



Investment in identity in the field-Nudging refugees' integration effort[☆]

Nora Grote, Tim Klausmann^{*}, Mario Scharfbillig

Johannes Gutenberg University of Mainz, Germany

ARTICLE INFO

JEL classification:

C93
D91
J15

Keywords:

Social identity
Identification preferences
Social preferences
Field experiment
Refugees

ABSTRACT

Social identity greatly affects behavior. However, less is known about an individual's investment in identification, i.e., in belonging to a social group. Using a language-learning platform utilized by refugees to learn the host country's language, we design a field experiment that allows us to make effort as an investment in a new group identity salient. The social identity in our treatment is a refugee's identification with the host society. We modified a mailing to 5600 refugees who use an online language-learning platform to learn the host country's language. These treatment emails make salient that improving the host country's language ability increases the belonging to the host society. Our analysis reveals that the treatment has a significant positive effect on the effort exerted on the language-learning platform, leading to more completed exercises and more time spent learning the host country's language. This suggests that refugees invest in becoming part of the host country's society for its social identity component. Our findings can inform policy considerations on the use of nudges for other integration measures intended for refugees and immigrants in general.

1. Introduction

Exploring further, the verb “to identify” is a necessary accompaniment of identity. There is something active about identity that cannot be ignored: it is not ‘just there’, it is not a ‘thing’, it must always be established.

[(Jenkins, 2008, p. 18)]

Identity is one of the most powerful social phenomena driving human behavior. A central question around identity is how individuals establish their identity. In general, identifying with social groups is human nature and an evolutionarily stable way to facilitate cooperation and coordination within groups. However, when individuals face a new environment with a different common identity, are people willing to invest in belonging to this identity? We tackle this question with refugees in their new host country.

[☆] We would like to thank Steffen Altmann, Alexander Cappelen, Florian Hett, Yan Chen, Uri Gneezy, Daniel Schunk, Sigrid Suetens, Bertil Tungodden, and Karin Johnston as well as the participants at the Spring School in Behavioral Economics in San Diego, the Workshop on Microeconomics in Lüneburg, the Current Topics in Public Sector Research Conference in Brno, the Microeconomics Seminar in Riezlern, the IMEBESS (2018) conference in Florence, the ESA World Meeting (2018), the 13th NCBEE in Odense, the COMPIE (2018) in Berlin, the Ph.D. course Intergenerational Economics in Frankfurt, the ZEW Workshop on the Economics of Immigration (2018) and the Advances with Field Experiments (2018) conference in Boston for helpful discussions and valuable feedback. This work was formerly released under the title “Preference for Identification in the Field-Nudging Refugees Integration Effort”. We are grateful for the opportunity to work with the LinguaTV team and obtain their unique data. We received financial support from the research priority program Interdisciplinary Public Policy (IPP) at the University of Mainz.

^{*} Correspondence to: Jakob-Welder-Weg 4, 55128 Mainz, Germany.

E-mail address: tim.klausmann@uni-mainz.de (T. Klausmann).

Much research has shown that established social identities have an independent and substantial effect on behavior, such as public good contributions (Benjamin et al., 2016; Canelo et al., 2017; Charness et al., 2014), outgroup discrimination (Hoff and Pandey, 2006), test performance (Benjamin et al., 2010; Hoff and Pandey, 2014), honesty (Cohn et al., 2014, 2010), altruism (Chen and Li, 2009) and cooperation (Chen et al., 2014). Social identities are multiple within each society (e.g., “German”, “academic”, “female”, “immigrant”), but only a few identities are salient to a person at any point in time (Shayo, 2009). Furthermore, studies point out that identification is always something active and subject to change (Akerlof and Kranton, 2000; Jenkins, 2008; Shayo, 2020). While such research has brought us far in understanding that individuals invest in social identification, less is known about an individual's actual investment in establishing identification in a new social group. In addition, much of the previous work on exploring whether participants are willing to spend resources to change or stay in their social group(s) was examined in a lab setting (Hargreaves Heap and Zizzo, 2009; Hett et al., 2020).

Refugees¹ present an interesting case for investigating the importance of social identities and how individuals invest in becoming part of a group in real life. In contrast to established identities as discussed above, refugees at the time of arrival in a host country are clearly faced with a new social identity setting. By investigating the large refugee influx in the EU in 2015 (see Brückner et al., 2016; Kroh et al., 2017; Brückner et al., 2018), we seek to address the question of whether individuals invest in a new identity with a policy relevant population. We test in a real-life setting whether refugees are willing to invest effort to belong to the host country's social identity. In the context of this study, we treat belonging to a host country's society as a social identity and investment in learning the host country's language as an investment in a stronger belonging to that identity.

Membership in a group is a cognitive process by which people self-identify with the group (Tajfel, 1974). The two key factors determining identification are the social status of a group and the perceived similarity of a person to group members (Akerlof and Kranton, 2000). Social status is important because people not only receive utility from consumption, but also from esteem gained from peers (Akerlof, 2017). Similarity relies on social categorization (Haslam, 1997), which allows people to define their surrounding in an us vs. them construct using a number of relevant categories (Tajfel, 1978). Although a person may choose to which group she belongs (Akerlof and Kranton, 2000), identification is not automatic and may require some investment or change in behavior for it to be credible to the self and to others. A refugee may, for example, need to invest in group-specific skills and symbols, such as wearing clothes in the fashion of the group, reading (local) newspapers or, what this paper investigates, learning the host country language.

An important factor for the success of the refugee integration process is learning the host country's language, both as a means of learning about customs and as an end in itself (Clots-Figueras and Masella, 2013; Hicks et al., 2015). Investigating changes in effort invested in learning a new language is particularly interesting because it is a way to improve interaction efficiency and because it is valued by the local population (?). This allows for increasing acceptance within the host society, making it easier for the individual to identify and to be identified by others (Bauer et al., 2005; Dustmann, 1994; Georgiadis and Manning, 2013; Jenkins, 2008).

From observational data alone, it is often not possible to differentiate the reasons individuals decide to be part of a group. In the case of language learning of refugees or migrants, the challenge is to separate the identity investment from multiple other reasons why immigrants might invest in learning the host country's language, the most obvious being the economic benefits of becoming employed and receiving higher wages (Chiswick and Miller, 1995; Dustmann et al., 2003; Lazear, 1999; Mcmanus et al., 1983). With our experiment, we try tackle the issue by attempting to separate the social identity incentive for investments in language learning from other incentives. We make use of an experimental setting involving a large number of newly arrived refugees. We designed an email treatment for users of an online language learning platform, which contrasts a neutral framing with an identity framing. The identity framing aims to make salient that, all else being equal, investing in learning the host country's language increases the sense of belonging to the host society.

Refugees have the option to increase their own group's relative status (e.g., by succeeding by their group's stereotypical norms, values, and behavior, e.g., along religious norms if religion differs) or become more similar to the native population by learning the local language(s). Learning the language arguably increases the similarity. Thus, refugees with better language skills can engage in “social free-riding” (Bernard et al., 2016), i.e., they can receive the social status that the local population enjoys.

In designing the study, we utilized a data set of refugees already registered on an online language-learning platform. The emails we crafted informed all users that the platform was newly available as a mobile application. Apart from one paragraph in the email and the email subject line, all participants received the same email text. Additionally, all platform users received as reminders two slight variations of the first email. Our treatment has two separate arms. One identity treatment arm informed participants in the subject line and body of the mailing that by learning German they could “become part of the German community” (identity gain treatment). The second treatment arm stressed the participants could “remain part of the German community” (identity loss treatment). In the treatment arms, we also mentioned that learning the language helps to increase their belonging to the group (i.e., the host country's society). By doing so, we identified the effect of an identity-framed nudge within an email on language learning.

Furthermore, we first argue why the identity-framed nudge might have only manipulated the identity dimension and, in doing so, allowed us to isolate this dimension of learning the language. Secondly, we claim that language learning is a group-specific investment (Clots-Figueras and Masella, 2013). Given that both these assumptions hold, our treatment effect measures the effect of an additional investment motivated by identity concerns about becoming part of a group. Moreover, our intervention comes at

¹ By the term refugee we refer to a group of migrants that requested asylum in their current country of residence independent of their current legal status. This group consists of people who are still awaiting a decision on their status as well as those with different granted legal statuses and rejected asylum applicants.

close to no cost as additional text in an email is free, which makes a cost-benefit calculation in case of a positive treatment effect trivially positive.

Several theoretical drivers could lead to an effect of our intervention. On the one hand, our treatment would act through a perceived encouraged social interaction of treated refugees with society. From this point, refugees have a strong interest to conform to the newly salient group, Germans, incentivizing them to increase language learning. This might be driven by the desire for peer esteem (Akerlof, 2017), which depends on the achievement in an activity valued by the peer group, in this case language ability. Alternatively, our treatment makes salient that learning German reduces the distance to the dominant peer group, German society (Shayo, 2009; Bernard et al., 2016), which in turn reduces the disutility from social distance. With our experiment we cannot distinguish between these drivers.

