Empowerment on the move? An experiment on supporting forcibly displaced people in Greece *

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Abstract:

Legal empowerment could help forcibly displaced people who face high levels of violence and exploitation and few incentives to report. What is the demand for legal empowerment amongst forcibly displaced people? Does legal information lead to changes in well-being? We study legal empowerment through a randomized field experiment with 1,707 refugees and asylum seekers in Greece. We use an encouragement design to understand both variation in information-seeking behavior and the impact of information. At baseline, nearly half of the study participants were unaware of how to seek help after experiencing violence. Comparing generic (website-based) and personalized (WhatsApp chat with a caseworker) legal information against a control, we find more demand for generic than personalized legal information. Both treatments improved participants' knowledge of exploitation under Greek law (by 0.23–0.7 SD) and increased confidence in responding to violence (by 0.26–0.57 SD) three months after treatment, but complier average treatment effects are larger for personalized than for generic treatments. Impacts on other outcomes were limited. We identify a trade-off between the higher uptake of generic information and the more effective personalized conversations, advancing our understanding of the demand for legal empowerment and suggesting actionable strategies for supporting forcibly displaced people.

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Exploitation and violence negatively impact immigrants' well-being and diminish the economic returns to immigration (Dancygier and Laitin, 2014). The International Labor Organization estimates that 28 million people experienced exploitation in 2022 and that immigrants were more than three times more likely to be in forced labour than other workers (ILO, 2022). Forcibly displaced people are particularly at risk: according to the UN Special Rapporteur, "Where the rights of refugees to freedom of movement, to work, to access education or training, were limited, the risks of exploitation increased" (Mullally, 2023). The problem is pernicious when victims with uncertain legal status experiencing high levels of violence face incentives not to seek remedies (e.g. Dipoppa, 2023). In our representative sample of forcibly displaced people in Greece in 2023 (discussed below), only 56% of people reported that they know where to seek assistance when they experience harm.

Amidst these risks of exploitation and violence, what is the demand for legal empowerment amongst forcibly displaced people? Can legal empowerment increase displaced people's confidence in their ability to report violence and improve other aspects of well-being? Providing information on rights and responsibilities under national or international law and advice on how to exercise these rights is hypothesized to empower marginalized groups (e.g. Maru, 2006). To support the over 100 million people forcibly displaced in 2023, governments and civil society are rapidly adopting technologically-aided programs that aim to do just that (UNHCR, 2023; International Rescue Committee, 2023; Siegel, Wolff and Weinstein, 2023). Despite the high stakes of helping people fleeing violence and facing exploitation, there is minimal systematic evidence on what works. Many studies of the impact of information assume that everyone will receive it, without considering who chooses to engage. In this paper, we use a randomized field experiment with behavioral measures of autonomous demand for legal information and survey measures of well-being 3 months after treatment to address these questions and to understand how legal empowerment can improve the welfare of forcibly displaced people.

Contribution

States are incentivized to integrate migrants (Fouka, 2023) and help them live free from violence (Dancygier and Laitin, 2014). We study whether legal information can empower forcibly displaced people by increasing their knowledge of exploitation and their confidence in reporting abuse. Legal information and aid or legal “empowerment" is widely hypothesized to support marginalized people (Di Giovanni and Bercovich, 2021) by clarifying rights and responsibilities under national or international law and to support people in the process of accessing their rights (Sandefur and Siddiqi, 2013; Maru and Gauri, 2018; Dhital, Ho and Satterthwaite, 2022). Existing evidence shows mixed results on the impact of legal assistance in the American court system and in immigration processes (e.g. Greiner, Pattanayak and Hennessy, 2012; Riaz, 2021) and more systematic evidence is needed (Goodwin and Maru, 2017) for how empowerment can work for forcibly displaced people (Purkey, 2014).

Discrimination and exploitation are difficult problems to address given how both information gaps (Boittin, Archer and Mo, 2016) and relative deprivation (Mo, 2018) create the conditions for them. While migrants are typically more vulnerable to labour exploitation than non-migrants, forcibly displaced people’s past experiences of violence and contingent legal status disempower them, especially because they face specific incentives not to report to state authorities (Dipoppa, 2023; Comino, Mastrobuoni and Nicolò, 2020; Dhingra, Kilborn and Woldemikael, 2022).

Our research builds on previous work that explores how information content changes knowledge and beliefs (Lieberman, Posner and Tsai, 2014; Behavioural Insights, 2014) and how it impacts displaced people (Urbina, Moya and Rozo, 2023; Beber and Scacco, 2022). A key contribution is that we consider what shapes demand for information in addition to its impact. This builds on previous studies of information where all participants are exposed to information and incorporates the idea that people, and especially those who are marginalized, should make autonomous choices about whether to seek out content or not, with an impact on if and how information works. We use an experimental design to explore differences in preferences over different types of information provision (Vandeweerdt, Dinesen and Sønderskov, 2023) and their impact on improving forcibly displaced people’s well-being.

Seeking Asylum in Greece

We study legal empowerment in Greece, a key European entry point for asylum seekers. Between 2014–2022, over 1.2 million displaced people arrived. The signature of the EU–Turkey agreement in March 2016 meant asylum seekers had to submit claims in Greece and could no longer travel onward. This turned Greece from a transit into a hosting country.

Human rights organizations criticize Greece's asylum process as opaque and lengthy, creating poor living conditions for refugees and asylum seekers (European Council on Refugees and Exiles, 2021). In Greece and in our sample, people mostly originate from Afghanistan, the DRC, Iraq, Iran, and Syria. 38% are women. 70% experienced serious violence in the year before they came to Greece. 28% were assaulted or sexually assaulted on their way to Greece, and 10% since they arrived. 85% of the sample suffer from moderate to severe mental distress.

These conditions are not unique to Greece. People who flee their place of habitual residence often find themselves in unfamiliar and hostile environments: exploitative working conditions, reduced access to livelihoods, dispersed families and social networks, and changes in their legal status vis-a-vis the state put them in a particularly vulnerable position in their country of asylum (ICMPD, 2018). To address a lack of clear and credible legal information on these and other topics, grassroots organizations and large NGOs provide legal information to forcibly displaced in many refugee-receiving countries including Greece.

We worked with two civil society organizations. First, IRC Hellas manages Refugee.Info, a website delivering generic legal information to refugees and asylum seekers in 6 languages. The website hosts articles that cover various topics, including making asylum claims, accessing public services, and enjoying rights under Greek and international law. Second, volunteer law student caseworkers from the Mobile Info Team (MIT) utilize the same information found on *Refugee.Info*. However, they personalize this information for asylum seekers and refugees by providing it through direct one-on-one WhatsApp chat conversations. Users can exchange written messages or voice notes in different languages asking questions about issues they face in Greece.

There are trade-offs across these two modalities of information provision. Generic information is low cost for users to access easy to scale. However, it is harder to navigate: people have to sift through information to find what they need. Users must be literate, both literally and digitally, and not feel overwhelmed when landing on the website in order to process the detailed information.

Personalized legal information has a higher cost to access because it requires reaching out and engaging with a person. Each additional user requires caseworker staff support, which helps users to navigate information relevant to them. Personalized support can also address the discrepancies between the theoretical rights a person has and the reality of the situation in Greece, allowing them to focus their attention and efforts where it actually matters.

Research Design

We hypothesised that legal information would improve people's well-being. We also imagined that differences in the cost of accessing different kinds of information might lead to differences in demand for different types of information. At the same time, different information modalities might shape whether information leads to change. Lower access costs would lead to more demand for generic information, while support with information navigation should lead to higher effectiveness and changes in well-being for individuals assigned to personalized information. For the full set of pre-registered hypotheses and analysis, see sections A and I in the Appendix.[1]

To test these hypotheses, we implemented a three-arm randomized control trial (a consort diagram is in Appendix section B). We worked with UNHCR Greece to identify a sample that included n_1=3,755 respondents from UNHCR's proGres database of refugees and asylum seekers, who were believed to be still in Greece and reachable by phone (information on sampling and ethics are available in Appendix sections C and D). Five months after collecting baseline data, we randomly assigned participants to either an encouragement to seek out generic information, personalized information, or a control group. Participants in the generic and personalized information groups received a set of three WhatsApp text messages, one voice message and one call over the span of ten days to encourage them to take up the treatment, while the control group received a placebo message and call to maintain contact information. We collected endline data three months after the intervention, which represents a hard test of our hypotheses on the impact of information on well-being given the four-week recall periods used in our survey questions and the typically shorter time between treatment and measurement in other studies (Grigorieff, Roth and Ubfal, 2020; Lergetporer and Woessmann, 2023).

As is typical with marginalized and mobile populations, sample attrition is a major concern. To minimize attrition between baseline and endline, enumerators called respondents up to 10

[1]We separately pre-registered hypotheses for take-up and impact and these pre-analysis plans are provided in the supplemental materials.

times, at different hours, and collected secondary contacts. We also compensated participants for their time. For more details, see Appendix section E. Overall, 45.6% (n_2=1,707) of baseline respondents participated in the endline survey. However, the retention rate is well-balanced across treatment conditions: The retention rate in the control group is 45.4% (95% CI: 42.6–48.2), in the generic information group 44.3% (95% CI: 41.6–47.1) and 46.7% (95% CI: 43.8–49.3) in the personalized information group. Table A.6 in the Appendix shows that these small differences are not statistically significant at conventional levels. Appendix Figure A.3 shows that when expanding the attrition analysis to include interactions between treatment groups and baseline characteristics, two out of 20 interactions are statistically significant at the 5% level. In contrast to attrition, item non-response was not an issue in our survey. Table A.7 shows that item non-response was below 1% for all outcomes.

Measurement

In addition to treatment assignment, we measured legal information engagement using pre-registered behavioral outcomes that capture whether a participant engaged with either generic or personalized legal information (see Appendix Section F). We have two measures: at least one click on the link we sent to respondents and, as a robustness check, sustained engagement equal to one if respondents spent at least 60 seconds on the Refugee.Info website (generic treatment) or started a conversation with MIT (personalized treatment). A technical error limited measurement of take-up for an initial subset of the sample and we conservatively code 0 engagement for these respondents. We use two-sample IV analysis leveraging the compliance rates for the sample for which we have complete data to show that our results are robust to this measurement error. Appendix Section G provides more details. We also conducted an exploratory, descriptive analysis using qualitative administrative data on anonymized chat conversations between refugees and asylum seekers and caseworkers.

We focus on a multidimensional understanding of refugee and asylum seeker well-being captured in our survey data. A *Knowledge of exploitation* index includes questions on whether (i) ...an employer withholding your salary is exploitation, (ii)...an employer not agreeing to give you a raise when you ask for it is exploitation, and (iii)...an employer taking your identity documents and not giving them back is exploitation. *Coping with violence* combines two questions focusing on speaking about violence with people the respondent knows and knowing where to seek assistance. *Exposure to violence* is an index of whether respondents (i) were detained against their will, (ii) were assaulted or sexually assaulted, (iii) were made to sign a document without fully understanding what it meant, (iv) were forced to work by someone, or (v) if someone had taken and kept their identification documents. An index of *Documents and services* includes tax ID number, social security number, and bank account. The *Kessler-6* mental distress scale captures mental distress in the past four weeks (Kessler et al., 2003). A digit span measure is proxy variable for transitory state anxiety levels (Barker et al., 2022). We measured respondents' level of integration by adapting Harder et al. (2018)'s index to capture economic, navigational (i.e. the ability to navigate the host country), social, linguistic, and psychological integration in Greece.

The Impact of Legal Information

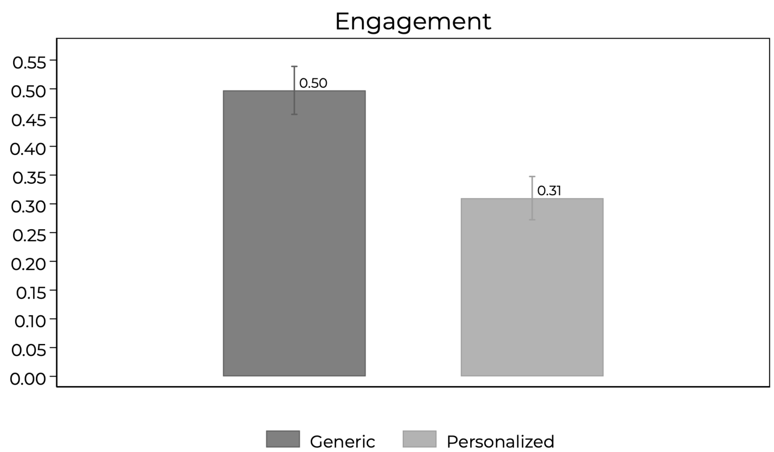

Figure 1: Take-up of the intervention using the dummy variable *Clicked* as a compliance measure (equal to 1 if the respondent clicked on their link at least once, 0 otherwise). 95% confidence intervals. See Appendix M for further results.

First, we explore demand for information by exploring differential take-up for the generic and the personalized legal information provision. We focus on click rate for our main analysis (the main results are robust to using our second measure of higher engagement; see Appendix table A.5.). The take-up results are displayed in Figure 1.

Participants in the study chose generic information with a higher take-up rate of about 50% (95% CI: 46–54). This compares with 31% (95% CI: 27–35) for participants in the personalized treatment. The finding that engaging with information is more than 50% higher for generic compared to personalized legal information provides initial support for our hypothesis that accessing a website requires less effort than accessing caseworker support.

