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Unregistered work among refugees: Evidence from a list experiment in Germany[*]

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Abstract

The integration of refugees in host countries' labor markets is complicated by structural barriers, missing formal qualification and language deficiencies. This leads to widespread concern that refugees may end up in informal and precarious employment relationships. Empirical evidence on the prevalence of unregistered work is missing, however, due to the sensitive and illegal nature of this phenomenon. In this paper, we conduct a list experiment to measure unregistered work among refugees in Germany. Our results indicate that 31% have had experience with an unregistered job since their arrival. Refugees who report that they do not have work permission show a significantly higher likelihood of experiencing unregistered work. Furthermore, the lack of post-secondary education and vocational degrees, and a low German proficiency predict the risk to work without registration.

Keywords: Unregistered work, Informal employment, List experiment, Refugees, Germany, Survey experiment
JEL Codes: J46, J61, C83

1 Introduction

The number of refugees worldwide is at a record high and millions of people are seeking protection in another country (UNHCR, 2021). Given the unstable economic, ecological, and security situation in the countries of origin, many refugees will stay in their current host countries for a long period of time and often even for good. The integration of these individuals into the labor market is a crucial step towards a sustainable inclusion in the host societies, but this challenges refugees, companies and political decision-makers alike. Recent studies show that refugees need significantly more time to find employment than other groups of migrants (Brell et al., 2020; Dustmann et al., 2017; Fasani et al., 2021). Major obstacles include certain aspects of asylum regulations, e.g., spatial dispersion policies, physical and mental health problems caused by traumatic experiences during forced displacement, low formal qualifications, and low proficiency in the host countries' languages.

At the same time, refugees show a great willingness to work in order to earn a living, pay back debts from their flight, and support their relatives still living in their countries of origin (Hartmann et al., 2018). This combination provides fertile ground for a situation in which refugees engage in the informal labor market.[1] This is problematic for both refugees and host countries. On the one hand, unregistered work exposes the affected workers to exploitative work conditions, bears the risk of negative consequences for their legal status, and potentially slows down their integration into the formal labor market. On the other hand, it also harms society as a whole through tax evasion, benefit fraud, and unfair competition of companies relying on cheap informal labor (European Commission, 2019; International Labor Organisation, 2017, 2013).

In this paper, we measure the prevalence of unregistered work among refugees in Germany and their risk factors. We answer the following research questions: What is the share of refugees who experienced to work without registration since their arrival in Germany? And what characteristics are related to a higher risk of being exposed to unregistered work? So far, our knowledge about the prevalence of unregistered work in general and among refugees in particular is very limited and relies mainly on anecdotal evidence (e.g., Deutschlandfunk, 2016; ZEIT Online, 2017; Süddeutsche Zeitung, 2016). The difficulty of gaining empirical evidence on this topic is rooted in the nature of unregistered work not being documented. Fear of possible sanctions and feelings of shame complicate the data collection because affected individuals tend to remain silent about their experiences.

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[1]   Unregistered work is defined in Germany as doing jobs with the intention to earn money that are not registered with the tax authorities or the social insurance (§1 *Schwarzarbeitsbekämpfungsgesetz*). Throughout this paper, we will use the term "unregistered work" interchangeably for "illegal work", "informal employment", and "undeclared work".

Existing approaches of directly or indirectly asking individuals in surveys about experiences with undeclared work (e.g., European Commission, 2019; Feld and Larsen, 2012) are therefore prone to social desirability bias and likely underestimate the true prevalence of unregistered work.[2]

We overcome the challenge of measuring unregistered work by implementing a list experiment in a survey among 1,200 refugees in the German state of Baden-Württemberg. In a list experiment, researchers randomly assign the respondents to two groups. The control group is confronted with a list of non-sensitive items, in our case possible experiences on the German labor market. The treatment group receives the same list plus the sensitive item of interest, here the experience of unregistered work. In both groups, the participants do not have to state which of the items they have experienced already only how many in total. From the differences of the replies between the two groups, we can then infer the share of respondents who have experienced unregistered work. This method guarantees a high degree of anonymity to respondents and thus helps to elicit truthful responses to sensitive questions that reveal inappropriate behavior or illegal activities which are related to shame and social desirability bias (Droitcour et al., 1991; Kuklinski et al., 1997).[3]

List experiments have been successfully applied in political and social sciences to estimate the prevalence 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 our paper, Kirchner et al. (2013) examine methods to overcome the social desirability bias in answering behavior to questions about undeclared work activities in Germany. They report a significantly higher prevalence rate of undeclared work in a list experiment compared to direct questioning.

We find that about 30% of refugees in our sample have experienced at least one episode of working without registration since their arrival in Germany. Thus, the share of respondents with that experience is as high as three fourths of those in regular employment at that time of the survey or at some point since their arrival in Germany (together 41%). In a subgroup analysis, we identify particularly vulnerable groups, e.g., female refugees, Syrians, refugees with an approved asylum application, refugees without a work permission, or refugees who worked in occupations that are prone to unregistered work

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[2] Other methods rely on macroeconomic indicators, such as national accounting (e.g., Schneider and Boockmann, 2016, 2018), aggregated labor input and output ratios (e.g., Williams et al., 2017), or the electricity consumption (e.g., Altindağ et al., 2020) to indirectly estimate the size of the informal economy. None of these methods can differentiate the share of unregistered work on the overall informal economy or can identify how particular subgroups of individuals are affected.

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

in Germany among others. Since many of these characteristics are highly correlated, we implement a multivariate analysis. We identify the lack of a work permission, low levels of vocational and post-secondary education, and low German proficiency as predictive risk factors.

Our work contributes to understanding the phenomenon of unregistered work among refugees. While the few existing studies on this topic exclusively focus on the impact of refugee inflows in countries with a large informal sector such as Turkey (Altindağ et al., 2020) or Jordan (Lockhart, 2019), we focus on Germany as a developed country with a regulated labor market and a relatively small informal sector (Medina and Schneider, 2018). Furthermore, our findings complement the literature that discusses the integration of refugees in formal labor markets (Dustmann et al., 2017; Fasani et al., 2021). We find that many of the characteristics that are identified as obstacles for the successful inclusion of refugees in the formal labor market are in turn positively correlated with experiences at the informal labor market. By identifying these predictive risk factors, our study provides a starting point for a discussion about policy instruments to reduce undeclared work among refugees.

In the remainder of this paper, we first describe the institutional context and summarize the situation of refugees at the German labor market in Section 2. Section 3 then provides information on the data collection followed by the presentation of the list experiment and a summary of sample characteristics. In Section 4, we lay out the empirical strategy. In Section 5, we present the results. Section 6 discusses possible limitations of the study and concludes.

2   Institutional context in Germany

According to the economic theory of crime, individuals engage in illegal activities when the expected utility of these activities 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. The latter includes inactivity as well as preparing for later entry by getting education or participating in a training course.[4] 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 potential obstacles that might hinder their labor market integration.

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[4]   Regular employment could be combined with unregistered work, e.g., by doing some unregistered work as a side job to a regular employment. However, we abstract from these specific arrangements and focus on the general circumstances and mechanisms.

