Ethnic Enclaves and Cultural Assimilation

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March 27, 2022

Abstract

This paper studies whether growing up in an ethnic enclave slows down immigrants' cultural assimilation. To measure cultural behavior, I rely on individual-level administrative data on drug usage and focus on a culturally charged consumption: the usage of hormonal contraceptives by young immigrant women. To provide causal estimates of neighborhood influence, I exploit the quasi-random allocation of asylum seekers to government housing in the Netherlands between 1996 and 2012. While there is evidence of cultural assimilation over time, it is slow and cannot be accelerated by limiting the formation of ethnic enclaves.

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

Keywords: Cultural Assimilation, Contraceptive Usage, Neighborhood Effect

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I wish to thank Jérôme Adda, Yann Algan, Olof Åslund, Sascha Becker, Michèle Bélot, Alberto Bisin, Leah Boustan, Leonardo Bursztyn, Michela Carlana, Elena Cettolin, Joseph Doyle, Per-Anders Edin, Rachel Kranton, Boris van Leeuwen, Katrine Loken, Eleonora Pattachini, Giovanni Peri, Pauline Rossi, David Schindler, Jan Stuhler, Sigrid Suetens, Marco Tabellini, and Noam Yuchtman together with seminar participants at EEA, RES, SOLE, SMYE, EALE, European Meeting of the Econometric Society, IZA Migration Meeting, CREST, EUI, Tilburg, and Alicante 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

Cultural distance and the perceived willingness to adopt mainstream culture are important factors in explaining natives' support (or lack thereof) for immigration (Bansak et al., 2016; Adida et al., 2014a). Economic factors are not the only drivers of attitudes towards immigration (Card et al., 2012; Tabellini, 2020; Alesina and Tabelini, 2020). Given the importance of neighborhoods in shaping social networks, it is a common tool, considered to accelerate cultural assimilation, to avoid clustering people from the same origin (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 neighborhood ethnic concentration causally affects cultural assimilation.[1] This is challenging for two reasons. First, measuring culture is not straightforward. Second, one needs to exploit quasi-experimental variation in where immigrants live to provide causal estimates of neighborhood effects. I study this question in the Dutch context where the combination of rich administrative data and a residential experiment allows me to overcome these two issues.

My main measure of cultural assimilation is the usage of hormonal contraceptives by young immigrant women (between 16 and 20 of age).[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 teenage women. In particular, the pill is seen as an empowerment device through its connection to more equal gender roles (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).

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. They are observed at a relatively high frequency (annually) for the entire population. The data confirms the intuition behind this outcome. There are large differences in contraceptive usage

[1] In 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.

[2] As can be seen in the Pre-Analysis Plan (PAP) attached in Appendix J, this outcome has always been listed as the main one for this study.

between native-Dutch and non-Western immigrant populations. By age 17, 63.5% of native-born individuals have used hormonal contraceptives at least once compared with only 14.3% of first-generation immigrants. At age 20, these proportions are 86.1% and 36.5%, respectively. 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 two things; (i) immigrants have much more conservative opinions than natives and (ii) immigrant women who use contraceptives hold substantially more liberal views than those who do not. They are more likely to consider sex before marriage as normal and are more tolerant towards homosexuality and less judgmental towards female pre-marital sexuality.

To draw causal inference on neighborhood characteristics, I focus on asylum seekers in the Netherlands. On arrival in the country, they are cared for and hosted by a public organization, the Central Agency for the Reception of Asylum Seekers (COA). Allocation of asylum seekers into collective accommodation is decided according to availability of places and not according to preferences. I use this institutional setting as a mechanism that quasi-randomly disperses asylum seekers across the country. Ethnic concentration at the time of assignment by the COA is exogenous and used to estimate causal effects.

COA policy is to have their residents open to the community where they are located. The collective housing they manage is not closed, children go to local schools, parents can work. For those reasons, I treat allocation by the COA as equivalent to allocation to a neighborhood. On average, asylum seekers remain more than two years (27 months) in COA accommodation. Three years after assignment, 30.7% live in the same municipality, and 27.5% either live in the same neighborhood (equivalent to a large census tract in the US) or in one adjacent to the place they were assigned. I focus on asylum seekers who arrived before the age of 16 (when the main outcome starts to be observed). Therefore, I study the environment in which they grow up rather than the one in which they live as adults.

I find evidence of cultural assimilation. Immigrants who arrive younger and, who have therefore lived longer in the country are more likely to behave like natives, i.e. they take hormonal contraceptives. Using family fixed effects, I compare sisters who migrated at the same time, but at different ages. I find that arriving one year younger is associated with a

1.08 percentage point increase in the probability of using hormonal contraceptives by age 20. I interpret this rate of convergence to be slow, since it is an order of magnitude lower than the initial gap between natives and immigrants.

I do not find evidence that this slow convergence is driven by ethnic clustering. Naively regressing contraceptive usage on the size of the ethnic community (after they leave COA accommodation, so when it is endogenous) shows a negative correlation. However, the causal effect (using concentration at the time of COA assignment) is very small in magnitude and statistically insignificant. This result is robust to using different definitions of ethnic concentration, namely the share of natives in the neighborhood, the share of immigrants with a Muslim background, and the log of immigrants from the same country. Using machine-learning techniques, in particular generalized random forests (Athey et al., 2019), I do not find evidence that this baseline result hides a significant effect on a relevant sub-population.

I perform three important robustness checks: (i) I use other measures of assimilation (obtaining the Dutch citizenship and marrying a native) with the same identification strategy (placement of asylum seekers), (ii) still using the same strategy, I use another outcome not directly related to cultural assimilation but known to be affected by ethnic enclaves, i.e, educational attainment and (iii) I use the main outcome (hormonal contraceptives) with an alternative identification strategy. The results on cultural assimilation carry over to other outcomes, i.e., I do not find an effect on Dutch citizenship and marriage with a native. This additional analysis also shows that the identification strategy used in this paper has power. It can capture an effect: I find that a large ethnic community in the neighborhood causes lower educational achievements. The alternative identification strategy used to perform (iii) consists in regressing hormonal contraceptive usage on ethnic concentration experienced during childhood and family fixed effects. Conditional on sorting into neighborhoods being time-invariant within a family, this strategy provides causal estimates. It can be implemented on a much larger sample (six to seven times larger, between 32,000 and 40,000 people) since it is not restricted to asylum seekers. The results confirm the absence of a neighborhood effect.

To sum up, the outcome "contraceptive usage" changes with the treatment "arriving younger" (evidence on cultural assimilation), and the outcome "education" changes with the treatment "neighborhood". Therefore, I am confident that I could pick up an effect of "neighborhood" on "contraceptive usage" (the main focus of the paper) if there was one. In the meantime, other

cultural outcomes using the "asylum seekers strategy" and another identification strategy based on a much larger sample confirm the main result.

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., 2021; Gagliarducci and Tabellini, 2021). Bisin et al. (2008) show a correlation between the strength of religious identity and neighborhood characteristics in the U.K. Abramitzky et al. (2020a) look at the effect of leaving a Jewish ethnic enclave in the U.S. during the age of mass migration (1850-1914). They do not find an effect on cultural transmission (measured as the probability of giving a Jewish-sounding name to their children). My findings are consistent with theirs. Using the timing of the arrival of Italian priests in immigrant communities in the US during the age of mass migration, Gagliarducci and Tabellini (2021) find a negative effect of religious organizations on the social assimilation of Italian immigrants. Note that the focus of the authors is different from the one of this paper; they study how organized a community is rather than ethnic concentration per se. Focusing on mass deportation to Kazakhstan under Stalin's era, Jarotschkin and Zhuravskaya (2019) finds evidence that the exiled populations influence the gender norms of natives. Despite the important differences in context, I focus on the influence of a majority group on a minority one, rather than the other way around.[3] Algan et al. (2021) breaks down the immigrants' decision to give an Arabic-sounding name to their children into a vertical, a horizontal, and an economic incentive channel. While the horizontal channel is statistically significant, it is substantially lower than the other two.[4]

My second contribution to the literature is to propose a revealed preference measure of cultural assimilation, i.e., using hormonal contraceptives.[5] A sense of identity on the part of

[3]In the empirical setting of this paper, there are clear minority and majority groups (asylum seekers and natives). I focus on the influence from the majority to the minority. This differs from Merlino et al. (2019) who study exposure to minorities on assortative mating. It also differs from settings where only minority groups interact, see Bazzi et al. (2019) for the effect of ethnic fractionalization on nation-building.