Following our pre-analysis plans, we estimate the impact of generic and personalized legal information provision separately and together.[2] Figure 2 displays the intention-to-treat (ITT) estimates for our main outcomes in their suspected causal order, from proximate to remote. Starting with people's knowledge of exploitative situations, we find that assigning participants to either information provision mode increased their ability to correctly identify *knowledge of exploitation* by about 0.17 (95% CI 0.03–0.30) SD. While the effect of personalized information provision is 0.22 SD (95% CI 0.06–0.37), the benefits of generic information provision are somewhat smaller and not statistically significant. For our measure of *coping with violence*, we find similar effects: while the pooled estimate is 0.15 (95% CI 0.02–0.29) SD, the effects of personalized information provision are slightly larger (0.18 SD; 95% CI 0.02–0.33) compared to generic information provision (0.13 SD; 95% CI -0.03–0.29). For our measure of exposure to violence, all the ITT estimates are negative, but small and outside of conventional significance levels.

The pattern is similar for the remaining outcomes *documents and services*, the *Kessler-6*, the *digit-span measure*, and the *IPL index* aggregated over the five dimensions: all ITT effects are small, and none is statistically significant.

[2]The pooled estimates are also justified by the similarity of the ITT estimates across the two modes of information provision.

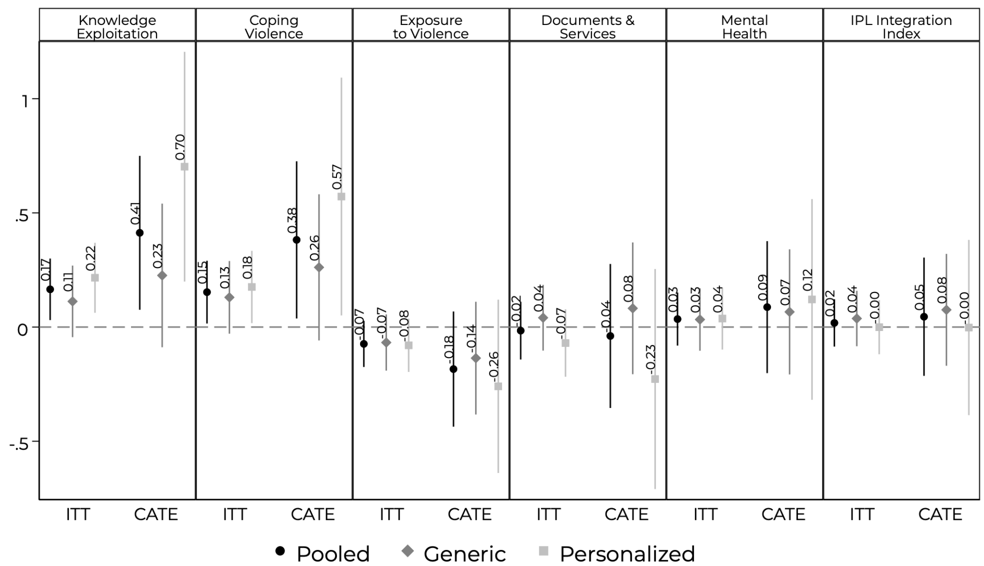

Figure 2: Intention-to-treat (ITT) and complier average treatment effects (CATE). 95% confidence intervals; with standard errors clustered at the household level. See Appendix M for further results.

Next, we report the effect of the information provision for those refugees and asylum seekers who complied with the encouragement and, depending on the treatment, engaged with Refugee.Info or reached out to MIT (see Appendix section J). In light of the take-up rates between 30 and 50 %, the complier average treatment effects (CATE) are an important quantity of interest to assess the benefits of generic and personalized legal information for the subset of beneficiaries who were willing to engage with information when encouraged to do so. As expected, we find large effects for respondents who complied, and similar patterns to the ITT results. The CATE estimates reveal that personalized information increased participants' ability to correctly identify instances of exploitation (0.70 SD; 95% CI 0.20–1.21), an effect that is about three times as large as the CATE estimate for the generic treatment (0.23 SD; 95% CI -0.09–0.54). Similarly, the CATE for responding to violence is more than twice as large for the personalized (0.57 SD; 95% CI 0.05–1.09) as for the generic treatment (0.26 SD; 95% CI -0.06-0.58)

Discussion

To further understand this finding, we explore who engages with legal information and what might explain why personalized information is more effective. Our research design allows us to leverage pre-treatment characteristics measured at baseline to profile compliers and compare them to participants who do not seek out information, even if encouraged to do so ("never-takers") (Marbach and Hangartner, 2020). The results, presented in Appendix Figure A.9, suggest that compliers in the generic treatment arm are similar to never-takers in all observed characteristics, including gender, age, time in Greece, legal status, living in camps, experiences of discrimination, and willingness to leave Greece. In contrast, compliers in the personalized treatment are less likely to consider leaving Greece and more likely to report discrimination than never-takers. This suggests that participants are likely considering expected benefits when deciding whether to invest the time to seek personalized help.

What explains why personalized legal information is more effective? The experiment was designed to keep as many things about the two legal information treatments the same to identify if the difference between access and navigation costs explains differences in information effectiveness. Both services use the same core legal information resources. However, the involvement of another person in personalized information is an important factor that sets it apart.

First, human connection could be a difference between chat conversations and website information. An anonymized transcript from a chat conversation in December 2022 illustrates this potential: "*Client: I am really happy to hear from you. ...I have spent a lot of time in here, we have suffered mental health issues. Caseworker: Thank you so much for opening your heart to me and telling me your story. I appreciate you trusting me. I am so sorry to hear about all the difficulties you have been through and I cannot imagine how difficult this situation must be.*"

We use our survey data on the experience of using the two treatments to create a summary index of a question that asks participants who reported having visited Refugee.Info or conversed with MIT about how the service made them feel (for example "connected"). In Appendix Figure A.11, we find that the distribution of the index is very similar across both treatments. This suggests that despite the potential of conversations to be more empathetic, this was not a salient enough part of the experience to be recalled during our follow-up survey.

Turning to content, we compare information on browsing behavior on Refugee.Info with (anonymized) chat conversations coded by topic that come from MIT's administrative data (see Appendix Figure A.10). The most common topic for both treatments was general legal rights in Greece (80%

and 85% in the generic and personalized treatment, respectively), suggesting sizeable overlap in information content. However, participants also sought different information: About 50% of people looked up access to non-health services and working in Greece on *Refugee.info*, compared with 21% and 8% for personalized information. We also find that in chat conversations, more than half of refugees and asylum seekers received information about legal rights that are specific to their individual cases and not available via generic legal information. This suggests that the chat conversations are able to provide tailored information that would be difficult, if not impossible, to get from generic sources.

Conclusion

States have incentives to support disempowered members of society but often lack the tools or policies that do so most effectively. We explore the demand for and impact of legal empowerment, a common intervention used to support marginalized people. We find that how information is provided matters. Half of people encouraged to participate were willing to engage with generic information provided on a website. The take-up rate for personalized information via chat conversation was just above 30%.

In addition to shaping demand, information delivery shapes impact: we find that for those who complied with the personalized treatment, this intervention is about twice as effective as generic information at changing knowledge of exploitation and coping with violence. Legal information did not show an impact on more distant outcomes such as access to services, mental health or integration. Given the population that participated in this study has been in Greece for a longer period of time and has already been exposed to different types of information over time, it is possible that this is a lower bound on the impact of information for marginalized populations, especially in the earlier stages of a displacement crisis.

Together, these findings have implications for states that seek to improve the well-being of displaced people and ultimately reduce exploitation and violence targeting marginalized people. Our experimental estimates support further investments in legal empowerment and future initiatives should aim to combine the high-take up of generic content with referrals to personalized legal support to maximize the distinct advantages of both approaches.

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Appendix

A Pre-registered hypotheses and outcomes

Table A.1: Pre-registered hypotheses focusing on the main effects

Hypothesis	Description
Panel A: Pre-registered hypotheses for take-up	
Hypothesis 1	*Take-up rate rate is higher for static information than for the dynamic information.* We expect the take-up rates for static information provision to be higher than dynamic information provision. Accessing static information is less demanding for participants than accessing dynamic information where they must provide information about themselves and have a more active role to engage in a conversation.
Panel B: Pre-registered hypotheses for the experiment	
Hypothesis 2	*Participants treated with static and dynamic information will report larger effects on well-being compared with untreated participants.* We expect information provision of either type (static and dynamic) to lead treated participants to report larger effects on well-being.
Hypothesis 3	*Participants treated with dynamic information will report larger effects on well-being compared with participants treated with static information.* We expect the effect for the dynamic information group to be larger than for the static information group. Although accessing static information is less demanding for participants (i.e., for dynamic information respondents must provide information about themselves and have a more active role to engage in a conversation.), we expect personalized 1-1 conversations to be more effective and therefore to have larger effects on well-being than static information provision.

Table A.2: Pre-registered measures of participant well-being.

Outcome measure	Description
Identifying exploitation	We combine the questions below and form an index for identifying instances of exploitation, which is the cumulative sum of correctly answering each of the following questions: In your opinion … • …an employer who withholds your salary is that exploitation? (correct answer: Yes) • …an employer not agreeing to give you a raise when you ask for it is that exploitation? (correct answer: No) • …an employer taking your identity documents and not giving them back is that exploitation? (correct answer: Yes)
Responding to instances of exploitation	We make an index about how participants respond to instances of exploitation by combining the answers to the questions below: • How often do you talk about these things (i.e., instances of exploitation) with people you know? • Do you (would you) know where to seek assistance for these kinds of situations (if you had to)?
Exploitation index	We combine the questions below and form an exploitation index, which is the cumulative sum of answering 'Yes' to each of the following questions: In the past four weeks … • …were you detained against your will? • …were you assaulted or sexually assaulted? • …were you ever made to sign a document without fully understanding what it means? (for instance, a work contract or a contract for your housing or any official document) • …were you forced to work by someone? • …did anyone ever take and keep your identification, for example, your passport or driver's license?
Current documents and services	We make an index based on how many documents and services respondents already have. These are captured by the following questions: • Do you have a PAAYPA number? (temporary social insurance number) and/or Do you have a social security number/AMKA number?† • Do you have a tax ID number / AFM number / fiscal registration number? • Did you open a Greek bank account?
Mental health scale	K6 mental health scale following Kessler et al. (2003)
Digit Span	We use better performance in the digit span assessment test as a proxy for lower levels of transitory state anxiety (Hodges and Spielberger, 1969)
Integration Score	Integration index following Harder et al. (2018)

†: A PAAYPA is a temporary social security number for asylum seekers which allows them to access services like public health care, and to work. AMKA is is a permanent social security number. Having an AMKA number allows a person not only to work in Greece but also gives the holder access to health care, employment protection, benefits, and other state services.

A.0.1 ITT

Table A.3: Intention-to-treat effects

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A: Pooled treatments							
Pooled	0.165*	0.153*	-0.074	-0.016	0.035	-0.031	0.018
	(0.069)	(0.070)	(0.052)	(0.064)	(0.059)	(0.081)	(0.053)
N	1707	1696	1706	1707	1706	1092	1700
Panel B: Generic information							
Generic	0.112	0.130	-0.068	0.041	0.033	-0.015	0.037
	(0.080)	(0.081)	(0.063)	(0.073)	(0.070)	(0.091)	(0.062)
N	1123	1118	1123	1123	1123	715	1120
Panel C: Personalized information							
Personalized	0.216**	0.176*	-0.080	-0.070	0.037	-0.047	-0.001
	(0.078)	(0.081)	(0.059)	(0.075)	(0.069)	(0.093)	(0.060)
N	1152	1143	1151	1152	1151	730	1147

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

A.0.2 Local average treatment effects

- Clicked

Table A.4: Main outcomes (LATE). Treatment: Click rate.

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A. Second-stage regressions (Instrument == Pooled)							
Clicked	0.413*	0.382*	-0.184	-0.039	0.087	-0.070	0.045
	(0.172)	(0.176)	(0.129)	(0.161)	(0.147)	(0.184)	(0.132)
F stat. (first stage)	433.8	431	434.1	433.8	434.1	305.3	431.6
N	1707	1696	1706	1707	1706	1092	1700
Panel B. Second-stage regressions (Instrument == Generic information)							
Clicked	0.226	0.261	-0.136	0.082	0.066	-0.029	0.076
	(0.160)	(0.163)	(0.126)	(0.147)	(0.140)	(0.175)	(0.125)
F stat. (first stage)	349.7	348.5	349.7	349.7	349.7	244.7	346.9
N	1123	1118	1123	1123	1123	715	1120
Panel C. Second-stage regressions (Instrument == Personalized information)							
Clicked	0.702**	0.572*	-0.260	-0.228	0.121	-0.133	-0.002
	(0.257)	(0.266)	(0.194)	(0.246)	(0.224)	(0.262)	(0.196)
F stat. (first stage)	204.8	202.2	205	204.8	205	159.7	203.9
N	1152	1143	1151	1152	1151	730	1147

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

- Engagement

Table A.5: Main outcomes (LATE). Treatment: Engagement.

