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# Communication Barriers and Infant Health: Intergenerational Effects of Randomly Allocating Refugees Across Language Regions

Discussion Paper no. 2021-05

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**JEL Classification:** F22, I12, J13, J24, J61, J62

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# Communication Barriers and Infant Health: Intergenerational Effects of Randomly Allocating Refugees Across Language Regions.*

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June 8, 2021

**Abstract**

This paper investigates the intergenerational effect of communication barriers on child health at birth using a natural experiment in Switzerland. We leverage the fact that refugees arriving in Switzerland originate from places that have large shares of French (or Italian) speakers for historical reasons and upon arrival are by law randomly allocated across states that are dominated by different languages but subject to the same jurisdiction. Our findings based on administrative records of all refugee arrivals and birth events between 2010 and 2017 show that children born to mothers who were exogenously allocated to an environment that matched their linguistic heritage are on average 72 gram heavier (or 2.2%) than those that were allocated to an unfamiliar language environment. The differences are driven by growth rather than gestation and manifest in a 2.9 percentage point difference in low birth weight incidence. We find substantial dose-response relationships in terms of language exposure in both, the origin country and the destination region. Moreover, French (Italian) exposed refugees only benefit from French-(Italian-) speaking destinations, but not vice versa. Contrasting the language match with co-ethnic networks, we find that high quality networks are acting as a substitute rather than a complement.

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**Acknowledgments:** We thank Marcella Aslan, Ivan Balbuzanov, Victoria Baranov, Christina Bastianon, Sascha Becker, Andreas Beerli, Pietro Biroli, Giuliano Bonoli, Nicole Black, Caroline Chuard, Gabriella Conti, Christian Dustmann, Delia Furtado, Libertad Gonzales, Dominik Hangartner, Macartan Humphreys, Andrea Ichino, David Johnston, Sampada KC, Joris Kennis, Umair Khalil, Gustav Kjellsson, Jianghong Li, Maarten Lindeboom, Jason Lindo, Shiko Maruyama, Daniel Meierrieks, José Muller-Dugic, Caitlin Myers, Arash Naghavi, Carol Propper, Laura Renner, Heather Royer, Didier Ruedin, Clémentine Sadania, Alex Scacco, Max Schaub, Hannes Schwandt, Kevin Staub, Susan Steiner, Steven Stillman, Alex Teytelboym, Maria Theißen, Rainer Winkelmann, Yves Zenou, and conference participants at AHES (Melbourne), FRIAS (Freiburg), the Health, Migration and Integration Symposium (Maastricht), the Economics of Health and well-being Workshop (Hepburn Springs), Essen Health Workshop, IFO (Dresden), IAB-ECSR (Nürnberg), as well as seminar participants at Monash University, and the University of Mannheim for thoughtful comments. We are also grateful to Lilia Goetz, Jasper Jansen, Aaron Lauterbach, Bao-An Nguyen, David Sebastiampillai, Thomas Tichelbäcker, and Gizem Ünsal for very able research assistance. **Ethics approval:** Monash University Human Research Ethics Committee No. 21726 and WZB Research Ethics Committee No. 2019474. **Disclosure:** No author has any interests to declare.

# 1 Introduction

Language proficiency is widely accepted as the single most important human capital component for immigrants to foster economic and social participation in the host society (e.g., Portes and Rumbaut 1996; Lazear 1999; Brell et al. 2020). Crucially, language skills are not only associated with various integration outcomes of the new arrivals,[1] but are also correlated across generations, resulting in persistently lower socioeconomic outcomes of children born and raised by parents with local language deficiencies (e.g., Rooth and Ekberg 2003; Bleakley and Chin 2008; Casey and Dustmann 2008). In this study, we demonstrate that exogenous attribution of local communication skills affects child well-being already in-utero, captured with health at birth —a key indicator in the modern economic literature to study the transmission of inequality across generations (see Almond et al. 2018, for a review). Our findings provide strong evidence that the intergenerational transmission of inequality already takes place during pregnancy, thereby highlighting the importance of early policy intervention to counter systematic disadvantages of vulnerable groups.

Consistent with a key idea of Grossman (1972)'s model, that (language) skills may not only influence health investments via the access to resources (via, for example, income) but also efficiency of health investments via the ability to process information and to communicate, medical studies have long argued that a lack of communication skills can have detrimental effects on in-utero as well as neonatal infant health through inadequate knowledge of health-related behavior, such as utilization of health-care services (e.g., Obregon et al. 2019; Palau et al. 2019). Yet, inferring a causal effect remains a challenge for this strand of research that is thus far exclusively based on small, observational, and explorative studies. Here, we demonstrate that quasi-random assignment of the ability to communicate through geographical allocation of refugees to different language regions significantly affects their future children's well-being already at birth. To this end, we estimate the causal effect of maternal communication barriers on infant well-being via objective anthropometric neonatal health measures (i.e., birth weight, gestation, infant mortality) that have become ubiquitous in the modern economic literature.

More concretely, we leverage a unique natural experiment in Switzerland, a country that

[1] Local language skills correlate with a variety of integration outcomes, including labor market performance (e.g., Dustmann and Fabbri 2003; Bleakley and Chin 2004; Chiswick and Miller 2007; Auer 2018), well-being (e.g., Beiser and Hou 2001), voting (e.g., Johnson et al. 2003; Houle 2019) or social capital (e.g., Cheung and Philimore 2014).

is characterized by distinct language areas including German, French and Italian (Eugster et al. 2017), and which receives a large number of refugees seeking asylum from linguistically diverse origin countries —including French and Italian. One would expect refugees —like other immigrants —to select themselves into specific locations to improve their social and economic well-being based on their expected returns (e.g., Card 2009; Belot and Hatton 2012; Watson 2013).[2] Swiss refugee policy, however, imposes a remarkably strict allocation regime that prevents refugees from choosing their location freely. Concretely, caseworkers of the Swiss immigration authority remotely and —by law—randomly allocate newly arriving asylum seekers to one of the country's 26 cantons (states) and, therewith, to distinct language regions. Hence, this policy determines randomly whether a refugee from an origin country with a sizeable share of French- or Italian-speakers is matched to an familiar language environment and thus more likely to be able to communicate with the local population, including but not limited to doctors and nurses. This allows us to apply a generalized difference-in-difference estimator that accounts fully flexibly for both origin as well as destination fixed effects and thereby estimate the causal effect of maternal exposure to a familiar (matched) language environment on child health outcomes.

Our data comprises administrative accounts of all asylum seekers who arrived between 2010 and 2017 and all child birth events in the country with detailed health information. We find no evidence of any compositional differences, selection imbalances, or differential fertility choices between refugees whose language either matches or does not match their allocated region. However, we observe economically relevant and statistically significant positive effects of exposure to a familiar language environment on child health at birth.

In Figure 1, we preview our main finding, by showing for each language region in Switzerland (x-axis) the simple average log birth weights across refugee groups originating from French- or Italian-speaking countries (with officially French-speaking countries of birth [COB] and COB with a significant French-/Italian-speaking population as subgroups), or from a country where no Swiss language is spoken. For the majority of refugees originating from a country were no Swiss language is spoken (grey line), health at birth (here measured as log birth weight) is mostly flat across regions and highest in the German-speaking region of Switzerland. At the same time, refugees from both Italian- and French-exposed countries show substantially lower health levels. Yet, when allocated to a familiar language environment, these refugees

[2] As Bauer et al. (2005) have shown for the US, immigrant location choice may even be directly linked to language proficiency.

are at par and even surpass the reference group. This pattern is clearest for refugees from an officially French-speaking origin (as opposed to some French/Italian exposure, but not as official language in the COB) who are allocated to a monolingual French-speaking canton. Refugees exposed to Italian in their COB fare much better in the Italian-speaking region of Switzerland. Strikingly, however, neither group is benefiting from the respective other match region.[3]

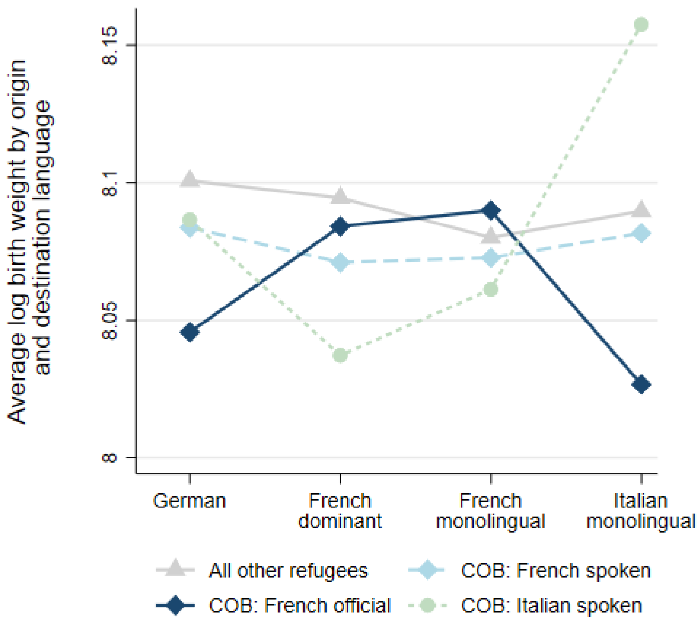


**Figure 1:** Raw average child health at birth (log birth weight) across origins and destination language exposure

*Note*: Figure displays the raw averages of log birth weight by country of birth [COB] and destination canton language exposure. We distinguish four mutually exclusive groups of refugees: those that are coming from a country that has French as an official language, where a significant portion of the population speaks French, or Italian, and all other refugees. These are allocated to four regions in Switzerland: dominantly German-speaking, dominantly but bilingual French, monolingual French, and monolingual Italian. *Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

In our preferred specification, we find that the children of mothers who by chance ended up in a familiar language environment weigh, on average, 72 grams more compared to co-national mothers who simultaneously arrived in Switzerland but were less fortunate and have been allocated to an unfamiliar language region. Relative to the average birth weight in our refugee population of about 3,200 grams, this amounts to a 2.2% increase in birth weight.

[3]Given that both French and Italian are Roman languages with some level of similarity, this absence of "spillover effects" may come surprising. However, as we show in Figure A.2.1 in the Appendix, the share of residents in the Italian-speaking region who are regularly speaking French, and vice versa, is very low ($< 10\%$) —even among doctors. This highlights the sharp language borders in Switzerland which we will elaborate in Section 2.2 below.

The effect is not only present at the average of the birth weight distribution but also at the lower tail, where changes in weight can substantially alter infant- and later life well-being: the clinical *Low Birth Weight* [LBW] indicator (weight <2,500 gram) decreases by 2.9 percentage points (from a mean of 6.98 percent). The results are highly robust to various alternative specifications, including non-parametric bounds, non-linear estimation, various sub-sample analyses and do remain stable when including non-refugee immigrants in the control group in a triple-difference setting. Moreover, we apply and adjust recent advances in generalized differences-in-difference estimation and analyze potential sub-DiDs separately to further support the generalizability of our results (c.f. Goodman-Bacon 2018a; Chaisemartin and D'Haultfoeuille 2020).

The observed effect sizes are substantial compared to the effect of targeted prenatal care or educational policies. Chou et al. (2010), for instance, instrument women's schooling with variation in new junior high school openings following an educational reform in Taiwan. They report a reduction in LBW incidences of approximately 0.24 percentage points when mothers have (better) access to education. Hoynes et al. (2015) report a 0.35 percentage point decline in LBW incidence for a $1,000 (per year) increase in earned income tax credit in the US for the general population, and —closer to our setting —a 0.75 percentage point decline among low educated black mothers. Regarding more targeted interventions, access to nutritional food programs for low-income mothers in the US, for instance, is associated with a 27 gram increase in average birth weight (Rossin-Slater 2013), roughly a third of the effect we find. Perhaps closest to our setting, Cygan-Rehm and Karbownik (2020) show that providing earlier prenatal care —where information is the likely mechanism —to becoming mothers in Poland increases their children's birth weight by 0.3 to 0.8% (9–21 gram), and reduces LBW by 0.4 to 1 percentage points. Relating our finding to these studies highlights the relative importance of maternal ability to access health-relevant information through language skills for infants' health production.

Interestingly, we find no significant differences in gestation, which is consistent with the information treatment in Cygan-Rehm and Karbownik (2020). We interpret these results as evidence that the worse infant health is driven by lower weight-for-gestational age or 'Intrauterine Growth Restriction'. IUGR is caused —among other factors —by maternal stress, malnutrition, and lack of medical care for untreated conditions (Pallotto and Kilbride 2006; Lodygenski et al. 2008; Figueras and Gardosi 2011); all of which are plausibly present in the context of refugee migration experiences (e.g., Bischoff et al. 2009; Frattini et al. 2020).

Importantly, though, IUGR is preventable in many cases if detected early on: for example, even a low-dose intake of Aspirin started in early pregnancy can already reduce the risk of fetal growth restriction (e.g., Bujold et al. 2010, for a meta analysis).

Likely key mechanisms for the effect of communication barriers on infant health include (a) less income and thus fewer resources, (b) health investments, that is, nutrition, smoking, and other health-related behavior, as well as information about available health care services relevant for both, the mother's and the child's well-being, and (c) the capacity to process medical instructions, that is, following procedures or medication (patient-doctor match). Note that we interpret communication barriers broadly, following the seminal contribution by Lazear (1999),[4] that may include language as well as culture —such as customs, trust, expectations, and beliefs. Both dimensions are important for trading information and very closely linked to each other. Our data does not allow us to draw definitive conclusions, however, additional results suggest that the observed effect is mainly driven by a lack of access to health-relevant information. We find evidence that a larger local network substitutes for part of the language match effect, especially when this network comprises of (relatively well-informed) refugee mothers whose children were born in the previous year. While these networks can hardly influence the quality or language match with doctors and health personnel, they seem to be important for generating knowledge about health services and health-related behavior. Moreover, the positive effect of a familiar language environment on infant health is largest when mothers originate from countries in which deliveries are usually not attended by medical staff, that is, when they are less likely familiar with a sophisticated health care systems, such as attending regular check ups. While communication hurdles with doctors likely play a role in our setting as well (e.g., Alsan et al. 2019; Hill et al. 2018; Weiss 2020), these results indicate that access to information on available services (e.g. the availability of free ultra sound screening at the gynecologist) likely explain part of the positive effect of being allocated to a familiar language environment. Stress is a likely mediator of the effect we find, assuming that becoming refugee mothers who are unable to communicate are very likely under severe stress. Short-term stress often affects birth weight via gestation (e.g., Persson and Rossin-Slater 2018). This would point towards a more persistent form of stress and trauma being at play here, which affects the fetus without affecting gestation. Eventually, we provide further evidence that our observe effects are neither driven by possible earnings differences through

[4] Lazear (1999, pS96) defines culture to include "some notion of shared values, beliefs, expectations, customs, jargon, and rituals" and language as "the set of common sounds and symbols by which individuals communicate".

employment nor by (language) assimilation through longer residence in Switzerland.

Our findings, first and foremost, contribute to the literature on the "economics of language" (Chiswick and Miller 2007; Ginsburgh and Weber 2020) —a strand of research in which the endogeneity of language skills and selection into migration poses particularly great challenges to causal identification.[5] Bleakley and Chin (2004) make an important contribution by showing that earnings of adult immigrants are significantly higher if they arrived to the US as children, exploiting arrival around the "critical age period" (using immigrants from non-English speaking backgrounds as controls in an instrumental variable approach). Berg et al. (2014) extends this setting by looking at immigrant children siblings that arrived together before and after the critical age period. Perhaps most credibly identified, a small but growing number of studies use refugee allocation, building on the seminal contribution by Edin et al. (2003). In our context, Auer (2018) and Hangartner and Schmid (2021) leverage the Swiss allocation policy to show positive employment effects for the language match.[6] All of these studies find significant —arguably causal —benefits from being able to communicate in the local language on immigrants' own well-being.

Expanding the picture to the second generation, we are to the best of our knowledge the first to address the important question of the causal relationship between maternal skills and child health in a quasi-experimental setting. Whilst the overall (correlational) relationship between child health and socioeconomic background of the mother are fairly well documented (Behrman and Wolfe 1987; Cutler and Lleras-Muney 2006; Currie 2009; Almond and Mazumder 2011; Evans and Fitzgerald 2017; Almond et al. 2018),[7] causal effects of specific human capital dimensions and of language in particular are notoriously hard to identify due to their inherently endogenous character (c.f., Chiswick and Miller 1995; Dustmann and van Soest 2001, 2002). Therefore, parental skill–based effects on the child health production function have mostly been assessed indirectly using environmental shocks (and heterogeneity

[5]Various approaches have been used to recover the causal effect of immigrant language fluency on labor market performance, such as Dustmann and Fabbri (2003) and Gonzalez (2005), who apply semi- and non-parametric bounding approaches.

[6]The effect of language classes has been assessed recently in France (Lochmann et al. 2019), Texas (Chin et al. 2013), and Denmark (Arendt et al. 2020).

[7]Additional evidence provided by systematic descriptive comparisons across countries (Gakidou et al. 2010) and twin studies (Lundborg 2013) also suggest a positive relationship between maternal skills/education and infant health.

across maternal skills),[8] or exogenous policy changes, such as college openings, discontinuities in the school entry age, and changes in compulsory schooling laws (Currie and Moretti 2003; Chou et al. 2010; Grytten et al. 2014). In sum, causal evidence of parental skills is mixed at best, and much less related than the correlation would suggest (e.g., Lindeboom et al. 2009; McCrary and Royer 2011). Moreover, few have assessed the causal effect of parents' language skills on children's well-being, despite a long-standing debate on the intergenerational transmission of life chances through language (see discussion in Abramitzky et al. 2021). Bleakley and Chin (2008) apply the critical period IV to show that parental English skills improve language acquisition and school performance of immigrants' children. To date, the only two studies assessing the effect of parental language skills on infant health are Aoki and Santiago (2018) and Black and Kunz (2019), who again utilize the critical period IV in the UK and Australia, respectively but find no clear effects. For understanding the implications of our findings it is not least important to consider the well-established relationship between health at birth and future life chances (Almond et al. 2018; Currie 2009; Currie and Stabile 2003; Currie et al. 2010). The nature of refugee migration describes an inherently vulnerable population that faces (additional) hurdles on their path to socioeconomic participation. Yet, understanding the role of language as a key human capital component also relates to other groups at the lower end of the social strata. Our study contributes to this debate by providing an arguably upper-bound on intergenerational transmission of inequality via human capital more broadly (Aizer and Currie 2014; Chetty et al. 2014; Conti et al. 2019).

Eventually, our findings are relevant for policies seeking to improve integration and well-being in diverse societies. While the transparency and neutrality of random refugee allocation mechanisms —which are in place in a number of developed countries —undoubtedly have some merit, its shortcomings become strikingly clear when locations differ in key factors, such as language (a recent focus has emerged aiming to improve allocation of refugees, e.g. Delacrétaz et al. 2016; Bansak et al. 2018). We perform several additional analyses, which indicate that access to information (e.g., through networks) can partly compensate for being allocated to an unfamiliar language environment. Hence, relatively mild interventions such as mother groups, information campaigns, or interpreter services might already carry large benefits —for (refugee) migrants in other foreign language contexts as well. From a general

[8] These include nutrition (Lindeboom et al. 2010; Almond and Mazumder 2011; Van Ewijk 2011), exposure to violence (Currie et al. 2020), air quality (Currie and Neidell 2005; Currie and Schwandt 2016; Lleras-Muney 2010; Imelda 2018; Alexander and Schwandt 2019; Mouganie et al. 2020), water quality (Alsan and Goldin 2019), toxication (Currie and Schmieder 2009), or earthquakes (Menclova and Stillman 2020). Notably, these adverse shocks are often concentrated among lower socioeconomic groups, and, thus, likely to cement social inequalities (Currie and Hyson 1999; Van den Berg et al. 2006; Shrestha 2020).

perspective, we argue that the disproportionate health risks faced by children of refugees should be recognized when policymakers weigh the costs and benefits of allocating refugees independent of their language and other skills.

# 2 Institutional setting and data

## 2.1 Allocation of Asylum Seekers in Switzerland

Refugee-policy in Switzerland demands by law that newly arriving asylum seekers are to be allocated *(conditionally) randomly* across the country's 26 cantons (read: states), proportional to the cantons' residence population and independent of individual characteristics such as language proficiency (FAA-142.31 1998). In the following, we list the key aspects of this policy for our research (see also Auer 2018; Bansak et al. 2018; Slotwinski et al. 2019; Couttenier et al. 2019; Marten et al. 2019).

Persons who enter the country and request asylum are initially transferred to one of the federal processing centers (*Bundesasylzentren*). After registration, which includes a preliminary medical check-up, a team of allocation officers in the headquarters of the State Secretary for Migration (SEM) assigns the asylum seeker to a canton. Importantly, the SEM allocation team performs this placement remotely without direct contact to the asylum seekers.[9] Importantly, within these groups, allocation is random by default (SEM 2015; see also Auer 2018, Couttenier et al. 2019, Marten et al. 2019). In some situations, Swiss asylum law allows for the suspension of random allocation: in case of family reunification, a person can be assigned to the spouse's, parents' or children's canton of residence. In addition, asylum seekers with medical conditions that require special treatment (e.g., in a specific hospital) are usually allocated to the respective canton. Theoretically, asylum seekers are also granted the right to request a change of the residence canton (FAA-142.31 1998). In practice, however, such requests do not occur often and are rarely accepted by the involved cantons, even if the person states plausible reasons (see also Hangartner and Schmid 2021, who describe court decisions on that matter). For instance, the State Secretariat's handbook (SEM 2015) lists the following exemplary case (translated from German):

[9]Random allocation is further stratified with the aim to balance the number of unaccompanied minors and asylum seekers. The main origin nationalities (e.g., Afghanistan, Eritrea, Syria) are balanced. Which country is regarded a *main sending country* is time-variant and depends on the number of recent arrivals.

> According to section 28 of the Asylum Act, the SEM or the cantonal authorities may *allocate a place of stay to asylum seekers.* Refugees *must stay* in the canton to which they have been allocated (Id. art. 74, para. 1.), but may choose to *reside anywhere within that canton* (Foreign Nationals Act art. 36.). If they wish to relocate to another canton, they must apply for permission from the new canton (Id. art. 37, para. 1.).
>
> *Case Constellations: Negative Decision*
> The asylum-seeker requests assignment to a particular canton and cites other reasons than those mentioned above; for example Italian language skills and the desire to be assigned to the [Italian-speaking] canton of Ticino. *The SEM grants the right to be heard in these cases and then justifies the rejection of the re-allocation carefully and appropriately.*

Apart from these channels a handful of practical reasons *could* result in a suspension of random allocation, especially if it is not meaningful to transfer an asylum seeker from a reception center to a canton across the country —in particular, if the person already requested asylum in another European country to which they should be sent back to (*Dublin-cases*), or if the person is detained by the police. In such cases, the asylum seeker is often transferred to a nearby canton. To capture exemptions and to inform the allocation officer about these practical considerations (e.g., whether the person is in custody), caseworkers in the reception centers can enter a free text into a database that is subsequently used by the central allocation officers to assign a canton. Crucially, we obtained this data, so that we observe – and condition on – all relevant information that the allocation officers hold. This allows us to adjust for any potential deviations from the random allocation as we describe in detail in Sections 2 and 3 below, as well as in the Appendix, Section A.3.

Once allocated, asylum seekers must reside in the canton until they obtain a positive asylum decision and a temporary residence permit as a refugee. According to the State Secretariat's most recent annual report (SEM 2020) the average duration of first instance asylum procedures over the last 7 years was approximately 300 days (Hainmueller et al. 2016, report an average of 665 days for the early 2000s). However, even after refugee status may have been granted, the spatial residence restriction to the initially assigned canton remains in effect as long as a person receives social benefits, for instance, in case of unemployment (FNIA-142.20 2008, Art. 37). Moreover, the spatial restriction remains in effect for individuals who have their asylum request rejected but cannot be returned to their country of origin, so-called 'temporarily admitted persons'. Yet, the post-allocation sorting maybe endogenous. For instance, the possibility to move to another canton may correlate with skills through higher employment probabilities, which, in turn, may predict health outcomes. Therefore, we focus exclusively on the initial allocation decision instead of any subsequent residence cantons. However, in Section 3.4 we also show that moving as well as giving birth in another –then the assigned– canton does not correlate with the match between origin and destination language.

**ZEMIS - registry of asylum seekers:** We obtained the full register of all individuals who filed an asylum request in Switzerland between January $1^{st}$ 2010 and December $31^{st}$ 2017. On average, 23,056 arrivals are registered in a year, resulting in a total population of 184,455 individuals, of which 57,105 are women (30.96%). The data includes standard sociodemographic characteristics, such as the nationality, age, and sex, as well as complete administrative information on the arrival date, at which reception center, the time stamp and ID of the allocation officer (who remotely allocate refugees), and the eventual canton the asylum seekers have been allocated to.

Given the possibility of suspending random allocation —particularly for family reunification —we expect and observe a larger share of refugees from officially French–speaking countries allocated to French–speaking cantons than we would expected under random chance alone. To see why this occurs, note that refugees from a French-/Italian-speaking country have a greater likelihood of having relatives in the French/Italian-speaking region as these were free to settle either due to positive asylum requests (and financial independence) or as being regular immigrants, as compared to immigrants from other countries that self-selected independent of the language dimension. We return to this point when assessing potential network effects. Note that we obtain *all* information that is available to the allocation officers at the SEM who perform the remote allocation of asylum seekers, thus, allowing us to test whether placement (conditional on requests, such as family reunification) is indeed uncorrelated with the language match. It is also important to note that in our data only approximately 28.16% of refugees make such a request, and that the allocation officers make numerous allocations a day limiting the scope for any optimized selection.

There are two main approaches that can be taken, which we both pursue below. First, one can either exclude all refugees with a request, or, second, condition on requests and assess potential selection concerns. Either approach will solve the selection problem in the allocation decision. For the main analysis, we prefer using the full sample for several reasons: first and foremost, making a request as asylum seeker is potentially selected, hence, making no request is —relative to a random set of refugees —selected, too. Consequently, dropping cases with requests would restrict external validity and, therefore, would limit the generalizability of our results. Second, statistical uncertainty would increase due to a smaller sample size. Reassuringly, either approach renders similar results with generally larger magnitudes in the restricted sample.

To condition on potential exceptions of the randomization, we make use of the free-text entries after standardization (a detailed description is provided in the Appendix A.3). In brief, we first extract topics and features via Blei et al. (2003)'s Latent Dirichlet Allocation to structure the topics used in the later analyses. We then extract the common features (349 in total), such as 'brother', 'mother', 'acquaintances', or medical issues and validate the predictive power of these features via Regression Trees and Random Forrest algorithms (Breiman 2001; Breiman et al. 1984). That is, we predict whether a refugee made a request to be allocated to a Roman-speaking canton (French or Italian) based on all free-text features extracted. Subsequently, we defined the most predictive features and —in accordance with the privacy rules of the SEM —created a set of indicator variables: for example, the indicator *core family* equals one if anywhere in the free text the words 'mother', 'son', 'daughter' or 'spouse' were used (including various synonyms in various languages).[10] The resulting data set contains an individual identifier, allocation canton and exact date of placement, arrival center, all extracted request characteristics (canton requested, core family, other family, peers, etc.), as well as sociodemographics, such as sex, date of birth, and country of origin. In the Appendix Table A.4.1, we present the descriptive statistics of this data.

## 2.2 Language regions and refugee language

The key feature of our study stems from the fact that refugee allocation in Switzerland —as explained above —is independent of individual (language) skills, while, at the same time, the country is marked by language regions with remarkably sharp margins. These mostly coincide with cantonal borders such that 22 out of 26 cantons are monolingual (see Figure 2, Panel

[10] The data contains sensitive information such as health conditions, family violence, or personal details about family members already residing in Switzerland. To assure privacy protection the SEM allowed us to extract this set of indicator variables from a separate data set without sociodemographic information and to link it back to the full registry via the anonymized social security number. All core variables are listed in Table A.1.1 in the Appendix.

A). The majority of Swiss cantons is German–speaking (17 cantons, approximately 70% of the resident population), four cantons are distinctively French–speaking and the canton of Ticino is the only canton with Italian as official language.[11] As a consequence, asylum seekers who originate from a French- or Italian-speaking country (approximately 30% of all refugees in our population) may be allocated to a language environment in Switzerland that is either completely alien or familiar to them.[12]

We focus on *potential* language skills rather than reported individual language skills. That is, we apply an indirect measure of language proficiency through past exposure to French or Italian based on the prevalence of either language in the refugees' country of origin, in accordance with the literature (e.g., Bleakley and Chin 2004). One may think proficiency might be better self-assessed, that is, by asking whether a refugee speaks a language and how they would assess their skills in a survey. However, this is endogenous to innate ability and confidence and, hence, problematic.[13] We argue that *potential* language skills based on the dominant languages in a refugee's origin country are exogenous and therefore more appropriate for identifying causal effects, and allow at best for an attenuating hence conservative bias.[14]

To assess potential dose-response relationships, we further disentangle language exposure in the origin into *officially* French-speaking countries and countries where French or Italian is *spoken* but not as officially recognized language.[15] Obviously, some countries have only a minortity share of people that speak either French or Italian. However, it is by no means clear whether refugees that arrive in Switzerland are not disproportionally drawn from these.

