

Ethnic Enclaves and Cultural Assimilation

Pascal Achard*

March 25, 2023

Abstract

Using two separate identification strategies: the quasi-random allocation of asylum seekers in the Netherlands and between-siblings variation in neighborhood characteristics, this paper studies if growing-up in an ethnic enclave slows down immigrants' cultural assimilation. Looking at a culturally charged consumption, the usage of hormonal contraceptives by young immigrant women, and the probability to marry a native, I find that the influence of neighborhood varies with age. While the overall effects of limiting ethnic enclaves during adolescence are not significant, prolonged exposure to natives during childhood, before the age of 10, increases cultural assimilation.

JEL Codes: I12, J15, N44, R23, Z12

Keywords: Cultural Assimilation, Contraceptive Usage, Neighborhood Effect

*CREST, pascal.achard@ensae.fr

I wish to thank Jérôme Adda, Yann Algan, Olof Åslund, Sascha Becker, Michèle Bélot, Alberto Bisin, Leah Boustan, Leonardo Bursztn, Michela Carlana, Elena Cettolin, Joseph Doyle, Per-Anders Edin, Rachel Kranton, Boris van Leeuwen, Katrine Loken, Eleonora Pattachini, Giovanni Peri, Alessandro Riboni, Pauline Rossi, David Schindler, Jan Stuhler, Sigrid Suetens, Marco Tabellini and Yves Zenou together with seminar participants at EEA, RES, SOLE, SMYE, EALE, European Meeting of the Econometric Society, IZA Migration Meeting, CREST, EUI, Tilburg, Alicante and Oxford for helpful comments. I acknowledge financial support from the EUI presidential funds and from ODISSEI. This study is registered in the AEA RCT Registry, and the unique identifying number is: "AEARCTR-0002857".

1 Introduction

The lasting presence of Non-Western immigrants in Europe and the United States has fueled heated political debates on their cultural integration in the host society (Bansak et al., 2016; Adida et al., 2014a; Tabellini, 2020; Alesina and Tabellini, 2020). Preventing the formation of ethnic enclaves is often considered as a tool to foster cultural assimilation. The rationale behind this idea is that mixing immigrants with natives, in particular when they are young, accelerates the adoption of natives’ mainstream norms and attitudes (Bisin and Verdier, 2010; Bisin et al., 2016; Bazzi et al., 2019; Abramitzky et al., 2020a). The objective of this paper is to assess empirically whether growing-up in an ethnic enclave causally affects cultural assimilation.¹

My main measure of cultural assimilation is the usage of hormonal contraceptives by immigrant teenage women.² Following the sexual revolution of the 1960s, perspectives on female premarital sexuality changed in the Western world. One example of the more liberal views is the widespread usage of hormonal contraception among young women (Goldin and Katz, 2002; Bailey, 2006). However, more conservative gender norms still apply in many non-Western countries and by extension among many immigrant communities (Algan et al., 2013; Adida et al., 2014b).

In the Netherlands, hormonal contraceptives are not sold as over-the-counter medication; they must be prescribed by health care providers. Consequently, despite their personal and intimate nature, their usage is recorded (free of measurement error) in administrative health registries. Descriptive statistics confirm the intuition behind this outcome. There are large differences in hormonal contraceptive usage between native-Dutch and non-Western immigrant populations. By age 20, 86.1% of native-born individuals have used hormonal contraceptives at least once compared with only 36.5% of first-generation immigrants. There are no such differences in the usage of other drugs (anti-inflammatory, antihistamines, etc.). Merging (on an individual identifier) administrative data on drug usage and a survey on attitudes towards sexuality shows that using hormonal contraceptives is a good proxy for adopting Western views on sexuality, and potentially a good indicator for being sexually active. To complement the findings of this paper, I use another outcome, i.e. marrying a native.³

¹In this paper, I use the term “cultural assimilation” in a loose way that encompasses “cultural integration”. I define them as adopting a behavior typical of natives, and I do not make any normative statement, relying on those terms for ease of expression.

²This outcome was specified in the Pre-Analysis Plan (PAP) attached in Appendix G.

³For ease of expression, I refer to using hormonal contraceptives as the *contraception outcome* and marrying a native as the *marriage outcome*.

To draw causal inference on neighborhood characteristics, I use and compare two identification strategies. The first one focuses on asylum seekers. Upon arrival in the Netherlands, they are cared for and hosted by a public organization, the Central Agency for the Reception of Asylum Seekers (COA). This agency quasi-randomly disperses asylum seekers into its accommodations across the country. Neighborhood ethnic concentration at the time of assignment by the COA is exogenous and used to estimate causal effects. I refer to this approach as the asylum seekers' strategy.

The second approach relies on family fixed effects and uses differences in ethnic concentration experienced by siblings. If a family moves or if the ethnic composition of a neighborhood changes over time, two sisters (who are not the same age) will be exposed to different environments during their childhood and adolescence. Conditional on sorting into neighborhoods being time-invariant within a family, this strategy produces causal estimates. I provide statistical evidence supporting this assumption. I refer to this approach as the sibling strategy. It complements the first one in several ways. It can be implemented on a much larger sample since it is not restricted to asylum seekers. It provides variation in cumulated exposure. Instead of looking at ethnic concentration at one point in time, i.e. when asylum seekers are assigned to neighborhoods, this approach relies on variation in neighborhood characteristics experienced during the entire childhood and adolescence.

In both cases, I study the environment in which young immigrants grow up rather than the one in which they live as adults. Childhood and adolescence are periods where individual core values, such as gender norms or the distinction between in-group and out-group members are still malleable (Boucher et al., 2021; Alan et al., 2021). It is a particularly relevant age to study the adoption of cultural norms. I measure ethnic concentration as the share of natives and refer to an ethnic enclave as a neighborhood where this share is low.

The baseline results using the asylum seekers' strategy are small in magnitude and not statistically significant for both the contraception and marriage outcomes. An overall null effect can however hide significant heterogeneity on meaningful subgroups. Following a large body of literature that has shown the effects of early intervention on educational outcomes, health and cognitive development (Almond et al., 2018; Barham et al., 2013; Cappelen et al., 2020) and intermarriage (Merlino et al., 2019), I look whether the effect is stronger for exposure during childhood (below 10) or adolescence (above 10).⁴ This

⁴In this paper, there are clear minority and majority groups (asylum seekers/immigrants and natives). I focus on the influence from the majority to the minority. This differs from studies which look at exposure to minorities on the behavior of members of the majority group Merlino et al. (2019); Carrell et al. (2019);

distinction is also informative about mechanisms. A stronger effect during childhood is consistent with the idea that many deeply rooted attitudes are shaped at a young age and more malleable during this period. On the other hand, changing the composition of the pool of potential sexual partners during adolescence corresponds to a supply shock.

The baseline estimate does not hide significant heterogeneous effects for the contraception outcome. By looking at a subsample of girls allocated by the COA before the age of 10, I find that an increase in the share of natives in the neighborhood has a significant positive effect on the marriage outcome. Increasing this share by 10 percentage points, which corresponds to moving from the median to the 75th percentile of the distribution, increases the marriage outcome by 1.7 percentage point. This result is robust to using Machine-Learning techniques designed to estimate Conditional Average Treatment Effects (Athey et al., 2019). Using the sibling strategy, a 10 p.p. increase in the (cumulated) share of natives increases the contraception outcome by 1.7 percentage point. When breaking down this effect between exposure before and after 10 years old, only the former is significant.

To assess the magnitude of the results, I compare the estimate to the difference in outcomes between natives and immigrants. For the contraception outcome, this difference amounts to 50 points (36% vs 86%). For the marriage outcome, this difference is even larger, 76 p.p. (89 % vs 13%).⁵ The neighborhood effect is an order of magnitude below this metric. Therefore, while the direction of the effects is positive, I interpret their size to be modest.

While the results of the two strategies are not numerical identical, a coherent picture emerges from the two outcomes and the two strategies. This is apparent when one focuses on the magnitude of the effect and the pattern of heterogeneity in addition to looking at statistical significance. In both cases, the size of the effects is small and both strategies find larger effects on exposure during childhood. The takeaway of this paper is therefore that ethnic concentration has a positive but relatively small effect on cultural assimilation and that it is concentrated on exposure during childhood.

Finseraas et al. (2019). In particular, Merlino et al. (2019) show that exposure to minorities at school increases the probability for Whites to have romantic relationship with Blacks later in their lives. It also differs from settings where only minority groups interact, see Bazzi et al. (2019) for the effect of ethnic fractionalization on nation-building.

⁵An alternative measure of magnitude is the ratio of the estimate to the mean outcome. Given that the unconditional probability to marry a native in the baseline sample is 13%, a 1.7 p.p. increase is by no means small. The unconditional probability of using contraception by the age of 20 is 36.1%, an additional 1.7 p.p. corresponds to a 4.7% increase relative to the mean outcome. Given that in the setting of this paper, immigrants are in contact with natives from a young age, I find it relevant to take natives' results as a benchmark.

I perform two important robustness checks. First, using the asylum seekers' strategy, I study the effect of ethnic enclaves on educational attainment. I find that the share of natives has a statistically significant positive effect on the baseline sample, meaning on girls who arrived before and after the age of 10. The fact that the asylum seekers' strategy is able to find an effect on an important outcome alleviates concerns over its potential lack of statistical power. This positive effect is also an interesting result on its own and adds evidence on the importance of neighborhood effects' on education (Aslund et al., 2011; Chetty et al., 2016a; Chetty and Hendren, 2018a; Billings et al., 2022). Secondly, I check that the effect on marriage is general to the population of asylum seekers, i.e. boys and girls, and not just limited to the latter. Using the asylum seekers strategy, I find that a 10 percentage point increase in the share of natives increase the marriage outcome by 1.2 percentage point for boys. This effect is weakly significant.

When it comes to policy recommendations and assessing the overall effects of growing up in an ethnic enclave on children, this paper provides evidence that limiting ethnic enclaves has a positive effect on the integration of young immigrants. This follows from both positive effects on education and cultural assimilation. It also shows that mixing natives and immigrants increases cultural assimilation when it happens at a young age.

My main contribution to the vast and rapidly growing literature on cultural transmission (Bisin and Verdier, 2000; Bisin et al., 2004) and cultural assimilation (Kuran and Sandholm, 2008; Bisin et al., 2008, 2016) is to provide causal estimates of neighborhood influence (Jarotschkin and Zhuravskaya, 2019; Abramitzky et al., 2020a; Algan et al., 2022; Gagliarducci and Tabellini, 2021; Jaschke et al., 2022).⁶ In particular, I focus on exposure during childhood and adolescence and find a differentiated effect by age. To my knowledge, this evidence is new in the literature on cultural assimilation. This corroborates evidence showing that there are critical periods in the development of children and teenagers van den Berg et al. (2014); Chetty and Hendren (2018a); Chetty et al. (2016b). It complements the findings from Boucher et al. (2021) who show the positive effect of early mixing on the social integration of young Syrian refugees in Turkey. In particular, I show that the effects of mixing can be long-lasting.

My second contribution to the literature is to propose a revealed preference measure of cultural assimilation, i.e., using hormonal contraceptives.⁷ It complements previous

⁶Although this paper documents the strong persistence of cultural traits, in particular attitudes on sexuality and their extensions to gender norms, I restrict attention to changes which occur within a generation - as opposed to Alesina et al. (2013); Giuliano and Nunn (2021) who focus on a much larger time horizon.

⁷This outcome has also been used in the context of development countries. See Kohler et al. (2001) and Munshi and Myaux (2006).

work which has focused on immigrants’ sense of identity (Bisin et al., 2008, 2016; Dahl et al., 2022). Someone who grew up in the West may still self-identify as belonging to the country of origin. Yet, she could already have adapted (at least partially) to the mainstream culture. Therefore, I do not focus on stated preferences, but rather on a revealed one, as measured by specific types of consumption (Atkin et al., 2021).⁸ Last but not least, the fact that usage of hormonal contraceptives is observable in administrative data provides this measure with important “statistical” properties; availability for large samples, absence of measurement error, possibility to link with other registries.

My third contribution is to the literature that has relied on so-called “dispersal policies” in Denmark (Damm, 2009; Damm and Dustmann, 2014), Sweden (Dahlberg et al., 2012; Aslund et al., 2003, 2011), and Norway (Bratsberg et al., 2021), to provide causal estimate of neighborhood effects.⁹ The purpose of these policies is to spread out asylum seekers throughout the country. I rely on a similar natural experiment in the Netherlands (Beckers and Borghans, 2011). The literature has found positive effects of larger ethnic communities on labor markets outcomes of the parents, i.e. adult asylum seekers at the time of their arrival (Aslund et al., 2003; Beckers and Borghans, 2011; Damm, 2009), and mixed evidence on the educational achievements of their children (Aslund et al., 2011; Danzer et al., 2018). The neighborhood literature has extensively looked at educational outcomes and points to a negative influence of ethnic enclaves (or, more generally, low-performing peers); see Chyn and Katz (2021). My results are consistent with their findings. In contrast to prior literature (Aslund et al., 2003; Damm, 2009; Aslund et al., 2011; Damm and Dustmann, 2014; Danzer et al., 2018), I study neighborhood influence on a different dimension, i.e., cultural assimilation, and at a more disaggregated level, equivalent to a large census tract in the US (similar to Kling et al. (2007); Aslund et al. (2011); Chetty et al. (2016a)).¹⁰

⁸Harder et al. (2018) propose a framework to evaluate measures of immigrants assimilation. Contraceptive usage meets one of the most important criteria laid out, namely construct validity through correlation with other predictors of assimilation. This is assessed in Section 3.

⁹Concerns have been expressed about the identification strategy followed in Dahlberg et al. (2012), see Nekby and Pettersson-Lidbom (2017) and Dahlberg et al. (2017). Three main criticisms were addressed; about (i) the reliability of the measure of preference, (ii) the endogeneity of sample selection, and (iii) the mismeasurement of the refugee placement program. None of these concerns applies to this paper; (i) is discussed at length in Section 3, (ii) robustness to sample choices, are discussed in 5.4, and (iii) does not apply here. Dahlberg et al. (2012) identify the number of asylum seekers allocated to a municipality indirectly, through the amount of money transferred to a city to host refugees. By contrast, I use very granular data on collective accommodations; see Section 4.1.1.

¹⁰Other empirical strategies have been used to assess neighborhood effects: neighborhood fixed effects (Bertrand et al., 2000; Grönqvist, 2006), a housing voucher lottery (Kling et al., 2007; Chetty et al., 2016a), and quasi-random variation by focusing on movers (Chetty and Hendren, 2018b). Harding et al. (2021) provide a discussion on potential differences in results between different identification strategies.

The rest of the paper is organized as follows: Section 2 presents the data, while Section 3 discusses the use of hormonal contraceptives as a measure of cultural assimilation. Section 4 presents the asylum seekers strategy and section 5 details its results. Section 6 presents the siblings strategy and its results. Section 7 presents robustness checks. The last section discusses the findings and concludes.

2 Description of the Data

I combine three sources of data: Dutch administrative registries collected and maintained by the Centraal Bureau voor de Statistiek (CBS), information on the location and operating dates of accommodations run by the COA, and survey data on sexual attitudes and behaviors. The CBS offers a very rich set of administrative datasets, linkable through a unique individual identifier. This allows us to assemble information on various topics (medicine usage, neighborhood ethnic concentration, family situation, etc.), to link parents to children and surveys to administrative registries.

2.1 Usage of Contraceptives

The Dutch healthcare system fully reimburses a set of “basic” drugs. The CBS collects their usage at the individual level on an annual basis. Hormonal contraception for women younger than 21 is part of the basic package. The data are collected on the entire population living in the Netherlands and is not self-reported.

Data are collected from dispensed (and not just prescribed) medicine. If a drug is prescribed, but not collected, it does not appear in the registries. These data are available for the years 2006-2019. For contraceptives, the most disaggregated entry (in the ATC4 classification) is the category G03A, “Hormonal contraceptives for systemic use”. It includes the pill, patches, injections, and implants.¹¹

The objective of the mechanism designer (the Dutch government) is to make contraception accessible to avoid unwanted pregnancies. Hormonal contraceptives are therefore easily available and affordable to young women. Contraceptives in the Netherlands can be prescribed by a GP and not necessarily by a gynecologist. Parental consent is not

I indirectly contribute to this literature by using two identification strategies.

