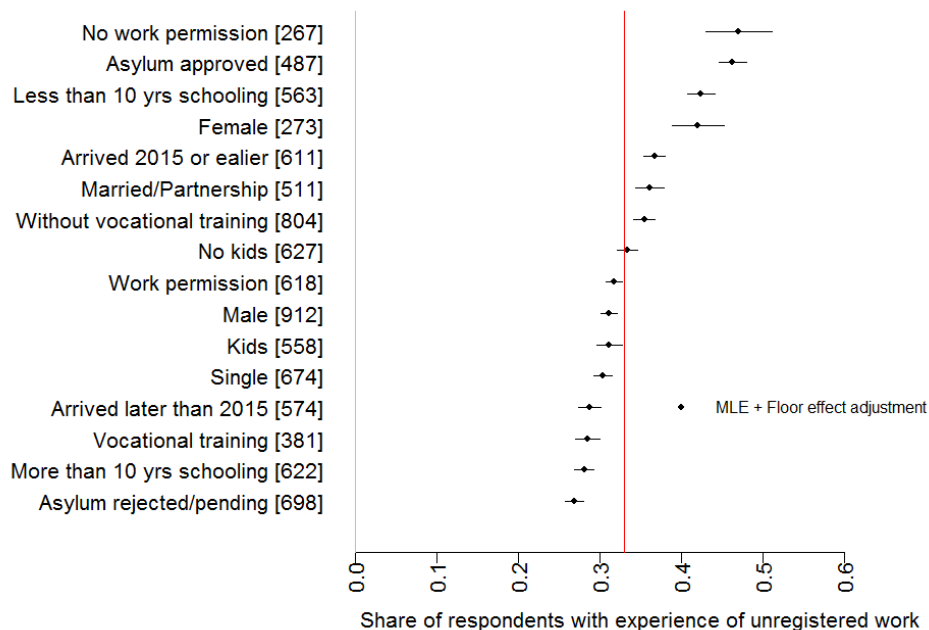
Given the large share of refugees who experienced unregistered work in Germany already, it is important to find out more about the driving forces behind this phenomenon, such that targeted and efficient measures to prevent its widespread occurrence can be developed and implemented. Unfortunately, the list experiment per se is not designed to measure causal relationships, so we cannot test whether a more secure legal status, for instance, leads to less experience with unregistered work in Germany. What we can do, however, is to look into unregistered work experience in certain subgroups, thereby providing important information that may hint at possible directions for public action and future research. This differentiates the list experiment from other methods to derive estimates for unregistered work from national accounting or indirect survey questions, where such subgroup analysis is not possible.

5.2.1 Predicted shares by subgroup

To start, we use the estimated coefficients from the model with adjustments for floor effects to predict the share of experience with unregistered work for several subgroups, defined by non-exclusive personal characteristics. Figure 2 presents the results, which display a wide range from 27% for refugees whose asylum application was rejected or still pending at the time of the interview up to 47% for respondents who stated that they did not have a work permission. Other groups with significantly higher than average shares of experience with unregistered work include individuals with approved asylum, less than ten years of schooling, females, people living in a partnership or marriage, and those without vocational training. On the other hand, being single, possessing a vocational training degree, and having spent at least 10 years in school is associated with significantly lower than average experience.

These numbers show that there is large variation in the exposure to unregistered work between subgroups. They hint towards potential risk factors, in particular, whether the respondent reports to have a work permission, the education level, marital status, and gender. Finally, the results provide a plausibility check for our interpretation of the share of unregistered work experience as cumulative measure since arrival in Germany. When we look at the estimated shares of people who immigrated in 2015 or earlier and those who arrived later, we find significantly higher shares for those participants with more time spent in Germany at the time of the interview. This

Figure 2: Share of respondents with unregistered work experience by subgroups



Note: The diamonds show the shares of respondents with unregistered work experience estimated by Maximum-Likelihood estimation (MLE) with adjustment for floor effects. The solid lines represent the 95% confidence intervals. The vertical line indicates the share of unregistered work experience in the overall sample (33%, see Table 4). The number of observations in the different subgroups is shown in the square brackets.

gives us confidence that the applied method leads to internally consistent estimates.

With respect to the external validity of our findings, Figure 2 suggests that the results obtained from our non-random sample may be a good approximation for the overall group of refugees in Germany, as the deviations seem to cancel each other out. On the one hand, individuals in the sample have spent less time in Germany and report a lower percentage of approved asylum cases than the population of refugees, which suggests lower rates of experience with unregistered work. On the other hand, our respondents are more likely to lack work permission and spent fewer years in school, indicating a higher exposure to unregistered work. Thus, the findings in this study provide a good starting point for decision-making and future analyses of this topic.

5.2.2 Multivariate analysis

While Figure 2 reveals important subgroup differences in the experience of unregistered work, it is very likely that some of these characteristics are correlated with each other. For instance, the probability to obtain a positive asylum decision is higher for refugees who arrived earlier in

Germany (45% vs. 36%), or male refugees are more often singles than women (62% vs. 38%). In the next step, we therefore implement a more systematic multivariate regression analysis in order to account for possible correlations between the individual factors and identify respondent characteristics that are more strongly associated with unregistered work than others. Although the estimated models do not identify causal relationships, the results of this exercise provide a first indication of which characteristics might be particularly predictive and could therefore be relevant from a policy perspective and for more in-depth research. Given our discussion of the legal framework in Section 2, we are particularly interested in the factors determining the labor market access (asylum approved and self-reported work permission) and the level of benefits (being in a partnership and presence of children). In addition, we also look at the time of arrival, current labor market activity (defined as working, searching for a job, or participating in training), and education-related information (years of schooling and possession of a vocational degree), since these are important characteristics for entering the regular labor market.

Following the presentation of results in the previous section, we implement both a linear regression and two binomial logistic regression models estimated by MLE with and without floor effect adjustment. We estimate the following model

$$Y_i = f(X_i, \gamma) + T_i g(X_i, \delta) + \epsilon_i$$

with $E(\epsilon_i | X_i, T_i) = 0$ and (γ, δ) is a vector of two unknown parameters. $f(x, \gamma) = E(Y_i(0) | X_i = x)$ is the regression model for the conditional expectations of the control items given a set of covariates X_i . $g(x, \delta) = Pr(Z_{i,J+1}(1) = 1 | X_i = x)$ is the regression model for the conditional expectations of the sensitive item given X_i . We aim to estimate $g(x, \delta)$ in order to determine the association between respondents characteristics and their answer to the sensitive item. We report the results for $g(x, \delta)$ in Table 5. For the sake of conciseness, we do not report the estimated coefficients for $f(x, \gamma)$ since these baseline effects would only reveal the correlations between characteristics and the count of experiences with the non-sensitive items in the control group.¹⁹ OLS coefficients are reported in percentage points and the MLE results in log-odds.