[11] All three languages plus a Romansh enclave enjoy constitutionally equal footing (Council 1999, Art. 4). However, in a given canton, with the exception of four bilingual cantons in the center of the country, other languages than the dominant local one are practically absent in everyday life (see also Figure A.2.1 in the Appendix).

[12] Note that there is no case in the recent past of refugees emigrating from a German-speaking country.

[13] Put differently, we view the language in the country as less endogenous to the behavioral choices and backgrounds refugees have. For instance, in Syria —one of the largest origin countries in our sample —"[m]any *educated* Syrians also speak English or French, but English is more widely understood" (emphasis added Etheredge et al. 2010, p.9ff). Using self- or interviewer-assessed language spoken, which often only reflects the mother tongue, is likely to fail in capturing skills of understanding. The relationship between language spoken might thus correlate with socio-economic status, which would confound our results. This is not the case when defining language on the country level.

[14] The argument that such indirect measures of potential language skills are less endogenous than individual (self-reported) proficiency has been stressed in other contexts as well. For instance, Lemieux (2006) based on Mincer, who argues that is better to measure *potential* rather than *actual* individual experience.

[15] The colonial past and the path to independence plays an important role for the prevalence of languages. For instance, French is the official and by far most prevalent language in Senegal, while Italian exists in Somalia as a recognized secondary language but with limited prevalence only. Another common example is Algeria where French is lingua franca but not officially recognized. We explain the language indicators in more detail in Section 2.2 below.

There are numerous examples of ethnic, religious, or language minorities being the target of exclusion and violence, such as the Kurds in Turkey or the Yazidi in Iraq. In order to capture all these elements and again to allow for a conservative bias at most, we consider *any* country that has a significant part of the population speaking either French or Italian, or where these languages have official status. As expected restricting to official increases the effect we find.

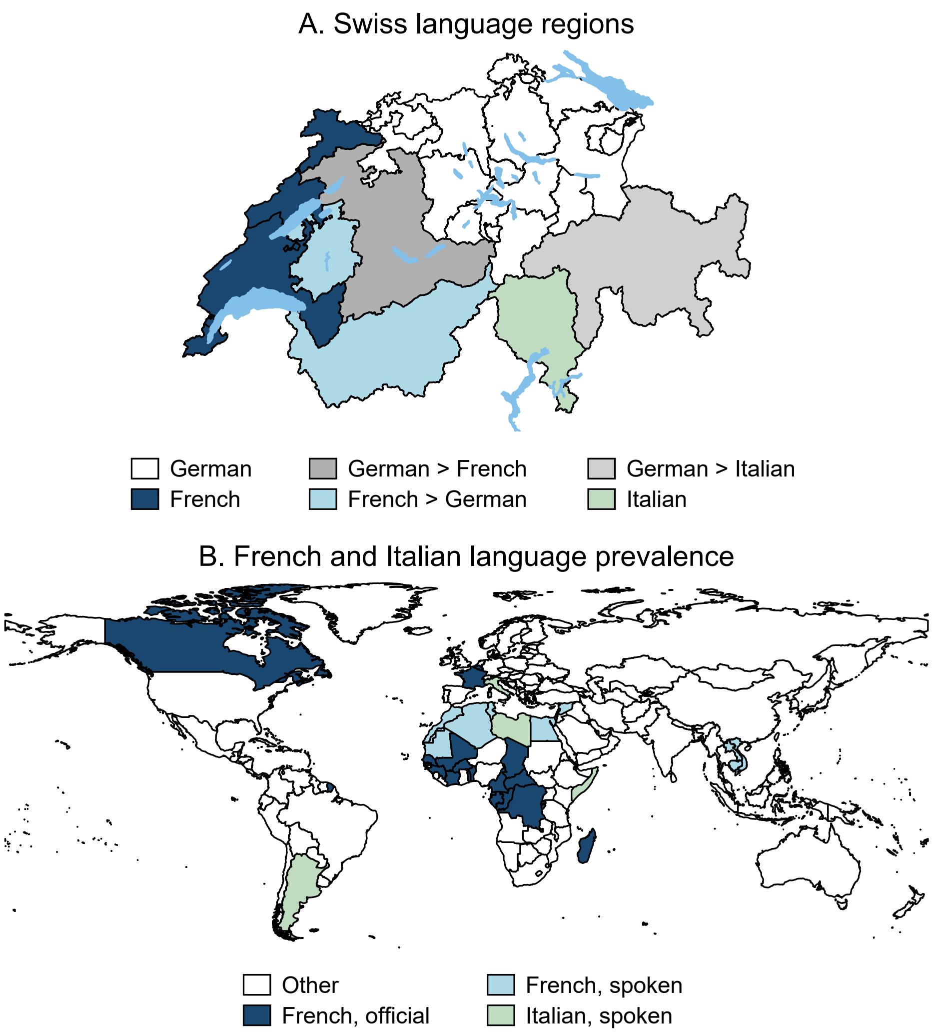


**Figure 2:** Dominant languages in Switzerland and around the World

*Note*: The maps show the dominant languages in Switzerland across cantons (Panel A) and refugees' countries of origin according to the prevalence of French/Italian. The country of origin language exposure is taken from the CIA Facebook (Central Intelligence Agency 2018) and defined to be some exposure (*spoken*) if French or Italian is named among the main languages and *official* if French is an official language (there is no Italian official, nor German spoken or official country in our refugee population). In the Appendix, Figure B.1.1 we show the sample shares and country representation using the effective weighting approach (Aronow and Samii 2016).
*Source:* ZEMIS 2010-2017, CIA 2018, own representation.

**Language usage:** The spoken language of the particular cantons refugees are allocated to are taken from the Federal Statistical Office FSO (2020b), see Figure 2.[16] As stated above, 22

[16]The detailed classification can be found in the Appendix, Table A.4.1 alongside descriptive statistics.

out of 26 cantons are mono-lingual, thus, stratit-forward to classify. For most of our analyses we classify the remaining four by their population shares, that is, whether French is the dominant language spoken. This classification is warranted not least because all multi-lingual cantons have significant shares of a dominant language: In the German-speaking canton of Bern, less than 10% of the population speak French (*German > French* in Figure 2), while in the French-speaking cantons of Fribourg and Valais, about 30% each have German as their first language (*French > German*). In Grisons, German is the majority language, with approximately 15% and 13% speaking Italian and Romansh dialect, respectively (*German > Italian*). As a robustness check, we also exclude refugees allocated to multi-lingual cantons, again increasing the effects we find.

The language borders' distinct nature is highlighted by the small shares of residents who regularly speak another official Swiss language than their home canton's one. Figure A.2.1 in the Appendix provides the language distribution using micro census on 2.3 million individuals between 2010 and 2017, showing that the share of residents (including health personnel) that commonly uses a different language than their dominant local one at home or at work is less than 10 percent. This language pattern does not exclude the possibility that refugees search for doctors who are proficient in their mother tongue. However, it highlights the overall low probability to receive language-adequate services without additional search effort and information.[17]

With regard to the countries of origin our aim is to assess a refugee's exogenous language exposure in their origin. The CIA World Factbook (Central Intelligence Agency 2018) provides a single and consistent data source of languages spoken and official languages in each of the countries we observe in our data.[18] Following the extant economic literature (e.g. Bleakley and Chin 2004), we use the distinction between *spoken/main* and *official* language as a measure of dose capturing different intensities of exposure to French/Italian, because not everywhere where French or Italian is spoken it is also the official language, while the reverse in our definition always holds.[19]

[17] Moreover, any patient-doctor language match could occur for other languages too (e.g. English, Russian, Chinese, or another language of a salient immigrant group in Switzerland).

[18] We also compare this list of French- or Italian-speaking countries which are present among the Swiss refugee population's countries of origin with CEPII's dyadic country-level data (c.f. Mayer and Zignago 2011).

[19] We define *French official* countries: Benin, Burkina Faso, Burundi, Cameroon, Central African Republic, Chad, Congo(Brazzaville), Congo(Kinshasa), Côte d'Ivoire, Djibouti, Equatorial Guinea, Guinea, Haiti, Niger, Rwanda, Senegal, Togo, and *French spoken* additionally: Algeria, Egypt, Lebanon, Morocco, Syria, Tunisia, and *some Italian*: Libya and Somalia. Italian is spoken by minorities in Somalia and Libya. No origin country has Italian as official language. The full list is shown in Appendix Table A.6.1.

In our preferred specification we use a binary indicator and assign 1 to French or Italian language exposure if the person's country of origin has French or Italian *either* as official or as *spoken* language, and 0 otherwise, and define the corresponding language match indicator as 1 if the state is monolingual or dominantly French or Italian-speaking, respectively. That is,

$$\text{Language Match}_{i_{or}} = 1[\text{origin language}_{i_o} = k \wedge \text{destination language}_{i_r} = k]$$

which is individual $i$'s match, as coming from origin $o$ that speaks either $k = (F, I)$, with (French spoken, official French) $= F$ or (Italian spoken) $= I$ and similarly, for the allocated canton (region) $r$, where either (dominant French, mono-lingual French) $= F$ or mono-lingual Italian $= I$. For most of our analyses we collapse all the matching groups into one indicator, *match* or *no match*, to ease interpretation, but assess in detail the separate and possibly heterogeneous effects below. The case where French is an official language and the destination canton is monolingual French is arguably the cleanest way to define allocation to a familiar language environment, and we do find the largest effects in this category. However, we prefer to make conservative choice whenever possible and keep as many refugee mothers as possible. We view these as lower and upper bound of the general match effect. As a placebo test we use the same procedure to classify English, Spanish and Portuguese language.

### 2.3 Health Production and health services

Every resident of Switzerland —including asylum seekers —must be covered by health insurance and is automatically given a standardized social security number, which we subsequently leverage to add individual health outcomes. In asylum reception centers, medical staff treats patients and policyholders are exempted from premium or out-of-pocket payments in case they cannot afford it later on (KVG-832.10 1994). Importantly, this universal health care scheme rules out that different health outcomes are driven by variation in insurance coverage (e.g., Fitzpatrick 2018; Goodman-Bacon 2018b; Ma and Simon 2020), whereby language skills could themselves affect coverage propensity (e.g., Dillender 2017). From a general perspective, the health standards in Switzerland are above average compared to international standards (e.g., Adams et al. 2018).

Part of the standard procedure for pregnant women includes fully covered in-patient services at the hospital and visits at the gynecologist during pregnancy that are not restricted in number if necessary. Services at the gynecologist include standard ultra sound checks, tracking the fetus' growth, checking the mother's blood values. The gynecologist is also typically the professional who refers the mother to a hospital for delivery or for further checks during pregnancy. However, visits at the gynecologist and formal registration at the hospital have to be organized by the mother herself. This includes appointments in the local language, questionnaires and forms about midwifes and prenatal care, medical conditions and similar information important for giving birth and to avoid complications, as well as providing information about lactation counseling and infant care. Arguably, this task of acquiring the necessary information from such a procedure can turn out challenging for a recently arrived mother without language skills and potential exposure to an entirely different health system in her country of origin.

Moreover, patient–doctor communication at Swiss health facilities is complicated by language barriers. Despite increasing numbers of interpreters, Gehrig et al. (2012) discuss that a significant share of in-hospital interaction with foreign-speaking patients is still dependent on ad-hoc translations by non-professionals such as acquaintances or proficient hospital staff (Bischoff et al. 2009, see also). Interpretation services in standard care —including home visits by midwifes —have been largely absent (Ikhilor et al. 2017). Many immigrant women never receive consultations in their mother language and even in case of emergencies, interpretation services are scarce (e.g., Ikhilor et al. 2017). Bischoff et al. (2007) report inadequate language concordance between nurses and asylum seekers in Geneva in about half of the cohort, resulting in an under-reporting of past experiences of traumatic events and significantly fewer referrals to psychological care.

**BEVNAT - vital statistics:** Our second registry data is the universe of all births that took place in Switzerland in the years 2010 to end of 2017, including individual identifiers based on the social security numbers (of the mothers) that allow us to merge refugee history to the birth records.[20] Overall in our data there are 681,124 births from 479,097 distinct mothers.[21] Of those 3,296 cases of stillbirth (a rate of 4.8 in 1,000) and live-birth children were followed up until one year post birth to assess one year infant mortality (2,543, or 3.8 in 1,000, excluding stillbirths). These figures are consistent with aggregate statistics published

[20]We use the 2010 to 2017 refugee population because 2010 marks the first year where the birth registry was equipped with the unique social security identifier to allow for linkage with other administrative accounts.

[21]There are 412,503 births from 280,164 distinct native mothers, immigrants: 257,604 / 194,427, and refugees: 10,798 / 7,872, including all births, twin births and stillbirths.

by the Swiss Statistical Office (FSO 2020a). Further descriptive statistics on the state of child health at birth in Switzerland are presented in the Appendix A.5.

As our main outcomes, we use birth weight, gestation, and weight-by-gestation (often referred to as intrauterine growth restriction, IUGR). The former is probably the most common measure for health at birth and its later consequences.[22] A summary and comparison across developed countries on these is given in Blencowe et al. (2019) and Chen et al. (2016), respectively. We further present discrete outcomes typically considered, that is, (very) low birth weight ([<1500] <2500 grams at birth), (very) preterm ([< 224] < 259 days), small-for-gestational age (lowest percentile of the distribution; e.g. Tolsa et al. 2004), and one-year infant mortality. Due to the relatively small sample size in relation to the fortunately rare event of infant mortality, we are cautious not to overstate these results, however, they point in the same direction as our preferred outcomes.

For most of our analyses we focus on live births following an established literature (e.g., Chen et al. 2016), which assures comparability to other countries and allows for benchmarking to other influences to the child health production function.[23] Furthermore, we mainly focus on the first birth observed in Switzerland to prevent endogenous fertility selection.[24] From the 10,798 births of refugee women we observe, 2,926 are higher order births, 105 twin births, 61 stillbirths, 12 are below 500 grams (c.f. Chen et al. 2016), 6 newborns had missing key variables (birth weight, gestation [5]), which we exclude.[25] 138 mothers had stateless status or no available nationality information, which we include, conservatively, in the control group but drop in a robustness exercise due to the ambiguity of the potential language exposure. This provides us with 7,683 mothers/birth events in our main sample. We test the appropriateness of each of these selection criteria and potential correlation with our main variable, the language match, in Section 3.4, in Table A.5.1 we present several descriptive statistics of these.

Our preferred specification additionally includes commonly used exogenous birth character-

[22]There may be measures other than birth weight that are more predictive of child health and development (e.g. Conti et al. 2020). However, as the authors point out, birth weight continues to be the preferred measure as it is routinely recorded in birth registries.

[23]This means excluding still births, and including only live births with more than 500 grams that are older than 153 days of gestation and have non-missing values in either birth weight or gestation. We also assess whether a corresponding sample selection indicator is correlated with our core variables and find no evidence of endogenous sample selection.

[24]We use the first birth for refugees in Switzerland, which is exogenous with respect to the cantonal allocation. For immigrants, earlier refugees and natives, we do not observe when they entered the country or whether they had a birth before 2010, thus use the first observed birth in the respective analyses.

[25]If the first birth was a stillbirth, we do not use the second birth but exclude the mother from the sample.

istics from this data source, such as parity, sex assigned at birth of the child, and (perhaps) less exogenous ones, such as seasonality of conception and age at birth of the mother; none is correlated with the allocation to a familiar language environment (i.e. language match) as shown below.[26] The covariates do not affect the magnitudes of the effects we report but only their statistical precision, lending further credence to their inclusion and the random assignment.

We complement the universe of refugees and their birth-, and child health outcomes with auxiliary information on both, destination location (canton) and origin country, such as development status (GDP), conflict, female rights, and average elevation as placebo check. On the cantonal level, we add data on the local health care infrastructure, such as the number of hospitals and the cantonal c-section rate, which we describe in more detail in Appendix A and corresponding Table A.1.1.

### 2.4 Communication barriers and infant health

Notably, we capture a language match as a refugee women who has been exposed to French or Italian in the country of origin and who is subsequently allocated to a Swiss canton in which the local language corresponds to her past exposure. That is, we intentionally look at whether or not the local language environment is familiar with regard to the general local population in the destination, which includes, but is not limited to interactions with general practitioners and hospital staff. Here, a growing literature highlights the importance of the *patient–doctor match* (e.g., Hill et al. 2018; Weiss 2020). When patient and doctor share the same language it is not only beneficial for mere information transmission but it may also improve the efficacy of health services through indirect channels, such as increased levels of trust (e.g., Stepanikova et al. 2006; Clayman et al. 2010; Fields et al. 2016). This has been corroborated in observational as well as experimental settings. Aelbrecht et al. (2019) assess around 50,000 patients from 31 countries to show that the experience of negative interactions with physicians significantly increases when patients lack language skills. In an RCT in the US, Alsan et al. (2019) show that black patients who were randomly matched to co-

[26]Note that, among our native mothers, seasonality has a very small but robust association with birth weight early in the calendar year, consistent with the findings in Currie and Schwandt (2013) who attribute lagged influenza effects as the likely mechanism. Yet, we find no such correlation in our arguably much smaller sample of refugees. The magnitudes here as well as in Currie and Schwandt are very small, which renders them hard to estimate. Nevertheless, we control for the month of conception in our preferred specification to account for potential seasonality.

racial doctors are more willing to agree to invasive health services, which could significantly close the black-white male gap in cardiovascular mortality according to the authors. In Australia, Black and Kunz (2019) provide suggestive evidence for differential health care use of immigrants depending on the availability of a co-national general practitioner in the local area. The results of a small qualitative study in Switzerland suggest that communication barriers between patients and health personnel have even lead to situations where refugee mothers did not understand which interventions have been performed and for what reason (Ikhilor et al. 2017).

We argue that the benefits of being allocated to familiar language environment should stretch beyond the patient–doctor match. Especially during pregnancies, many important consultations involve midwifes and nurses and take place outside the institutionalized framework of hospitals and birth houses. In the case of Switzerland, organizational and financial constraints result in interpreter services being extremely rarely used in these contexts (c.f. Ikhilor et al. 2017). On an even more general level, we expect that being allocated to a familiar language environment also improves becoming mothers' access to pregnancy-relevant information. While communication barriers with health personnel may affect the efficacy of medical treatment (e.g., when a patient cannot process all relevant instructions during an appointment), initial knowledge about available services and where to call on them should be equally affected. For instance, as we will show below, many refugees originate from countries where deliveries commonly take place in the absence of a professional. These health system shortcomings likely extend to the pregnancy as well, so that one cannot expect knowledge about comprehensive ultra-sound scans, for instance. That is, in many cases, pregnant refugees may not be aware of the service available to them, so that they have to rely on the local environment to get information. Moreover, as elaborated in Section 2.3, even when refugees have gained knowledge about recommended visits to the gynecologist or the importance of registering with a hospital, the necessary bureaucratic steps typically require some knowledge of the local language, especially given the absence of adequate interpretation services on-site (Bischoff et al. 2009; Gehrig et al. 2012; Ikhilor et al. 2017). In sum, allocation to a familiar language environment can be expected to be not only beneficial for processing information provided by doctors and health personnel but to facilitate knowledge about and access to health services in the first place. In addition to these benefits that relate to the provision of institutionalized health services, being able to communicate with the general population —and in particular with (recent) mothers —may already improve a becoming mother's health behavior. This channels is also supported by the strong evidence on the benefits of prenatal classes (Currie and Gruber 1996; Cygan-Rehm and Karbownik 2020). In Section 5.2 below we assess the role of local networks, providing evidence that such informal diffusion of health-related knowledge may indeed explain —in part —the benefits of being allocated to a familiar language environment.

Apart from formal and informal access to information about health-related behavior and about the availability of health care services, and apart from the direct benefits of a patient–doctor match, a familiar language environment may also improve maternal well-being during pregnancy, which is directly related to in-utero health of their children (Persson and Rossin-Slater 2018). Particularly maternal stress can have serious effects on children if not dealt with appropriately (e.g. Gitau et al. 1998; Torche and Villarreal 2014; Currie et al. 2020; de Oliveira et al. 2021). Aizer et al. (2015) demonstrate the long-lasting nature of the maternal stress–infant health relationship by comparing siblings. Moreover, they find that these negative effects are particularly persistent among mothers with lower human capital, thus, reinforcing the intergenerational transmission of disadvantage.[27] By definition, exposure to stressful situations is very common among refugees who flee from war and persecution and who often migrate under life-threatening conditions, regularly being confronted with violence, exploitation, and abuse. Refugee mothers who have been placed into an unfamiliar language environment in Switzerland may face more difficulties to find way of addressing and mitigating the experience of such stressful events.

# 3 Empirical design and specification

Our framework for understanding the role of a language match in the child health production is an interaction model between mothers' country of origin $O$ language exposure and the dominant language environment in their allocated destination region $R$. To fix ideas, we are interested in the children's health production function:

$$Y = f(X, W), \tag{1}$$

[27] Highlighting the potential long-run impact of prenatal stress on labor market outcomes, Atella et al. (2020) report significantly lower earnings of workers whose mothers were exposed to quasi-random Nazi raids in Italy during World War 2. Hinke et al. (2019) further corroborate the negative effects of maternal stress – induced by a death or major illness of a relative or friend – during and soon after pregnancy during early childhood, but report that they fade out between the ages 11–13.

this conceptual model follows McCrary and Royer (2011) in the related case of educational skills. Child health is modeled to depend on choices $X$ of the mother (i.e. prenatal care, nutrition) as well as their endowments $W$. We are interested how the endowments —language exposure in origin country and allocated destination location —affect child health. The mother's choices may depend on their resources $I$, and endowments and, thereby, the language match and thus communication $C$, such that $X = g(I, C, O, R)$, thus

$$Y = f(g(I, C, O, R), O, R). \tag{2}$$

In this model, the communication skills may influence child health via choices made, that is, prenatal care, smoking, nutrition, etc. via shifting $X$ or $g$, or by shifting productivity $f$, for example, via the ability to process information and communication (Grossman 1972), termed indirect and direct Grossman-effect, respectively by McCrary and Royer (2011). As for education, there are good reason to think that communication human capital —here proxied by the interaction between language exposure (personal) and language spoken (location) —has direct as well as indirect effects. For example, the patient–doctor match has recently been shown to be relevant to health and compliance with instructions in various contexts (Alsan et al. 2019; Black and Kunz 2019; Weiss 2020; Hill et al. 2020). Further, several studies find consistently positive benefits of prenatal classes that are aimed to provide information and support on the process of giving birth (Currie and Gruber 1996; Cygan-Rehm and Karbownik 2020). In this study, we only observe a very limited set of choices and investments, thus, for the most part we assess the reduced form effect of the language match that covers both, direct as well as indirect effects, yet, we discuss heterogeneity to address potential mechanisms below.

The interactive effects of the region (canton) and origin, make it easy to see why simple randomization to region is not sufficient for identification. This is because $R$-specific characteristics might make mothers more productive even in the absence of language effects. For instance, health infrastructure —especially hospitals —might vary with location and urbanization.[28] In fact, the German-speaking region seems more beneficial for all other refugees (who do not come from a language area represented in Switzerland), natives and even non-

[28] Gentili et al. (2017) show that health outcomes for elderly and nursing arrangements differ across language regions in Switzerland, attributing them to different 'health cultures'. Furthermore, Liebert and Mäder (2018) show that a higher physician density in the region substantially lowers stillbirth and infant mortality in Switzerland.

refugee immigrants. In Figure 6, we show that all these potential control groups have a very similar pattern, where the most advantageous birth environment is in the German-speaking region. This is perhaps unsurprising given that the urban centers in the German-speaking part comprise the economically most prosperous regions in the country. Analogously, in the raw data the $O$-background appears to be less advantageous for refugees from both French- and Italian-speaking countries, which is again unsurprising as those countries (often West African nations) belong to the poorest in economic terms.

To isolate the language effect, we therefore use refugees who do not come from a language area represented in Switzerland as the control group in a difference-in-differences design to difference-out any general imbalances between local areas and/or ethnicities/backgrounds. Our empirical specification is, hence, a simple partially linear approximation to equation (2), a straightforward generalized difference-in-differences [DiD] model

$$y_{iort} = \alpha + \tau \text{Language Match}_{ior} + x'_{iort}\beta + \delta_{i_o} + \delta_{i_r} + \delta_{i_t} + \varepsilon_{iort}, \tag{3}$$

where $y_{iort}$ are different birth outcomes, for example birth weight of a child born to a refugee mother (child-mother pair $i$) from origin $o$ (ethnicity), allocated to region $r$ (health environment) in time $t$ (arrival time). Language Match$_{ior}$ corresponds to different measures of the language match —our main variable of interest is the interaction of originating from a French- (Italian-) speaking country and being allocated to a French- (Italian-) speaking canton, $\tau$ is our main coefficient of interest. The allocation canton fixed effects $\delta_{i_r}$, country of origin fixed effects $\delta_{i_o}$ and arrival month and year fixed effects $\delta_{i_t}$, control flexibly for any baseline differences. Thus our identification is driven by the between-canton language comparison of differences in outcomes between refugees coming from language-matching origins and concurrently arriving refugees that have no language representation in Switzerland. We will complement our main strategy with various control groups, perform placebo tests, and robustness tests, such as varying our definition of the language match.

We present various specifications of this model and add covariates in a step-wise fashion. In our preferred specification, we only use exogenous characteristics $x_{iort}$ such as sex assigned at birth of the child or pre-asylum characteristics related to the allocation requests made and the initial allocation itself. Specifically, we adjust for allocation center fixed effects, an indicator whether the female refugee stated a core family member residing in Switzerland, family reunification, whether pregnancy or any medical issues were reported, and, finally, requested canton fixed effects. Moreover, we control for birth-relevant characteristics, namely seasonality using conception month, age at birth of the mother, and the number of previous children, all flexibly via indicator variables.[29] Recall that we focus on the *first birth in Switzerland*, thus, all subsequent children born after the first-born child in Switzerland are excluded from the analysis to avoid biases from higher order fertility selection.

### 3.1 Inference

The treatment level —where the random assignment took place —is $R$ (canton), of which there are 26 in Switzerland. We therefore report additionally to the standard heterogeneity-robust clustered (Bertrand et al. 2004), wild-cluster bootstrapped standard errors accounting for the limited number of clusters.

Further, since we are working with the full administrative records, sampling variability is arguably a minor issue in our setting. In contrast, as our treatment —language origin-destination pair —is the main source of statistical uncertainty, thus we are most concerned about inference on the randomization. We provide complementary significance-tests based on the (Fischer) randomization inference (Young 2019), which randomly re-assigns refugees to cantons (thus treatment status), recalculates the match effect, and finally tests how different the observed effect is from these.

### 3.2 Quasi-experimental design and heterogeneity

The Swiss refugee allocation embeds various treatment and control groups. Using a single match indicator is a parsimonious and general way to assess the average language match effect, yet, it may hide important heterogeneity across 'treatment arms'.

We present a simplified version of our experimental design. It shares similarities with cross-over experimental designs (Kershner and Federer 1981; Jones and Kenward 2014), often regarded as being among the most credible research designs.[30] Figure 3 there are three

[29] Since our model is highly-saturated we expect minor influence of non-linearity in the outcome variables, however, in the Appendix C, Table C.1.2 we present corresponding assessments of the potential non-linearity using poisson and negative binomial for non-negative, and bias-reduced probit models for binary outcomes (c.f. Kunz et al., Forthcoming).

[30] It is not a cross-over design, in the literal sense, as these observe the same individual over time being once in the treatment and once in the control group. Here we observe for example refugees from a French-speaking country as treatment and as control against refugees from an Italian-speaking origin, but not the same individual mothers.

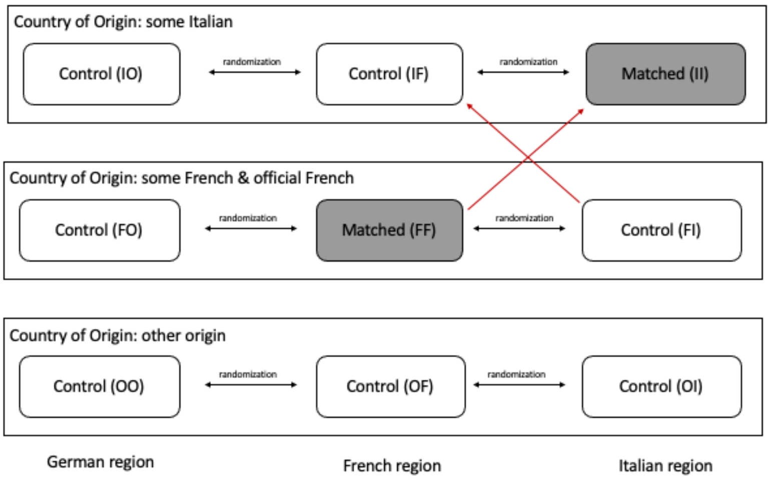


**Figure 3:** SIMPLIFIED EMPIRICAL DESIGN OF THE REFUGEE ALLOCATION

groups of refugees, Italian-speaking country of origin (top), French (middle), and others (bottom), allocated to three language regions in Switzerland, German, French and Italian. Thus, in contrast to a standard 2x2 DiD design, we have *seven* 2x2 sub-DiDs in this 3x3 matrix. Both language matching groups —French exposed refugees in French regions (FF) and Italian exposed in Italian speaking region (II) —can each be compared to three control pairs.