[4]Although 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.

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

immigrants (Bisin et al., 2008, 2016; Dahl et al., 2021) does not provide the full picture of the acculturation process. 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 preference, but rather on revealed preferences, as measured by specific types of consumption (Atkin et al., 2021).[6] 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.[7] 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). 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. (2016)).[8] Using geographically disaggregated data allows one

[6]Harder et al. (2018) propose a framework to evaluate measures of immigrants assimilation. Contraceptive usage meets two of the most important criteria laid out, namely construct validity through contrasted group and through correlation with other predictors of assimilation. To satisfy the former, immigrants who have been in the country longer should be classified as more integrated. This is the purpose of Section 4. To satisfy the latter, contraceptive usage should correlate with other predictors of assimilation. This is assessed in Section 3.

[7]Concerns 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 6.3, 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 5.1.2.

[8]Other empirical strategies have been used to assess neighborhood effects, using neighborhood fixed effects (Bertrand et al., 2000; Grönqvist, 2006), a housing voucher lottery (Kling et al., 2007; Chetty et al., 2016), and quasi-random variation by focusing on movers (Chetty and Hendren, 2018b). I indirectly contribute to this

better to approximate the environment one grows up in.

Beyond the contribution of the identification strategy and the main measure, taken separately, their combination allows to provide high quality empirical evidence on an important policy question. The implications of my findings are that limiting ethnic clustering, through housing policies for instance, does not accelerate cultural assimilation, but will have a positive effect on the educational achievement of immigrants.

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 shows evidence of convergence through time. Section 5 presents how the placement of asylum seekers can be used as a quasi-experiment and details the identification strategy. Section 6 presents causal neighborhood effects on contraceptive usage, while Section 7 shows results using other outcome variables (marriage with a native, acquiring the Dutch citizenship and educational attainment). 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. Contraception for women younger than 21

literature, since one of the robustness checks (see Section 6.3.3) is to replicate the analysis with an alternative identification strategy. Harding et al. (2021) provide a discussion on potential differences in results between different identification strategies. 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.

is part of the basic package.[9] The data are collected on the entire population living in the Netherlands. There is no self-reporting and no measurement error.

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.[10] This 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 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 accommo-

[9] In 2011, a reform of the health system lowered the age from 25 to 21. To maximize the number of observations, I focus on a limit up to the age of 21 throughout the period 2006-2019.

[10] Usage of contraceptives appears in the data in the following specific cases: (i) women showing up at the pharmacy with a prescription that was already used in a previous year to buy contraceptives (or with an old tablet in the case of the pill), (ii) a young woman showing up with a prescription (old or new), but who wants to pay in cash so that no records of payment are kept (and potentially found by her parents).

[11] More information is available on the WHO website. G03A is itself subdivided into four categories: G03AA progesterones and oestrogens, fixed combinations, G03AB progesterones and oestrogens, sequential preparations, G03AC progesterones 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.

[12] In 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.

dations 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 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 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

Restrictions Based on Countries of Origin Non-Western immigrants refer to immigrants from Africa, the Middle East and South-East Asia.[13] Muslim immigrants refer to people originating from a mostly Muslim country (member of the Organization of Islamic Cooperation, see Table A2).

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 contraceptives 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]

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 contraceptives 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

[13] More 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

[14] There 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 of 3.56%.

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 anti-inflammatory (the second-most common medicine) and 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 hormonal contraceptives.

Table 1: Differences in usage between immigrants and natives

Age	Number of young women			Percentage		
	Natives	1^{st} Gen	2^{nd} Gen	Natives	1^{st} Gen	2^{nd} Gen
			Hormonal Contraceptives			
16	660,524	12,506	91,614	44.3	7.3	15
17	660,524	12,506	91,614	63.5	14.3	24.1
18	660,524	12,506	91,614	75.4	21.9	32
19	660,524	12,506	91,614	82.2	29.5	39
20	660,524	12,506	91,614	86.1	36.5	45.3
			Anti-inflammatory			
20	660,524	12,506	91,614	44.2	44.5	46.2
			Any Drug			
20	660,524	12,506	91,614	97.9	94.3	96.2

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 other medication at least once by a certain age. Women are classified into three groups: natives, first-generation immigrants, and second-generation (Non-Western) immigrants.

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. However, 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.

[15]Table 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%).

Immigrants who use hormonal contraceptives (as measured in administrative registries) hold more liberal views than those who do not. Table 2 reports linear regressions of various outcomes measured in "Sex under the age of 25" on age fixed effects and a dummy for having used contraceptives at least once by the age of 20.[16] Conditional on age, and compared to the mean outcome, 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 be judgmental on pre-marital female sexuality. They also report having sex at a much higher rate. In light of these results, I interpret the usage of hormonal contraceptives as a proxy for adopting natives-type attitudes regarding sexuality. The usage of hormonal contraceptives is also a strong indication of (reported) sexual activity.

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

	Before Marriage	Homosexuality	Female Sexuality	Knowledge Index	Had Sex
Contraceptive	0.30***	0.11***	0.20***	0.94***	0.45***
	(0.038)	(0.032)	(0.039)	(0.165)	(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 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 several sexual partners. It also includes a knowledge index ranging from 0 to 8 and a dummy for having had sex.

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 this measure.

[16] "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.

3.2 Heuristic Discussion

The rationale behind using hormonal contraceptives as a measure of cultural assimilation is that it captures a dimension in which views and attitudes between immigrants 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, allowing us to study behavioral updates and transitional dynamics. 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 Hormonal contraceptives should be thought of as a complement to already established measures; name-giving, fertility, marriage with a native and survey questions on identity and attachment to the home country or country of origin. Cultural assimilation is a multi-dimensional process, and no outcome alone can fully capture it. I highlight below how hormonal contraceptives relate to other measures. I also show in Section 7 that the results of this paper carry out to other measures.

A very popular outcome in the literature is whether immigrants give a native-sounding name to their children. 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.

Fertility and marrying a native are key demographic characteristics. Their importance cannot

be overemphasized. Realization of these outcomes typically occur later in life (age 25-35) and corresponds to rather rare events (the unconditional probability for marrying a native in the baseline sample is 14%, see Table F1). These outcomes are probably less suited for a population of young immigrants. This paper focuses on an exogenous variation occurring when immigrants are children. The emphasis on childhood/early teenage years make it relevant to measure an outcome when these immigrants are teenagers/young adults. The outcome more immediately follows the treatment.

Survey measures on religiosity and identity are also very informative. However, their sample size is however much smaller than administrative data. It is difficult to observe both an outcome and an exogenous variation on a large enough sample. On the other hand, contraceptive usage is a high-frequency culturally-charged consumption that is observable (free of measurement error) for the entire population.

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 very 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 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 for two reasons: first, differences between immigrants and natives are very large (see Table 1). Even small changes that would only narrow the gap between the two groups, could come out as statistically significant. Second, this outcome is not inelastic. As we discuss in the next section, usage increases the longer immigrants live in the Netherlands. This shows that there is some variation to be explained.

4 Convergence Through Time

To assess assimilation through time causally, one would rely on exogenous variation in length of exposure to the destination country. This could occur if the age at migration was random. Depending on how old immigrants were when they arrived, some will have lived longer in the destination country when they reach adulthood.

Following van den Berg et al. (2014), I compare first-generation immigrants who migrated at the same time, but at different ages, holding the family effects fixed. Imagine two sisters, both arriving in January 2001, one at the age of 5, the other at the age of 8. The former will be 20 years old in 2013, and the latter in 2016. I compare their usage rate in those years and identify the effects of exposure length with the three-year difference on arrival. This partials out time-invariant family characteristics. For instance, siblings share the same parents, who can be more or less conservatives; the siblings probably go to see the same GP and attend the same schools. While the remaining variation does not come from a quasi-experiment, it eliminates very important sources of bias.