Looking at Table 5, we first note that the estimated coefficients almost always point to the same direction across the different models, which means that conclusions about the sign of the

¹⁹ The complete results are available from the authors upon request.

Table 5: Multivariate analysis of the list experiment

	(1)		(2)		(3)	
	OLS		MLE		MLE	
Treatment	-0.063	(0.208)	-5.114	(1.373)***	-0.692	(1.104)
Arrived later than 2015 \times T	-0.177	(0.113)	-0.869	(0.612)	-0.633	(0.536)
Asylum approved \times T	0.029	(0.123)	0.112	(0.584)	0.781	(0.465)*
Work permission \times T	-0.241	(0.116)**	-1.011	(0.681)	-1.107	(0.727)
Female \times T	0.066	(0.124)	0.090	(0.812)	0.770	(0.622)
Married/Partnership \times T	0.229	(0.149)	1.250	(0.841)	0.959	(0.719)
No kids \times T	0.331	(0.152)**	2.083	(0.833)**	0.984	(0.782)
Labor market activity \times T	0.074	(0.121)	0.901	(0.763)	0.638	(0.627)
Vocational degree \times T	-0.027	(0.132)	0.112	(0.643)	-0.516	(0.489)
≥ 10 yrs schooling \times T	-0.005	(0.118)	1.012	(0.625)	-1.004	(0.449)**
Num. of observations	1185		1185		1185	
Regional controls	yes		yes		yes	
Adjustment floor effects	no		no		yes	

Note: The dependent variable is the response to the list experiment. It includes the natural numbers between 0 and 5 for respondents in the control group, and between 0 and 6 for individuals assigned to the treatment group. Column 1 reports marginal effects obtained by linear regression (OLS), columns 2 and 3 changes in log-odds derived from Maximum-Likelihood estimation (MLE). The adjustment for floor effects is incorporated in column 3. The values in parentheses depict the corresponding robust standard errors. Regional controls are dummies for the districts of the residence that were not balanced in Table A.1.

examined relationships do not depend on the chosen empirical method. This is not the case for statistical significance, however. While we observe several statistically significant factors, they are typically not the same in the different models. As the aim of this exercise is not to work out causal relationships, but to go one step further in our exploratory analysis of potentially relevant predictors, we will treat every variable with at least one occurrence of a statistically significant correlation in Table 5 as candidate worthy of further inspection.

With this in mind, we see that most of the considered variables do not pass this threshold. This includes arrival time, gender, being in a partnership, current labor market activity, and possessing a vocational degree. We observe at least one statistically significant estimate for the following four characteristics: approved asylum status, self-reported work permission, being childless, and being more educated. These variables still seem to possess some predictive power after we control for correlations with the other potentially relevant characteristics. More specifically, having an approved asylum status and being childless are associated with higher unregistered work experience, whereas stating to possess a work permission and having spent at least 10 years in school correlate with individuals who had less exposure to this kind of jobs. Interestingly, the positive result for approved asylum status and the negative one for self-reported

work permission seem to contradict each other, as a positive decision on the asylum request automatically triggers the right to work in Germany. Also, the strong positive correlation measured for being childless directly challenges the notion that higher benefit levels for refugee families lead to more unregistered work. To gain more insights into the driving forces of these results, we therefore look more closely at the respective subgroups.

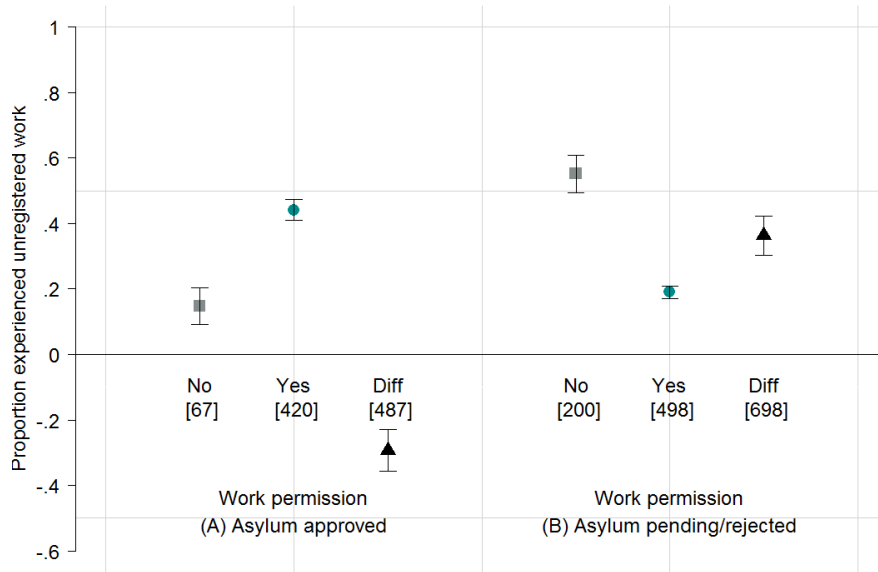
5.2.3 Further subgroup analyses

We start by reconsidering the connection between asylum status and reported work permission in order to better understand the counteracting estimates observed in Table 5. In theory, all refugees with a positive asylum outcome are legally allowed to work, and the same is true for most individuals who are still waiting for a decision or whose application was rejected, although the latter group has to go through some paper work to get the specific permission for each individual job offer. Yet 14% of our respondents with an approved asylum case and 29% of those rejected or still waiting for a decision reported not to be allowed to work. It is therefore possible that the respective survey question measures individual perceptions (which may have been shaped by negative decisions with respect to specific job offers) rather than the actual legal status. With this in mind, we repeat the multivariate analysis and include interaction terms between approved asylum status and self-reported work permission (see, Table C.1 in the Appendix). We then use the results from this regression to predict the share of respondents with unregistered work experience for the 2x2 subgroups defined by approved asylum and self-reported work permission. Figure 3 shows the predicted shares in absolute levels and by how much they differ from each other as well as the respective 95% asymptotic confidence intervals.

The results indicate an interesting interaction between approved asylum and work permission. For those respondents with a secure legal status (Panel A), the predicted share of experience with unregistered work is significantly higher among individuals who stated to have a work permit than among those without (43% to 18%). In the group of respondents with pending or rejected cases, however, the outcome is exactly opposite, with individuals who report not being allowed to work displaying a much stronger tendency to have worked without registration than their counterparts with stated work permission (55% to 20%).