For instance, the four groups in the bottom left corner constitute a simple straight-forward DiD that compares French-speaking refugees in Switzerland's French-speaking region (FF) with those allocated to the German region (FO), and contrasts this difference to the difference *other* refugees have in French (OF) and German region (OO), such that

$$DiD_1 = y_{FF} - y_{FO} - (y_{OF} - y_{OO})$$

Similarly, one can use the Italian region as control, or Italian-exposed refugees in French and German regions. The Italian exposed refugees in the Italian-speaking region have the same three comparison groups. Finally the two treatment groups (FF) and (II) can be compared against each other.[31] It is, however, important to note that refugees originating from an

[31] In sum this gives seven sub-DiDs in this setup. Namely, $DiD_1 = y_{FF} - y_{FO} - (y_{OF} - y_{OO})$, $DiD_2 = y_{FF} - y_{FI} - (y_{OF} - y_{OI})$, $DiD_3 = y_{FF} - y_{FO} - (y_{IF} - y_{IO})$, $DiD_4 = y_{II} - y_{IO} - (y_{FI} - y_{FO})$, $DiD_5 = y_{II} - y_{IO} - (y_{OI} - y_{OO})$, $DiD_6 = y_{II} - y_{IF} - (y_{OI} - y_{OF})$, and $DiD_7 = y_{FF} - y_{FO} - (y_{II} - y_{IF})$. Our setup even allows for a more refined dis-aggregation of 2x2 DiDs, as we can additionally distinguish between origins that have a large share of French speakers and that have French as an official language, and similarly on the canton level, with monolingual versus bilingual regions that do not exclusively speak French. The 4x4 matrix is presented in Figure B.2.1 and discussed in detail. These 25 sub-DiDs are, however, based on increasingly smaller samples, we therefore focus on the 3x3 case and refrain from overemphasizing small sample results.

Italian-speaking country are a small group and there is only one Italian-speaking canton. Still, we belief that testing our hypothesis across all languages to be worthwhile, keeping in mind potential small sample issues.

Recently the aggregation of treatment effect in related settings have been questioned (i.e. Goodman-Bacon 2018a; Chaisemartin and D'Haultfoeuille 2020), due to the potential negative weighting of the sub-DiDs. We will return to this design choice when assessing the implicit weighting in our setup and follow Goodman-Bacon's recommendation in presenting the various DiDs separately. Another issue recently emphasized is the representativity of the regression results (Aronow and Samii 2016; Miller et al. 2019) that are known to place greater weight on groups whose 'treatment' status are not well-explained by the covariates. We, therefore, present the effective weights of our estimates to show which refugees are most likely represented by the reported effects.

### 3.3 Identification and selection

We are interested in whether children born to French- or Italian-language matched mothers are in better health at birth than what would be expected in the absence of their mothers' language match. We use a difference-in-differences design to study these and control flexibly for origin, destination, time and allocation-specific details. Consequently, identification relies on the assumption that refugees originating from French or Italian language environments would have the same change ('path') than other refugees in the absence of their respective language matches, conditional on covariates.

Randomization assures that these groups act as counterfactuals. The overall refugee population's random assignment to cantons was confirmed before (Auer 2018; Bansak et al. 2016; Couttenier et al. 2019). Here, we focus on the case of refugee women, our core sample of refugee mothers and the more general language match classification (including bilingual cantons and language exposure in countries where French or Italian is commonly spoken but not an official language), neither has been assessed or used before. Note that in our setup, even if (conditional) randomization would be violated, results would still be robust as long as violations of full randomization affects refugees from all origins equally. Yet, any selection that varies with match-exposure might confound our comparison, for example a benevolent allocation officer might send refugees likely to benefit from the language match into the respective region, against the explicit law cited above. A number of facts support our main identifying assumption.

First, the overall allocations match very well the theoretical (required by law) allocation shares across all years (cf. Appendix A, Table A.4.1). This holds for our sample of female refugees as well as for the overall refugee population (c.f. Couttenier et al. 2019), which confirms that the random allocation process is broadly followed by the allocation officers, also for the subset of refugee mothers. Additionally, as Marten et al. (2019) note, selection from the side of the central allocation officers is unlikely since they process several allocations a day without directly interacting with their 'clients', thus, making any sophisticated selection unlikely.

Second, since our difference-in-differences model varies in space rather than time we cannot assess the usual pretends. Yet, we have access to the universe of births in the same time frame, thus, can show the general patterns across regions for different control groups: natives, immigrants (incl. accepted refugees arrived before 2010) and assess whether their 'paths' are similar. Figure 6, shows that all groups other than the FF and II behave similarly in their respective control regions. Not only is the German-speaking region most beneficial for all refugees irrespective of their background, but neither does the Italian region help the French exposed, nor the French region the Italian exposed refugees, which is striking.[32]

It might, however, still be the case that although the overall allocation is being carried out in line with the law, those that benefit most are allocated in their language match and those less likely to benefit making the overall numbers appear consistent with the regulation. Therefore, we thirdly assess whether the populations of refugee mothers are indeed similar in terms of composition. Therefore, we use equation (3) but replace the outcome variable with relevant covariate to assess balance across covariates (as is standard practice in such settings, e.g. Pei et al. 2018; Freyaldenhoven et al. 2019).[33] The results are presented in Figure 4.

The balancing regressions are reported for two samples: all female refugees and prospective refugee mothers (we discuss whether there is selection into motherhood below). Neither

[32] Here we group all immigrants together, later we use the immigrants from French/Italian speaking countries separately in a triple difference (DiDiD) framework as robustness test.

[33] In DiD studies, researchers often compare the simple treatment and control difference in characteristics in the pre-period, since our DiD is in space rather than time we belief our approach is more informative. The advantage of our approach is that it uses the full sample (thus has more power to detect differences) and assess not only differences in pre-treatment characteristics but also other mechanical issues in our estimation, for more information on this approach see discussion in (Pei et al. 2018).

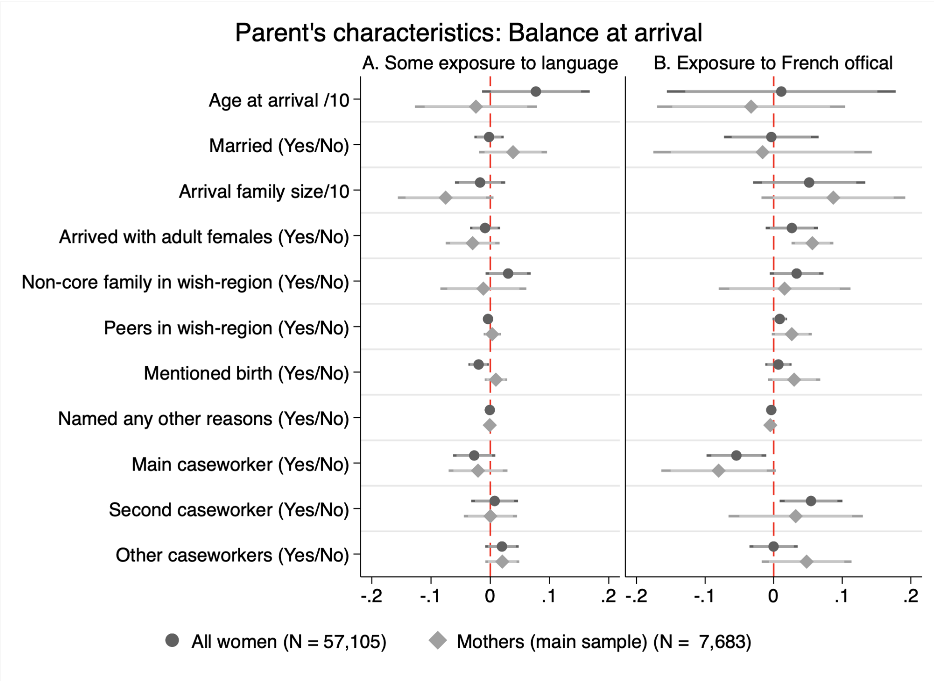


**Figure 4:** Covariate balance among all refugee women and main sample mothers, by any language and French-official match

*Note*: The Figure plots coefficient estimates of regressions of the from in equation (3), using various outcomes in separated regressions, standard errors are clustered and 95% (dark) and 90% (light) confidence intervals are shown. Number of observation indicate our sample selection, where the mother characteristics are assessed in the same sample as in our main regressions, in Column 2 of Table 1, Appendix Table B.1.1 presents the results in table-form.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

indicates that our setup predicts any of the pre-treatment characteristics. Moreover, often they even change signs when comparing the definition from at least some language exposure in the origin country (French or Italian in matching regions, left Panel) to large exposure (only official French speaking countries as treatment group, right Panel). This is reassuring as our main estimates of these will tend in the same direction.

Assessing the individual coefficient estimates, we see that neither the mothers' arrival characteristics in terms of age and whether being married differ in any of the groups. To-be mothers from an officially French-speaking country who are allocated to a French-speaking canton have a slightly larger family/arrival cohort size (the number of refugees allocated together) which also includes more adult women. There is no difference as mentioned in the mothers' requests with regard to family members already residing in Switzerland, nor do they have more peers (relatives, friends, non-close family) in matched regions. Mentioned birth or any other reason in the form does not differ by language match. Finally, we assess whether caseworkers are more likely to send refugees to a language-matching canton, which is not the case overall. Consequently, refugee women seem indeed as good as randomly assigned conditional on allocation characteristics. In Appendix A, Table B.1.1 we confirm these arguments using randomization inference tests (which in this case however are less conservative, as less likely to reject).

### 3.4 Post-placement selection and interpretation

A final potential issue for the interpretation of our results —even assuming fully random allocation —is post-allocation selection (for a very instructive treatment on this issue, see McCrary and Royer 2011). In our case, the key question is whether refugee women have a different propensity to give birth or to give birth sooner/younger. Therefore, we consider various post-allocation characteristics to assess whether the composition of refugee mothers changes with the language match. Figure 5 —analogous to Figure 4 —presents as outcome variables post-allocation and sample selection variables, serving two purposes: first, that the sample selections we perform do not bias our results and, second, that the interpretation of the language match is not confounded by important childbearing characteristics or fertility choices.[34]

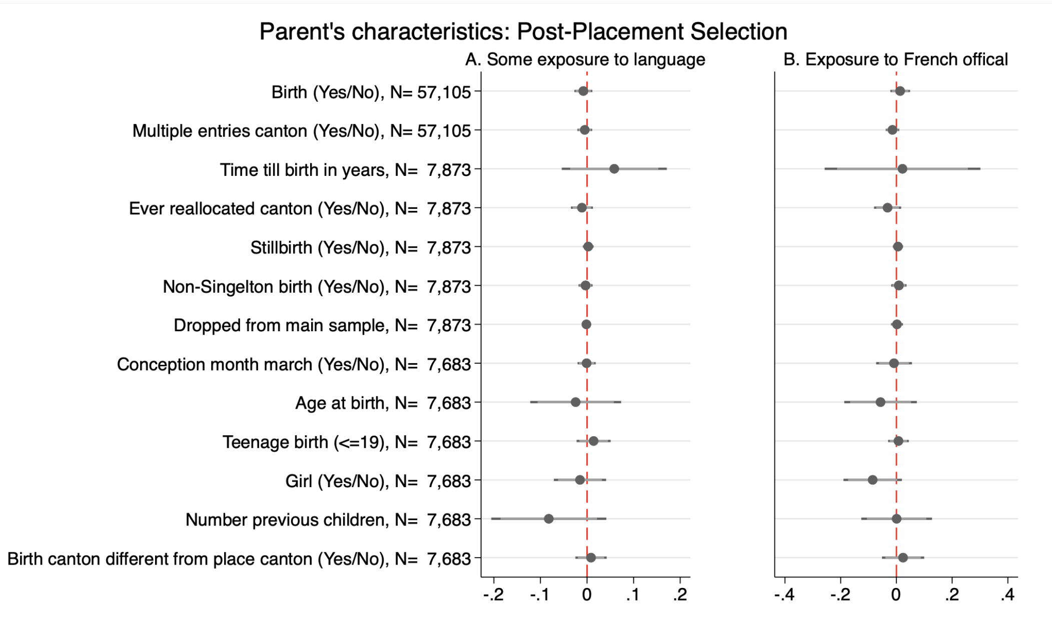


**Figure 5:** SELECTION INTO MOTHERHOOD, THE SAMPLE AND IMPORTANT MOTHER AND BIRTH CHARACTERISTICS

*Note*: Post-allocation balance in characteristics, regressions analogous to those in Figure 4, see notes for details. Appendix Table B.1.2 presents the results in table-form along side randomization inference standard errors.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

[34] Note that this implicitly tests for return or onward migration before birth. Of course return migration and selection into motherhood could potentially cancel each other out, but we have no reason to belief this would be the case.

Most importantly, language-match and other refugee women do not differ in their fertility choice, nor is there any difference in the timing until birth, which could have changed how we think about the benefits of speaking the local language.[35] Selection biases could also arise from false multiple entries into the system; in case the mother was reallocated —that is, moved to another canton after the first allocation, or from mothers being allocated after giving birth in Switzerland. None of them seems to be related to either of our definitions of the language match. Moreover, these are very small categories due to the high quality administrative data. The next set of indicators addresses the few sample selections we perform, that is, we only include first birth, singleton birth, and not too low birth weight or too young children (again following the standard classification in Chen et al. 2016); all of which we find to be uncorrelated with language match in our setup.

Finally, we test whether any of the risk factors for health at birth —identified in a large and well documented medical literature —vary with the language match. There is some evidence that seasonality may be correlated with birth weight (e.g., see discussion in Currie and Schwandt 2013), in Switzerland the main drop in birth weight is in March (see Appendix Figure A.5.1). Yet, language-matching mothers do not have a higher or lower propensity to conceive in March. Further risk factors include age of the mother (and teenage births), whether the mother had previous (live) births[36] and whether the child is a boy or a girl.

It is reassuring that none of these risk factors are varying with the language match, suggesting that neither allocation nor self-selection is benefiting those with the highest expected birth health returns. In sum, it seems that the refugee mothers that were allocated to a canton that matches a language spoken in their home country are similar in the most important birth-related respects to those that were sent to other regions, and, hence, form a valid comparison group.

# 4 Results: Language match and child health outcomes

In Figure 6 we show the comparison between match and non-match across the four refugee groups (other = neither French nor Italian, French spoken, French official, Italian spoken)

[35] Note that we do not observe completed fertility in our sample as we only observe mothers for a limited amount of time, yet we observe how soon after arrival they give birth. Combined with the inclusion of arrival month and year fixed effects all mothers are only compared to mothers that arrived at the same time, thus had the same amount of time to give birth. This makes any issue arising from censoring unlikely.

[36] Previous children are a proxy for parity, which is not explicitly covered in our data.

relative to fellow country refugees in the German-speaking region, and similarly for natives and immigrants. We also show that refugees from a non-Swiss language background have very similar patterns of child health outcomes as both Swiss natives as well as (non-refugee) immigrants, again with the German-speaking region performing best. Impressively, this pattern disappears for refugees whose allocation to the 'worse' French/Italian region means residence in a familiar language environment. Both, French- and Italian-matched refugee mothers clearly give birth to more healthy children than their co-national peers in the German-speaking cantons in terms of birth weight. Again, the effect is stronger for refugees from an officially French-speaking country compared to origin countries where French is spoken, but not an official language.

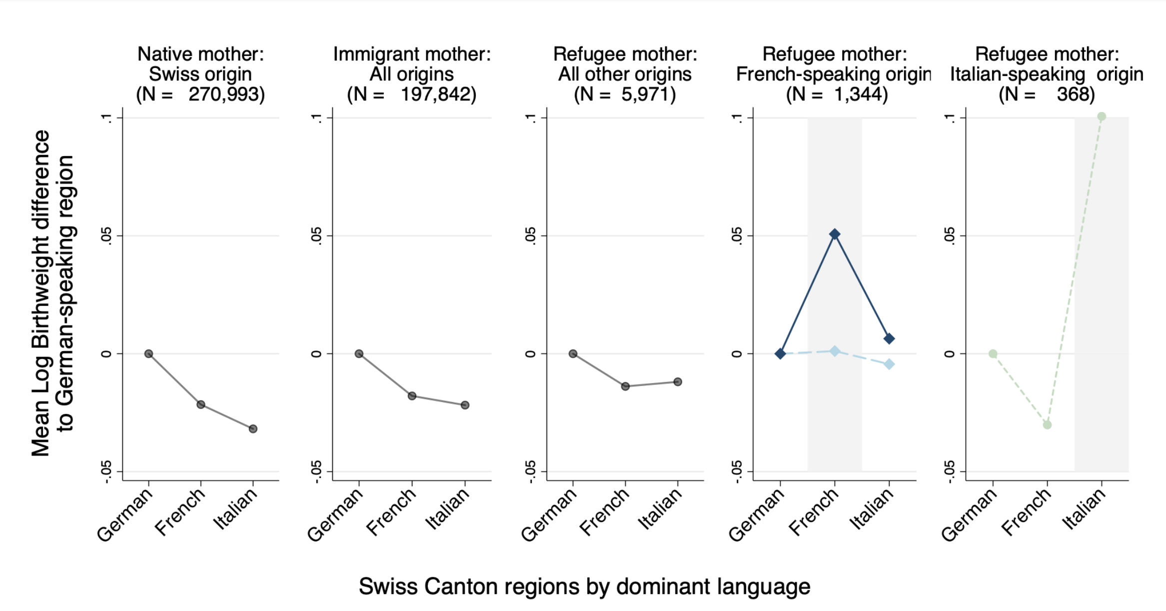


**Figure 6:** Match and non-Match regions average log birth weight differences by country of origin and nativity-migration status

*Note*: Figure presents mean log birth weight of our main sample, standardized by within ethnicity using the German-region as baseline. Panel A, native mothers, B immigrants and earlier refugees. Panel C shows refugee mothers from language regions not represented in Switzerland, D some French exposure (light blue) and official French exposure (dark blue), and E for those from some Italian exposed origins (light green). The respective match groups are shown with the light gray background.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

In Appendix C, in Figure C.2 and Table C.1, we aggregate the comparison using a binary match/non-match comparison which shows that aggregate across the treatment groups. The simple aggregate comparison alongside standard errors shows that the combined language match effect – for the four language groups (other, some French, official French, and some Italian) – is clearly positive and large in magnitude, indicating that refugee mothers being allocated to a familiar language environment are having roughly 2% heavier babies. Table C.1 estimates further confirm what was already evident from Figure 6, that neither do the

French-exposed refugees benefit from the Italian-speaking region, nor do Italian-speakers benefit from a French-surrounding neither economically nor statistically.[37]

As noted above, there is a somewhat larger share of official French exposed refugee mothers in the French region than expected under random chance alone, a simple explanation is that these are more likely to be in the position to claim family reunification than other refugees if their relatives settled in this region. Noteworthy, this elevated share is not observed for either some French in French, some Italian in Italian, nor official French in bilingual but dominantly French speaking regions, all of which exhibit match effects in our estimations. We show that our results do not rest on these, we condition on family reunification and also drop all refugees that requested a specific canton later on in the main analysis below. Nevertheless, in Table C.1 we use common bounding approaches to address whether the elevated share impacts our aggregate evidence. We find that Horowitz and Manski (2000) and Lee (2009) conservative bounding approaches return intervals that are almost exclusively in the positive domain, together with earlier evidence on the non-existence of compositional differences (Figure 4) and post-allocation selection (Figure 5), this raises the credibility that there is indeed a benefit of being placed in a familiar language environment.

In Table 1, we present a series of alternative specifications for the three main outcomes: log birth weight (Panel a), gestation (Panel b) and weight-by-gestation (IUGR, Panel c). Column 1 augments the descriptive aggregate evidence above with individual data on canton-, origin-, as well as arrival month and year-fixed effects, that is, the generalized DiD model as motivated in eq. (3) above. Log birth weight is clearly affected by the language match. The coefficient is statistically important as indicated by all three types of standard errors and large in magnitude. Being allocated to a familiar language environment corresponds to an approximately 2% increase in birth weight, which amounts to an additional 65 grams at the sample mean of 3261.68 grams. Consecutively adding covariates this language-match effect is almost unchanged, which is what we would expect under random allocation. Our preferred specification is thus Column 3 where adjusting for allocation-relevant- and birth-characteristics renders the most precisely estimated coefficient of 2.2% higher birth weight on average (or 72 gram).[38]

[37] In the Appendix Figure C.1 we present the average outcomes referring to the initial Figure 1 for gestation, IUGR, and proportion of low birth weight. Appendix Figure C.2 shows the collapsed version, match versus non-match by country of birth language exposure.

[38] As pointed out by Oster (2017) this type of coefficient-stability argument is only meaningful if the added covariates are informative. The $R^2$ doubles in our case, which raises confidence that this exercise is meaningful.

**Table 1:** Main results

| | GDiD | Covars | Conditional | French off | No Bilang | Placebo treatments: Eng/ Esp | Placebo treatments: Elevation |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel a.* Log birth weight | | | | | | | |
| match | 0.018 | 0.020 | 0.022 | 0.064 | 0.030 | 0.000 | -0.007 |
| | (0.007) | (0.008) | (0.008) | (0.016) | (0.009) | (0.009) | (0.008) |
| Wild-Bootstrap | $p = 0.06$ | $p = 0.08$ | $p = 0.06$ | $p = 0.01$ | $p = 0.02$ | $p = 1.00$ | $p = 0.40$ |
| RI | $p = 0.06$ | $p = 0.06$ | $p = 0.02$ | $p = 0.01$ | $p = 0.03$ | $p = 0.99$ | $p = 0.53$ |
| Control Mean | 8.06 | 8.06 | 8.06 | 8.02 | 8.05 | 8.09 | 8.11 |
| R2 | 0.03 | 0.04 | 0.06 | 0.06 | 0.08 | 0.05 | 0.06 |
| *Panel b.* Gestation in Days | | | | | | | |
| match | 0.478 | 0.443 | 0.465 | 0.801 | 0.959 | -0.459 | -0.515 |
| | (0.540) | (0.563) | (0.569) | (1.098) | (0.547) | (0.755) | (0.583) |
| Wild-Bootstrap | $p = 0.37$ | $p = 0.44$ | $p = 0.41$ | $p = 0.44$ | $p = 0.08$ | $p = 0.60$ | $p = 0.43$ |
| RI | $p = 0.45$ | $p = 0.49$ | $p = 0.48$ | $p = 0.59$ | $p = 0.22$ | $p = 0.60$ | $p = 0.52$ |
| Control Mean | 274.68 | 274.72 | 274.70 | 274.24 | 274.35 | 278.69 | 277.80 |
| R2 | 0.04 | 0.05 | 0.06 | 0.06 | 0.07 | 0.06 | 0.06 |
| *Panel c.* IUGR (Weight/Gestation) | | | | | | | |
| match match | 0.184 | 0.201 | 0.221 | 0.629 | 0.291 | 0.042 | -0.048 |
| | (0.068) | (0.076) | (0.073) | (0.173) | (0.074) | (0.103) | (0.066) |
| Wild-Bootstrap | $p = 0.06$ | $p = 0.07$ | $p = 0.06$ | $p = 0.02$ | $p = 0.01$ | $p = 0.70$ | $p = 0.45$ |
| RI | $p = 0.04$ | $p = 0.04$ | $p = 0.02$ | $p = 0.01$ | $p = 0.01$ | $p = 0.81$ | $p = 0.57$ |
| Control Mean | 11.67 | 11.66 | 11.63 | 11.34 | 11.59 | 11.79 | 12.11 |
| R2 | 0.03 | 0.04 | 0.07 | 0.08 | 0.09 | 0.07 | 0.07 |
| *N* | 7,683 | 7,683 | 7,683 | 7,426 | 5,850 | 6,306 | 7,683 |
| Canton FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Country FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Placement Year & Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Request FE | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Birth characteristics FE | | | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes*: Table presents coefficient estimates of the model (3) regression, and cluster (Canton) robust standard errors, wild cluster Bootstrapped p-values (499 replications), and randomization inference (Canton-level re-randomization) p-values (99 replications), and the counterfactual control mean, estimated as raw mean of the non-treatment group in the respective estimation sample. *Column 1* only includes Canton, country, placement year and -month fixed effects. *Column 2* adds characteristics used in the conditional random allocation (reception center, core family, medical reasons, Canton allocation request). *Column 3* —our main specification —adds additional birth characteristics (sex assigned at birth of child, age of the mother in years [indicators], conception month [indicators], parity - number of children [indicators]. *Column 4* redefines treatment as official language, thus places refugees from countries with some French- (i.e. Algeria) or Italian-exposure (i.e. Somalia) in the control group. *Column 5* reverts to Column 3, but excludes multi-language Cantons: Grisons, Bern, Fribourg and Vallais. *Columns 6 and 7* redefine the origin-allocation Canton match to equal 1 if refugees from English/Spanish/Portuguese-speaking countries are allocated Cantons with Roman language or if refugees from countries with above-average elevation are placed in higher elevated Cantons, respectively.
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

Model specification (4) provides perhaps the cleanest treatment group with refugees that come from a country where French is the official language only. As expected, the match effect is significantly larger at 6.4% (or 200 gram from 3127.58 gram). In comparison to the literature Rossin-Slater 2013; Cygan-Rehm and Karbownik 2020, this magnitude is substantial, albeit meaningful considering that, first, refugee countries with French as official language rank among the poorest in economic and health terms and, second, refugees from these countries are almost certainly to be able to communicate in French, so that the gains from a familiar language environment ought to be largest (further country-of-origin heterogeneity is discussed in Section 5.)[39] Restricting the language match to monolingual French-speaking cantons only (Column 5) similarly increases the language match relative to Column 3. These results indicate that there is a type of *dose-response* where more exposure on both origin and destination raises health at birth. We will return to this aspect below.

Eventually, Columns 6 and 7 present complementary placebo checks. In 6 we drop French and Italian-exposed refugees but use English and Spanish/Portuguese-exposed refugees (all from the Roman language family), pretending they would match in the French or Italian region, respectively. In 7 we test whether our language match might be picking up other factors, thus we mimic the distribution of match via categorizing origins according to their average altitude as above-average elevation countries, and the same for the cantons in Switzerland. The match indicator equals 1 if a refugee likely from a mountainous region is randomly placed in a mountainous region in Switzerland. Reassuringly, neither placebo specification shows any sign of match effects.

Besides birth weight, gestation (often preterm birth incidence) is the second key indicator for neonatal health (e.g., Chawanpaiboon et al. 2019). However, in Panel b of Table 1 we show that the difference in birth weight from Panel a is not 'explained' by differential length of gestation, thus seems to be a purely low birth weight for gestational age effect. Interestingly this is in line with findings by (e.g., Cygan-Rehm and Karbownik 2020) for incentivized pre-natal care class up-take, another information treatment.

To reflect the growth of the fetus relative to its gestational age we simply divide live births' weight by their gestation in days. As a consequence the main impact of the language match is on the weight-by-gestation, as shown in Panel c. To put our results into perspective and to provide leads for likely mechanisms, it is useful to briefly reflect on the medical determinants and consequences of IUGR. Intrauterine life prioritizes the growth of the brain over other organs. In a situation of inadequate supply with nutrients, supporting the brain growth goes at the expense of general growth, eventually leading to newborns in the lowest percentiles of the birth weight (and height) distribution, that is, *small-for-gestational age* SGA (Valsamakis et al. 2006). The "failure of the fetus to achieve its full growth potential" (Smith et al. 1997)

[39] Note that there are two approaches to estimating the counterfactual mean; one is to use the average among refugees from French and Italian heritage in the German region, another is to calculate the mean in the match group and subtract $\tau$, or from the simple difference in differences in raw means. Neither impacts our results but change the magnitudes, here we use the raw mean in the treatment group and subtract $\tau$, thus as the treatment groups change the mean changes.

can have many causes, mostly related to placental insufficiency, and about one third being of genetic origin (Pallotto and Kilbride 2006; Valsamakis et al. 2006; Hendrix and Berghella 2008).[40] Established risk factors include low socioeconomic status (Pallotto and Kilbride 2006), maternal malnutrition, alcoholism, smoking, hypoxia and, not least, psychological stress (e.g., Neerhof 1995; Hendrix and Berghella 2008; Lodygenski et al. 2008; Figueras and Gardosi 2011; Salam et al. 2014).

The consequences of growth restriction can be severe. In-utero, IUGR increases the risk of fetal- as well as perinatal death by five to ten times (Pallotto and Kilbride 2006; Marconi et al. 2009).[41] Moreover, growth restricted children continue to face negative long-term effects on health (e.g., Rueda-Clausen et al. 2011) and cognitive ability (e.g., Strauss 2000).[42] Crucially, IUGR is preventable to a certain extent and can be alleviated if detected early (Pallotto and Kilbride 2006). In particular, if IUGR is caused by maternal stress or habits (e.g., nutrition), which again could be treated or corrected if detected early.