4.1 Sample Definition and Characteristics

I follow first-generation women with at least one sister (same mother and father), who arrived at the same time (i.e., who first appear in the Dutch registries, at the most one month apart). Because of sample-size issues, I focus primarily on incomplete spells in this section (young women observed from 18 to 20 years of age).

Sample characteristics are reported in Table 3. Panel A details individual characteristics, and Panel B details family characteristics. The distribution of age on arrival is relatively symmetric (the mean and median are very close: 7.88 and 7.7 years of age, respectively); 50% of the sample arrived between 5 and 10 years of age. Slightly more than half, 51.1%, originated from Afghanistan and Iraq, while 21.35% came from either Morocco or Turkey. For most girls, the highest recorded education is lower or upper secondary school (see Table A3 for a description). Few of the girls have started higher education by the age of 20. The median age between the oldest and the youngest sisters is three years. Reassuringly, there is variation in contraceptive usage within families; in more than one third of them (35%), at least one sister was using hormonal contraceptives, and another one was not.

Table 3: Descriptive Statistics - Convergence Sample

Panel A: Individual Characteristics ($n = 5,701$)	
Age at arrival (in years)	
25^{th} percentile	5.19
Median	7.7
75^{th} percentile	10.46
Mean	7.88
Educational Attainment	
Missing Value	536
Lower Secondary	2,423
Upper Secondary	2,131
College and Above	611
Main Countries of Origin	
Afghanistan	30.66%
Iraq	20.44%
Morocco	11.09%
Turkey	10.26%
Somalia	4.39%
Panel B: Family Characteristics ($n = 2,566$)	
Difference between oldest and youngest sister (in years)	
25^{th} percentile	1.83
Median	3
75^{th} percentile	4.67
Mean	3.43
Educational Attainment of the Father	
Missing Value	671
Primary School	680
Lower Secondary	382
Upper Secondary	424
College	183
Above College	226
Difference in contraceptive usage between sisters	
Both using	368
None using	1,299
Some using, some not	899

Notes: This table reports descriptive statistics on young women followed in the convergence analysis. Panel A details individual characteristics, and Panel B details the characteristics of their family. Panel A reports the age at migration, the education level by categories (see Table A3), and the main countries of origin. Panel B reports the information on the age difference between the oldest and the youngest sibling in families, the difference in contraceptive usage between sisters, and the education level of the household head. Categories in Panel B correspond to those reported in Table A3, where Primary School is anything below Lower Secondary.

4.2 Differences in Behavior over Time

I estimate the following equation :

$$y^a_{i(f)} = \alpha + \lambda_f + \beta \, \text{age}_{i(f)} + X_i + \epsilon_{i(f)} \tag{1}$$

Where $y^a_{i(f)}$ is a dummy for having taken contraceptives at least once by age a ($a \in \{16, 17, 18, 19, 20\}$) for individual i in family f. λ_f are family fixed effects and X_i is a dummy variable for the first-born child in the family (van den Berg et al., 2014; Abramitzky et al., 2020b). The variable of interest is $age_{i(f)}$, the age at which individual i arrived in the country. Standard errors are clustered at the family level. Results are reported in Table 4 where the first column does not include X_i.

All coefficients are negative and highly significant. Arriving one year younger (and having been in the country an additional year) increases the probability of using hormonal contraceptives. Paths start to differ early. An effect can be detected even for young ages, when the conditional mean is very low (6.31% or 11.54% at ages 16 or 17). This is noteworthy, since at these ages, children are very likely to live with their parents. They do not wait to be out of the house before adopting the native mainstream behavior. The effect is smaller in absolute values at younger ages (0.46 percentage point at age 16 versus, 1.08 percentage points at age 20), but is, relative to the mean, larger for early teenagers. There is no evidence that this difference is driven by birth order. If anything the coefficients are larger when a dummy for the first-born is included (compare Columns (1) and (2)).

Although the effect is quite large when compared to unconditional means, it is relatively small when one considers the gap between immigrants and natives. If we extrapolate these findings, two sisters born 16 years apart (the largest difference one could theoretically observe in the sample) will have a 17.28 percentage points difference in the probability of using contraceptives (at age 20). This is (quite substantially) larger than the mean comparison between first and second generations in Table 1 (8.8 percentage points), but little compared to the difference with natives (49.6 percentage points).

This result establishes one important fact that justifies ex-post usage of hormonal contraceptives as a measure of cultural behavior: there is an evolution in immigrants' behavior over time. This outcome is not fully inelastic. With 5,700 observations and a quite stringent specification

(a very large number of fixed effects), these regressions are able to precisely estimate the effect of what could be considered a "small-scale" policy, i.e., arriving one year younger. Given that contraceptive usage changes with length of exposure, it is relevant to see whether the environment (i.e., neighborhood ethnic concentration) influences it. The coefficients reported in Table 4 can also be used as a benchmark to assess the magnitude of neighborhood effects.

Table 4: Convergence with Time

Contraceptive Usage	(1)	(2)	Mean Usage	No. of Obs
By Age 16	-0.46**	-0.48*	6.31	3,912
	(0.22)	(0.26)		
By Age 17	-0.75**	-0.79**	11.54	4,767
	(0.23)	(0.27)		
By Age 18	-1.31***	-1.44***	17.05	5,701
	(0.23)	(0.27)		
By Age 19	-1.26***	-1.53***	24.26	5,701
	(0.27)	(0.31)		
By Age 20	-1.08***	-1.31***	30.99	5,701
	(0.28)	(0.33)		
Family FE	YES	YES		
1^{st} Born Dummy	NO	YES		

Notes: Each cell reports the results of a linear regression where the outcome is a dummy variable for having taken contraceptives at least once by a certain age. The explanatory variables include age at arrival (reported coefficients), family fixed effects, and, in Column (2), a dummy for being the oldest child in a family. The regressions are estimated on the sample of first-generation women who were living in the Netherlands between the ages of 16 and 20 during the period from 2006 to 2019. It is limited to families where at least two sisters were followed from the age of 18 to 20. Standard errors are clustered at the family level.

Robustness The results are robust to altering the sample definition; see the discussion in Appendix C (Tables C1, C2, and C3). I also make sure that the estimates are not driven by the extensive margin. There is no effect of increased in length of exposure on the probability of using any drug, i.e., the extensive margin (see Table C4).

5 Institutional Settings and Identification Strategy

A neighborhood's ethnic composition can slow down cultural assimilation. Since it is easier to interact with people from the same ethnicity, living in a neighborhood with a large immigrant community provides more peers and role models with customs and rules that are distinct from the natives' mainstream culture. This also increases the probability of being identified as a non-complier if someone moves away from the community norm. 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 natural experiment used in this paper 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 taken care of by a public organization in charge of welcoming asylum seekers and accommodating them, the Central Agency for the Reception of Asylum Seekers (COA). Since asylum seekers do not choose where they go, their assignment at arrival provides the exogenous variation required to make causal inference. I then look at their contraceptive usage later in life.

5.1 Institutional Setting

5.1.1 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. Once they have been given six days to recover from their trip, they are interviewed by the Police to check their identity and their motives for seeking asylum. 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.[17] Since 1996, the COA is responsible 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.

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

[17]The level at which randomization takes place is different from the other so-called "dispersal policies" (Aslund et al., 2003; Damm, 2009). 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 allows us to look at more disaggregated definitions of neighborhoods. 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, nor on the country of origin (as a sole indication of eligibility to the policy, see Damm (2009)).

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 a real 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 6.4. 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 the assignment.

5.1.2 Identification of the Experimental Population

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 D2.

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.[18] I 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. The interested reader can find the exact algorithm used to define the sample in Appendix A.2. Results are robust to altering the sample, as discussed in Appendix A.3.

[18]Before 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.