¹¹More information is available on the WHO website. G03A is itself subdivided into four categories: G03AA progestones and oestrogens, fixed combinations, G03AB progestones and oestrogens, sequential preparations, G03AC progestones and G03AD emergency contraceptives. Although the category G03A includes emergency contraceptives, the morning after pill is not part of the basic package, and thus not recorded in the data.

necessary after a girl turns 16. According to the Personal Data Protection Act (Wet Bescherming Persoonsgegevens, abbreviated WBP), parents cannot access information on their children’s treatments once they are older than 16.

2.2 Data on COA Accommodation and Ethnic Concentration

The Netherlands are composed of 380 municipalities. Their sizes vary between 4,000 (Ameland) and 850,000 (Amsterdam) inhabitants. A zip code in the Netherlands is composed of 4 digits and 2 letters. The four digits divide the country into more than 4,000 areas with a median population of 2,647 inhabitants.¹² This is the definition of “neighborhood”, as used in this paper.

I combine administrative registries on where people live with information on all accommodations listed at the COA between 1995 and 2012. Both can be merged at the address level. Asylum seekers are under the responsibility of the COA from the moment they start their asylum application. During this period, they can either stay (i) in collective accommodation together with other asylum seekers or (ii) with relatives already living in the country. In both cases, the address where they are staying is known to the COA. There are a total of 17,000 different addresses used by the COA for the period 1995-2012, out of which 15,500 could be located by the CBS.

2.3 Other Administrative Registries

In addition to individual level information on where people live and what drugs they use, I exploit administrative registries on citizenship, educational attainment, marriage and migration history. Appendix A.1.1 provides a list of the datasets used.

2.4 Survey Data

Survey on Sexual Behavior in The Netherlands I use the survey “Sex under the age of 25” collected in 2017 by SOAIDS, Rutgers and CBS. This is a large scale (more than 20,000 respondents) survey carried out over a representative sample of people aged 12 to 25 in the Netherlands. This information can be merged on an individual identifier with registry data. I look at several dimensions of attitudes towards sexuality and (reported) behavior: namely positive opinion on sex before marriage, on homosexuality (attitude

¹²In 2017, the country was made of 4,066 zip4 areas. The median population is 2,647, while the mean is 3,413 inhabitants. The entire zipcode (“zip 6”) roughly corresponds to the street level. The zip4 level is large enough to approximate the environment in which someone lives. It is larger than the zip6 area and therefore less likely to miss part of the social interactions.

towards two men kissing in the street), on female sexuality (attitudes towards women having had several sexual partners), a knowledge index (ranging from 0 to 8 mostly on knowledge about STIs), a question on ever having had sex and, conditional on the former, if the respondent uses condoms. Appendix A.1.2 provides more information on variables' construction.

Survey on Immigrant assimilation in France To provide illustrative evidence on how contraceptive relates to other measures of immigrants' social assimilation, I use an additional data source from outside the Netherlands, namely the French survey "Trajectoires et Origines" (TeO). TeO was carried out in 2008/2009 by the National Statistical Institute (INSEE) and the National Institute for Demographic Studies (INED) on a representative sample of second generation immigrants. In particular, it asks questions both about pill usage and dimensions of social assimilation more commonly used in the literature. I look at three relevant ones: (i) social network; proxied by the likelihood to report having a majority of friends from the same origin, (ii) identity; proxied by the likelihood to fully identify as French and (iii) importance of religion. Appendix A.1.3 provides more information on variables construction.

2.5 Some Definitions Used to Characterize Samples

Definition of Non-Western Non-Western immigrants refer to immigrants from Africa, the Middle East and South-East Asia.¹³

Restrictions Based on Age and Presence in the Netherlands To ensure that women have spent at least five years in the Netherlands, I focus on teenagers who have lived continuously in the Netherlands between the ages of 16 to 20 years old. This ensures that I do not capture a mechanical effect of access to healthcare. I focus primarily on complete spells (all observations from 16 to 20 years of age). Data on the contraception outcome are available from 2006 onwards. Immigrants who arrived before the age of 16, but turned 16 before 2006, are not in the complete spell sample. In robustness checks, I also focus on those with incomplete spells, i.e., women whose contraceptive usage is observed from age 18 until age 20.¹⁴ Since hormonal contraception is the main outcome

¹³More specifically coming from the following regions: North-Africa, East Africa, South Africa, West Africa, South Asia, South East Asia, Middle East, and Central Asia as listed in Table A1

¹⁴There is a limited risk of misclassification when using the incomplete spell sample. This would come from women who started using hormonal contraceptives at the age of 16 or 17 and stopped later on. To assess how likely this is, I compare usage rates by the age of 20 among complete spells to hypothetical results if consumption was only observed from the age of 18 onwards. There would be a misclassification

used throughout this paper, these restrictions apply to samples used for other outcomes.

3 Culturally Charged Consumption

3.1 Differences between Immigrants and Natives

Table 1 reports the probability for natives, first-generation and second-generation (Non-Western) immigrants to have used the contraception outcome at least once at all ages between 16 and 20 years old. There is a striking difference between natives and immigrants. While almost all native females have taken contraceptives at least once by the age of 20 (86.1%), this proportion is much smaller for immigrants. The percentages for first and second generations are, respectively, 36.5% and 45.3%.

By the age of 20 (there is no reason to look at a starting age for medicine other than contraceptives), there is no substantial difference between immigrants and natives in the usage of other drugs such as anti-inflammatory or of any medicine (extensive margin).¹⁵ This shows two things: (i) immigrants have access to healthcare and (ii) the difference between natives and immigrants is specific to the contraception outcome.

Relation to Reported Sexual Behavior and Attitudes Immigrant women have much more conservative views than natives on all aspects measured in the “Sex under the age of 25” survey. They are also less likely to report being sexually active. For all outcomes, the difference between the two groups is very large, see Table B2.¹⁶ Immigrants who use the contraception outcome (as measured in administrative registries) hold more liberal views than those who do not. Table 2 reports linear regressions of various measures taken from “Sex under the age of 25” on age fixed effects and the contraception outcome.¹⁷ Conditional on age, and compared to the mean values, immigrant women who use hormonal contraceptives are twice as likely to consider sex before marriage as normal, 65% more likely to hold positive views on homosexuality, and 59% less likely to

of 3.56%.

¹⁵Table B1 shows the most common medicines used by young women. Hormonal contraceptives are the most frequent type (24.76% of all medicines), with anti-inflammatory being a distant second (6.13%).

¹⁶However, conditional on reporting sexual activity, immigrants are equally likely to use condoms. This shows that differences in usage of hormonal contraceptives is not just a difference in the method of contraception used.

¹⁷“Contraceptive” is a dummy taking the value of one if a woman has used hormonal contraceptives at least once between the age of 16 and 20. Since the survey was carried out in 2017, some women were interviewed potentially before they started using hormonal contraceptives. For instance, someone who started at the age of 19, and who was 18 at the time of the survey. Therefore I use the distinction (contraceptives or not) as a time-invariant signal of immigrants’ type.

Table 1: Differences in usage between immigrants and natives

Age	Natives	1 st Gen	2 nd Gen
Hormonal Contraceptives (%)			
16	44.3	7.3	15
17	63.5	14.3	24.1
18	75.4	21.9	32
19	82.2	29.5	39
20	86.1	36.5	45.3
Anti-inflammatory (%)			
20	44.2	44.5	46.2
Any Drug (%)			
20	97.9	94.3	96.2
No Obs	660,524	12,506	91,614

Notes: This table reports the number of young women who were living in the Netherlands between the ages of 16 to 20, together with the percentage who have used contraceptives, anti-inflammatory medicine, or any medication at least once by a certain age. Women are classified into three groups: natives, first-generation immigrants, and second-generation (Non-Western) immigrants.

be judgmental on pre-marital female sexuality. They also report having sex at a much higher rate. The contraception outcome is also a strong indication of (reported) sexual activity.¹⁸ In light of these results, I interpret the contraception outcome as a proxy for adopting native-type attitudes regarding sexuality.

Relation to Other Measures of assimilation Using data from TeO, Table B3 shows that using the pill is also associated with other measures of social assimilation (social network, identity and importance of religion). Observing these differences in another country (France), and for dimensions other than attitudes regarding sexuality, shows the relevance and external validity of the contraception outcome.

3.2 Heuristic Discussion

The rationale behind using the contraception outcome as a measure of cultural assimilation is that it captures a dimension in which views and attitudes between immigrants

¹⁸Tables B4 and B5 provide additional evidence supporting this interpretation. Using information from the “Sex under the age of 25” survey, Table B4 shows that immigrants and natives use similar methods of contraception. Using information from “Trajectoires et Origines”, Table B5 shows that immigrant women report virginity and abstinence more often than native women as a reason for not using the pill.

Table 2: Differences between taking/not taking hormonal contraceptives for immigrants

	Before Marriage	Homosexuality	Female Sexuality	Knowledge Index	Had Sex
Contraceptive	0.30*** (0.038)	0.11*** (0.032)	0.20*** (0.039)	0.94*** (0.165)	0.45*** (0.037)
Mean Outcome	0.32	0.17	0.34	6.39	0.49
No. of Obs	538	538	538	538	538
R-Squared	0.14	0.04	0.09	0.08	0.32

Notes: This table reports the results from several linear regressions where the outcomes are measures on attitudes and (reported) behavior regarding sexuality. Explanatory variables are age fixed effects and a dummy for whether or not a woman has used hormonal contraceptives between the ages of 16 to 20. The sample is made of first- and second-generation (Non-Western) immigrants who answered the survey “Sex under the age of 25”. Outcome variables include a dummy for having positive views on sexual intercourse before marriage, on two men kissing in the street, and on women having several sexual partners. It also includes a knowledge index ranging from 0 to 8 and a dummy for having had sex.

and natives are opposite and cannot be reconciled. The generalization of the pill in the Western World follows the sexual revolution of the 1960s, which modified views on female pre-marital sexuality. It corresponds to societal changes which have not been experienced (or experienced to a much lesser extent) outside the Western World. Although potentially controversial at first sight, this outcome is a good reflection of the topic it wishes to address. Young immigrants often feel torn between two sets of cultural values: those from the origin countries and those from the destination countries. This outcome fully mirrors the conflicting aspect of cultural change.

Some “Statistical” Properties Observations come from administrative registries. This alleviates concerns over measurement error (which would typically arise when using survey data) and small sample size. This measure is repeated, I observe whether young immigrants end up behaving like natives but also how quickly they do so. This outcome is not a stated preference, but rather a revealed one about an easily accessible good. It is the result of a personal decision that can be hidden (from parents) easily and does not require reciprocation.

Relation to Other Measures The contraception outcome should be thought of as a complement to already established measures. A very popular one in the literature is whether immigrants give a native-sounding name to their children Algan et al. (2022); Abramitzky et al. (2020b). This measures cultural transmission rather than assimilation. In the former, the parents are the decision-makers who perpetuate (or do not perpetuate) their traditions. In the latter, the children themselves decide whether or not to follow the mainstream (native) behavior.

Survey measures on religiosity and identity are also very informative (Casey and Dustmann, 2010; Bisin et al., 2016). However, their sample size is much smaller than administrative data.

Given that the Netherlands do not accept dual citizenship, becoming a Dutch citizen is a measure of assimilation to the destination country. However, one can break legal ties with her origin country while not adapting fully to natives’ standards. Education is a measure of integration, although mostly of economic rather than cultural assimilation.¹⁹

Correlation between different measures I look at the correlation between different potential measures of social and cultural integration available in the data with the marriage outcome. Results are shown in Table 3²⁰ Contraception has the highest correlation with the marriage outcome. Given that marriage with a native is seen as the “final stage of assimilation” Gordon (1964) and is used extensively in the literature Bisin et al. (2004); Fryer (2007), I take this strong correlation as evidence that the contraception outcome captures a relevant dimension of culture.

Table 3: Correlation with the Marriage outcome

	Marriage
Contraception	0.425
Citizenship	0.266
Education	0.177
N.o. Obs	20,178

Notes: This table reports the (polychoric) correlation coefficient between the binary variable “being married to a native” with having the Dutch citizenship by the age of 20, having used hormonal contraceptives at least once by the age of 20 and a categorical variable for educational attainment (see Section 7 for a discussion based on the categories from Table A2.). Observations are married women from Table 1 with non-missing information on education.

¹⁹I do not consider education level to be a “cultural” variable, for two reasons: (i) the vast literature on the educational attainment of immigrants has not labeled education as a “cultural” outcome, and (ii) there is no evidence that second-generation immigrant women have lower educational achievements than second-generation male immigrants (Algan et al., 2010). This would be a necessary condition for educational attainment to be given a cultural interpretation. Figlio et al. (2019) relate cultural traits, in particular long-term orientation, to educational performance. This would plead in favor of interpreting education success as cultural. However, Figlio et al. (2019) is based on country level variation, which I hold constant by using origin country fixed effects. This partials out the variation that Figlio et al. (2019) use to identify culture.

²⁰I report polychoric correlation coefficients to account for the binary and categorical nature of the variables. This approach is also suited if one considers that the binary and categorical outcomes are driven by a latent variable summarizing cultural assimilation. Pearson correlation coefficients are lower in magnitude but display the same ordering.

Potential Questions and Concerns I anticipate two potential concerns: that the estimation suffers from measurement error bias, and that the main outcome is “too demanding”. Since I use contraceptives as a proxy, one could be concerned that the estimation is plagued with measurement error. This does not mean that contraceptive usage would be misreported, but that it is a noisy measure of an underlying factor. Note that, under the assumptions of classical measurement error, mismeasurement in the outcome variable leads to inflated standard errors. Precision of the results is not a problem in this paper.

Norms surrounding female pre-marital sexuality are very entrenched in one’s culture. A concern could be that this dimension is the last one to change in the process of cultural assimilation. Using it as the main outcome could drive all the results downwards. This is unlikely to be the case since differences between immigrants and natives are very large (see Table 1). Even small changes that would only narrow the gap between the two groups, can come out as statistically significant.

4 Placement of asylum seekers

To provide causal estimates, one must rely on exogenous variation in residential choices. The ideal large-scale real-life experiment would be to (i) take young girls whose cultural background differs from that of natives; (ii) allocate them randomly to different neighborhoods; (iii) let them grow up in the assigned places until they become teenagers or young adults; and then (iv) observe their behavior. The placement of asylum seekers in the Netherlands very much resembles the ideal setting. I observe young girls who arrived in the Netherlands as asylum seekers. When they arrive in the country, they are quasi-randomly dispersed across the country by the Central Agency for the Reception of Asylum Seekers (COA). This assignment provides the exogenous variation required to make causal inference. I then look at the relevant outcomes later in their lives.

4.1 Asylum Placement Procedure

Asylum seekers arriving in the Netherlands by plane or at another point on the border can claim asylum once they are in the country. They are placed under the responsibility of the COA, which is responsible for accommodating them. I exploit this allocation as an exogenous variation on where asylum seekers live.²¹ Since 1996, the COA is responsible

²¹The level at which randomization takes place is different from the other so-called “dispersal policies” (Aslund et al., 2003; Damm, 2009; Bratsberg et al., 2021). In most designs, asylum seekers are allocated to municipalities, which are rather aggregate levels (and which should be thought of as polygons on a map). In the case of this paper, randomization occurs at the address level (a point on a map). This

for the housing of all asylum seekers in the Netherlands (Beckers and Borghans, 2011). When a family needs to be hosted, the COA looks for a suitable location. The allocation is done centrally, and the main criterion is availability of a place.²²

There are two obvious threats to the exogeneity of this allocation. First, asylum seekers could choose to live in a specific location, or the COA could send families to specific neighborhoods based on unobservable characteristics. Although possible on paper, this scenario seems unlikely in practice. From discussions with COA personnel, the main difficulty when allocating families is to find an accommodation ready to host a family when it arrives in the country. The difficulty comes from the limited supply of housing.

The second threat is the existence of family ties in the Netherlands. Asylum seekers with family or friends already living in the Netherlands could use their pre-existing ties to influence where they live. This is potentially a concern, since asylum seekers have the possibility to stay with their relatives. However, this case is very well documented in the data. Asylum seekers who live with family members still have to register their address with the COA. Since this allocation cannot be considered as exogenous, I only consider asylum seekers living in collective housing. There is no evidence that the results are not externally valid as discussed in Section 5.5. Because there are no definite guidelines on how the allocation is made (as it is mostly based on availability), I perform numerous statistical tests to back up the exogeneity of assignment.