Overall, our results provide a striking picture of substantially higher birth weight —relative to interventions assessed in the literature —when refugee mothers by chance are allocated to a familiar language environment. This effect holds to a number of different model specifications and placebo checks.

### 4.1 Robustness

The estimated positive causal effect of the language match on infant health is robust to a number of additional checks and specifications. First, in Table C.1.1, we show that our conclusions are largely confirmed by drawing on various sub-samples. In Columns (2–5) we drop all refugee mothers that provide any request; use only refugees from Africa (the continent with the highest refugee prevalence); drop stateless refugees from the control group; and drop the two largest sending countries (Syria —some French —and Eritrea —control). In Columns

[40] Note that in our design, ethnicity is captured by the country of origin fixed effects. That is, we do not observe actual ethnicity but the randomization makes it unlikely that our results are effected by ethnicity differences.

[41] Other negative health outcomes, such as respiratory distress syndrome and chronic lung disease (Tyson et al. 1995; Bernstein et al. 2000), severe intraventricular hemorrhage (Simchen et al. 2000; Yinon et al. 2005), sepsis (McIntire et al. 1999) or short-term memory difficulties (Geva et al. 2006) are also closely related to IUGR, and it regularly compromises the immune system (Chandra and Matsumura 1979).

[42] Berthelon et al. (2018) find that children face lower levels of cognitive development at the age of 2, if they were exposed in-utero to maternal stress, while Pryor et al. (1995) and Hollo et al. (2002) find average IQ reductions of 4 to 8 points. IUGR adults show lower teacher ratings, lower academic achievement and are less likely to work in a managerial position (Strauss 2000).

(7–8) we also drop all refugees that provide any request but consider a language match only for origin countries where French is an official language; and, finally, we use only English-speaking countries as control group. The positive effect of the language match is even larger in 6 out of 7 alternative sample definitions than in our preferred conservative specification (1), lending further support that our reported coefficient is —if anything —a conservative lower-bound estimate.

Second, we confirm that the OLS approximation is innocuous in our setting by running various alternative specifications exemplary for the log birth weight specification, which would be most prone to issues due to the log-transformation of birth weight. Concretely, in Table C.1.2 we estimate an OLS model on the non-logged birth weight, a Poisson (Silva and Tenreyro 2006), and negative binomial generalized linear regression specification for the log birth weight (also on the non-logged birth weight; Jones 2000). The results are almost indistinguishable from those discussed here for the overall match effect, and slightly attenuated for the French-official only match.

Third, we evaluate the triple difference model using non-refugee immigrants in the birth data as additional control group.[43] As was already suggested in Figure 6, all immigrant groups (and Swiss natives) have very similar trends compared to refugees that have no language match in Switzerland. Table C.1.3 presents the respective triple-difference [DiDiD] regressions results. Interestingly, immigrants (and settled refugees) have no detectable benefit from the language match, which could be explained by self-selection into locations (after acceptance) or time in the country and subsequent language acquisition. Reassuringly, for our effect of interest, the additional control group strengthens the conclusions drawn above, by further increasing statistical significance due to the much increased sample size.

## 4.2 A perspective on weighting

Before we turn to effect heterogeneity and investigate likely mechanisms in more detail, we briefly discuss the issue of (implicit) regression weighting in our empirical design. As noted above, the defining match indicator across languages (French, Italian) and extent of exposure (spoken, official) is a parsimonious way to gauge the effect of maternal language familiarity on birth weight, thus improving external validity and representativity.

[43] As noted above these may include refugees that arrived before our observation period, that may have been accepted and able to move.

However, in any fixed effect regression the identification is driven by so called switchers (Miller et al. 2019), which could lead to a situation where the estimated effects are potentially not representative of the whole sample. A simple way to assess these issues is by calculating the implied weights in the regression, following Aronow and Samii (2016). Appendix A, Table A.5.1 shows the sample statistics both overall in the estimation sample and including the effective weighting implied by our estimation. It is evident that the regressions using all comparison groups are much closer to the overall refugee population than those based only on the sample of refugees from a French official country only. For instance, refugee mothers have 0.441 previous children on average, in the 'effectively-weighted' sample corresponding to Column 3, these are 0.408, but in the restricted French-official sample (Column 4) the expected mean is only 0.227. This corroborates our more conservative and representative approach.[44]

Aggregating different treatment groups is not always innocuous. Recent contributions (e.g., Goodman-Bacon 2018a; Chaisemartin and D'Haultfoeuille 2020) have cast doubt on the validity of aggregating staggered treatment groups, that is, when more than 2x2 possible comparisons exist. This is precisely the case in our setting, where treatment groups can act as controls for other treatment groups —in extreme cases —their weights in the aggregation could be negative. While the treatments discussed in the literature so far do not cover our case of spacial variation (instead of variation over time) and switching of treatment, it is obvious from Figure 3 that we are aggregating across different 2x2 DiDs, thus similar problems of potentially negative implicit weights might arise.

Again, a simple way to assess whether this is an issue in our application is to show all implied treatment groups and corresponding difference-in-differences estimates separately (Goodman-Bacon 2018a, proposes this in his Appendix), as shown in Figure 7. For example, among the seven possible DiDs for log birth weight, see Figure 3, for the 3 outcomes) only one comparison is negative and marginal significant, which may be expected under random chance alone. Also almost all of these are insignificantly different from our main specification and almost all are larger than our preferred specification, again confirming that we apply the most conservative approach. It is important to note that for all comparisons involving I-refugees (Italian-speaking origin) and the one I-canton in Switzerland (Ticino) we expect more variability due to the much smaller treatment group. Yet, they are strongly consistent

[44] In Appendix B, Figure B.1.1 we show the different effective origin country sample weights.

with our hypothesis but perhaps should not be over-interpreted.[45]

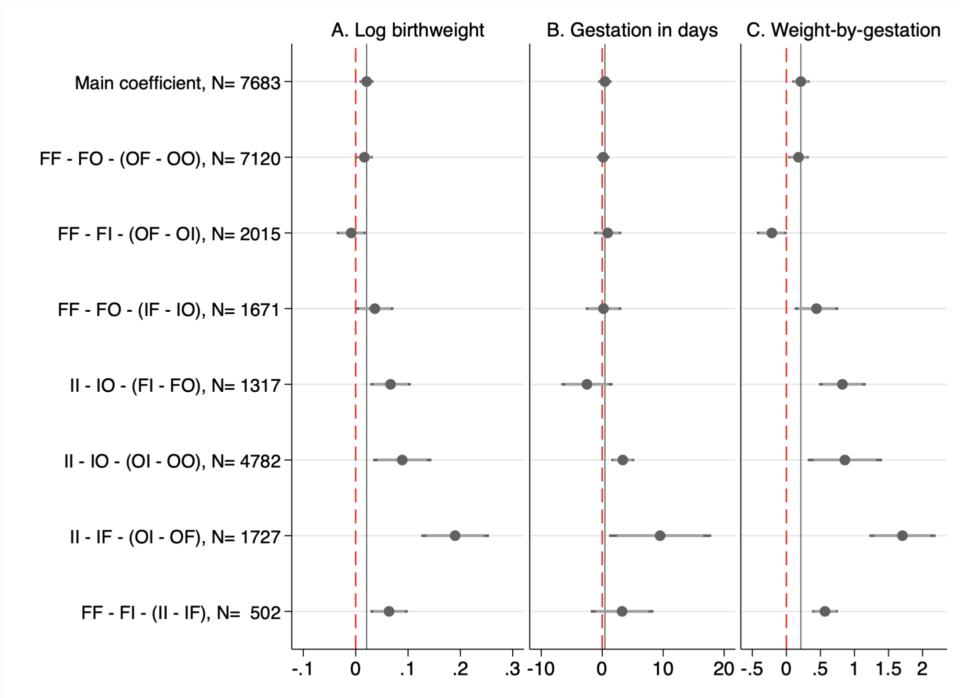


**Figure 7:** DiD DECOMPOSITION

*Note*: Figure displays separate DiD regressions keeping only one of the refugee match groups and areas in Switzerland. The first row displays the aggregate estimates from Table 1 Column 3 all three panels (log birth weight, gestation, and IUGR). The sub-DiD comparisons can be seen in Design Figure 3. The first corresponds to the bottom DiD, comparing the difference between F-rench-refugees in F-rench-speaking compared to those O-ther areas which O-ther refugees in the O-ther areas (German). In Appendix Figure B.2.2 we further split these into 25 groups that are however often very small.

*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

## 4.3 Clinical health indicators

Next, we investigate whether the average health effect is driven by specific parts of the birth distribution. Concretely, we replace our continuous outcomes (birth weight, gestation and weight-by-gestation) with clinical indicators of ill-health at birth that are regularly adopted in the literature: Column 2 in Table 2 shows that the probability of a child being born with low birth weight ($< 2,500$ grams; e.g. Blencowe et al. 2019) decreases when refugee mothers

[45] Our setup even allows for a further disaggregation, distinguishing between bilingual French and French regions, and French Official and some exposure. This design would imply 25 separated 2x2 subDiDs. Obviously, sample sizes become relatively small, yet, they tend to confirm the disaggregated treatment effects presented in Figure 7 (F-rench, I-talian, O-ther origin/canton combinations indicated with capital letters).

are allocated to a familiar language environment.[46] We do not find such an effect for very low birth weight ($< 1,500$ grams; Column 1), nor high birth weight probability ($> 4,500$ grams; Column 3) of the language match. This shows that not only is the middle of the birth weight distribution affected but the important and extensively studied LBW cut-off that is highly predictive of future health outcomes and chances in life.

**Table 2:** ALTERNATIVE BIRTH OUTCOMES: CLINICAL HEALTH INDICATORS

| | vLBW ×100 | LBW ×100 | HBW ×100 | vPT ×100 | PT ×100 | LT ×100 | SGA ×100 | IMR ×1000 |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel a.* Match | | | | | | | | |
| match | 0.449 | -2.928 | 0.578 | -0.254 | 0.997 | -0.247 | -2.767 | -0.806 |
| | (0.436) | (1.393) | (0.552) | (0.312) | (1.305) | (0.255) | (1.589) | (4.336) |
| Wild-Bootstrap | $p = 0.37$ | $p = 0.05$ | $p = 0.36$ | $p = 0.52$ | $p = 0.46$ | $p = 0.41$ | $p = 0.08$ | $p = 0.91$ |
| RI | $p = 0.34$ | $p = 0.12$ | $p = 0.33$ | $p = 0.50$ | $p = 0.52$ | $p = 0.31$ | $p = 0.13$ | $p = 0.87$ |
| Control Mean | 0.36 | 6.98 | -0.31 | 0.25 | 5.49 | 0.25 | 12.23 | 6.21 |
| R2 | 0.03 | 0.04 | 0.03 | 0.02 | 0.04 | 0.04 | 0.05 | 0.02 |
| *Panel b.* Match only French official | | | | | | | | |
| match | 0.149 | -8.414 | 0.373 | -0.762 | -3.362 | -1.858 | -12.848 | -2.909 |
| | (1.069) | (3.054) | (0.192) | (0.914) | (2.790) | (1.122) | (3.579) | (1.796) |
| Wild-Bootstrap | $p = 0.89$ | $p = 0.04$ | $p = 0.06$ | $p = 0.67$ | $p = 0.26$ | $p = 0.18$ | $p = 0.03$ | $p = 0.17$ |
| RI | $p = 0.83$ | $p = 0.04$ | $p = 0.15$ | $p = 0.89$ | $p = 0.33$ | $p = 0.23$ | $p = 0.05$ | $p = 0.10$ |
| Control Mean | 0.74 | 10.18 | -0.37 | 0.76 | 7.79 | 1.86 | 20.81 | 2.91 |
| R2 | 0.03 | 0.04 | 0.03 | 0.02 | 0.04 | 0.04 | 0.05 | 0.02 |
| *N* | 7,683 | 7,683 | 7,683 | 7,683 | 7,683 | 7,683 | 7,683 | 7,683 |
| Canton FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Country FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Place Year & Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Request FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Birth characteristics FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes*: Table presents coefficient estimates of the model (3) regression, and cluster (Canton) robust standard errors, analogous to Column 3 and 4 in Table 1 for various discrete outcomes. Table C.1.2 estimates a bias reduced fixed effect probit model (Kunz et al., Forthcoming) on the low birth weight indicator, for example, to show that the linear probability model specification is appropriate. *vLBW* and LBW = (very) low birth weight; *HBW* = high birth weight; *vPT* = very pre-term (<196days); *PT* = pre-term (< 259days); *LT* = late-term (> 294days); *SGA* = small for gestational age (lowest 10 percent percentile of weight/gestation distributiuon); *IMR* = infant mortality. *Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

Echoing our earlier results, we do not observe any significant effect whatsoever on pre- and late-term probabilities (Columns 4 to 6; e.g. Chawanpaiboon et al. 2019). Yet, again, the binary indicator for *small-for-gestational age* [SGA] children (ratio is within the lowest 10-percentile of the distribution Smith et al. 1997) is statistically significant and large in magnitude. Being allocated to a familiar language environment reduces the probability of an SGA-child by 3% for some language exposure and 13% for refugees from an officially French speaking country, respectively.

[46] We also estimate a bias reduced fixed effect probit model (Kunz et al., Forthcoming), with almost indistinguishable results, confirming the linear probability esitmates presented here.

Eventually, a language match for refugees from an officially French-speaking country reduces infant mortality —the most extreme outcome —by approximately 3 per 1,000 live births. This effect is only marginally significant for the official French upper bound and only one standard error approach. This is likely because the very few occurrences in our data, thus shall not be over-interpreted. Yet, as one year IMR is often a direct consequence of LWB births, it is consistent with our main results, to provide a reference its magnitude corresponds to the IMR disadvantage of the US (ranked $51^{st}$) compared to the top-performing Scandinavian countries (Chen et al. 2016).[47]

# 5 Heterogeneity

Having established that allocation to a French- (Italian-) speaking region only makes a difference for refugee mothers from a respective French- (Italian-) speaking country of origin, the obvious explanation is that these mothers are likely able to communicate with their environment in case of a language match. To corroborate the proposed mechanism of a *large informational burden* of refugee mothers allocated to an unfamiliar language environment —and not least to inform about potential avenues for policy —we test for possible heterogeneity at the origin–level (Section 5.1). We assess factors that are plausibly relevant for the effect size, such as being exposed to major violence in the origin country. On the other hand, we assess factors that could directly alter/substitute the informational burden of the refugee mother through networks (Section 5.2) or residence duration (Section 5.3).

## 5.1 Heterogeneity, by individual, country and region characteristics

In this section, we assess heterogeneity to highlight groups most affected by the language match and examine potential channels. In doing so, we run a series of regressions in which the match indicator in model (3) is replaced with $\tau_d d \times match + \tau_{\bar{d}}(1-d) \times match$, with $d$ representing various subgroup indicators.[48]

We assess a series of possible dimensions that we group into country of origin–, destination

[47] Note that we consider stillbirth as potential post-placement selection and have assessed this outcome in Section 3.4 above. Table 5 shows that being allocated to a familiar language has no significant effect on stillbirth, which, however, may also be driven by the small number of such events.

[48] This is analogous to estimating sub-sample regressions, with the additional advantage of estimating the large array of fixed effects on the full sample. The complementary sub-sample approach —available upon request —renders qualitatively similar estimates.

canton–, and individual–level heterogeneity. Among the latter two, we find little variation in size of the language match coefficient, so that we show the corresponding regression results in the Appendix C, Figure C.2.1. The only significant differences we observe is by sex assigned at birth of the newborn, with boys benefiting more if their mothers has been allocated to a familiar language environment. This is consistent with the *fragile males hypothesis*, which describes the general pattern that a male fetus shows a stronger susceptibility for environmental influences (e.g., Eriksson et al. 2010; Barham et al. 2013). Moreover, the language match coefficient is greater when the mother arrives without her family so that she who could not rely on an immediate family network at arrival (Fadlon and Nielsen 2019).[49] This finding is consistent with the beneficial effects of a local network, which we will elaborate in Section 5.2. Eventually, the analysis provides some indication that riskier pregnancies other than the newborn's sex, that is, when the mother is older than 35 years and/or it is her first baby, benefit more from a familiar language environment.

In Figure 8, we focus on the heterogeneity of the mothers' origin, where the most variation can be observed. Each upper dark marker corresponds to the coefficient of $(1 - d)$ and the lower lighter one to $d$, which is when the statement is true. Confidence intervals are based on robust standard errors clustered on the treatment region (canton) level, $Pr(d)$ denotes the sample share with $d = 1$, and *p-val* the p-value of an F-test for equality of the two coefficients (first one for Panel A. and second for Panel B).A description of the data sources and operationalization is provided in Appendix A, Table A.1.1.

In general, we would expect that refugees are more "dependent" on the language match if they arrive from a country with a dysfunctional health system, that has been tormented by conflict and/or where women's rights are underdeveloped. This is because refugees from a higher-income country or where health standards are closer to Switzerland are likely better equipped to process health-relevant information, for instance, through knowledge of typical services or simply through education. Moreover, trauma and physical consequences of exposure to violence and conflict should increase the language match effect if this means that fewer/no communication barriers in Switzerland result in better treatment.

[49]We do not observe family directly. However, given the details provided in the administrative file, it is straightforward to construct an arrival-cohort identifier. We categorize refugees coming from the same origin, with the same request, being allocated by the same caseworker, sent to the same canton, on the same day and in consecutive order as pseudo family. We return to this cohort measure below.

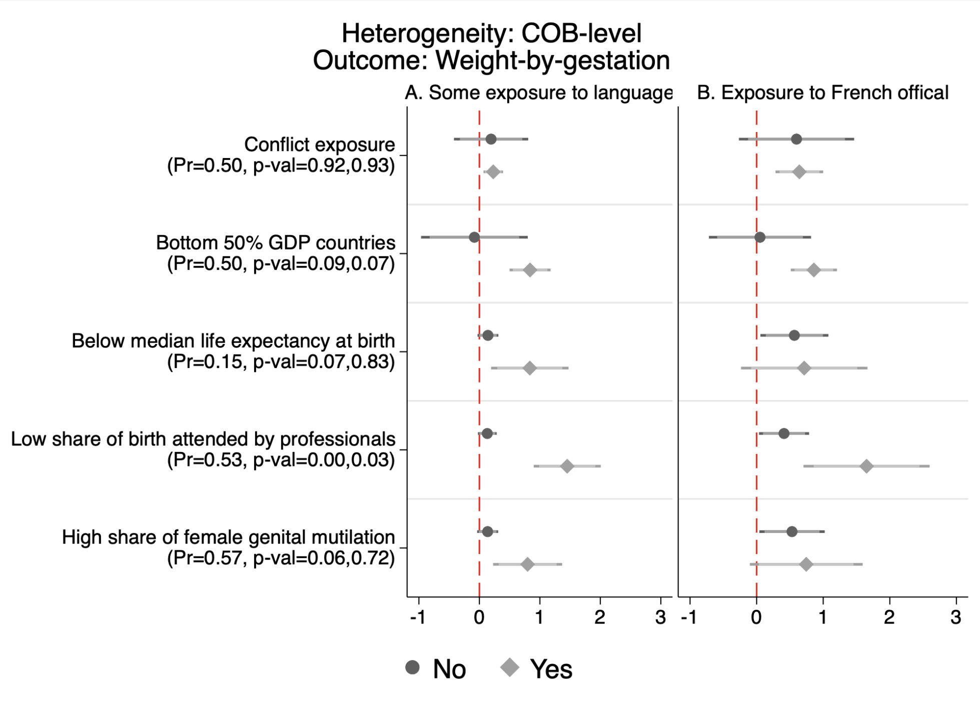


**Figure 8:** Treatment effect heterogeneity by country of origin

*Note*: Figure presents decomposition of treatment effects for various sub-groupings. The upper dark line corresponds to the coefficient of $(1-d)$ and the lower lighter one to $d$, which is when the statement is true. Confidence intervals are based on robust standard errors clustered on the region (treatment) level. $Pr(d)$ corresponds to the share of the sample for whom $d = 1$, and p-val for the p value of an F-test for equality of the two estimates. The different panels correspond to different levels of groupings: A- canton, B - Country, C - individual.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, BFS 2018, VDEM 2018 own calculations.

The results in Figure 8 appear to confirm the hypothesis that the benefit from match is largest for those likely in the most need. Refugees fleeing from war-torn and economically weak countries, benefit more from a language match compared to otherwise similar refugees who originated from a relatively better-performing country, the former being statistically insignificant however. This pattern is strongest and most statistically robust when we compare mothers along their origin's development level of the health sector (in particular, share of birth attended by professionals and prevalence of female genital mutilation). Similarly, life expectancy and GDP as more general proxies for (health-sector) development indicate that mothers benefit stronger from the language match if they were exposed to worse conditions in their origin countries.

### 5.2 Networks

Due to the limited behavioral measures in our data, we cannot definitively show that communication barriers are the driving force behind worse infant health of refugees allocated to an unfamiliar language environment. However, in addition to convincing causal patterns above, we provide further evidence on the informational benefit of language skills by proposing different network measures as potential substitutes for the individual-region language match.

Pre-existing networks of co-nationals or other peers may, on the one hand, reduce incentives of newly arrived immigrants to adopt the host country language (e.g., Lazear 1999; Bazzi et al. 2019).[50] At the same time, often studies find 'positive' (from the point of view of the individual) effects of local networks, including economic performance (Beaman 2012; Nicoletti et al. 2018), higher usage of maternity and prenatal care (Aizer and Currie 2004), access to public services (Bertrand et al. 2000), medicaid participation (Grossman and Khalil 2020) and health behavior in general (Grossman 1972).

In our setting, we therefore assess whether exposure to different local (cantonal) networks —from refugees in general to co-national refugee women who previously gave birth —may improve knowledge about pregnancy and health services and could act as an anchor point to alleviate stress and other maternal conditions related to infant health. In other words, refugee networks may compensate for the negative consequences of being placed into an unfamiliar language environment, hence, we would expect the language-match coefficient to *decrease* with cantonal network exposure. Accordingly, we apply two main measures, first prior refugee allocation from the same origin in the same destination. Second, we also calculate a measure of prior motherhood in the origin-destination cell, which are most likely to be the best informed and, hence, constitute a 'higher-quality' network.

Thus, we augment equation (3) with the interaction term of language match and our network measure

$$\begin{aligned} y_{iort} &= \alpha + \tau \text{Language Match}_{i_{or}} \times \text{network}_{iort} \\ &\quad + \gamma \text{network}_{iort} + x'_{iort}\beta + \delta_{i_o} + \delta_{i_r} + \delta_{i_t} + \varepsilon_{iort}, \end{aligned} \tag{4}$$

[50] In fact, part of the justification of assigning refugees randomly across cantons, is to prevent the emergence of (ethnic) enclaves according to the SEM (Leuba 2017).

we use a common definition in the literature on immigrant networks of the following form:[51]

$$\text{network}_{iort} = \ln\left(\sum_{i \neq j, t<k} j_{ort}\right) \tag{5}$$

where the network size is defined as the log of the leave-one-out cumulative sums of the number of refugees from the same origin country $o$,[52] in the same allocated region $r$ (canton), before the allocation $t$ took place.

We use five different measures where *network quality* arguably increases. First —Column 2 in Table 3 —we take the sum of all refugees that arrived before refugee $i$ from the same origin-destination-cell, $o \times r$. In Column 3, we use the birth records of both, refugees and immigrants, to construct the network of co-national mothers $o \times c$ who gave birth before the refugee mother $i$ (whole sample period), while we restrict this network to previous refugee mothers from Column 5. Eventually, we restrict these two network measures to only recent refugee mothers (from our main sample) who gave birth in the year prior (–365 days) to the respective refugee's $i$ birth, as shown in Columns 4 and 6 of Table 3. Because of the inclusion of fixed effects on the $c$, $o$, and $t$ —arrival time level, the main difference of our and the Bertrand et al. (2000) approach is that instead of taking all refugees in a given year, we only use those arriving before the refugee mother $i$, thus, creating an exogenous measure in the sense that it only uses pre-allocation information.[53]

Figure A.7.1 in Appendix A, plots the cumulative leave-one-out sums of each of these network measures. Naturally, this measure is highly-skewed and larger for large sending countries and receiving cantons. We therefore use the log-form to assess relative network size. In Panel b of Table 3, we additionally present a second measure of contact availability that does not constrain the form, as in log- or square root-transformations. That is, we categorize local network size into four quartiles to account for possible non-linear effects.

[51] Bertrand et al. (2000) use a measure of *contact availability* $ln((N_{i_{or}}/N_{i_r})/(N_{i_o}/N_i))$ which is very similar to our measure given the choice of our fixed effects and the log-structure, one main difference is that we have exact placement and can thus exploit the variation over (exact) arrival times as well. This however increases the chance of a 0 count which renders the log-transformation problematic (which Bertrand et al. (2000) circumvent by looking at large enclaves only), we use $\ln(x+1)$ but tested various specifications of it, that is, just using the raw number, $\ln(x+0.0001)$, sine transformation; all of which give qualitatively similar results.

[52] Here the leave one out refers to the whole family that we assessed above rater than the refugee herself only.

[53] We exclude mothers arriving in 2010, reducing the sample size slightly to calculate meaningful measures of the networks (because we do not observe births prior to 2010), therefore we present the main result as baseline in Column (1) with this sample. Including these does not alter the results, likely because the regressions include the time of arrival and hence only compares within cohort where all would suffer proportionally from the attenuated network size measure.

**Table 3:** Networks

| | Baseline | Prior allocated refugees | Prior mothers all | Prior mothers year prior | Prior refugee mothers all | Prior refugee mothers year prior |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel a.* Outcome: Weight/Gestation, network measure: $\log(size+1)$ | | | | | | |
| match | 0.227 | 0.540 | 0.338 | 0.512 | 0.437 | 0.446 |
| | (0.070) | (0.248) | (0.289) | (0.209) | (0.200) | (0.123) |
| network | | -0.066 | 0.067 | 0.047 | 0.050 | 0.021 |
| | | (0.050) | (0.032) | (0.037) | (0.031) | (0.042) |
| match× network | | -0.062 | -0.037 | -0.125 | -0.087 | -0.125 |
| | | (0.046) | (0.070) | (0.072) | (0.069) | (0.055) |
| *Panel b.* Outcome: Weight/Gestation, network measure: residual-network quartiles | | | | | | |
| match | 0.227 | 0.542 | 0.254 | 0.341 | 0.376 | 0.380 |
| | (0.070) | (0.096) | (0.115) | (0.109) | (0.111) | (0.105) |
| $1[network > q_{25}]$ | | 0.061 | 0.189 | 0.135 | 0.131 | 0.070 |
| | | (0.070) | (0.071) | (0.049) | (0.062) | (0.086) |
| $1[network > q_{50}]$ | | 0.117 | 0.209 | 0.114 | 0.235 | 0.092 |
| | | (0.102) | (0.095) | (0.071) | (0.060) | (0.095) |
| $1[network > q_{75}]$ | | 0.132 | 0.156 | 0.084 | 0.218 | 0.041 |
| | | (0.136) | (0.112) | (0.090) | (0.081) | (0.118) |
| match $\times 1[network > q_{25}]$ | | -0.438 | 0.006 | -0.229 | -0.168 | -0.192 |
| | | (0.155) | (0.228) | (0.205) | (0.140) | (0.144) |
| match $\times 1[network > q_{50}]$ | | -0.432 | -0.168 | -0.152 | -0.206 | -0.148 |
| | | (0.162) | (0.191) | (0.151) | (0.163) | (0.182) |
| match $\times 1[network > q_{75}]$ | | -0.480 | 0.376 | 0.181 | -0.617 | -0.588 |
| | | (0.140) | (0.346) | (0.930) | (0.113) | (0.139) |
| *Panel c.* Outcome: network measure $\log(size+1)$ | | | | | | |
| match | | 0.139 | 0.384 | 0.321 | 0.137 | 0.099 |
| | | (0.067) | (0.096) | (0.082) | (0.086) | (0.101) |
| *N* | 7,590 | 7,590 | 7,590 | 7,590 | 7,590 | 7,590 |
| Canton FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Country FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Placement Year & Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Request FE | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Birth characteristics FE | | | ✓ | ✓ | ✓ | ✓ |

*Notes*: Table presents coefficient estimates of the model (3) regression, and cluster (Canton) robust standard errors, analogous to Column 3 in Table 1 including interactions and and measures of various network measures, Column (2) all co-ethnic (same origin - same allocation) refugees arriving before refugee, Column (3) all co-ethnic immigrant mothers, (4) - that gave birth in the exact year prior, (5) - only refugee mothers from the allocation file (that were randomized), (6) - that gave birth just in the year before own birth. Sample size smaller since we drop the first year to calculate meaningful networks. Panel a. network calculated using $log(x+1)$, Panel b. using quantiles of the network size, and Panel c. shows that the network is not correlated with other refugees but earlier immigrant (mothers) as expected.
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

Beforehand, in Panel c of Table 3, we address whether there is any selection in terms of network size, analogous to the results in Figure 5 using $\log(network+1)$ as an outcome. Reassuringly, we do not find significantly larger networks for any of the refugee-based network measures, i.e., Columns 2, 5 and 6. Yet, there is a larger share of co-national mothers as hypothesized above. This is because language matching mothers get allocated to regions where other immigrant women selected themselves into. Thus, despite being random, the areas differ not only by language but also by the pre-existing potential networks, most likely due to language.