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A. Second-stage regressions (Instrument == Pooled)							
E. (both)	1.006*	0.929*	-0.448	-0.095	0.213	-0.160	0.111
	(0.424)	(0.432)	(0.315)	(0.392)	(0.359)	(0.423)	(0.325)
F stat. (first stage)	133.6	132.9	133.7	133.6	133.7	96.70	131.3
N	1707	1696	1706	1707	1706	1092	1700
Panel B. Second-stage regressions (Instrument == Generic information)							
E. (RI)	0.491	0.570	-0.295	0.178	0.144	-0.058	0.165
	(0.350)	(0.359)	(0.274)	(0.320)	(0.303)	(0.351)	(0.274)
F stat. (first stage)	143.8	142.6	143.8	143.8	143.8	109.4	141.4
N	1123	1118	1123	1123	1123	715	1120
Panel C. Second-stage regressions (Instrument == Personalized information)							
E. (MIT)	1.921**	1.545*	-0.710	-0.622	0.331	-0.331	-0.006
	(0.745)	(0.738)	(0.538)	(0.672)	(0.614)	(0.652)	(0.540)
F stat. (first stage)	46.40	46.60	46.40	46.40	46.40	39.40	45.60
N	1152	1143	1151	1152	1151	730	1147

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

B Research design

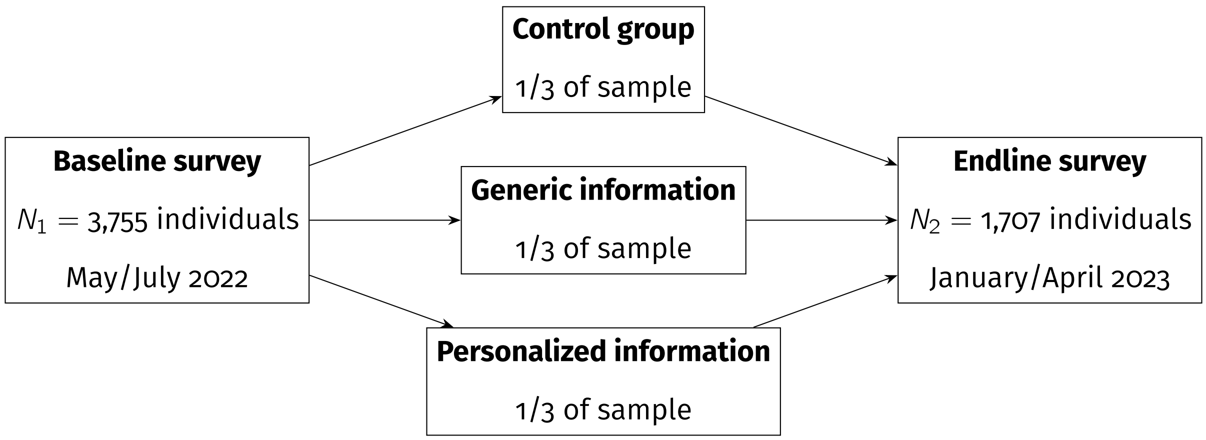

Figure A.1: Experimental design

C Sampling strategy

UNHCR Greece's proGres database was used to generate the sampling frame. As of November 2021 when UNHCR shared the dataset, it included 98,014 individuals. All the refugees[1] and asylum seekers who had ever been beneficiaries of blanket cash assistance were included in proGres. The dataset covers the universe of asylum seekers and refugees in the country known to UNHCR in November 2021.

Out of these 98,014 individuals, 59,464 were over 18 years old, had arrived after 2015, UNHCR had no information that they had left Greece and had registered a phone number.[2]

From this set of 59,464 observations, we randomly selected 10,000 refugees and, out of these, excluded those who were born in 1921 or earlier and whose first names were missing. In addition, we developed a prediction model for the probability of obtaining recognized refugee status for asylum seekers in proGres. Based on this prediction model, we selected 20,000 asylum seekers with the highest probability of becoming refugees. Out of these 20,000 asylum seekers, we kept one person for every household (with the selected person being the one with the highest probability of becoming a refugee). As a result, the original sampling frame provided to the survey firm consisted of 9,875 refugees and 15,285 asylum seekers.

The survey firm was then tasked to randomly call these 25,160 potential participants. To be

[1]Once the asylum application has been processed there are three outcomes: one can be granted refugee status, subsidiary protection or be rejected (with some pathways to appeal). The main difference between refugee status and subsidiary protection is that those who are granted refugee status have the right to a three-year residency permit and the right to family reunification, whereas those who are only granted subsidiary protection receive a one-year (renewable for two more years) residency permit that does not come with the right to family reunification.People with refugee status or subsidiary protection are referred to as 'refugees' throughout the paper.

[2]Access to a mobile phone is a condition of eligibility to participate in the study. However, it might be that mobile phones are not accessible to specific vulnerable groups. It is worth noting, however, that this population has a very high proportion of mobile phone users. Over 93% of all refugees globally have some mobile phone coverage (ITU, 2017). 86% of refugees and asylum seekers in UNHCR's *proGres* database have registered at least one mobile phone.

eligible refugee and asylum seeker participants had to report being currently in Greece. Initially, the data collection protocol included reaching out to every eligible participant 10 times in order to maximize the chances of including people within Greece and eligible for participation in the sample. In practice, respondents either responded within 3 phone calls or were not reachable (either because the phone was disconnected, the number was invalid, the intended participant was not in Greece, or because the person was not known at that number). Because the survey firm was close to exhausting the sampling frame, a further 7,578 contact details were provided. As a result, although our initial sample was weighed toward high probability asylum seekers, in practice, the sample contained a random draw of contactable asylum seekers in Greece at the time of the study.[3]

In total, the survey firm completed surveys with 3,755 respondents. To be a survey participant, individuals needed to provide informed consent, to report currently residing in Greece, to confirm their identity and that they were 18 years or older at the time of the interview.

[3]We followed a similar workflow–albeit at a smaller scale–for selecting these additional 7,578 potential contacts. For refugees, we randomly selected 3,000 refugees from the proGres dataset who were not already selected for the original sampling frame. Out of these 3,000 refugees, we dropped everyone who was born in 1921 or earlier and whose first name was missing. For asylum seekers, we identified 10,000 asylum seekers with the highest probability of becoming refugees and who were not already included in the sample. We then dropped any individual who had a 5% or lower probability of becoming a refugee. This supplementary sampling frame of refugees and asylum seekers consisted of 2,935 refugees and 4,643 asylum seekers.

D Ethics

D.1 Collecting sensitive data

This study passed through institutional research ethics processes at [REDACTED] and [REDACTED], as well as through ethical review at both UNHCR Greece and IRC.

One important ethical challenge posed by this project is the collection of sensitive data from vulnerable populations. We pursued several strategies to mitigate this challenge. First, we took care to only elicit the minimum required sensitive information from participants in the study. In addition to ethics reviews at both universities, our humanitarian partners (UNHCR, IRC Hellas and Mobile Info Team) and the survey firm provided feedback on data collection to ensure that it met their ethical standards. This included regular information sharing and feedback on the content of our surveys and protocols such that they were designed as sensitively as possible. The enumerators hired by the survey firm participated in training specifically designed for researchers working with vulnerable populations discussing difficult or sensitive questions delivered by the authors of this study prior to data collection.[4] During the enumerators' training and pilot, some questions deemed too sensitive were removed or rephrased. Just before the baseline, in March 2022, MIT coordinators and caseworkers attended a training about exploitation and working with survivors provided by A21, an organization that works to fight exploitation and human trafficking.

Second, in line with best practice, whenever sensitive information about protection issues could be surfaced during the research, we provided phone numbers of service providers in Greece for those who needed and/or those who asked for it. 21% were given such a number either at baseline or endline.

Third, for their participation in the baseline and endline, participants received a compensation of ≈ 11 EUR in the form of phone credit. The amount was agreed upon after consultation with humanitarian actors, the survey firm and the constraints that mobile phone companies have in Greece.

Finally, a key ethical concern that can arise from research designs such as this one is that

[4] Some questions were also dropped because they were less relevant, for example the political component of the IPL index was excluded from the survey for this reason.

if a program or intervention is found to have positive benefits that it is not withheld from a population that can benefits. As the effects of information provision were found to be positive, in the fall 2023 we sent all participants for whom it was relevant a link to both Refugee.Info and/or MIT. We also hope that this study will encourage donors to fund these organizations and/or similar projects.

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E Robustness

E.1 Attrition

E.1.1 Minimizing attrition

To minimize attrition between baseline and endline, we adopted several strategies following Alrababah et al. (2023). First, enumerators called respondents up to ten times, at different times of the day, on different days and when necessary after working hours to include employed respondents. Second, we asked participants to save the enumerators' phone number, collected secondary contacts (within and outside the household), and whenever possible, used the same enumerators for all waves of data collection. We also informed respondents at baseline that they would be contacted again. Third, we called them in the week after the first message of the intervention was sent. This short check-in call helped us stay in touch with respondents and answer any questions they may have, as well as updating secondary contacts. Fourth, we compensated respondents for their time by sending them phone credit. Those who were no longer in Greece at endline but participated in the survey could provide the number of someone in Greece to receive phone credit.

These strategies helped us retain 45% of our sample. This population is very mobile within Greece and they are also likely to move outside of the country as we see from the endline data. Doing the baseline interview in person might have helped minimizing attrition but this was not possible when we designed the study. As 96% of our sample had been in Greece for more than two years at the time of the interview, survey fatigue might also be at play. These retention rates are in line with other phone-based panel surveys, especially when considering the vulnerability and mobility of our population.

E.1.2 Differential attrition

Overall attrition rate: 55%

- Control group: 55%
- Generic information group: 56%

- Personalized information group: 53%

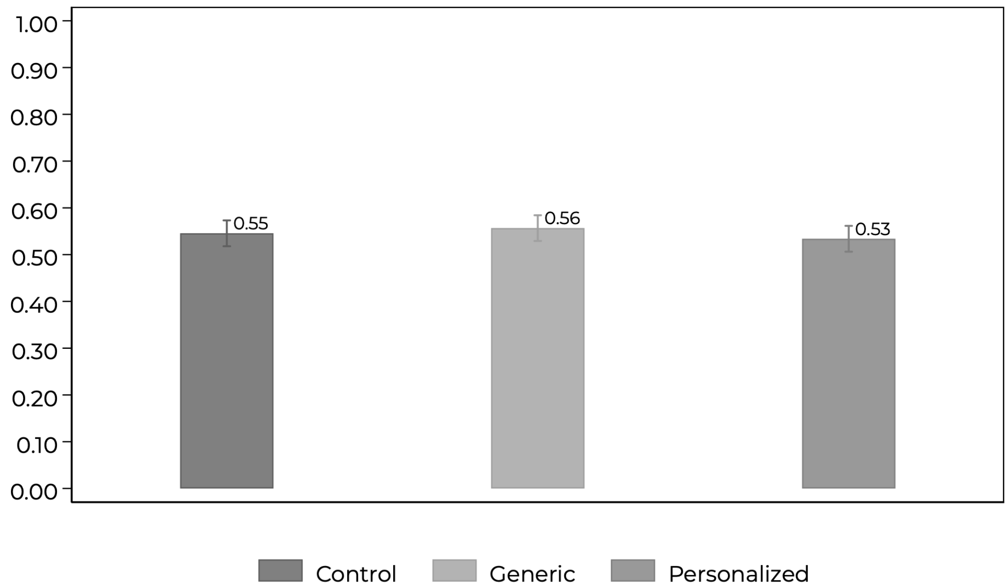


Figure A.2: Response rate by treatment arms. 95% confidence intervals.

E.1.3 Selective attrition

Table A.6: Attrition analysis using variables from baseline survey

	(1) ITV 2	(2) ITV 2	(3) ITV 2
Generic	-0.012	-0.007	0.061
	(0.020)	(0.020)	(0.116)
Personalized	0.012	0.012	0.188
	(0.020)	(0.020)	(0.116)
Age		0.003^{**}	0.004^{*}
		(0.001)	(0.002)
Woman		-0.002	-0.007
		(0.017)	(0.029)
Contact Attempts		-0.033^{***}	-0.023
		(0.008)	(0.016)
Employed		-0.007	-0.031
		(0.025)	(0.044)
Docs. & Serv.		0.008	0.013
		(0.009)	(0.014)
Kessler 6		-0.001	0.002
		(0.001)	(0.002)
Exp. Resp.		0.013	0.025
		(0.012)	(0.021)
Exp. Ident.		0.013	0.011
		(0.010)	(0.017)
Exp. Index		-0.005	-0.043^{**}
		(0.009)	(0.015)
IPL Index		0.239^{***}	0.298^{**}
		(0.060)	(0.103)
p-value (joint sign.)			0.0001
R-Squared	0.003	0.023	0.028
Interactions			✓
N	3755	3729	3729

Notes: Variable values are from the baseline survey. All models include a dummy variable for batch. Standard errors are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

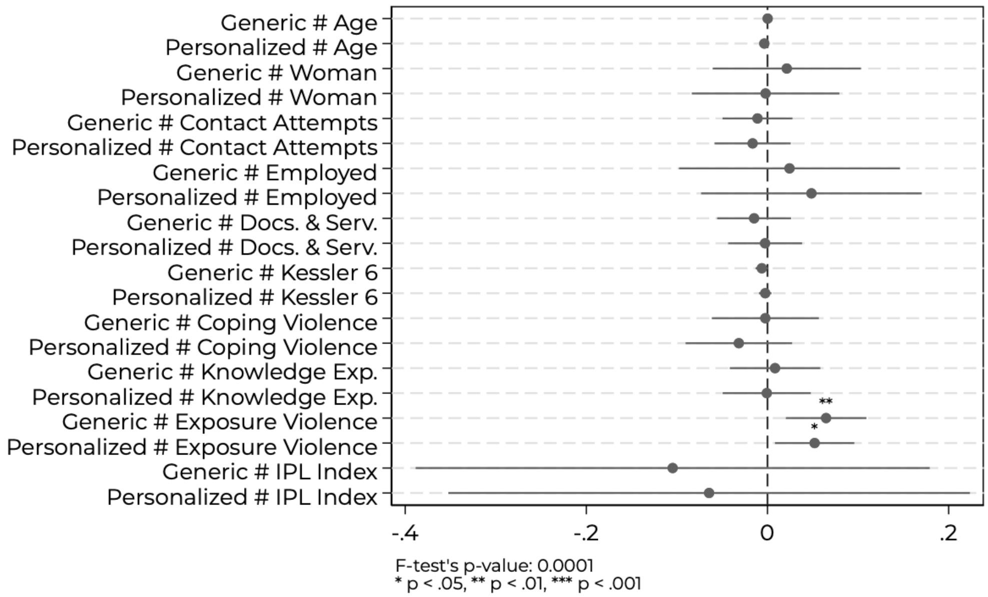


Figure A.3: Estimated coefficients of the interaction terms from model (3) in Table A.6.