Table 5: Descriptive Statistics - Baseline Sample

Panel A: Characteristics of the Household Head ($n = 5,227$)	
Age at arrival (in years)	
25^{th} percentile	31.92
Median	36.25
75^{th} percentile	41.48
Mean	0.78
Educational attainment (no. of individuals)	
Missing Value	914
Primary School	1,296
Lower Secondary	950
Upper Secondary	1,169
College	459
Above College	439
Number of young children	
At Arrival	2.53
At age 16	1.40
Geographical Distribution, no. of neighborhoods	
At Arrival	441
At Age16	1,652
Panel B: Characteristics of the teenage girls ($n = 6,525$)	
Age at arrival (in years)	
25^{th} percentile	4.55
Median	6.89
75^{th} percentile	9.72
Mean	7.30
Year of Assignment	
10^{th} percentile	1997
25^{th} percentile	1999
Median	2001
75^{th} percentile	2003
90^{th} percentile	2008

Notes: This table reports descriptive statistics of household heads (Panel A) and of the young women followed in the baseline sample (Panel B). 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 A3, where Primary School is anything below Lower Secondary.

Sample Characteristics Table 5 reports sample characteristics, panel A of the household head, and panel B of their daughters. Table D1 reports the distribution of the main countries of origin. Three elements stand out: first, there is a variation in the characteristics of the household head, in particular their educational attainment. Second, it is clear that the location at assignment is constrained. Asylum seekers settle in far fewer distinct neighborhoods at assignment (441 different neighborhoods) than when the outcome of their daughter starts being measured, i.e., at the age of 16 (1652 different neighborhoods). Finally, we must consider the sample of girls who arrived at a relatively young age, with a mean age at arrival of 7.3 years old.

Table 6: Mobility after assignment

	No. still living in the NL	Neighborhood (%)	Neighborhood or Adjacent (%)	Municipality (%)
After 1 year	6,511	77.7	78.7	79.6
After 2 years	6,470	39.2	42.4	44.8
After 3 years	6,408	23.3	27.5	30.7
After 4 years	6,391	13.3	18.3	22.3
After 5 years	6,404	9.1	14.4	18.4
After 6 years	6,432	7.1	12.7	17
After 7 years	6,437	5.9	11.6	16.1
After 8 years	6,444	5.1	11	15.3

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.

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. In Table 6, 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.[19]

After 3 years, 30.7% of the assigned girls are living in the same municipality, and 23.3% in

[19]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.

the same neighborhood. 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. 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. Whether this is enough to expect neighborhood effects is an empirical question, addressed in Sections 6 and 7.

5.2 Measuring Ethnic Concentration

For three reasons, it is not straightforward to define a measure of ethnic concentration: (i) where to set the geographical boundaries (at which geographical level do we aggregate information)? (ii) According to which criteria can we define an influential group? As people from the same country of origin, of from a specific religious group? Finally, (iii) which is more appropriate a relative or an absolute measure?[20]

Geographic and Ethnic Boundaries This choice 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. In particular, young immigrants can identify as being immigrants – versus a native – or can identify with a religious group. Since the potentially influential group is less clear for immigrants who arrived at a younger age, there is a risk to define it ex-post on the basis of the results.

[20]When 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.

To alleviate concerns about tailoring the definition of treatment, I group individuals according to dimensions highlighted as important in the literature. I focus on three: country of origin, Muslim background, and natives. The first one is the most natural starting point, the second is relevant for the situation (and public debate) in Western Europe (Bisin et al., 2008; Adida et al., 2014a; Bansak et al., 2016), and the third one is more neutral (for all immigrants, regardless of their origin, natives are an out-group).[21] I use the granular nature of the data and focus on close neighborhoods (zip4). To calculate ethnic concentration, I pool together first- and second-generation immigrants living in COA facilities as well as in the rest of the neighborhood.

Absolute and Relative Measure They correspond to different models of social interactions. In the case of relative measures (such as shares), an implicit assumption is that all neighbors have an equal chance to interact (all part of the denominator). This is well suited to measure the prevalence of a sub-group in a neighborhood. On the other hand, absolute measures specify a subgroup with whom one is more likely to interact. It then matters if the group is big or small, regardless of the neighborhood size.

Therefore, shares are well suited for larger groups, such as natives and Muslim immigrants and absolute measures for smaller groups, such as countries of origin. This is why I define treatment in three different ways: shares of natives, share of Muslims and log of immigrants from the same country of origin.[22] [23]

These different measures convey different information. While the share of natives and Muslims is highly (negatively) correlated share and log measures are weakly correlated, i.e., 0.26-0.27 (see Table D3). The different measures capture different dimensions of ethnic concentration. The fact that findings coincide for different definitions of treatment is an indication of the robustness

[21] In section 6.2, I show that the baseline ITT results hold when using Generalized Random Forests. These techniques provide confidence intervals that are not inflated by the risk of multiple hypothesis testing. This ensures that there is no over-rejection due to the definition of treatment.

[22] More precisely, I use ln(1+no. of same country). Using an Inverse Hyperbolic Sine (IHS, such that $IHS(x) = \ln(x + \sqrt{x^2 + 1})$) transformation gives almost numerically identical results and is correlated almost one with the log measure, see Table D3.

[23] Note that, on the contrary, looking at the effect of the number (absolute measure) of natives or Muslims would correlate with size of the neighborhood, while the share (relative measure) of immigrants from the same origin would be very small. The 75^{th} percentile of the distribution of people from the same country is 30 people (a few families) in an neighborhood of 3,413 inhabitants on average. Looking at the changes expressed in percentage points would focus on an irrelevant marginal policy. Indeed, most asylum seekers come from countries for which the existing stock of immigrants in the Netherlands was very small.

of the results.

Variation in Ethnic Concentration at Assignment Table 7 reports summary statistics about COA accommodation characteristics (Panel A), namely, the number of asylum seekers living there and the time spent by asylum seekers 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 concentration (measured with the three different variables) both at the time of assignment and when the daughter reaches age 16. This table shows two important things: first there is variation in ethnic concentration at the time of arrival (variation that will be used for identification). Second, although concentration is lower 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 the age 16 and at assignment. Identification of neighborhood effects exploits a variation similar in magnitude to what is observed outside the experimental setting.

5.3 Balancing Tests

The identifying assumption is that conditional on the variables used to allocate asylum seekers to their accommodation, neighborhood ethnic concentration 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; in particular the year of arrival and the country of origin, to be used as controls.

To test the exogeneity of assignment, I regress ethnic concentration at the time of assignment on characteristics of the household head.[24] 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 sample). I estimate the following equation:

[24]The head is identified as the father of the girl if he arrived at the same time; otherwise, it is the mother. Unaccompanied children are not associated with a head and thus are not included in the balancing tests.

Table 7: Descriptive Statistics Accommodation and Ethic Composition

Panel A: Information on Accommodations		
	Time spent	
25^{th} percentile	375	
Median	540	
75^{th} percentile	985	
Mean	819	
	Nb of people	
25^{th} percentile	11	
Median	106	
75^{th} percentile	217	
Mean	139.77	
Panel B: Information Ethnic Composition Surroundings		
	At Arrival	At Age 16
	Log of Country	
25^{th} percentile	1.39	2.30
Median	2.64	3.09
75^{th} percentile	3.50	3.89
Mean	2.40	3.14
	Share of Natives	
25^{th} percentile	76.88	70.55
Median	85.04	81.63
75^{th} percentile	90.64	88.60
Mean	80.41	77.75
	Share of Muslims	
25^{th} percentile	2.34	2.90
Median	5.11	6.77
75^{th} percentile	10.14	14.54
Mean	8.79	10.64

Notes: This table reports characteristics of COA accommodation and neighborhoods where young women stayed. Panel A details the number of asylum seekers registered at the COA at the time of arrival and the time spent at that location. Panel B reports the distribution of ethnic community size at arrival and when the girl turns 16. for all three measures of concentration.

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

where $\text{concentration}_{i,t,h}$ refers to the three variables used to account for ethnic concentration experienced by individual i who was assigned to neighborhood h in year t. θ_t are year of arrival fixed effects and λ_c are country of origin fixed effects.[25] The standard errors are clustered at the origin country and regional level.[26] The results for concentration at the neighborhood level are reported in Table 8. For each measure of concentration, I report results with and without (Dutch) region fixed effects (since they are used in the main ITT specification). I also report the F-test of equality of all the education dummies, together with the p-value associated with this test. I focus primarily on education, since a first-order concern would be that household heads are allocated to places more favorable towards their employment prospect (something that could correlate with ethnic composition).