In Panel a, the regression results on the weight-by-gestation outcome reveal several interesting patterns: First, independent of their *quality*, all network types have a *negative* effect on the beneficial effect of the language match for children's weight-by-gestation ratio (interaction in Panel a). This means that network exposure likely compensates for otherwise more problematic communication barriers, thus is a substitute rather than a complement in the child health production function. Second, the compensatory power is larger for the mother networks (Columns 3-6) than for the general ones that include men and women without children (Col. 2). However, third, networks can never fully replace the advantages of a familiar language environment, as indicated by the effect size and statistical significance of the *match* coefficient. Interestingly, the non-current refugee co-nationals do not significantly impact the child health at birth, yet once looking at refugee women allocated just before that had a child in the year prior the substitution effect is large and significant.

Eventually, the disaggregation in Panel b suggests that a higher number of refugee mothers in the area, i.e. a larger network, increases the compensatory network effect (Column 5 and 6), while the general refugee network effect (including men and non-mothers, Column 2) is relatively insensitive to variations in size. This is plausible insofar as refugees are always embedded in some sort of general network —starting from the moment of arrival in a reception center together with many other refugees. However, the likelihood to encounter refugee mothers and establish an information exchange that benefits health outcomes is arguably more dependent on (possible) network size.

Overall, local networks seem to substitute rather than complement effects of a familiar language environment on neonatal health. Lazear (1999)'s model of language acquisition predicts language assimilation to be fastest when a proportionally small minority encounters a coherent native culture because when only few peers speak the same language the immigrant's incentive is greatest to adopt the majority language. However, not all networks are equally beneficial as *network quality* is likely important. While a large number of refugees residing in a mother's location makes little difference in our estimations, neonatal health improves when exposed to more mothers (native and immigrant) and especially more refugee mothers, and mothers in the network recently gave birth themselves in Switzerland. It appears, that recent (refugee) mothers partly resemble and substitute for a familiar language environment and function as channel to exchange health-relevant information. Albright possible, it is unlikely that the co-ethnic peers can substitute for sophisticated medial instructions (from doctor-patient communication), thus we interpret this as suggestive evidence that a sizable part of the match effect being driven by maternal health behavior (i.e. what to eat), signing up for check-ups or the reduced stress from supporting peers, dimensions arguably more influenceable by peers.[54]

### 5.3 Residence duration in CHE

Finally, we address an encompassing literature on immigrant assimilation, stressing that —over time —immigrants converge to the host country population in various domains, including language skills (e.g., Waters and Jimenez 2005) and health (e.g., Giuntella and Stella 2017). Vice versa, when women arrive already pregnant, they may not be able to benefit from better communication ability due to closing time windows for (medical) interventions. In Figure 5 above, we have established that mothers allocated to a familiar language environment neither show increased fertility, nor do they differ in terms of birth timing after arrival from otherwise similar non-match refugee mothers. Here, we interact the period between arrival and delivery with the language match indicator for French speaking origins and exposure to French as an official language in the origin country ($1(t-p=k) \times match$). Note that across all refugee groups the length in the country has a monotonic and positive effect on the birth outcomes, as expected. Thus, the results in the Figure 9 show the *additional benefit* to this 'assimilation' of refugees with potential previous language exposure, in other words at what time is the language-match effect most pronounced.

[54]This is in line with the findings by Aizer and Currie (2004), Deri (2005), Devillanova (2008) and McMillan (2019) whereupon immigrant networks increase health-care utilization and improve health outcomes.

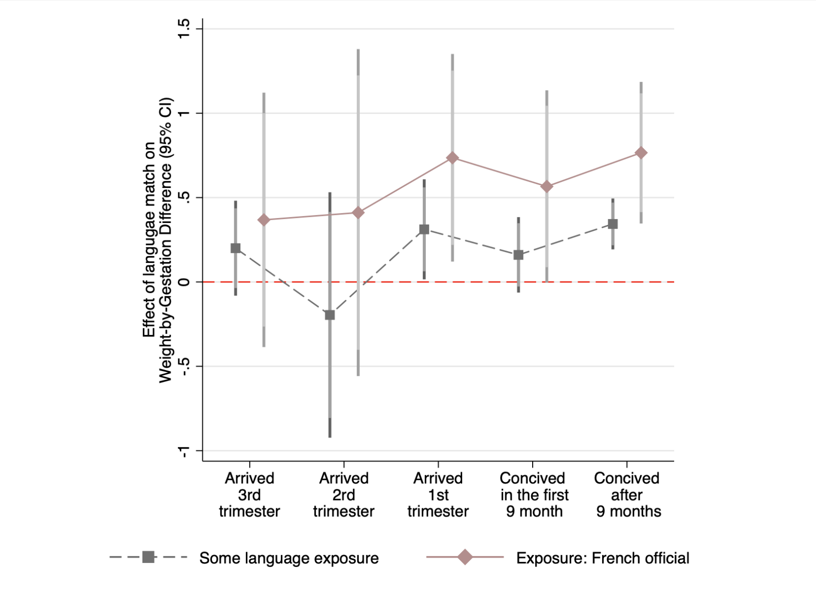


**Figure 9:** Match effects over time and across birth outcomes

*Note*: Figure displays coefficient estimates from main generalized DiD model interacted with time-in-Switzerland for our main outcome weight-by-gestation, and 95% (dark) and 90% (light) confidence intervals are shown. The results on log birth-weight, gestation and LWB incidences are presented in Figure C.1.1.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

Strikingly, for mothers who arrived in the later stages of her pregnancy (third trimester) i.e. when there was little time left for potential interventions, the additional benefit of a familiar language environment is largely absent. It only turns statistically significant for mothers who arrived first trimester pregnant or conceived after arrival in Switzerland. This again is consistent with the prenatal care intervention being most beneficial when started in the first-trimester (Cygan-Rehm and Karbownik 2020). Similarly, Almond and Mazumder (2011) and Van Ewijk (2011) report larger negative effects of Ramadan-induced malnutrition when maternal fasting takes place in the first or second trimester of the pregnancy, and Lavy et al. (2016) find that children of Ethiopian women fare significantly better when they migrated to Israel —with substantially better health infrastructure —early in their pregnancies. Figure 9 thereby also supports the hypothesis that inadequate medical treatment because of language barriers functions as the likely mechanism. Especially for conditions related to fetal growth, early detection, communication, and timely intervention is key. Note that these timing effects also strongly indicate that financial resources —through employment —that may be correlated with health-relevant skills and behavior are unlikely affecting our results, given employment rates of refugee women are close to 0% in the first two years after arrival and those of potential male partners only reach about 10% in the second year. The fact that we observe substantial benefits of the language even when to-be-mothers just arrived, and independent of whether they arrive with a family or alone (shown in Figure C.2.1 in the Appendix), makes employment and financial recourse explanations as core reasons for the match effect unlikely.

## 6 Conclusion

Health at birth is a strong predictor of socioeconomic outcomes and well-being later in life, including future health (e.g., Almond and Mazumder 2011) and earnings (e.g., Black et al. 2007). An important role in determining infant health is attributed to maternal characteristics, which is eventually manifested in the *intergenerational transmission of inequality*. In fact, child health at birth has become a primary indicator to study the transmission of inequality across generations (inter alia Rosenzweig and Schultz 1983; Currie and Hyson 1999; Case et al. 2002; Currie 2009; Aizer and Currie 2014; Figlio et al. 2014; Caruos and Miller 2015; Almond et al. 2018)

Adding to this, the results in this study suggest that there is a non-negligible causal effect of parental language skills on child health outcomes. The importance of language proficiency as the essential human capital component for immigrants has been shown in various domains (e.g., Chiswick and Miller 2007; Auer 2018; Houle 2019; Brell et al. 2020). By demonstrating that conditionally random matches of the local language with refugees' individual language heritage *also affects their children's health*, we advance the extant literature, where causal statements are inherently hard to establish. Leveraging this unique natural experiment allows us to recover the causal effect of origin-destination language matches in a generalized difference-in-differences setting, where we compare co-national mothers allocated to a familiar vs. an unfamiliar language environment where refugee mother's from a different language background act as control group. Thereby, canton-, origin country-, year-, and arrival-time fixed effects account for possible genetic differences across groups and any other spatial or temporal variation that might affect health at birth.

Pregnancies in a familiar language environment result in higher birth weight —at the mean of approximately 3,200 grams —by about 72 grams, or 2.2%; and a reduction of low birth weight incidences by approximately 2.9 percentage points. This effect is sizable compared to policy interventions, such as prenatal care (0.3 to 0.8% in birth weight; Cygan-Rehm and Karbownik 2020) or nutritional programs for low-income mothers (approximately 0.8% in birth weight; Rossin-Slater 2013). It is comparable to major reforms, such as introducing a $1,000 increase in earned income tax credit in the US (-0.35 to -1.36 pp in LBW; Hoynes et al. 2015) or building schools in Taiwan (0.24 pp less LBW incidences; Chou et al. 2010).

We do not observe significant changes in gestation as the result from a language match. This indicates that the benefits of the language match materialize in lower risk of children born that are small for gestational age. This intrauterine growth restriction can often be treated if detected early and is —among others —caused by maternal stress and lack of medical support; factors that are arguably present among refugee women fleeing from war and persecution. Second, mothers who arrive in Switzerland in or after the second-trimester do not significantly benefit from a familiar language environment, thus, corroborating (non-detected) intrauterine growth restriction as the prime consequence. The effects become sizable for conceptions just prior (first trimester) or after arrival in Switzerland and stay large for those conceiving in Switzerland.

Overall, our results point towards a general communication benefit that is driven by both the refugee mother's local environment and with doctors and health personnel. Both channels are supported by recent exploratory studies. Ikhilor et al. (2017) report that immigrant mothers in Switzerland with limited language skills are less likely to take-up services, for instance, because they are not aware of them or unaware of insurance coverage. Moreover, communication barriers in obstetric care for migrants in Switzerland led to situations where young mothers were often unable to comprehend the interventions by medical staff. There is no comprehensive data in Switzerland that would allow us to test directly whether doctor appointments, medication, etc. vary with exposure to a familiar language environment. However, we show that the benefits of being pregnant in a familiar language environment tend to be larger among mothers with (likely) riskier pregnancies, that is, in case the mother is older than 35 years, it is her first baby, or when the child is a boy. Moreover, we observe larger effects for refugee mothers originating from countries characterized by a week or nonexistent health sector (below average GDP, few births attended by medical staff) or where female genital mutilation is common. Arguably, without communication barriers these mothers can benefit substantially from a sophisticated health system with universal care, as opposed to their co-national peers in an unfamiliar language environment that may hamper knowledge about and access to services.

In addition, our data allows us to create local network measures that also capture network quality using exact arrival time information. That is, we can show that the language-match effect decreases (but remains significantly positive) when more refugees are present in the vicinity of the to-become mother (see also Lazear 1999). This compensatory mechanism —when pregnant women have access to peers who hold health-relevant information and/or know gatekeepers and can gather information —is more pronounced when the local network is comprised of refugee mothers, and even more when these refugee mothers had their deliveries in the year prior. This again points towards general communication barriers that impede access to information and subsequently proper utilization of health services (e.g., knowledge about regular free ultra-sound checks which may not be available in most origin countries), as the driving factor, which can —in part —be substituted by high-quality networks.

Eventually, timing is essential for being able to benefit from a familiar language environment. Leveraging a public transport strike, Evans and Lien (2005) find indication that missing a prenatal visit in earlier stages of the pregnancy is more detrimental than later on. The benefits of providing adequate health care as early into the pregnancy as possible has been corroborated by Cygan-Rehm and Karbownik (2020) who evaluate a policy in Poland designed to speed up the initial prenatal care visit. In our case, the timing of arrival determines the window of opportunity for providing health services. Refugee mothers who arrived in Switzerland already pregnant in the second or third trimester do not benefit from being allocated to a similar language environment in terms of their children's neonatal health, is consistent with this interpretation. For those who arrived pregnant in the first trimester, however, and those who were already living in Switzerland at the time of conception, we observe stable and significantly positive effects. This finding is important for two reasons: First, it demonstrates that a certain amount of time is required to detect and counteract potential issues during pregnancy. Second, the fact that effects are already significantly positive for refugee mothers who arrived pregnant in the first trimester and that these are similar to mothers with longer residence duration in Switzerland rules out that (economic) assimilation drives our results. Economic integration, in our refugee population, typically begins place after being in the country for about two years and in any case not before asylum has been granted (which takes about one year in Switzerland, on average, c.f. Auer 2018). Thus, the six-month-period that is required for our effects to stabilize and become significantly positive is unlikely to be driven by economic and rather by communication-related channels.

Our results also have immediate implications for policy design. For refugee-receiving countries with different language regions (i.e., Belgium, Switzerland, Canada, perhaps even the European Union) an allocation process that accounts for the refugees' language may be more appropriate, as this does not only increase employment (Auer 2018; Bansak et al. 2018; Delacrétaz et al. 2016), but —given the intergenerational effects reported found in this study —also the well-being and integration of their future children. Further, our results corroborate that language is a key determinant of integration disadvantages, which is potentially important for targeted policy interventions, such as language learning courses, mother groups, or translator services in public institutions and hospitals (c.f., Bischoff et al. 2009; Bollini et al. 2009; Corman et al. 2018; Sandner et al. 2018; Doyle 2019; Arendt et al. 2020; Cygan-Rehm and Karbownik 2020). Factoring in the (future) well-being of the children and the critical period of early childhood, the benefits of such relatively mild interventions are likely higher than previously thought. Targeted interventions, in combination with a refugee allocation mechanism that takes the well-being of the second-generation into account, may mitigate (additional) disadvantages refugees from culturally and linguistically diverse backgrounds face. Thus, such efforts could reduce the intergenerational transmission of inequality and thereby benefit host societies at large. In sum, we argue that the disproportionate health risks faced by children of refugees should be recognized when policymakers weigh the costs and benefits of allocating refugees independent of their language or network endowments.

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## A Additional data information

Table A.1.1 presents the main and auxiliary datasets we use.

### A.1 Data sources

**Table A.1.1:** Dataset overview and sources

| File | Description | Source | Accessed | Level | |
|---|---|---|---|---|---|
| **Foreigner registry** | | | | | |
| ZEMIS | Asylum seeker registry & canton allocation (2010-2017) | SEM | 2018/08/14 | Individual-time | |
| **Birth registry** | | | | | |
| BEVNAT I | Full birth registry (2010-2017) | FSO | 2018/11/19 | Individual-time | |
| BEVNAT II | Stillbirth registry (2010-2017) | FSO | 2018/11/19 | Individual-time | |
| BEVNAT III | Death registry children < 1 year (2010-2017) | FSO | 2018/11/19 | Individual-time | |
| **Country of origin data** | | | | | |
| Language | Classified German, Spanish, Portuguese, English, French, Italian as an (official) language; whether was colony; main language spoken | CIA Facebook | 2019/06/05 | Country | link |
| Alternative language | Indicator for common (official) language with Switzerland | CEPII | 06/03/2020 | Dyadic ctry-year | link |
| Health | Indices for healthy life expectancy at birth; maternal mortality per 10,000; proportion births attended by professional; | UN data | 2018/10/29 | Country | link |
| Genital mutilation | Share of 0-14 and 15-50 years old females with genital mutilation | UNICEF data | 2020/11/09 | Country | link |
| Education | Education shares (total and female) 2010 | Barro & Lee | 2020/10/30 | Country | link |
| GDP | GDP and GDP PPP (International), GDP p.c. in 2010 USD | Worldbank | 2020/11/03 | Ctry-arrival-year | link |
| Conflict | Conflict (> 25 battle-related deaths per year) and war (> 1000 deaths); three year lag before arrival | UCDP-PRIO v20 | 2020/11/03 | Ctry-arrival-year | link |
| Elevation | Average elevation off the country (for placebo) | | 2019/03/09 | Country | |
| Female rights | Laws to protect women and protection against domestic abuse (e.g., rape in marriages, women required by law to obey their husbands; 2018 values) | UN Women data | 2020/11/06 | Country | link |
| **Canton data** | | | | | |
| Language | Classifies cantons by language (German, French, Italian and multiple) and by main language (German, French, Italian) | FSO | 2017/10/30 | Canton | link |
| Health infrastructure | Counts of the number of hospitals, number of neonatology clinics, Number hospitals with more than 10> births | BAG | 2020/10/28 | Canton-birth-year | link |
| Vote right-wing | Average SPP (right wing) vote share in 2007 general election | FSO | 2020/11/05 | Canton | link |
| Elevation | Average elevation of canton (placebo) | FSO | 2019/03/19 | Canton | |

## A.2 Languages of the resident population

Figure A.2.1 provides evidence on the remarkably sharp language borders in Switzerland using micro census data on approximately 2.3 million individuals between 2010 and 2017. We define a binary indicator on spoken language (German, French, Italian) that takes on the value 1 if the language is either the respondents main language or regularly spoken by the respondent at home or at work. Note that most Swiss learn a second Swiss language at school, which assumably enable them lead basic conversations Accordingly, the share of residents in a monolingual canton who are proficient another Swiss language (e.g., French-speaker in the German-speaking region) is always below 10 percent. The shares for multilingual cantons is driven by the fact that the largest bilingual canton (Bern) has a German-speaking population of about 90 percent. These patterns hold for both, the resident population and the health personnel (incl. doctors) in particular. In combination with the lack of systematic interpreter services in the Swiss health sector (e.g., Ikhilor et al. 2017), it is therefore safe to assume that French- or Italian-speaking refugees do not have access to an equivalently proficient professional in another language region, yet alone being able to communicate in French or Italian with the general population.

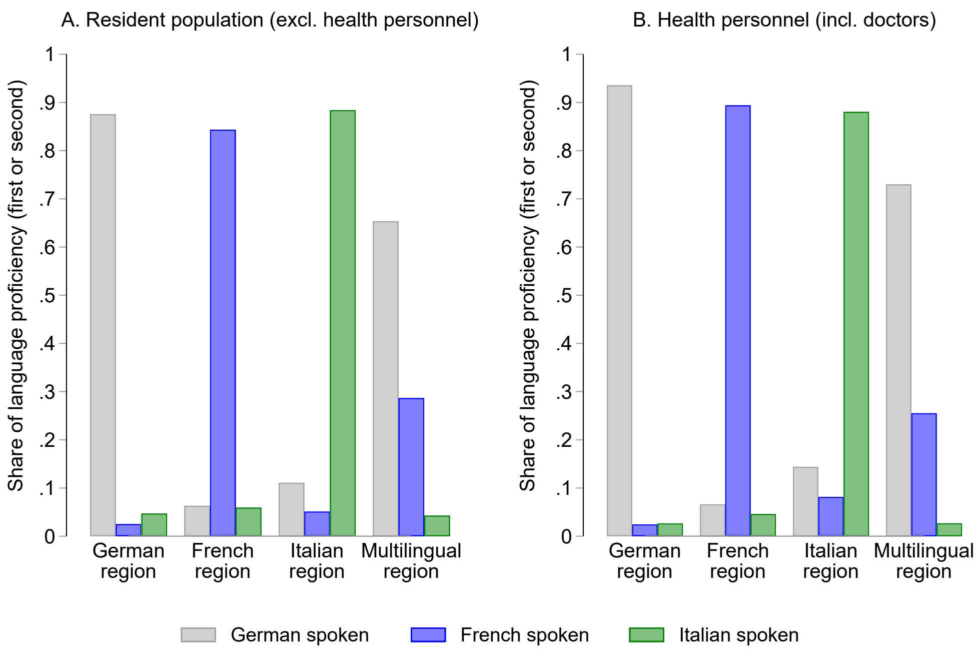


**Figure A.2.1:** LANGUAGE SHARES BY LANGUAGE REGIONS

*Note*: The figure shows the share of residents (panel A) and health sector professionals (panel B) speaking German, French, or Italian as first or a second language across the Swiss language regions.
*Source:* Swiss micro census 2010-2017, own representation.

## A.3 Allocation request

Requests and allocation-relevant notes are made by the caseworker in free-text and submitted to the central allocation office of the SEM. Importantly, the refugees are not known to the allocation officers, and we observe all information that are provided to the allocation officer in the requests. We also tested whether some allocation officers are more likely to send refugees to matching language regions, which is not the case in our data as shown in Figure 4 (for more details on allocation data and refugee allocation, see also Auer 2018; Couttenier et al. 2019; Marten et al. 2019).

For confidentiality and (personal) security reasons the SEM does not allow the request file to be accessed along with individual background characteristics or outcomes of the refugees. Alternatively, the SEM provided us with the canton request and free-texts, along with a pseudo identifier. We categorized the free-text into topics in the form of a limited set of indicator variables. The SEM then merged these identifiers back to the main data, which includes exact time of arrival and identifiers to relate the refugee allocation to births and birth outcomes (see Section A.5).

To make the roughly 240 thousand free-texts[55] usable (all refugees, of which 28.16% provided a request), we first extract topics and features via Blei et al. (2003)'s Latent Dirichlet Allocation [LDA]. We run this algorithm over all entries —independent of region and placement requests (which are not part of the estimation in this step) or eventual allocation.

Figure A.3.1 plots the stated categories across cantons which refugees requested. There is a slightly different distribution of the main (algorithmically-selected) 30 topics stated in AI, AR, NW, UR, all of which are less populated cantons. Overall, however, the reasons are relatively balanced across all 26 cantons.

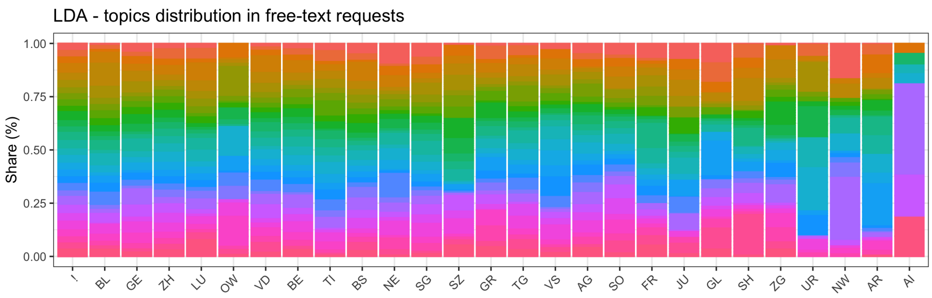


**Figure A.3.1:** LDA TOPIC DISTRIBUTION ACROSS CANTONS

*Note*: Figure plots the share of topics named over the canton requested. All requests are cleaned for auxiliary words, such as "and" or "the" and for words too rare or too common, then the most common features of 30 broad topics are extracted via the LDA algorithm in R. The figure then simply plots the share of these 30 topics across the cantons requested.
*Source:* SEM free-text asylum seekers' requests, 2008-2017.

Based on the most common features found in the LDA analysis, such as "brother", "medical", "police" and many more, we defined indicator variables whether any of these features were stated. We use all indicators (349 features) in a regression tree analysis to validate their predictive power of the features extracted. As outcome, we define an indicator whether a specific canton was requested, e.g., French-speaking canton VS ($y_i = 1$) and separately for

[55] This is slightly larger than our estimation sample as it included all refugee arrivals from 2008 to 2018, for those 2008 - 2010 we don't have outcome data thus these are not used. In the free text file there is no way to tell them apart, thus the elevated sample size.

Italian-speaking.[56] To validate the most important features, we run regression trees and Random Forrest algorithms (Breiman 2001; Breiman et al. 1984) and assess the predictive power of the reasons to request a roman (French and/or Italian) canton. In this context decision trees are beneficial as they allow for higher interactions and various combination of features. This is done to assure that we are capturing indeed the main reasons for requests.

Based on the importance, we then classify topics – whether the refugee stated any related concept thereof. For example, (close) family reunification is a valid reason to be placed, thus we capture the topics *core family* brother, sister, mother, father and all variations thereof that appeared in the LDA. Conversely, *non-core family reunification* is not a valid reason, we classify as well a non-core family indicator, if anywhere in the free-text a non-core family was stated: such as uncle, cousin and again all variations there of that appears in the LDA analysis.[57] Note, a particular request is allowed to have both, core-family as well as non-core family reasons. In Figure A.3.2, we depict the predictive power of these concepts, in particular, we show the mean decrease in accuracy of the selected topics when leaving out the respective topic. Again, this only shows the distribution of requested cantons, in the data we were provided with we can not assess the actual allocation in the de-identified data, as shown in the main text conditional on core reasons all other extracted reasons are uncorrelated with the match indicator.

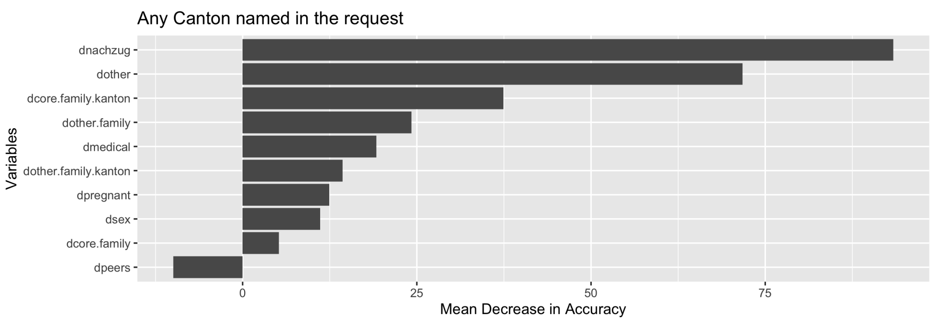


**Figure A.3.2:** LDA TOPIC DISTRIBUTION ACROSS CANTONS

*Note*: Figure plots mean decrease in accuracy of dropping the indicated variable fitted with 1,000 regression trees, trained on a sample of 50,000 refugees and tested on the remaining. *Source:* SEM free-text asylum seekers' requests, 2008-2017.

To summarize, individual privacy and anonymity is protected by reducing the dimensionality of potentially allocation-relevant free-text entries by classifying those into topics as suggested by the LDA above. In the main analysis we will use valid reasons such as medical, core family, as controls, and non-core family and peers as checks for potential remaining sample selection.

[56] Note, that we do not use the actual allocation decision in this part of data extraction, which was not provided to us. If requests did not state a canton wish in the dedicated column but described in the free-text we extract those as well and included them in the indicator.

[57] We create the following indicator variables: any canton named in the placement request (and which), core family, core family and canton wish, other family and other family and canton requested, peers (i.e. friends), medical reasons mentioned, pregnancy mentioned, family reunion, pregnancy mentioned, other health issues, imprisonment and sex specific prosecution. Again we limited us to this set as the SEM allowed only a spars set of covariates to be extracted from this high risk data set.

## A.4 Refugee data

We briefly present descriptive statistics of our refugee sample. First, we describe the patterns of arrivals over time, second, their allocation to cantons compared to expected shares. Overall, the Swiss administrative data is of high quality. Among 234,783 entries there are only 256 identical copies of refugees, 31 have missing background information (e.g., arrival date) and 37 lack allocation information. We drop these but retain all other refugees to assess potential selection concerns. Arrivals are relatively stable across years with a spike in 2015 due to the arrivals of Syrian refugees that was experienced in many European countries. The share of female arrivals has been relatively constant and close to 30% (Figure A.4.1).

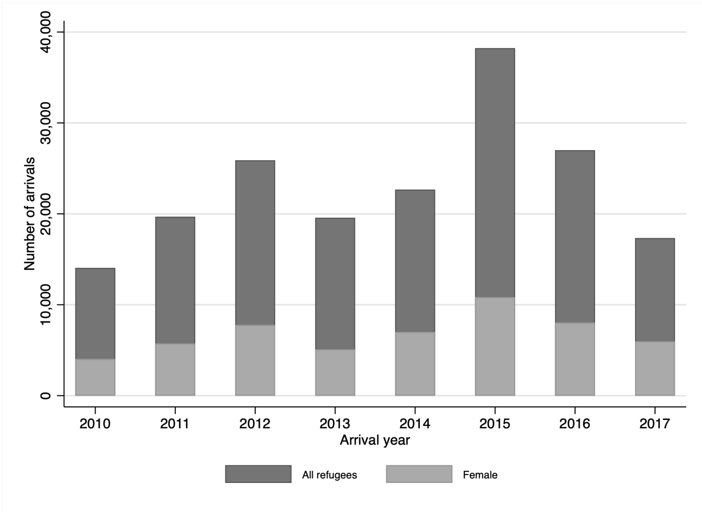


**Figure A.4.1:** Number of total and female refugee arrivals across years

*Note*: Figure plots total and female arrivals in the years 2010-2017, based on first observed system entry.
*Source:* ZEMIS 2010-2017.