Our findings show that our intervention succeeds in significantly increasing some aspects of language-learning behavior, and that we find consistently positive point estimates. We also observe an increased likelihood of activities such as: opening the email; clicking a link to the platform as well as to more new logins; completed exercises; points achieved; and more time spent on the platform after four weeks after sending the first treatment mail. Our results reveal relevant increases in learning; for example, learning time on average doubles relative to the control group after four and eight weeks such that the treatment group learns five additional minutes. This effect is driven by learners who used the learning platform prior to our intervention. Given the assumptions discussed above hold, our results suggest that refugees invest effort in being part of the host country's society. While we cannot observe the process by which participants were selected for our experiment, using post-treatment survey data we can show our sample seems to be representative for the refugee influx in Germany around 2015 for a large set of variables, the only exception being that our sample is on average more educated. Finally, we also find no significant difference between the identity gain and identity loss treatment.

This paper contributes to several strands in the literature. First, we contribute to the empirical literature on identity in economics, as recently summarized by Shayo (2020) and Charness and Chen (2020). By showing that individuals actively invest in language learning through a group identity treatment, we provide evidence for endogenous identity formation and a preference for identification. This means that identities may be optimized, flexible, and subject to an individual's history (Akerlof and Kranton, 2000; Jenkins, 2008; Tajfel, 1974, 1978; Tajfel and Turner, 1986). Our setting is especially relevant given that we are the first to experimentally investigate the effect of an identity not assigned by an experimenter on longer term behavior outside the lab.

Closely related to our work is Hett et al. (2020). They show that individuals sacrifice monetary payoff in order to belong to a group based on certain group characteristics and interpret this as a preference for identification. During the experiment, status differences are induced by a quiz performance split, such that groups consisting of better performers may be more desirable. Hargreaves Heap and Zizzo (2009) show that people are willing to trade group memberships at a premium, from which they conclude that there exists a "psychological benefit" for groups in an interpersonal trust game setting. Where they differ from our work is that both papers are laboratory experiments using lab measures and self-chosen identity outside the lab (i.e. football club fandom). Furthermore, both Hargreaves Heap and Zizzo (2009) and Hett et al. (2020) measure identity choice using short-term monetary investments, while we look at investments in terms of effort over time. Our intervention further differs from prior work by targeting an identity which cannot easily be claimed by paying a fee. The difference between lab and the field is important in this case, as Hoff and Pandey (2006) have shown that the predictions from the identity literature on out-group punishment from the lab do not hold for lower caste members in India. Additionally, in the lab experiments above, individuals switch groups in exchange for monetary payoff and thus endure a cost of not being identified with what they would prefer for the duration of the experiment.

Our setting differs in two further dimensions. First, learning a new language is a long-term investment in identity; therefore, the participants do not automatically switch the group upon learning. This may reduce the incentive to change behavior as the participants face low and incremental changes in identity and at the same time their other social identity (refugee) which may frown upon the language investment. Second, language learning works as an investment in what others identify the participant with and what they identify themselves with in only one dimensions of identification (others being clothing, style, adherence to traditions and customs, etc.) further reducing the incentive to invest.

Our research also contributes to the growing literature on integration of immigrants and in particular the role of identification with the host country's society. The effects of a migrant's self-assigned identity on integration have received increasing attention in recent years (Constant et al., 2009; Epstein and Gang, 2010; Epstein and Heizler, 2015) and are found to be relevant to outcomes in the labor market (Bisin et al., 2016, 2011; Manning and Roy, 2009). However, this line of research relies mostly on survey evidence, correlating identification with important economic outcomes such as labor market performance (Cameron et al., 2015). They usually find that higher identification with the host country's society and societal values has a positive association. Some studies also find negative associations because subjects may choose oppositional identities,² where actions towards others' group norms are repellent (Austen-Smith and Fryer, 2005; Battu and Zenou, 2009; Fryer and Torelli, 2010; Bisin et al., 2011; Fordham and Ogbu, 1986; Ogbu, 1999; Patacchini and Zenou, 2016; Bisin and Verdier, 2000; Dehdari and Gehring, 2017; Monscheuer, 2018). In contrast to this work, our study aims at identifying a causal link and suggests that on average refugees want to belong to the host country's society, thus providing evidence against a widespread oppositional identity formation for this specific immigrant group in Germany.³

² Also called "acting white" in the context of African American integration (Austen-Smith and Fryer, 2005).

³ In line with the argument above by Akerlof (2017), refugees presented with the choice of valuing German language ability more through our intervention do on average choose to learn more and not reject the German peer group's values.

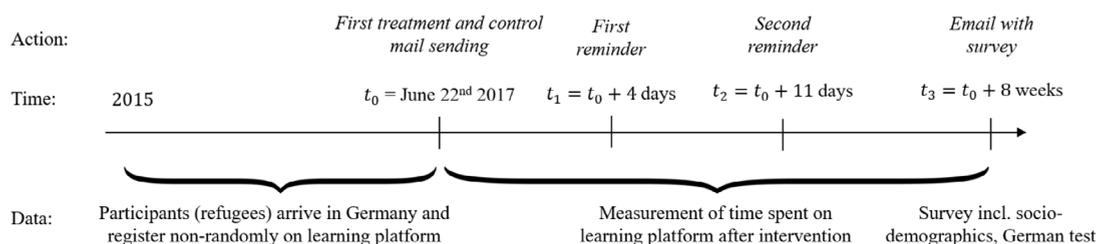


Fig. 1. Intervention design.

Note: The figure provides an overview of our intervention, including data collection, timing, and the elements of our intervention.

We also provide evidence that the effort invested in a group identity can be nudged (Thaler and Sunstein, 2008).⁴ We directly communicate with the users of a language-learning platform via email, prompting them to change their behavior depending on their preferences for identification. Importantly, this nudge can only work if refugees have not previously optimized over their choice of identities, and when the investment in learning the language is not their best response already. In fact, in their study Hoff and Pandey (2014) conclude that the choice of identity is frame-dependent, which means that participants were not aware of all dimensions of their identity at all times. Therefore, making the identity-building component of language learning salient may change behavior. Our findings can inform policy on nudging for other integration measures aimed at immigrants in general.

Finally, our identity intervention is framed as a potential loss for one third of our sample and as a potential gain for another third, based on the vast literature showing the effect of reference dependent preferences in which losses loom larger than gains (Kahneman and Tversky, 2011; Tversky and Kahneman, 1991). There are numerous effects consistent with this finding, such as the endowment effect (Thaler, 1980). In field experiments, the additional motivation effect of loss framing is found to be successful in motivating teachers (Fryer et al., 2012; Levitt et al., 2016), marketing messages (Bertrand et al., 2010), or worker productivity (Hossain and List, 2012).⁵ One could argue that refugees do not own the new identity, however, Kőszegi and Rabin (2007) would posit that expectations can count as reference points towards which individuals can exhibit loss aversion. With our specific subject pool of newly arrived refugees, we cannot support such loss aversion regarding the identity dimension.

2. Study design

2.1. Participants and context

During the height of the refugee influx in Germany in 2015, the language-learning platform we partnered with had donated several thousand licenses, worth three-months of learning German,⁶ to the Federal Employment Agency for distribution to refugees. The distribution of these licenses was neither random nor tracked in any way. Our participants were 5600 refugees who received one of the donated licenses and successfully registered on the learning platform. In June 2017, we sent emails informing our participants about the platform being newly available as a mobile application (compare Fig. 1). Before we sent our treatment emails, activity on the language-learning platform was low, and 89% of the licenses had already expired.⁷ We extended or renewed all licenses for the time span of our identity intervention and observation period of two months.

On the platform, the language courses are organized similarly to a language textbook. Courses include visual, audio, and textual elements and consist of a large number of videos showing conversations in German. Each language lesson concludes with exercises and tests. The learner can repeat or skip lessons and follow their own speed of learning. The learning platform is available as a desktop version and, at the time of our intervention, as a new mobile application, which differs from the desktop version only in the presentation style.

⁴ In their review article, Damgaard and Nielsen (2017) conclude that in the field of education, reminders frequently show robust positive effects. Additionally, reminders can be effective for real effort tasks and habit formation. Calzolari and Nardotto (2016) find that their reminders not only lead to short term actions but achieve real-effort outcomes. In a gym setting, they find signs of habit formation. In our context, this finding would translate to continuous language learning instead of a short spike in learning only.

⁵ While early results like Banks et al. (1995) find large advantages of loss-framing of health-related messages, recent work established small effects on health outcomes, and overview articles show even opposing results. O'Keefe and Jensen (2009) argue in line with Kahneman and Tversky (2011) and find small advantages of loss-framed messages compared to gain-framed ones. Gallagher and Updegraff (2012) find larger effects for gain-framed messages. Karlan et al. (2016, 2012) analyze savings behavior and cannot detect differences between loss- and gain-framing in reminder messages.

⁶ Such three-month licenses are sold to private customers for €89.70.

⁷ Some users were able to renew their account by typing in the same donation key again after it expired. Less than 1% of users found this loophole and used it.

2.2. Treatment

We designed three different emails sent to equally sized groups. We stratified over usage before the intervention and the date and time of registration to the platform. More specifically, we randomized triplet-wise, drawing three individuals and assigning them randomly to one of the treatments in order to improve the balancing of our treatment assignment. Ex-post randomization checks confirm no significant differences between our treatment and control groups (see Table A1 in Appendix).⁸

Our treatment consists of the email subject and content.⁹ The control group received a baseline email, which only informed participants about the new opportunity to learn on the mobile application and the two new months of free usage. A second group (“identity gain”) received the same information, but we added: “Become part of the German community” at the end of the subject line and the following paragraph to the email:

Become part of the German community: Learning German will help you become part of the German society. It will allow you to connect with others and help you feel at home.