4.1.1 Sample Characteristics

I combine information on the exact addresses and operating dates of all COA accommodation (meaning collective and non-collective housing) for the period between 1996 and 2012 with administrative registries on where people live. The COA operates various types of accommodations, which are listed in Table C2.

Definition of the Main Sample To identify the experimental population, I look at immigrants who were registered at an address (building level) listed by the COA.²³ I

allows us to look at more disaggregated definitions of neighborhoods (Billings et al., 2022). Identification of the experimental population relies on the granularity of the data (asylum seekers are identified through living at the address of a COA accommodation, an approach that I refer to as the “address approach”), and not on aggregate information about the number of asylum seekers (Dahlberg et al., 2012) allocated to each municipality.

²²Elements of the procedure have been summarized in a communication between the Dutch government and the European Commission and are reproduced in Appendix C.

²³Before 2000, the COA would register asylum seekers to the municipality if they had been hosted for a year. This is when they start appearing in registries. After 2000, registration happens after six months. This is how I reconstitute the beginning of their stay at an address.

exclude asylum seekers whose placement is not a collective center. If someone is registered successively in two eligible places, I keep the one where she has stayed the longest.²⁴

Descriptive Statistics Table 4 reports sample characteristics, panel A of the household head, and panel B of the young women followed on the baseline analysis. Table C1 reports the distribution of the main countries of origin. There is variation in the characteristics of the household head. The distribution of their educational attainment is close to uniform if one groups the two highest categories. There is evidence that the location at assignment is constrained. Asylum seekers settle in far fewer distinct neighborhoods at assignment (437 different neighborhoods) than when the outcome of their daughter starts being measured, i.e., at the age of 16 (1,636 different neighborhoods).

4.2 Treatment Characteristics

Assignment and Later Residential Choices While being hosted by the COA, asylum seekers are free to go outside of their place of residence, and their children go to local schools. After 6 months, they can also look for a regular job. By no means are they kept in closed centers; living there means interacting with the local community.²⁵

Measuring Ethnic Concentration I measure ethnic concentration by the share of natives among people of similar ages. I define the latter as children being two years younger up to three years older. I focus on children and teenagers of similar ages to proxy for asylum seekers' social environment and potential friends.²⁶ It is not straightforward to define what peer group to look at. It is not clear ex-ante how young immigrants define the frontier between in- and out-group. Natives always correspond to an out-group and

²⁴The interested reader can find the exact algorithm used to define the sample in Appendix A.2. Results are robust to altering the conditions listed to be in the sample, as discussed in Appendix A.3.

²⁵In Table C3, I calculate the number of asylum seekers still residing in the Netherlands 1 to 8 years after assignment, and I compute how many live in the same neighborhood (or one adjacent to it) and in the same municipality. After 3 years, 30.7% of the assigned girls are living in the same municipality, and 23.3% in the same neighborhood. As time passes, fewer asylum seekers remain at their assignment location. An honest description of exposure length is that a substantial share (at least a quarter) of young asylum seekers spent an important part of their childhood in a particular environment because of their assignment by the COA.

²⁶When assessing the effects of neighborhoods, I focus on women who migrated at the latest in 2012 (last year of COA data). Therefore, I look at women who turned 16 in 2006 up until those who turned 20 in 2019. They were born at the earliest in 1990 and at the latest in 1999. I cannot use measures of behaviors (other women taking hormonal contraceptives) as the main explanatory variable. It would restrict my sample to those who migrated between 2006 and 2012 (since the data on drugs are only available from 2006). This is why I use ethnic concentration instead of contraceptive usage.

Table 4: Descriptive Statistics - Asylum Seekers' Strategy

Household Head $n = 4,944$		Young Women $n = 6,341$	
Age at Arrival		Age at Arrival	
25 th percentile	31.99	Median	6.99
Median	36.33	Mean	7.44
75 th percentile	41.57		
Male		Year of Assignment	
Mean	0.77	Median	2001
Educational Attainment, no. of individuals		Time Spent in COA Accommodation	
Missing Value	754	Median	544
Primary School	1,266	Mean	824.9
Lower Secondary	927		
Upper Secondary	1,123		
College	448		
Above College	426		
Number of young children		No. of people in COA Accommodation	
At arrival	2.55	Median	109
At age16	1.41	Mean	141.40
Geographical Distribution no. of neighborhoods		Share of Natives at arrival	
At arrival	437	25 th percentile	69.94
At age16	1,636	Median	81.99
		75 th percentile	88.92
		Mean	74.31
		Share of Natives at age 16	
		25 th percentile	61.75
		Median	78.80
		75 th percentile	87.41
		Mean	72.30

Notes: This table reports descriptive statistics of household heads (Panel A) and of the young women (Panel B) followed in the baseline sample. Panel A reports the age at migration, the education level (by categories) and the number of children (who are younger than 16) of the household head. It also reports the number of neighborhoods where they are registered (both at assignment and when the eldest girl turns 16). Panel B reports the information on the age at arrival and the year of assignment. Education categories in Panel A correspond to those reported in Table A2. It reports characteristics of COA accommodation and neighborhoods where young women stayed, the number of asylum seekers registered at the COA at the time of arrival, time spent at that location and the share of natives at arrival and when the girl turns 16.

are therefore a more neutral choice.²⁷ I use the granular nature of the data and focus on close neighborhoods (zip4).

Variation in Neighborhood Characteristics at Assignment Table 4 reports summary statistics about the characteristics of COA accommodation (Panel A), namely, the number of asylum seekers living there and the time they spend in these locations. The median length of stay is roughly one-and-a-half years while the average is more than two years.

Panel B gives summary information on the share of natives both at the time of assignment and when the daughter reaches 16. This table shows two important things: first there is variation in the share of natives at the time of arrival (variation that will be used for identification). Second, although the share of natives is larger at arrival than at the age of 16 (something which indirectly indicates that there is sorting into neighborhoods after assignment), there is a substantial overlap between the distributions of concentration at age 16 and at assignment. Identification of neighborhood effects exploits a variation similar in magnitude to what is observed outside the experimental setting.

4.3 Balancing Tests

The identifying assumption is that conditional on the variables used to allocate asylum seekers to their accommodation, the share of natives in the neighborhood is exogenous. This means that an individual with given characteristics could be sent to different types of neighborhoods. As explained above, the exact assignment algorithm is not known. Therefore, I rely on the main observable variables to proxy for the characteristics entering the allocation decision.

To test the exogeneity of assignment, I regress the share of natives at the time of assignment on characteristics of the household head. They include the gender, age at migration, number of children below 16 on arrival, and dummy variables for education level (bearing in mind that this information is available for 85% of household heads in the

²⁷Defining a relevant peer group runs the risk of circular reasoning: the definition of treatment is related to the results of the analysis. There is a tautology between which group is potentially influential (the treatment) and whether that group has an effect (the result). This problem is particularly acute for immigrants who arrive at a young age. While their parents have only been socialized in the country of origin, and a natural guess is that they are more inclined to interact with countrymen, it is not clear *ex ante* that their children identify as strongly as their parents with their country of origin. Fouka and Tabellini (2022) has shown that the in-group/out-group boundary could switch over time. In particular, young immigrants can identify as being immigrants – versus a native – or can identify with a religious group. Drouhot and Nee (2019) identify religion to be the prominent factor limiting assimilation in Europe.

sample). I stress information on education, since a first-order concern is that household heads are allocated to places more favorable to their employment prospect (something that could correlate with ethnic composition) or that more educated individuals are able to “game the system” and obtain an allocation they deem more suitable. I estimate the following equation:

$$\text{sh natives}_{i,t,h} = \alpha + \beta X_i + \lambda_c + \theta_t + \epsilon_{i,t,h} \quad (1)$$

where $\text{sh natives}_{i,t,h}$ is the share of natives (of similar age) in neighborhood h and year t where individual i was assigned. θ_t are year of arrival fixed effects and λ_c are country of origin fixed effects. The standard errors are clustered at the neighborhood level. In Table 5, I report results with and without origin country fixed effects to check that allocation is not made along nationalities. I also report the F-test for all household head characteristics, together with the F-test associated with the educational categories.

It is clear from Table 5 that neighborhood composition is not correlated with observable characteristics. The null hypothesis, that all reported coefficients are equal to zero, is not rejected at standard levels (corresponding p-values of 0.55 and 0.44). Individual coefficients are also insignificant, only one out of sixteen is marginally significant. In particular, more educated asylum seekers are not more likely to live in a neighborhood with a higher share of natives. There is no evidence of sorting at assignment. In Section C.4, I (i) provide additional evidence on the power of this test (ability to detect sorting into neighborhoods) and (ii) show that a different statistical test (based on randomly reshuffling asylum seekers to different locations) provides the same conclusion.

5 Empirical Analysis

5.1 Specification

I estimate the following equation:

$$y_i = \alpha + \beta \text{sh native}_{i,h,t} + \pi X_i + \lambda_c + \theta_t + \epsilon_{i,t} \quad (2)$$

where $\text{sh native}_{i,h,t}$ is the share of natives (of similar age) to whom individual i in neighborhood h is exposed at the beginning of year t . X_i are individual and family controls. Individual controls include age at migration (risen to power one to three), and family controls include the education, gender and age at migration of the household head (the latter also raised at power one to three). θ_t are year of arrival fixed effect and λ_c are country of origin fixed effects. I focus on two binary outcome variable y_i : having taken contraceptives at least once by the age of 20 (contraception outcome) and being married

Table 5: Balancing Table - Asylum Seekers' Strategy

Male	1.656*	1.266
	(0.985)	(0.997)
Nb of children	0.375	0.219
	(0.281)	(0.284)
Age at Migration	-0.001	0.036
	(0.066)	(0.050)
Primary School	0.222	0.082
	(1.109)	(1.072)
Lower Secondary	0.719	1.481
	(0.856)	(0.955)
Upper Secondary	0.316	0.797
	(0.866)	(0.951)
College	0.739	1.157
	(1.041)	(1.067)
Above College	0.432	0.171
	(1.387)	(1.374)
N Obs	4,944	4,944
R squared	0.06	0.15
Mean	74.62	74.62
F Test - All above	0.89	0.99
P Value - All above	0.53	0.44
F Test - Education	0.19	0.70
P Value - Education	0.97	0.63
Assignment Year	YES	YES
Origin Country	NO	YES

Notes: This table estimates equation 1 on the sample of household heads (of women from the experimental population with complete spells). The outcome is ethnic concentration measured as the share of natives of similar ages (-2 to +3 years old) the year of assignment by COA. Explanatory variables include fixed effects for the country of origin and assignment year, together with the gender of the head, the age of the household head, the number of children below 16, and dummies for educational attainment of the head, where the baseline category is lacking observation (15% of the sample). The head is identified as the father if he arrived at the same time as his daughter; otherwise, it is the mother. I report specifications with and without origin country fixed effects. Standard errors are clustered at the neighborhood level.

to a native (marriage outcome).

As an important contribution of the paper is to introduce the contraception outcome, I define the baseline sample as women whose contraceptive usage can be observed between the ages of 16 and 20, see subsection 2.5. For comparability, I keep the same sample for the marriage outcome. I report an Intention to Treat (ITT) where I use a neighborhood’s share of natives at the time of assignment.²⁸ The standard errors are clustered at the neighborhood level.

5.2 Baseline Estimates

The results are reported in Table 6. Column 1 includes individual controls, and column 2 adds family controls. Column 3 adds neighborhood characteristics: for contraception, the predicted share of contraceptive usage among women aged 16 to 20 in the municipality; for marriage, the “status score”, an index quantifying the “quality” of the neighborhood.²⁹

In empirical applications, there is often no clear guideline to select control variables. Belloni et al. (2014b,a) have developed a Double LASSO procedure to do so. In the fourth column, I use their algorithm to select control variables from the set used in the third one (with categorical values included as separate dummies in the algorithm). This is my preferred specification as it is the least subject to specification searching.³⁰ In the absence of detailed information on the assignment algorithm, an agnostic approach to select controls also seems appropriate.

For both the contraception and marriage outcomes, the ITT results are very small in magnitude and not statistically significant.

²⁸In Table D1, I report a naive estimation where I use the share of natives at age 16 as if it was exogenous. I also use control variables measured at age 16. Comparing causal and naive estimations helps to understand the direction of the bias and the public perception on ethnic enclaves. When looking at naive regressions, there is a positive association between a large share of natives and the contraception outcome. Compared to the null effect for contraception, this is indicative that sorting into neighborhoods biases the estimates upwards. More conservative immigrants tend to live in ethnic enclaves. This is also in line with the public perception that ethnic concentration slows down cultural assimilation. The naive estimation is not statistically significant for the marriage outcome. In order to use the same specification for both outcomes, I rely on information on share of natives at age 16, when contraception starts being observed. This may not be the relevant age for marriage. I report the results for completeness.

²⁹Every four years, the Netherlands Institute for Social Research produces a ranking of all the four-digit zip-code areas based on the average income in a neighborhood, the percentage of people with a low income, the percentage of poorly-educated people, and the percentage of people who do not work. These characteristics are summarized in one composite characteristic: the *status score*. To compute scores for years without one, I use a linear projection.

³⁰In Table 6 and subsequent tables, I use the tuning parameters set in Belloni et al. (2014a). The variables selected by LASSO are not reported for ease of presentation and are available upon request.

Table 6: Baseline Results - Asylum Seekers' Strategy

	Contraception			
Share Natives	0.0001 (0.00030)	0.0001 (0.00030)	-0.0000 (0.00034)	-0.0002 (0.00032)
N Obs	6,341	6,341	6,341	6,341
Mean Outcome	0.41	0.41	0.41	0.41
R squared	0.063	0.068	0.068	0.032
	Marriage			
Share Natives	0.0006 (0.00054)	0.0005 (0.00054)	0.0005 (0.00055)	0.0004 (0.00061)
N Obs	716	716	716	716
Mean Outcome	0.13	0.13	0.13	0.13
R squared	0.175	0.208	0.208	0.025
Origin FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Age at Migration	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Neighborhood Characteristics	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

Notes: This table reports estimations of equation 2. The outcome variables are a dummy for having taken contraceptives at least once by the age of 20 and one for being married to a native. The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. All specifications control for country of origin and assignment year fixed effects together with age at migration (specification reported in the first column). Additional controls are added successively. The second column adds household head characteristics, and the third adds neighborhood characteristics, i.e. the predicted share of teenagers using contraceptives at the municipality level for the contraception outcome and the “statusscore”, see footnote 29 for the marriage outcome. The fourth column uses a subset of the controls used in the third one, i.e. those selected according to the Double Debiased procedure (Belloni et al., 2014b). The sample only includes teenage women with a complete spell. Standard errors are clustered at the neighborhood level.

5.3 Heterogeneity

I use specific techniques, generalized random forests, or GRF (Athey et al., 2019), to account for multiple hypothesis testing when looking at treatment effect heterogeneity.³¹

There are no evidence of heterogeneous effects for contraception.³³ In Table 7, I show Conditional Treatment Effect (CTE) estimated with GRF and OLS for the marriage outcome on the baseline sample and a subsample of young women who migrated before the age of 10. This dimension of heterogeneity is interesting to understand the mechanisms behind a neighborhood effect. While exposure during adolescence and early adulthood change the pool of possible sexual partners, being exposed at a young age does not modify the supply of potential boyfriends. Observing an effect below the age of 10 is indicative that childhood is a critical period where one develops gender norms and when the distinction between in- and out-group is still malleable.

When comparing estimates from OLS and GRF, it appears that OLS coefficients are smaller for the baseline effect. Note that the conditioning differs between the two methods; the control variables used for OLS are different from the variables which enter the GRF algorithm. This is because GRF is not suited for categorical variables (most importantly origin country fixed effects). The results are however qualitatively similar.

To sum up the results from the asylum seekers' strategy. There is a uniform absence of effect for contraception and a modest one, in absolute value, on girls who arrived before 10 for the marriage outcome. Put together, these estimates indicate that there is a positive, albeit small, effect of ethnic enclaves. The effect is not driven by larger pool of potential sexual partners but more likely through changes in preferences and attitudes shaped during childhood.