To minimize attrition between baseline and endline, we adopted several strategies. All respondents were also contacted for a short check-in call during the week after the first message was sent. This call served three purposes: keeping in touch with respondents, obtaining additional secondary contacts and encouraging them to take the treatment.

E.2 Item missingness

Table A.7 show the rates of "Don't Know" (DK) and "Refuse to Answer" (RTA) for each of components of the outcomes evaluated in our study. We observe low rates of both DK and RTA across all components. As a result, we determined that mean imputation for each component of our outcomes would be a sufficient approach to address DK and RTA responses. The low incidence of both types of answers suggest that the impact of such imputations on our findings and conclusions is minimal.

	Baseline				Endline			
	DK	DK (Percent)	RTA	RTA (Percent)	DK	DK (Percent)	RTA	RTA (Percent)
Mental Health								
Felt nervous	11	0.3%	4	0.1%	0	0.0%	6	0.2%
Felt hopeless	15	0.4%	4	0.1%	2	0.1%	7	0.2%
Felt restless	22	0.6%	7	0.2%	2	0.1%	7	0.2%
Everything was an effort	64	1.7%	7	0.2%	8	0.5%	7	0.2%
Felt depressed	32	0.9%	7	0.2%	5	0.3%	8	0.2%
Felt worthless	44	1.2%	5	0.1%	12	0.7%	8	0.2%
Knowledge Exploitaiton								
Employer witholding salary is exp.			18	0.5%			11	0.3%
Exployer expropriating ID docs. is exp.			13	0.3%			8	0.2%
Employer disagreeing to give raise is exp.			38	1.0%			7	0.2%
Coping with Violence								
Talking about expl.	111	3.0%	29	0.8%	45	2.6%	3	0.1%
Know where to seek help.			29	0.8%			3	0.1%
Exposure to Violence								
Forced labor	12	0.3%	7	0.2%	1	0.1%	1	0.0%
Detained	3	0.1%	6	0.2%	0	0.0%	0	0.0%
Assaulted	7	0.2%	11	0.3%	0	0.0%	1	0.0%
ID docs. expropriated	24	0.6%	6	0.2%	6	0.4%	0	0.0%
Forced to sign doc.	98	2.6%	3	0.1%	10	0.6%	2	0.1%
Psychological Integration								
Connected	34	0.9%	3	0.1%	8	0.5%	1	0.0%
Outsider	24	0.6%	3	0.1%	8	0.5%	1	0.0%
Linguistic Integration								
Read	53	1.4%	1	0.0%	20	1.2%	0	0.0%
Speak	36	1.0%	1	0.0%	14	0.8%	3	0.1%
Social Integration								
Meals	8	0.2%	7	0.2%	7	0.4%	1	0.0%
Contacts	8	0.2%	4	0.1%	7	0.4%	1	0.0%
Navigational Integration								
See a doctor	61	1.6%	0	0.0%	38	2.2%	0	0.0%
Find job	71	1.9%	6	0.2%	62	3.6%	2	0.1%
Economic Integration								
Employed	0	0.0%	1	0.0%	2	0.1%	4	0.1%
Unemployed: what have been doing	16	0.7%	4	0.2%	1	0.1%	5	0.5%

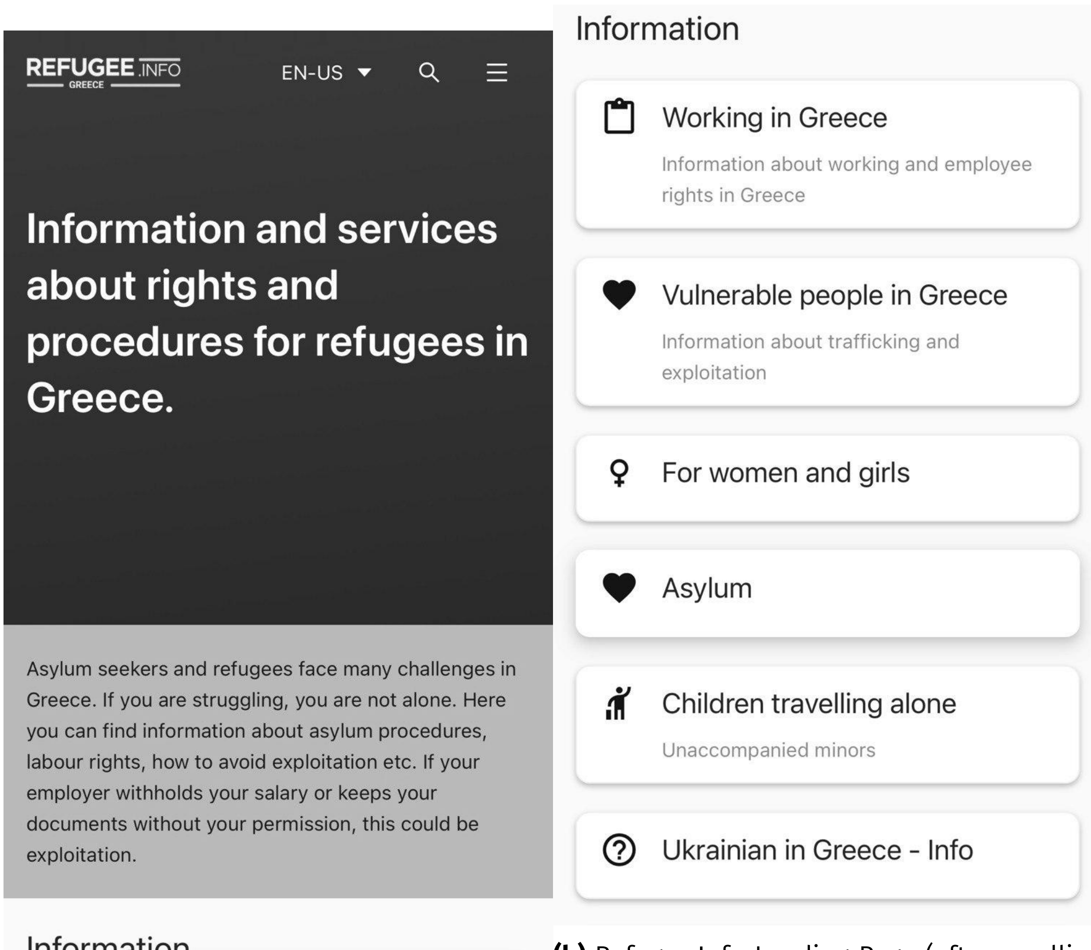
Notes: This table represents the distribution of "Don't Know" and "Refuse to Answer" responses for each component of the outcomes evaluated in our study. It is important to note that for variables associated with *Knowledge Exploitation*, "Don't Know" responses are not shown. This is because, within the context of our study, such responses are considered valid rather than missing data. Therefore, these instances do not undergo mean imputation. The same logic applies to the question associated with "Know where to seek help", as a "Don't Know" response is considered valid for the purpose of our study. Lastly, for the variable associated with *Unemployed: what have been doing*, the calculation of percentages of "Don't Know" and "Refused to Answer" is based on a different denominator compared to the other variables. This adjustment is accounts for the display logic implemented in the survey, as only respondents who were not employed were asked this question.

Table A.7: Item Non-Response

F Refuge.Info and MIT Interface

This appendix section provides illustrations of the Refugee.Info and MIT interface that research participants saw when they engaged in the study.

F.1 Generic information (Refugee.Info website)



(a) Refugee.Info Landing Page

(b) Refugee.Info Landing Page (after scrolling down)

Figure A.4: Landing page of Refugee.Info's mobile version. For the duration of the study, the Messenger, Facebook and WhatsApp buttons on the Refugee.Ino website were turned off and participants could not reach the Refugee.Info team via the website.

On Refugee.Info, users can access information in 6 languages (English, Arabic, Dari/Farsi, French, Urdu and Ukrainian). The website provides articles on the asylum process, working in Greece, legal rights, access to services such as health and education, how to obtain documents etc. It also has articles and a map listing all the services available and which organizations offer what. The "Documents and procedures" section gives step by step explanations on how to register a birth or how to renew expired documents for example. The "Working in Greece" section provides information on workers and employees' rights in Greece. It also includes an article on exploitation and human trafficking and where to seek for help. There are also three sections aimed at particular groups: "Children travelling alone", "Vulnerable people in Greece" and "Women and girls" (information on women's rights in Greece, gender-based violence and how to seek help).

F.2 Personalized information (Mobile Info Team)

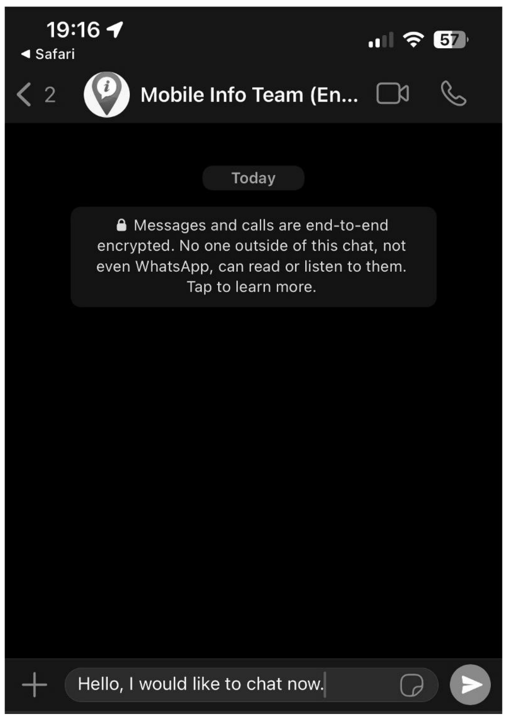


Figure A.5: WhatsApp chat with Mobile Info Team

On the WhatsApp hotlines managed by Mobile Info Team, users can exchange messages in 5 languages (English, Arabic, Dari/Farsi, French, Urdu) about issues they face in Greece. These 1-1 chat conversations can also be done via voice messages which allows populations with low rates of literacy to access information. Case workers all have a legal background but do not necessarily speak the mother tongue of asylum seekers and refugees. Mobile Info Team relies on a network of interpreters. It is made clear to the users that their messages will also be shared with them.

In addition to the number of requests, this can create some delays. During the experiment, MIT case workers typically acknowledged reception of the first message within 4 days and aimed to reply in the shortest time possible.

If assigned to the personalized treatment group, participants first received a message on WhatsApp with a prefilled message saying "Hello, I would like to chat now." They could either edit it or send it as is. They then received an answer from MIT case workers asking how they may help. Within the experiment all the participants who got in touch with Mobile Info Team received an answer. In instances where respondents submitted questions concerning their individual situations in Greece, they were provided with a personalized response by MIT 88% of the time. The delay for the case to be solved depends on the complexity of the request and the number of interactions needed between the case worker and the user. All exchanges are done via voice notes or written messages, they never happen live as MIT does not operate a 24/7 hotline with interpreters and case workers on hold.

F.3 Topics covered

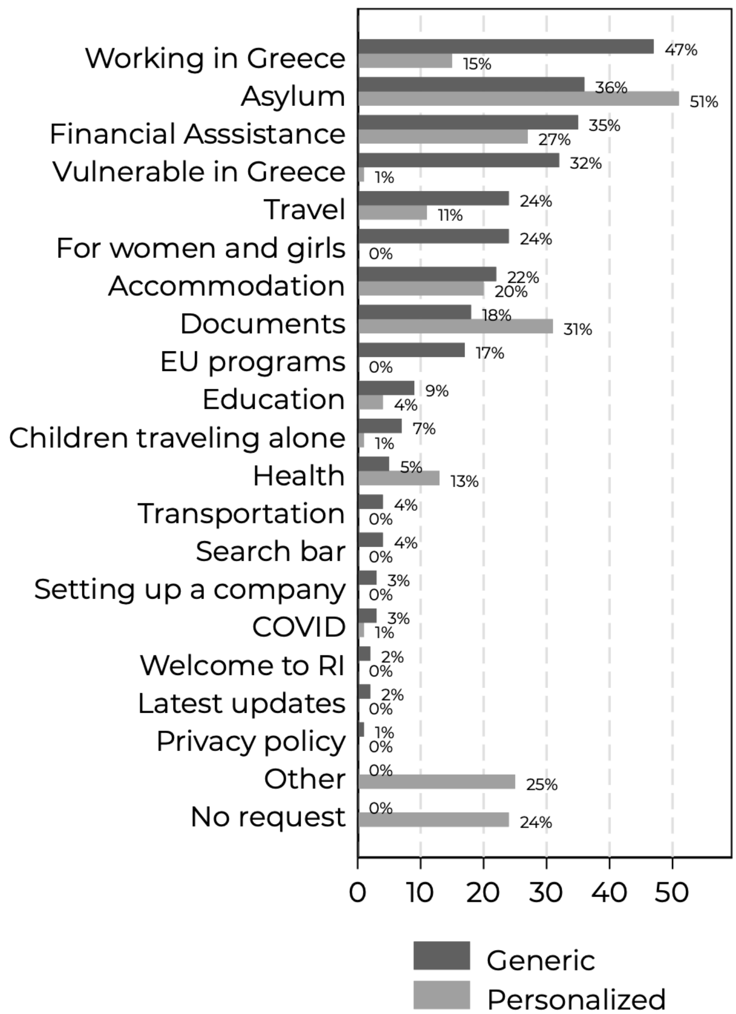


Figure A.6: Topics covered

G Two-sample Instrumental Variable Regressions

We don't fully observe take-up rates for batch 1 respondents from the personalized information group. Specifically, what is unobserved is whether or not they clicked on the links that were included in message 1. Nonetheless, the links were working for them–meaning that they would have been able to receive personalized information if they wanted to. This was solved after message 1 had been sent, meaning that we do observe take-up rates from messages 2 & 3 for batch 1 personalized information group. Batch 1 generic information group and the entire batch 2 was not affected by this technical issue.