In a similar way, the third group (“identity loss”) received an email including the following paragraph:

*Stay part of the German community: Learning German will help you stay part of the German society. It will ensure you stay connected and do not feel isolated.*¹⁰

We sent these emails to the same individuals three times, whereby the second and third mailings were marked as being reminders in the subject line. Additionally, for all three groups the second email contained the phrase: “Learning German helps you find a job”, while the third email contained the phrase: “Learning German is important”. The primary language of the emails was German, with translations to Arabic, Persian, and English below. On top of the email, one could click on the name of each language to go directly to the corresponding part of the email. As almost all licenses were expired prior to our treatment and users only learned about the extension through our emails, we are confident that all effects work directly through our emails.

With the treatment emails, we aim to nudge effort investment in language learning. Given our setup, the mailings were discussed with the research community and phrased carefully to get as close to a pure identity nudge that is readable and strong in the respective languages. Economic benefits may nevertheless be implied. Note that the control group also receives an email reminding them of the language-learning opportunity, which itself implies a potential value to it. This means that economic benefits only interfere with our treatment effect if the treatment part of the message makes economic benefits additionally salient. Further, the first reminder email mentions the economic prospects of language learning to all participants. In Section 3.5, we make use of the full data structure and the reminders to argue why the salience of the economic dimension does not likely increase between control and treated groups.

It could be further questioned whether refugees who arrived two years before the study (in 2015) can be nudged with an identity loss intervention. In other words, can they lose something they do not yet own? These refugees arrived in the midst of the “Refugees Welcome” movement (Connolly, 2015). Hence, it seems likely that they experienced relatively intense initial interaction, for example, with volunteers, allowing them to feel well-integrated. Improving language skills is central for keeping their gained social connections and thus their standing within the host society. However, in many situations our participants are still treated as refugees when receiving a donated license for the language-learning platform. Empirically, we find no significant difference between the identity gain treatment and the identity loss treatment. Therefore, we combine both these treatments into an average treatment effect in most analysis, while providing the distinction in Appendix.

2.3. Predictions

Given the survey evidence gathered from the refugee influx into Europe in 2015 (Brücker et al., 2016), it seems plausible that most refugees in Germany do indeed want to be part of German society. However, they also receive a profusion of new and conflicting information and have different experiences with German society and its institutions, which makes the wish to integrate potentially less salient. Therefore, making it salient to them — i.e., that learning the language increases their sense of belonging — should nudge them into exerting more effort to learning German. Additionally, coming to Germany might be evidence of a specific country choice. This leads to our conservative hypothesis:¹¹

H1: *An identity-framed nudge increases the effort invested in learning the host country’s language.*

As discussed above, we argue that the effect we find is due to the refugees’ desire to belong to the society even absent any economic benefits. On the one hand, we argue that our intervention can be interpreted as information on identity formation by making the difference in language skills to the peer group of the German society more salient. On the other hand, we understand language learning as a specific investment that is most valuable in the culture in which it is spoken and can be understood as an investment in this culture’s identity as valued by German society. Therefore, our extended hypothesis is:

H2: *An identity intervention increases investment in acquiring the identity of the host country.*

⁸ The share of females differs marginally ($p < 10\%$). In our regressions, we control for all available variables, including gender.

⁹ Printouts of the mailings can be found in Appendix C.

¹⁰ We keep the kernel state, “the basic, root state mentioned in the message’s description of the consequence” (O’Keefe and Jensen, 2009) constant over treatment arms. This simply means that we refrain from using negations such as: “Not learning German will hinder your integration”. We keep the action (learning) constant, while the consequence depends on the frame (become vs. stay part of Germany).

¹¹ We preregistered the treatment and outcome variables on [socialsciregistry.org](https://www.socialsciregistry.org) with the ID AEARCTR-0002270, as Grote et al. (2017).

Table 1
Descriptives.

	N	Mean	Median	SD	Min	Max
<i>Control variables</i>						
Months since registration	5573	13.176	14	4.344	2	24
Active before treatment (d)	5573	0.549	1	0.498	0	1
Female (d)	5572	0.256	0	0.436	0	1
<i>Mailings</i>						
Opened (d)	5570	0.660	1	0.474	0	1
Clicked (d)	5434	0.224	0	0.417	0	1
<i>Learning behavior before treatment</i>						
Logins	5573	20.439	6	43.373	1	1125
Exercises	5573	94.953	1	269.275	0	3481
Points	5573	5755.625	72	16197.524	0	215057
Learn time	5573	405.410	12	1142.477	0	15517

Note: Months since registration measures the time a learner was registered on the platform before the first intervention email was sent. Active before treatment is a dummy variable that takes the value of 1 when a learner spent time on the platform before the intervention and 0 otherwise. The dummy variable Female takes the value of 1 when learner *i* is female and is 0 otherwise. Opened is a dummy variable that takes the value of 1 when the learner opened at least one of the intervention emails. Clicked is a dummy equal to 1 if the learner clicked at least once on a link to the language learning platform presented in the intervention emails. Logins is the number of times the learner logged on to the learning platform, Exercises reports how many exercises a learner completed. Points refers to how many points the learner collected, and Learn time to how much time the learner spent on the platform (in minutes), all measured by the platform prior to the intervention.

3. Results

3.1. Data and descriptive results

Overall, we employ three sources of data. First, we observe an individual's activity on the language-learning platform. For a period of three months, we obtained cross-sectional data extracts from the backend of the platform. This data includes gender (self-assigned), registration dates on the platform, and information on the learning activity (number of logins, number of exercises started, and points achieved per unit).

Second, another data source stems from the mailings itself. We observe email bounces,¹² openings,¹³ and interactions with the links in the email (clicks). The data on mailings and platform usage cover all 5600 individuals in our sample. Table 1 shows descriptive statistics obtained from the backend of the language learning platform and the mailings. First participants registered two years prior to our intervention. About one quarter (25.6%) of our sample is female.

Lastly, eight weeks after our intervention, we sent an extensive questionnaire to all participants in our sample. Like the treatment, the survey was administered in four languages: German, English and the two most common mother tongues, Arabic and Farsi. We incentivized survey participation by a lottery offering prizes.¹⁴ This survey provided us with information on demographics, life events, employment history and status, ethnic identity and identification, locus of control, and German skills for a subsample of our treatment population. The German skills were measured using a short and standardized German test developed by the Goethe Institute, the reference institution for German language learning. Out of our original sample, 8.9% ($n=496$) filled the survey completely (14% started the survey). We provide a detailed presentation of the data generated from this survey in Appendix A. We conclude that those who replied to our survey are similar to the population of refugees who arrived in 2015 and 2016 as surveyed by Brücker et al. (2018). However, in comparison to that sample, we observe on average better educated refugees. We do not discuss the null effects on language skills and labor market outcomes in the results section as our treatment effect on language learning was significant, but too small to expect a meaningful effect on language skills and therefore also on economic outcomes.

To examine whether our treatments do affect learning behavior on the language platform, we will consider four different outcome measures. Firstly, we consider the number of logins to the platform, indicating whether and how often the language learning platform was accessed by a participant. Prior to our intervention, participants on average logged in 20 times, the median participant logged in six times, and a few participants logged in more than 500 times. On the platform, a learner can either solve exercises or study new elements by watching videos or listening to audio recordings. We consider the number of exercises an individual completed as well as the number of points an individual gained by solving exercises. Beyond counting interactions, points received for working on an exercise depend on the correctness of the answer and the complexity of the exercise. As a last indicator for learning behavior on the platform, we use learning time, which captures the amount of time an individual spent on the online language learning platform in general. Learning time, logins, and exercises are therefore measures for effort, while points additionally measure achievement.

¹² Our emails were successfully sent to almost all the registered users in our sample with a low bounce rate (2.55%).

¹³ This measure is not perfect, as we only observe the opening of an email for some email client and user settings. Therefore, this measure is a lower bound.

¹⁴ The lottery was framed as a gift handed out only to some participants because of potential sensitivities of conservative Muslims towards lotteries (Falk et al., 2018).

On average, individuals completed 95 exercises, gained 5756 points, and spent 6 h and 45 min on the platform before the start of our mailing intervention. Of the participants registered on the platform, 45% never started learning. To control for previous activity in our analysis we create a dummy variable, which is equal to one of the participants registered on the platform who completed at least one exercise (active before treatment). Activity during the four weeks prior to our intervention was very low. Only 194 individuals (3.5% of our sample) logged in at all and gained on average 141 points. This is consistent with previous findings that the use of voluntary online learning is low in general (Escueta et al., 2017), although for our study this is mainly due to the expired licenses in our sample.