³¹GRF is a fully non-parametric method that resembles locally weighted maximum likelihood. Instead of using kernel weights in the objective function, it uses those provided by random forests. Observations that more often fall in the same leaf are given more weight. This allows to overcome the curse of dimensionality, which is common in non-parametric estimations. The data-splitting is honest (Athey and Imbens, 2016), in the sense that different subsamples are used to select nodes in the causal trees and estimate conditional expectations. This ensures that inference on treatment effect heterogeneity is not driven by idiosyncrasies in the groups selected to study heterogeneity.

³²The variables which enter the GRF algorithm are the same as the controls used in equation 2 but do not include categorical variables (most importantly origin country). GRF is ill-suited to work with non-ordinal variables. I separate the baseline sample into a training subsample (80%) on which I perform the estimation and a test subsample that I use to predict the treatment effect.

³³Figure D1 plots the cumulated density of predicted treatment effect on the test subsample. The distribution is centered around zero with roughly half of the cumulated density to the left and the right of zero. Predicted treatment is also very small in absolute on the entire distribution.

Table 7: Asylum Seekers' Strategy - Marriage - Heterogeneous Effects

	Entire Sample	Age Migration <10
	OLS	
Share of Natives	0.0004 (0.00061)	0.0017*** (0.00059)
N Obs	716	549
Mean Outcome	0.13	0.13
R squared	0.025	0.019
Origin FE	YES	YES
Year of Assignment FE	YES	YES
Age at Migration	YES	YES
Family Controls	YES	YES
Status Score	YES	YES
LASSO Selection	YES	YES
	GRF	
Share of Natives	0.00120* (0.00067)	0.00185*** (0.00067)
N Obs	716	549

Notes: This table reports the conditional average partial effects estimated using OLS and Generalized Random Forests (Athey et al., 2019). The outcome variable is being married to a native. The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. The following variables are used to build causal forests: year of assignment, age at migration, household head characteristics (including education) and *stausscore*, see footnote 29. Controls in the OLS regressions are chosen according to the algorithm by Belloni et al. (2014b) among the variables which enter the GRF algorithm and country of origin fixed effects. The sample is limited to married women. OLS standard errors are clustered at the neighborhood level.

5.4 Robustness Checks

The robustness checks differ for each outcome, depending on the result.

5.4.1 Usage of Hormonal Contraceptives

Using Duration Models The relevant outcome may not be having used contraceptives by the age of 20, but starting to use them at a younger age. To address this concern, I estimate a duration model rather than linear regressions. In Table D3, I reproduce the baseline analysis but use an exponential MLE. The results confirm those of the linear specifications.

Non-linearity in community size I reproduce Table 6 but include a quadratic term. These results are reported in Table D4 and do not point to a non-linear effect.

5.4.2 Marriage with a Native

Restricting the sample to married individuals Results on marriage hold when the unmarried women are kept in the sample, see Table D5.

5.5 External Validity

The question of external validity is twofold: (i) Do the results carry over to the general population of (first and second generation) immigrants? and (ii) Are the results (from the asylum seekers' strategy) driven by selection into treatment? The latter arises if asylum seekers are absent from the baseline sample in a non-random fashion. There are two main reasons why they would not appear in the baseline sample: either they did not live in a collective COA accommodation (their address is listed at the COA, but it does not qualify to be considered exogenously assigned), or they were staying in a collective accommodation administered by the COA but not long enough to appear in municipal registries (remember that the rule to be registered is having been there for six months after 2000, and a year before to 2000, see footnote 23).

Section 6 addresses (i). To assess (ii), I do several checks, reported in Appendix A.4. In particular, I check that most asylum seekers (the potentially eligible population) can be found through their registered addresses using the baseline criteria. This is the case for 73% of them, which is a very large number. I also check that observable characteristics are similar between the experimental and the other potentially eligible (but non-experimental) populations. The differences between the two groups, albeit statistically significant, are very small in magnitude.

5.5.1 Alternative Samples

I check four alternative samples to make sure that the results are not driven by potentially arbitrary decisions made at the data stage. In particular, I look at the following four departures from the baseline sample: (i) *incomplete spells*, where I include young women being observed from at least age 18 (so including teenagers starting to be observed at ages 17 and 18, see subsection 2.5), (ii) *first assigned*, where I focus on first assignment in case an asylum seeker has been registered in two COA accommodations (instead of focusing on the longest spell), (iii) *variation 1*, in which I relax the baseline criteria to include people living in self-arranged accommodation, which was referred to as living “with friends or relatives”, and (iv) *variation 2*, in which criteria to be assigned are relaxed, self-arranged accommodation is still excluded; but if an address is associated with different types of accommodation, I keep it in the sample as long as one of them correspond to collective housing (as compared to the baseline sample where all of them had to). The algorithms are detailed in Section A.2. Further discussion (and comparison between samples) can be found in Appendix A.3.

Samples are very similar regardless of the choices made when constructing the baseline dataset. The incomplete spell sample is substantially larger (+20%). Results are robust to these variations (see Table D6).³⁴

6 Sibling variation in average exposure

In this section, I use another identification strategy to estimate the effects of ethnic enclaves on cultural assimilation. Instead of relying on the quasi-random allocation of asylum seekers to neighborhoods, I use within family variation (comparison between sisters) in exposure to natives. This approach is not limited to asylum seekers; it includes all first- and second-generation non-Western immigrants. This strategy looks at a different type of treatment than in the asylum seekers’ strategy. Instead of focusing on variation in exposure at one point in time, I look at the cumulated exposure to natives during childhood and adolescence.

6.1 Identification Strategy and Balancing Tests

Consider the following equation

³⁴For contraception, results are omitted for ease of presentation but also do not show evidence of an effect. They can be provided upon request.

$$y_i = \alpha + \beta \overline{\text{sh natives}_i} + X_i + \epsilon_i \quad (3)$$

where $\overline{\text{sh natives}_i} = \frac{1}{13} \beta \sum_{j=4}^{16} (\text{sh natives}_{i,j})$ is the average share of natives (of similar ages) individual i is exposed to between the ages of 4 to 16 years old (index j).³⁵ There is an endogeneity problem if $\mathbb{E}[\epsilon_i | X_i] \neq 0$ which can occur because of omitted variable. The identifying assumption on which is based the sibling strategy is that controlling for family effects solves this problem. It implies that sorting into neighborhoods is time-invariant within families. This motivates estimating the equation below:

$$y_{i(f)} = \alpha + \lambda_f + \beta \overline{\text{sh natives}_{i(f)}} + \epsilon_{i(f)} \quad (4)$$

where λ_f is a family fixed-effect and $i(f)$ emphasizes that individual i belongs to family f .

Identification comes from variation in ethnic concentration experienced by sisters. Given that they are not the same age in the same year, they will necessarily experience variation in neighborhood characteristics. I exploit variation coming both from families moving to a new neighborhood and families staying in a neighborhood where the share of natives changes.³⁶

Table 8: Balancing Test - Sibling Strategy

	F test	Nb covariates	Nb significant	Nb obs
Share Natives	86.33	98	17	31,622
Residuals	0.40	98	1	31,622

Notes: This table reports results from regressing the average share of natives of similar ages on individual characteristics, namely origin country, mother and father education and the number of children in the family. The table reports the number of covariates, the F-test of the null hypothesis all variables are equal to zero and the number of coefficients which are individually significant at the 5% threshold. It reports results from using share of natives and residualized shares (from a separate regression of share of natives on family fixed effects). Standard errors are clustered at the family level.

To provide statistical evidence supporting this identification strategy, I do the following test. In an equation with family fixed effects, the variation that is ultimately used to estimate β is the part of $\overline{\text{sh natives}_i}$ that is not explained by λ_f . Under the identifying assumption, this residual variation should be orthogonal to individual characteristics. To

³⁵Given that similar age is defined as children two years younger, I cannot calculate $\text{sh natives}_{i,j}$ for children younger than 2. I chose four years old as the starting age to increase the sample size.

³⁶I depart from Chetty and Hendren (2018a). In particular, focusing only on movers would greatly decrease my sample size.

test this hypothesis, I proceed as follows: first I regress the average share of natives on family fixed effects and predict the residuals. Then I regress these residuals on individual characteristics, namely origin country fixed effects, mother and father education and the number of siblings in a family. I perform a F-test which I report in Table 8.

The F-test for this residualized regression is very small and not statistically significant. I also report the number of coefficients whose individual significance test is above the 5% threshold. To show the power of this test, I report the same information when the actual share of natives (rather than the residualized one) is used as an outcome. In this case, tests of individual and joint significance show a strong link between individual characteristics and neighborhood environment. P-values are very small for the regression of actual share ($p < 0.01$) and very large when using residuals ($p > 0.99$). I take it as validating evidence of the sibling strategy.

6.2 Baseline Results

6.2.1 Sample definitions and characteristics

Table 9 reports information on the treatment variable, together with descriptive statistics on families. These include the age difference between siblings, the difference between siblings in exposure to natives, educational attainment of the mother and how many families have all siblings using contraception. Table E1 reports the distribution of the main origin countries.

Table 9 shows that immigrants in the sibling strategy more often live in ethnic enclaves than those followed in the asylum seekers' strategy. The average mean share of natives is 55% which is much lower than the 74% reported in Table 4. Siblings are relatively close in age with a mean difference of three and a half years old. There is variation in exposure to natives between siblings, the average difference is around 4 percentage points. Last but not least there is variation in the outcome variables, roughly a third of families have at least one sister using contraception and another not.

6.2.2 Contraceptive Usage

I estimate equation 4 and report the results in Table 10. I also look at the effect of average exposure for different age bounds, namely before and after 10.³⁷

³⁷I do not report results for the marriage outcome in the main text. The sibling strategy requires two sisters from the baseline sample (used in Table 10) to be married, which dramatically reduce the sample size. It is not possible to reach a conclusion with this limited sample size. See Table E5.

Table 9: Descriptive Statistics - Sibling Strategy

Panel A: Characteristics of the Families		Panel B: (Average) Share of Natives	
Age difference between siblings		Treatment Intensity	
25 th percentile	2	25 th percentile	31.81
Median	3.17	Median	60.67
75 th percentile	4.75	75 th percentile	78.33
Mean	3.44	Mean	55.06
Difference in exposure between siblings			
25 th percentile	0.93		
Median	2.41		
75 th percentile	5.30		
Mean	4.09		
Educational Attainment of the Mother			
Missing Value	3,941		
Primary School	4,302		
Lower Secondary	2,674		
Upper Secondary	2,449		
College	816		
Above College	575		
Difference in behavior between siblings			
Both	2,996		
None	6,993		
Diff	4,768		
N families	14,757	N Obs	31,622

Notes: This table reports descriptive statistics of young women followed in the sibling strategy. Panel A reports information on the difference in age between the oldest and youngest sibling in families, the difference in exposure to natives, the difference in outcomes between siblings and the education level of the mother. Categories in Panel A correspond to those reported in Table A2. Panel B details the treatment variable.

Table 10: Sibling Strategy - Baseline Results

	All ages	4 to 10	11 to 16
Share Natives	0.0017** (0.00072)	0.0014*** (0.00051)	0.0002 (0.00062)
N Obs	31,622	31,622	31,622
Mean Outcome	0.36	0.36	0.36
R squared	0.650	0.650	0.650

Notes: This table reports coefficients from estimating equation 4. The main outcome is having used hormonal contraceptives at least once by the age of 20 years old. The explanatory variables are family fixed effects and average share of natives one is exposed to during childhood and adolescence. The first column averages exposure experienced over all the ages from 4 to 16 while the second and third columns limit the averaging to age brackets: 4 to 10 years old and 11 to 16 years old. Standard errors are clustered at the family level.

Results cannot directly be compared to those in Table 6 since a one percentage point increase in the sibling strategy correspond to an increase in exposure of one percent over 13 years. An increase of 10 percentage points over 13 years increases the probability to behave like a native by 1.74 p.p. Compared to the asylum seekers' strategy, results show evidence of a statistically significant neighborhood effect. Beyond statistical significance, results are small in magnitude. The difference in the definition of treatment between the two strategies help understand these results. Prolonged contact, as measure by cumulative exposure, is more likely to lead to friendships and generate peer effects.

Confirming the picture from the asylum seekers' strategy, the effect is concentrated on exposure at an early age, see column 2 of Table 10. This is additional evidence that results are not driven by the availability of sexual partners but rather by changes in attitudes at critical ages.

6.3 Robustness checks

Families with more children correspond to more observations in this strategy. To make sure that this composition effect does not drive the results, I re-run previous regressions but weight them so that each family has the same importance. Results can be found in Tables E2. Results are also robust to including birth order fixed effects, see Table E4.

7 Robustness Checks of the Experimental Design

I perform three general robustness checks. First, I provide further evidence that the asylum seekers' strategy has power. Then, I provide evidence that the seemingly different results between the two identification strategies come from the fact that they focus on a

different type of treatment; cumulated exposure vs measured at one point in time. Lastly, I look at the effect of ethnic enclaves on boys.

7.1 Power of the asylum seekers strategy

In Section 5, I showed that the asylum seekers had an effect on an important subsample, women who arrived before the age of 10. I want to provide additional evidence supporting the power of the experiment. To do so, I take another outcome more commonly used in the neighborhood literature, i.e; educational attainment. I focus on the same sample that produced the baseline results, i.e., Table 6.

The Education System in the Netherlands The Dutch education system is characterized by the existence of several high school tracks that students can access after primary school (at the age of 12). I create a categorical value based on CBS categorization of the highest level attended, see Table A2.³⁸

Results Table D7 replicates the ITT baseline analysis where I use an ordered probit model to account for the categorical nature of the outcome variable. The effect is positive and statistically significant on the baseline sample. In addition to showing the power of the asylum seekers' strategy, it is an interesting result that one should consider when assessing the overall effect of growing-up in an ethnic enclave.

7.2 Effect on boys

I reproduce the analysis from the asylum seekers' strategy on boys. I select them on the same criteria of presence in the Netherlands and COA allocation as I did for girls. The baseline effects are reported in Table F1 and show a positive effect of exposure to natives on the marriage outcome.

7.3 Coherence between the two strategies

The two identification strategies rely on two different treatments, one is cumulative (since averaged over many years), the other is not. Although the two strategies provide qualitatively similar results, the sibling strategy gives a statistically significant result for the

³⁸I focus on the attended rather than the completed level to account for the fact that asylum seekers may have started school later. Therefore, by the age of 20, they may have started a level, but not finished it yet.

contraception outcome when the asylum seekers' strategy does not. To provide evidence that this discrepancy is driven by differences in the types of treatments and not by other aspects of these empirical strategies, I perform the following check. I look at the samples used in Tables 10 but instead of averaging exposure, I select randomly one year (either through all ages or from 4 to 10 or 11 to 16) and use that value to characterize exposure to natives. In coherence with results from the asylum seekers' strategy, I do not find an effect on the contraception outcome. Results are shown in tables E3.

8 Conclusion

This paper studies whether growing-up in an ethnic enclave slows down immigrants' cultural assimilation. My main outcome to measuring cultural behavior is the use of hormonal contraceptives by teenage women. Using survey data (merged on an individual identifier with administrative registries), I show that it is a good proxy for adopting Western views on sexuality, and potentially a good indicator for being sexually active.

To provide causal evidence on neighborhood ethnic composition, I use two identification strategies. The first one exploits the placement of asylum seekers in the Netherlands and the second one uses variation in neighborhood characteristics experienced by siblings. The former relies on variation at one point in time, when asylum seekers are quasi-randomly allocated in the country, while the latter uses cumulated variation in childhood and adolescence. I find evidence that ethnic enclaves have a negative effect on educational attainment and cultural assimilation. The latter effect is modest when one compares its magnitude to the difference in behaviors between natives and immigrants. It is concentrated on exposure at an early age, before 10 years old.