2.1 Access to the labor market

An important institutional aspect for the labor market integration of refugees[5] is whether and when they are allowed to work. In Germany, this depends on a combination of legal status and time passed since the asylum application was submitted (see Figure 1).[6] Refugees can apply for asylum directly after entering the country to start the formal process and obtain a residence permission until receiving a decision on the application (*Aufenthaltsgestattung*). During the first three months of the asylum process, asylum seekers are not allowed to take up employment. With the beginning of the fourth month, they can start searching for jobs and, with an employer's intention to hire them, apply for work permission at the local foreigners' office for a specific position and employer. 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.[7] 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 Germany, the requirement to apply for a specific work permission becomes obsolete and the asylum seeker can commence any work.[8]

After examination of the asylum case, the decision of the Federal Office for Migration and Refugees (*Bundesamt für Migration und Flüchtlinge*, BAMF) can have two outcomes. One is to recognize the need for protection in the form of granting asylum, refugee status, subsidiary protection, and prohibition of deportation (*Asylberechtigung*, *Flüchtlingsschutz*, *Subsidiärer Schutz*, and *Abschiebungsverbot*). In all four 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 permit (*Niederlassungserlaubnis*) after three to five years. The requirements for application include among others: independence from government benefits, intermediate proficiency in German, health insurance, accommodation, and a spotless criminal record.[9]

Alternatively, the BAMF rejects the asylum application. Then, the individual has to either leave the country (*Ausreisepflicht*) or apply for temporary tolerance (*Duldung*) that

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[5] In Germany, the word "refugee" is used both to describe people fleeing their home countries in general and as a specific legal status. If not stated otherwise, we refer to the general concept in this paper.

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

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

[8] According to Bundestag (2019), an asylum decision is reached after 13-14 months on average, but it can take much longer in complicated cases or if the asylum seeker appeals the decision in court.

[9] 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.

Figure 1: The asylum process in Germany
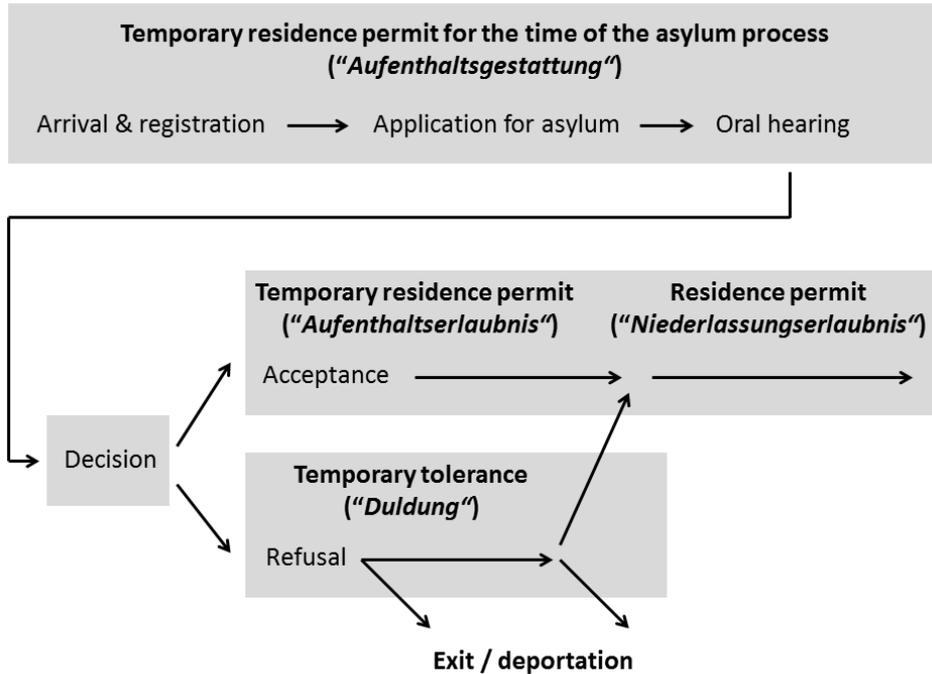
Note - Own illustration based on Statistisches Bundesamt (2020) and BAMF (2019).

lasts six months and can be renewed multiple times (BAMF, 2019; Sachverständigenrat deutscher Stiftungen für Integration und Migration, 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. Individuals who have lived in Germany with temporary tolerance for more than seven years can apply for a temporary residence permit and a permanent permit later, if they have acquired a certain state of integration, which is determined by the refugee's ability to earn their own living, their German proficiency, and cultural knowledge.

Summing up, refugees in Germany gain access to the labor market relatively early.[10] During the first three months after entering the country, and in case their asylum application is rejected and they have to leave the country, refugees are not allowed to work at all. In all other cases, they have the chance to take up employment either with an unrestricted or with an employer-specific work permit. Thus, formal obstacles should not hinder their labor market integration in theory. However, the exact conditions and regulations for

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[10] In comparison, the earliest possibility to work in France and the UK 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).

each legal status can be hard to understand, and actually starting a job requires to find an employer and to take a number of administrative steps with an outcome that may not be transparent for the refugee.

2.2 Welfare and asylum benefits system

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. Considering an adult refugee without children, the monthly benefits for an asylum seeker (*Asylbewerberleistungen*) in 2018, the year of our data collection, is 354 Euros in the first 15 months after the asylum application and 416 Euros afterwards (GGUA, 2018).[11] For a childless couple, the rates are 636 and 748 Euros, and for a family with two children under the age of six the rates amount to 1,064 and 1,228 Euros, respectively. In each case, the government additionally provides accommodation (mostly rooms in residences for asylum seekers, but also private apartments) and covers expenses for necessary medical treatment. The government also organizes and covers childcare, 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*, version from October 24, 2015).

The benefits for asylum seekers can 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, see AIDA, 2019b,a). At the same time, refugees face high benefit reduction rates of 75% when they start regular employment (GGUA, 2019). This lowers the incentives to work and results in a phenomenon known as the "welfare trap", which makes it hard for involved individuals to become independent of the welfare system (e.g., Blank, 2002; Blundell, 2001; Lemieux and Milligan, 2008).

Overall, this system provides incentives to pursue unregistered work because earnings from informal employment are not subject to benefit reduction, and therefore, directly increase the disposable income. This might be particularly relevant for refugees with low education, and those who lack the necessary formal certificates to work in their profession, because their formal wages tend to be relatively low.[12] However, it is also worth considering the non-monetary advantages of taking up regular employment that may counteract the financial disincentives built into the support system for refugees. First, working in a regular job is important for most individuals since it increases self-

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[11] 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.

[12] 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).

esteem and leads to higher social acceptance (see, e.g., Dooley and Prause, 1995; Herbst, 2013). Second, and probably more important, being financially independent is typically required to obtain a permanent residence permit and also helps on the rental market. These aspects might counteract the incentives of unregistered work to a certain extent.

2.3 Penalties for unregistered work

Besides legal alternatives and financial incentives to work without registration, Becker's framework features another determinant of the expected utility from illegal behavior: the probability of being detected and fined, and the severity of penalties (Becker, 1968). The higher 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 this behavior.

In Germany, the fight against illegal economic activity is spearheaded by the Financial Investigation Office for Clandestine Employment at the Federal Customs Administration which monitors companies and carries out unannounced control visits. If unregistered work is detected, the penalty can reach up to several thousand Euros and five years in jail.[13] 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 permit (see section 2.1). 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 low for years, mainly due to the substantial under-staffing at the Financial Investigation Office (see various reports in German, e.g., Bundesrechnungshof, 2008; Deutsche Handwerkszeitung, 2018; Tagesschau, 2020). Thus, there may be many opportunities to work without registration in sectors that are difficult to monitor, e.g., construction, gastronomy, and household services.

Summing up, several factors influence the probability to work without registration in Germany. (1) While asylum seekers are allowed to start looking for a job very early, 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. (2) The financial support for refugees should cover the basic need without (other) income, yet it may build a significant entry barrier to regular jobs and an incentive to accept unregistered work at the same time. And (3), although sentences for convicted illegal workers may be severe, the probability of

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[13] A number of laws and directives can be applied to punish 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).

detection is very low. Given these circumstances, we expect to find a significant amount of experience with unregistered work among refugees in Germany.

2.4 The official employment situation of refugees in Germany in 2018

Most refugees in Germany report a strong desire to work and to earn their living (Hartmann et al., 2018). Given the obstacles created by the institutional environment, and the fact that most refugees entered the country without knowledge of German, it is not surprising, that the labor market integration of the latest cohorts of refugees was moderate in 2018, even though the economy had expanded in the previous years and many employers reported problems to fill vacancies. More specifically, the nationally representative IAB-BAMF-SOEP refugee survey indicates that 21% of adult refugees who arrived between 2013 and 2016 had found a job by the second half of 2017, including apprenticeships and marginal employment (Brücker et al., 2019). At the time of our survey in spring and summer of 2018, this share had already increased to 26%. This indicates substantial progress over time.