3.2. Estimation

Online learning data in general — like our data — exhibits large variances due to many learners with low activity and few that are very active. Therefore, of major concern is the precision of our estimates, especially of statistical confidence. To tackle this, we rely on estimating our treatment effect using OLS regressions to identify partial treatment effects instead of mean comparisons in most of the following analysis. To judge the significance of our results, we first compute heteroscedasticity-robust Huber–White standard errors I (MacKinnon and White, 1985) to take into account the distribution of our outcome variables. These should be a conservative estimate of our treatment effect that ignores the precision gains from triplet-wise stratification. Secondly, we compute standard errors derived from randomization inference (Heß, 2017) that take the triplet-wise stratification into account. With these standard errors, we do not rely on asymptotic properties of classic inference. For interpretation we rely on the conservative estimates but report the p-values using randomization inference (RI) as well as robust standard errors (SE) for transparency.¹⁵

From the platform backend, we do have several observations over time for each outcome measure that allow us to compute the effects after different time periods. For brevity, we report the effect of our intervention in regression analyses only midway, after four weeks, and at our final observation (eight weeks after the first email).¹⁶ We therefore regress model (1), using one of the two points in time for identifying the treatment effect:

$$y_{i,post} = \alpha + \beta identity + \gamma y_{i,pre} + \sum_k \delta_k X_{i,k} + \epsilon_i \quad (1)$$

The dependent variable $y_{i,post}$ represents a reaction to the mailing on learning behavior in the platform (e.g. number of exercises four weeks after the intervention started). The variable $identity$ is our treatment indicator. The indicator $identity$ is equal to one for an individual receiving any kind of identity-framed email. Following McKenzie (2012), we include the individual value of the dependent variable prior to our intervention $y_{i,pre}$ where available. This increases the precision of estimates compared to using variation after the start of the intervention only. $X_{i,k}$ are the control variables gender, months since registration on the platform, and an indicator for any activity before the intervention. Adding these controls further reduces noise in the dependent variable and thus increases the precision of our treatment estimates. Thus, in the following we will always report the results that include all available controls and report the less precise results without controls in the appendix.

3.3. Results of the intervention

We first discuss the immediate reactions to our intervention. To document a direct response to the identity framed nudge, we analyze three outcomes: email opening, which can vary due to the treatment in the subject line; clicking on one of the links in the email to download the application or open the platform in a browser; and whether a participant logged in to the platform. Note that this is not necessarily the path participants need to go to learn more. Participants who keep the platform open in a browser window might just switch to it, neither using a link nor logging in. Others might react by visiting the platform later without using the link. Participants who ever used a remember-me-cookie might use the link without logging in. Lastly, device switches might cause us to observe an email opening without usage of the link. As a baseline, 66% of the participants opened at least one of our emails, and 22.4% used the link in the mailing to reach the platform (see Table 1). This is an exceptionally high rate, compared to other studies using email interventions (e.g. Chen et al., 2018), and might indicate a high interest in the language-related emails.

The first two columns of Table 2 aggregate the effect of the treatment email and its reminders, compared to the control emails. The last column aggregates first logins over a period covering the three mailings and two additional weeks in which we potentially attribute logins to our mailings.¹⁷ We find that the identity treatment increases the likelihood of an email being opened by 8.1 percentage points ($p < 0.01$ for SE and RI) and the click rate by 2.3 percentage points ($p < 0.1$ for SE; $p < 0.05$ for RI). The identity treatment also leads to 1.3 percentage points ($p < 0.1$ for SE and RI) more logins. Given that, overall, 9.2% of our sample logged in within eight weeks after our intervention. This implies that users in the identity treatments are 14% more likely to log in compared

¹⁵ Both standard errors are comparable in size; however, contrary to the suggestions from econometric theory (Heß, 2017), the randomization inference standard errors are slightly larger for some regressions.

¹⁶ Figure A1 shows that the effects are consistent at all other points in time.

¹⁷ When analyzing the first email and the two reminders separately, we see that only the first email had a significant treatment effect on both opening the email and clicking on the link, while the reminders were insignificant.

Table 2
Reactions to mailings.

Variables	(1) Opened (d)	(2) Clicked (d)	(3) New login 4 weeks (d)
Identity treatment (d)	0.081*** (0.014)	0.023* (0.012)	0.013* (0.007)
Months since registration	-0.001 (0.001)	0.000 (0.001)	-0.009*** (0.001)
Active before treatment (d)	0.038*** (0.013)	0.025** (0.011)	0.079*** (0.007)
Female (d)	-0.048*** (0.015)	-0.060*** (0.012)	-0.020** (0.008)
Constant	0.608*** (0.024)	0.210*** (0.021)	0.147*** (0.015)
Observations	5,569	5,433	5,572
R-squared	0.010	0.005	0.041
RI <i>p</i> -value of ident. treat.	0	0.046	0.067
RI SE of <i>p</i> -value	0	0.0021	0.0025
RI repetitions	10000	10000	10000

Note: The dependent variable is a dummy variable that takes the value of 1 when the learner opened at least one of the intervention emails in column (1), when the learner clicked at least once on a link to the language learning platform presented in the intervention emails in column (2), and when the learner logged onto the learning platform at least once within four weeks of the intervention in column (3). The explanatory variable of main interest is identity treatment, which takes the value of 1 if learner *i* participated in the identity treatment and is 0 otherwise (control group — reminder only). Months since registration measures the time a learner was registered on the platform before the first intervention email was sent. Active before intervention is a dummy variable that takes the value of 1 when a learner spent time on the platform before the intervention and 0 otherwise. The dummy variable Female takes the value of 1 when learner *i* is female and is 0 otherwise. Coefficient estimates are from an ordinary least squares estimations. Asterisk and the standard errors in parentheses are heteroscedasticity-robust Huber–White standard errors (* significant at 10%; ** sign. at 5%; *** sign. at 1%). Additionally, randomization inference-based *p*-values, their standard error, and the number of permutations to compute these *p*-values are reported for the identity treatment dummy at the bottom of the table. This takes into account the triple-wise stratification of the intervention.

to the control treatment. Overall, we conclude that the identity-framed subject line and email body induce users to open more emails, click more on the link, and login more to the platform.¹⁸

Next, we report our findings regarding our measures for effort spent on language learning, the main variables of interest. Fig. 2 shows descriptively the percentage of people having newly logged-in over time and the mean for our learning behavior variables for treatment and control group during the intervention period. We observe that individuals receiving the identity-framed email spent more time, earned more points, and started more exercises than individuals in the control group.¹⁹

We estimate treatment effects and standard errors using model (1). The first three columns in Table 3 report the effect on learning behavior after four weeks, the last three columns report the results measured eight weeks after our first email was sent. We consistently find positive effects on all measures available: after four weeks, the treatment groups learned 5.4 min longer than the control group on average ($p < 0.01$ for SE and RI). This means 334 extra hours of learning overall (i.e., from all treated individuals together). In a regression without controls, our effects are qualitatively the same but not significant, see appendix Table A4. However, our baseline estimation controls for a substantial amount of noise introduced by heterogeneity of learners. When adding controls, our standard errors decrease dramatically by factor ten on average, but the point estimates change only marginally (e.g., learn time 4.7 vs. 5.4 min). In our baseline regression, the number of exercise and total points increased significantly on the 5% level using robust standard errors and on the 10% level using randomization inference. Regarding the treatment effect on exercise count and points measures eight weeks after the intervention, standard errors increase, and point estimates shrink. Both positive point estimates are independent of the estimation method not statistically different from 0. At this point in time, individuals in the treatment groups learn on average 8.3 min more compared to the control group. Taken together, we see that identity-framed emails have a positive effect on effort spent learning the host country's language. However, for points, more closely resembling achievement, the positive effect seems to fade out over time.²⁰

The extent to which participants in the identity gain treatment and the identity loss treatment groups react to our mailings is very similar, albeit individuals in the loss treatment group seem to react slightly stronger (compare Figure A2). However, the

¹⁸ We also tested for conditional effects of the treatment on clicking the link and logging in if subjects opened the email. Again, our treatment could work, even if individuals do not open an email, due to the subject line. Hence, the decision to open the email may be induced by the treatment itself and therefore could be endogenous. For clicking on a link in the email, we find no significant treatment effects, but we do find that the probability of logging in at least once during four weeks after the first email increases by 2.4% in the treatment conditional on having opened the email (see Table A3).

¹⁹ The recording of the variable learnTime was changed two weeks prior to our intervention by our cooperation partner, which prevents us from showing a prior trend of it.

²⁰ The marginal effects of our treatment at all other points in time are summarized in Figure A1 in the appendix. We also report robustness checks using winsorizing as well as trimming (both 1st and 99th percentile) in Tables A7 and A8.