It is clear from Table 8 that neighborhood composition is not correlated with observable characteristics. More educated asylum seekers are not less likely to live in an ethnically concentrated area. If anything, they seem to be more likely. The p-values of the F-tests are above the standard rejection levels. Note that they are slightly better when region fixed effects are included. Individual coefficients on "Male", "No. of Children" and, "Age at Migration" are also not statistically significant. There is no evidence of sorting at assignment. In Section D.1, 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.

5.4 Naive Estimation

Before providing causal estimates, I show the results of a naive regression where concentration at age 16 is taken as exogenous. The specifications mirror those of the baseline results (detailed below) and are reported in Table 9. Comparing causal and naive estimations helps us to understand the direction of the bias and the direction of the public debate.

[25] When looking at the log of immigrants from the same country, I restrict the sample to the fifteen main countries of origin, as listed in Table D1.

[26] See discussion in footnote 28.

Table 8: Balancing Test

	Share Natives		Share Muslims		Log Country	
Male	0.448	0.422	-0.322	-0.214	-0.001	-0.013
	(0.595)	(0.488)	(0.425)	(0.349)	(0.048)	(0.046)
Nb of children	0.041	0.082	0.049	0.033	-0.012	-0.014
	(0.210)	(0.192)	(0.139)	(0.124)	(0.017)	(0.015)
Age at Migration	0.025	0.024	-0.024	-0.021	-0.002	-0.002
	(0.034)	(0.028)	(0.023)	(0.020)	(0.003)	(0.002)
Primary School	-0.630	-0.520	0.713	0.448	-0.012	-0.023
	(0.696)	(0.542)	(0.515)	(0.394)	(0.060)	(0.058)
Lower Secondary	0.651	0.630	-0.552	-0.574	0.003	0.010
	(0.750)	(0.613)	(0.543)	(0.434)	(0.067)	(0.066)
Upper Secondary	-0.293	-0.203	0.431	0.245	-0.105	-0.114*
	(0.935)	(0.830)	(0.681)	(0.597)	(0.066)	(0.064)
College	0.097	-0.092	-0.093	-0.086	-0.116	-0.137*
	(0.834)	(0.705)	(0.615)	(0.515)	(0.082)	(0.078)
Above College	-0.995	-0.111	0.918	0.078	-0.003	-0.035
	(1.110)	(0.945)	(0.755)	(0.621)	(0.070)	(0.066)
No. of Obs	5,227	5,227	5,227	5,227	4,288	4,288
R squared	0.12	0.41	0.08	0.39	0.28	0.35
F Test	1.04	0.96	2.08	1.56	1.27	1.64
P Value	0.39	0.44	0.07	0.17	0.28	0.15
Assignment Year FE	YES	YES	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES	YES	YES
Regional FE	NO	YES	NO	YES	NO	YES

Notes: This table estimates equation 2 on the sample of household heads (of women from the experimental population with complete spells). Ethnic concentration is measured in three different ways: share of natives, share of Muslims, and log of the number of immigrants from the same country in the neighborhood. It is measured the year of assignment by the COA. Explanatory variables include the country of origin and the fixed effects for the assignment year, together with the gender of the head, the age of the household head, the number of children below 16, and the number of dummies for education levels, where the baseline category is lacking observation (15% of the sample). For each measure of concentration, I report specifications with and without regional fixed effects. Standard errors are clustered at the country of origin and at regional level. F-test reports the test statistics of the null hypothesis: the coefficients for all education levels are zero.

There is a negative association between large ethnic communities and the probability of using hormonal contraceptives. Compared to the main null effect, this is indicative that sorting into neighborhoods biases the estimates downwards. More conservative immigrants tend to live in ethnic enclaves. This is also in line with the public perception that ethnic enclaves slow down cultural assimilation.

Table 9: Naive Estimation - Contraceptive Usage

Share of Natives			
Concentration	0.0007	0.0008*	0.0009*
	(0.00042)	(0.00043)	(0.00047)
N Obs	6,505	6,505	6,505
Mean Outcome	0.41	0.41	0.41
R squared	0.070	0.073	0.074
Share of Muslims			
Concentration	-0.0011**	-0.0013**	-0.0015**
	(0.00057)	(0.00058)	(0.00065)
N Obs	6,505	6,505	6,505
Mean Outcome	0.41	0.41	0.41
R squared	0.071	0.074	0.074
Ln of Country			
Concentration	-0.0225***	-0.0218***	-0.0215***
	(0.00703)	(0.00715)	(0.00727)
N Obs	5,378	5,378	5,378
Mean Outcome	0.40	0.40	0.40
R squared	0.057	0.062	0.062
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	NO	YES	YES
Average Contraceptive Usage	NO	NO	YES

Notes: This table reports estimations of equation 3 (the main regressor being concentration at the time a girl turns 16 y.o.). The outcome variable is a dummy for having taken hormonal contraceptives at least once by the age of 20. All specifications control for country of origin, year of assignment and regional 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 hormonal contraceptives at the municipality level. The sample only includes teenage women with a complete spell. Standard errors are clustered at the origin countries and regions. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

6 Empirical Analysis

6.1 Baseline Results

I estimate the following equation:

$$y_i = \alpha + \beta \text{ concentration}_{i,h,t} + \pi X_i + \lambda_c + \theta_t + \xi_r + \epsilon_{i,t} \tag{3}$$

where y_i is a dummy for having taken contraceptives at least once by the age of 20, concentration$_{i,h,t}$ is the concentration experienced by individual i in neighborhood h at the beginning of year t. I use three values for concentration: share of natives, share of Muslims and log of people from the same country. 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 is year of arrival fixed effect, λ_c is country of origin fixed effects, and ξ_r are region fixed effects.[27] The latter control for time-invariant regional differences, in particular the existence of a "bible belt" in the Netherlands. The baseline specification follows an ITT strategy in which I use a neighborhood's ethnic concentration at the time of assignment. The standard errors are clustered at the country of origin and at regional level. Results are robust to alternative choices of cluster level.[28]

The results are reported in Table 10. Column 1 includes individual controls, and column 2 adds family controls. Column 3 adds the predicted share of contraceptive usage among women aged 16 to 20 at the municipality level, to control for a finer geographical information than

[27] These are the COROP (Coördinatiecommissie Regionaal Onderzoeksprogramma) regions, see Figure A1. They split the Netherlands into 40 areas. They correspond to the NUTS3 level, comparable, for instance to "départements" in France (as used in Algan et al. (2021)). It is common in the literature to include location fixed effects (Aslund et al., 2011; Damm and Dustmann, 2014; Algan et al., 2021). An alternative would be to use municipality fixed effects. As shown in Table A4, there are very few treated individuals per municipality. Including municipality FE would greatly reduce the variation used for identification. This is why I use regional FE instead.

[28] Standard errors are usually clustered at the treatment level. However, the treatment level is different when concentration is measured by share (defined at the neighborhood level) or country of origin (defined at the neighborhood/country of origin level, since treatment is different for each country of origin). Choosing the latter increases the number of clusters which goes against common practice. To stay in line with the choice of FE, I cluster the standard errors at the country of origin and regional level and provide evidence that inference is unchanged when using different levels.

region fixed effects.[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. In the absence of detailed information on the assignment algorithm, an agnostic approach to select controls also seems appropriate.

The ITT results are very small in magnitude and not statistically significant. Remember from Table 4 that arriving one year younger was roughly associated with a 1 percentage point increase in contraceptive usage. Coefficients in my preferred specification (the last column) are between 10 and 100 times smaller.

6.2 Heterogeneity and Potential Mechanisms

The null effect can be the average between a positive and a negative result. It can also be on average very small but significant for some relevant subgroups. Alternatively, the absence of effects could come from a flaw in the experimental design; either if girls (i) do not stay long enough at their assigned neighborhood or (ii) were too young when they arrived. Looking at treatment effect heterogeneity can unveil these scenarios. It could also be indicative of potential mechanisms at play.

I look at four dimensions of heterogeneity: (i) age at arrival (above/below 9; the rounding down of the 75^{th} percentile, see Table 5), (ii) whether she stayed in a COA accommodation for a period longer or shorter than a year, (iii) number of asylum seekers in the same COA location (more than 100 asylum seekers; closest threshold to the median size, see Table 7) and (iv) the education level of her household head (i.e., college or above).