Following Angrist and Krueger (1992)'s influential article, we conduct a two-sample instrumental variable estimation by taking advantage of the fully observable take-up rates from batch 2. Using Choi and Shen (2019)'s *weaktsiv* Stata package, we estimate the first-stage of the instrumental variable estimation using batch 1 (control and generic-information groups) and batch 2 (control, generic- and personalized-information groups), and then run the second-stage using the full sample (including batch 1 personalized information group). We use Pacini and Windmeijer (2016)'s robust standard errors. Table A.8 shows the results. The LATE estimates for the effects of personalized information on exploitation identification and responding to instances of exploitation are similar in magnitude to the ones shown in table A.4, which uses the full sample for both first- and second-stages. Standard errors are different given that in the two-sample IV we use Pacini and Windmeijer (2016)'s robust standard errors, while in the one-sample IV we use clustered standard errors at the household level.

Table A.8: Main outcomes (LATE). Treatment: Click rate. Using two-sample IV estimation.

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A. Second-stage regressions (Instrument == Pooled)							
Clicked	0.365^{**}	0.338^{**}	-0.163^{+}	-0.035	0.077	-0.063	0.040
	(0.113)	(0.115)	(0.084)	(0.106)	(0.096)	(0.123)	(0.087)
F stat. (first stage)	457.3	455.4	458.3	457.3	458.3	324.6	454.4
N	1707	1696	1706	1707	1706	1092	1700
Panel B. Second-stage regressions (Instrument == Generic information)							
Clicked	0.226^{*}	0.261^{**}	-0.136^{+}	0.082	0.066	-0.029	0.076
	(0.096)	(0.098)	(0.075)	(0.089)	(0.084)	(0.105)	(0.074)
F stat. (first stage)	567.2	564.8	567.2	567.2	567.2	386.6	562.4
N	1123	1118	1123	1123	1123	715	1120
Panel C. Second-stage regressions (Instrument == Personalized information)							
Clicked	0.567^{***}	0.461^{***}	-0.209^{*}	-0.184	0.098	-0.111	-0.002
	(0.125)	(0.128)	(0.095)	(0.120)	(0.109)	(0.133)	(0.096)
F stat. (first stage)	257.7	257.4	259.4	257.7	259.4	194.5	259.1
N	1152	1143	1151	1152	1151	730	1147

Notes: All models include a dummy variable for batch. Pacini and Windmeijer (2016)'s robust standard errors in parentheses. † 0.1 * 0.05 ** 0.01 *** 0.001

H Sustained Engagement

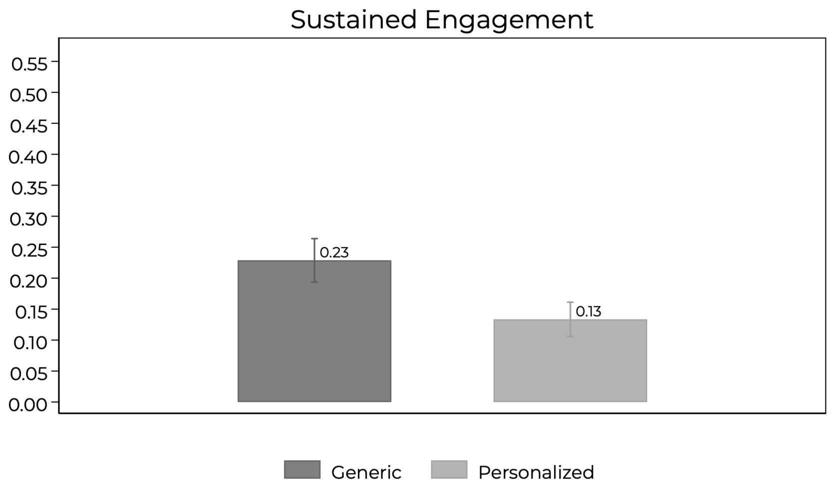


Figure A.7: Take-up of the intervention using the sustained engagement measure. Equal to one if respondents spent at least 60 seconds on the Refugee.Info website (generic treatment) or appeared in the user database of MIT (personalized treatment). 95% confidence intervals.

I Full pre-registered heterogeneity analysis

We expect the effect of information provision to be heterogeneous across specific sub-groups within our population. In particular:

Hypothesis 3 *We expect the effect for both Generic and Personalized information provision to differ between men and women.*

Table A.9: Heterogeneity analysis by gender

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A: ITT							
Panel A.1.: Pooled							
Men	0.192*	0.161+	-0.042	-0.007	-0.022	0.021	-0.009
	(0.088)	(0.089)	(0.069)	(0.080)	(0.072)	(0.103)	(0.070)
Women	0.121	0.132	-0.121	-0.028	0.132	-0.113	0.073
	(0.109)	(0.113)	(0.074)	(0.108)	(0.102)	(0.132)	(0.080)
Panel A.2.: Generic							
Men	0.146	0.155	-0.050	0.068	0.054	-0.014	-0.010
	(0.104)	(0.103)	(0.085)	(0.091)	(0.085)	(0.116)	(0.081)
Women	0.059	0.081	-0.095	0.002	-0.000	-0.015	0.125
	(0.121)	(0.130)	(0.087)	(0.123)	(0.119)	(0.152)	(0.092)
Panel A.3.: Personalized							
Men	0.235*	0.166	-0.035	-0.078	-0.093	0.052	-0.008
	(0.098)	(0.101)	(0.077)	(0.093)	(0.083)	(0.119)	(0.079)
Women	0.185	0.185	-0.147	-0.056	0.266*	-0.223	0.022
	(0.129)	(0.134)	(0.090)	(0.126)	(0.120)	(0.153)	(0.094)
Panel B: LATE(Clicked)							
Panel B.1.: Pooled							
Men	0.462*	0.387+	-0.100	-0.016	-0.053	0.046	-0.023
	(0.212)	(0.215)	(0.165)	(0.193)	(0.172)	(0.228)	(0.168)
Women	0.323	0.354	-0.322	-0.074	0.352	-0.268	0.196
	(0.289)	(0.303)	(0.199)	(0.289)	(0.273)	(0.315)	(0.214)
Panel B.2.: Generic							
Men	0.283	0.300	-0.097	0.132	0.105	-0.026	-0.019
	(0.201)	(0.201)	(0.165)	(0.178)	(0.164)	(0.221)	(0.158)
Women	0.126	0.174	-0.204	0.004	-0.001	-0.029	0.270
	(0.257)	(0.278)	(0.185)	(0.263)	(0.254)	(0.290)	(0.199)
Panel B.3.: Personalized							
Men	0.730*	0.514	-0.110	-0.241	-0.286	0.137	-0.026
	(0.310)	(0.313)	(0.239)	(0.290)	(0.258)	(0.312)	(0.243)
Women	0.655	0.661	-0.522	-0.201	0.945*	-0.708	0.078
	(0.460)	(0.489)	(0.324)	(0.447)	(0.441)	(0.493)	(0.335)
N (Men)	1081	1074	1080	1081	1080	688	1075
N (Women)	626	622	626	626	626	404	625

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Hypothesis 4 *We expect the effect for both Generic and Personalized information provision to be larger for asylum seekers than for refugees.*

Table A.10: Heterogeneity analysis by legal status

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A: ITT							
Panel A.1.: Pooled							
A. Seeker	0.154^{+}	0.269**	0.003	0.034	0.001	0.024	0.092
	(0.087)	(0.091)	(0.067)	(0.087)	(0.079)	(0.100)	(0.070)
BIP	0.180	-0.016	-0.188*	-0.084	0.084	-0.108	-0.087
	(0.112)	(0.109)	(0.079)	(0.093)	(0.088)	(0.135)	(0.080)
Panel A.2.: Generic							
A. Seeker	0.106	0.192^{+}	0.022	0.057	-0.012	0.008	0.040
	(0.102)	(0.106)	(0.081)	(0.100)	(0.092)	(0.115)	(0.083)
BIP	0.123	0.039	-0.194*	0.020	0.094	-0.050	0.033
	(0.128)	(0.126)	(0.098)	(0.106)	(0.106)	(0.150)	(0.089)
Panel A.3.: Personalized							
A. Seeker	0.199*	0.341**	-0.013	0.015	0.011	0.040	0.140^{+}
	(0.098)	(0.104)	(0.077)	(0.101)	(0.090)	(0.116)	(0.078)
BIP	0.236^{+}	-0.073	-0.184*	-0.186^{+}	0.066	-0.181	-0.206*
	(0.128)	(0.129)	(0.093)	(0.111)	(0.106)	(0.156)	(0.095)
Panel B: LATE (Clicked)							
Panel B.1.: Pooled							
A. Seeker	0.365+	0.638**	0.007	0.082	0.001	0.051	0.219
	(0.207)	(0.218)	(0.160)	(0.207)	(0.187)	(0.212)	(0.168)
BIP	0.486	-0.044	-0.505*	-0.227	0.225	-0.273	-0.234
	(0.302)	(0.295)	(0.216)	(0.252)	(0.237)	(0.340)	(0.214)
Panel B.2.: Generic							
A. Seeker	0.201	0.361+	0.041	0.108	-0.022	0.014	0.077
	(0.191)	(0.200)	(0.152)	(0.188)	(0.173)	(0.198)	(0.158)
BIP	0.272	0.086	-0.430*	0.043	0.208	-0.111	0.073
	(0.285)	(0.278)	(0.218)	(0.234)	(0.232)	(0.330)	(0.198)
Panel B.3.: Personalized							
A. Seeker	0.624*	1.066**	-0.042	0.047	0.035	0.107	0.440+
	(0.311)	(0.335)	(0.242)	(0.315)	(0.283)	(0.310)	(0.246)
BIP	0.814+	-0.253	-0.633+	-0.642+	0.227	-0.538	-0.711*
	(0.444)	(0.446)	(0.330)	(0.389)	(0.364)	(0.467)	(0.335)
N (A. Seeker)	1007	1003	1007	1007	1007	636	1004
N (BIP)	700	693	699	700	699	456	696

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Hypothesis 5 *We expect the effect for both Generic and Personalized information provision to be larger for respondents who have spent less time in Greece vs those who have already spent more time in the country.*

Table A.11: Heterogeneity analysis by time in Greece

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A: ITT							
Panel A.1.: Pooled							
Month < median	0.197^{+}	0.191^{+}	-0.081	-0.087	0.184*	0.140	0.021
	(0.100)	(0.100)	(0.073)	(0.090)	(0.086)	(0.120)	(0.072)
Month > median	0.132	0.112	-0.065	0.050	-0.110	-0.182^{+}	0.022
	(0.094)	(0.098)	(0.073)	(0.092)	(0.080)	(0.108)	(0.077)
Panel A.2.: Generic							
Month < median	0.146	0.289*	-0.049	0.044	0.127	0.161	0.037
	(0.117)	(0.117)	(0.090)	(0.102)	(0.103)	(0.135)	(0.087)
Month > median	0.079	-0.026	-0.085	0.040	-0.054	-0.170	0.035
	(0.108)	(0.112)	(0.088)	(0.105)	(0.093)	(0.124)	(0.087)
Panel A.3.: Personalized							
Month < median	0.249*	0.088	-0.114	-0.223*	0.244*	0.121	0.004
	(0.113)	(0.117)	(0.087)	(0.106)	(0.101)	(0.133)	(0.082)
Month > median	0.179^{+}	0.236*	-0.047	0.058	-0.159^{+}	-0.189	0.010
	(0.108)	(0.110)	(0.082)	(0.106)	(0.093)	(0.130)	(0.088)
Panel B: LATE (Clicked)							
Panel B.1.: Pooled							
Month < median	0.507+	0.493+	-0.209	-0.225	0.476*	0.328	0.055
	(0.259)	(0.260)	(0.190)	(0.233)	(0.222)	(0.279)	(0.185)
Month > median	0.319	0.272	-0.157	0.121	-0.266	-0.406+	0.052
	(0.229)	(0.237)	(0.175)	(0.222)	(0.193)	(0.241)	(0.187)
Panel B.2.: Generic							
Month < median	0.308	0.608*	-0.103	0.093	0.266	0.320	0.078
	(0.246)	(0.247)	(0.189)	(0.215)	(0.214)	(0.268)	(0.183)
Month > median	0.153	-0.049	-0.165	0.077	-0.103	-0.315	0.067
	(0.208)	(0.216)	(0.169)	(0.202)	(0.179)	(0.228)	(0.168)
Panel B.3.: Personalized							
Month < median	0.840*	0.297	-0.383	-0.750*	0.822*	0.347	0.014
	(0.383)	(0.395)	(0.298)	(0.361)	(0.342)	(0.379)	(0.277)
Month > median	0.566	0.746*	-0.149	0.184	-0.499+	-0.519	0.033
	(0.346)	(0.355)	(0.256)	(0.331)	(0.293)	(0.356)	(0.276)
N (Month < median)	816	811	816	816	816	514	812
N (Month > median)	891	885	890	891	890	578	888