This pattern is difficult to interpret and explain with just the data at hand. We offer two possibilities here, however, that seem the most plausible to us: First, refugees in general tend

Figure 3: Experience of unregistered work by approved asylum status and work permission



Note: Each square and circle represents the estimated proportion for the subgroups of interest. Triangles represent the absolute difference between those two proportions, respectively. Estimations are based on a Maximum-Likelihood estimation (MLE) with adjustment for floor effects. The solid lines represent the 95% asymptotic confidence intervals.

to be highly motivated to work, but very anxious about any illegal activity during their asylum process, such that the experience with unregistered work remains low in this group (proxied by the low number for individuals with pending asylum decision, but being allowed to work). Once obtaining their decision, they become less cautious when they either get their approval and thus a more stable legal status (group with approval and work permission) or are rejected and wrongly believe they are not allowed to work anymore and will leave the country soon (group with neither approval nor work permission). Second, as basically every respondent in our sample was theoretically allowed to work, individuals stating not being allowed to work may use that as rationalization of their behavior. In the case of approved applications for asylum, a no answer could justify why the respective individual does not actively participate in the labor market at all (I would, if I was allowed to). The same logic could apply for individuals with rejected or pending applications, just that the answer here may rationalize either inactivity or why the respondent is not pursuing legal or registered work.

Next, we take a look at the significant coefficient of not having children in Table 5 and examine its potential connection to different household constellations and the associated benefit levels. Thus, we interact the two variables marital status and the presence of children in the

household in our empirical model and predict the share of individuals with unregistered work experience for (A) single individuals without children, (B) married individuals without children, (C) single parents with children, and (D) married parents with children.²⁰

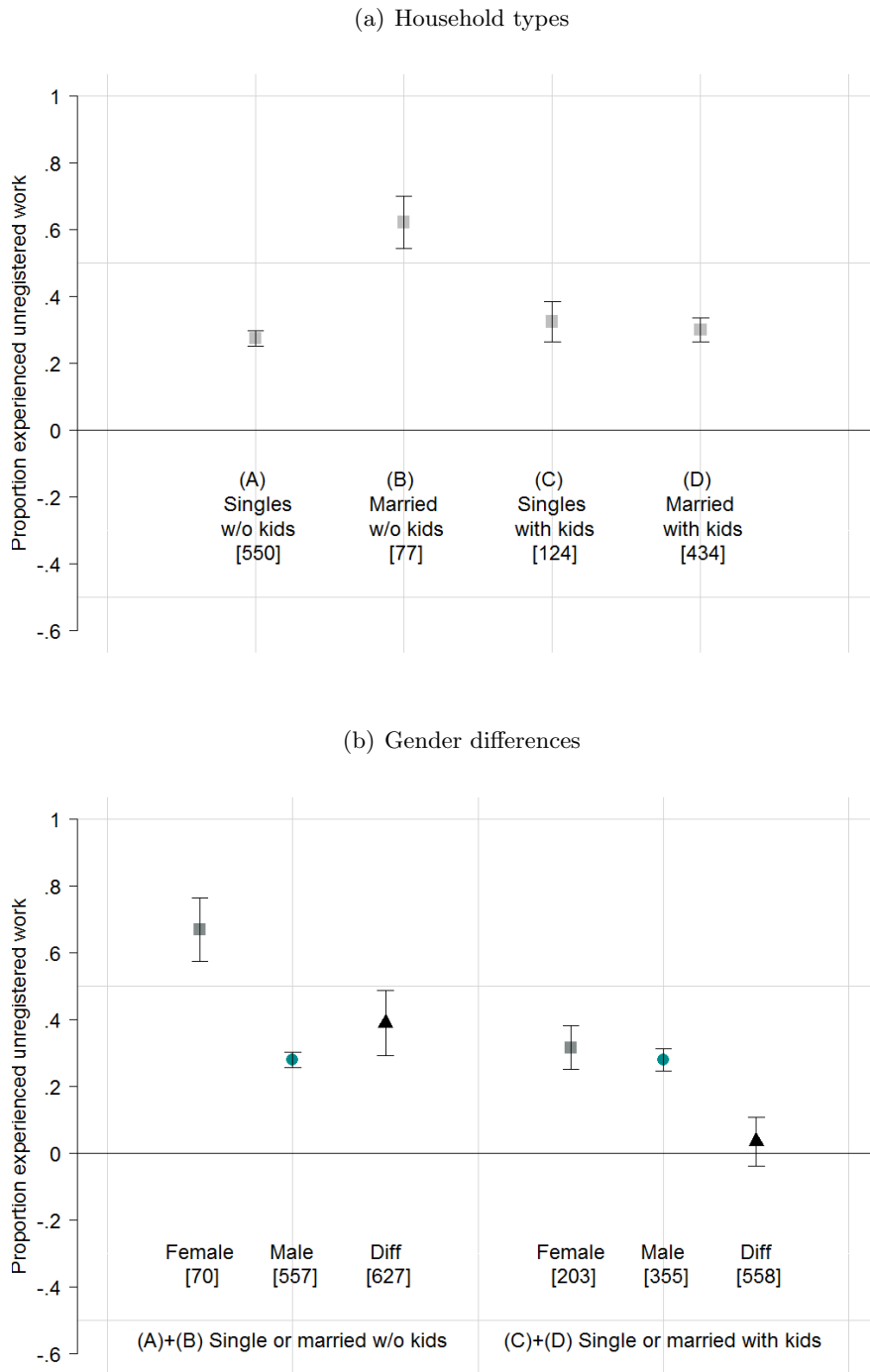
Panel (a) of Figure 4 displays the results in the order of ascending benefit levels for the respective type of household from left to right. We find very similar shares of experience with unregistered work for all household types (at roughly 28%), with the striking exception of married couples without children, in which more than six out of ten individuals have been exposed to this kind of work already (62%). Households with children obtain higher benefits than couples without children but report lower shares of unregistered work experience.²¹ This suggests again that the level of benefits does not seem to play a role in this context. However, it could also be the case that higher benefits exert some influence on the work behavior of refugees, but this effect is counteracted by the time demands of child rearing or concerns about the possible consequences of getting caught for the whole family.

Working out exactly which of these arguments (or some other) holds is beyond the scope of descriptive results. What we can do, however, is to provide more information on this issue in order to lend some support to one side or the other. To this end, we look at the experience with unregistered work by gender and presence of children. If there is a pull towards unregistered work from higher benefit levels which is countered by having less available time to work because of the demands of bringing up the children, we would expect to see a specific gender pattern in the experience with unregistered work. In particular, given that childcare is a predominantly female domain in the home-country societies of most refugees, male refugees should exhibit more experience with unregistered work when they are in a household with children, while we would expect the opposite for female refugees. Judging from the results in Panel (b) of Figure 4, we do find a much smaller rate of experience with unregistered work among women with children than for those without (31% to 65%, respectively), but there is no difference in the results for men (both groups at around 26 %). The numbers are therefore inconclusive and could also be explained by a much stronger exposure of female refugees to unregistered work in general (e.g., because of a higher tendency to work in household-related jobs), which does not fully realize when they have to care for children and are thus less active on the labor market.