In terms of job quality, 52% of employed refugees worked in jobs with skilled tasks in 2018, whereas 44% worked in low-paid assistant jobs which are often offered with temporary contracts and concentrated in low-wage sectors such as cleaning, gastronomy, and agriculture. Less than a quarter of employed refugees had permanent contracts (Hartmann et al., 2018). The average gross earnings of working refugees were comparatively low but steadily increasing (about 800 Euro in 2016 and 1,300 Euro in 2018). A refugee with a full-time job earned about 55% of the median wage of a full-time employee in Germany (Brücker et al., 2019). This indicates that the labor market integration of refugees was improving, but still rested on weak foundations.

3 Data collection

3.1 The ifm Refugee Survey 2018

For this study, we use 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 (BW), the third largest of Germany's federal states with over 10 million inhabitants. It was conducted between mid-April and mid-July 2018 to obtain a comprehensive overview of the state of labor market integration of refugees three years after the increased inflow of asylum seekers in 2015. 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 residents. For logistical reasons, we used a clustered sampling approach and selected two

to three districts from each of the state's four administrative regions. In each district, the interviews took place in almost all of the medium- to large-sized residences (20 to 200 inhabitants).[14]

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. To facilitate the contact with the residents, most interviewers are of Middle Eastern or African origin and thus spoke the mother tongue of a substantial part of our target group. The interviewers worked in mixed-gender teams of three to five persons, depending on the size of the targeted residence. They actively recruited participants by approaching individuals in public spaces, community rooms, and at the doors of the private rooms to explain the study objectives. Although we did not offer monetary incentives or in-kind gifts for participation, about half of the contacted people agreed to participate in the survey.

The questionnaire was available in the languages of the main countries of origin of asylum seekers: English, Arabic, and Persian. Combined, about 70-80% of refugees come from countries in which one of these three languages is an official language. The questions covered a whole range of items, starting with demographic information, the migration and asylum process, education and professional experience in the home country, as well as self-assessed skills and preferences. The core of the survey was a 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 vocational training. The interviews were conducted as computer-assisted personal interviews (CAPI) and lasted between 25 and 60 minutes.

3.2 The list experiment

The list experiment was implemented at the end of the section about labor market integration in Germany. Respondents were randomly assigned to a treatment and a control group. Individuals in the control group were shown a list of five non-sensitive items, in our case, standard experiences on the German labor market they may have had already, and were asked to answer the following question:

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[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 group and the police during that time.

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) I have found a job within 2 months of searching.

(4) I have found a job with the help of the local employment agency.

(5) I work/have worked in a job with requirements lower than my education.

Individuals in 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

The interviewers explained to the respondents that they did not have to provide an answer to any individual item, but only indicate the number of experiences already made. Thus, not even the interviewers could infer which of the items were experienced by the respondent. This provides a high degree of anonymity (see Droitcour et al., 1991; Wolter and Laier, 2014), which is important when asking sensitive questions in general, and may be crucial for refugees who might have low trust in German official institutions, and face the risk of losing their work or residence permit in case of illegal behavior. To strengthen this aspect, we instructed the interviewers to read out and explain the task, but then hand over the tablets to the respondents who typed in the corresponding number themselves. This added an extra layer of confidentiality and protection, as the interviewers did not see the respondents' answers. We provide screenshots of the list experiment in the survey in Figure A.1 a and b in Appendix A.

In setting up this design, we followed the recommendations in the literature by choosing control items that show thematic coherence to experiences on the labor market (Droitcour et al., 1991) and by introducing a negative correlation between items (Glynn, 2013), e.g., between items (1) or (2) and (3). Furthermore, none of the items should create a strong resonance which would lead to a bias called contrast effects (Glynn, 2013; Kuklinski et al., 1997). Finally, the whole approach was examined and approved by the ethics commission of the University of Freiburg (Approval EK 13/17).

3.3 Sample statistics

The questionnaire was completed by 1,259 respondents. In Table 1, we report mean values and standard deviations of individual characteristics for all respondents in the sample (columns 1 and 2). Women represent a quarter of the sample 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, and respondents went to school for 9.5 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 stayed in Germany with a temporary tolerance which must be renewed every six months). Given that we targeted refugee residences for the survey, hardly any respondents in our sample lived in private apartments. 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. Given the fact that refugees are almost randomly distributed across states and districts in Germany according to a national distribution quota based on tax income and population levels, the ifm Refugee sample should be relatively similar to the national population of refugees living in publicly provided residences.

We assess the representativeness of our sample to the population of refugees in Germany by reporting the corresponding summary statistics of the 2018 wave of the IAB-BAMF-SOEP refugee sample.[15] In columns 3-4, we show the weighted averages and standard deviations for refugees in the IAB-BAMF-SOEP sample who lived in the state of BW in 2018 (both in private housing and refugee residences). In columns 5-6, we additionally report the respective numbers for the whole of Germany. The ifm refugee sample is very similar to 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 mirrors the average years of education and the percentage of people who are still waiting for their asylum decision.

We find differences between our sample and the IAB-BAMF-SOEP sample which reflect our approach of recruiting study participants in refugee residences. Respondents in our sample spent less time in Germany, live under less stable legal conditions (37% vs. less than 10% of participants had a asylum application rejected), and are less advanced

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[15] 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 the Central Register of Foreigners in Germany (*Ausländerzentralregister*) as sampling frame. More information about the IAB-BAMF-SOEP refugee sample and how it can be accessed can be found at www.diw.de.

Table 1: Descriptives statistics of the ifm Refugee sample and representativeness

	ifm sample BW 2018		IAB-BAMF-SOEP BW 2018		IAB-BAMF-SOEP Germany 2018	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Personal characteristics						
Female	0.240	(0.427)	0.250	(0.433)	0.298	(0.457)
Age in years	31.4	(9.6)	31.0	(9.3)	31.8	(10.7)
Married/Partnership	0.435	(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	(3.1)	9.6	(2.8)	9.9	(3.1)
Country of origins						
Syria	0.225	(0.418)	0.407	(0.492)	0.426	(0.495)
Afghanistan	0.162	(0.369)	0.151	(0.358)	0.151	(0.358)
Iraq	0.140	(0.347)	0.096	(0.295)	0.102	(0.303)
Gambia	0.128	(0.334)	0.064	(0.245)	0.010	(0.100)
Nigeria	0.102	(0.303)	0.015	(0.121)	0.019	(0.136)
Iran	0.061	(0.240)	0.009	(0.097)	0.026	(0.160)
African countries	0.103	(0.304)	0.102	(0.302)	0.132	(0.339)
Other countries	0.079	(0.269)	0.156	(0.364)	0.134	(0.340)
Situation in Germany						
Years since arrival	2.4	(1.2)	3.0	(0.8)	3.0	(0.8)
Private apartment	0.031	(0.174)	0.625	(0.485)	0.743	(0.437)
Work in last 7 days	0.260	(0.439)	0.460	(0.499)	0.354	(0.478)
Asylum application						
Pending	0.203	(0.402)	0.252	(0.434)	0.156	(0.363)
Approved	0.422	(0.494)	0.675	(0.469)	0.750	(0.433)
Rejected	0.375	(0.484)	0.073	(0.260)	0.093	(0.291)
Num. of observations	1,259		457		4,184	
Weighted			71,636		669,183	

Note - BW is the abbreviation for Baden-Württemberg. 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.

in terms of their labor market integration (26% in our sample report some kind of work-related activity vs. 46% in BW and 35% in the German-wide sample). In line with these numbers, our sample contains less Syrians, but more individuals from Iraq, Gambia, Nigeria, and Iran. This is reasonable as the majority of Syrians entered Germany three years before the interview and almost automatically obtain refugee protection status, which means that they are less likely to still live in central residences. In contrast, individuals from Iraq and Iran have lower admission rates, and those from Gambia and Nigeria are hardly accepted at all (see BAMF, 2020). Obtaining private housing is therefore difficult for these groups.