Next, we compare the allocation of refugees across countries with the expected share based on the population-proportional quotas set by the Swiss government. Table A.4.1 presents descriptive statistics of the allocated refugee population by canton. Columns 1 and 2 represent our language coding of the states (Column 1 being our robustness definition). The next columns present the allocation quotas per canton set in 1999 and their only revision in 2018 (after our sample period). Over the sample period, the average share of allocated refugees matches the official quotas fairly well. This is not only true at the mean but also across years, where minor deviations are likely due to small sample sizes. In Columns 14 to 16 we show the share of refugees coming from a French- or Italian-speaking background (the coding is detailed in Table A.6.1). The country of origin can either have French as an official language, or a spoken language. There are no refugees from officially Italian-speaking counties in our sample, but both, Somalia and Libya have some Italian-speaking populations. As noted above, the share of refugees from officially French-speaking origins is slightly elevated, but the same is not true for origins where French or Italian is a spoken language. The main driver are family reunification requests that are part of the allocation regulation. In the main text, we show that conditional on those, even the refugees from officially French-speaking origins are uncorrelated with important observables and well documented risk-factors for infant health. The share of requests for allocation to a specific canton is even tilted to the German speaking cantons that are on average more prosperous.

# Table A.4.1: Descriptive statistics of all and female refugees by allocation state

| State | Canton language | | Allocation quotas and realizations | | | | | | | | | | | Origin language | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Official | Main | Law '99 | Law '18 | Mean | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | French official | French spoken | Italian spoken | Age at arrival | Share request | Nobs |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) |
| Panel A: Females | | | | | | | | | | | | | | | | | | | |
| AG | Ger | Ger | 7.7 | 7.9 | 7.4 | 7.9 | 7.9 | 8.0 | 8.2 | 7.6 | 6.6 | 6.7 | 6.9 | 2.8 | 19.5 | 0.0 | 21.0 | 34.1 | 4,878 |
| SG | Ger | Ger | 6.0 | 5.9 | 6.0 | 6.4 | 5.9 | 5.7 | 6.3 | 5.8 | 6.6 | 5.2 | 5.9 | 2.4 | 17.6 | 0.1 | 20.5 | 40.4 | 3,941 |
| LU | Ger | Ger | 4.9 | 4.8 | 5.1 | 4.5 | 4.9 | 6.0 | 4.3 | 5.4 | 5.1 | 5.4 | 4.4 | 2.5 | 19.4 | 0.0 | 20.2 | 38.1 | 3,354 |
| BL | Ger | Ger | 3.7 | 3.4 | 4.1 | 4.0 | 3.7 | 4.4 | 5.2 | 4.2 | 3.4 | 4.2 | 3.8 | 1.3 | 13.1 | 0.0 | 21.2 | 62.6 | 2,671 |
| SO | Ger | Ger | 3.5 | 3.2 | 3.7 | 3.6 | 3.3 | 4.1 | 3.5 | 3.8 | 3.7 | 3.8 | 3.8 | 3.9 | 19.7 | 0.1 | 20.4 | 34.3 | 2,453 |
| BS | Ger | Ger | 2.3 | 2.3 | 2.7 | 3.1 | 2.9 | 1.9 | 2.6 | 2.7 | 2.2 | 3.1 | 4.3 | 2.3 | 17.9 | 0.0 | 21.9 | 72.0 | 1,804 |
| TG | Ger | Ger | 2.8 | 3.2 | 2.4 | 2.3 | 2.5 | 2.4 | 2.6 | 2.9 | 2.0 | 2.4 | 2.4 | 1.5 | 13.8 | 0.0 | 22.2 | 57.1 | 1,578 |
| ZH | Ger | Ger | 17.0 | 17.7 | 16.2 | 18.1 | 16.1 | 17.3 | 16.1 | 15.4 | 16.2 | 15.7 | 14.6 | 2.8 | 20.1 | 0.1 | 20.6 | 45.1 | 10,634 |
| SZ | Ger | Ger | 1.8 | 1.9 | 1.9 | 2.0 | 1.9 | 1.6 | 1.8 | 1.4 | 2.2 | 1.9 | 1.9 | 0.5 | 16.3 | 0.0 | 21.3 | 37.3 | 1,222 |
| ZG | Ger | Ger | 1.4 | 1.5 | 1.2 | 1.4 | 1.4 | 1.3 | 1.3 | 1.2 | 1.3 | 0.9 | 1.2 | 1.2 | 13.2 | 0.1 | 20.8 | 47.3 | 820 |
| SH | Ger | Ger | 1.1 | 1.0 | 1.1 | 1.2 | 1.0 | 1.0 | 1.6 | 1.2 | 1.2 | 1.0 | 1.0 | 0.9 | 17.4 | 0.0 | 21.9 | 49.1 | 746 |
| AR | Ger | Ger | 0.8 | 0.7 | 0.7 | 0.7 | 0.8 | 0.4 | 0.6 | 0.7 | 0.9 | 0.4 | 0.9 | 1.4 | 17.9 | 0.0 | 20.8 | 45.7 | 442 |
| GL | Ger | Ger | 0.6 | 0.5 | 0.5 | 0.7 | 0.4 | 0.5 | 0.4 | 0.5 | 0.7 | 0.5 | 0.6 | 0.3 | 10.0 | 0.0 | 22.0 | 37.2 | 349 |
| UR | Ger | Ger | 0.5 | 0.4 | 0.4 | 0.4 | 0.5 | 0.4 | 0.3 | 0.4 | 0.6 | 0.4 | 0.4 | 0.0 | 14.4 | 0.0 | 21.7 | 48.6 | 292 |
| OW | Ger | Ger | 0.5 | 0.4 | 0.4 | 0.7 | 0.5 | 0.3 | 0.4 | 0.4 | 0.4 | 0.1 | 0.3 | 0.4 | 7.1 | 0.0 | 20.5 | 40.9 | 252 |
| NW | Ger | Ger | 0.5 | 0.5 | 0.4 | 0.3 | 0.6 | 0.3 | 0.6 | 0.3 | 0.4 | 0.3 | 0.4 | 0.0 | 16.0 | 0.0 | 21.2 | 46.5 | 256 |
| AI | Ger | Ger | 0.2 | 0.2 | 0.2 | 0.2 | 0.3 | 0.3 | 0.2 | 0.2 | 0.2 | 0.2 | 0.1 | 0.7 | 17.1 | 0.1 | 22.7 | 39.7 | 146 |
| GR | Bil | Ger | 2.7 | 2.3 | 2.4 | 2.1 | 2.1 | 2.5 | 2.0 | 2.7 | 2.3 | 2.4 | 2.8 | 1.6 | 16.6 | 0.0 | 20.4 | 34.8 | 1,571 |
| BE | Bil | Ger | 13.5 | 12.2 | 14.3 | 12.8 | 14.3 | 14.5 | 15.4 | 15.1 | 14.8 | 14.0 | 12.9 | 2.9 | 19.6 | 0.0 | 20.7 | 34.0 | 9,419 |
| VD | Fre | Fre | 8.8 | 9.3 | 8.6 | 8.3 | 9.1 | 8.3 | 9.1 | 8.4 | 7.9 | 8.8 | 9.3 | 7.6 | 24.2 | 0.0 | 20.6 | 40.7 | 5,625 |
| GE | Fre | Fre | 5.2 | 5.8 | 5.7 | 6.1 | 6.1 | 5.2 | 5.1 | 5.4 | 5.6 | 6.0 | 6.5 | 6.3 | 19.8 | 0.1 | 21.4 | 42.6 | 3,760 |
| NE | Fre | Fre | 2.4 | 2.1 | 2.2 | 2.2 | 2.3 | 2.3 | 1.4 | 2.3 | 2.0 | 2.5 | 2.2 | 5.8 | 20.8 | 0.1 | 20.8 | 41.4 | 1,436 |
| JU | Fre | Fre | 1.0 | 0.9 | 0.9 | 0.7 | 1.0 | 1.1 | 0.9 | 0.9 | 0.9 | 0.8 | 1.0 | 3.6 | 20.2 | 0.0 | 20.8 | 31.7 | 618 |
| VS | Bil | Fre | 3.9 | 4.0 | 5.1 | 4.0 | 4.2 | 3.9 | 4.4 | 4.5 | 6.4 | 6.8 | 6.0 | 5.8 | 19.1 | 0.0 | 21.1 | 19.6 | 3,380 |
| FR | Bil | Fre | 3.3 | 3.7 | 3.2 | 2.9 | 2.9 | 3.0 | 2.4 | 3.5 | 3.4 | 3.3 | 3.7 | 5.4 | 20.9 | 0.0 | 20.7 | 36.7 | 2,087 |
| TI | Ita | Ita | 3.9 | 4.2 | 3.0 | 3.3 | 3.2 | 3.0 | 3.5 | 3.1 | 2.8 | 2.9 | 2.8 | 2.9 | 19.1 | 0.0 | 21.2 | 38.7 | 1,999 |
| Panel B: All refugees | | | | | | | | | | | | | | | | | | | |
| AG | Ger | Ger | 7.7 | 7.9 | 7.8 | 7.9 | 7.5 | 7.9 | 8.2 | 7.6 | 8.0 | 7.6 | 7.6 | 5.8 | 25.7 | 0.0 | 22.1 | 19.3 | 14,382 |
| SG | Ger | Ger | 6.0 | 5.9 | 5.9 | 6.0 | 6.1 | 5.9 | 6.1 | 6.0 | 5.9 | 5.6 | 5.8 | 5.6 | 25.7 | 0.0 | 21.8 | 23.4 | 10,887 |
| LU | Ger | Ger | 4.9 | 4.8 | 5.2 | 4.8 | 5.0 | 4.9 | 4.5 | 5.8 | 5.6 | 5.4 | 5.0 | 5.2 | 25.4 | 0.0 | 21.4 | 23.2 | 9,594 |
| SO | Ger | Ger | 3.5 | 3.2 | 3.8 | 3.4 | 3.3 | 3.7 | 4.1 | 4.0 | 3.9 | 3.9 | 3.8 | 6.1 | 25.6 | 0.0 | 21.8 | 20.0 | 7,014 |
| BL | Ger | Ger | 3.7 | 3.4 | 3.7 | 3.9 | 3.9 | 3.9 | 4.1 | 3.7 | 3.4 | 4.0 | 3.1 | 3.8 | 18.3 | 0.0 | 21.8 | 52.0 | 6,876 |
| TG | Ger | Ger | 2.8 | 3.2 | 2.8 | 3.2 | 3.0 | 2.6 | 3.2 | 2.9 | 2.0 | 2.8 | 3.5 | 4.4 | 21.9 | 0.0 | 23.3 | 51.4 | 5,100 |
| BS | Ger | Ger | 2.3 | 2.3 | 2.7 | 3.0 | 2.6 | 2.0 | 2.3 | 2.2 | 1.8 | 3.6 | 5.1 | 4.1 | 24.0 | 0.0 | 23.1 | 69.4 | 4,940 |
| ZH | Ger | Ger | 17.0 | 17.7 | 16.1 | 17.0 | 16.3 | 17.2 | 14.5 | 15.1 | 16.1 | 16.2 | 17.1 | 5.4 | 27.3 | 0.0 | 21.9 | 35.5 | 29,784 |
| SZ | Ger | Ger | 1.8 | 1.9 | 1.9 | 1.8 | 1.9 | 1.9 | 2.1 | 2.0 | 2.1 | 1.8 | 1.8 | 3.9 | 25.2 | 0.0 | 22.6 | 20.7 | 3,573 |
| ZG | Ger | Ger | 1.4 | 1.5 | 1.3 | 1.3 | 1.5 | 1.5 | 1.5 | 1.4 | 1.2 | 1.0 | 1.0 | 4.0 | 24.8 | 0.1 | 22.3 | 25.2 | 2,370 |
| SH | Ger | Ger | 1.1 | 1.0 | 1.2 | 1.1 | 1.2 | 1.2 | 1.4 | 1.2 | 1.2 | 1.1 | 1.1 | 3.7 | 26.0 | 0.0 | 22.4 | 25.8 | 2,217 |
| AR | Ger | Ger | 0.8 | 0.7 | 0.8 | 0.7 | 0.8 | 0.8 | 0.9 | 0.8 | 0.9 | 0.5 | 0.9 | 1.7 | 23.4 | 0.0 | 22.4 | 22.3 | 1,479 |
| GL | Ger | Ger | 0.6 | 0.5 | 0.6 | 0.5 | 0.6 | 0.6 | 0.6 | 0.6 | 0.6 | 0.6 | 0.6 | 0.4 | 20.0 | 0.0 | 23.4 | 17.8 | 1,093 |
| NW | Ger | Ger | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 | 0.6 | 0.5 | 0.5 | 0.5 | 0.5 | 0.1 | 23.0 | 0.1 | 23.3 | 18.8 | 922 |
| UR | Ger | Ger | 0.5 | 0.4 | 0.5 | 0.4 | 0.5 | 0.5 | 0.4 | 0.5 | 0.5 | 0.5 | 0.4 | 0.7 | 18.9 | 0.1 | 22.8 | 27.2 | 896 |
| OW | Ger | Ger | 0.5 | 0.4 | 0.4 | 0.5 | 0.5 | 0.5 | 0.4 | 0.4 | 0.4 | 0.1 | 0.1 | 0.6 | 18.6 | 0.0 | 22.4 | 27.9 | 649 |
| AI | Ger | Ger | 0.2 | 0.2 | 0.2 | 0.2 | 0.2 | 0.2 | 0.2 | 0.2 | 0.2 | 0.2 | 0.2 | 0.3 | 17.4 | 0.1 | 22.6 | 20.3 | 380 |
| GR | Bil | Ger | 2.7 | 2.3 | 2.8 | 2.7 | 2.6 | 2.8 | 2.7 | 2.9 | 3.0 | 2.8 | 3.0 | 5.3 | 24.7 | 0.0 | 22.4 | 24.0 | 5,221 |
| BE | Bil | Ger | 13.5 | 12.2 | 14.1 | 13.5 | 13.5 | 13.5 | 14.8 | 14.8 | 14.7 | 14.2 | 13.2 | 6.5 | 25.7 | 0.0 | 21.8 | 23.0 | 26,074 |
| VD | Fre | Fre | 8.8 | 9.3 | 8.2 | 8.4 | 8.5 | 8.3 | 8.5 | 8.1 | 8.1 | 8.0 | 8.0 | 10.7 | 27.7 | 0.0 | 22.0 | 26.0 | 15,149 |
| GE | Fre | Fre | 5.2 | 5.8 | 5.6 | 5.9 | 5.8 | 5.4 | 5.6 | 5.2 | 5.7 | 5.9 | 5.2 | 9.8 | 25.8 | 0.0 | 22.4 | 26.5 | 10,289 |
| NE | Fre | Fre | 2.4 | 2.1 | 2.2 | 2.2 | 2.5 | 2.4 | 1.7 | 2.4 | 2.2 | 2.3 | 1.8 | 9.5 | 27.0 | 0.0 | 22.1 | 24.6 | 4,078 |
| JU | Fre | Fre | 1.0 | 0.9 | 1.1 | 1.1 | 1.0 | 1.1 | 1.1 | 1.1 | 1.1 | 1.1 | 1.0 | 10.2 | 28.2 | 0.0 | 23.3 | 16.3 | 1,993 |
| VS | Bil | Fre | 3.9 | 4.0 | 4.0 | 3.6 | 3.9 | 3.7 | 3.8 | 4.0 | 4.3 | 4.1 | 4.0 | 8.6 | 24.0 | 0.0 | 21.2 | 17.0 | 7,321 |
| FR | Bil | Fre | 3.3 | 3.7 | 3.4 | 3.1 | 3.2 | 3.4 | 3.0 | 3.4 | 3.7 | 3.5 | 3.5 | 8.5 | 26.6 | 0.0 | 22.0 | 20.3 | 6,257 |
| TI | Ita | Ita | 3.9 | 4.2 | 3.2 | 3.5 | 3.4 | 3.7 | 3.9 | 3.2 | 2.9 | 2.7 | 2.8 | 6.3 | 30.5 | 0.0 | 22.8 | 32.7 | 5,917 |

*Note:* Canton/states stand for Zurich (ZH), Bern / Berne (BE), Luzern (LU), Uri (UR), Schwyz (SZ), Unterwalden (Obwalden (OW),Nidwalden (NW)), Glarus (GL), Zug (ZG), Freiburg / Fribourg (FR), Solothurn (SO),Basel Stadt (BS), Basel Land (BL)), Schaffhausen (SH), Appenzell Ausserrhoden (AR), Appenzell Innerrhoden (AI), Sankt Gallen (SG), Graubünden (GR), Aargau (AG), Thurgau (TG), Ticino (TI), Vaud (VD), Valais / Wallis (VS), Neuchatel (NE), Genève (GE), Jura (JU).
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

## A.5 Birth and vital statistics data

Based on the individual identifiers (anonymized social security number), we link the mother identifier in the birth registry to the refugee allocation registry. Note that the birth registry covers all birth events in Switzerland, including native and other immigrant mothers, which we use for a number of comparisons and robustness checks. Again the data is of high quality, there are only 28 exact duplicates. From the 10,798 births to refugee women, we exclude from the main analysis, following (Chen et al. 2016), that is 2,926 higher order births, 105 twin births, 61 stillbirths (which we assess separately), 12 too light (< 500grams), 6 too young (< 153days), in Figure 5 we show that neither is correlated with either match indicators. 5 events had missing key variables, such as birth weight or gestation, which we also exclude, and 138 mothers were stateless or had no nationality information, which we include in the control group but drop in a robustness exercise due to the ambiguity of the potential language exposure. First, we describe our sample properties, relative to the Swiss and immigrant populations.

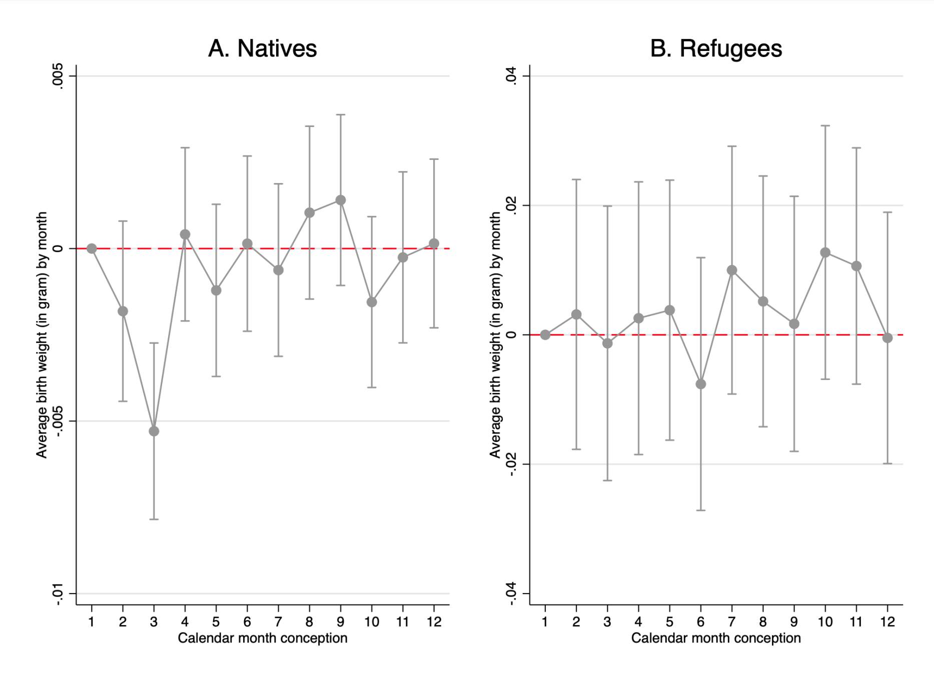


**Figure A.5.1:** Average birth weight, by month of conception, relative to January, natives and refugees

*Note*: Figure plots average birth weight by conception month for natives and refugees in Switzerland.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, own calculations.

In Figure A.5.1, we replicate the descriptive associations presented in Currie and Schwandt (2013) for the native mothers in our sample (Panel A) and among refugees (Panel B). Similar to their finding, deliveries early in the year have lighter babies, which is consistent —as Currie and Schwandt (2013) hypothesize —with a lagged influenza season effect. Interestingly, we do not find the March effect in our refugee sample, where the birth outcomes are rather smooth over the year. In the main text we also show that there is no selection into (March) births and that our results change little if conception month dummies are included in the regressions.

In Figure A.5.2 we show that there also is no structural shift in any point of the arrival distribution. The number of births are decreasing with arrivals in the last sample year, which

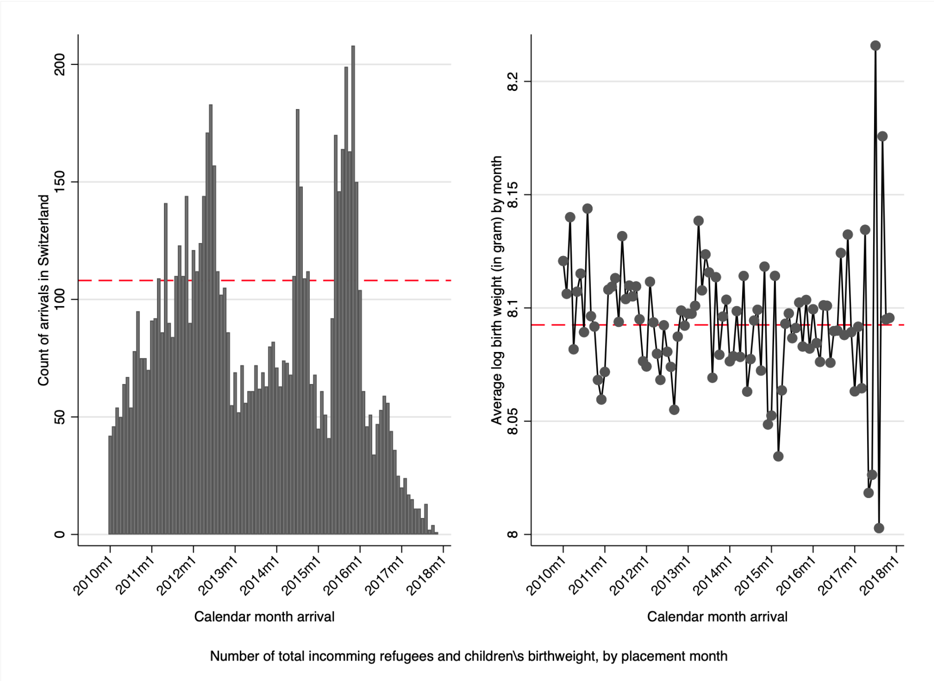


**Figure A.5.2:** Number of refugee mothers arrival and birth weight by month of arrival

*Note*: Figure plots total and female arrivals in the years 2010-2017, based on first observed system entry.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, own calculations.

has little impact on our results as we always account for the time of allocation, thus all mothers are only compared within their arrival cohort. We also excluded refugees arriving in 2017 as a robustness check, with very similar results. In the right Panel, we see that birth weight appears to be cycling in a random fashion around an average of just below 8.1 log grams, again the increased uncertainty in 2017 is visible.

Finally, in Table A.5.1 we show selected descriptives of our sample, by nativity groups (natives, immigrants, refugee (all births), refugee (first birth an minor sample restrictions), refugees that are matched with any language exposure, and french official exposure matched mothers). The Swiss and immigrant mothers in our sample are on average older than our refugee mothers, both all refugee births (multiple included) and our sample are very similar, 27 years of age. They have less overall and previous children, as we mainly assess the first birth in Switzerland, which is likely driven by the relative short time frame we observe the refugee mothers. Refugee mothers are more often married that natives or immigrants, possibly due to the higher marriage rates in the refugees home countries, where often marriages as a source of insurance is more common than in western societies. Twinning on the other hand is very similar, so is the lack of any missing females, that is prevalent in many origin countries.

Looking at the raw birth outcomes in our main sample, in Panel b. it is reassuring that the refugees do not suffer disproportionally as one might have expected. The average log birth weight is relatively similar to the main population, though they are shorter, and exhibit one stillbirth per 1,000 births more than natives and immigrants, and 1.5 deaths in the first year of live. That this is less pronounced in our main sample is due to the exclusion of very low birth weight, as well as gestation, as discussed above. Note that for the refugee sample these are very small numbers so statistical uncertainty is a problem when interpreting these.

In columns (5) and (6) we present the implied samples based on the effective weighting approach by Aronow and Samii (2016), which clearly shows that the regressions using only the refugee mothers from French officially speaking countries are less representative of the overall refugee population. There is a clear internal and external validity trade-off where these refugees are possibly the clearest treatment group, but less representative and more negatively 'selected' the the average refugee well captured using all refugees exposed to either some French or Italian, in their respective match regions (cf. Column 5).

**Table A.5.1:** DESCRIPTIVES OF MOTHER AND BIRTH CHARACTERISTICS

| | Natives | Immigrants/ Former ref. | Refugees | Main sample Refugees | Effective weights sample Match | French off |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel a.* Mother and birth characteristics | | | | | | |
| Mother age at birth | 31.730 | 31.159 | 27.780 | 27.351 | 27.388 | 28.293 |
| | (0.009) | (0.012) | (0.063) | (0.063) | (0.206) | (0.410) |
| N | 450,604 | 299,961 | 10,798 | 7,683 | 7,674 | 7,418 |
| Number of children | 0.527 | 0.478 | 0.435 | 0.232 | 0.214 | 0.129 |
| | (0.001) | (0.001) | (0.004) | (0.005) | (0.014) | (0.022) |
| N | 450,604 | 299,961 | 10,798 | 7,683 | 7,674 | 7,418 |
| Number previous children | 0.746 | 0.651 | 0.671 | 0.403 | 0.408 | 0.227 |
| | (0.002) | (0.002) | (0.010) | (0.010) | (0.031) | (0.045) |
| N | 448,690 | 298,468 | 10,737 | 7,683 | 7,674 | 7,418 |
| Married at birth (Yes/No) | 0.770 | 0.792 | 0.441 | 0.401 | 0.492 | 0.171 |
| | (0.001) | (0.001) | (0.006) | (0.006) | (0.018) | (0.025) |
| N | 450,604 | 299,961 | 10,798 | 7,683 | 7,674 | 7,418 |
| Month married at birth | 46.523 | 55.051 | 70.163 | 69.403 | 67.135 | 47.688 |
| | (0.082) | (0.104) | (0.947) | (1.039) | (2.853) | (6.926) |
| N | 346,848 | 237,442 | 4,765 | 3,078 | 3,076 | 2,917 |
| Number of children at birth | 1.039 | 1.039 | 1.026 | - | | |
| (i.e., twins) | (0.000) | (0.001) | (0.002) | | | |
| N | 450,603 | 299,962 | 10,798 | | | |
| Child is a girl (Yes/No) | 0.486 | 0.487 | 0.485 | 0.482 | 0.485 | 0.514 |
| | (0.001) | (0.001) | (0.005) | (0.006) | (0.018) | (0.035) |
| N | 450,604 | 299,961 | 10,798 | 7,683 | 7,674 | 7,418 |
| *Panel b.* Outcome variables | | | | | | |
| Log birth weight | 8.164 | 8.192 | 8.083 | 8.093 | 8.081 | 8.079 |
| | (0.001) | (0.001) | (0.002) | (0.002) | (0.006) | (0.013) |
| N | 450,465 | 299,820 | 10,798 | 7,683 | 7,674 | 7,418 |
| Days of gestation | 274.547 | 274.277 | 276.475 | 277.697 | 275.867 | 275.831 |
| | (0.027) | (0.034) | (0.162) | (0.140) | (0.425) | (0.802) |
| N | 414,757 | 269,190 | 10,783 | 7,683 | 7,674 | 7,418 |
| Rate of stillbirth in 1,000 | 4.239 | 4.971 | 5.649 | - | | |
| | (0.110) | (0.147) | (0.819) | | | |
| N | 450,603 | 299,962 | 10,798 | | | |
| IMR | 3.314 | 3.836 | 5.309 | 3.384 | 5.233 | 0.264 |
| (excluding stillbirths) | (0.091) | (0.121) | (0.746) | (0.663) | (2.684) | (0.117) |
| N | 448,694 | 298,470 | 10,737 | 7,683 | 7,674 | 7,418 |

*Notes*: Table presents means across samples of mothers: natives, immigrants and former refugees (indistinguishable in our data due to identical residence permits), refugees arriving in 2010-2017, and our core sample refugees. *IMR* refers to rate of one year mortality in a 1,000. Columns 5 and 6 are weighted means and show our effective sample, following Aronow and Samii (2016). More specifically we use our main regression specification, i.e. Column 3 in Table 1. We regress the match indicator on the full set of controls and fixed effects, square the residual from these regressions and weight the means by these. The reduced sample size is due to the fact that some observations do not contribute to the match effect, i.e. singleton countries of origin (as common in any fixed effect regression Miller et al. (2019)), we do not drop them from the main sample as they still contribute to the estimation of for example year of arrival fixed effects and thus help estimating these more precisely.
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

## A.6 Language definition and country of origin information

In this Section we describe details of our language classification. Our main data source is the CIA Factbook data: There, for instance, Benin is listed as *"French (official), Fon and Yoruba (most common vernaculars in south), tribal languages (at least six major ones in north)"*. Accordingly, our classification defines Benin as *French spoken = 1* and *official French = 1*, but *Italian spoken = 0.* For Algeria the CIA lists *"Arabic (official), French (lingua franca), Berber or Tamazight (official); dialects include Kabyle Berber (Taqbaylit), Shawiya Berber (Tacawit), Mzab Berber, Tuareg Berber (Tamahaq)"*, which is (*official French = 0*, *French spoken = 1*, *Italian spoken = 0*), and finally Libya *"Arabic (official), Italian, English (all widely understood in the major cities); Berber (Nafusi, Ghadamis, Suknah, Awjilah, Tamasheq)"*, hence (0,0,1). We also compared the resulting list of French- or Italian-speaking countries (Table A.6.1) with CEPII's dyadic country-level data (c.f. Mayer and Zignago 2011). Table A.6.1 shows these for countries in our sample.