References

- Abramitzky, Ran, Leah Boustan, and Dylan Connor**, “Leaving the Enclave: Historical Evidence on Immigrant Mobility from the Industrial Removal Office,” NBER Working Papers 27372 June 2020.
- , – , and **Katherine Eriksson**, “Do Immigrants Assimilate More Slowly Today Than in the Past?,” *American Economic Review: Insights*, March 2020, *2* (1), 125–41.
- Adida, Claire L., David D. Laitin, and Marie-Anne Valfort**, “Muslims in France: Identifying a Discriminatory Equilibrium,” *Journal of Population Economics*, 2014, *27* (4), 1039–1086.
- , – , and – , “Women, Muslim Immigrants, and Economic Integration in France,” *Economics & Politics*, March 2014, *26* (1), 79–95.
- Alan, Sule, Ceren Baysan, Mert Gumren, and Elif Kubilay**, “Building Social Cohesion in Ethnically Mixed Schools: An Intervention on Perspective Taking,” *The Quarterly Journal of Economics*, 03 2021, *136* (4), 2147–2194.
- Alesina, Alberto and Marco Tabellini**, “The Political Effects of Immigration: Culture or Economics?,” *Journal of Economic Literature*, 2020. Forthcoming.
- , **Paola Giuliano, and Nathan Nunn**, “On the Origins of Gender Roles: Women and the Plough,” *The Quarterly Journal of Economics*, May 2013, *128* (2), 469–530.
- Algan, Yann, Alberto Bisin, Allan Manning, and Thierry Verdier**, “Cultural Integration of Immigrants in Europe,” Post-Print hal-01496731, HAL January 2013.
- , **Christian Dustmann, Albrecht Glitz, and Alan Manning**, “The Economic Situation of First and Second-Generation Immigrants in France, Germany and the United Kingdom,” *The Economic Journal*, February 2010, *120* (542), F4–F30.
- , **Clément Malgouyres, Thierry Mayer, and Mathias Thoenig**, “The Economic Incentives of Cultural Transmission: Spatial Evidence from Naming Patterns Across France,” *The Economic Journal*, February 2022, *132* (642), 437–470.
- Almond, Douglas, Janet Currie, and Valentina Duque**, “Childhood Circumstances and Adult Outcomes: Act II,” *Journal of Economic Literature*, December 2018, *56* (4), 1360–1446.
- Ammermueller, Andreas and Jorn-Steffen Pischke**, “Peer Effects in European Primary Schools: Evidence from the Progress in International Reading Literacy Study,” *Journal of Labor Economics*, July 2009, *27* (3), 315–348.

- Aslund, Olof, Per-Anders Edin, Peter Fredriksson, and Hans Groenqvist**, “Peers, Neighborhoods, and Immigrant Student Achievement: Evidence from a Placement Policy,” *American Economic Journal: Applied Economics*, April 2011, 3 (2), 67–95.
- , **Peter Fredriksson, and Per-Anders Edin**, “Ethnic Enclaves and the Economic Success of Immigrants? Evidence from a Natural Experiment,” *The Quarterly Journal of Economics*, 2003, 118 (1), 329–357.
- Athey, Susan and Guido Imbens**, “Recursive Partitioning for Heterogeneous Causal Effects,” *Proceedings of the National Academy of Science*, July 2016, 113 (27), 7353–7360.
- , **Julie Tibshirani, and Stefan Wager**, “Generalized Random Forests,” *Annals of Statistics*, 2019, 47 (2), 1148–1178.
- Atkin, David, Eve Colson-Sihra, and Moses Shayo**, “How Do We Choose Our Identity? A Revealed Preference Approach Using Food Consumption,” *Journal of Political Economy*, 2021, 129 (4), 1193–1251.
- Bailey, Martha J.**, “More Power to the Pill: The Impact of Contraceptive Freedom on Women’s Life Cycle Labor Supply,” *The Quarterly Journal of Economics*, 2006, 121 (1), 289–320.
- Bansak, Kirk, Jens Hainmueller, and Dominik Hangartner**, “How Economic, Humanitarian, and Religious Concerns shape European Attitudes toward Asylum Seekers,” *Science*, October 2016, 354 (6309), 217–222.
- Barham, Tania, Karen Macours, and John A. Maluccio**, “Boys’ Cognitive Skill Formation and Physical Growth: Long-Term Experimental Evidence on Critical Ages for Early Childhood Interventions,” *American Economic Review*, 2013, 103 (3), 467–471.
- Bazzi, Samuel, Arya Gaduh, Alexander D. Rothenberg, and Maisy Wong**, “Unity in Diversity? How Intergroup Contact Can Foster Nation Building,” *American Economic Review*, November 2019, 109 (11), 3978–4025.
- Beckers, Pascal and Lex Borghans**, “Segregation in Neighbourhoods and Labour Market Outcomes of Immigrants: Evidence from Random Assignment in the Netherlands,” MERIT Working Papers 068 2011.

- Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen**, “High-Dimensional Methods and Inference on Structural and Treatment Effects,” *Journal of Economic Perspectives*, 2014, 28 (2), 29–50.
- , – , **and** – , “Inference on Treatment Effects after Selection among High-Dimensional Controls,” *The Review of Economic Studies*, 2014, 81 (2), 608–650.
- Bertrand, Marianne, Erzo F. P. Luttmer, and Sendhil Mullainathan**, “Network Effects and Welfare Cultures,” *The Quarterly Journal of Economics*, 2000, 115 (3), 1019–1055.
- Billings, Stephen B, Mark Hoekstra, and Gabriel Pons Rotger**, “The Scale and Nature of Neighborhood Effects on Children: Evidence from a Danish Social Housing Experiment,” NBER Working Papers 30764 December 2022.
- Bisin, Alberto and Thierry Verdier**, “Beyond the Melting Pot: Cultural Transmission, Marriage, and the Evolution of Ethnic and Religious Traits,” *The Quarterly Journal of Economics*, April 2000, 115 (3), 955–988.
- **and** – , “The Economics of Cultural Transmission and Socialization,” in Alberto Bisin Jess Ben-habib and Matt Jackson, eds., *Handbook of Social Economics*, Vol. 1A of *Handbook of Social Economics*, Elsevier, November 2010.
- , **Eleonora Patacchini, Thierry Verdier, and Yves Zenou**, “Are Muslim Immigrants Different in Terms of Cultural Integration?,” *Journal of the European Economic Association*, April 2008, 6 (2-3), 445–456.
- , – , – , **and** – , “Bend it like Beckham: Ethnic Identity and Integration,” *European Economic Review*, 2016, 90 (C), 146–164.
- , **Giorgio Topa, and Thierry Verdier**, “Religious Inter-marriage and Socialization in the United States,” *Journal of Political Economy*, June 2004, 112 (3), 615–664.
- Boucher, Vincent, Semih Tumen, Michael Vlassopoulos, Jackline Wahba, and Yves Zenou**, “Ethnic Mixing in Early Childhood: Evidence from a Randomized Field Experiment and a Structural Model,” IZA Discussion Papers 14260 April 2021.
- Bratsberg, Bernt, Jeremy Ferwerda, Henning Finseraas, and Andreas Kot-sadam**, “How Settlement Locations and Local Networks Influence Immigrant Political Integration,” *American Journal of Political Science*, 2021, 65 (3), 551–565.

- Cappelen, Alexander, John List, Anya Samek, and Bertil Tungodden**, “The Effect of Early-Childhood Education on Social Preferences,” *Journal of Political Economy*, July 2020, *128* (7), 2739–2758.
- Carrell, Scott E., Mark Hoekstra, and James E. West**, “The Impact of College Diversity on Behavior toward Minorities,” *American Economic Journal: Economic Policy*, November 2019, *11* (4), 159–182.
- Casey, Teresa and Christian Dustmann**, “Immigrants’ Identity, Economic Outcomes and the Transmission of Identity across Generations,” *The Economic Journal*, 2010, *120* (542), F31–F51.
- Chetty, Raj and Nathaniel Hendren**, “The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects,” *The Quarterly Journal of Economics*, 2018, *133* (3), 1107–1162.
- and – , “The Impacts of Neighborhoods on Intergenerational Mobility II: County-Level Estimates,” *The Quarterly Journal of Economics*, 2018, *133* (3), 1163–1228.
- , – , and **Lawrence F. Katz**, “The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment,” *American Economic Review*, April 2016, *106* (4), 855–902.
- , – , **Frina Lin, Jeremy Majerovitz, and Benjamin Scuderi**, “Childhood Environment and Gender Gaps in Adulthood,” *American Economic Review*, May 2016, *106* (5), 282–288.
- Chyn, Eric and Lawrence F. Katz**, “Neighborhoods Matter: Assessing the Evidence for Place Effects,” *Journal of Economic Perspectives*, November 2021, *35* (4), 197–222.
- Dahl, Gordon B., Christina Felfe, Paul Frijters, and Helmut Rainer**, “Caught between Cultures: Unintended Consequences of Improving Opportunity for Immigrant Girls,” *The Review of Economic Studies*, 2022, *89* (5), 2491–2528.
- Dahlberg, Matz, Karin Edmark, and Heléne Berg**, “Revisiting the Relationship between Ethnic Diversity and Preferences for Redistribution: Reply,” *The Scandinavian Journal of Economics*, April 2017, *119* (2), 288–294.
- , – , and **Heléne Lundqvist**, “Ethnic Diversity and Preferences for Redistribution,” *Journal of Political Economy*, 2012, *120* (1), 41–76.
- Damm, Anna Piil**, “Ethnic Enclaves and Immigrant Labor Market Outcomes: Quasi-Experimental Evidence,” *Journal of Labor Economics*, April 2009, *27* (2), 281–314.

- **and Christian Dustmann**, “Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?,” *American Economic Review*, June 2014, *104* (6), 1806–32.
- Danzer, Alexander M., Carsten Feuerbaum, Marc Piopiunik, and Ludger Woessmann**, “Growing Up in Ethnic Enclaves: Language Proficiency and Educational Attainment of Immigrant Children,” IZA Discussion Papers 11608 June 2018.
- Drouhot, Lucas G. and Victor Nee**, “Assimilation and the Second Generation in Europe and America: Blending and Segregating Social Dynamics Between Immigrants and Natives,” *Annual Review of Sociology*, 2019, *45* (1), 177–199.
- Figlio, David, Paola Giuliano, Umut Özek, and Paola Sapienza**, “Long-Term Orientation and Educational Performance,” *American Economic Journal: Economic Policy*, November 2019, *11* (4), 272–309.
- Finseraas, Henning, Torbjørn Hanson, Åshild A. Johnsen, Andreas Kotsadam, and Gaute Torsvik**, “Trust, Ethnic Diversity, and Personal Contact: A field Experiment,” *Journal of Public Economics*, 2019, *173* (C), 72–84.
- Fouka, Vasiliki and Marco Tabellini**, “Changing In-Group Boundaries: The Effect of Immigration on Race Relations in the United States,” *American Political Science Review*, August 2022, *116* (3), 968–984.
- G., Jr. Fryer Roland**, “Guess Who’s Been Coming to Dinner? Trends in Interracial Marriage over the 20th Century,” *Journal of Economic Perspectives*, June 2007, *21* (2), 71–90.
- Gagliarducci, Stefano and Marco Tabellini**, “Faith and Assimilation: Italian Immigrants in the US,” EIEF Working Papers 2102 2021.
- Giuliano, Paola and Nathan Nunn**, “Understanding Cultural Persistence and Change,” *The Review of Economic Studies*, July 2021, *88* (4), 1541–1581.
- Goldin, Claudia and Lawrence F. Katz**, “The Power of the Pill: Oral Contraceptives and Women’s Career and Marriage Decisions,” *Journal of Political Economy*, August 2002, *110* (4), 730–770.
- Gordon, Milton M.**, *Assimilation in American Life: The Role of Race, Religion, and National Origins*, Oxford, New York: Oxford University Press, June 1964.
- Grönqvist, Hans**, “Ethnic Enclaves and the Attainments of Immigrant Children,” *European Sociological Review*, April 2006, *22* (4), 369–382.

- Harder, Niklas, Lucila Figueroa, Rachel M. Gillum, Dominik Hangartner, David D. Laitin, and Jens Hainmueller**, “Multidimensional Measure of Immigrant Integration,” *Proceedings of the National Academy of Sciences*, 2018, 115 (45), 11483–11488.
- Harding, David J., Lisa Sanbonmatsu, Greg J. Duncan, Lisa A. Gennetian, Lawrence F. Katz, Ronald C. Kessler, Jeffrey R. Kling, Matthew Sciandra, and Jens Ludwig**, “Evaluating Contradictory Experimental and Non-Experimental Estimates of Neighborhood Effects on Economic Outcomes for Adults,” *Housing Policy Debate*, 2021. Forthcoming.
- Jarotschkin, Alexandra and Ekaterina Zhuravskaya**, “Diffusion of Gender Norms: Evidence from Stalin’s Ethnic Deportations,” CEPR Discussion Papers 13865 July 2019.
- Jaschke, Philipp, Sulin Sardoschau, and Marco Tabellini**, “Scared Straight? Threat and Assimilation of Refugees in Germany,” NBER Working Papers 30381 August 2022.
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz**, “Experimental Analysis of Neighborhood Effects,” *Econometrica*, January 2007, 75 (1), 83–119.
- Kohler, Hans-Peter, Jere B. Behrman, and Susan C. Watkins**, “The Density of Social Networks and Fertility Decisions: Evidence from South Nyanza District, Kenya,” *Demography*, February 2001, 38 (1), 43–58.
- Kuran, Timur and William Sandholm**, “Cultural Integration and Its Discontents,” *The Review of Economic Studies*, January 2008, 75 (1), 201–228.
- Merlino, Luca Paolo, Max Friedrich Steinhardt, and Liam Wren-Lewis**, “More than Just Friends? School Peers and Adult Interracial Relationships,” *Journal of Labor Economics*, 2019, 37 (3), 663–713.
- Munshi, Kaivan and Jacques Myaux**, “Social Norms and the Fertility Transition,” *Journal of Development Economics*, June 2006, 80 (1), 1–38.
- Nekby, Lena and Per Pettersson-Lidbom**, “Revisiting the Relationship between Ethnic Diversity and Preferences for Redistribution: Comment,” *The Scandinavian Journal of Economics*, 2017, 119 (2), 268–287.
- Tabellini, Marco**, “Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration,” *The Review of Economic Studies*, 2020, 87 (1), 454–486.

van den Berg, Gerard J., Petter Lundborg, Paul Nystedt, and Dan-Olof Rooth,
“Critical Periods During Childhood and Adolescence,” *Journal of the European Economic Association*, 2014, 12 (6), 1521–1557.

For online publication only

A More details on the data

A.1 Information on data sources

A.1.1 Administrative Registries

The project number (as registered at CBS, Statistics Netherlands) is 8401. The registries used are: GBA Perssonstab, VRL Migmotbus, GBA Adresobject, GBA Verbintenispartnerbus, Kindoudertab, GBA Nationaliteitbus, Hoogsteopltab, Medicijntab,

Information about them can be accessed on the website of CBS

A.1.2 Sex Under the Age of 25

Online Information: Available on the website of Rutgers

Questions Used:

- Before Marriage (variable code D1_1) What do you think of the following situation. Sexual Intercourse before marriage. Modality 1 is very negative, modality 4 very positive. Modalities 1, 2 and 3 are coded as 0, modality 4 as 1.
- Homosexuality (variable code D3_3) What do you think of the following situation. Two guys kissing in the street. Modality 1 is very negative, modality 5 very positive. Modalities 1, 2, 3 and 4 are coded as 0, modality 5 as 1.
- Female Sexuality (variable code D2_5) What do you think of the following situation. A girl who has sex with many different guys. Modality 1 is very negative, modality 5 very positive. Modalities 1 and 2 are coded as 0, modality 3, 4 and 5 are coded as 1.³⁹
- Knowledge Index, Answer to the following questions: (i) The pill reduces the chance that you will contract an STD NO, (ii) If you wash well after sex, you are less likely to contract an STI NO, (iii) You can buy the morning after pill at the drugstore without a doctor's prescription YES, (iv) If you don't have any physical complaints, you can still have an STD YES, (v) There are STDs from which girls can no longer have children YES, (vi) If you take the pill, you won't be able to have children later

³⁹Note that for questions "Before Marriage", "Homosexuality" "Female Sexuality", changing the threshold in the binary variables does not change qualitatively the results, neither their statistical significance.

NO, (vii) A girl always bleeds during the first intercourse NO, (viii) Most STIs go away on their own NO.

- Had Sex (variables code F12A and F12B) Have you ever had sexual intercourse (separate questions for men and women)
- Use Condoms (variables code J15A and J15B) Did you use condoms (separate questions for men and women)
- Modes of contraception (variable Q2) listed in Table B4. This question is asked to young women who reported having had sex (variable above) and who are currently using contraception (variable Q1).