²⁰ Table C.1 in the Appendix reports the full results for this model.

²¹ As in Section 2.2, we take the case of families with two children as reference point here. Single adults with only one child receive lower benefits than a married couple without children.

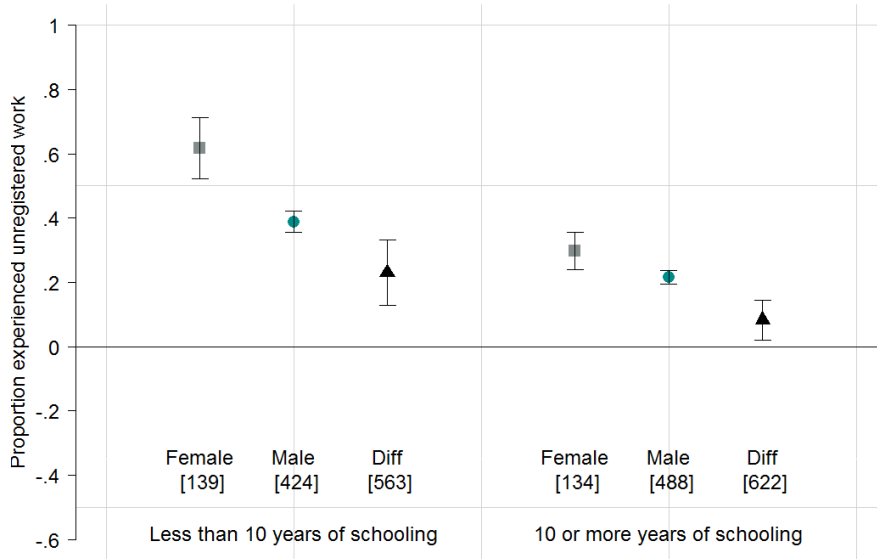
Figure 4: Experience of unregistered work by household type and gender



Note: Each square and circle represents the estimated proportion for the subgroups of interest. Triangles represent the absolute difference between those two proportions, respectively. Estimations are based on a Maximum-Likelihood estimation (MLE) with adjustment for floor effects. The solid lines represent the 95% asymptotic confidence intervals.

To conclude our discussion of statistically significant results in Table 5 and follow up on the observation of a potentially larger exposure of female refugees to jobs, we finally take a deeper

Figure 5: Experience of unregistered work by education and vocational degree



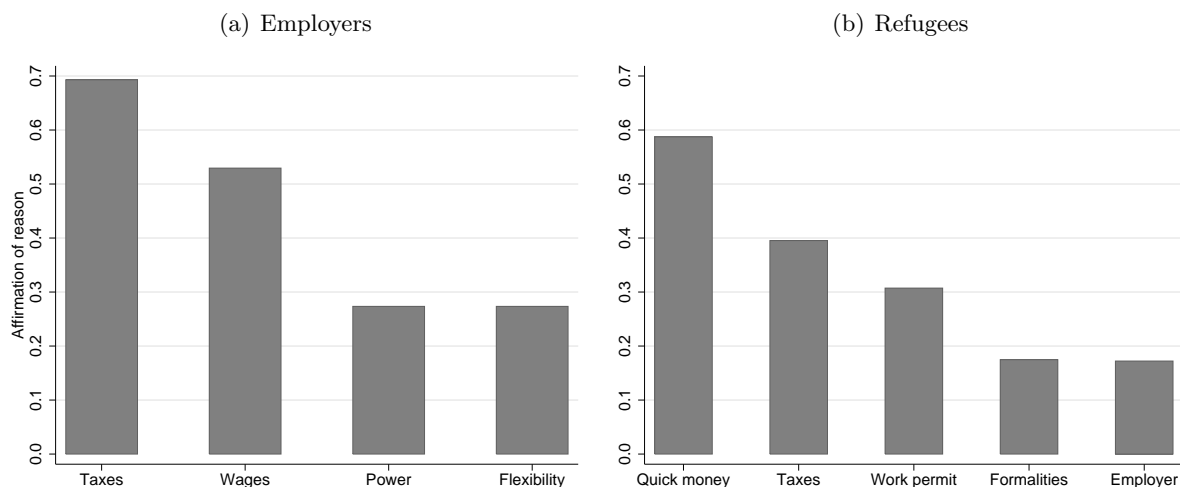
Note: Each square and circle represent the estimated proportion for the subgroups of interest. Triangles represent the absolute difference between those two proportions, respectively. Estimations are based on a Maximum-Likelihood estimation (MLE) with adjustment for floor effects. The solid lines represent the 95% asymptotic confidence intervals.

look at unregistered work experience by educational level (less than 10 years of schooling vs. at least 10 years) and gender. Looking at the predicted rates of experience for the resulting four groups in Figure 5, we first note that the negative correlation between unregistered work experience and education holds for both genders, but seems to be stronger for female refugees. Thus, we find a large gender gap in unregistered work experience among individuals with shorter school attendance (62% to 39% for females and males, respectively), but a much smaller one in the group with at least 10 years of formal education (30% to 23%). Female refugees seem to benefit the most from higher education, may indicate a promising way to address the high rates of unregistered work experience in this group.

5.3 Possible reasons for unregistered work

After the list experiment, we additionally asked all survey participants directly about possible reasons and explanations for why refugees might experience unregistered work in Germany. As an introduction to the question, we first explained that some employers hire refugees, but do not officially register them, and that we are interested in learning about possible reasons for this behavior. The respondents could then choose from pre-formulated answer options as well as

Figure 6: Possible reasons for unregistered work from the perspective of refugees



Note: The shares relate to the number of participants answering the respective question (746 in Panel a, and 807 in Panel b). The exact wordings of the two questions are: (a) "Some employers in Germany hire refugees, but do not officially register them as workers. In your opinion, what are the main reasons for this?", and (b) "What do you think are the main reasons for refugees to work in jobs without official registration?". In both cases, the respondents were free to volunteer as many possible reasons as they wanted.

suggest alternative motives. Although the question did not imply any wrongdoing on the part of the refugees, only 59% volunteered any reason at all. This contrasts with 94% who answered the list experiment in the treatment group and demonstrates the strength of refugees' reservations about discussing illegal behavior explicitly.

Panel (a) of Figure 6 reveals that survey participants rated the financial motives of employers as more relevant than any possible non-monetary reasons for not registering their refugee employees. About 70% think that employers do not register their workers to save tax payments and social security contributions. Likewise, the possibility to pay lower wages or to shirk wages was stated by 53% of respondents. The arguments that employers prefer the possibility to hire and fire workers at will or want to have power over them were only brought up by 27%, respectively. Finally, 5% provided alternative reasons, for instance, the lack of work permission.