These differences indicate that the ifm sample is not representative for the population of 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 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 Empirical strategy

4.1 Identifying assumptions

With the list experiment, we measure the share of respondents who experienced at least one episode of unregistered work since their arrival in Germany by comparing the answering behavior of respondents between treatment and control group. The identification of this parameter is based on three assumptions: (1) successful randomization of the treatment, (2) no design effects, and (3) the absence of liars (Blair and Imai, 2012). In this section, we discuss the plausibility and implications of these assumptions. To relate to the existing literature on list experiments, we use the notation of Imai (2011) and Blair and Imai (2012). Further details on the methodology are provided in Appendix C.

We denote T as the treatment indicator, with $t = 0$ for the control group and $t = 1$ for the treatment group. The total number of control items is indicated by J. In this study, individuals in the control group face a list of five items $J = 5$ with $j = 1, ..., 5$ whereas the members of the treatment group see the same list plus the sensitive item, thus $J + 1 = 6$. Furthermore, we denote the affirmation state of respondent i to each item j as binary indicator $Z_{ij}(t)$ for $j = 1, ..., 5$ and $t = 0, 1$. For example, $Z_{i3}(1) = 1$ therefore means that respondent i found a job in Germany within 2 months of searching (situation 3 of the experiment) given that he or she was part of the treatment group. In the case of the sensitive item, the experience of unregistered work, $Z_{i,J+1}(t)$ is only relevant for the

treatment group $Z_{i6}(1)$, as respondents in the control group do not face it in their list of experiences. $Z_{i,j}^*$ indicates the truthful answer of respondent i to the j-th item with $j = 1, ..., J + 1$. Finally, the recorded number in the experiment is the sum of items that were experienced by the respondents Y_i. It is $Y_i(0) = \sum_{j=1}^{J} Z_{ij}(0)$ in the control group and $Y_i(1) = \sum_{j=1}^{J+1} Z_{ij}(1)$ in the treatment group.

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

$$\{\{Z_{ij}(0), Z_{ij}(1)\}_{j=1}^{J}, Z_{i,J+1}(1)\} \perp T_i. \tag{1}$$

Assumption (1) implies that potential and truthful responses to the items of the list experiment are jointly independent of the treatment assignment. This means that the respondents in treatment and control group should be very similar to each other in all aspects, including their experiences with the sensitive and non-sensitive situations in our list experiment. Then, we can treat them as reasonable counterfactuals for each other. In our case, assumption (1) should hold because the survey software "harvestyourdata" randomly allocated the respondents to the two experimental groups. We assess if randomization was successful by comparing the characteristics of respondents in the treatment and the control group in Table B.1. The richness of data collected in the ifm Refugee survey allows us to consider personal and family characteristics, education and skill variables, information on the labor market integration, legal aspects and regional information in the balancing tests. The results show that the groups are very similar with the regional distribution as the only significant difference among all considered characteristics.[16] Additionally, a test for joint significance of respondent characteristics with respect to predicting treatment assignment leads to insignificant results (see p-value: 0.493 in Table B.1). All these indicators suggest that randomization was successful.

Assumption (2) *No design effect.* For all respondents $i = 1, ..., N$, including the sensitive item does not change the response behavior to the control items,

$$\sum_{j=1}^{J} Z_{ij}(0) = \sum_{j=1}^{J} Z_{ij}(1). \tag{2}$$

If assumption (2) is violated and respondents change 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 actual differences in the

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[16] We find a significantly higher share of individuals in the treatment group in the district of Freiburg and a significantly lower share in the district of Karlsruhe.

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). To examine whether this assumption holds here, we apply the test developed by Blair and Imai (2012) to detect violations of the no-design-effect assumption and do not find evidence for a design effect in our data.[17] We therefore consider assumption (2) fulfilled in our application.

Assumption (3) *No liars.* For all respondents $i = 1, ..., N$, the answer to the sensitive item represents a truthful response,

$$Z_{i,J+1}(1) = Z^*_{i,J+1}. \tag{3}$$

Assumption (3) is violated if respondents fear for their anonymity because they would have to indicate that they experienced all or none of the non-sensitive items when answering honestly (Kuklinski et al., 1997). *Ceiling effects* occur when respondents experienced all non-sensitive items and answer with count J instead of $J + 1$, because agreeing with all statements would identify them as having experienced the sensitive situation. *Floor effects* result out of a similar reaction. Respondents who have not experienced any of the control items but only experienced the sensitive item, might believe that honestly answering with '1' could reveal too much about their work behavior and therefore indicate zero. Both types of violations lead to an underestimation of the parameter of interest.

In our setting, any bias resulting from ceiling effects should be minimal since most refugees had limited labor market experience in Germany at the time of the experiment. This is reflected in a very low proportion of respondents in the control group who affirm all non-sensitive items (see, Table 2). However, a high proportion (about 62%) of respondents answer with count '0' which indicates that assumption (3) is violated by floor effects. We discuss how we implement a procedure to model floor effects and to correct the resulting bias in the next section.

4.2 Adjusting for floor effects

In the presence of floor effects, one option would be to derive bounds on the true share of respondents who experienced unregistered work (Blair and Imai, 2012; Manski, 2007). This approach is easy to implement and does not require any additional assumptions. We report the bounds in the results section of the paper, and show details on their calculation in Appendix C.4. However, the range between the lower and upper bound is wide, and the estimation of bounds cannot be incorporated in a multivariate analysis. Therefore, Blair and Imai (2012) propose an alternative approach of directly modelling floor effects. They assume that the group of liars consists of two types of respondents - those who answer

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[17] Appendix C.2 provides more information about this procedure and reports the results.

dishonestly due to general privacy concerns, and those who base the lying decision on their answers to the control items (which leads to ceiling and/or floor effects). In order to distinguish these liars and to directly model floor effects, it is necessary to impose an additional assumption.

Assumption (4) *Conditional independence.* For all $y = 1, ..., J$, the respondents' truthful answer to the sensitive item is independent of their answers to the control items conditional on pretreatment covariates $X_i = x$,

$$Pr(Y_i(0) = y | Z^*_{i,J+1} = 1, X_i = x) = Pr(Y_i(0) = y | Z^*_{i,J+1} = 0, X_i = x). \qquad (4)$$

Assumption (4) is similar to the Conditional Independence Assumption in observational studies. We assume that we can control for the covariates that explain the systematic differences between the two types of liars. Following the recommendation of Blair and Imai (2012), we condition on pretreatment covariates that have high predictive power in explaining respondents' answering behavior to the control items. For example, "activity on the labor market" might be an important control variable because it could explain the differences in lying behavior. Subjects without experience at the formal labor market may answer dishonestly because their responses to the control items are zero. Respondents with experience on the formal labor market might answer dishonestly independent of their responses to the control items and due to general privacy concerns. Since the control items in our experiment largely focus on experiences during job search, we use an indicator for gender, indicators of vocational degree, and an indicator for current or already successful job search as additional pre-treatment covariates to capture respondents activity on the labor market when we implement the correction procedure.

4.3   Identification and estimation

Under assumptions (1) to (3), the proportion of refugees who experienced unregistered work can be identified from the difference between the count averages of the two comparison groups. We can easily estimate this proportion using a difference-in-means (DiM) estimator of the form

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

where N_1 and N_0 represent the number of observations in the treatment and control group, and $\hat{\tau}_{DiM}$ 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 start by running linear regressions (OLS)

without covariates. The coefficient from this regression is numerically equivalent to the DiM estimator. We include regional indicators as controls to account for the regional imbalances between the treatment and control groups.