A.1.3 Trajectoires et Origines (Te0)

Online Information: Available on the website of INED

Questions Used:

- Friend - Same Origin (variable code a_rorig) Proportion of friends from the same origin, more than half is coded as 1, half or less than half is coded as 0.
- Identity (variable code x_apparf) Opinion on the following statement: “I identify as French”, i.e. “Je me sens français(e)”. Completely agree is coded as 1, partially agree, partially disagree and completely disagree are coded as 0.
- Importance of Religion (variable code r_impvie) Importance of religion in your life, very important is coded as 1, relatively important, not very important, not important at all are coded as 0.
- Virginity and Abstinence (mentioned in Table B5, variable codes c_nobbpq.e and c_nobbpq.f) specific reasons for not using the contraceptive pill.

A.2 Algorithm to identify the baseline sample (and variations)

1. Limit the population to immigrants not coming from Western Europe, Northern Europe and North America as listed in Table A1.
2. Restrict the attention to the first two addresses in which they were registered.
3. To qualify as assigned according to the baseline, **variation 1** and **variation 2** definitions, someone must live at:

Table A1: Grouping countries into regions

Region	Countries
North Africa	Algeria Egypt Libya Morocco Sudan Tunisia
East Africa	Burundi Comoros Djibouti Eritrea Ethiopia Kenya Madagascar Malawi Mozambique Rwanda Seychelles Somalia Uganda Tanzania Zambia Zimbabwe
Central Africa	Angola Cameroon Central African Republic Chad D.R. Congo Zaire Equatorial Guinea Gabon
South Africa	Botswana Namibia South-Africa
West Africa	Benin Burkina Faso Cape Verde Ivory Cost Gambia Ghana Guinea Guinea-Bissau Liberia Mali Mauritania Niger Nigeria Senegal Sierra Leone Togo
Latin America	Antigua Barbuda Argentina Bolivia Brazil Chili Colombia Cuba Dominican Republic Ecuador El Salvador Guatemala Guyana Haiti Honduras Jamaica Mexico Nicaragua Panama Peru Surinam Trinidad Tobago Uruguay Venezuela USA Canada
North America	
Central Asia	Kazakhstan Kyrgyzstan Turkmenistan Tajikistan Uzbekistan
East Asia	China Korea South-Korea North-Korea Japan Mongolia Taiwan
South-East Asia	Cambodia Indonesia Laos Malaysia Myanmar Philippines Singapore Thailand Vietnam
South Asia	Afghanistan Bangladesh Bhutan India Iran Nepal Pakis Sri Lanka Tibet
Middle East	Armenia Azerbaijan Bahrain Cyprus Georgia Iraq Israel Jordan Kuwait Lebanon Oman Qatar Saudi Arabia Palestine Syria Turkey United Arab Emirates Yemen
Eastern Europe	Belarus Bulgaria Czech Republic Hungary Poland Moldavia Romania Russian Federation Slovakia Ukraine
Northern Europe	Estonia Latvia Lithuania Great-Britain
Southern Europe	Albania Bosnia Herzegovina Croatia Greece Italy Montenegro Portugal Slovenia Spain Yugoslavia Kosovo Macedonia
Western Europe	Austria Belgium France Germany Netherlands Switzerland
Oceania	Australia Samoa

Note : This list corresponds to the sub-continental grouping used by UN Stats, see online

Table A2: Categorization of Educational Attainment

Name	Categories
Basisonderwijs	Primary School
Praktijkonderwijs	Lower Secondary
Vmbo-b/k	Lower Secondary
Mbo1	Lower Secondary
Vmbo-g/t	Lower Secondary
Havo-, vwo-onderbouw	Lower Secondary
Mbo2	Upper Secondary
Mbo3	Upper Secondary
Mbo4	Upper Secondary
Havo-bovenbouw	Upper Secondary
Vwo-bovenbouw	Upper Secondary
Hbo-associate degree	College
Hbo-bachelor	College
Wo-bachelor	College
Hbo-master	Above College
Wo-master	Above College
Doctor	Above College

Notes: Names correspond to entries in CBS data, categories are those taken from the Standaard Onderwijsindeling 2021, Tables 3a-3b. They correspond to the International Standard Classification of Education (ISCE) of 2011.

- (a) An address that was an operating COA accommodation at the time of registration
- (b) (i) Since the same address can be listed several times (as different types of accommodation, see list in Table C2) and (ii) some types of accommodations are “problematic” (because one can not be sure that allocation there is exogenous), I create a variable coding problems. Each address can be a problem several times (i.e. for each type of accommodation it is listed under).
- (c) Problematic cases are Gemeentewoning, Administratief geplaatst and Zelf-Zorg Arrangement (**In variation 1, ZZA is not considered problematic**). I count the number of problems for an address together with the number of accommodation types it is listed under.
- (d) (For baseline and **variation 1**) I do not consider addresses where there is at least one problem.
- (e) (For **variation 2**) I do not consider addresses where there are only problems

For incomplete spells, the algorithm is the same. What changes is the age at which young women start being observed in health registries.

A.3 Alternative Samples

How do samples relate to each other? Table A3 reports the merges between the baseline and each alternative sample. Note that merges are made on individual and neighborhood identifiers. It shows if the same people have been localized at the same place. Samples vary little because of decisions made at the data stage. The most striking element is that including incomplete spells increase the sample size by 20%.

Table A3: Comparing Samples

	Incomplete	First Assignment	Variation 1	Variation 2
Only in Baseline	0	660	73	73
Not at all in Baseline	1,243	683	113	99
In both	6,341	5,681	6,268	6,268

ITT results with Alternative Samples Results of the preferred specification (using the Double Debiased Lasso algorithm of Belloni et al. (2014b)) are reported in Table D6 and confirm the baseline results.

A.4 External Validity

To address external validity concerns, I need to identify asylum seekers who could have been allocated an accommodation by the COA but were not. I do not know this population but use the richness of the data to approximate it. To do so, I take the following steps:

- I focus on immigrants listed as asylum seekers over the period 1996-2012.⁴⁰
- I restrict the population to those who have at least one daughter with an incomplete spell of contraceptive usage.⁴¹
- I restrict the population to Non-Western immigrants or immigrants coming from a country listed in Table C1.
- I keep one observation per potential household head.

⁴⁰This information is taken from a registry dataset collecting motives for migration, see subsection A.1.1. Note that I did not include this information when drawing the baseline sample since it is not fully reliable. If different members of a family of asylum seekers do not arrive together, the ones who arrived later are classified as family migration and not asylum. Despite these limitations, this is a starting point to assess external validity.

⁴¹ I perform this check on the incomplete spell sample to maximize the number of observations with non zero weights in Table D2.

This gives me a pool of potential participants.

I then merge this pool to the population of individuals assigned to treatment. The first reassuring element is that 73% of the potential participants could be located as living in an eligible (according to the baseline criteria) COA accommodation.⁴²

I then check that observable characteristics are similar in the experimental and non-experimental samples (i.e., other potential participants not found through COA addresses). I run linear regressions on a dummy for the following household head characteristics: gender, age at migration, and number of children. Results are reported in Table A4. I also compare the distribution of educational attainment between the two groups. Table A5 reports the observed distribution together with the one that would hold under \mathbb{H}_0 ; both distributions are identical. In Figure A1, I also report the distribution of countries of origin. While the differences between the two samples are statistically significant, their magnitude is small. In Table A4, the value of the dummy coefficients are small compared to the sample means. In Table A5, the two distributions greatly overlap and the histograms in Figure A1 have the same shape.

Table A4: Asylum Seekers’ Strategy - External Validity - Mean Comparison (In/Out) Experiment

	Male	Nb Children	Age at Arrival
In Experiment	0.010 (0.011)	0.172*** (0.031)	0.988*** (0.159)
No. of Obs	10,858	10,858	10,858
Mean Outcome	0.55	2.72	35.28
R-squared	0.00	0.00	0.00

Notes: This table reports the mean differences between asylum seekers (household heads) located as living in an eligible COA accommodation (in experiment) and those who could not.

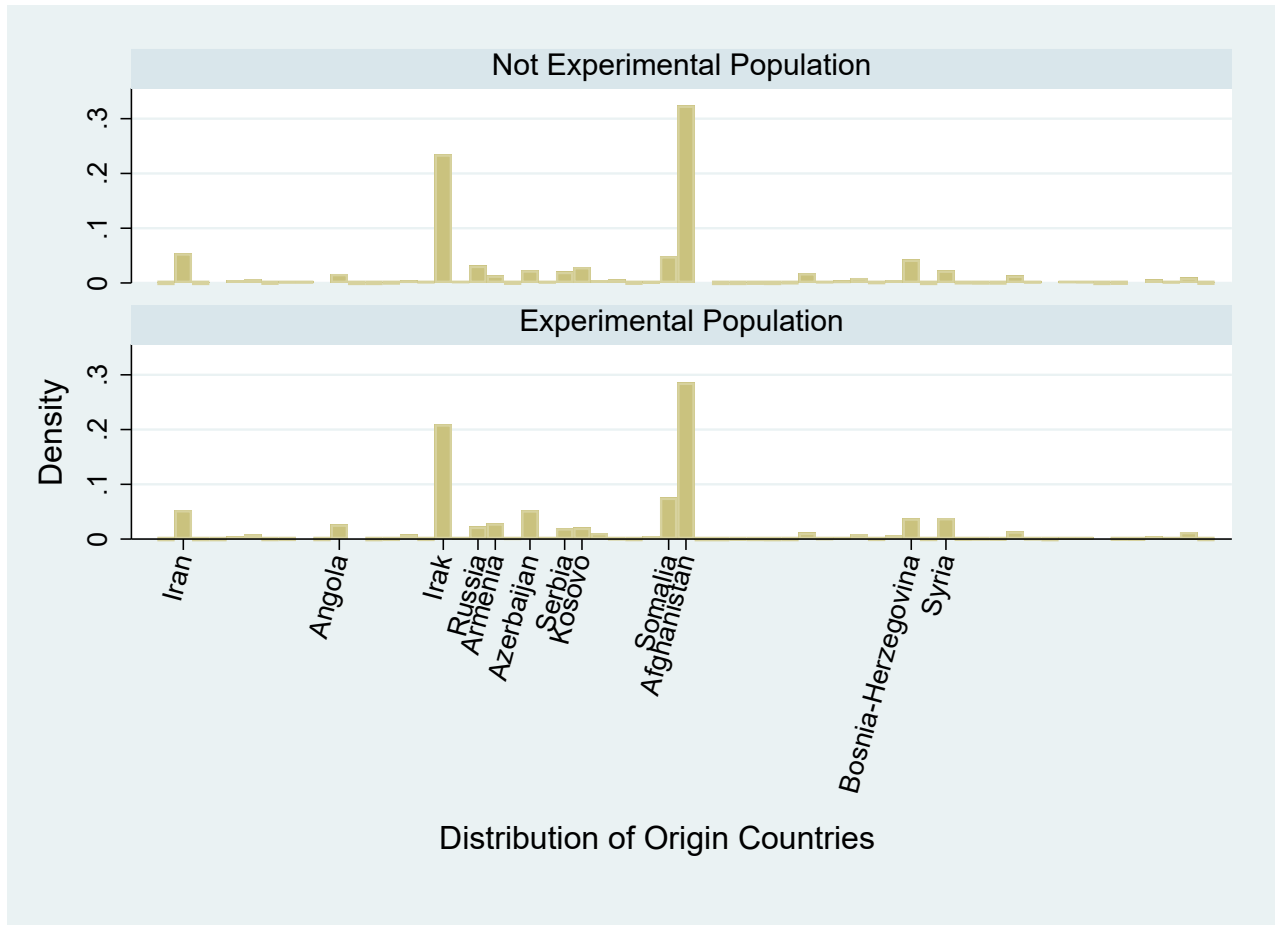
⁴²To obtain this number, calculate how many people are identified as being part of the experiment (“experimental” column of Table A5) and divide by the total number of observations in Table A4.

Table A5: Asylum Seekers' Strategy - External Validity - Distribution of Education Level (In/Out) Experiment

	Not Experimental	Experimental	Total
	Missing		
Observed	514	1,287	1,801
Expected under \mathbb{H}_0	476.2	1,324.8	
	Primary School		
Observed	740	2,199	2,939
Expected under \mathbb{H}_0	777.1	2,161.9	
	Lower Secondary		
Observed	496	1,506	2,002
Expected under \mathbb{H}_0	529.4	1,472.6	
	Upper Secondary		
Observed	582	1,672	2,254
Expected under \mathbb{H}_0	596.0	1,658.0	
	College		
Observed	280	696	976
Expected under \mathbb{H}_0	258.1	717.9	
	Above College		
Observed	259	627	886
Expected under \mathbb{H}_0	234.3	651.7	

Notes: This table reports the distribution of educational attainment between asylum seekers (household head) located in an eligible COA accommodation (experimental) and those who were not located (non-experimental). For each education category, I also report the number of experimental and non-experimental observations which should be observed under the null hypothesis that the two distributions are the same. Looking the difference between the observed and expected number of individuals give an indication of how different the samples are.

Figure A1: External Validity - Origin Countries - (In/Out) Experiment



Although this is reassuring, I perform a last check. I collapse the data by origin country and arrival year. This gives an indication at the origin country/year of arrival level of how many asylum seekers could appear in the baseline analysis and how many actually do. I calculate the probability of being in the sample and create weights which are equal to the inverse of these probabilities. They give more importance to combinations of origin country/year of arrival which are under-represented compared to what they could be. I then rerun the baselines regressions. Results are reported in Table D2.⁴³

⁴³Note that some combination of origin country and arrival year are not present in both samples and thus cannot be given a non-zero weight. Fortunately, they are very few individuals in these country/year groups. This is not a major concern in terms of bias since regressions include country and arrival year FE. This is the reason why they are fewer observations in Table D2 than in Table 6.

B Additional Material on Contraceptive Usage as a Culturally charged consumption

Table B1: Main Drugs Used by Young Women

ATC4 Code	ATC4 Name	Nb of occurrences	Percentage	Cumulated Percentage
G03A	Hormonal Contraceptives	3,918,958	24.76	24.76
M01A	Anti-inflammatory	971,426	6.13	30.9
R06A	Anti-histamines	725,766	4.58	35.49
J01C	Beta lactam antibiotics	718,868	4.54	40.03
D07A	Corticosteroids	699,210	4.41	44.45

Notes: This table reports the five largest frequencies of drug used by young women between the ages of 16 to 20 who were living in the Netherlands between 2006 and 2019.

Table B2: Immigrants and Natives - Views and Attitudes on Sexuality

	Before Marriage	Homosexuality	Female Sexuality	Knowledge Index	Had Sex	Use condoms
Immigrants	-0.32*** (0.021)	-0.12*** (0.017)	- 0.08*** (0.021)	-0.68*** (0.084)	-0.26*** (0.021)	-0.02 (0.029)
Mean Outcome	0.63	0.28	0.42	7.04	0.74	0.72
No. of Obs	9,611	9,611	9,611	9,611	9,611	6,874
R-Squared	0.03	0.00	0.01	0.04	0.13	0.00

Notes: This table reports the results from several linear regressions where the outcomes are measures of attitudes and (reported) behavior regarding sexuality. Explanatory variables are age fixed effects and a dummy for being a first or second generation (Non-Western) immigrants. The sample is made of young women who have been living in the Netherlands between the ages of 16 to 20 and who also answered the survey “Sex under the age of 25”. Outcome variables include a dummy for having positive views on sexual intercourse before marriage, on two men kissing in the street and on women having had several sexual partners. It also includes a knowledge index ranging from 0 to 8, a dummy for ever having had sex and conditional on the latter a dummy for using condoms.

Table B3: Relation with other measures - TeO data (France)

	Friend - Same Origin	Identity	Importance of Religion
Pill	-0.10** (0.048)	0.16*** (0.061)	-0.29*** (0.060)
Mean Outcome	0.18	0.52	0.44
R-Squared	0.043	0.038	0.056
No. of Observations	416	441	444

Notes: This table reports the results from several linear regressions where the outcomes are measures of friendship network, social identity and importance of religion. Explanatory variables are age fixed effects and a dummy for whether or not a woman reports contemporaneously using the contraceptive pill. The sample is made of second generation immigrants who answered the survey “Trajectoires et Origines”. Outcome variables include a dummy for having most of your friends from the same origin, for fully identifying as French and for reporting religion to be very important in your life. The sample is restricted to young women who were below 21 and who came from North or Sub-Saharan Africa, Turkey and Asian countries outside Vietnam, Cambodia and Laos (these were the closest geographical restrictions to the ones made for Tables 2 and B2).