The same pattern emerges in Panel (b), where we display the answers to a second direct question, this time asking for potential reasons of refugees to work without registration in Germany. As for the employer side, the respondents saw the monetary benefits as main driver for why refugees would work without registration. Almost 60% of the answers stated the motivation of earning money as quickly as possible, and 40% thought that refugees want to save taxes and social security contributions or avoid cuts in their benefits. By comparison, only 30%

of respondents indicated missing work permission as potential reason, 17% believed that the employer refuses to register the refugee, 17% thought that refugees want to avoid paperwork and formalities, and 9% provided alternative explanations, e.g., low German proficiency or lack of knowledge about the rules and laws in Germany. About 36% of respondents did not know what to answer or was not willing to provide an answer to this question.

The answers to these direct questions about the possible motives for working/hiring without registration suggest that monetary reasons for unregistered work clearly dominate non-monetary ones in the eyes of respondents. Possible counter-measures could therefore include more controls and higher sanctions on the employer side, and information campaigns explaining the risks of getting caught when working without registration on the one hand and highlighting the relative flexibility and generosity of the 450 Euro mini-job system on the other.

6 Discussion and conclusion

In this paper, we study the prevalence of unregistered work among refugees in Germany and work out predictive risk factors associated with it. Using a list experiment in a survey among more than 1,200 refugees in 2018, we find that almost one out of three refugees had worked without registration at some point of time since their arrival in Germany. This share is substantial and close to the fraction who reported to work in a regular job at the time of the survey or had been employed before (41%). Groups that are particularly exposed to experience unregistered employment are refugees without approved asylum and work permission, women without children, and women with low formal education in general. Methodologically, we note that a list experiment leads to a much higher willingness to provide information than direct questions about a sensitive topic like unregistered work.

Given the problems with unregistered work for society and refugees alike, our results may help in designing and implementing informed countermeasures for this specific group that go beyond the general recommendations that surely also apply in this case (i.e., to make regular employment more attractive and to increase the risks of irregular jobs by stepping up detection and persecution efforts). In particular, we can think of four areas in which concrete measures may help: (1) As unregistered work is defined and treated differently in different countries, authorities should inform refugees clearly and early on about what constitutes unregistered work in Germany and how being caught in such an activity may affect future decisions on the

asylum case, extensions of a temporary tolerance, or an application for permanent residence status. (2) Along the same lines, authorities should provide more information about the process of obtaining work permission for specific jobs early on, so that misperceptions about whether one is allowed to work can be reduced. Also, it is important for understanding and accepting the process to explain clearly why the job offers have to be checked, which is to uphold labor, health, and wage standards and prevent exploitative work conditions. (3) At the same time, we believe that streamlining and speeding up the process to check job offers and grant work permission may be very helpful for the acceptance of the system, as people may not understand why they are kept back for weeks when they could work and earn their living already. (4) As female refugees seem to be particularly exposed to unregistered jobs, particular attention should be paid to outreach information efforts to this group. While these measures may cost money and time initially, letting unregistered work run its course may be even more costly in the long run, especially if people get used to living with benefit receipt and complementary income from unregistered jobs.

We acknowledge that the results of our study come with some limitations. To start, we do not have a random sample of refugees and thus cannot claim perfect external validity, even though the findings of subgroup analysis suggest that it may serve as a good approximation. Additionally, the selection of the non-sensitive control items led to a majority of our respondents indicating zero experiences in the list experiment. We therefore relied on the Blair and Imai (2012) estimation procedure which adds another identifying assumption. Further implementations of list experiments in this setting need to work out a better set of control items to be less dependent on a particular adjustment mechanism. And finally, we formulated the sensitive item we were looking for as having had experience at any time since arrival in the destination country. Our main result thus reflects the rate of accumulated experience with unregistered work, which is interesting in the context of refugees, but does not provide any insight on the prevalence rate in a well-defined period of time, which may be more relevant to policy-makers, especially when tracked over time or compared to natives or other migrant groups.

Given the importance of the topic and the potential of list experiments to contribute significantly to an informed discussion about unregistered work, we strongly encourage future work in this area. An obvious starting point would be to repeat the analysis addressing the limitations laid out above. An important extension could be the inclusion of the native population

or previous migrants in the same experiment to define a benchmark in the whole population. Additionally, list experiments could be used to conduct rigorous randomized control trials to examine the effectiveness of measures to combat unregistered work. And finally, it would be important to complement our analysis of the supply side of unregistered work by studying the demand side (firms and private households) to obtain a more comprehensive understanding of the underlying mechanisms of unregistered work.

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Appendix - For Online Publication

A Balancing of sample characteristics

In order to test whether the random allocation of participants to the control and treatment group worked well, we check the balancing of various observed characteristics in Table A.1. Column 1 shows the mean values of the whole sample, columns 2 and 3 report the respective means for the control and the treatment group. Finally, column 4 displays the corresponding p-values of a simple t-test for statistical difference between the two groups.

Table A.1: Balancing of sample characteristics

	(1)	(2)	(3)	(4)
	All respondents	Control group	Treatment group	Δ p-value
Personal characteristics				
Female	0.240	0.248	0.232	0.493
Age in years	31.4	31.3	31.4	0.912
<i>Country of origin</i>				
Syria	0.225	0.237	0.212	0.292
Afghanistan	0.162	0.166	0.158	0.670
Iraq	0.140	0.138	0.141	0.865
Iran	0.061	0.061	0.061	0.992
Gambia	0.128	0.124	0.132	0.678
Nigeria	0.102	0.094	0.111	0.328
Other African countries	0.103	0.105	0.101	0.821
Other countries	0.079	0.074	0.084	0.518
Family characteristics				
Married/Partnership	0.435	0.443	0.428	0.591
No children	0.522	0.521	0.523	0.963
Num. children at home	2.071	2.070	2.072	0.985
Waiting for relatives	0.218	0.224	0.212	0.599
Education and skills				
Years of schooling	9.561	9.537	9.585	0.779
Work experience in yrs	6.469	6.476	6.461	0.975
Vocational training	0.317	0.327	0.307	0.459
No problems in reading	0.616	0.614	0.617	0.897
Reading not evaluated	0.110	0.111	0.109	0.904
German proficiency (\geq average)	0.604	0.606	0.601	0.865
Number of obs.	1,259	637	622	

< table continues on next page >

Almost all p-values are higher than 0.1, indicating that the two groups are very similar in their characteristics. The only exception is the regional distribution, where the regional units of Karlsruhe and Freiburg are under- and overrepresented in the treatment group. We will therefore control for the regional distribution in the empirical analysis. The joint p-value from