**Table A.6.1:** Language definition and descriptives by country of origin

| Country | Main language | Language exposure Fre | Fre-off | Ita | Birth outcomes weight | logweight | gestation | IUGR |
|---|---|---|---|---|---|---|---|---|
| Benin | FRENCH | 1 | 1 | 0 | 3600 | 8.17 | 273.75 | 13.18 |
| Burkina Faso | FRENCH | 1 | 1 | 0 | 3025 | 8.01 | 273.50 | 11.09 |
| Burundi | KIRUNDI | 1 | 1 | 0 | 3400 | 8.13 | 284.50 | 11.96 |
| Cameroon | FRENCH | 1 | 1 | 0 | 3363 | 8.11 | 277.58 | 12.07 |
| Chad | FRENCH | 1 | 1 | 0 | 2953 | 7.93 | 271.00 | 10.73 |
| Congo(Brazzaville) | FRENCH | 1 | 1 | 0 | 3190 | 8.06 | 263.00 | 12.14 |
| Congo(Kinshasa) | FRENCH | 1 | 1 | 0 | 3228 | 8.06 | 274.42 | 11.72 |
| Cote d'Ivoire | FRENCH | 1 | 1 | 0 | 3249 | 8.05 | 275.25 | 11.70 |
| Djibouti | FRENCH | 1 | 1 | 0 | 3200 | 8.07 | 279.40 | 11.46 |
| EquatorialGuinea | FRENCH | 1 | 1 | 0 | 3530 | 8.17 | 287.00 | 12.30 |
| Guinea | FRENCH | 1 | 1 | 0 | 2995 | 7.99 | 276.33 | 10.83 |
| Haiti | FRENCH | 1 | 1 | 0 | 3220 | 8.08 | 281.00 | 11.46 |
| Niger | FRENCH | 1 | 1 | 0 | 3090 | 8.04 | 273.00 | 11.32 |
| Rwanda | KINYARWANDA | 1 | 1 | 0 | 3323 | 8.10 | 275.00 | 12.10 |
| Senegal | FRENCH | 1 | 1 | 0 | 3298 | 8.09 | 274.60 | 12.00 |
| Togo | FRENCH | 1 | 1 | 0 | 3337 | 8.10 | 277.17 | 12.03 |
| Algeria | ARABIC | 1 | 0 | 0 | 3410 | 8.13 | 281.10 | 12.14 |
| Egypt | ARABIC | 1 | 0 | 0 | 3265 | 8.09 | 275.55 | 11.85 |
| Lebanon | ARABIC | 1 | 0 | 0 | 3583 | 8.18 | 280.83 | 12.74 |
| Morocco | ARABIC | 1 | 0 | 0 | 3316 | 8.09 | 278.39 | 11.87 |
| Syria | ARABIC | 1 | 0 | 0 | 3275 | 8.08 | 275.46 | 11.86 |
| Tunisia | ARABIC | 1 | 0 | 0 | 3188 | 8.02 | 272.36 | 11.55 |
| Italy | ITALIAN | 0 | 0 | 1 | 2819 | 7.89 | 257.33 | 10.70 |
| Libya | ARABIC | 0 | 0 | 1 | 3519 | 8.16 | 277.43 | 12.68 |
| Somalia | SOMALIAN | 0 | 0 | 1 | 3270 | 8.08 | 280.34 | 11.63 |
| Afghanistan | DARI, PASHTO | 0 | 0 | 0 | 3372 | 8.11 | 276.52 | 12.18 |
| Albania | ALBANIAN | 0 | 0 | 0 | 3490 | 8.14 | 279.14 | 12.48 |
| Angola | PORTUGUESE | 0 | 0 | 0 | 3190 | 8.05 | 274.00 | 11.59 |
| Armenia | ARMENIAN | 0 | 0 | 0 | 3323 | 8.10 | 279.23 | 11.89 |
| Azerbaijan | AZERBAIJANI | 0 | 0 | 0 | 3378 | 8.11 | 279.25 | 12.07 |
| Bangladesh | BANGLA | 0 | 0 | 0 | 2792 | 7.93 | 271.00 | 10.30 |
| Belarus | RUSSIAN | 0 | 0 | 0 | 3407 | 8.13 | 287.00 | 11.87 |
| Bosnia and Herzegovina | BOSNIAN | 0 | 0 | 0 | 3358 | 8.11 | 278.02 | 12.06 |
| Bulgaria | BULGARIAN | 0 | 0 | 0 | 2870 | 7.96 | 254.00 | 11.30 |
| CaboVerde | PORTUGUESE | 0 | 0 | 0 | 2630 | 7.87 | 279.00 | 9.43 |
| China | MANDARIN | 0 | 0 | 0 | 3470 | 8.13 | 278.31 | 12.42 |
| Colombia | SPANISH | 0 | 0 | 0 | 3291 | 8.09 | 272.14 | 12.04 |
| Croatia | CROATIAN | 0 | 0 | 0 | 3060 | 8.02 | 273.25 | 11.18 |
| Cuba | SPANISH | 0 | 0 | 0 | 3090 | 8.04 | 284.00 | 10.88 |
| Czechia | CZECH | 0 | 0 | 0 | 2950 | 7.99 | 273.00 | 10.81 |
| Eritrea | TIGRINAYA | 0 | 0 | 0 | 3340 | 8.10 | 279.31 | 11.93 |
| Ethiopia | AMHARIC | 0 | 0 | 0 | 3352 | 8.10 | 279.20 | 11.98 |
| Gambia | ENGLISH | 0 | 0 | 0 | 2912 | 7.95 | 272.50 | 10.61 |
| Georgia | GEORGIAN | 0 | 0 | 0 | 3304 | 8.09 | 276.42 | 11.94 |
| Ghana | ASANTE | 0 | 0 | 0 | 3289 | 8.09 | 280.13 | 11.73 |
| Guinea-Bissau | CRIOULO | 0 | 0 | 0 | 2748 | 7.85 | 262.80 | 10.20 |
| Hungary | HUNGARIAN | 0 | 0 | 0 | 1980 | 7.59 | 229.00 | 8.65 |
| India | HINDI | 0 | 0 | 0 | 3043 | 8.01 | 276.33 | 11.01 |
| Iran | PERSIAN | 0 | 0 | 0 | 3285 | 8.08 | 274.12 | 11.96 |
| Iraq | ARABIC | 0 | 0 | 0 | 3285 | 8.09 | 276.08 | 11.88 |
| Israel | HEBREW | 0 | 0 | 0 | 3820 | 8.25 | 284.00 | 13.45 |
| Jordan | ARABIC | 0 | 0 | 0 | 3265 | 8.08 | 272.83 | 12.00 |
| Kazakhstan | KAZAKH | 0 | 0 | 0 | 3685 | 8.20 | 282.50 | 13.10 |
| Kenya | ENGLISH | 0 | 0 | 0 | 3359 | 8.11 | 278.82 | 12.07 |
| Kyrgyzstan | KYRGYZ | 0 | 0 | 0 | 3490 | 8.15 | 284.67 | 12.23 |
| Liberia | ENGLISH | 0 | 0 | 0 | 3025 | 8.00 | 266.67 | 11.39 |
| Macedonia | MACEDONIAN | 0 | 0 | 0 | 3096 | 8.03 | 274.60 | 11.26 |
| Moldova | MOLDOVAN | 0 | 0 | 0 | 2500 | 7.82 | 266.00 | 9.40 |
| Mongolia | MONGOLIAN | 0 | 0 | 0 | 3439 | 8.12 | 275.73 | 12.40 |
| Montenegro | MONTENEGRIN | 0 | 0 | 0 | 3237 | 8.08 | 275.50 | 11.75 |
| Myanmar | BURNAMESE | 0 | 0 | 0 | 3153 | 8.05 | 275.00 | 11.43 |
| Nepal | NEPALI | 0 | 0 | 0 | 3739 | 8.22 | 279.80 | 13.36 |
| Netherlands | DUTCH | 0 | 0 | 0 | 2900 | 7.97 | 295.00 | 9.83 |
| Nicaragua | SPANISH | 0 | 0 | 0 | 3350 | 8.12 | 281.00 | 11.92 |
| Nigeria | ENGLISH | 0 | 0 | 0 | 3266 | 8.08 | 275.87 | 11.80 |
| Norway | NORWEGIAN | 0 | 0 | 0 | 3320 | 8.11 | 275.00 | 12.07 |
| Pakistan | PUNJABI | 0 | 0 | 0 | 2818 | 7.90 | 263.25 | 10.53 |
| Palestine | ARABIC, HEBREW | 0 | 0 | 0 | 3385 | 8.13 | 273.50 | 12.38 |
| Portugal | PORTUGUESE | 0 | 0 | 0 | 2870 | 7.96 | 273.00 | 10.51 |
| Romania | ROMANIAN | 0 | 0 | 0 | 2670 | 7.89 | 271.00 | 9.85 |
| Russia | RUSSIAN | 0 | 0 | 0 | 3455 | 8.14 | 278.72 | 12.39 |
| Saudi Arabia | ARABIC | 0 | 0 | 0 | 2610 | 7.85 | 258.00 | 10.03 |
| Serbia and Montenegro | SERBIAN | 0 | 0 | 0 | 3262 | 8.08 | 276.41 | 11.78 |
| South Africa | AFRIKAANS | 0 | 0 | 0 | 3800 | 8.24 | 291.00 | 13.06 |
| Spain | SPANISH | 0 | 0 | 0 | 3380 | 8.13 | 279.00 | 12.11 |
| Sri Lanka | TAMIL | 0 | 0 | 0 | 3150 | 8.04 | 274.38 | 11.45 |
| Sudan | ARABIC | 0 | 0 | 0 | 3415 | 8.12 | 276.55 | 12.33 |
| Tajikistan | TAJIK | 0 | 0 | 0 | 3420 | 8.14 | 273.00 | 12.53 |
| Tanzania | KISWAHILI | 0 | 0 | 0 | 3260 | 8.09 | 281.00 | 11.60 |
| Turkey | TURKISH | 0 | 0 | 0 | 3288 | 8.09 | 275.75 | 11.91 |
| Turkmenistan | TURKMEN | 0 | 0 | 0 | 3140 | 8.05 | 289.00 | 10.87 |
| Uganda | ENGLISH | 0 | 0 | 0 | 3332 | 8.09 | 272.00 | 12.18 |
| Ukraine | UKRANIAN | 0 | 0 | 0 | 3467 | 8.14 | 276.95 | 12.50 |
| Uzbekistan | UZBEK | 0 | 0 | 0 | 3247 | 8.08 | 276.14 | 11.76 |
| Venezuela | SPANISH | 0 | 0 | 0 | 3530 | 8.17 | 279.00 | 12.65 |
| Yemen | ARABIC | 0 | 0 | 0 | 3052 | 8.01 | 270.00 | 11.25 |
| Zimbabwe | SHONA | 0 | 0 | 0 | 3680 | 8.21 | 282.00 | 13.05 |
| _CH-born/stateless/NA | | 0 | 0 | 0 | 3364 | 8.11 | 279.55 | 12.03 |

*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

## A.7 Network measures

In the Figure A.7.1 we plot the distribution of the five network measures that we use, in Table 3. All of the measures exhibit a sizable skewness, which is why we use four quartiles in Table 3 panel B. Panel A uses all refugees from the same origin allocated to the same destination canton, before the refugee to-be mother in question (excluding anyone from their arrival family - arrived together, handled by the same caseworker, from same origin, into same destination, and allocated within the same day). Panel B, uses information from the birth registry and *all* mothers from the same origin and in the same destination, this includes and is in-fact highly dominated by (potentially) earlier refugees and immigrant mothers. Panels C uses the same definition but restricts to mothers in the year prior to the to-be mother in question (-356 days). Panels D and E use the birth registry but only refugee mothers, the definition is analogous to Panel B and C but restricted to this much smaller but much more important subset of mothers most likely to be the current peer group of the to-be mother in-question.

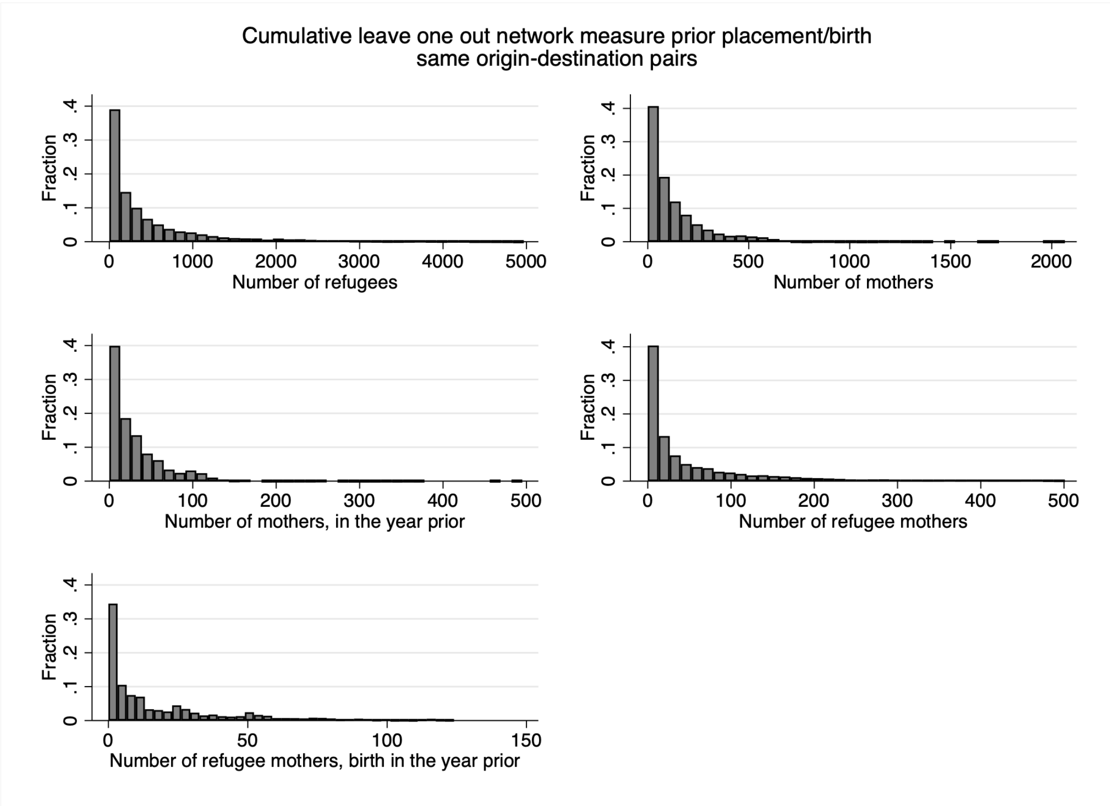


**Figure A.7.1:** Refugee' co-ethnic networks

*Note*: Figure plots refugees' network sizes according to different definitions, from Table 3 Panel A and C.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, own calculations.

## A.8 Employment

Figure A.8.1 provides the annual employment shares of refugees by their residence duration in Switzerland. The data stems from the ZEMIS foreign registry, covering all individuals stemming from the asylum process (mostly asylum seekers and accepted refugees) between 2010 and 2016. Note that this cohort is not identical with our refugee population that arrived between 2010 and 2017, since it also includes refugees who arrived before 2010 to increase sample size of refugees with longer residence duration. We observe a maximum number of 8 residence years for mothers who arrived in 2010. As shown in Figure A.8.1, employment shares are close to zero for women in the first years after arrival and soon level out at around 10 percent for females. Even potential refugee partners show rates not higher than approximately 30% overall. These low shares are also partly explained by various employment restrictions that are in place for asylum seekers (e.g., Slotwinski et al. 2019). Hence, we can safely assume that our health effects are not driven by earnings effects, especially when considering mothers (and couples) who arrived recently (see also Auer 2018; Marten et al. 2019).

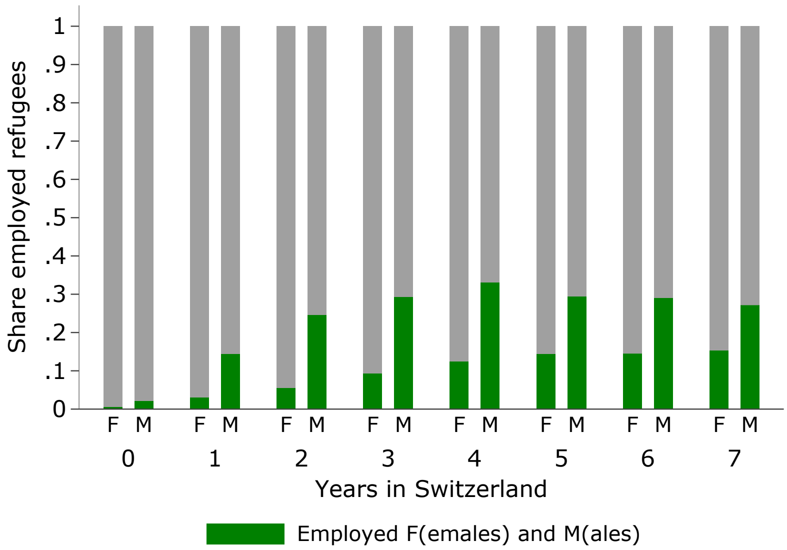


**Figure A.8.1:** Refugee networks

*Note*: Figure plots annual shares of employed female and male refugees by their residence duration in Switzerland. All individuals originating from the asylum system between 2010 and 2016 are included (asylum seekers, temporarily admitted persons, accepted refugees, accepted refugees with permanent residence permit).
*Source:* ZEMIS 2010-2016, own calculations.

# B Additional estimation information

This section presents additional results omitted from the main text for brevity.

## B.1 Additional descriptive statistics

We first present the numbers behind Figures 4 balancing tests in Table B.1.1, and corresponding randomization inference p-values, and Table B.1.2 for Figure 5. Figure B.1.1 presents the distribution of refugees in our main sample mapped across the world, which is contrasted using the effective weighting approach by Aronow and Samii (2016) implicit in our main specification (shown in Table 1 Column (3) - any language exposure and Column (4) French-official match).

**Table B.1.1:** Table of Figure 4: Covariate balance

| | Any language match | | French-official match | |
|---|---|---|---|---|
| | All women | Later mothers | All women | Later mothers |
| | (1) | (2) | (3) | (4) |
| Age at arrival /10 | 0.077 | -0.024 | 0.011 | -0.033 |
| | (0.046) | (0.053) | (0.085) | (0.070) |
| RI | $p = 0.13$ | $p = 0.70$ | $p = 0.91$ | $p = 0.71$ |
| Married at arrival (Yes/No) | -0.000 | 0.038 | 0.002 | -0.016 |
| | (0.013) | (0.029) | (0.034) | (0.081) |
| RI | $p = 0.97$ | $p = 0.24$ | $p = 0.95$ | $p = 0.79$ |
| Pseudo arrival family size/10 | -0.017 | -0.075 | 0.052 | 0.087 |
| | (0.022) | (0.041) | (0.042) | (0.054) |
| RI | $p = 0.42$ | $p = 0.15$ | $p = 0.41$ | $p = 0.21$ |
| Arrived with adult females (Yes/No) | -0.009 | -0.030 | 0.027 | 0.057 |
| | (0.013) | (0.023) | (0.020) | (0.016) |
| RI | $p = 0.49$ | $p = 0.25$ | $p = 0.29$ | $p = 0.01$ |
| Request form: Other family (Yes/No) | 0.030 | -0.012 | 0.033 | 0.016 |
| | (0.019) | (0.037) | (0.020) | (0.049) |
| RI | $p = 0.19$ | $p = 0.79$ | $p = 0.11$ | $p = 0.75$ |
| Request form: Peers (Yes/No) | -0.004 | 0.003 | 0.009 | 0.026 |
| | (0.004) | (0.007) | (0.005) | (0.015) |
| RI | $p = 0.38$ | $p = 0.70$ | $p = 0.14$ | $p = 0.13$ |
| Request form: Birth (Yes/No) | -0.020 | 0.009 | 0.007 | 0.030 |
| | (0.009) | (0.010) | (0.010) | (0.019) |
| RI | $p = 0.04$ | $p = 0.45$ | $p = 0.55$ | $p = 0.28$ |
| Request form: Other reasons (Yes/No) | -0.001 | -0.001 | -0.003 | -0.005 |
| | (0.001) | (0.001) | (0.002) | (0.004) |
| RI | $p = 0.34$ | $p = 0.49$ | $p = 0.16$ | $p = 0.44$ |
| Main caseworker (Yes/No) | -0.027 | -0.021 | -0.054 | -0.080 |
| | (0.018) | (0.025) | (0.022) | (0.043) |
| RI | $p = 0.13$ | $p = 0.46$ | $p = 0.08$ | $p = 0.15$ |
| Second main caseworker (Yes/No) | 0.007 | 0.000 | 0.054 | 0.032 |
| | (0.020) | (0.023) | (0.023) | (0.050) |
| RI | $p = 0.78$ | $p = 0.98$ | $p = 0.09$ | $p = 0.68$ |
| Other caseworkers (Yes/No) | 0.020 | 0.020 | -0.000 | 0.048 |
| | (0.014) | (0.015) | (0.018) | (0.033) |
| RI | $p = 0.31$ | $p = 0.23$ | $p = 0.99$ | $p = 0.17$ |
| $N$ | 57,105 | 7,683 | 57,105 | 7,683 |
| Origin country FEs | ✓ | ✓ | ✓ | ✓ |
| Destination Canton FEs | ✓ | ✓ | ✓ | ✓ |
| Month & Year FEs | ✓ | ✓ | ✓ | ✓ |
| Allocation characteristics indicators | ✓ | ✓ | ✓ | ✓ |

*Notes*: Table presents coefficient estimates of equation (3), identical to those presented in Figure 4. Additionally, include p-values from a randomization inference tests, strata (country of origin), treatment-cluster (Canton), 99 replications.
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

**Table B.1.2:** Table of Figure 5: Post-allocation selection

| | Any language match | French-official match |
|---|---|---|
| | (1) | (2) |
| *Fertility selection* | | |
| Birth | -0.008 | 0.014 |
| | (0.010) | (0.018) |
| RI | $p = 0.49$ | $p = 0.63$ |
| $N$ | 57,105 | 57,105 |
| Time in Switzerland till first birth | 0.058 | 0.022 |
| | (0.058) | (0.143) |
| RI | $p = 0.38$ | $p = 0.83$ |
| $N$ | 10,798 | 10,798 |
| *Sample selection* | | |
| Multiple entries | -0.005 | -0.014 |
| | (0.008) | (0.012) |
| RI | $p = 0.56$ | $p = 0.33$ |
| $N$ | 57,105 | 57,105 |
| Placed before birth | 0.058 | 0.022 |
| | (0.058) | (0.143) |
| RI | $p = 0.38$ | $p = 0.83$ |
| $N$ | 10,798 | 10,798 |
| Reallocated Canton | -0.011 | -0.032 |
| | (0.012) | (0.025) |
| RI | $p = 0.41$ | $p = 0.32$ |
| $N$ | 7,873 | 7,873 |
| Dropped main sample: stillbirth | 0.003 | 0.006 |
| | (0.006) | (0.009) |
| RI | $p = 0.75$ | $p = 0.93$ |
| $N$ | 7,873 | 7,873 |
| Dropped main sample: too young or light | -0.001 | 0.002 |
| | (0.004) | (0.011) |
| RI | $p = 0.81$ | $p = 0.77$ |
| $N$ | 7,873 | 7,873 |
| Dropped main sample: twins (or higher) | -0.003 | 0.009 |
| | (0.008) | (0.014) |
| RI | $p = 0.78$ | $p = 0.72$ |
| $N$ | 7,873 | 7,873 |
| *Selection on risk factors* | | |
| Conception month march (Yes/No) | -0.001 | -0.009 |
| | (0.010) | (0.033) |
| RI | $p = 0.92$ | $p = 0.88$ |
| $N$ | 7,683 | 7,683 |
| Age of mother at first birth in Switzerland /10 | -0.024 | -0.057 |
| | (0.050) | (0.066) |
| RI | $p = 0.70$ | $p = 0.49$ |
| $N$ | 7,683 | 7,683 |
| Teenage mother (Yes/No) | 0.014 | 0.007 |
| | (0.019) | (0.019) |
| RI | $p = 0.56$ | $p = 0.79$ |
| $N$ | 7,683 | 7,683 |
| Child is female (Yes/No) | -0.015 | -0.085 |
| | (0.029) | (0.053) |
| RI | $p = 0.71$ | $p = 0.28$ |
| $N$ | 7,683 | 7,683 |
| Previous children (Yes/No) | -0.082 | 0.001 |
| | (0.063) | (0.065) |
| RI | $p = 0.21$ | $p = 0.99$ |
| $N$ | 7,683 | 7,683 |
| Birth in different canton than placement (Yes/No) | -0.082 | 0.001 |
| match | 0.009 | 0.024 |
| | (0.017) | (0.039) |
| RI | $p = 0.60$ | $p = 0.54$ |
| $N$ | 7,683 | 7,683 |
| Origin country FEs | ✓ | ✓ |
| Destination Canton FEs | ✓ | ✓ |
| Month & Year FEs | ✓ | ✓ |
| Allocation characteristics indicators | ✓ | ✓ |

*Notes*: Table presents coefficient estimates of equation (3), identical to those presented in Figure 5. Additionally, include p-values from a randomization inference tests, strata (country of origin), treatment-cluster (Canton), 99 replications.
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

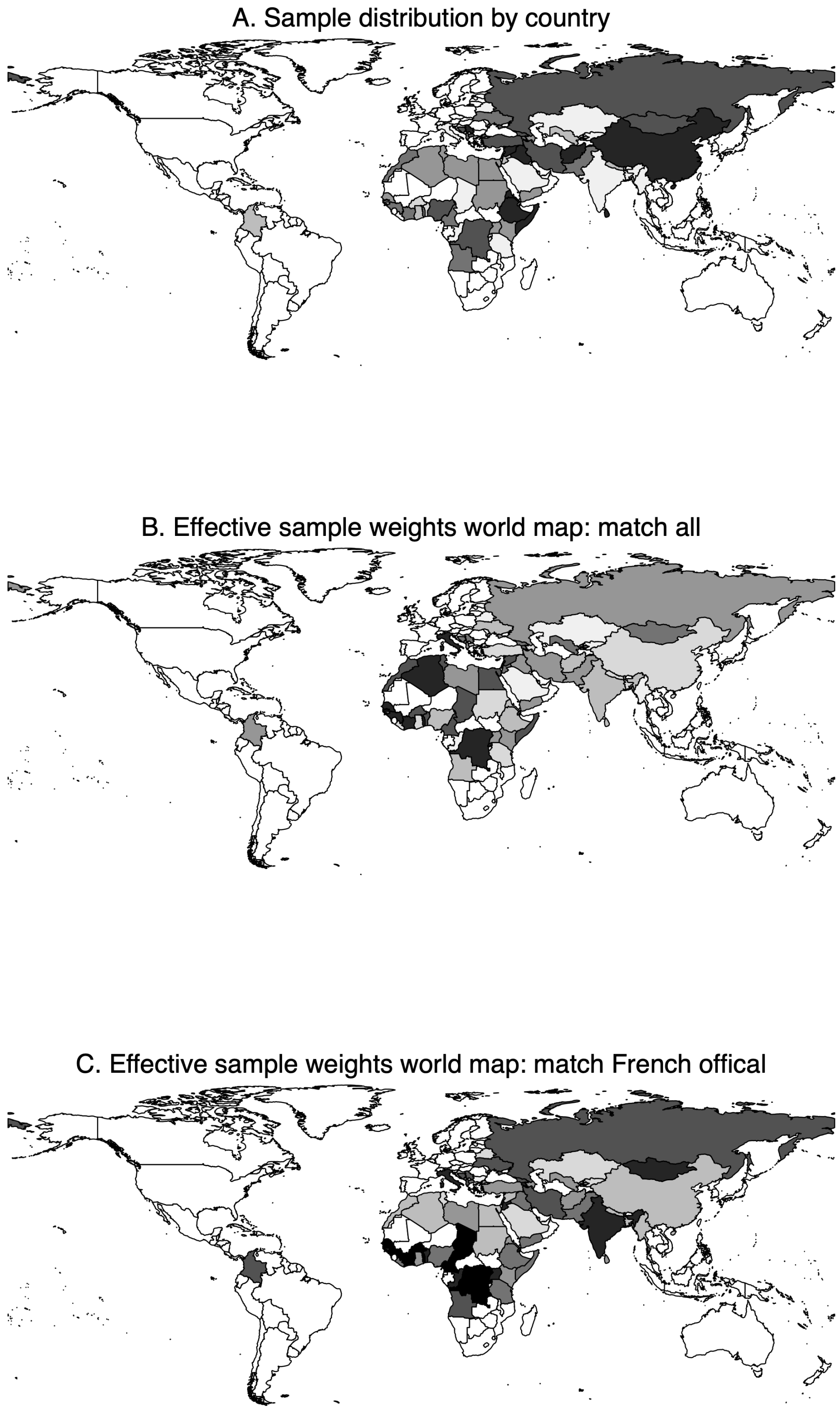


**Figure B.1.1:** Refugees' countries of origin and effective weights

*Note*: Figure displays the overall sample distribution of countries in A., with effective regression weights. based on the main model in B., and in the French official model C. Darker tones indicate higher shares and weights. See Appendix Table A.5.1, for their construction.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

## B.2 Sub-DiDs

In the text we discussed that our Difference-in-Differences effects can be decomposed into sub-components. Distinguishing Italian- from French-exposed refugees gives seven DiDs, yet we can go one step further and disaggregate the French-French category into official French-speaking country and French as a spoken language. Figure B.2.1 displays the setup in more detail.