Under assumptions (1) to (3), the joint distribution of respondent types $\tau_{yz} = Pr(Y_i(0) = y, Z^*_{i,J+1} = z)$ with $z = 0, 1$ is identified from the observed data. This allows us to estimate the proportion of refugees who experienced unregistered work more efficiently. Details on the identification of the joint distribution are presented in Appendix C.1. In order to use the information from the joint distribution, we follow Blair and Imai (2012) who use two binomial regression models to estimate the joint distribution of respondent types,

$$g(x, \delta) = Pr(Z^*_{i,J+1}(1) = 1 | X_i = x)$$

$$h_z(y; x, \psi_z) = Pr(Y_i(0) | Z^*_{i,J+1} = z, X_i = x)$$

with $x \in X$, $y = 0, ..., 5$, and $z = 0, 1$. Both models allow the incorporation of control variables which is important to conduct multivariate analysis and to identify correlations of the sensitive item and respondents' characteristics. $g(x, \delta)$ is the model for the conditional expectations of the sensitive item given X_i, and $h_z(y; x, \psi_z)$ is the model for the conditional expectations of the control items given a set of covariates X_i. We apply a Maximum-Likelihood estimation (MLE) as proposed by Imai (2011) and Blair and Imai (2012). Details on the likelihood function are presented in C.3.

Under assumptions (1) to (4), the joint distribution of respondent types τ_{yz} is identified even if floor effects exist. Following the Blair and Imai (2012) correction procedure to correct for floor effects, we adjust the likelihood function for the probability of incorrect reporting and estimate the resulting model with MLE (see Appendix C.4 for details). 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   Results

5.1   Experience with unregistered work

We start with a descriptive summary of the responses to the list experiment. In Table 2, we show the non-response rates as well as absolute and relative frequencies of the total counts for each experimental group. Three observations are noteworthy: First, the low percentage of non-responses (6% and 5% in the control and treatment group) indicates that the way the question was asked and administered did not lead to a large fraction of respondents unable or unwilling to answer. Second, we observe similar distributions of

answers in both groups, each strongly skewed to the right with almost 60% of respondents reporting count '0' as their answer. This distribution indicates the possibility of sizeable floor effects. And third, the mean number of reported items amounts to 0.547 in the control group and to 0.669 in the treatment group. Thus, a simple difference in means estimator yields a share of 12% of refugees who experienced unregistered work in Germany.

Table 2: Summary of responses to the list experiment

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

Table 3 depicts the result of the DiM estimator and the corresponding standard error in column 1. In column 2, we add regional fixed effects to control for the imbalanced distribution to the comparison groups over the four regions of the state. Both estimates are almost identical and statistically significant at the 5% significance level, suggesting that unregistered work has been experienced by a sizeable share of refugees (around 12%). In comparison, the list experiment of Kirchner et al. (2013) reveals that only 6.4% of the German population worked without registration in 2010. In column 3, we estimate the binomial logistic models with MLE as proposed by Imai (2011). The outcome is very similar with a share of 11% of respondents with experience of unregistered work. The smaller standard error shows that MLE leads to a higher precision in the estimates as it takes the joint distribution of counts into consideration rather than only the means.

In the presence of floor effects, the estimates in column 1-3 measure the lower bound of the share of refugees who experienced unregistered work since their arrival in Germany. The bounds imply that the true proportion of refugees who experienced unregistered work varies between 12% and 74% which is a large range and not very informative (for details on the calculation see Appendix C.4). In column 4, we account for the violation of the *No-liar* assumption caused by floor effects by applying the Blair and Imai (2012) correction approach. The share of respondents with unregistered work experience increases to 31% suggesting that every third refugee has already worked without registration since arriving in Germany. Compared to the 41% of respondents who reported having a job at the

Table 3: Results of the list experiment

	(1) DiM	(2) OLS	(3) MLE	(4) MLE
Share experienced unregistered work	0.122** (0.058)	0.118** (0.058)	0.113*** (0.025)	0.311*** (0.011)
Num. of observations	1185	1185	1185	1185
Regional controls	no	yes	yes	yes
Adjust for floor effects	no	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 without covariates which is equivalent to difference-in-means (DiM). In column (2), we control for regional imbalances. In columns (3), we implement the binary logistic regression estimated by Maximum-Likelihood estimation (MLE). In column (4), we additionally adjust for floor effects. Heteroskedasticity-consistent robust standard errors are presented in parentheses.

time of the interview or at some point in time before in Germany, this suggests that the experience with unregistered work is roughly three fourths as common as the experience with registered employment.

The size of the estimate appears large but plausible. In Section 2, we discuss the institutional and structural barriers that complicate an easy integration into the regular labor market. In particular, the complexity of the asylum process and status-specific work permits may be difficult to understand and the benefit system for refugees provides little financial incentive to start registered employment. Furthermore, the list experiment asked respondents for their overall cumulative experiences on the German labor market since their arrival instead of specifying a particular calendar year. Thus, the experiences with unregistered work could be spread out over a multi-year period. Our estimate also closely matches the assumed parameter of 25% of refugees working without registration in a simulation study on the changes of the shadow economy due to the refugee inflow in 2015 (Schneider and Boockmann, 2016). This indicates that we are in a ballpark that is generally considered very realistic.

5.2 Subgroup analysis

In the next step, we perform a subgroup analysis. We split the sample along different dimensions and report the resulting subgroup-specific shares of experience with unregistered work in Figure 2. The results display a wide range of that experience starting from 14% for refugees who reported having worked within the last seven days before the interview up to 43% for female respondents. Other groups with significantly higher than average shares of experience with unregistered work include refugees with approved asy-

lum, refugees who report to lack work permission, and those who worked in their home countries in occupations that are prone to unregistered work in Germany, e.g., construction, gastronomy, and household services. Individuals who had not worked in the last seven days as well as those without college degree or vocational training also show higher than average rates of experience with informal employment. On the contrary, being male, having good or very good German proficiency, being vocationally trained and possessing a college degree is associated with less experiences of unregistered work.

Figure 2: Share of respondents with unregistered work experience by subgroups
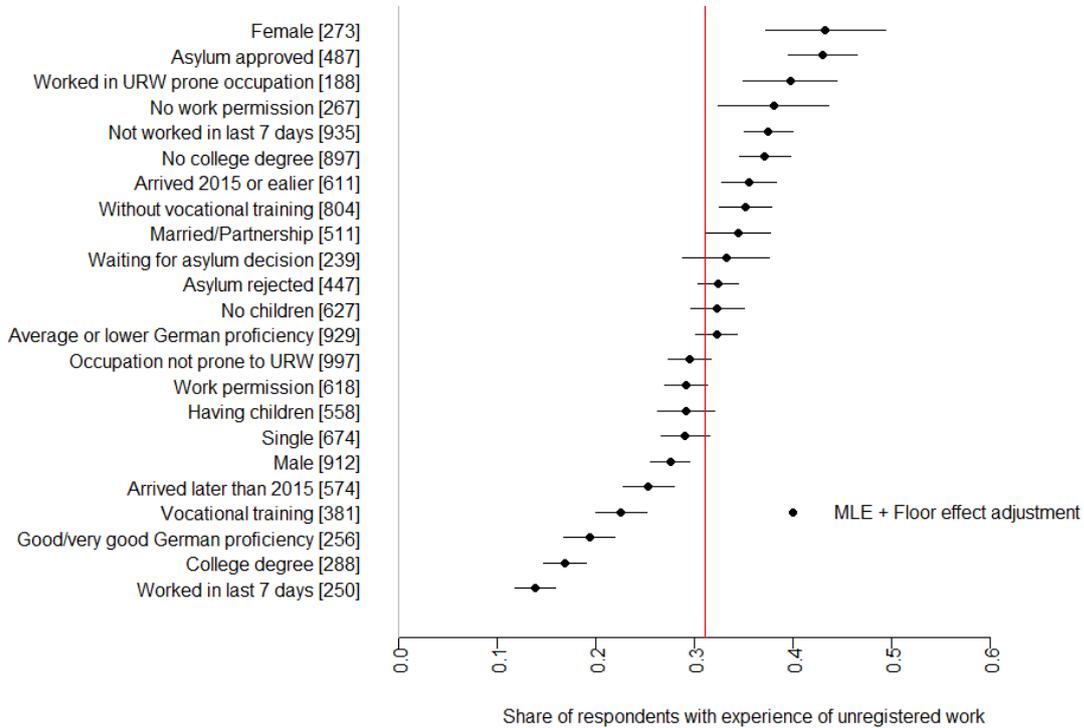
Note - The dots show the shares of respondents with unregistered work experience estimated by Maximum-Likelihood estimation (MLE) with adjustment for floor effects. The solid lines represent the 95% confidence intervals. The vertical line indicates the share of unregistered work experience in the overall sample (31%, see Table 3). The number of observations in the different subgroups is shown in the square brackets. We consider gastronomy, construction, sales, and household services as occupations prone to unregistered work (URW). ***, **, * denote statistical significance at the 1/5/10 percent level.