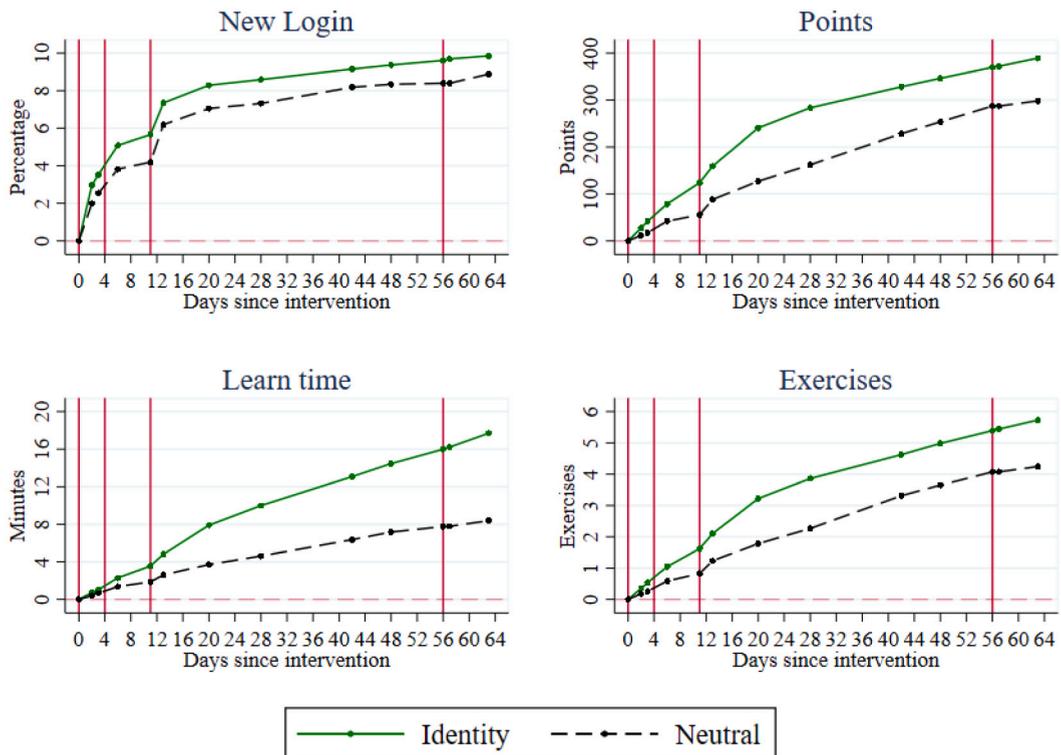


Fig. 2. Learning behavior over time.

Note: The figure reports the aggregate Learning behavior over the whole intervention period. Percentage of New Logins is the fraction of learners who logged on to the learning platform after the intervention (left top); New Points is the number of points the learner collected after the first mail (top right); New Learn Time is how much time the learner spent on the platform (in minutes) after the first mail (bottom left); and New Exercises reports how many exercises a learner completed after the first mail (bottom right). Red vertical lines mark dates on which we send emails to all participants.

difference between the identity gain treatment and the identity loss treatment is not statistically significant across all discussed outcome variables (compare Table A9). This is in line with other evidence showing that loss framing in a foreign language had no different effect from gain framing (Keysar et al., 2012), at least for those who were not addressed in their mother tongue.

The average absolute effect size of our treatment seems small, with few extra minutes and 1.6 additional exercises completed. Nevertheless, given that overall usage of the platform is low, the increase in activity through the identity framed emails is meaningful in relative terms: During the first four weeks after our identity intervention, treated individuals learn on average 2.1 times as long as individuals in the control group (9.9 min vs. 4.6 min) and complete 1.7 times as many exercises (3.9 vs 2.3, see Table A2 in the appendix).

We interpret these results as support for H1. The identity framed nudge increases opening rates and learning time significantly and with large relative effect sizes, while other measures point in the same direction but are not always significantly different from 0.

3.4. Results in the context of identity

While we can comfortably say that H1 holds, an important conceptual question is whether it is due to the willingness to invest in belonging to the host society’s identity for identity’s sake. Our experimental frame only makes belonging salient in the treatment group relative to the control group. As mentioned in the introduction, refugees have an incentive to self-identify with a social identity for the peer induced valuing of German, self-esteem and status or through peer pressure this identity brings, which is the mechanism we are implying. However, it is not clear whether this salience of belonging has implied other benefits as well. The strongest threat to our conclusion is that, while our intervention states “learning increases identification”, participants perceive “learning helps” only and attach a different reason to this. This might even be more likely for refugees, who arrive in a new country and receive a lot of new information because of new surroundings, norms, and customs. The most prominent alternative explanation that would explain an investment is economic prospects. Refugees may not be aware of the best path for successful economic integration in the host country. By sending them a message with information on language learning, we make salient that learning German is potentially one way to do so. This may nudge them to learn more German without being directly related to identity.

We cannot provide a complete answer based on our main experimental variation to counter this argument. However, for all participants in treatment and control group, our second mailing (reminder) contains a sentence that helps us tentatively separating

Table 3
Learning behavior.

Variables	(1)–(3) After 4 weeks			(4)–(6) After 8 weeks		
	Exercises	Points	Learn time	Exercises	Points	Learn time
	Identity treatment (d)	1.571** (0.753)	119.281** (54.931)	5.390*** (1.811)	1.294 (1.190)	79.907 (78.872)
Months since registration	−0.762*** (0.164)	−48.309*** (10.664)	−2.466*** (0.491)	−0.966*** (0.208)	−60.295*** (13.571)	−3.368*** (0.664)
Active before treatment (d)	1.982*** (0.693)	145.512** (60.112)	5.657*** (1.416)	3.211*** (0.977)	212.082*** (73.946)	8.091*** (2.692)
Female (d)	−0.318 (1.002)	−22.091 (77.493)	−0.580 (2.510)	−0.875 (1.321)	−45.514 (95.991)	−1.039 (4.010)
Dep. var. before interv.	1.015*** (0.003)	1.015*** (0.004)	1.010*** (0.002)	1.022*** (0.005)	1.021*** (0.005)	1.018*** (0.005)
Constant	9.939*** (2.117)	641.055*** (139.802)	29.971*** (6.171)	13.238*** (2.705)	856.849*** (175.723)	40.540*** (8.195)
Observations	5,572	5,572	5,572	5,572	5,572	5,572
R-squared	0.988	0.980	0.995	0.976	0.969	0.989
RI <i>p</i> -value of ident. treat.	0.062	0.057	0.0081	0.27	0.33	0.011
RI SE of <i>p</i> -value	0.0024	0.0023	0.00090	0.0045	0.0047	0.0010
RI repetitions	10000	10000	10000	10000	10000	10000

Note: The dependent variables report what learners achieved on the learning platform in the first four weeks after the first intervention email was sent in columns (1)–(3) and in the first eight weeks after the first intervention email was sent in columns (4)–(6). The dependent variable reports how many exercises a learner completed columns (1) and (4), how many points the learner collected in columns (2) and (5) and how much time the learner spent (in minutes) in columns (3) and (6). The explanatory variable of main interest is identity treatment which takes the value of 1 if learner *i* participated in the identity treatment and is 0 otherwise (control group — reminder only). Days since registration measures the time a learner was registered on the platform before the first intervention email was sent. Active before intervention is a dummy variable that takes the value of 1 when a learner spent time on the platform before the intervention and 0 otherwise. The dummy variable Female takes the value of 1 when learner *i* is female and is 0 otherwise. Coefficient estimates are from an ordinary least squares estimations. Asterisk and the standard errors in parentheses are heteroscedasticity-robust Huber–White standard errors (* significant at 10%; ** sign. at 5%; *** sign. at 1%). Additionally, randomization inference based *p*-values, their standard error and the number of permutation to compute these *p*-values are reported for the identity treatment dummy at the bottom of the table. This takes into account the triple-wise stratification of the intervention. Table A4 displays similar results without control variables which leads to much larger standard errors.

the alternative through “Learning German helps you find a job”. Compared to our treatment intervention, this is very precise information on economic integration. [Altmann and Traxler \(2014\)](#) find that making one dimension salient the second time should be less effective in activating that dimension. We claim that our treatment effect from the first mailing results from the identity dimension only (see H2). Following the alternative explanation, the treatment effect could be due to identity and economic prospects. According to this alternative explanation, identity and economic prospects are repeated in the treatment group; thus, treated individuals learn nothing new and should barely react. In the control group, however, there is new information of the economic prospects. Hence, the control group should react more strongly to the second mailing. According to this alternative explanation, we should thus expect a sizable negative treatment effect from the second mail that stems from the additional information the control group receives.

We can directly differentiate between the reactions to the mailings by comparing the click rates of the first two mailings. [Fig. 3](#) shows these shares of treatment and control group that clicked the link in a specific mailing (1 for the first mailing and 2 for the reminder that contains the sentence on economic prospects).

The treatment effect to the second email – the difference between the black lines in [Fig. 3](#) – is insignificant. The negative effect is small compared to the treatment effect on clicking for the first mailing, which is large and significant. This null effect of the second mailing does not support the alternative explanation that our treatment effect is due to identity and economic prospects. It is, however, in line with H2, a treatment effect due to identity only that is diminished due to repetition ([Altmann and Traxler, 2014](#)).

For completeness, we also show the reactions of our treatment on learning behavior. [Fig. 4](#) plots the platform behavior after the first two mailings by treatment. This data must be viewed with much more care, as the second email is four days after the first, and usage after the second email could be a reaction to the first as well. Here we observe larger immediate usage of the treatment and control group after the second mailing for three measures (exercises, points, and learning time), while for new logins the usage is larger after the first email, which is in line with a carry-over of the first emails regarding learning behavior over time.²¹ The treatment effect at all points in time after both first and second email is positive. While this effect is somewhat cumulative, the effect for exercises, points, and learning time seems to widen further over time after the second email, which is an argument against a catching-up of the control group (which would imply at least parallel trends for the learning variables after the second mail). Therefore, we interpret these results also as evidence in favor of H2.