Table A.1: < continued >

	(1)	(2)	(3)	(4)
	All respondents	Control group	Treatment group	Δ p-value
Labor market status				
Work permission	0.772	0.766	0.778	0.611
Labor market activity	0.781	0.763	0.799	0.122
Searching specific job	0.191	0.182	0.199	0.436
<i>Search with support by</i>				
Local employment office	0.215	0.214	0.217	0.879
Job Center	0.340	0.334	0.346	0.673
Migration characteristics				
Arrival before 2015	0.056	0.063	0.050	0.319
<i>Legal status</i>				
No decision yet	0.199	0.193	0.206	0.573
Accepted	0.415	0.427	0.404	0.399
Rejected	0.369	0.356	0.383	0.334
<i>Status of Rejection</i>				
Suspension of deport.	0.237	0.220	0.254	0.153
Asked to leave Germany	0.103	0.108	0.098	0.551
Regional characteristics				
<i>District of residence</i>				
Karlsruhe	0.316	0.349	0.283	0.012***
Stuttgart	0.277	0.259	0.296	0.145
Tübingen	0.110	0.124	0.096	0.119
Freiburg	0.293	0.265	0.322	0.028***
District is Landkreis	0.736	0.750	0.720	0.226
Joint significance		$\chi^2(34) = 37.02$ p-value = 0.331		
Number of obs.	1,259	637	622	

Note: LEA is the abbreviation for Local Employment Agencies. The p-value on the joint significance is from a test that differences between the characteristics of the control and treatment group are jointly zero.

a test of the hypothesis that assignment to the treatment and control group is unrelated to the observed characteristics is 0.331. This suggests that random assignment was successful and the two groups are credible counterfactuals for each other.

B Methodological details

B.1 Identification of the joint distribution of answers

Here, we describe details on the joint distribution of answers. In our application, there are 12 possible types of respondents $(Y_i(0), Z_{i,j+1}^*)$. $Y_i(0)$ denotes how many of the control items would be affirmatively answered by each respondent type. $Z_{i,j+1}^*$ indicates the true response to the sensitive item. All 12 respondent types are shown in Table B.1. For example, type $(Y_i(0), Z_{i,j+1}^*) = (3, 1)$ is a respondent that would affirmatively answer three of the control items and the sensitive item. Type $(Y_i(0), Z_{i,j+1}^*) = (3, 0)$ is a respondent that would affirmatively answer three of the control items but not the sensitive item. Consequently, those who answer with, for example, count ‘3’ in the control group can consist of respondent types (3,1) and (3,0). Those who answer with count ‘3’ in the treatment group are respondent types (3,0) and (2,1).

Table B.1: Respondent types across treatment and control group

Response Y_i	Treatment group ($T_i = 1$)	Control group ($T_i = 0$)
6	(5,1)	
5	(4,1)(5,0)	(5,1)(5,0)
4	(3,1)(4,0)	(4,1)(4,0)
3	(2,1)(3,0)	(3,1)(3,0)
2	(1,1)(2,0)	(2,1)(2,0)
1	(0,1)(1,0)	(1,1)(1,0)
0	(0,0)	(0,1)(0,0)

The population proportions of the respondent types (0,0) and (J,1), here (5,1), are known from the share of respondents that answer with $Y_i = 0$ and $Y_i = J + 1$, here $Y_i = 6$ in the treatment group. Thus, the population proportion of each type $\tau_{yz} = Pr(Y_i(0) = y, Z_{i,J+1}^* = z)$ with $z = 0, 1$ is identified from observed data under Assumptions 1–3 as

$$\begin{aligned}\tau_{y1} &= Pr(Y_i \leq y|T_i = 0) - Pr(Y_i \leq y|T_i = 1) \\ \tau_{y0} &= Pr(Y_i \leq y|T_i = 1) - Pr(Y_i < y|T_i = 0).\end{aligned}$$

To give an illustrative example, consider the identification of the population proportion of respondent types (3,1) and (3,0). For respondent type (3,1), we calculate the cumulative probability of answering $y = 0,1,2$, or 3 in the control group and subtract the cumulative probability of answering $y = 0,1,2$, or 3 in the treatment group,

$$\begin{aligned}\tau_{31} &= Pr(Y_i \leq 3|T_i = 0) - Pr(Y_i \leq 3|T_i = 1) \\ &= Pr[(3, 1) + (3, 0) + (2, 1) + (2, 0) + (1, 1) + (1, 0) + (0, 1) + (0, 0)] \\ &\quad - Pr[(3, 0) + (2, 1) + (2, 0) + (1, 1) + (1, 0) + (0, 1) + (0, 0)] = Pr(3, 1).\end{aligned}$$

For respondent type (3,0), we would calculate the cumulative probability of answering $y = 0,1,2$, or 3 in the treatment group and subtract the cumulative probability of answering $y = 0,1$, or 2 in the control group,

$$\begin{aligned}\tau_{30} &= Pr(Y_i \leq 3|T_i = 1) - Pr(Y_i < 3|T_i = 0) \\ &= Pr[(3, 0) + (2, 1) + (2, 0) + (1, 1) + (1, 0) + (0, 1) + (0, 0)] \\ &\quad - Pr[(2, 1) + (2, 0) + (1, 1) + (1, 0) + (0, 1) + (0, 0)] = Pr(3, 0).\end{aligned}$$

B.2 Test for detecting design effects

Blair and Imai (2012) designed a statistical test for detecting violations against the assumption of *No design effect* using the identification of the joint distribution as shown above. Under the null hypothesis of *No design effect*, belonging to the treatment group $T_i = 1$ and thus being confronted to the sensitive item, makes the count Y_i larger than the count in the control group but at most by one item, $Y_i(1) = Y_i(0) + Z_{i,J+1}(1)$. H_0 can be formalized by the following two restrictions

$$H_0 : \begin{cases} Pr(Y_i \leq y|T_i = 0) \geq Pr(Y_i \leq y|T_i = 1) & \text{for all } y = 0, \dots, J-1 \text{ and} \\ Pr(Y_i \leq y|T_i = 1) \geq Pr(Y_i \leq y-1|T_i = 0) & \text{for all } y = 1, \dots, J. \end{cases} \quad (1)$$

The first restriction implies that the cumulative probability of answering y in the control

group is equal or larger than the cumulative probability of answering y in the treatment group at each $y = 0, \dots, J - 1$. If for example, $y = 2$, the first restriction becomes $Pr(Y_i \leq 2|T_i = 0) \geq Pr(Y_i \leq 2|T_i = 1)$. Under H_0 , respondents for which two control items and the sensitive item are true, here type (2,1), respond $Y_i = 3$ instead of $Y_i = 2$ if confronted to the sensitive item. This lowers the cumulative probability at $y = 2$ in the treatment group.