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Hypothesis 6 *We expect the effect for both Generic and Personalized information provision to be larger for respondents living in camps than for respondents living outside of camps.*

Table A.12: Heterogeneity analysis by accommodation type

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A:ITT							
Panel A.1.: Pooled							
In camp	0.161	0.073	-0.063	-0.186	0.053	-0.055	-0.084
	(0.169)	(0.150)	(0.115)	(0.150)	(0.131)	(0.189)	(0.097)
Not in camp	0.165*	0.176*	-0.076	0.027	0.046	-0.021	0.032
	(0.075)	(0.079)	(0.058)	(0.072)	(0.064)	(0.089)	(0.060)
Panel A.2.: Generic							
In camp	0.110	0.022	-0.033	-0.095	0.077	-0.072	-0.054
	(0.191)	(0.172)	(0.135)	(0.166)	(0.158)	(0.212)	(0.115)
Not in camp	0.108	0.162+	-0.078	0.074	0.043	0.006	0.043
	(0.088)	(0.092)	(0.071)	(0.082)	(0.075)	(0.101)	(0.070)
Panel A.3.: Personalized							
In camp	0.205	0.130	-0.090	-0.285	0.036	-0.033	-0.123
	(0.189)	(0.178)	(0.136)	(0.174)	(0.154)	(0.229)	(0.114)
Not in camp	0.219*	0.189*	-0.075	-0.017	0.049	-0.048	0.021
	(0.086)	(0.091)	(0.066)	(0.083)	(0.075)	(0.102)	(0.069)
Panel B: LATE (Clicked)							
Panel B.1.: Pooled							
In camp	0.445	0.201	-0.174	-0.513	0.146	-0.130	-0.233
	(0.466)	(0.412)	(0.317)	(0.418)	(0.360)	(0.446)	(0.267)
Not in camp	0.402*	0.427*	-0.186	0.066	0.111	-0.047	0.077
	(0.183)	(0.193)	(0.140)	(0.174)	(0.156)	(0.201)	(0.146)
Panel B.2.: Generic							
In camp	0.220	0.044	-0.066	-0.189	0.154	-0.136	-0.108
	(0.379)	(0.340)	(0.268)	(0.329)	(0.312)	(0.397)	(0.229)
Not in camp	0.218	0.325+	-0.157	0.149	0.086	0.012	0.087
	(0.176)	(0.185)	(0.142)	(0.165)	(0.150)	(0.193)	(0.141)
Panel B.3.: Personalized							
In camp	0.918	0.580	-0.402	-1.276	0.161	-0.112	-0.545
	(0.873)	(0.802)	(0.605)	(0.830)	(0.683)	(0.763)	(0.504)
Not in camp	0.664*	0.573*	-0.226	-0.051	0.147	-0.131	0.063
	(0.262)	(0.278)	(0.201)	(0.252)	(0.227)	(0.276)	(0.208)
N (In camp)	355	355	355	355	355	201	353
N (Not in camp)	1352	1341	1351	1352	1351	891	1347

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Hypothesis 7 *We expect the effect for both Generic and Personalized information provision to be smaller for older refugees and asylum seekers than for younger refugees and asylum seekers.*

Table A.13: Heterogeneity analysis by age

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A: ITT							
Panel A.1.: Pooled							
Age < median	0.180^{+}	0.142	-0.071	-0.148	-0.068	-0.089	0.042
	(0.097)	(0.099)	(0.075)	(0.094)	(0.084)	(0.114)	(0.080)
Age > median	0.148	0.163^{+}	-0.075	0.106	0.131	0.025	-0.003
	(0.097)	(0.099)	(0.070)	(0.087)	(0.083)	(0.114)	(0.069)
Panel A.2.: Generic							
Age < median	0.122	0.141	-0.044	-0.098	-0.079	-0.099	0.024
	(0.114)	(0.118)	(0.093)	(0.106)	(0.099)	(0.128)	(0.094)
Age > median	0.107	0.121	-0.093	0.169^{+}	0.135	0.070	0.048
	(0.111)	(0.112)	(0.085)	(0.101)	(0.097)	(0.130)	(0.080)
Panel A.3.: Personalized							
Age < median	0.230*	0.143	-0.094	-0.191^{+}	-0.057	-0.081	0.057
	(0.109)	(0.113)	(0.086)	(0.109)	(0.096)	(0.132)	(0.089)
Age > median	0.192^{+}	0.207^{+}	-0.059	0.038	0.128	-0.023	-0.057
	(0.112)	(0.115)	(0.081)	(0.104)	(0.099)	(0.133)	(0.081)
Panel B: LATE (Clicked)							
Panel B.1.: Pooled							
Age < median	0.479+	0.376	-0.187	-0.393	-0.180	-0.224	0.112
	(0.260)	(0.264)	(0.200)	(0.252)	(0.225)	(0.286)	(0.213)
Age > median	0.350	0.386+	-0.178	0.249	0.308	0.052	-0.008
	(0.227)	(0.234)	(0.165)	(0.205)	(0.194)	(0.237)	(0.163)
Panel B.2.: Generic							
Age < median	0.250	0.286	-0.090	-0.201	-0.161	-0.204	0.050
	(0.233)	(0.239)	(0.189)	(0.216)	(0.204)	(0.264)	(0.193)
Age > median	0.211	0.240	-0.183	0.335+	0.267	0.126	0.095
	(0.219)	(0.222)	(0.168)	(0.200)	(0.192)	(0.232)	(0.159)
Panel B.3.: Personalized							
Age < median	0.834*	0.518	-0.339	-0.690+	-0.208	-0.255	0.208
	(0.404)	(0.413)	(0.310)	(0.399)	(0.349)	(0.412)	(0.323)
Age > median	0.567+	0.609+	-0.174	0.113	0.374	-0.056	-0.166
	(0.329)	(0.342)	(0.239)	(0.304)	(0.289)	(0.333)	(0.237)
N (Age < median)	830	824	829	830	830	547	826
N (Age > median)	877	872	877	877	876	545	874

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Hypothesis 8A Generic information effects are larger for people with higher levels of education.

Hypothesis 8B *Personalized information effects are larger for people with lower education.*

!

Table A.14: Heterogeneity analysis by education

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A: ITT							
Panel A.1.: Pooled							
Up to primary sch.	0.366**	0.191	-0.214*	-0.030	-0.038	0.104	0.100
	(0.126)	(0.120)	(0.088)	(0.107)	(0.105)	(0.130)	(0.083)
Post primary sch.	0.047	0.133	0.004	-0.000	0.077	-0.109	-0.026
	(0.081)	(0.086)	(0.064)	(0.081)	(0.071)	(0.102)	(0.067)
Panel A.2.: Generic							
Up to primary sch.	0.341*	0.112	-0.132	0.001	-0.015	0.151	0.151
	(0.148)	(0.140)	(0.108)	(0.120)	(0.123)	(0.144)	(0.098)
Post primary sch.	-0.005	0.142	-0.033	0.063	0.053	-0.103	-0.022
	(0.093)	(0.099)	(0.077)	(0.093)	(0.084)	(0.116)	(0.078)
Panel A.3.: Personalized							
Up to primary sch.	0.386**	0.257†	-0.281**	-0.055	-0.056	0.062	0.057
	(0.141)	(0.134)	(0.100)	(0.124)	(0.120)	(0.150)	(0.095)
Post primary sch.	0.104	0.124	0.042	-0.071	0.103	-0.116	-0.034
	(0.092)	(0.100)	(0.073)	(0.095)	(0.083)	(0.119)	(0.078)
Panel B: LATE (Clicked)							
Panel B.1.: Pooled							
Up to prim. sch.	0.949**	0.497	-0.557*	-0.077	-0.099	0.247	0.261
	(0.330)	(0.311)	(0.229)	(0.278)	(0.272)	(0.308)	(0.219)
Post-prim. sch.	0.116	0.325	0.009	-0.001	0.189	-0.243	-0.065
	(0.197)	(0.211)	(0.155)	(0.198)	(0.173)	(0.228)	(0.164)
Panel B.2.: Generic							
Up to prim. sch.	0.718*	0.236	-0.277	0.002	-0.031	0.311	0.320
	(0.315)	(0.295)	(0.227)	(0.251)	(0.258)	(0.296)	(0.209)
Post-prim. sch.	-0.010	0.278	-0.065	0.124	0.105	-0.190	-0.044
	(0.182)	(0.196)	(0.151)	(0.182)	(0.165)	(0.214)	(0.154)
Panel B.3.: Personalized							
Up to prim. sch.	1.231**	0.817+	-0.897**	-0.175	-0.177	0.170	0.185
	(0.461)	(0.436)	(0.327)	(0.396)	(0.383)	(0.408)	(0.304)
Post-prim. sch.	0.342	0.411	0.137	-0.235	0.338	-0.331	-0.110
	(0.304)	(0.332)	(0.240)	(0.314)	(0.272)	(0.342)	(0.255)
N (Up to primary sch.)	620	615	620	620	620	388	618
N (Post primary sch.)	1087	1081	1086	1087	1086	704	1082

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

J Complier average treatment effects

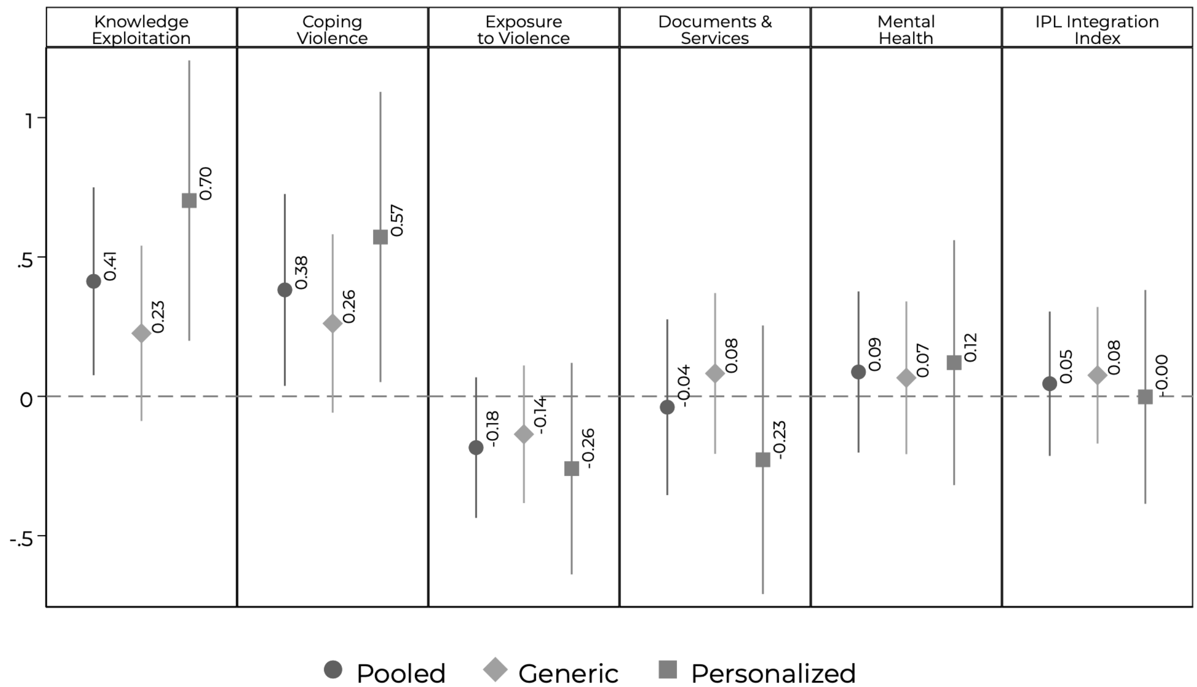


Figure A.8: Complier average treatment effects. 95% confidence intervals. Standard errors are clustered at the household level. See Appendix M for further results.

K Complier Analysis

This analysis explores how compliers differ from always-takers (people who engage with the information regardless of their encouragement) and never-takers (people who do not engage with the information regardless of their encouragement). This assessment is important to understand which groups of refugees our encouragement fails to reach and permits us to gauge the generalizability of our findings, for example, if a stronger encouragement (with financial incentives) would be able to convert some of the remaining never-takers into compliers (Marbach and Hangartner, 2020). Leveraging the pre-treatment refugee characteristics measured in the baseline survey, we can profile compliers and non-compliers. Given the small share of always-takers (2.1%, or 12 out of 568 respondents from the control group), i.e. refugees and asylum seekers who reach out to one of the two service providers even if assigned to the control group, estimates for this group will lack statistical precision. We therefore put our focus on comparing compliers with never-takers, which make up 211 and 928 respondents, respectively, of the sample. We use the methodology proposed by Marbach and Hangartner (2020) to profile compliers and non-compliers in terms of their background characteristics.

The results, presented in Figure A.9, suggest that compliers and never-takers are similar in terms of gender, age, time in Greece and legal status. Furthermore, differences in instances of exploitation, the likelihood of living in camps, and mental distress are small and not statistically significant. The only significant difference pertains to compliers' higher levels of education. Turning to non-pre-registered comparisons, we find that compliers and never-takers have equal probabilities of being employed and of experiencing discrimination. There is some evidence suggesting that compliers are slightly better integrated and are more likely to plan their future in Greece than those who do not.