As a plausibility check of our interpretation of the share of unregistered work experience as cumulative measure since arrival in Germany, we also split the sample of respondents into refugees who immigrated in 2015 or earlier and those who arrived later. We find that 36% of the individuals who spent more time in Germany at the time of the interview have experienced unregistered work, whereas this share amounts to 25% for later arrivals. This gives us confidence that the applied method leads to internally consistent estimates.

The subgroup analysis is also informative for the assessment of the external validity

of our findings. Respondents in our ifm Refugee sample spent less time in Germany on average. As shown in Table 1, respondents in our sample were less likely to have received a positive decision on their asylum application, and more likely to be rejected than the total refugee population. The findings for these subgroups suggest that the rate of experience with unregistered work would be higher in the population (see Figure 2). At the same time, we observe more individuals in formal employment in the population compared to our sample. This suggests a bias in the opposite direction, as respondents who report to work are the subgroup with the lowest share of experience with the sensitive item.[18]

5.3 Multivariate analysis

While Figure 2 reveals important subgroup differences in the experience of unregistered work, it is likely that some characteristics are correlated with each other. For instance, males report to possess a work permission significantly more often than women (80% vs. 69%) and are at the same time more often single (60% vs. 27%). We implement a multivariate regression analysis to account for possible correlations between these individual factors. List experiments do not allow to identify causal relationships between respondents' characteristics and the sensitive item, thus, we cannot identify which characteristics cause unregistered work. However, we can provide descriptive evidence on which characteristics are significantly correlated with the sensitive item and can be identified as predictive risk factors of experiencing unregistered work. The results are reported in Table 4.

Following our empirical strategy, Table 4 reports the estimates of binomial logistic models with adjustment for floor effects. In accordance with the literature and because the regression results for the model of the control items are not of interest in list experiments, we focus on the results for the sensitive item $g(x, \delta)$.[19] We start by controlling for gender, family status, and the presence of children (column 1). Then, we add information on the time of arrival in Germany and whether respondents report to possess a work permission (column 2), and finally also qualification characteristics, such as language proficiency and degrees (column 3). We do not focus on the asylum state as predictive risk factor as the status is subject to changes over time. However, we include the asylum status at the time of the interview, and indicators for regions and countries of origin as additional control variables.

All estimates mirror the pattern of the subgroup analysis in terms of the signs of the

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[18] Our sample also differs from the population of refugees in Germany in terms of the composition of the countries of origins. Specifically, we observe fewer Syrians and more individuals from African countries in our sample. As there is no reason to believe that origin itself is related to unregistered work and due to ethical considerations, we decided not to report subgroup results by country of origin.

[19] The complete results for both binary logistic models are displayed in Appendix D

Table 4: Multivariate analysis

	(1) MLE		(2) MLE		(3) MLE	
Female	0.641	(0.593)	0.667	(0.720)	0.685	(0.751)
Married/Partnership	0.609	(0.704)	0.769	(0.715)	0.674	(0.749)
No kids	0.676	(0.774)	0.622	(0.689)	0.665	(0.790)
Arrival later than 2015			-0.682	(0.602)	-1.124*	(0.614)
No work permission			1.281	(1.039)	1.212*	(0.701)
No vocational/college degree					0.944*	(0.488)
Low German proficiency					1.095*	(0.625)
URW prone occupation					0.668	(0.617)
Intercept	-2.027	(1.096)	-2.062	(1.075)	-3.478	(1.296)
Num. of observations	1,185		1,185		1,185	
Adjust for floor effects	Yes		Yes		Yes	
Additional controls	Yes		Yes		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, and 0, 1, 2, 3, 4, 5, or 6 for respondents assigned to the treatment group. We estimate binary logistic regression models by Maximum-Likelihood estimation (MLE) and adjustment for floor effects. We additionally control for regional imbalances, indicators of the countries of origin, asylum status and labor market activity at the time of the interview. The coefficients are reported in log-odds and can be transformed into a probability using $p(x) = 1/(1 + e^{-x})$. Heteroskedasticity-consistent robust standard errors are presented in parentheses. We consider gastronomy, construction, sales, and household services as occupations prone to unregistered work (URW). ***, **, * denote statistical significance at the 1/5/10 percent level.

relationships. We identify significant predictive risk factors of experiencing unregistered work in the full model in column 3. We find that while holding the other characteristics constant, respondents without work permission are significantly more likely to have worked without registration. Furthermore, respondents without vocational training or college degree as well as respondents whose German language proficiency was rated as average or below have a significant higher likelihood of experiencing unregistered work when we hold other respondent characteristics constant.

These findings fit well in the overall picture of factors that affect the labor market integration of refugees. Granting early labor market access is seen as an important policy to enable refugees to work, prevent the depreciation of human capital, and reduce the dependence on welfare (Brell et al., 2020). In theory, all refugees are allowed to search for employment starting with the fourth months after they formally applied for asylum in Germany (see Section 2.1). While refugees with a pending asylum decision and those rejected under temporary tolerance can apply for employer-specific work permission, those with a positive asylum decision are allowed to work without restriction. Yet 23% of our total sample reported not to be allowed to work. Among those respondents with approved temporary residence permission this number is 14%. It is possible that their perception of not having legal access to the labor market is shaped by negative experiences

or misunderstanding about their right to work.

Recent literature highlights the importance of foreign occupational degrees and their formal recognition on the labor market success of migrants (Brücker et al., 2021). A lack of vocational or post-secondary degrees complicates the access to primary segments of the labor market and restricts work opportunities of migrants to low-paying, precarious, and informal jobs. Likewise, there is a strong consensus in the literature about the importance of language knowledge for migrants' job finding probabilities and their overall labor market success (e.g., Brell et al., 2020; Auer, 2018; Dustmann and Fabbri, 2003). Fasani et al. (2021) and Bloch (2008) even claim that low language proficiency is the main obstacle to employability in Europe and the UK. It is therefore not surprising that people with low German proficiency may end up in unregistered work in higher numbers.

5.4 Self-reported reasons for unregistered work

After the list experiment, we directly asked all survey participants about possible reasons and explanations for why refugees might experience unregistered work in Germany. 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, both for the employer and refugee side. For each of these two sides, we then asked the respondents to choose one or more likely motivations from a predefined list and additionally gave them the option to provide as many further reasons as they wanted. Figure A.1 c and d in Appendix A provides screenshots of these direct questions.

Panel (a) of Figure 3 indicates that survey participants deemed the financial motives of employers as more relevant than non-monetary reasons for not registering their refugee employees. About 70% mentioned 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%. 5% of respondents (n=30) provided alternative reasons. Among those, 39% mentioned a lack of qualification, no language knowledge, and missing work permits or documents. Furthermore, 18% state that employers hire refugees without registration to help them earn a living.