The second restriction implies that, at the same time, that the cumulative probability of answering y in the treatment group is equal or larger than the cumulative probability of answering $y - 1$ in the control group at each $y = 1, \dots, J$. For $y = 2$ this leads to $Pr(Y_i \leq 2|T_i = 1) \geq Pr(Y_i \leq 1|T_i = 0)$. This is true, if respondents increase their count by at most 1. If they increase it by more than 1, the distribution of answers is shifted upwards in the treatment group and the restriction is no longer true for all values of y .

If both restrictions hold, all proportions of respondent types should be non-negative $\tau_{yz} \geq 0$ for all y and $z = 0, 1$. Consequently, H_0 can never be rejected when all τ_{yz} are non-negative. It is always rejected if all τ_{yz} are negative. If at least one of τ_{yz} is negative, it has to be tested whether this negative value occurs by chance. The proposed testing procedure implements two separate hypothesis tests for the two restrictions in equation (1) and uses a Bonferroni correction to combine the results from both tests. H_0 is rejected only if the minimum of the two p-values from the tests is less than $\alpha/2$. The test statistic and details on the test procedure can be found on p. 64-65 in Blair and Imai (2012).

Table B.2: Estimated proportions of respondent types $\hat{\tau}_{yz}$

y	0	1	2	3	4	5
τ_{y0}	0.621 (0.020)	0.171 (0.025)	0.078 (0.018)	0.010 (0.010)	-0.002 (0.007)	-0.001 (0.004)
τ_{y1}	0.041 (0.028)	0.010 (0.021)	0.041 (0.014)	0.015 (0.008)	0.009 (0.006)	0.005 (0.006)

Note: τ_{yz} are the estimated proportions of respondent types. y gives the number of affirmative answers to the control items and z the truthful answer to the sensitive item. For example, the share of respondents for which none of the control items is true and who never experienced unregistered work $\tau_{0,0} = Pr(0,0)$ is estimated to be 62.1%. We use the R command 'ict.test' from the list package to estimate the proportions (Blair et al., 2014).

In Table B.2, we show the estimated proportions of all 12 respondent types in our application. The large majority of proportions are non-negative. However, the point estimates of the shares $\tau_{4,0}$ and $\tau_{5,0}$ are negative but not significantly different from zero. This is an indication that we cannot reject H_0 . The implementation of the test confirms this result with a large p-value

of 1. Thus, we can conclude the assumption of *No design effects* can not be rejected in our application. The inclusion of the sensitive item to the list of experiences did not change the responses to the control items.

B.3 Implementation of the ML estimator

Imai (2011) proposes a maximum likelihood estimator that uses the information of the joint distribution of respondent types to overcome the loss of statistical efficiency that is inherent in non-linear and linear regressions. In this section, we summarize how he construct the likelihood function and apply this to our application. The starting point is the classification of respondent types in four groups according to their treatment status T_i and response Y_i .

1. $(T_i, Y_i) = (1, 0)$: The first group are respondents in the treatment group that answer with count '0', thus respondent type $(Y_i(0), Z_{i,j+1}^*) = (0, 0)$
2. $(T_i, Y_i) = (1, J + 1)$: The second group are respondents in the treatment group that answer with $J + 1$, which is count '6' in our application. They are respondent type $(Y_i(0), Z_{i,j+1}^*) = (J, 1) = (5, 1)$
3. $(T_i, Y_i) = (0, y)$: The third group are respondents in the control group that answer with 'y'. They belong to respondent type $(Y_i(0), Z_{i,j+1}^*) = (y, 1)$ or $(y, 0)$ which are $(0,1), (1,1), (2,1), (3,1), (4,1), (5,1), (0,0), (1,0), (2,0), (3,0), (4,0), (5,0)$.
4. $(T_i, Y_i) = (1, y)$ with $0 < y < J + 1$: The fourth group are respondents in the treatment group and answer with 'y', thus respondent type $(Y_i(0), Z_{i,j+1}^*) = (y, 0)$ or $(y - 1, 1)$. which are $(1,0), (2,0), (3,0), (4,0), (5,0), (0,1), (1,1), (2,1), (3,1), (4,1)$.

Using this classification where $\mathcal{J}(t, y)$ stand for respondents type $(T_i, Y_i) = (t, y)$, and defining $h_z(y; x, \psi_z) = Pr(Y_i(0) = y | Z_{i,J+1}^* = z, X_i = x)$ and $g(x, \delta) = Pr(Z_{i,J+1}^* = 1 | X_i = x)$, the likelihood function that applies to the observed data is

$$\begin{aligned}
L_{obs}(\psi_0, \psi_1, \gamma; \{Y_i, T_i, X_i\}) = & \underbrace{\prod_{i \in \mathcal{J}(1,0)} (1 - g(X_i, \delta)) h_0(0; X_i, \psi_0)}_{\text{Group 1}} \times \\
& \underbrace{\prod_{i \in \mathcal{J}(1,6)} g(X_i, \delta) h_1(5; X_i, \psi_1)}_{\text{Group 2}} \times \\
& \underbrace{\prod_{y=0}^J \prod_{i \in \mathcal{J}(0,y)} \{g(X_i, \delta) h_1(y; X_i, \psi_1) (1 - g(X_i, \delta)) h_0(y; X_i, \psi_0)\}}_{\text{Group 3}} \times \\
& \underbrace{\prod_{y=1}^J \prod_{i \in \mathcal{J}(1,y)} \{g(X_i, \delta) h_1(y-1; X_i, \psi_1) (1 - g(X_i, \delta)) h_0(y; X_i, \psi_0)\}}_{\text{Group 4}}
\end{aligned}$$

Imai (2011) proposes an Expectation-Maximization (EM) algorithm to solve the complex likelihood function. These regression models are implemented in the R programming package ‘list’ by (Blair et al., 2014) and in the empirical analysis.

B.4 Adjusting for floor effects

Floor effects occur when respondents for which only the sensitive item is true, answer with $Y_i = 0$ instead of giving the true answer $Y_i = 1$ because they fear that this would reveal that they have experienced unregistered work. As shown in Table B.3 in bold numbers, those who respond with $Y_i = 0$ but are respondent type (0,1) would be liars.

Blair and Imai (2012) discuss possible approaches when dealing with floor effects.²² They show the derivation of sharp bounds which can not be implemented in multivariate analysis. As an alternative solution, they propose a set-up to quantify the bias that occurs from floor effects and develop a statistical correction procedure.

First, they define the conditional probability of lying as

$$\underline{q} \equiv Pr(Y_i(1) = 0 | Y_i(0) = 0, Z_{i,J+1}^* = 1).$$

²² Blair and Imai (2012) discuss the procedure when floor and ceiling effects exist. Since we are facing floor effects only, we limit the presentation to this setting.