²¹ The difference between the treatment effects can only really be compared on the second day after the respective mailing, as our data are comprised of cross-sections at arbitrary points in time, and the second day after a respective mailing is the only intersection. On this day, the treatment effect after the second

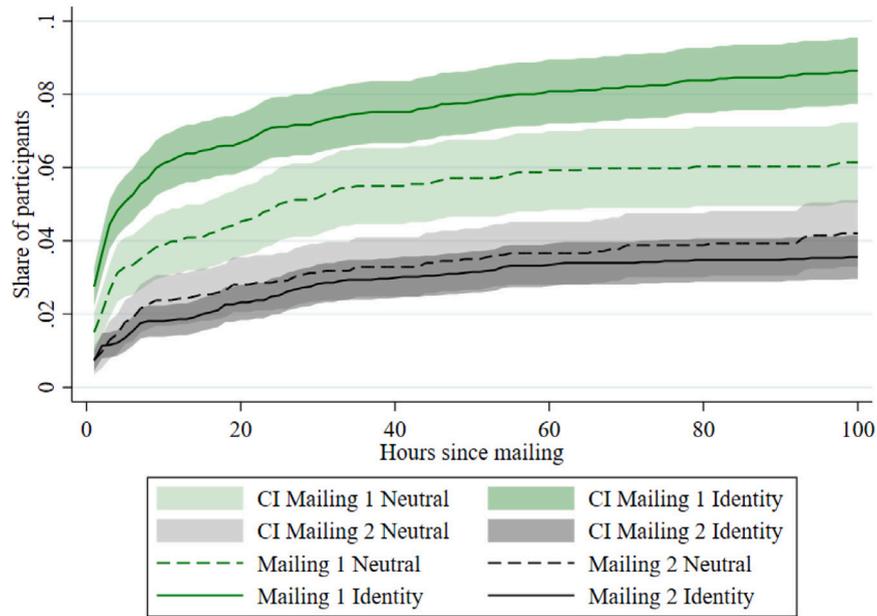


Fig. 3. Click rates over time.
 Note: The figure shows the share of participants that clicked one of the links in a mailing grouped by the mailing (first email (1) and second email (reminder, 2)) and treatment status. The shaded areas indicate 95% confidence intervals.

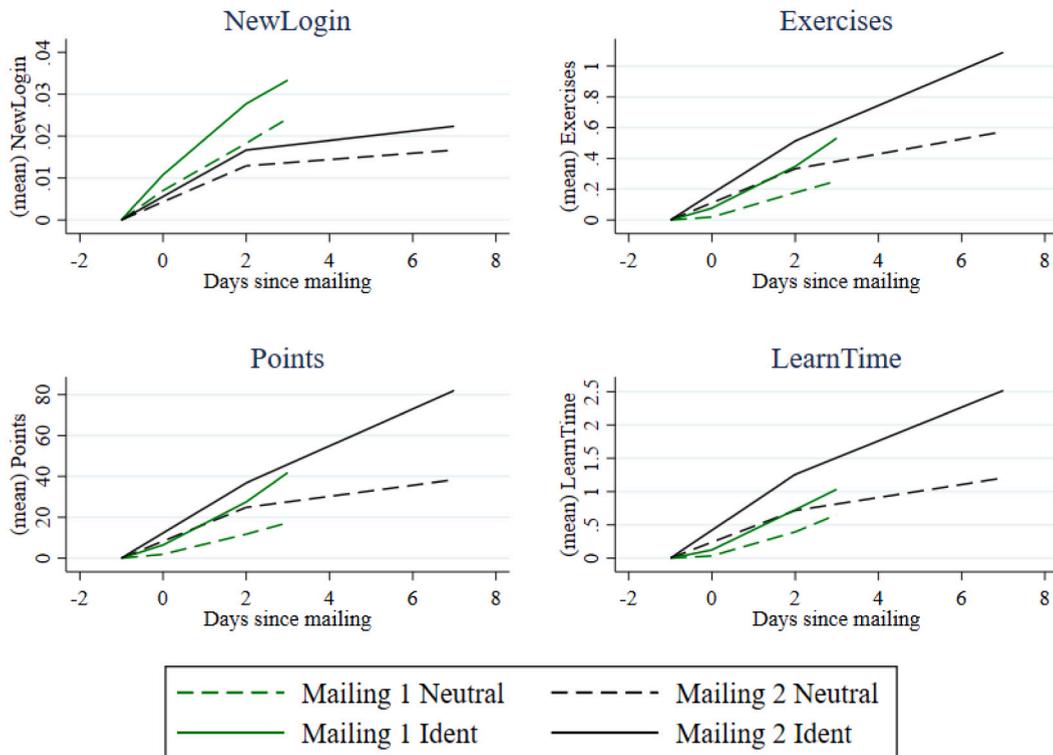


Fig. 4. Platform behavior after the first and second email.
 Note: The figure shows new logins, count of exercises and points, and the time learned on the platform in a short time frame after the first two mailings, separated into treatment and control group. Each group stock at the last observation before the mailing was subtracted. Percentage of New Logins is the fraction of learners who logged onto the learning platform after the last mailing. Exercises refers to how many exercises a learner completed. Points how many points refers to how many points a learner collected. Learning time is the time a learner spends on the platform (in minutes).

Table 4
Interaction with prior activity learning.

Variables	(1) Opened (d)	(2) Clicked (d)	(3) New login 4 weeks (d)	(4) After 4 weeks Exercises	(5) Points	(6) Learn time
Identity treatment (d)	0.095*** (0.021)	0.028* (0.017)	0.004 (0.008)	0.255 (0.379)	23.306 (35.363)	-0.183 (0.718)
Identity treatment *	-0.025 (0.027)	-0.010 (0.024)	0.017 (0.014)	2.401* (1.388)	175.135* (102.000)	10.171*** (3.326)
Active before treatment (d)				1.015*** (0.003)	1.015*** (0.004)	1.010*** (0.002)
Dep. var. before interv.						
Control variables	x	x	x	x	x	x
Observations	5,569	5,433	5,572	5,572	5,572	5,572
R-squared	0.010	0.005	0.041	0.988	0.981	0.995

Note: The dependent variable is a dummy variable that takes the value of 1 when the learner opened at least one of the intervention emails in column (1), when the learner clicked at least once on a link to the language-learning platform presented in the intervention emails in column (2) and when the learner logged onto the learning platform at least once within four weeks of the intervention in column (3). The dependent variables report what learners achieved on the learning platform in the first four weeks after the first intervention email was sent in columns (4)–(6). The explanatory variable of main interest is the identity treatment interacted with Activity before treatment, which takes the value of 1 if learner *i* participated in the identity treatment and was active before our intervention and is 0 otherwise. Days since registration, Active before intervention, and Female are added as control variables. Heteroscedasticity-robust Huber–White standard errors are in parentheses: * significant at 10%; ** sign. at 5%; *** sign. at 1%. Full table in the Table B.16 in the appendix.

Furthermore, from a more practical perspective, individuals who are only interested in economic integration and not identification with the host society are also more likely to think instrumentally about coming to Germany. They are most likely to have previously optimized their migration decision, i.e., they have chosen to leave their country behind and have chosen Germany as their best option as host country for the purpose of their own economic performance, given their abilities and preferences. As such, it seems unlikely that these individuals are easily influenced to learn (even) more German for economic benefits via the added section and the altered subject line in an email. In contrast, an identity-framed email from a German company highlighting the importance of speaking the host country's language credibly makes the language learning more attractive for identity motives.

All this leads us to conclude that it is more likely that our identity-framed nudge leads to an investment in a social identity, namely that of the host society. Alternative explanations such as economic opportunities, while possible, do not seem to fully explain the findings. Additionally, our outcome language learning is central in the acquisition of the host country's identity, while economic opportunities are often more important within the segregated communities of people from the same country of origin [Borjas \(1987\)](#). Overall, we interpret the findings of this section as support for H2 in that our identity intervention leads to an investment in this new identity.

3.5. Channels

From the platform data we can further examine some interesting aspects of who reacted to our treatment. It seems likely that refugees who have a high motivation to learn German prior to our intervention would also react more strongly to a treatment that makes the belonging dimension salient. We test this with our data and find that participants with previous activity on the platform reacted to the identity-framed nudge similarly to those who were not active before our intervention when looking at the mailing data (compare [Table 4](#)). They opened the email with a comparable probability (2.5 percentage points less, $p \gg 0.1^{22}$) and clicked on one of the links in the email comparably often (1 percentage point less, $p \gg 0.1$). However, participants with prior activity did react to our treatment more strongly when considering the learning outcomes exercise count (2.4 more, $p < 0.1$), points (175 more, $p < 0.1$), and learning time (10 min more, $p < 0.01$). This also means that our treatment effect is driven by increased learning behavior of previously active refugees. Our identity-framed nudge could remind them of a positive effect on their identification with the host society from previous learning and thereby drive our effect. Alternatively, participants who previously used the platform are more likely to return, i.e., they were happy with the provider in the past already.

Parallel to decomposing the platform usage, we can decompose the interaction with the intervention emails. We tracked within which language block of the email the corresponding link to the language-learning platform or the mobile application was clicked. Hence, we can proxy in which language individuals were reading the email, namely, in German, in their mother tongue, or English. In [Table 5](#), we interact clicking a link with whether it is in the German block and our treatment. We find that participants who clicked the link in the German section invest more effort in language learning. The exercise count increases by 12 ($p < 0.01$), the number of points by 917 ($p < 0.01$), and the learning time by 19.6 min ($p < 0.01$) after four weeks. We find no effect on new logins

mailing is smaller than after the first for new logins and points and larger for exercises and learning time, while all differences are small compared to the total treatment effect.

²² For estimations with interaction terms, we computed standard errors only and omitted randomization inference, as prior activity is not randomized.

Table 5
Interactions with clicked on german link.