Age at arrival captures whether exposure to co-ethnics is more important during childhood or closer to teenage years. Focusing on girls who arrived at an age older than 9 also addresses the concern that asylum seekers were too young when they arrived (to detect an effect). The level of education of the household head is a key family characteristic that can mediate an effect.

[29] I regress individual level data on contraceptive usage for the period 2006 - 2019 on municipality fixed effects and predict usage.

Table 10: ITT Estimation Contraceptive Usage

Share of Natives				
Concentration	-0.0001	-0.0001	-0.0002	-0.0003
	(0.00051)	(0.00052)	(0.00052)	(0.00051)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41	0.41
R squared	0.071	0.076	0.076	0.035
Share of Muslims				
Concentration	-0.0002	-0.0002	-0.0001	-0.0001
	(0.00072)	(0.00073)	(0.00074)	(0.00069)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41	0.41
R squared	0.071	0.076	0.076	0.034
Log of Country				
Concentration	0.0022	0.0012	0.0012	-0.0009
	(0.00603)	(0.00606)	(0.00604)	(0.00617)
No. of Obs	5,391	5,391	5,391	5,391
Mean Outcome	0.40	0.40	0.40	0.40
R squared	0.056	0.064	0.064	0.044
Regional FE	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	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 of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. All specifications control for country of origin, assignment year together with regional fixed effects, and 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 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 countries of origin and regions. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same country of origin.

Focusing on a duration of more than a year removes short stays and alleviates the concern that young girls stayed in the assigned neighborhood for too short a time. Looking at the number of asylum seekers tells us if specific accommodation dynamics are at play. For all of these dimensions, the threshold chosen to create a binary classification can be modified. As explained below, inference is robust to splitting the sample into different groups.

I use specific econometric techniques, generalized random forests, or GRF (Athey et al., 2019), to account for multiple hypothesis testing when looking at treatment effect heterogeneity. GRF 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 us 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. The variables fed to the GRF algorithm are those used in the third column of Table 10.

Conditional average treatment effects (CATE) are reported in Table 11, which reports coefficients, standard errors together with the size of the subgroup. The last row reports the unconditional effect (on the entire sample) which can be thought of as the grf counterpart to the ITT estimated by OLS. There is no evidence that an average null result hides a larger effect on a meaningful sub-population. Coefficients are very small; only one out of fifty-four is significant at the 10% level. CATE are very small for girls who arrived after the age of 9, girls who stayed at least a year, and girls to whom both applies. This alleviates concerns that the (absence of) effect is driven by a flaw in the experimental design.

6.3 Robustness Checks

6.3.1 Alternative Specifications

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 E1, I reproduce the baseline analysis

Table 11: Treatment Effect Heterogeneity - Generalized Random Forests - Contraceptive Usage

	Share of Natives			Share of Muslims			Log of Country		
	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs
Age migration < 9	0.0004	(0.00077)	4,519	-0.0008	(0.00132)	4,519	-0.0004	(0.00713)	3,800
Age migration > 9	0.0001	(0.00090)	2,006	0.0003	(0.00125)	2,006	0.0006	(0.01251)	1,591
Father low education	0.0005	(0.00071)	5,416	-0.0010	(0.00116)	5,416	-0.0010	(0.00714)	4,479
Father high education	-0.0008	(0.00152)	1,109	0.0017	(0.00217)	1,109	-0.0021	(0.01329)	912
Length of stay > 1 year	0.0004	(0.00075)	5,042	-0.0008	(0.00126)	5,042	-0.0016	(0.00691)	4,267
Center larger than 100	0.0005	(0.00081)	3,219	-0.0001	(0.00137)	3,219	0.0014	(0.01116)	2,564
Center smaller than 100	-0.0003	(0.00095)	3,306	-0.0010	(0.00139)	3,306	-0.0031	(0.00798)	2,827
Low education & long stay	0.0010	(0.00081)	4,151	-0.0023*	(0.00132)	4,151	-0.0024	(0.00769)	3,528
High education & long stay	-0.0020	(0.00174)	891	0.0026	(0.00246)	891	-0.0006	(0.01473)	739
Large center & long stay	0.0007	(0.00103)	2,202	-0.0007	(0.00171)	2,202	-0.0014	(0.01275)	1,840
Small center & long stay	0.0002	(0.00103)	2,840	-0.0017	(0.00154)	2,840	-0.0029	(0.00861)	2,427
Young & low education	0.0011	(0.00087)	3,676	-0.0019	(0.00142)	3,676	-0.0015	(0.00797)	3,091
Young & high education	-0.0023	(0.00195)	843	0.0037	(0.00257)	843	-0.0069	(0.01586)	709
Young & long stay	0.0000	(0.00087)	3,899	-0.0007	(0.00135)	3,899	-0.0010	(0.00748)	3,313
Old & low education	-0.0003	(0.00097)	1,740	0.0003	(0.00140)	1,740	0.0011	(0.01439)	1,388
Old & high education	0.0035	(0.00240)	266	-0.0021	(0.00336)	266	0.0070	(0.02503)	203
Old & long stay	0.0015	(0.00130)	1,143	-0.0026	(0.00188)	1,143	-0.0032	(0.01662)	954
Average Effect (ITT)	0.0002	(0.00063)	6,525	-0.0005	(0.00111)	6,525	0.0007	(0.00638)	5,391

Notes: This table reports the conditional average partial effects estimated using Generalized Random Forests (Athey et al., 2019). The outcome variable is having used contraceptives at least once by the age of 20. The following variables are used to build causal forests: country of origin, year of assignment, age at migration, household head characteristics (including education), region of assignment, and predicted municipal usage rate. Each row reports the effects of ethnic concentration (measured in three different ways) on a specific subgroup. For each measure and dimension of heterogeneity, I report CATE, standard errors, and the number of people in the sample who belonged to each specific subgroup. The last row reports the average partial effect on the entire sample, which has a similar interpretation to the baseline ITT estimates. Standard errors are clustered at the origin country and regional level.

but use an exponential MLE. The results are very similar to linear specifications.

IV Strategy Instead of estimating an ITT where the main coefficient is concentration at assignment, I follow an IV strategy where concentration at age 16 is instrumented by concentration at assignment. The results are reported in Table E2. While the F-tests of the first stage are large (larger than 10 for most specifications when concentration is measured with share of natives or log of co-nationals), the results are small in magnitude and not statistically significant.

The IV estimation for duration analysis, reported in Table E3, is a control function (Coviello et al., 2015). The results are very similar to linear specifications.

Performing the Analysis at the Municipality Level The neighborhood may not be the relevant level. It could be too small and miss part of the social interactions. Table E4 reproduces the baseline analysis when concentration is calculated at the municipality level rather than at the neighborhood level. These results confirm the baseline estimates.

Alternative Cluster Level Table E5 reports estimates from column 4 of Table 10 (my preferred specification) with different levels of clustering. More specifically, I report the following clustering levels: region and country of origin, region only, municipality only, municipality and country of origin, neighborhood only, neighborhood and country of origin, and robust standard errors. These are very similar between models and do not change the inference on the results.

Non-linearity in Community Size I reproduce Table 10 but interact concentration with a dummy for above the median concentration level (of the unconditional sample distribution summarized in Table 7). This is to ensure that concentration does not have an effect merely above a certain threshold. These results are reported in Table E6 and do not point in that direction.

6.3.2 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 thems correspond to collective housing (as compared to the baseline sample where all of them had to). The algorithms are detailled 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. Results are robust to these variations (see Table E7). The incomplete spell sample is substantially larger (+20%). I reproduce the heterogeneity analysis on that sample and report it in Table E9. Certain subgroups (in particular, girls who arrived after the age of 9) are now much larger (4,005 observations compared to 2,006 in Table 11). Heterogeneity results on that large sample still point to the absence of effect.

6.3.3 Alternative Identification Strategy

To provide further evidence supporting the absence of effect of ethnic concentration on cultural assimilation, I tackle the same question using an alternative strategy. Instead of relying on the quasi-random allocation of asylum seekers to neighborhoods in the Netherlands, I use within family variation (comparison between sisters) in ethnic concentration. I am not constrained to following asylum seekers and can include all first- and second-generation non-Western immigrants.