Table B.3: Respondent types when floor effects exist

Response Y_i	Treatment group ($T_i = 1$)	Control group ($T_i = 0$)
6	(5,1)	
5	(4,1)(5,0)	(5,1)(5,0)
4	(3,1)(4,0)	(4,1)(4,0)
3	(2,1)(3,0)	(3,1)(3,0)
2	(1,1)(2,0)	(2,1)(2,0)
1	$(1 - \underline{q}) \leftarrow$ (0,1)(1,0)	(1,1)(1,0)
0	(0,0)(0,1)	$\rightarrow \underline{q}$ (0,1)(0,0)

\underline{q} is the population proportion of liars who answer $Y_i = 0$ when they are assigned to the treatment group among those for which only the sensitive item is true $\tau_{0,1} = (0,1)$. When proportion \underline{q} is positive, respondents in the treatment group that answer $Y_i = 0$ are a mixture of type (0,0) and the proportion \underline{q} of type (0,1). Those in the treatment group that answer with $Y_i = 1$ are a mixture of type (1,0) and the $(1 - \underline{q})$ proportion of type (0,1). We can bound the true proportion of respondent type $\tau_{0,1}^*$ by focusing on the two scenarios in which all members of this group answer honestly or all members are liars. If all members answer honestly ($\underline{q} = 0$) the estimated proportion of respondent type $\tau_{0,1}$ is the true proportion. In our application the lower bound is $\tau_{0,1} = 0.041$. If all members are liars ($\underline{q} = 1$) the upper bound equals $Pr(Y_i = 0|T_i = 0)$ which is the probability to answer with count ‘0’ if assigned to the control group. This is $Pr(Y_i = 0|T_i = 0) = Pr(0,1) + Pr(0,0) = 0.041 + 0.621 = 0.662$ in our application. Using these bounds we can infer the sharp bounds of the population proportion of the respondents whose truthful answer is affirmative for the sensitive item as

$$\underbrace{\sum_{y=0}^J \{Pr(Y_i \leq y|T_i = 0) - Pr(Y_i \leq y)|T_i = 1\}}_{\text{Lower bound}} \leq Pr(Z_{i,J+1}^* = 1) \leq \underbrace{Pr(Y_i = 0|T_i = 0) + \sum_{y=1}^{J-1} \{Pr(Y_i \leq y|T_i = 0) - Pr(Y_i \leq y)|T_i = 1\}}_{\text{Upper bound}}.$$

Here, the sharp bounds imply that the true proportion of refugees who experienced unregistered work varies between 12% and 74% ($0.122 \leq Pr(Z_{i,J+1}^* = 1) \leq 0.74$) which is a large range and not very informative. In the following, we explain the Blair and Imai (2012) strategy to quantify the fraction of liars and the statistical correction procedure.

They impose an additional assumption that allows to directly model floor effects and to estimate the proportion of respondents for which the sensitive item is true as well as the relationship to respondents characteristics even if floor effects exist. This assumption (A4) can be formalized as

$$Pr(Y_i(0) = y | Z_{i,J+1}^* = 1, X_i = x) = Pr(Y_i(0) = y | Z_{i,J+1}^* = 0, X_i = x).$$

It implies that respondents answers to the control items are independent of their truthful answer to their sensitive item given pretreatment covariates X_i . This is similar to a conditional independence assumption in observational studies. Blair and Imai (2012) point out that it may be plausible if the control items are not substantially related to the sensitive item. In our application this could be critical if we do not condition on pretreatment characteristics because experiences on the labor market are likely be correlated with the experience of unregistered work. As an example, the assumption is violated if the probability of experiencing problems to get the vocational degree recognized is different between those who experienced unregistered work and those who did not. Therefore, we follow the recommendation of Blair and Imai (2012) and condition on pretreatment covariates that have high predictive power in explaining respondents answer behavior to the control item. Since the control items in our application largely focus on experiences during job search, we control for gender, indicators of vocational degree, and an indicator for current or already successful job search when we implement the correction procedure. We argue that given these covariates assumption A4 is likely to hold. Then, all respondents types τ_{yz} are identified even if floor effects exist.

Assumption A4 implies that the non-linear constraint $\sum_{j'=0}^J \tau_{j'1} = \tau_{j1}/(\tau_{j1} + \tau_{j0})$ is added to the model Blair and Imai (2012). Furthermore, the model for the control items simplifies to $h(y; x, \psi) = Pr(Y_i(0) = y | X_i = x)$ and the conditional probability of lying is added to the likelihood function as $\underline{q}(x, \kappa) = Pr(Y_i(1) = 0 | Y_i(0) = 0, Z_{i,J+1}^* = 1, X_i = x)$. The parameters $g(x, \delta)$, $h(y; x, \psi)$, and $\underline{q}(x, \kappa)$ can be estimated using the binomial logistic regression models that are implemented in the R package 'list' Blair et al. (2014).

C Additional results

Table C.1: Multivariate analysis of the list experiment

	(1)		(2)		(3)	
	MLE		MLE		MLE	
Intercept	-0.699	(0.882)	-0.809	(0.926)	-1.316	(0.917)
Arrived later than 2015	-0.644	(0.540)				
Asylum approved	-2.111	(1.742)				
Work permission	-1.744	(0.718)**				
Female	0.975	(0.649)	0.708	(0.597)	0.174	(0.815)
Married/partnership	1.085	(0.672)	-0.130	(0.816)	0.637	(0.672)
No kids	0.867	(0.692)	-0.074	(0.795)	0.415	(0.764)
Labor market activity	0.583	(0.588)	0.208	(0.544)	0.180	(0.530)
Vocational degree	-0.719	(0.527)	-0.681	(0.476)	-0.690	(0.491)
Asylum approved \times Work permission	3.277	(1.807)*				
Married/partnership \times No kids			1.490	(1.118)		
Married/partnership \times Female					1.534	(1.234)
Num. of observations	1185		1185		1185	
Regional controls	yes		yes		yes	
Adjust for floor effects	yes		no		yes	

Note: The dependent variable is the response to the list experiment question. It could be 0, 1, 2, 3, 4, or 5 for respondents in the control group. It could be 0, 1, 2, 3, 4, 5, or 6 for respondents assigned to the treatment group. We show the coefficients of the sensitive item interacted with different the covariates. All models are estimated by Maximum-Likelihood estimation (MLE) with the floor adjustment procedure and the coefficients presented in log-odds. In column 1, we add interaction terms between the asylum approval indicator and arrival time and work permission, respectively. In column 2, we add an interaction term between the relationship state and an indicator for children in the household. Finally in column 3, we add an interaction term between the relationship state and gender. Heteroskedasticity-consistent robust standard errors are presented in parentheses.