Variables	(1)	(2) After 4 weeks	(3)	(4)
	New login (d)	Exercises	Points	Learn time
Identity treatment (d)	-0.002 (0.007)	1.169 (0.751)	70.399 (54.131)	3.681* (1.922)
Clicked (d)	0.062** (0.026)	6.561 (4.019)	481.180* (251.224)	2.478 (3.732)
Identity treatment * Clicked (d)	0.053 (0.033)	-5.366 (4.229)	-325.688 (276.967)	-3.627 (4.966)
Identity * Clicked German Link (d)	0.003 (0.046)	12.106** (4.866)	917.012*** (342.149)	19.595** (8.756)
Clicked German Link (d)	0.097*** (0.037)	-5.935 (4.144)	-440.054* (261.335)	-1.414 (4.402)
Control variables	x	x	x	x
Observations	5,433	5,433	5,433	5,433
R-squared	0.105	0.988	0.981	0.995

Note: The dependent variables report what learners achieved on the learning platform in the first four weeks after the first intervention email was sent in columns (after 8 weeks in Table B.17). The dependent variable is a dummy variable that takes the value of 1 when the learner logged in at least once in column (1), how many exercises a learner completed column (2), how many points the learner collected in column (3), and how much time the learner spent (in minutes) in column (4). The explanatory variable of main interest is the identity treatment interacted with Clicked and interacted with German Link (“IdentityxClickedxGermanLink”), which takes the value of 1 if learner i participated in the identity treatment and clicked on the German Link in at least one of the emails and 0 otherwise. Days since registration, Active before intervention, Female, and the dependent variable before the intervention are added as control variables. Heteroscedasticity-robust Huber–White standard errors are in parentheses: * significant at 10%; ** sign. at 5%; *** sign. at 1%. Full table in Table B.17 in the appendix.

(0.3 percentage points more, $p \gg 0.1$). These strong and significantly positive effects for the interaction between clicking the link in the email section written in German and our identity treatment could arise from individuals who care more about the host countries’ identity reacting stronger to our treatment, which is in line with our preferred interpretation.

We further test if the preference for identification is gender specific. Table A6 in the Appendix reports the interaction between the treatment effect and self-reported gender. We find a significant negative interaction only for mail opening. Treated females are 7% less ($p < 0.05$) likely to open our email. The effects on all other variables are insignificant with large confidence intervals. Even though they opened the email less often, learning did not seem to be affected detrimentally. In total, we therefore do not find a clear gender difference caused by our identity treatment.

4. Conclusion

We investigate how refugees react to an identity-framed nudge and interpret this as their willingness to invest in the social identity of their host country. By using an experimental setting, we exogenously vary the salience of the language investments’ link to social identification. Our results show a significant treatment effect of the social identity treatment. Using secondary analysis we support our main hypothesis that the effect is solely due to the fact that individuals do actively seek to invest in group specific identity. We therefore provide causal evidence from the field that supports the claim that people are willing to invest time and effort in identity formation.

We make use of a sample of recently arrived refugees in Germany. Compared to receivers of the neutrally-framed email, receivers of an email making identity salient complete more exercises and spend more time on the language-learning platform. The effects are stronger for refugees with more prior activity and for those who read the email in the host country’s language. We do not find support for a stronger effect of loss framing relative to the gain framing identity, which may be due to a foreign language effect.

Our results support the concept of a preference for identification. While laboratory findings like [Hett et al. \(2020\)](#), [Hargreaves Heap and Zizzo \(2009\)](#) show that people are willing to forgo income for the sake of their identity, we document the process of formation. This formation could be motivated by a preference for identification, as documented by the laboratory studies.

Our results imply that it may pay off to communicate using identity-loaded messages with newly arrived immigrants to increase their effort spent on integration. This is in line with other research using identity-building activities, such as attending a citizenship ceremony ([Manning and Roy, 2009](#)). Our intervention comes at almost no cost as it only requires the addition of a few sentences to communication. This makes it a very low-cost potential policy tool for increasing integration effort.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.ejpoleco.2022.102321>.

References

- Akerlof, R., 2017. Value Formation : The Role of Esteem. *Games Econ. Behav.* 102, 1–19.
- Akerlof, G.A., Kranton, R.E., 2000. Economics and identity. *Q. J. Econ.* 115 (3), 715–753.
- Altmann, S., Traxler, C., 2014. Nudges at the dentist. *European Econ. Rev.* 72, 19–38.
- Austen-Smith, D., Fryer, R., 2005. An economic analysis of “acting white”. *Q. J. Econ.* 120 (May), 551–583.
- Banks, S., Salovey, P., Greener, S., Rothman, A.J., Moyer, A., Beauvais, J., Epel, E., 1995. The effects of message framing on mammography utilization. *Health Psychol.* 14 (2), 178–184.
- Battu, H., Zenou, Y., 2009. Oppositional identities and employment for ethnic minorities: Evidence from England. *Econom. J.* 120 (542), 52–71.
- Bauer, T., Epstein, G.S., Gang, I.N., 2005. Enclaves, language, and the location choice of migrants. *J. Popul. Econ.* 18 (4), 649–662.
- Benjamin, D.J., Choi, J.J., Fisher, G., 2016. Religious Identity and Economic Behavior. *Rev. Econ. Stat.* 98 (4), 617–637.
- Benjamin, D.J., Choi, J.J., Strickland, A.J., 2010. Social identity and preferences. *Amer. Econ. Rev.* 100 (4), 1913–1928.
- Bernard, M., Hett, F., Mechtel, M., 2016. Social identity and social free-riding. *European Econ. Rev.* 90 (C), 4–17.
- Bertrand, M., Karlan, D., Mullainathan, S., Shafir, E., Zinman, J., 2010. What's Advertising Content Worth? Evidence from a Consumer Credit Marketing Field Experiment. *Q. J. Econ.* 125 (1), 263–305.
- Bisin, A., Patacchini, E., Verdier, T., Zenou, Y., 2011. Ethnic identity and labour market outcomes of immigrants in Europe. *Econ. Policy* 26 (65), 57–92.
- Bisin, A., Patacchini, E., Verdier, T., Zenou, Y., 2016. Bend it like Beckham: Ethnic identity and integration. *European Econ. Rev.* 90, 146–164.
- Bisin, A., Verdier, T., 2000. “Beyond the Melting Pot”: Cultural Transmission, Marriage, and the Evolution of Ethnic and Religious Traits. *Q. J. Econ.* 115 (3), 955–988.
- Borjas, G.J., 1987. Self-Selection and the Earnings of Immigrants. *Am. Econ. Rev.* 77 (4), 531–553.
- Brücker, H., Rother, N., Schupp, J., 2018. IAB-BAMF-SOEP- Befragung von Geflüchteten 2016: Studiendesign, Feldergebnisse sowie Analysen zu schulischer wie beruflicher Qualifikation, Sprachkenntnissen sowie kognitiven Potenzialen. Technical Report, Berlin: Deutsches Institut für Wirtschaftsforschung (DIW), Bundesamt für Migration und Flüchtlinge, pp. 1–76.
- Brücker, H., Rother, N., Schupp, J., Babka von Gostomski, C., Böhm, A., Fendel, T., Friedrich, M., Giesselmann, M., Kosyakova, Y., Kroh, M., Kühne, S., Liebau, E., Richter, D., Romiti, A., Schacht, D., Scheible, J., Schmelzer, P., Siegert, M., Sirries, S., Trübsetz, P., Vallizadeh, E., 2016. Forced Migration, Arrival in Germany, and First Steps toward Integration. *Wochenbericht* 6 (48), 541–556.
- Calzolari, G., Nardotto, M., 2016. Effective Reminders. *Manage. Sci.* 51 (7), 1–18.
- Cameron, L., Erkal, N., Gangadharan, L., Zhang, M., 2015. Cultural integration: Experimental evidence of convergence in immigrants' preferences. *J. Econ. Behav. Organ.* 111, 38–58.
- Candelo, N., Croson, R.T., Li, S.X., 2017. Identity and social exclusion: An experiment with Hispanic immigrants in the US. *Exp. Econ.* 20 (2), 460–480.
- Charness, G., Chen, Y., 2020. Social Identity, Group Behavior, and Teams. *Annu. Rev. Econ.* 12 (1), 691–713.
- Charness, G., Masclet, D., Villeval, M.C., 2014. The Dark Side of Competition for Status. *Manage. Sci.* 60 (1), 38–55.
- Chen, Y., Farzan, R., Kraut, R., YeckehZaare, I., Zhang, A.F., 2018. Motivating Contributions to Public Information Goods: A Field Experiment on Wikipedia. Michigan State University, East Lansing, MI, pp. 1–61.
- Chen, Y., Li, S.X., 2009. Group Identity and Social Preferences. *Am. Econ. Rev.* 99 (1), 431–457.
- Chen, Y., Li, S.X., Liu, T.X., Shih, M., 2014. Which hat to wear? Impact of natural identities on coordination and cooperation. *Games Econ. Behav.* 84, 58–86.
- Chiswick, B.R., Miller, P.W., 1995. The Endogeneity between Language and Earnings: International Analyses. *J. Labor Econ.* 13 (2), 246–288.
- Clots-Figueras, I., Masella, P., 2013. Education, language and identity. *Econom. J.* 123 (570), 332–357.
- Cohn, A., Fehr, E., Marechal, M.A., 2014. Business culture and dishonesty in the banking industry. *Nature* 516 (729), 86–89.
- Cohn, A., Maréchal, M.A., Noll, T., 2010. Bad boys: How criminal identity salience affects rule violation. *Rev. Econom. Stud.* 82 (4), 1289–1308.
- Connolly, K., 2015. Germany greets refugees with help and kindness at Munich central station. *The Guardian*, Munich.
- Constant, A.F., Gataullina, L., Zimmermann, K.F., 2009. Ethnosing immigrants. *J. Econ. Behav. Organ.* 69 (3), 274–287.
- Damgaard, M.T., Nielsen, H.S., 2017. Nudging in Education. *Economics Working Papers*, Aarhus University.
- Dehdari, S., Gehring, K., 2017. The Origins of Common Identity: Division, Homogenization Policies and Identity Formation in Alsace-Lorraine. *CESifo Working Paper Series*, Uppsala University, Uppsala, pp. 1–96.
- Dustmann, C., 1994. Speaking fluency, writing fluency and earnings of migrants. *J. Popul. Econ.* 7 (2), 133–156.
- Dustmann, C., Fabbri, F., Preston, I., Wadsworth, J., 2003. Labour market performance of immigrants in the UK labour market. *Econ. J. Royal Econ. Soc.* 113, 695–717.
- Epstein, G.S., Gang, I.N., 2010. Chapter 1 Migration and Culture. *Migr. Cult.* 8 (5123), 1–21.
- Epstein, G.S., Heizler, O., 2015. Ethnic identity: A theoretical framework. *IZA J. Migr.* 4 (1), 9.
- Escueta, M., Quan, V., Nickow, A.J., Oreopoulos, P., 2017. Education Technology: An Evidence-Based Review. *NBER Working Paper Series*.
- Falk, A., Becker, A., Dohmen, T., Enke, B., Huffman, D.B., Sunde, U., 2018. Global Evidence on Economic Preferences. *Q. J. Econ.* 133 (4), 1645–1692.
- Fordham, S., Ogbu, J., 1986. Black Students School Success: Coping with the Burden of Acting White (Annotated). *Urban Rev.* 18 (3), 176–206.
- Fryer, R.G., Levitt, S., List, J., Sadoff, S., 2012. Enhancing the Efficacy of Teacher Incentives through Loss Aversion: A Field Experiment. *NBER Working Paper*, National Bureau of Economic Research, Cambridge, Massachusetts, p. 36.
- Fryer, R.G., Torelli, P., 2010. An empirical analysis of ‘acting white’. *J. Public Econ.* 94 (5–6), 380–396.
- Gallagher, K.M., Updegraff, J.A., 2012. Health message framing effects on attitudes, intentions, and behavior: A meta-analytic review. *Ann. Behav. Med.* 43 (1), 101–116.
- Georgiadis, A., Manning, A., 2013. One nation under a groove? Understanding national identity. *J. Econ. Behav. Organ.* 93, 166–185.
- Grote, N., Klausmann, T., Scharbillig, M., 2017. The Absolute and Relative Effect of an Identity Gain and Loss Treatment on Learning Behavior. *AEA RCT Registry*.
- Hargreaves Heap, S.P., Zizzo, D.J., 2009. The value of groups. *Amer. Econ. Rev.* 99 (1), 295–323.
- Haslam, S., 1997. Stereotyping and social influence: Foundations of stereotype sharedness. In: Spears, R., Oakes, P., Ellemers, N., Haslam, S. (Eds.), *The Social Psychology of Stereotyping and Group Life*. Blackwell, Oxford, UK & Cambridge, USA.
- Heß, S., 2017. Randomization inference with Stata: A guide and software. *Stata J.* 17 (3), 630–651.