I estimate the following equation:

$$y_{i(f)} = \alpha + \lambda_f + \sum_{j=5}^{15} \beta_j \, \text{concentration}_{i(f),j} + \epsilon_{i(f)} \tag{4}$$

where $y_{i(f)}$ is a dummy variable for having used contraceptives at least once by the age of 20 years old for individual i in family f. λ_f is a family fixed effect, and $\text{concentration}_{i(f),j}$ are measures of ethnic concentration of the neighborhoods where girls lived between the ages of 5

to 15.[30] Standard errors are clustered at the family level. Identification comes from variation in ethnic concentration experienced by sisters at the same age (but in different years). I exploit variation coming both from families moving to a new neighborhood and families staying in a neighborhood where the ethnic concentration changes.[31]

The identifying assumption is that sorting into neighborhoods is time-invariant within families. This approach complements the previous one in several dimensions: it shows that the main result holds (i) when the sample size becomes much larger, (ii) when the population is more representative of all young immigrants who grew up in the Netherlands (as compared to only asylum seekers), and (iii) when neighbors are people one chose and with whom interaction is potentially different than in the quasi-experimental setting used. The results are detailed in Appendix I and also point to the absence of a neighborhood effect.

6.4 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 quasi-experiment on asylum seekers) driven by selection into treatment? There are two main reasons why asylum seekers are absent from 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 18).

Appendix I 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

[30] I chose five years old as the starting age to increase the sample size. Results are robust to using other thresholds.

[31] I depart from Chetty and Hendren (2018a). In particular, focusing only on movers would greatly decrease my sample size. This would be a drawback since an interesting aspect of this section is to show that the absence of effect holds when the sample size is very large.

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.

7 Additional Results with Other Outcomes

I look at three other outcomes, two that relate to cultural assimilation; i.e., the probability of getting the Dutch citizenship, and the probability of marrying a native, and one that does not; i.e. educational achievement.[32] The purpose of this exercise is twofold: (i) to check that the absence of effect on hormonal contraceptives; carries over to other measures more commonly used in the literature on cultural assimilation and (ii) to show that the experiment used to identify neighborhood effects has power. Finding an effect on a different outcome is an ex-post validation of the study design. In what follows, I follow on the same sample that produced the baseline results, i.e., Table 10. Since few women in the sample are married, I report the results for the outcome "marrying a native" in appendix, see Tables F1 and F2, and focus the discussion on the other two additional outcomes.[33]

7.1 Citizenship Outcome

Why This Outcome? The Netherlands have very restrictive rules when it comes to dual citizenship. In most cases, when one gets the Dutch citizenship, one has to renounce to any other. This is therefore considered a measure of social assimilation. I use registry information collecting Dutch and foreign citizenship and create a binary variable taking value one if an immigrant has the Dutch citizenship by the age of 20.

[32] I 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 male immigrants (Algan et al., 2010). This would be a necessary condition for educational attainment to be given a cultural interpretation.

[33] In the PAP, I pre-registered secondary outcomes (fertility and marriage), which turned out to be ill-suited for my sample, given its age distribution. Therefore I moved to other outcomes that are probably more relevant for teenagers or young adults. Note that for both education and citizenship, I report the results from GRF, which indirectly account for multiple hypothesis testing.

Results The baseline results are reported in Table G1.[34] Coefficients are very small in magnitude and statistically insignificant. I report the heterogeneity analysis in Table G5.[35]

Robustness Results are robust to using alternative samples (see Table G2), and do not change when using a different clustering level (see Table G3) and when using weighted regressions (to address external validity concerns, see Table G4). In all cases, the results point to the absence of effect.

7.2 Educational Outcome

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 several outcome variables based on the highest level attended.[36]

In particular, I create a categorical value based on CBS categorization and a continuous variable based on years of education.[37] The third variable is a binary outcome for having completed general versus vocational training (see Table A3). I use it as the main outcome in this section, to ensure similarity with the baseline results, i.e., contraceptive usage is a binary outcome. All results are robust to using the alternative measures (ordinal and continuous).

[34] In the third column, I use information on the "quality" of zip4 areas provided by the Netherlands Institute for Social Research, the *status score* to account for infra-municipal characteristics. Every four years, this government agency 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.

[35] There is no evidence of heterogeneous effect when concentration is measured as the share of natives or share of immigrants with a Muslim background. There is evidence of a positive effect (larger community size increases the probability of having the Dutch citizenship), among people residing in large collective housing (> 100 people) for more than a year. I interpret this effect has coming from an information channel that runs through co-nationals. The probability of finding co-nationals is greater when accommodation is larger and an increase the length of the stay is associated with more intense contact with that group. In the absence of effect from natives, and given that the heterogeneous effect is limited to a very specific subgroup, I do not consider this as evidence of increased assimilation.

[36] I 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.

[37] Note that it is not straightforward to transform educational attainment in The Netherlands into a continuous variable. This is because of the many different tracks. To have the continuous and categorical variables in line with each other, I constrain years of education to be non-decreasing in the associated categorical variable, see Table A3.

Results Table H1 replicates the ITT baseline analysis while Table H7 reports heterogeneity analysis.[38] Note that I control for "statusscore" in the third column (see footnote 34), meaning I partial out neighborhood quality and vary ethnic composition only. A different picture emerges when looking at educational rather than cultural outcomes. The effect of increased exposure to co-ethnics is negative and significant when using the three measures of concentration.

When looking at heterogeneity results, the average effect is significant for presence of Muslims and significant for meaningful subgroups, mostly highly educated fathers, when concentration is measured with the log of co-nationals. Typically those who would sort out of ethnic enclaves to shelter their children from lower achieving peers and role models. The complementary measure for the presence of natives is also significant at the 5% level overall and is very similar in magnitude among subgroups (although each coefficient is less precisely estimated).

Looking at another outcome not only gives credibility to the quasi-experiment used in this paper. It also provides interesting results about the role played by ethnic enclaves. The patterns of heterogeneity that emerges from Table H7 provides material to understand mechanisms through which ethnic concentration operates. None of these dimensions (and the potential explanations they carry with them) matter when it comes to cultural behavior. If the main focus of the paper was educational achievements and not adoption of cultural behavior, the conclusion would be very different.

Robustness These results are robust to changing the level of clustering (see Table H6), altering the sample (see Table H4), using weighed regressions (to address external validity concerns, see Table H5), using a continuous variable (see Table H3), and an ordered probit specification (see Table H2). Note that Table H7 provides ITT results that are also robust to MHT.[39]

[38] Some individuals are missing in the educational registries, which is why there are fewer observations in Table H1 than in Table 10.

[39] By the age of 20, some girls may not have started the educational level that will ultimately be the highest they attend. To account for this potential measurement error, I group the two highest levels in the ordinal variable (and top-code the continuous one). The results are robust to these changes and available upon request.

8 Discussion and Conclusion

This paper studies whether the cultural behavior of immigrants converges with that of the natives, and whether this process is influenced by neighborhood ethnic composition. My main outcome to measuring cultural assimilation 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. There is evidence of convergence in behavior: immigrants who arrived at a younger age are more likely to use hormonal contraceptives. However, the rate of assimilation is slow. To test whether it can be accelerated by limiting the formation of ethnic enclaves, I exploit the placement of asylum seekers in the Netherlands. While I find that growing up in an environment with a large ethnic community has a negative effect on educational achievements, I do not find an effect on the probability of using contraceptives.