A similar pattern emerges in Panel (b), where we directly asked for potential reasons of refugees to work without registration in Germany. Again, 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. Furthermore, 30% of the respondents indicated a missing work permission

Figure 3: Possible reasons for unregistered work from the perspective of refugees

(a) Employers
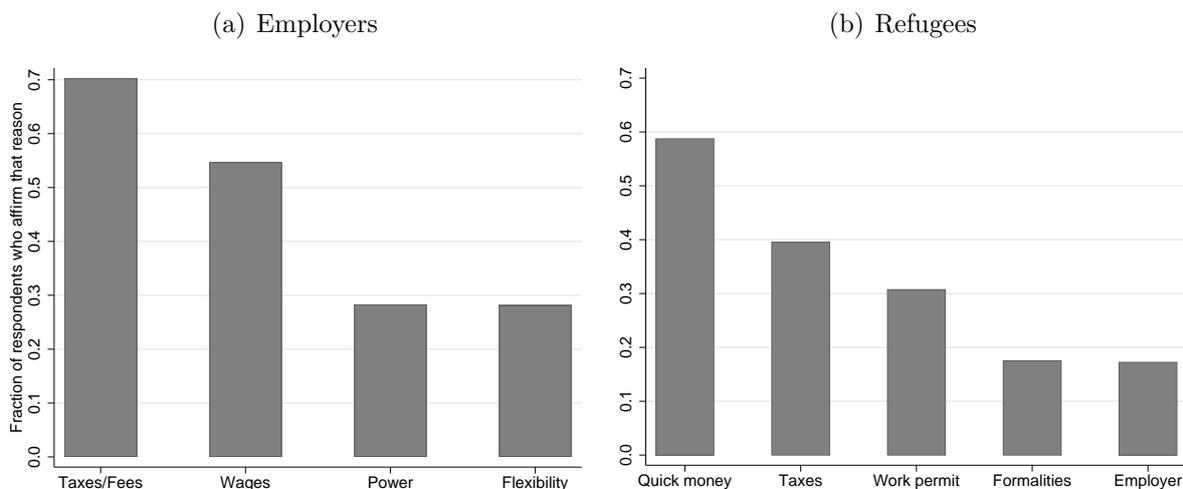
(b) Refugees

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 name as many possible reasons as they wanted.

as potential reason, 17% believed that the employer refuses to register the refugee, and 17% thought that refugees want to avoid paperwork and formalities. 9% (n=60) provided other explanations, such as the need to earn money to support families (35%), missing proficiency of the German language (22%), no chance to find better work (18%), and missing knowledge about laws and rules in Germany (12%). 36% of respondents did not know what to answer or were not willing to provide an answer to this question.

The answers to the direct questions about possible motives for working or hiring without registration suggest that monetary reasons for unregistered work dominate non-monetary ones in the eyes of respondents. Furthermore, the answers support the findings from the empirical analysis. A low German proficiency, a missing work permit, and a lack of qualification are mentioned as reasons for the occurrence of unregistered work.

6   Discussion and conclusion

In this paper, we study the prevalence of unregistered work among refugees in Germany. Using a list experiment in a survey among more than 1,200 refugees living in centralized residences in 2018, we find that almost one out of three refugees has worked without registration since having arrived 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 before (41%). Groups that are particularly vulnerable to experience unregistered employment are female refugees, refugees who worked in their home countries in an occupation that is prone to

illegal employment in Germany, those who report not being allowed to work, and those who have not completed a vocational training or college degree. By implementing a multivariate analysis to account for correlations between different respondent characteristics, we identify the perception of not being allowed to work, a lack of post-secondary educational degrees, and an average or lower proficiency of the German language as predictive risk factors.

Given the negative consequences of unregistered work, it is important to design and implement informed countermeasures. Our findings point towards three areas in which concrete measures may help to reduce the prevalence of informal employment relationships. First, it is important to reduce misperceptions about work permits for refugees. Authorities should provide targeted information about the process of obtaining a work permission and how requirements change with asylum states. It is important that refugees understand that the administrative steps to obtain a job-specific work permission if the asylum application is not granted or pending are installed to maintain labor, health, and wage standards and to prevent exploitative working conditions. At the same time, we believe that streamlining and speeding up the process may be very helpful for the acceptance of the system, as the affected individuals may not understand why they are kept back for weeks when they could work already. Second, as unregistered work is defined and treated differently in different countries, authorities should inform refugees clearly and at an early stage about unregistered work in Germany and how being caught affects future decisions on the asylum case, extensions of a temporary tolerance, or an application for permanent residence status. Third, given the consensus in the literature about the importance of language proficiency and qualification as determinants of integration in the host country's labor market and our finding about predictive risk factors of experiencing unregistered work, policy makers should incentivize and support investments in language and human capital acquisition, for example through courses specifically tailored to this group, more support in mastering the complicated procedures for the recognition of foreign degrees, and more liberal naturalization policies.

We acknowledge that the results of our study come with certain limitations. To start, we do not have a random sample of refugees which limits the external validity. Our sample is restricted to refugees who live in centralized public residences at the time of the interview. It is not representative for the population of refugees regarding certain characteristics. On the one hand, some groups of refugees who are under-represented in our sample (e.g., refugees with approved asylum) appear to having more experience with unregistered work. On the other hand, the general population of refugees seems stronger attached to the regular labor market at the time of the survey, which leads to a bias in the opposite direction.

Another limitation comes from the existence of floor effects. Our selection of non-sensitive items led to a majority of respondents indicating zero experiences in the list experiment. We solved this problem with the adjustment procedure proposed by Blair and Imai (2012) which required to impose a non-testable identifying assumption. Finally, in our study, we formulated the sensitive item as having had experience with unregistered work at any time since arrival in Germany. Therefore, our estimate of interest is defined as the rate of accumulated experience with unregistered work. This is interesting but does not provide insight on the prevalence rate in a well-defined period of time, which may be more relevant to policy-makers, especially when it comes to developments over time or the comparison to natives or other migrant groups.

Given the importance of the topic and the potential of list experiments to contribute 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 or other migrant cohorts in the same experiment to define a benchmark in the whole population. Additionally, list experiments could be used in randomized control trials to examine the effectiveness of measures to combat unregistered work. 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 among refugees.

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Appendix - for online publication

A   Implementation of the list experiment

Figure A.1: Screenshots of list experiment

(a) Items control group

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If you think about your experiences in the German labor market so far: How many of the following situations have you experienced?

•I have/had difficulties to get my academic degree recognized.

•I have/had difficulties to get my professional education recognized.

•I have found a job within 2 months of searching.

•I have found a job with the help of the local employment agency.

•I work/have worked in a job which is lower than my education.

0-5 Your Answer

(b) Items treatment group

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If you think about your experiences in the German labor market so far: How many of the following situations have you experienced?

•I have/had difficulties to get my academic degree recognized.

•I have/had difficulties to get my professional education recognized.

•I have found a job within 2 months of searching.

•I have found a job with the help of the local employment agency.

•I work/have worked in a job which is lower than my education.

•I work/have worked in a job in which I was not officially registered.

0-6 Your Answer

(c) Direct question on employer motivation

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Some employers in Germany hire refugees, but do not officially register them as workers. In your opinion, what are the main reasons for this?
[WAIT FOR ANSWER AND CHECK ALL OPTIONS THAT APPLY]

☐ TO BE MORE FLEXIBLE TO HIRE AND FIRE WORKERS

☐ TO SAVE TAXES AND SOCIAL SECURITY CONTRIBUTIONS

☐ TO BE ABLE TO PAY LOWER WAGES / SHIRK ON WAGES

☐ TO HAVE GREATER POWER OVER THE WORKER

☐ SOMETHING ELSE

☐ I DON'T KNOW / I DON'T WANT TO ANSWER

(d) Direct question on refugee motivation

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What do you think are the main reasons for refugees to work without official registration?