Table B4: Mode of Contraception

Mode of Contraception	Native	Immigrant	Total
Pill	3,453	111	3,654
Condoms	505	26	531
Pill and Condoms	970	27	997
Contraceptive Injection	75	0	75
Patch	-	0	-
Ring	63	-	-
Hormonal Implant	93	-	-
IUD	868	37	905
Else	33	0	33
Total	6,155	206	6,361

Chi square test= 11.9, p-value= 0.156

Notes: This table reports the distribution of modes of contraception among young women who answered the survey “Sex under the age of 25”. The sample to whom this question is asked are those who reported ever having had sex and who are currently using contraceptives. The table also reports a χ^2 test of the null hypothesis: the two distributions are the same. Entries “-” correspond to values smaller than 10 that cannot be disclosed for reasons of confidentiality.

Table B5: Reasons for not using the pill - TeO data (France)

	Virginity	Abstinence
Immigrant	0.17*** (0.036)	0.25*** (0.043)
Mean Outcome (Natives)	0.07	0.13
R-Squared	0.039	0.087
No. of Observations	606	606

Notes: This table reports regressions of a dummy for not using the contraceptive pill because of “virginity” or “abstinence” on age fixed effects and a dummy for immigrant status. The sample is restricted to young women who were below 21 and who either were native or second-generation immigrants from North or Sub-Saharan Africa, Turkey and Asian countries outside Vietnam, Cambodia and Laos (these were the closest geographical restrictions to the ones made for Tables 2 and B2).

C Additional Material on the Institutional Setting and the Identification Strategy

C.1 Information from the Ad-Hoc Query on allocation of refugees to municipalities for integration purposes asked by the European Commission in February 2013

The European Commission addressed the two following questions to member states (MS):

1. Does your Member State regulate the dispersal of refugees and other persons that have been granted protection to municipalities once they have received a residence permit?
2. Does your MS share the Swedish experiences of a general shortage of available housing for newly arrived migrants?

and received the following answer from the Dutch government:

1. Yes, for over 20 years the Netherlands knows a obligatory system (by law, the Huisvestingwet or Law on Housing), in which municipalities (gemeenten) have to offer housing with priority to admitted asylum seekers. Every semester, each municipality is given, by the Ministry of Security and Justice, a ‘quotum’, the number of admitted asylum seekers that will have to be housed in that semester. The number of people that will have to be housed is based on the number of expected admittances in that half year, divided by the equation of the population of each individual municipality to the whole population of the Netherlands. Distribution of the number of refugees that must be housed is not based on economic or social principles, every municipality has to participate in this housing program. So, the local housing of admitted asylum seekers is an obligatory task of all the municipalities, who, in turn, will have to make agreements with local housing corporations/associations on the number and pace at which the houses become available. Thus this task needs planning, thinking ahead as the Netherlands is also confronted with a lack of (socially fitting) houses for refugees and other starters. The COA, the Dutch Organization for the Reception of Asylum seekers, has (by law) the task of ‘distributing’ the new admitted asylum seekers among the municipalities. Admitted asylum seekers are obliged to accept the offered house, otherwise they will loose the right for reception and support. Once housed they are free to re-allocate themselves. But there is only a right on priority on housing the very first time. The municipalities are not directly supported for the housing of refugees, but receive an additional fund of €1000

for each housed refugee, for social inclusion actions. Admitted asylum seekers are obliged to follow an integration-course.

2. Yes, the Netherlands shares the Swedish experience of a general shortage in available houses. As most admitted asylum seekers have to fall back on social benefits, they have to be housed on the social housing market (not the high-end of the market), in which they have to compete with others like starters. As said, they are housed with priority. This off course causes some friction, but as the admitted asylum seekers are dispersed among all municipalities, de ‘burden’ is equally shared. This keeps the system still going.

C.2 Origin countries

Table C1: Asylum Seekers’ Strategy - Main Origin Countries

Country	Share	Number
Afghanistan	0.223	1412
Irak	0.161	1018
Somalia	0.072	455
Iran	0.060	380
Azerbaijan	0.044	279
Russia	0.043	274
Syria	0.037	235
Bosnia Herzegovina	0.036	227
Armenia	0.033	211
Angola	0.028	178
Kosovo	0.021	131
Serbia	0.020	127
Turkey	0.018	111
Sudan	0.016	100
Zaire	0.014	91

C.3 More on mobility after COA assignment

The zip4 level is very small (equivalent to a large US census tract) and expecting people to live so close to their assigned location is probably too restrictive. This is why I also show how many live in the same municipality. Since that level can be very large, I calculate how many live in the same zip4 or in one adjacent to it. To do so, I geocode all adjacent zip4 areas in the country. This creates a series of larger (and overlapping) entities with a median population of around 20,000 inhabitants. This is still very close to the exact address of assignment.

Table C2: Types of COA Accomodation

Type of accommodation	Nb of centre appearing at the same address				Brief Description
	1	2	3	4	
Aanvullende opvang	206	42	12		Collective housing, for emergency when no other location available
Administratief geplaats	2,791	872	205	44	Individual housing found by a.s. with contacts in the NL
Alternatieve tijdelijke capaciteit	12	15			
Asielzoekerscentrum	105	90	49	27	Collective housing
Contingent	68	31	17		
Gemeentewoning	3,916	440	80	20	Individual housing for a.s. after being granted refugee status
Kinderwoongroep	23				Small scale location for unaccompanied minors
Kleinschalige Centrale Opvangseenhden	1,610	174	53	25	Small scale location for unaccompanied minors
Kleinschalige wooneenheid	16				Small scale location for unaccompanied minors
Opvang- en Onderzoekcentrum	14	15			Ter Apel centre where all a.s. start the application process
Orientatie & Inburgeringslocatie	10	17	24	12	Centre for people being denied refugee status prior to leaving
Terugkeerlocatie	38	65	36	20	Centre for families being denied refugee status prior to leaving
Tijdelijke Noodvoorziening	17	24	21	16	Collective housing, for emergency when no other location available
Zelf Zorg Arrangement	8,553	2,354	440	95	Individual housing found by a.s. with contacts in the NL

Note : The first column lists the main types of accommodations while the last column briefly describes them. The second column shows the distribution among types of accommodation for addresses that identify only one type of accommodation. The third column shows the distribution among types of accommodation for addresses that are identified as two types. The fourth column shows the distribution among types of accommodation for addresses that are identified as three types. The fifth column shows the distribution among types of accommodation for addresses that are identified as four types. The entry (2,4), i.e. 205 should be read as follows: among the addresses under which three accommodations are listed, 205 of them are *Administratief geplaats*, meaning the same 205 addresses appear again twice in the same column under different types of accommodation.

24% of the people living in the same municipality after three years thus no longer in the same neighborhood. More than half (57%) of those who live in the same municipality, but not in the same neighborhood, live in an adjacent neighborhood. Therefore, assignment influences future residential choices.

Table C3: Asylum Seekers' Strategy - Mobility after assignment

	No. still living in NL	Neighborhood (%)	Neighborhood or Adjacent (%)	Municipality (%)
After 1 year	6327	77.3	78.3	79.2
After 2 years	6286	39.5	42.7	45.2
After 3 years	6231	23.5	27.7	31
After 4 years	6217	13.5	18.6	22.6
After 5 years	6228	9.3	14.7	18.7
After 6 years	6261	7.2	12.9	17.2
After 7 years	6264	6.1	11.8	16.3
After 8 years	6272	5.1	11.1	15.5

Notes: For up to eight years after arrival and for three different geographical areas (zip4, zip4, or adjacent zip4 and municipality), this table reports the number of people still living in the Netherlands and the percentage living in the same geographical area.

C.4 More on balancing

Power of the test To give more credibility to the tests reported in Table 5, I show that they have power against the alternative of sorting. I regress ethnic concentration in the neighborhood where young women live when they are 16 on characteristics of the household head at that time (meaning the household head's age when their daughter turns 16, the number of children that year, etc.). These are the same regressions at age 16 that I ran at assignment. Regressions at age 16 should reject the absence of sorting.

Results are reported in Table C4. The picture is very different from the balancing regressions of Table 5. There is clear evidence of sorting. The F-tests reject the null at conventional levels. Being able to reject the null of no sorting gives credit to the results in Table 5 and establish the exogeneity of assignment.

Robustness of the balancing test and Additional Evidence I perform a second balancing test following Ammermueller and Pischke (2009). I randomly assign asylum seekers to COA accommodation and compare the distribution of observable characteristics (education level) in the actual data and simulated samples. I test (and fail to reject) \mathbb{H}_0 that the distributions are the same. More precisely, I calculate:

$$P = \sum_{c=1}^C \sum_{j=1}^L \frac{(n_{c,j} - \hat{n}_{c,j})^2}{n_{c,j}}$$

Where $n_{c,j}$ is the number of household head with education level j in location c (where C is the total number of accommodation opened that year) and $\hat{n}_{c,j}$ is the predicted number using random assignment such that $\hat{n}_{c,j} = \frac{1}{500} \sum_{s=1}^{500} \hat{n}_{c,j,s}$ where $\hat{n}_{c,j,s}$ is the number of household head with education level j in centre c in simulation s .

$$P \sim \chi^2 \text{ with } \frac{C-1}{J-1} \text{ degrees of freedom}$$

The results are reported in Table C5.

Table C4: Asylum Seekers' Strategy - Balancing Table - Power of the test

Male	2.849***	1.165
	(0.759)	(0.774)
Nb of children	0.033	0.628**
	(0.249)	(0.278)
Age	-0.031	-0.010
	(0.048)	(0.054)
Primary School	-0.873	-1.486
	(0.985)	(1.000)
Lower Secondary	1.238	0.879
	(1.027)	(1.077)
Upper Secondary	2.082**	1.222
	(0.941)	(0.956)
College	0.792	0.358
	(1.164)	(1.189)
Above College	1.346	0.913
	(1.241)	(1.251)
N Obs	4,943	4,943
R squared	0.01	0.05
Mean	72.25	72.25
F Test - All above	3.57	2.55
P Value - All above	0.00	0.01
F Test - Education	2.57	2.15
P Value - Education	0.03	0.06
Assignment Year	YES	YES
Origin Country	NO	YES

Notes: This table estimates equation 1 on the sample of household heads (of women from the experimental population with complete spells). The outcome is ethnic concentration measured as the share of natives of similar ages (-2 to +3 years old) when the oldest daughter turns 16. Explanatory variables include fixed effects for the country of origin and the assignment year, together with the gender of the head, the age of the household head, the number of children below 16, and dummies for education attainment of the household head, where the baseline category is lacking observation (15% of the sample). The head is identified as the father if he arrived at the same time as his daughter; otherwise, it is the mother. I report specifications with and without origin country fixed effects. Standard errors are clustered at the neighborhood level.

Table C5: Reallocating asylum seekers between addresses

Year	Chi Squarred Test	Degrees of freedom	Critical Values
1996	167.67	2874	3000
1997	170.58	3126	3257
1998	205.83	4458	4614
1999	254.00	5790	5968
2000	370.55	8598	8815
2001	431.32	8382	8596
2002	208.25	7338	7538
2003	179.88	3738	3881
2004	33.00	2370	2484
2005	49.30	1182	1263
2006	41.97	1290	1375
2007	23.90	930	1002
2008	51.37	1218	1300
2009	65.80	1218	1300
2010	37.17	1470	1560
2011	27.28	1038	1114
2012	26.92	714	777

Notes: For each assignment year, I randomly reallocate household heads (who arrived that year) to COA accommodations (opened that year) 500 times. I calculate the average number of household heads with a particular education level at each COA address each year. I then calculate for each year the χ^2 test of the difference between the observed distribution and the simulated one (i.e. the average of the 500 draws). I report the test statistics together with the degrees of freedom of the test for each year and the critical values (at the 95th percentile). The number of degrees of freedom is the product between the number of education groups (i.e. 5) and the number of COA accommodation which welcomed household heads (of the experimental population) that year. Since the χ^2 statistics is lower than in the critical values, I fail to reject \mathbb{H}_0 (equality between the two distributions) for all years.

D Additional Material with the Asylum Seekers Strategy

Table D1: Naive Estimation - Asylum Seekers' Strategy

Contraceptive Usage			
Share Natives	0.0006*	0.0007**	0.0007**
	(0.00032)	(0.00032)	(0.00035)
N Obs	6,341	6,341	6,341
Mean Outcome	0.41	0.41	0.41
R squared	0.063	0.066	0.066
Marriage			
Share Natives	0.0002	0.0003	0.0008
	(0.00072)	(0.00074)	(0.00096)
N Obs	716	716	715
Mean Outcome	0.13	0.13	0.13
R squared	0.222	0.237	0.235
Origin FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Age at Migration	YES	YES	YES
Family Controls	NO	YES	YES
Neighborhood Characteristics	NO	NO	YES
LASSO Selection	NO	NO	NO

Notes: This table reports estimations of equation 2. The outcome variables are a dummy for having taken contraceptives at least once by the age of 20 and one for being married to a native. The main independent variable is the share of natives in the neighborhood at the time girls turn 16. All specifications control for country of origin and assignment year fixed effects together with age at migration (specification reported in the first column). Additional controls are added successively. The second column adds household head characteristics, and the third adds neighborhood characteristics, i.e. the predicted share of teenagers using contraceptives at the municipality level for the contraception outcome and the “statusscore”, see footnote 29 for the marriage outcome. Control variables and share of natives are measured at age 16. The sample only includes teenage women with a complete spell. Standard errors are clustered at the neighborhood level.

Table D2: Asylum Seekers' Strategy - Marriage - Weighted Regressions

	Entire Sample	Age Migration <10	Age Migration <10 & Educated Parents
Share Natives	0.0003 (0.00062)	0.0014* (0.00072)	0.0023*** (0.00083)
N Obs	646	501	322
Mean Outcome	0.13	0.13	0.14
R squared	0.017	0.016	0.031
Origin FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Age at Migration	YES	YES	YES
Family Controls	YES	YES	YES
Status Score	YES	YES	YES
LASSO Selection	YES	YES	YES

Notes: This table reports OLS estimates of the ITT specification. The outcome variable is being married to a native. The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. Controls are chosen according to the algorithm by Belloni et al. (2014b) among the following: year of assignment fixed effects, age at migration, household head characteristics (including education), a measure of neighborhood “quality” the *stausscore*, see footnote 29 and country of origin fixed effects. Observations are weighted as to give more importance to combinations of origin country/arrival year which are potentially under-represented, see Section A.4. Educated Parents refer to the educational attainment of the household head, i.e. at least upper secondary. Standard errors are clustered at the neighborhood level.

Table D3: Asylum Seekers' Strategy - Contraception - Duration Analysis

Share Natives	0.0001 (0.00093)	0.0001 (0.00091)	-0.0004 (0.00100)
N Obs	6,341	6,341	6,341
Mean Outcome	0.41	0.41	0.41
Origin FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Age at Migration	YES	YES	YES
Family Controls	NO	YES	YES
Average Contraceptive Usage	NO	NO	YES

Notes: This table reports estimations of equation 2. The outcome variable is the age young women, between 16 and 20 start using hormonal contraceptives. The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. All specifications control for country of origin and year of assignment fixed effects together with age at migration (specification reported in the first column). Additional controls are added successively, the second column adds household head characteristics, the third adds the predicted share of teenagers using contraceptives at the municipality level. The hazard rate is parametrized as an exponential function. The sample only includes teenage women with a complete spell. Standard errors are clustered at the neighborhood level.