Together, the analysis suggests that people who choose to engage with legal information and those who do not are similar in most background characteristics. Based on this, we might expect that if a stronger encouragement increased the compliance rate (by turning some of the never-takers into compliers) the CATE for these additional compliers might be similar to the estimates obtained for the sample analyzed here. The fact that education is correlated with take-up provides some additional evidence that the cost of accessing legal information, which may be higher for people with lower levels of education, plays a role in shaping engagement.

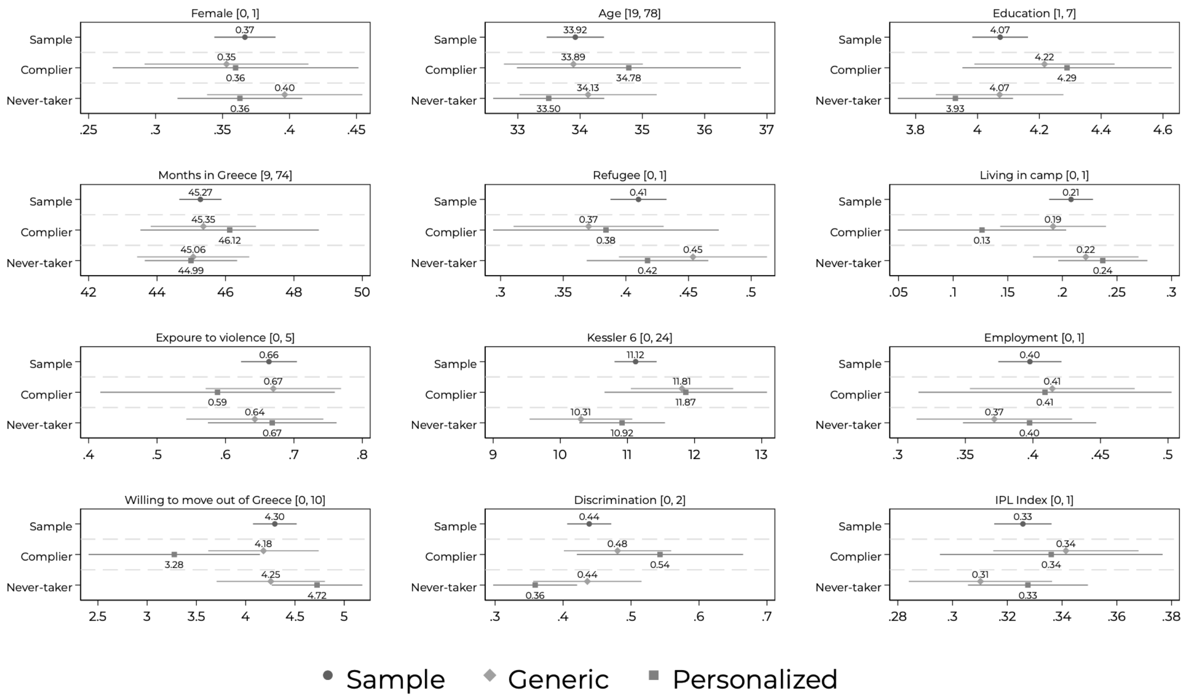


Figure A.9: Descriptive statistics (mean and 95% bootstrap confidence intervals) for the complier and noncomplier subpopulations of asylum seekers and refugees in Greece who took part in the post-treatment survey. *Education, willingness to move out of Greece, discrimination* and *IPL index* were not pre-registered

L Mechanisms

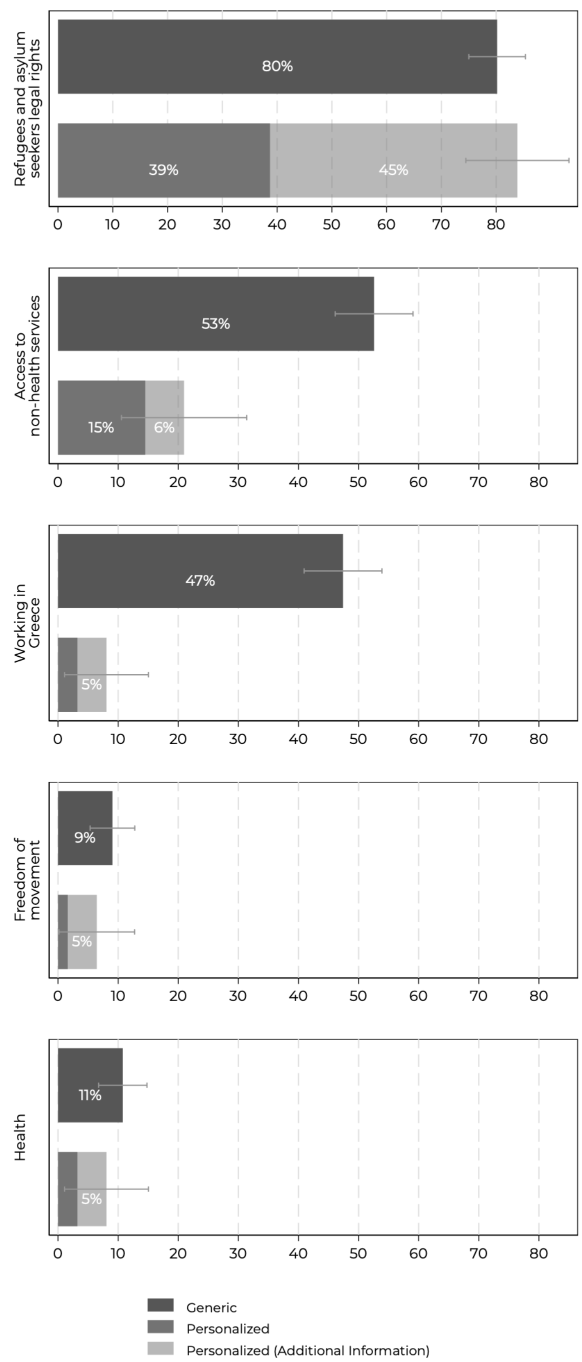


Figure A.10: Descriptive comparison of topics and content covered in generic (darkest grey) and personalized treatments. Medium grey indicates information about topics covered in the personalized treatment that is also available on the Refugee.Info website, light grey covers information not available on Refugee.Info. 95% confidence intervals.

L.1 Feeling Index

To understand whether human connection can help to explain the difference between personalized and generic legal information, we asked whether using this information made refugees and asylum seekers feel listened to (+1) / connected (+1) / hopeful (+1) / nothing (0) / down (-1).

Figure A.11 shows the results.[5]

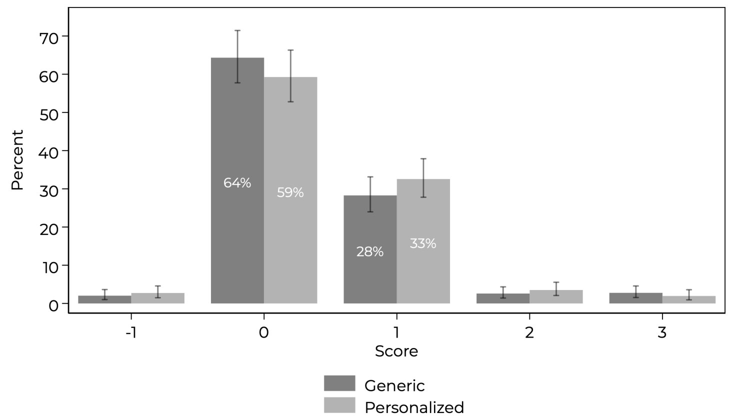


Figure A.11: Descriptive comparison of the "feeling index", summarizing the degree to which respondents felt being "heard", between refugees who took the generic (darker grey) and (lighter grey) personalized treatment. 95% confidence intervals using the Poisson distribution.

[5]This figure shows the results for all respondents who report having used one of these services, independent of the treatment they were assigned to. Appendix figure A.12 provides estimates for those respondents who complied with the encouragement, measured again using a click on the bit.ly link. The findings are very similar.

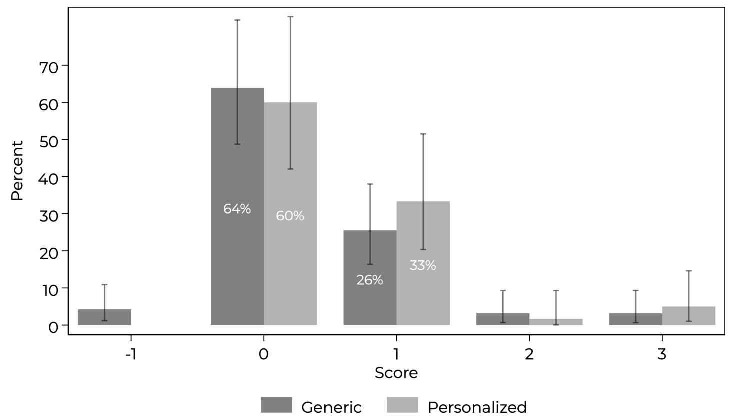


Figure A.12: Descriptive comparison of the “feeling index”. This figure only includes respondents from the treatment groups who used the assigned information platform. 95% confidence intervals using the Poisson distribution.

M Tables from figures in the paper

Table A.15: Take-up.

	(1) Engagement	(2) Sustained Engagement
Generic	0.495***	0.229***
	(0.022)	(0.018)
Personalized	0.307***	0.134***
	(0.020)	(0.015)
Constant	-0.000	0.000
	(0.000)	(0.000)
N	1707	1707

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.16: Intention-to-treat effects

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A: Pooled treatments							
Pooled	0.165*	0.153*	-0.074	-0.016	0.035	-0.031	0.018
	(0.069)	(0.070)	(0.052)	(0.064)	(0.059)	(0.081)	(0.053)
batch=2	0.025	0.044	0.005	0.035	-0.195***	0.066	0.130**
	(0.064)	(0.066)	(0.051)	(0.061)	(0.058)	(0.075)	(0.050)
Constant	-0.132*	0.048	-0.479***	0.159*	-0.433***	-0.037	0.142**
	(0.065)	(0.066)	(0.048)	(0.062)	(0.055)	(0.077)	(0.050)
N	1707	1696	1706	1707	1706	1092	1700
Panel B: Generic information							
Generic	0.112	0.130	-0.068	0.041	0.033	-0.015	0.037
	(0.080)	(0.081)	(0.063)	(0.073)	(0.070)	(0.091)	(0.062)
batch=2	0.051	0.076	-0.003	0.030	-0.154*	0.180*	0.121^{+}
	(0.080)	(0.081)	(0.063)	(0.073)	(0.070)	(0.091)	(0.062)
Constant	-0.145*	0.033	-0.475***	0.161*	-0.453***	-0.093	0.146**
	(0.070)	(0.071)	(0.052)	(0.066)	(0.058)	(0.081)	(0.054)
N	1123	1118	1123	1123	1123	715	1120
Panel C: Personalized information							
Personalized	0.216**	0.176*	-0.080	-0.070	0.037	-0.047	-0.001
	(0.078)	(0.081)	(0.059)	(0.075)	(0.069)	(0.093)	(0.060)
batch=2	0.006	0.036	-0.008	-0.013	-0.242***	-0.022	0.122*
	(0.078)	(0.081)	(0.059)	(0.075)	(0.069)	(0.093)	(0.060)
Constant	-0.123^{+}	0.052	-0.473***	0.182**	-0.410***	0.007	0.146**
	(0.069)	(0.071)	(0.051)	(0.066)	(0.058)	(0.082)	(0.053)
N	1152	1143	1151	1152	1151	730	1147

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.17: Main outcomes (LATE). Treatment: Click rate on generic and personalized information combined (pooled treatments).

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A. Second-stage regressions (Instrument == Pooled)							
Clicked	0.413*	0.382*	-0.184	-0.039	0.087	-0.070	0.045
	(0.172)	(0.176)	(0.129)	(0.161)	(0.147)	(0.184)	(0.132)
batch = 2	-0.015	0.006	0.023	0.038	-0.204***	0.073	0.125*
	(0.066)	(0.068)	(0.052)	(0.062)	(0.059)	(0.076)	(0.051)
Constant	-0.113+	0.067	-0.488***	0.157**	-0.428***	-0.040	0.144**
	(0.060)	(0.061)	(0.044)	(0.057)	(0.050)	(0.071)	(0.046)
Panel B. First-stage regressions							
VARIABLES	Clicked	Clicked	Clicked	Clicked	Clicked	Clicked	Clicked
Instr. (pooled)	0.400***	0.401***	0.401***	0.400***	0.401***	0.438***	0.400***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.019)	(0.015)
batch = 2	0.097***	0.099***	0.098***	0.097***	0.098***	0.088***	0.099***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.025)	(0.020)
Constant	-0.047***	-0.048***	-0.047***	-0.047***	-0.047***	-0.043***	-0.048***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)	(0.010)
F stat. (first stage)	433.8	431	434.1	433.8	434.1	305.3	431.6
N	1707	1696	1706	1707	1706	1092	1700

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.18: Main outcomes (LATE). Treatment: Click rate on generic information.