[WAIT FOR ANSWER AND CHECK ALL OPTIONS THAT APPLY]

☐ TO AVOID THE NECESSARY FORMALITIES / PAPERWORK

☐ TO SAVE TAXES / SOCIAL SECURITY CONTRIBUTIONS / AVOID CUTS IN BENEFITS

☐ BECAUSE THEY ARE NOT OFFICIALLY ALLOWED TO WORK

☐ TO EARN MONEY AS QUICKLY AS POSSIBLE

☐ BECAUSE THE EMPLOYERS DO NOT OFFER OFFICIALLY REGISTERED JOBS

☐ SOMETHING ELSE

☐ I DON'T KNOW / I DON'T WANT TO ANSWER

Note - Screenshots from the list experiment implemented with software "Harvestyourdata".

B   Balancing of sample characteristics

In order to test whether the random allocation of participants to control and treatment group worked well, we examine whether the individuals in both groups are similar on average over a large number of observed characteristics. To this end, Table B.1 first reports the mean values of the whole sample in column 1, and the respective means for control and treatment group in columns 2 and 3. Column 4 displays the corresponding p-values of a simple t-test for statistical difference between the two groups.

Table B.1: Balancing of sample characteristics

	(1)	(2)	(3)	(4)
	All	Control group	Treatment group	p-value diff.
Personal characteristics				
Female	0.240	0.248	0.232	0.493
Age in years	31.4	31.3	31.4	0.912
Country of origin				
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
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
No vocational/college degree	0.521	0.502	0.540	0.178
No problems in reading	0.616	0.614	0.617	0.897
Average or below German proficiency	0.782	0.797	0.767	0.189
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
Search with support by				
Local employment office	0.215	0.214	0.217	0.879
Job center	0.340	0.334	0.346	0.673

< table continues on next page >

	(1)	(2)	(3)	(4)
	All	Control group	Treatment group	p-value diff.
Migration characteristics				
Arrival 2015 or earlier	0.490	0.491	0.489	0.926
Legal status				
No decision yet	0.203	0.198	0.207	0.671
Accepted	0.422	0.437	0.407	0.278
Rejected	0.375	0.365	0.386	0.450
Status of Rejection				
Temporary tolerance	0.237	0.220	0.254	0.153
Asked to leave Germany	0.103	0.108	0.098	0.551
Regional characteristics				
District of residence				
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
Joint significance	$\chi^2(30) = 29.042$, p-value $= 0.493$			
Num. of observations	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.

Almost all p-values are larger than 0.1, indicating that the two groups are very similar in their characteristics. The only exception is the regional distribution, where the treatment group is under-represented in the region of Karlsruhe and over-represented in the Freiburg area. We therefore explicitly control for the regional distribution in the empirical analysis. The p-value from a joint test of the hypothesis that assignment to the treatment and control group is unrelated to all of these observed characteristics is 0.493. This suggests that random assignment was successful and the two groups are credible counterfactuals for each other.

C   Methodological details

C.1   Identification of the joint distribution of answers

In this section, we describe details on the joint distribution of answers in more details. 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 C.1. For example, type $(Y_i(0), Z_{i,j+1}^*) = (3, 1)$ is a respondent who 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 C.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

$$\tau_{y1} = Pr(Y_i \leq y | T_i = 0) - Pr(Y_i \leq y | T_i = 1)$$

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

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,

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

For respondent type (3,0), we 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,

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

C.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

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

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 $y = 0, ..., J-1$. 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, 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 $y = 1, ..., J$. 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 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 C.2: Estimated proportions of respondent types $\hat{\tau}_{yz}$

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

Note - $\hat{\tau}_{yz}$ 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 or respondents for which none of the control items is true and who never experienced unregistered work $\tau_{00} = Pr(0, 0)$ 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 C.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 τ_{40} and τ_{50} 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* cannot 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.

C.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 constructs 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

$$L_{obs}(\psi_0, \psi_1, \gamma; \{Y_i, T_i, X_i\}) = \underbrace{\prod_{i \in \mathcal{J}(1,0)} (1 - g(X_i, \delta)) h_0(0; X_i, \psi_0)}_{\text{Group 1}} \times$$

$$\underbrace{\prod_{i \in \mathcal{J}(1,6)} g(X_i, \delta) h_1(5; X_i, \psi_1)}_{\text{Group 2}} \times$$

$$\underbrace{\prod_{y=0}^{J} \prod_{i \in \mathcal{J}(0,y)} \{g(X_i, \delta) h_1(y; X_i, \psi_1)(1 - g(X_i, \delta)) h_0(y; X_i, \psi_0)\}}_{\text{Group 3}} \times$$

$$\underbrace{\prod_{y=1}^{J} \prod_{i \in \mathcal{J}(1,y)} \{g(X_i, \delta) h_1(y - 1; X_i, \psi_1)(1 - g(X_i, \delta)) h_0(y; X_i, \psi_0)\}}_{\text{Group 4}}$$

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.

C.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 C.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.[20] First, they define the conditional probability of lying as

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

Table C.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 $\tau_{01} = (0,1)$. 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).

First Blair and Imai (2012) show how to bound the true proportion of respondent type τ^*_{01} 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 τ_{01} is the true proportion. In our application the lower bound is $\tau_{01} = 0.041$. 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

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[20] 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.

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. Furthermore, sharp bounds cannot be implemented in a 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.

By imposing the additional assumption (4), it is possible 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 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. 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 implemented in the R package 'list' Blair et al. (2014).

D Additional results

Table D.4: Models for control items and sensitive item

	(1) MLE		(2) MLE		(3) MLE	
Model for Sensitive item $g(x,\delta)$						
Female	0.641	(0.593)	0.667	(0.720)	0.685	(0.751)
Married/Partnership	0.609	(0.704)	0.769	(0.715)	0.674	(0.749)
No kids	0.676	(0.774)	0.622	(0.689)	0.665	(0.790)
Arrival later than 2015			-0.682	(0.602)	-1.124*	(0.614)
No work permission			1.281	(1.039)	1.212*	(0.701)
No voc./college degree					0.944*	(0.488)
Low German proficiency					1.095*	(0.625)
URW prone occupation					0.668	(0.617)
Intercept	-2.027	(1.096)	-2.062	(1.075)	-3.478	(1.296)
Model for Control items $h_z(y;x,\psi_z)$						
Female	-0.744***	(0.205)	0.747***	(0.156)	-0.678***	(0.147)
Married/Partnership	-0.325***	(0.144)	-0.351***	(0.135)	-0.253**	(0.133)
No kids	0.061	(0.134)	0.045	(0.130)	0.071	(0.129)
Arrival later than 2015			-0.060	(0.094)	-0.034***	(0.093)
No work permission			-0.706***	(0.162)	-0.605***	(0.139)
No voc./college degree					-0.570***	(0.093)
Low German proficiency					-0.354***	(0.100)
URW prone occupation					0.129	(0.113)
Intercept	-2.524	(0.205)	-2.201	(0.217)	-1.696	(0.230)
Num. of observations	1,185		1,185		1,185	
Adjust for floor effects	Yes		Yes		Yes	
Additional controls	Yes		Yes		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, and 0, 1, 2, 3, 4, 5, or 6 for respondents assigned to the treatment group. We estimate binary logistic regression models by Maximum-Likelihood estimation (MLE) and adjustment for floor effects. We additionally control for regional imbalances, indicators of the countries of origin, asylum status and labor market activity at the time of the interview. The coefficients are reported in log-odds and can be transformed into a probability using $p(x) = 1/(1 + e^{-x})$. Heteroskedasticity-consistent robust standard errors are presented in parentheses. We consider gastronomy, construction, sales, and household services as occupations prone to unregistered work (URW). ***, **, * denote statistical significance at the 1/5/10 percent level.