Table D4: Asylum Seekers' Strategy - Contraception - Non-Linear Effect

Share of Natives	-0.0008 (0.00121)	-0.0010 (0.00124)	-0.0013 (0.00126)	-0.0008 (0.00126)
Share of Natives Squared	0.0000 (0.00001)	0.0000 (0.00001)	0.0000 (0.00001)	0.0000 (0.00001)
N Obs	6,341	6,341	6,341	6,341
Mean Outcome	0.41	0.41	0.41	0.41
R squared	0.063	0.068	0.068	0.033
Origin FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Age at Migration	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Average Contraceptive Usage	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

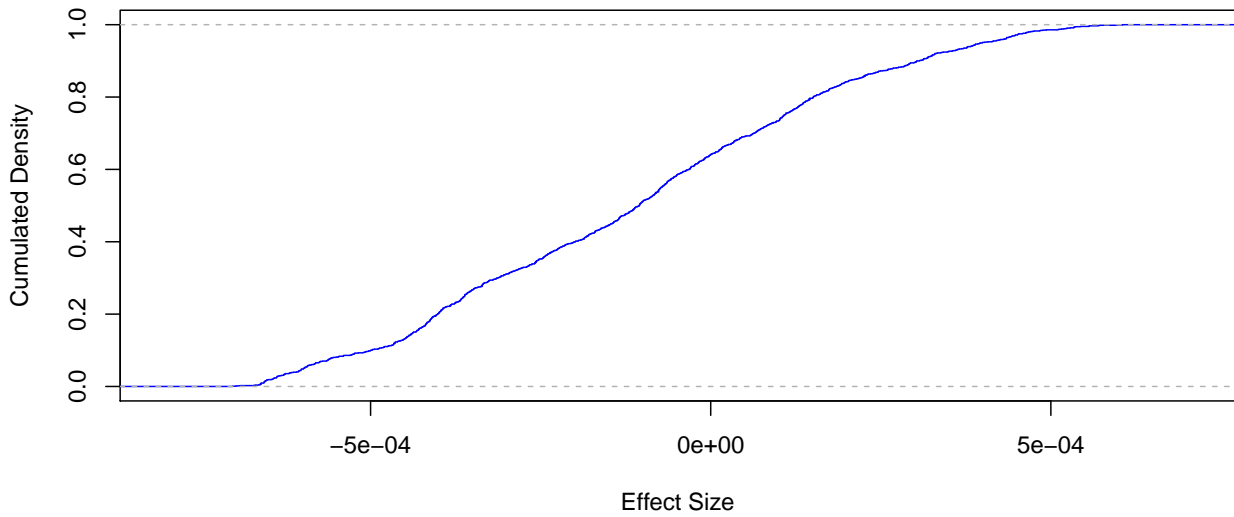
Notes: This table reports estimations from a non-linear (quadratic) extension of equation 2. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. The main independent variables are the share of natives in the neighborhood at the time of assignment by COA and its square. All specifications control for country of origin and year of assignment fixed effects together with age at migration (specification reported in the first column). Additional controls are added successively, the second column adds household head characteristics, the third adds the predicted share of teenagers using contraceptives at the municipality level. The fourth column uses a subset of the controls used in the third one, i.e. those selected according to the Double Debiased procedure (Belloni et al., 2014b). The sample only includes teenage women with a complete spell. Standard errors are clustered at the neighborhood level.

Table D5: Asylum Seekers' Strategy - Marriage - Keeping unmarried women

	Entire Sample	Age Migration <10	Age Migration <10 & Educated Parents
	OLS		
Share Natives	0.0002** (0.00008)	0.0003*** (0.00010)	0.0004*** (0.00010)
N Obs	6,341	4,859	3,009
Mean Outcome	0.01	0.01	0.02
R squared	0.015	0.015	0.019
Origin FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Age at Migration	YES	YES	YES
Familly Controls	YES	YES	YES
Status Score	YES	YES	YES
LASSO Selection	YES	YES	YES
	GRF		
Share Natives	0.00004	0.00014	0.00021***
Standard Errors	(0.00009)	(0.00010)	(0.00008)
N Obs	6,341	4,859	3,009

Notes: This table reports the conditional average treatment effects estimated using OLS and Generalized Random Forests (Athey et al., 2019). The outcome variable is being married to a native. The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. The following variables are used to build causal forests: year of assignment, age at migration, household head characteristics (including education) and a measure of neighborhood “quality” the *stausscore*, see footnote 29. Controls in the OLS regressions are chosen according to the algorithm by Belloni et al. (2014b) among the variables which enter the GRF algorithm and country of origin fixed effects. Educated Parents refer to the educational attainment of the household head, i.e. at least upper secondary. The OLS estimates are clustered at the neighborhood level.

Figure D1: Asylum Seekers' Strategy - Heterogeneity Analysis - Contraception



Notes: This figure represents the cumulative distribution of treatment effect on the contraception outcome. Treatment effect is estimated on each individual from a test subsample which comprise 20% of the original sample from Table 6. Estimation is done using Generalized Random Forests (Athey et al., 2019) where the variables entering the algorithm are age at migration, the education, gender and age at migration of the household head, year of arrival and neighborhood characteristics (predicted contraception usage at the municipality level).

Table D6: Asylum Seekers' Strategy - Alternative Samples - Marriage

	Incomplete	First Assignment	Variation1	Variation2
Panel A: Entire Sample				
Share Natives	0.0006 (0.00049)	0.0001 (0.00058)	0.0005 (0.00062)	0.0004 (0.00061)
N Obs	1,003	718	718	718
Mean Outcome	0.13	0.13	0.13	0.13
R squared	0.036	0.015	0.024	0.025
Panel B: Arrived < 10				
Share Natives	0.0016*** (0.00063)	0.0015** (0.00064)	0.0019*** (0.00056)	0.0017*** (0.00057)
N Obs	650	551	551	550
Mean Outcome	0.13	0.13	0.13	0.13
R squared	0.042	0.017	0.021	0.019
Origin FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Age at Migration	YES	YES	YES	YES
Family Controls	YES	YES	YES	YES
Status Score	YES	YES	YES	YES
LASSO Selection	YES	YES	YES	YES

Notes: This table reports estimations of equation 2. The outcome variable is a dummy for being married to a native. The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. All specifications use the Double Debiased procedure (Belloni et al., 2014b) to chose control variables among the following: country of origin and year of assignment fixed effects together with age at migration, household head characteristics and a measure of neighborhood “quality” the *statusscore*, see footnote 29. The different samples correspond to alternatives from the baseline one. Their detailed definitions can be found in subsection A.2. Standard errors are clustered at the neighborhood level.

Table D7: Asylum Seekers' Strategy - Education

Share Natives	0.0017** (0.00068)	0.0017** (0.00075)	0.0018** (0.00070)
N Obs	5,784	5,784	5,784
Mean Outcome	3.61	3.61	3.61
Origin FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Age at Migration	YES	YES	YES
Family Controls	NO	YES	YES
Status Score	NO	NO	YES

Notes: This table reports estimations of equation 2 using an ordered probit MLE. The outcome variable is an ordered variable for educational attainment (see Table A2). The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. All specifications control for country of origin and year of assignment effects together with age at migration (specification reported in the first column). Additional controls are added successively, the second column adds household head characteristics, the third adds a measure of neighborhood “quality”, the status score (see footnote 29). The sample only includes teenage women followed in Table 6 with non-missing information on educational attainment. Standard errors are clustered at the neighborhood level.

E Additional Material with the Sibling Strategy

E.1 Sample Characteristics

Table E1: Sibling Strategy - Origin Countries Usage

	Share
Morocco	0.363
Turkey	0.304
Indonesia	0.047
Afghanistan	0.044
Irak	0.037

E.2 Results and Robustness

Table E2: Sibling strategy - Contraception - Weighted Regressions

	All ages	4 to 10	11 to 16
Concentration	0.0017** (0.00074)	0.0015*** (0.00052)	0.0002 (0.00063)
N Obs	31,622	31,622	31,622
Mean Outcome	0.36	0.36	0.36
R squared	0.658	0.658	0.657

Notes: This table reports coefficients from estimating equation 4. The main outcome is having used hormonal contraceptives at least once by the age of 20 years old. The explanatory variables are family fixed effects and average share of natives one is exposed to during childhood and adolescence. The first column averages exposure experienced over all the ages from 4 to 16 while the second and third columns limit the averaging to age brackets: 4 to 10 years old and 11 to 16 years old. Standard errors are clustered at the family level. Observations are weighted so that each family has equal importance in the regressions.

Table E3: Coherence between the two strategies - Contraception

	All ages	4 to 10	11 to 16
Share Natives	-0.0001 (0.00029)	0.0005* (0.00030)	-0.0001 (0.00040)
N Obs	31,622	31,622	31,622
Mean Outcome	0.36	0.36	0.36
R squared	0.650	0.650	0.650

Notes: This table reports coefficients from estimating equation 4. The main outcome is having used hormonal contraceptives at least once by the age of 20 years old. The explanatory variables are family fixed effects and the share of natives one is exposed at a random point in time during childhood and adolescence. The first column chooses a random year between all the ages from 4 to 16 while the second and third columns limit the selection of the random year to age brackets: 4 to 10 years old and 11 to 16 years old. Standard errors are clustered at the family level.

Table E4: Sibling Strategy - Contraception - Birth Order

	All ages	4 to 10	11 to 16
Concentration	0.0016** (0.00075)	0.0014*** (0.00052)	0.0000 (0.00063)
N Obs	31,622	31,622	31,622
Mean Outcome	0.36	0.36	0.36
R squared	0.650	0.650	0.650

Notes: This table reports coefficients from estimating equation 4. The main outcome is having used hormonal contraceptives at least once by the age of 20 years old. The explanatory variables are family fixed effects, birth order fixed effects and the average share of natives one is exposed to during childhood and adolescence. The first column averages exposure experienced over all the ages from 4 to 16 while the second and third columns limit the averaging to age brackets: 4 to 10 years old and 11 to 16 years old. Standard errors are clustered at the family level.

Table E5: Sibling Strategy - Marriage

	All ages	4 to 10	11 to 16
Share Natives	0.00112 (0.001040)	0.00089 (0.000811)	0.00030 (0.000635)
N Obs	2,470	2,470	2,470
Mean Outcome	0.03	0.03	0.03
R squared	0.700	0.700	0.699

Notes: This table reports coefficients from estimating equation 4. The main outcome is being married to a native. The explanatory variables are family fixed effects and average share of natives one is exposed to during childhood and adolescence. The first column averages exposure experienced over all the ages from 4 to 16 while the second and third columns limit the averaging to age brackets: 4 to 10 years old and 11 to 16 years old. Standard errors are clustered at the family level. The sample corresponds to families from table 10, the baseline sample of the sibling strategy, where two sisters are married.

F Additional Results on Boys

Table F1: Asylum Seekers' Strategy - Marriage - Boys

	Entire Sample	Age Migration <10	Age Migration <10 and Educated Parents
Share Natives	0.0012* (0.00059)	0.0013 (0.00110)	0.0018** (0.00090)
N Obs	414	292	187
Mean Outcome	0.15	0.16	0.14
R squared	0.009	0.020	0.033
Origin FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Age at Migration	YES	YES	YES
Family Controls	YES	YES	YES
Status Score	YES	YES	YES
LASSO Selection	YES	YES	YES

Notes: This table reports OLS estimates of the ITT specification. The outcome variable is being married to a native. The main independent variable is the share of natives in the neighborhood at the time of assignment by COA. Controls are chosen according to the algorithm by Belloni et al. (2014b) among the following: year of assignment fixed effects, age at migration, household head characteristics (including education) and a measure of neighborhood “quality”, the *stausscore*, see footnote 29 and country of origin fixed effects. The sample is made of boys who have been living in the Netherlands from the age of 16 to 20 and who were placed in a COA accommodation when arriving in the Netherlands. Educated Parents refer to the educational attainment of the household head, i.e. at least upper secondary. Standard errors are clustered at the neighborhood level.

G Pre Analysis Plan

The Transitional Dynamics of Cultural Integration: Quasi-Experimental evidence from asylum seekers' placement in the Netherlands

Pascal Achard*

April 3, 2018

Abstract

This paper documents how quickly immigrants adopt the cultural behaviors of natives and studies if growing up in an ethnic enclave slows down or speeds up the dynamics of convergence. To measure cultural behavior, I use administrative data on prescription of contraceptives to women. To identify neighborhood effects, I use the random assignment of asylum seekers to welcome centers in the Netherlands in the 1990s and 2000s. To capture social interactions and isolate peer effects, I merge the information on prescriptions with administrative data on schools attended by teenage women.

Key information related to the submission to the RCT registry:

- This project is not a RCT but relies on quasi-experimental evidence. I submit the main elements of the empirical strategy (outcome variables, exogenous source of variation, main data source, relevant literature for comparison...) for transparency.
- I have not yet received access to the data (I should in a few days after the submission).
- I have not requested approval from the IRB of my university for two reasons:
 - I am not collecting data, I will use already anonymized data from the Dutch Statistical Agency.
 - I signed a contract including data privacy clauses with the Dutch Statistical Agency.

Brief description of the Project

The objective of this project is to study the cultural integration of immigrants:

1. It would document how immigrants' cultural behavior converges to that of natives (descriptive part)
2. It would see whether convergence is faster for immigrants who live in a environment with more/fewer natives ("neighborhood hypothesis").

Cultural behavior would be primarily measured with prescription of contraceptives. Identification of the "neighborhood effect" would rely on the random assignment of asylum seekers to welcome centers in the 1990s and 2000s.

*European University Institute, pascal.achard@eui.eu

More detailed presentation

- Outcomes of interest (for both descriptive analysis and “neighborhood hypothesis”):
 - The main outcome is prescription of contraceptives to women.
 - I would also look at other outcomes that are pertinent for immigrant women: probability of being married to a native, fertility and probability of working.
 - I would also look at the probability of marrying a native for immigrant men (to see if the effect is different for men and women).
- Population of interest. I will focus successively on two populations:
 - All adult women in the Netherlands (native, immigrants, part of the “experimental population” or not). For the different outcomes detailed above, I would compare natives and immigrants (descriptive analysis).
 - “Experimental population”, those who “did not fully choose where to live”. This population is given by the asylum seekers who were welcomed and hosted by the COA from 1996 to 2016. For the primary outcome, women who arrived young in the Netherlands or daughters of asylum seekers hosted by COA.
- Treatment variable (for the “neighborhood hypothesis”) means to be exposed to a different proportion of natives/immigrants, either in the neighborhood where women live or in the school they attend.
- Assignment to Treatment (for the “neighborhood hypothesis”), i.e. mechanism through which asylum seekers were sent to “neighborhoods” with fewer/more natives. Asylum seekers from 1996 to 2016 were sent randomly to COA (Centraal Organ opvang Asielzoekers) welcome centers. They often had to wait many months/years in these welcome centers before they were granted refugee status. To identify them, I would follow the strategy developed in Beckers and Borghans (2011).
- Estimation method
 - Linear models
 - * Ordinary Least Squares with neighborhood characteristics at the time of migration (as in Åslund and Fredriksson (2009); Åslund et al. (2011)) or current characteristics (ITT interpretation as in Damm and Dustmann (2014)).
 - * Instrumental Variable where in the first stage, I would regress characteristics in the year of interest on those at the time of migration (in a fashion similar to Edin et al. (2003); Damm (2009)). This allows to identify an effect on the subpopulation which has not moved.
 - Non linear models
 - * I would also use models for duration analysis (where the outcome would be at what time do women start taking contraceptives) with the different strategies mentioned above.

References

- Beckers, P. and Borghans, L. (2011). Segregation in neighbourhoods and labour market outcomes of immigrants: Evidence from random assignment in the Netherlands. Technical report.
- Damm, A. P. (2009). Ethnic Enclaves and Immigrant Labor Market Outcomes: Quasi-Experimental Evidence. *Journal of Labor Economics*, 27(2):281–314.

- Damm, A. P. and Dustmann, C. (2014). Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior? *American Economic Review*, 104(6):1806–32.
- Edin, P.-A., Fredriksson, P., and Åslund, O. (2003). Ethnic Enclaves and the Economic Success of Immigrants? Evidence from a Natural Experiment. *The Quarterly Journal of Economics*, 118(1):329–357.
- Åslund, O., Edin, P.-A., Fredriksson, P., and Grönqvist, H. (2011). Peers, Neighborhoods, and Immigrant Student Achievement: Evidence from a Placement Policy. *American Economic Journal: Applied Economics*, 3(2):67–95.
- Åslund, O. and Fredriksson, P. (2009). Peer Effects in Welfare Dependence: Quasi-Experimental Evidence. *Journal of Human Resources*, 44(3).