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A. Second-stage regressions (Instrument == Pooled)							
Clicked	0.226	0.261	-0.136	0.082	0.066	-0.029	0.076
	(0.160)	(0.163)	(0.126)	(0.147)	(0.140)	(0.175)	(0.125)
batch = 2	0.033	0.055	0.007	0.024	-0.159*	0.181*	0.115+
	(0.080)	(0.082)	(0.062)	(0.073)	(0.070)	(0.091)	(0.062)
Constant	-0.136*	0.043	-0.480***	0.164**	-0.450***	-0.094	0.149**
	(0.066)	(0.067)	(0.049)	(0.062)	(0.055)	(0.078)	(0.051)
Panel B. First-stage regressions							
VARIABLES	Clicked	Clicked	Clicked	Clicked	Clicked	Clicked	Clicked
Instr. (RI)	0.497***	0.498***	0.497***	0.497***	0.497***	0.522***	0.496***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.027)	(0.022)
batch = 2	0.077***	0.080***	0.077***	0.077***	0.077***	0.059**	0.079***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.027)	(0.021)
Constant	-0.037***	-0.039***	-0.037***	-0.037***	-0.037***	-0.029**	-0.038***
	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)	(0.013)	(0.010)
F stat. (first stage)	349.7	348.5	349.7	349.7	349.7	244.7	346.9
N	1123	1118	1123	1123	1123	715	1120

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.19: Main outcomes (LATE). Treatment: Click rate on personalized information.

	(1) Know. Exploit.	(2) Cope Violence	(3) Exposure Violence	(4) Docs. &. Serv.	(5) K6	(6) Digit Span	(7) IPL Index
Panel A. Second-stage regressions (Instrument == Pooled)							
Clicked	0.702**	0.572*	-0.260	-0.228	0.121	-0.133	-0.002
	(0.257)	(0.266)	(0.194)	(0.246)	(0.224)	(0.262)	(0.196)
batch = 2	-0.044	-0.005	0.011	0.003	-0.251***	-0.013	0.122*
	(0.080)	(0.084)	(0.061)	(0.076)	(0.070)	(0.095)	(0.061)
Constant	-0.099	0.072	-0.482***	0.174**	-0.406***	0.002	0.146**
	(0.064)	(0.066)	(0.048)	(0.061)	(0.054)	(0.077)	(0.049)
Panel B. First-stage regressions							
VARIABLES	Clicked	Clicked	Clicked	Clicked	Clicked	Clicked	Clicked
Instr. (MIT)	0.307***	0.307***	0.308***	0.307***	0.308***	0.357***	0.308***
	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)	(0.025)	(0.019)
batch = 2	0.071***	0.072***	0.072***	0.071***	0.072***	0.070***	0.072***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.026)	(0.020)
Constant	-0.034***	-0.035***	-0.035***	-0.034***	-0.035***	-0.034***	-0.035***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)	(0.010)
F stat. (first stage)	204.8	202.2	205	204.8	205	159.7	203.9
N	1152	1143	1151	1152	1151	730	1147

Notes: All models include a dummy variable for batch. Standard errors in parentheses are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

M.1 Tables from figures in the Appendix

Table A.20: Take-up.

	(1) Attrition
Control	0.546***
	(0.014)
Generic	0.557***
	(0.014)
Personalized	0.534***
	(0.014)
N	3755

Notes: Robust standard errors in parentheses. † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.21: Attrition analysis using variables from baseline survey

	(1) ITV 2	(2) ITV 2	(3) ITV 2
Generic	-0.012	-0.007	0.061
	(0.020)	(0.020)	(0.116)
Personalized	0.012	0.012	0.188
	(0.020)	(0.020)	(0.116)
batch=2	-0.052**	-0.049**	-0.048**
	(0.017)	(0.017)	(0.017)
Age		0.003**	0.004*
		(0.001)	(0.002)
Woman		-0.002	-0.007
		(0.017)	(0.029)
Contact Attempts		-0.033***	-0.023
		(0.008)	(0.016)
Employed		-0.007	-0.031
		(0.025)	(0.044)
Docs. & Serv.		0.008	0.013
		(0.009)	(0.014)
Kessler 6		-0.001	0.002
		(0.001)	(0.002)
Coping Violence		0.013	0.025
		(0.012)	(0.021)
Knowledge Exp.		0.013	0.011
		(0.010)	(0.017)
Exposure Violence		-0.005	-0.043**
		(0.009)	(0.015)
IPL Index		0.239***	0.298**
		(0.060)	(0.103)
Generic×Age			0.000
			(0.002)
Personalized×Age			-0.004
			(0.002)
Generic×Woman			0.021
			(0.042)
Personalized×Woman			-0.002
			(0.042)
Generic×Contact Attempts			-0.011
			(0.020)
Personalized×Contact Attempts			-0.016
			(0.021)
Generic×Employed			0.024
			(0.062)
Personalized×Employed			0.049
			(0.062)
Generic×Docs. & Serv.			-0.015
			(0.021)
Personalized×Docs. & Serv.			-0.003
			(0.021)
Generic×Kessler 6			-0.006
			(0.003)
Personalized×Kessler 6			-0.003
			(0.004)
Generic×Coping Violence			-0.002
			(0.030)
Personalized×Coping Violence			-0.032
			(0.030)
Generic×Knowledge Exp.			0.008
			(0.025)
Personalized×Knowledge Exp.			-0.001
			(0.025)
Generic×Exposure Violence			0.065**
			(0.023)
Personalized×Exposure Violence			0.052*
			(0.022)
Generic×IPL Index			-0.105
			(0.145)
Personalized×IPL Index			-0.064
			(0.147)
p-value (joint sign.)			0.0001
R-Squared	0.003	0.023	0.028
Interactions			✓
N	3755	3729	3729

Notes: Variable values are from the baseline survey. All models include a dummy variable for batch. Standard errors are clustered at the household level. † 0.1 * 0.05 ** 0.01 *** 0.001

Profiling

Table A.22: Profiling - Female

	(1)	(2)	(3)
Complier	0.352***	0.353***	0.360***
	(0.025)	(0.031)	(0.047)
Never-taker	0.377***	0.396***	0.363***
	(0.018)	(0.030)	(0.024)
Sample	0.367***	0.375***	0.362***
	(0.012)	(0.014)	(0.014)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.23: Profiling - Age

	(1)	(2)	(3)
Complier	34.178***	33.892***	34.783***
	(0.466)	(0.569)	(0.917)
Never-taker	33.755***	34.129***	33.496***
	(0.360)	(0.562)	(0.455)
Sample	33.923***	34.012***	33.891***
	(0.235)	(0.275)	(0.283)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.24: Profiling - Education

	(1)	(2)	(3)
Complier	4.204***	4.217***	4.289***
	(0.098)	(0.116)	(0.173)
Never-taker	3.987***	4.071***	3.928***
	(0.071)	(0.105)	(0.095)
Sample	4.073***	4.143***	4.039***
	(0.046)	(0.056)	(0.055)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.25: Profiling - Months in Greece

	(1)	(2)	(3)
Complier	45.643***	45.353***	46.122***
	(0.665)	(0.784)	(1.330)
Never-taker	45.018***	45.056***	44.991***
	(0.505)	(0.836)	(0.689)
Sample	45.267***	45.203***	45.338***
	(0.315)	(0.405)	(0.418)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.26: Profiling - Refugee

	(1)	(2)	(3)
Complier	0.377^{***}	0.370^{***}	0.384^{***}
	(0.025)	(0.031)	(0.046)
Never-taker	0.432^{***}	0.454^{***}	0.417^{***}
	(0.019)	(0.030)	(0.025)
Sample	0.410^{***}	0.412^{***}	0.407^{***}
	(0.011)	(0.015)	(0.015)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.27: Profiling - Living in camp

	(1)	(2)	(3)
Complier	0.174^{***}	0.191^{***}	0.126^{**}
	(0.020)	(0.025)	(0.039)
Never-taker	0.231^{***}	0.221^{***}	0.237^{***}
	(0.016)	(0.025)	(0.021)
Sample	0.208^{***}	0.207^{***}	0.203^{***}
	(0.010)	(0.012)	(0.012)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.28: Profiling - Exposure to violence

	(1)	(2)	(3)
Complier	0.672^{***}	0.670^{***}	0.588^{***}
	(0.045)	(0.050)	(0.087)
Never-taker	0.658^{***}	0.643^{***}	0.668^{***}
	(0.035)	(0.051)	(0.048)
Sample	0.664^{***}	0.656^{***}	0.644^{***}
	(0.021)	(0.025)	(0.026)
N	1706	1123	1151

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.29: Profiling - Kessler 6

	(1)	(2)	(3)
Complier	11.802^{***}	11.813^{***}	11.869^{***}
	(0.332)	(0.389)	(0.617)
Never-taker	10.670^{***}	10.307^{***}	10.921^{***}
	(0.251)	(0.390)	(0.326)
Sample	11.121^{***}	11.053^{***}	11.212^{***}
	(0.160)	(0.190)	(0.187)
N	1706	1123	1151

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.30: Profiling - Employment

	(1)	(2)	(3)
Complier	0.414^{***}	0.414^{***}	0.409^{***}
	(0.025)	(0.031)	(0.048)
Never-taker	0.387^{***}	0.371^{***}	0.398^{***}
	(0.019)	(0.029)	(0.025)
Sample	0.398^{***}	0.393^{***}	0.401^{***}
	(0.012)	(0.015)	(0.015)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.31: Profiling - Willing to move out of Greece

	(1)	(2)	(3)
Complier	3.951^{***}	4.181^{***}	3.275^{***}
	(0.238)	(0.286)	(0.444)
Never-taker	4.527^{***}	4.255^{***}	4.725^{***}
	(0.183)	(0.281)	(0.235)
Sample	4.296^{***}	4.218^{***}	4.277^{***}
	(0.113)	(0.144)	(0.136)
N	1551	1036	1039

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.32: Profiling - Discrimination

	(1)	(2)	(3)
Complier	0.511^{***}	0.480^{***}	0.543^{***}
	(0.034)	(0.040)	(0.062)
Never-taker	0.390^{***}	0.436^{***}	0.359^{***}
	(0.024)	(0.041)	(0.031)
Sample	0.438^{***}	0.458^{***}	0.415^{***}
	(0.017)	(0.021)	(0.019)
N	1706	1123	1151

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Table A.33: Profiling - IPL Index

	(1)	(2)	(3)
Complier	0.334^{***}	0.341^{***}	0.336^{***}
	(0.011)	(0.014)	(0.021)
Never-taker	0.320^{***}	0.310^{***}	0.328^{***}
	(0.008)	(0.013)	(0.011)
Sample	0.326^{***}	0.326^{***}	0.330^{***}
	(0.005)	(0.007)	(0.006)
N	1707	1123	1152

Notes: Standard errors based on asymptotic theory are estimated. . † 0.1 * 0.05 ** 0.01 *** 0.001

Feelings

Table A.34: Feelings - All

Treatment	Score	Mean	SE	LB	UB	Level
Generic	-1	2.03	0.01	1.02	3.64	95
Generic	0	64.33	0.03	57.74	71.45	95
Generic	1	28.28	0.02	23.98	33.13	95
Generic	2	2.59	0.01	1.41	4.34	95
Generic	3	2.77	0.01	1.55	4.57	95
Personalized	-1	2.73	0.01	1.49	4.58	95
Personalized	0	59.26	0.03	52.78	66.31	95
Personalized	1	32.55	0.03	27.80	37.88	95
Personalized	2	3.51	0.01	2.08	5.55	95
Personalized	3	1.95	0.01	0.93	3.58	95

Notes: Generic N = 541; Personalized N = 513

Table A.35: Feelings - Based on treatment group assignment and reported usage

Treatment	Score	Mean	SE	LB	UB	Level
Generic	-1	4.26	0.02	1.16	10.90	95
Generic	0	63.83	0.08	48.71	82.16	95
Generic	1	25.53	0.05	16.36	37.99	95
Generic	2	3.19	0.02	0.66	9.33	95
Generic	3	3.19	0.02	0.66	9.33	95
Personalized	-1	0.00	0.00	0.00	0.00	95
Personalized	0	60.00	0.10	42.02	83.07	95
Personalized	1	33.33	0.07	20.36	51.48	95
Personalized	2	1.67	0.02	0.04	9.29	95
Personalized	3	5.00	0.03	1.03	14.61	95

Notes: Generic N = 94; Personalized N = 60

Table A.36: Topics covered (by treatment group)

Topic	Treatment	Mean	SE	LB	UB	Level
Access to non-health services	Generic	52.59	0.03	46.11	59.06	95
	Personalized (total)	20.97	0.05	10.55	31.39	95
	Personalized (standard)	14.52	0.05	5.50	23.53	95
	Personalized (additional)	6.45	0.03	0.16	12.74	95
Freedom of movement	Generic	9.05	0.02	5.33	12.77	95
	Personalized (total)	6.45	0.03	0.16	12.74	95
	Personalized (standard)	1.61	0.02	-1.61	4.84	95
	Personalized (additional)	4.84	0.03	-0.66	10.33	95
Health	Generic	10.78	0.02	6.76	14.80	95
	Personalized (total)	8.06	0.03	1.09	15.04	95
	Personalized (standard)	3.23	0.02	-1.30	7.75	95
	Personalized (additional)	4.84	0.03	-0.66	10.33	95
Refugees and asylum seekers legal rights	Generic	80.17	0.03	75.00	85.34	95
	Personalized (total)	83.87	0.05	74.45	93.29	95
	Personalized (standard)	38.71	0.06	26.24	51.18	95
	Personalized (additional)	45.16	0.06	32.42	57.90	95
Working in Greece	Generic	47.41	0.03	40.94	53.89	95
	Personalized (total)	8.06	0.03	1.09	15.04	95
	Personalized (standard)	3.23	0.02	-1.30	7.75	95
	Personalized (additional)	4.84	0.03	-0.66	10.33	95

Notes: Generic N = 232; Personalized N = 62

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