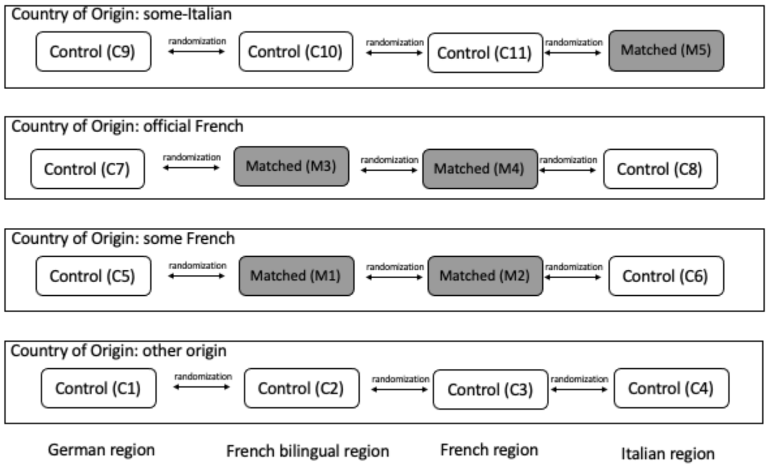


**Figure B.2.1:** 4X4 DESIGN MATRIX

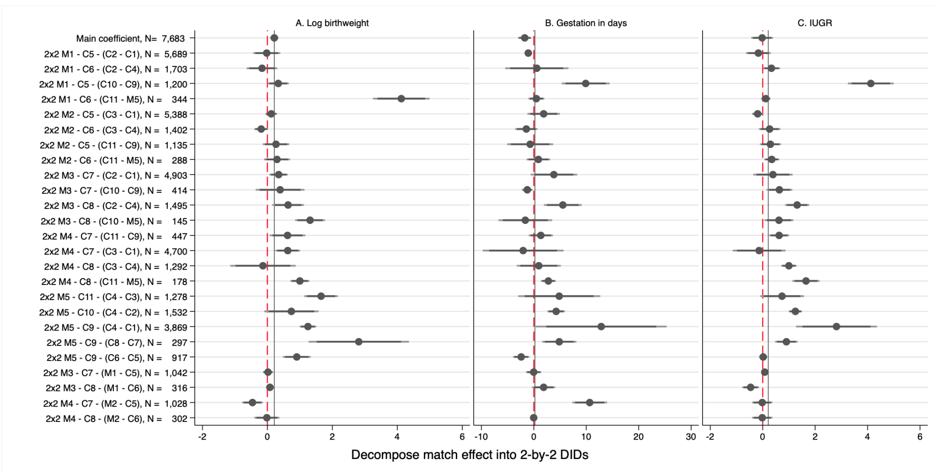


**Figure B.2.2:** DECOMPOSING THE AVERAGE TREATMENT EFFECT

Although the samples get small in some instances, the 'match' effects are almost never significantly different from our main effects reported mostly positive and often significant, for example in the log birthweight regressions, only one is significantly negative out of 25 possible decompositions, and three negative but highly insignificant. This shows that negative weighting as discussed by Goodman-Bacon 2018a is very unlikely in our setup.

# C Additional results

This appendix presents several complementary results to our main analysis. First, we present in Figure C.1 for our other main outcomes, gestation, weight-by-gestation, LBW, corresponding to Figure 1. Next, we present the aggregate comparison, in Figure C.2, we show the more standard treatment and control comparison, and in Table C.1 we present the aggregated (to country of origin and canton of destination) estimate of our main effect. The table in Column 2 and 3 shows that only French exposed refugees benefit from the French environment and Italian exposed from the Italian destination, but importantly not vice versa.

Since in the mono-lingual french cantons, group there is a slightly larger number of French exposed females, we present conservative bounds. Following Horowitz and Manski (2000) we artificially increase the respective FO population using both the lowest and highest observed log birth weights to impute the outcome values. It is well-known that these bounds are very conservative and often too large to make meaningful bounds. Lee (2009) ads an assumption on the monotonicity, thereby achieving tighter bounds. In our application this means that F-refugees only have a larger propensity to be placed in the F-region (not lower), which we belief is reasonable in our application. Thus, we proceed by trimming the FF group by dropping observation from the lower part and upper part of the distribution (96th and 31st percentile in our case). Note that across the three treatment groups the excess is very small, our treatment group is 4.8% of the overall sample, and the target is 4.6%. The small relative size of the treatment group is beneficial if there are heterogeneous treatment effect, see Słoczyński (2020).

Next we present several robustness tests in Section C.1, Table C.1.1, presents various subsamples. Table C.1.2 tests for the functional from, and in Table C.1.3, we augment equation (3)

$$\begin{aligned} y_{iort} = \alpha + \gamma \text{Language Match}_{i_{or}} + \tau \text{Language Match}_{i_{or}} \times Refugee_i \\ + x'_{iort}\beta + \delta_{i_o} \times Refugee_i + \delta_{i_r} \times Refugee_i + \delta_{i_t} + \varepsilon_{iort}, \end{aligned}$$

Further we present in Figure C.1.1 for our other main outcomes, gestation, weight-by-gestation, LBW, corresponding to Figure 9.

In the last Section C.2 we present further discussion of the heterogeneity of the match effect across individual birth heterogeneity (i.e. gender) and allocation canton heterogeneity.

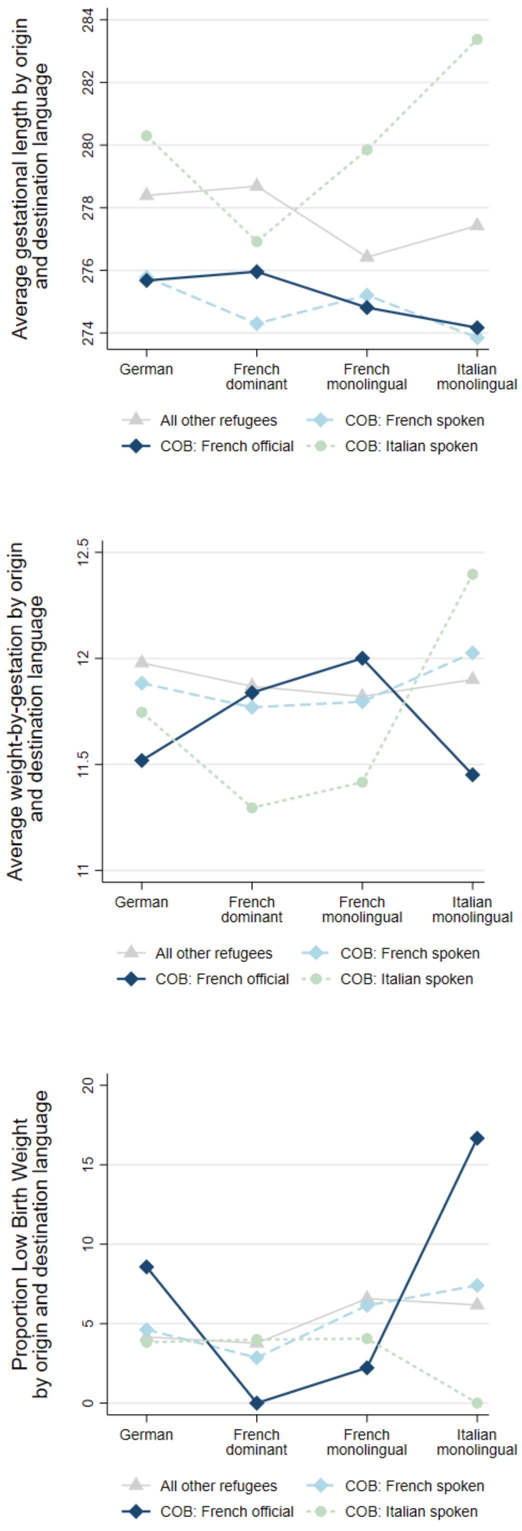


**Figure C.1:** Descriptive average comparison for other main outcomes

*Note*: Figure presents analogous mean comparisons for gestational age in days, weight-by-gestation, and proportion LBW as in Figure 1, see notes therein.

*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

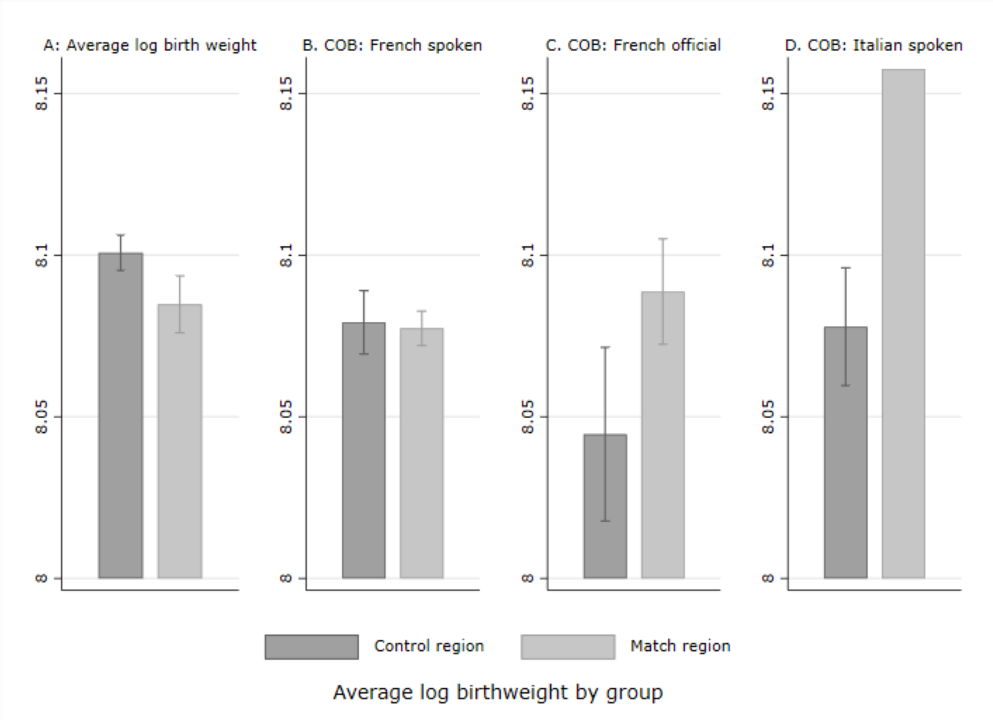


**Figure C.2:** Match and non-Match regions average log birth weight differences by country of origin

*Note*: Figure presents raw means and 90 percent confidence intervals based on Canon-level clustered standard errors of the log birth weight means across refugee origins and destination pairs. Note that the Italian region is singleton, hence no standard error is provided, since this group is relatively small the uncertainty is very large for this group. Panel A, includes all non-French and -Italian exposed refugees. Control regions are the German-speaking cantons and match are both both French and Italian regions. In Panel B, all refugees that origin from a country that has French either as a main or an official language, C the subset of official French speaking countries, D for those where a large enough share of the population speaks Italian. In B, C, and D only the respective matches for the refugee mothers as match region.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

**Table C.1:** Descriptive aggregate comparison

| | | Country of origin | | Bounds | |
|---|---|---|---|---|---|
| | Overall | French | Italian | Horowitz-Manski | Lee |
| | (1) | (2) | (3) | (4) | (5) |
| match | 0.021 | | | [-0.075; 0.364] | [-0.005; 0.033] |
| | (0.009) | | | | |
| French-speaking destination | | 0.018 | -0.018 | | |
| | | (0.008) | (0.002) | | |
| Italian-speaking destination | | -0.035 | 0.081 | | |
| | | (0.035) | (0.000) | | |

*Notes*: Table present coefficients and cluster-robust standard errors on the Canton level. French-speaking destination includes *main* French-speaking destinations. The coefficients in Column 1 are based on aggregate population-weighted regressions of log birthweight on match indicator, 3 indicators for language origin, and 4 for language destination. Column 2 separates the French from the Italian region and assesses whether French exposed benefit from the Italian region, and Column 3 analogously for the Italian-exposed refugees. Column 4 and 5 are similar to Column 1 but replace the log birthweight with adjusted values, in 4 imputing the excess control group with the lowest (highest) possible value of the log birthweight distribution based on Horowitz and Manski (2000) and in 5 trimming the FF group by those at the bottom (and top) of the birthweight distribution following Lee (2009).
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

## C.1 Robustness

**Table C.1.1:** Robustness main Table 1: Various sub samples

| | Match | | | | | Match: French Official | | | CEPII-definition | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Base | No request | Africa only | No stateless | No Syria & Eritrea | Base | No request | Control English | Match | Official French |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel a.* Log birth weight | | | | | | | | | | |
| match | 0.020 | 0.008 | 0.053 | 0.020 | 0.054 | 0.063 | 0.033 | 0.040 | 0.055 | 0.046 |
| | (0.007) | (0.006) | (0.015) | (0.007) | (0.014) | (0.016) | (0.019) | (0.015) | (0.017) | (0.017) |
| Control Mean | 8.06 | 8.07 | 8.03 | 8.06 | 8.03 | 8.03 | 8.06 | 8.04 | 8.04 | 8.04 |
| R2 | 0.05 | 0.05 | 0.05 | 0.05 | 0.06 | 0.05 | 0.05 | 0.05 | 0.05 | 0.05 |
| *Panel b.* Gestation in days | | | | | | | | | | |
| match | 0.404 | -0.629 | 0.483 | 0.404 | 0.922 | 0.677 | -0.139 | -0.680 | 0.553 | -0.109 |
| | (0.563) | (0.678) | (1.332) | (0.563) | (1.557) | (1.116) | (1.530) | (1.517) | (1.024) | (1.348) |
| Control Mean | 274.76 | 275.34 | 274.88 | 274.76 | 274.58 | 274.37 | 275.19 | 275.18 | 274.78 | 275.00 |
| R2 | 0.05 | 0.05 | 0.04 | 0.05 | 0.05 | 0.05 | 0.05 | 0.04 | 0.05 | 0.05 |
| *Panel c.* IUGR (weight/gestation) | | | | | | | | | | |
| match | 0.207 | 0.128 | 0.571 | 0.208 | 0.550 | 0.623 | 0.354 | 0.466 | 0.539 | 0.497 |
| | (0.066) | (0.056) | (0.140) | (0.066) | (0.128) | (0.168) | (0.191) | (0.134) | (0.184) | (0.167) |
| Control Mean | 11.65 | 11.73 | 11.39 | 11.65 | 11.41 | 11.34 | 11.62 | 11.41 | 11.43 | 11.44 |
| R2 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 |
| *N* | 7,683 | 5,252 | 4,287 | 7,553 | 6,351 | 7,683 | 5,252 | 4,123 | 7,680 | 7,680 |
| Canton FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Country FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Place Year & Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Request FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Birth characteristics FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes*: Table present coefficient estimates from alternative samples to our main results in Table 1, replicating as Base (Column 1 & 6) our main specifications. Column 2 drops all refugees that provide any request, 3 uses only refugees from Africa, 4 drops stateless from the control groups, 5 drops the two largest sending countries (Syria and Eritrea), 7 again drops all refugees that provide any request but in the match regressions based on French is a official language, in 8 we use as control only English-speaking countries. Columns 9 and 10 use the CEPII language match definition, main difference is that it categorizes Tunisia, Algeria, Lebanon, Morocco from main to official match, and dropping Tunisia, Algeria from some match, it also classifies Somalia to non-Italian, which hence becomes a control. *Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

**Table C.1.2:** ROBUSTNESS MAIN TABLE 1 AND 2: FUNCTIONAL FORM

| | Baseline Log Weight | OLS Weight | Poisson Weight | Negbin Weight | Baseline LBW×100 | FEBR Probit LBW |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel a.* Any language exposure (French, Italian) | | | | | | |
| match | 0.022 | 67.038 | 0.020 | 0.021 | -2.928 | -0.274 |
| | (0.008) | (21.965) | (0.007) | (0.007) | (1.393) | (0.123) |
| Percent increase | | 0.02 | | | | |
| AME | | | | | | -2.89 |
| R2 | 0.06 | 0.07 | | | 0.04 | |
| Control Mean $y$ | 8.06 | 3203.22 | 3203.22 | 3203.22 | 6.98 | 0.31 |
| *Panel b.* Official French only | | | | | | |
| match | 0.063 | 169.362 | 0.052 | 0.053 | -8.414 | -0.695 |
| | (0.017) | (45.943) | (0.014) | (0.013) | (3.054) | (0.214) |
| Percent increase | | 0.05 | | | | |
| AME ×100 | | | | | | -7.34 |
| R2 | 0.06 | 0.07 | | | 0.04 | |
| Control Mean $y$ | 8.03 | 3127.58 | 3127.58 | 3127.58 | 10.18 | 0.71 |
| $N$ | 7,683 | 7,683 | 7,683 | 7,683 | 7,683 | 7,683 |
| Canton FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Country FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Placement Year & Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Request FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Birth characteristics FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes*: The Table presents coefficient estimates from alternative specifications of the main functional form. Panel a. uses the match and b. the match French-official. Columns 1 and 2 are the same as in Table 1 and 2 respectively, for comparison. Column 2 is estimated using OLS on the non-logged birth weight. Column 3 a Poisson and 4 negative binominal generalized linear regression also on the non-logged birth weight. Column 6 estimates a bias reduced fixed effect Probit model (Kunz et al., Forthcoming) on the Low Birth Weight indicator. Percent increase is calculated as the coefficient/average in the OLS model, for the Probit model we calculate the average marginal effect.
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

**Table C.1.3:** Robustness main Table 1: DiDiD

| | Not imputed | GDiD | Covars | Conditional | French off | No Bilang |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel a.* Log birth weight | | | | | | |
| match | -0.001 | -0.001 | -0.001 | -0.000 | -0.003 | -0.000 |
| | (0.003) | (0.004) | (0.004) | (0.004) | (0.006) | (0.003) |
| **match × refugee** | 0.019 | 0.019 | 0.020 | 0.020 | 0.065 | 0.028 |
| | (0.007) | (0.006) | (0.007) | (0.007) | (0.019) | (0.008) |
| R2 | 0.02 | 0.02 | 0.02 | 0.04 | 0.04 | 0.05 |
| *Panel b.* Gestation in Days | | | | | | |
| match | 0.108 | 0.119 | 0.119 | 0.090 | -0.355 | 0.165 |
| | (0.173) | (0.175) | (0.175) | (0.174) | (0.542) | (0.231) |
| **match × refugee** | 0.308 | 0.312 | 0.270 | 0.198 | 1.031 | 0.632 |
| | (0.490) | (0.487) | (0.501) | (0.498) | (1.223) | (0.573) |
| R2 | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| *Panel c.* IUGR (Weight/Gestation) | | | | | | |
| match | -0.010 | -0.010 | -0.010 | 0.001 | -0.038 | 0.020 |
| | (0.032) | (0.033) | (0.033) | (0.031) | (0.053) | (0.034) |
| **match × refugee** | 0.191 | 0.188 | 0.200 | 0.204 | 0.654 | 0.249 |
| | (0.063) | (0.062) | (0.068) | (0.064) | (0.188) | (0.076) |
| R2 | 0.03 | 0.03 | 0.03 | 0.06 | 0.06 | 0.07 |
| *N* | 58,114 | 58,114 | 58,114 | 58,114 | 53,954 | 47,763 |
| Canton FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Country FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Placement Year & Month FE | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Request FE | | | ✓ | ✓ | ✓ | ✓ |
| Birth characteristics FE | | | | ✓ | ✓ | ✓ |

*Notes*: Table is analogous to the main Table 1. Imputed values for immigrants and earlier refugees, place Canton, place year and place month are replaced with birth Canton, birth year and month for immigrant only, all others are imputed 0s, ie. for the request characteristics in Column 3 and 4. For immigrants we only use first observed birth.
*Source:* ZEMIS 2010-2017, BEVNAT 2010-2017, CIA 2018, own calculations.

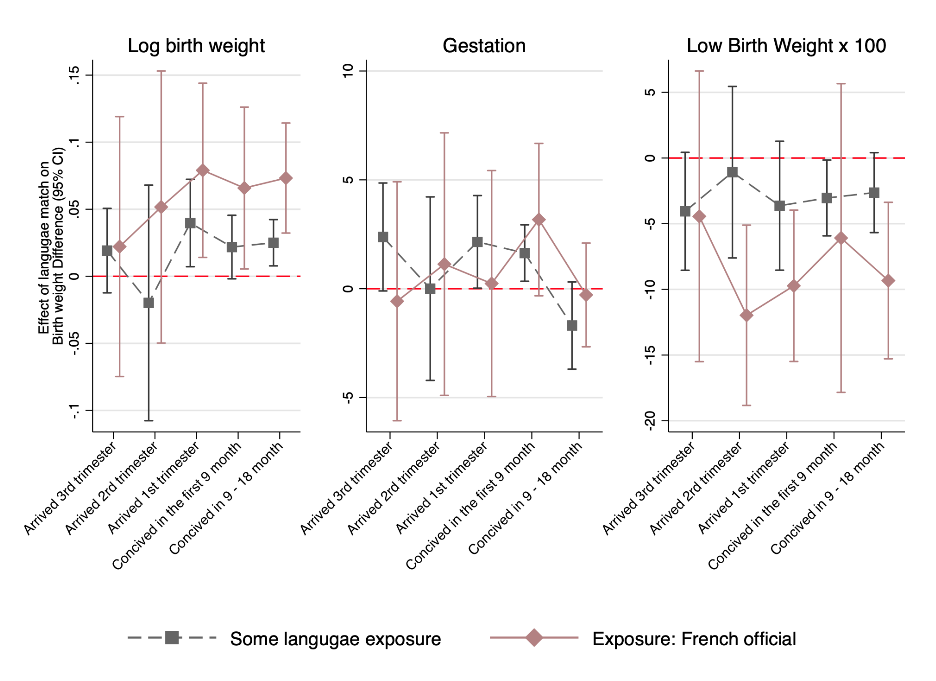


**Figure C.1.1:** Match effects over time and across birth outcomes

*Note*: Figure displays coefficient estimates from main generalized DiD model interacted with time-in-Switzerland for our main outcomes, log birth weight, gestation, and IUGR.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, own calculations.

## C.2 Allocation canton– and individual–level heterogeneity

**Allocation-canton heterogeneity:** An obvious question is whether the local conditions in the allocated canton influence the (size of the) language match effect (for regional health infrastructure variation within Switzerland, see Panczak et al. 2014). With regard to the local health infrastructure it is important to understand that even temporal intra-cantonal mobility is largely restricted for refugees in Switzerland. Theoretically, this even includes visits to hospitals or other health infrastructure if not explicitly approved upfront by local authorities. Hence, it is very likely that prenatal services and deliveries take place in the canton of allocation (even in cases where travel distances across cantonal borders would have been shorter). As discussed, we see that the births are not more likely to take place in an other canton for match and non-match refugees, c.f. 5. We hypothesize that less conducive cantonal characteristics increase the benefits of being allocated to a familiar language region. However, these analyses lack statistical power due to limited variation in characteristics across the limited number of cantons (N=26). Hence, we should refrain from making causal claims and rather suggest likely channels. Moreover, differences are incremental albeit a generally high-quality and relatively homogeneous health care system in Switzerland. If anything the positive language match effect is slightly larger in cantons with a high rate of c-sections (Card et al. 2019), but there is no evidence on a difference based on potentially unnecessary c-sections, which we measure by the ratio of Friday-to-Monday births in the canton (since many preventable c-sections occur on Friday nights (Halla et al. 2020)). We find a small indirect effect of anti-immigrant attitudes measured by support for the dominant right wing party. Possibly, this heterogeneity could be driven by sentiments of the local population and health personnel which are less pronounced or easier to overcome if refugees and locals share the same language. Again, these differences should be interpreted with caution.

**Individual heterogeneity:** In addition, we test the influence of several pregnancy-related individual characteristics. Apart from boys (fragile males hypothesis as discussed in the main text), maternal risk factors, for instance whether the mother is above the age of 34 and/or had no previous birth experience (e.g. Bertrand et al. 2000) slightly affect the size of the language match coefficient. Although every birth is inherently different this may be indicative of an informational channel of our results: for births in which complications are more likely, communication becomes more important.As noted above, past experience can only include previous children born abroad, as we only assess health effects on the first child born in Switzerland to avoid endogenous outcomes. Hence, previous children can not correspond to country-specific health care knowledge.

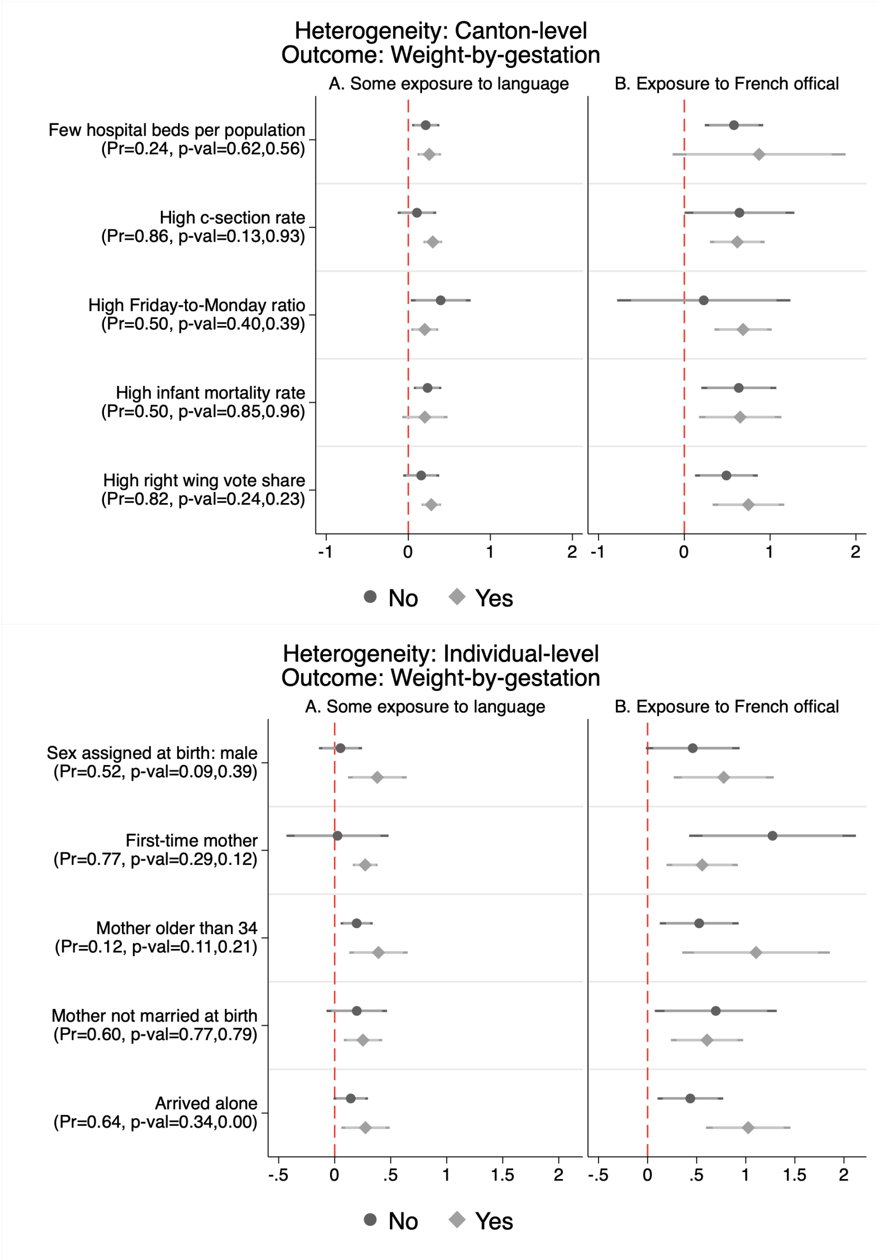


**Figure C.2.1:** Treatment effect heterogeneity by individual and destination

*Note*: Figure presents decomposition of treatment effects for various sub-groupings. The upper dark line corresponds to the coefficient of $(1-d)$ and the lower lighter one to $d$, which is when the statement is true. Confidence intervals are based on robust standard errors clustered on the region (treatment) level. $Pr(d)$ corresponds to the share of the sample for whom $d = 1$, and p-val for the p value of an F-test for equality of the two estimates. The different panels correspond to different levels of groupings: A- canton, B - Country, C - individual.
*Source:* BEVNAT 2010-2017, ZEMIS 2010-2017, CIA 2018, BFS 2018, VDEM 2018 own calculations.