- Hett, F., Mechtel, M., Kröll, M., 2020. The structure and behavioral effects of revealed social identity preferences. *The Economic Journal* 130 (632), 2569–2595.
- Hicks, D.L., Santacreu-Vasut, E., Shoham, A., 2015. Does mother tongue make for women's work? Linguistics, household labor, and gender identity. *J. Econ. Behav. Organ.* 110, 19–44.
- Hoff, K., Pandey, P., 2006. Discrimination, Social Identity, and Durable Inequalities. *Amer. Econ. Rev.* 96 (2), 206–211.
- Hoff, K., Pandey, P., 2014. Making up people-The effect of identity on performance in a modernizing society. *J. Dev. Econ.* 106, 118–131.
- Hossain, T., List, J.A., 2012. The Behavioralist Visits the Factory: Increasing Productivity Using Simple Framing Manipulations. *Manage. Sci.* 58 (12), 2151–2167.
- Jenkins, R., 2008. *Social Identity*, third ed. Routledge, Abingdon, pp. 1–246.
- Kahneman, D., Tversky, A., 2011. An Analysis of Decision under Risk. *Econometrica*, Vol. 47, No. 2, March 1979 47 (2), 263–292.
- Karlan, D., McConnell, M., Mullainathan, S., Zinman, J., 2016. Getting to the Top of Mind: How Reminders Increase Saving. *Manage. Sci.* 62 (12), 3393–3411.
- Karlan, D., Morten, M., Zinman, J., 2012. A Personal Touch: Text Messaging for Loan Repayment. NBER Working Papers, February, National Bureau of Economic Research, Cambridge, Massachusetts, pp. 1–17.
- Keysar, B., Hayakawa, S.L., An, S.G., 2012. The Foreign-Language Effect. *Psychol. Sci.* 23 (6), 661–668.
- Kőszegi, B., Rabin, M., 2007. Reference-dependent risk attitudes. *Amer. Econ. Rev.* 97 (4), 1047–1073.
- Kroh, M., Böhm, A., Brücker, H., Jacobsen, J., Kühne, S., Liebau, E., Scheible, J.A., Schupp, J., Siebert, M., Trübswetter, P., Rother, N., Schupp, J., 2017. IAB-BAMF-SOEP-Befragung von Geflüchteten: Überblick und erste Ergebnisse. In: IAB-BAMF-SOEP-Befragung Von Geflüchteten 2016: Studiendesign, Feldergebnisse Sowie Analysen Zu Schulischer Wie Beruflicher Qualifikation, Sprachkenntnissen Sowie Kognitiven Potenzialen, Vol. 123. DIW Berlin, p. 80.
- Lazear, E.P., 1999. Culture and Language. *J. Polit. Econ.* 107 (S6), S95–S126.
- Levitt, S.D., List, J.A., Neckermann, S., Sadoff, S., 2016. The behavioralist goes to school: Leveraging behavioral economics to improve educational performance. *Am. Econ. J.: Econ. Policy* 8 (4), 183–219.
- MacKinnon, J.G., White, H., 1985. Some heteroskedasticity-consistent covariance matrix estimators with improved finite sample properties. *J. Econometrics* 29 (3), 305–325.
- Manning, A., Roy, S., 2009. Culture clash or culture club? National identity in Britain. *Econom. J.* 120 (542), 72–100.
- McKenzie, D., 2012. Beyond baseline and follow-up: The case for more T in experiments. *J. Dev. Econ.* 99 (2), 210–221.
- Mcmanus, W., Gould, W., Welch, F., 1983. Earnings of Hispanic Men: The Role of English Language Proficiency. *J. Labor Econ.* 1 (2), 101–130.
- Monscheuer, O., 2018. National Attachment and the Integration of Second Generation Immigrants in the US. In: IZA Conference. Institute for the Study of Labor (IZA), University of Heidelberg, Heidelberg, p. 53.
- Ogbu, J.U., 1999. Beyond Language: Ebonics, Proper English, and Identity in a Black-American Speech Community. *Am. Educ. Res. J.* 36 (2), 147–184.
- O'Keefe, D.J., Jensen, J.D., 2009. The relative persuasiveness of gain-framed and loss-framed messages for encouraging disease detection behaviors: A meta-analytic review. *J. Commun.* 59 (2), 296–316.
- Patacchini, E., Zenou, Y., 2016. Racial identity and education in social networks. *Social Networks* 44, 85–94.
- Shayo, M., 2009. A model of social identity with an application to political economy: Nation, class, and redistribution. *Am. Political Sci. Rev.* 103 (2), 147–174.
- Shayo, M., 2020. Social Identity and Economic Policy. *Annu. Rev. Econ.* 12 (1), 355–389.
- Tajfel, H., 1974. Social identity and intergroup behaviour. *Soc. Sci. Inform.* 13 (2), 65–93.
- Tajfel, H., 1978. *Differentiation Between Social Groups: Studies in the Social Psychology of Intergroup Relations*. Academic Press, Oxford, England.
- Tajfel, H., Turner, J.C., 1986. *The Social Identity Theory of Intergroup Behavior*. In: Worchel, S., Austin, W.G. (Eds.), *The Social Psychology of Intergroup Relations*. Nelson-Hall, Chicago, US, pp. 7–24.
- Thaler, R.H., 1980. Towards a positive theory of consumer choice. *J. Econ. Behav. Organ.* 1, 39–60.
- Thaler, R.H., Sunstein, C.R., 2008. *Nudge: Improving Decisions About Health, Wealth, and Happiness*. Yale University Press, p. 312.
- Tversky, A., Kahneman, D., 1991. Loss Aversion in Riskless Choice: A Reference-Dependent Model. *Q. J. Econ.* 106 (4), 1039–1061.