As indicated in the PAP, the main objective of the project was to look at the effects of ethnic concentration on cultural assimilation (measured with contraceptive usage). To give strength to these findings: I had to (i) show that hormonal contraceptives are not inelastic, i.e., usage is sensitive to the length of the stay in the country; and (ii) check that ethnic enclaves have an effect on another outcome to show that the setting used in the paper is a well-powered experiment. It is important to weave well these different results and review what can be learned from these findings (including from the absence of an effect) on three main questions: Do immigrants converge to the native mainstream? Do they benefit from living in ethnic enclaves? And how important are vertical and horizontal dimensions in the assimilation dynamics of young immigrants?[40]

Do immigrants converge to the behaviors of natives? Research on cultural norms and social attitudes provides two consistent sets of apparently conflicting evidence. One the one hand, cultural norms are deeply entrenched and very persistent over the long run (Fernandez, 2007,

[40]In a simple model of Bayesian updating, Abadie (2020) discusses the benefits of reporting non significant results in empirical economics and shows that failing to reject a null hypothesis often brings more information over rejecting it. Given that sample sizes in most empirical work are nowadays large, effects which are small in magnitude can be detected. If the emphasis is placed on the binary decision "reject/failing to reject", the latter can be more informative. The sample size used in the main regressions of this paper is large, and the findings come from a well-powered experiment. Following the argumentation of Abadie (2020), the findings are very informative.

2013; Alesina et al., 2013; Giuliano and Nunn, 2021). Yet they are not immutable. They respond to economic conditions (Giuliano and Spilimbergo, 2014), economic incentives (Algan et al., 2010) and information treatment (Bursztyn et al., 2020). The relevant margin may not be whether cultural norms can change, but how fast they can change, and by how much. The empirical findings of this paper points towards slow convergence; an immigrant who fully grew up in a Western country still holds different views and attitudes. It is realistic that the gap can only be closed after several generations.

Do immigrants benefit from living in ethnic enclaves? Many trade-offs should be taken into account to give a welfare assessment of ethnic enclaves: their effect can be different on parents and children; they can be beneficial to children in some dimensions, but detrimental in others. A larger community can extend the parents' network and help them find a job (Aslund et al., 2003; Damm, 2009; Beckers and Borghans, 2011). It may also provide young immigrants with highly educated role models and improve their educational attainment (Aslund et al., 2011). On the other hand, it may bring them further away from the natives' mainstream cultural standards and expose them to worse social environments (Damm and Dustmann, 2014). It is beyond the scope of this analysis to make welfare claims. Although I do not find an effect of neighborhoods on cultural assimilation, I do find an overall negative effect on educational attainment. This result is in line with a large fraction of the literature (Grönqvist, 2006; Chetty et al., 2016; Chetty and Hendren, 2018b) and is indicative that young immigrants do not benefit from growing up in an ethnic enclave.

How important are horizontal and vertical dimensions? The two main findings of the paper (evidence of slow convergence and absence of effect from the neighborhood) may seem contradictory. If there is evidence of convergence, something must be driving it. The immigrants' environment is not limited to the neighborhood where they live. The media they have access to and the curriculum of the school they attend do not necessarily vary with neighborhood. Deciding to stay in a country is an indirect way of increasing the time spent in contact with its culture. Therefore, the environment as a whole can matter, while the nearby neighborhood is not an important factor. This may sound surprising (and even disappointing) as limiting ethnic enclaves is often advocated as an (easy) mechanism to accelerate cultural assimilation. The strong intergenerational differences between natives and immigrants together with the absence of a neighborhood effect points to the prevalence of vertical over horizontal channels (see also

Schindler and Westcott (2021); Algan et al. (2021)). The preeminence of vertical channels also helps to understand why convergence is not faster.

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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 Perssontab
- 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.[41]

- 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 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)

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.

[41] Note 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.

Figure A1: Map of COROP (Regional FE) in The Netherlands

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
North America	USA Canada
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: Members of the Organisation of Islamic Cooperation

Republic of AZERBAIJAN, Hashemite Kingdom of JORDAN, Islamic Republic of AFGHANISTAN
Republic of ALBANIA, State of The UNITED ARAB EMIRATES
Republic of INDONESIA, Republic of UZBEKISTAN, Republic of UGANDA
Islamic Republic of IRAN, Islamic Republic of PAKISTAN, Kingdom of BAHRAIN
BRUNEI-DARUSSALAM, People's Republic of BANGLADESH, Republic of BENIN
BURKINA-FASO, Republic of TAJIKISTAN, Republic of TURKEY
Turkmenistan, Republic of CHAD, Republic of TOGO, Republic of TUNISIA
People's Democratic Republic of ALGERIA, Republic of DJIBOUTI
Kingdom of SAUDI ARABIA, Republic of SENEGAL, Republic of The SUDAN
SYRIAN Arab Republic, Republic of SURINAME, Republic of SIERRA LEONE
Republic of SOMALIA, Republic of IRAQ, Sultanate of OMAN, Republic of GABON
Republic of The Gambia, Republic of GUYANA, Republic of GUINEA
Republic of GUINEA-BISSAU, State of PALESTINE, Union of The COMOROS
KYRGYZ Republic, State of QATAR, Republic of KAZAKHSTAN
Republic of CAMEROON, Republic of COTE D'IVOIRE, State of KUWAIT
Republic of LEBANON, Libya, Republic of MALDIVES, Republic of MALI, MALAYSIA
Arab Republic of EGYPT, Kingdom of MOROCCO, Islamic Republic of MAURITANIA
Republic of MOZAMBIQUE, Republic of NIGER, Federal Republic of NIGERIA
Republic of YEMEN

Table A3: Categorization of Educational Attainment

Name	Categories	Years of Schooling	Dummy
Praktijkonderwijs	Lower Secondary	12	0
Vmbo-b/k	Lower Secondary	12	0
Mbo1	Lower Secondary	12	0
Vmbo-g/t	Lower Secondary	12	0
Havo-, vwo-onderbouw	Lower Secondary	12	0
Mbo2	Upper Secondary	16	0
Mbo3	Upper Secondary	16	0
Mbo4	Upper Secondary	16	0
Havo-bovenbouw	Upper Secondary	16	1
Vwo-bovenbouw	Upper Secondary	16	1
Hbo-associate degree	College	18	1
Hbo-bachelor	College	18	1
Wo-bachelor	College	19	1
Hbo-master	Above College	20	1
Wo-master	Above College	21	1
Doctor	Above College	24	1

Notes: Taken from the Standaard Onderwijsindeling 2021, Tables 3a-3b

Table A4: Municipality and COROP (Regional FE) - Nb of treated individuals

	Municipality	COROP
10^{th} percentile	2	44.5
25^{th} percentile	5	72.5
Median	17	145
75^{th} percentile	39	214.5
90^{th} percentile	77	308
Mean	29.4	163.1

Notes: I collapse the number of observations in the baseline sample per municipality of assignment (first column) and region of assignment (second column). The median entries should be read as follows: half of the municipalities had fewer than 17 asylum seekers being assigned to them and half of the regions had 145 asylum seekers assigned to them.

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:

   (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 D2) 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 Gemeentewoming, 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 A5 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 A5: Comparing Samples

	Incomplete	First Assignment	Variation 1	Variation 2
Only in Baseline	0	669	78	76
Not at all in Baseline	1,313	669	383	389
In both	6,525	5,856	6,447	6,449

Balancing Tests with Alternative Samples Balancing tests with these alternative samples are summarized in Table D4. I only report the F-test and associated p-values of the null hypothesis; no effect of household head education level on ethnic concentration. They do not raise any alarm when concentration is measured with shares (considering that the baseline ITT specification includes regional fixed effects) and are potentially worrisome for log of co-nationals in variation 1 (the one including individuals who lived is Self-Arranged Accommodation). This validates ex-post the choice to not include them in the main sample.

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 E7 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.[42]
- I restrict the population to those who have at least one daughter with an incomplete spell of contraceptive usage.[43]

[42] This 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.

[43] I perform this check on the incomplete spell sample to maximize the number of observations with non zero weights in Tables E8, G4, H5.

- I restrict the population to Non-Western immigrants or those coming from countries listed in Table D1.
- I keep one observation per potential household head.

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.[44]

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 A6. I also compare the distribution of educational attainment between the two groups. Table A7 reports the observed distribution together with the one that would hold under $\mathbb{H}_0$; both distributions are identical. In Figure A2, 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 A6, the value of the dummy coefficients are small compared to the sample means. In Table A7, the two distributions greatly overlap and the histograms in Figure A2 have the same shape.

Table A6: External Validity - Mean Comparison In and Out of the Experiment

	Male	Nb Children	Age at Arrival
In Experiment	0.010	0.172***	0.988***
	(0.011)	(0.031)	(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 located as living in an eligible COA accommodation (in experiment) and those who could not.

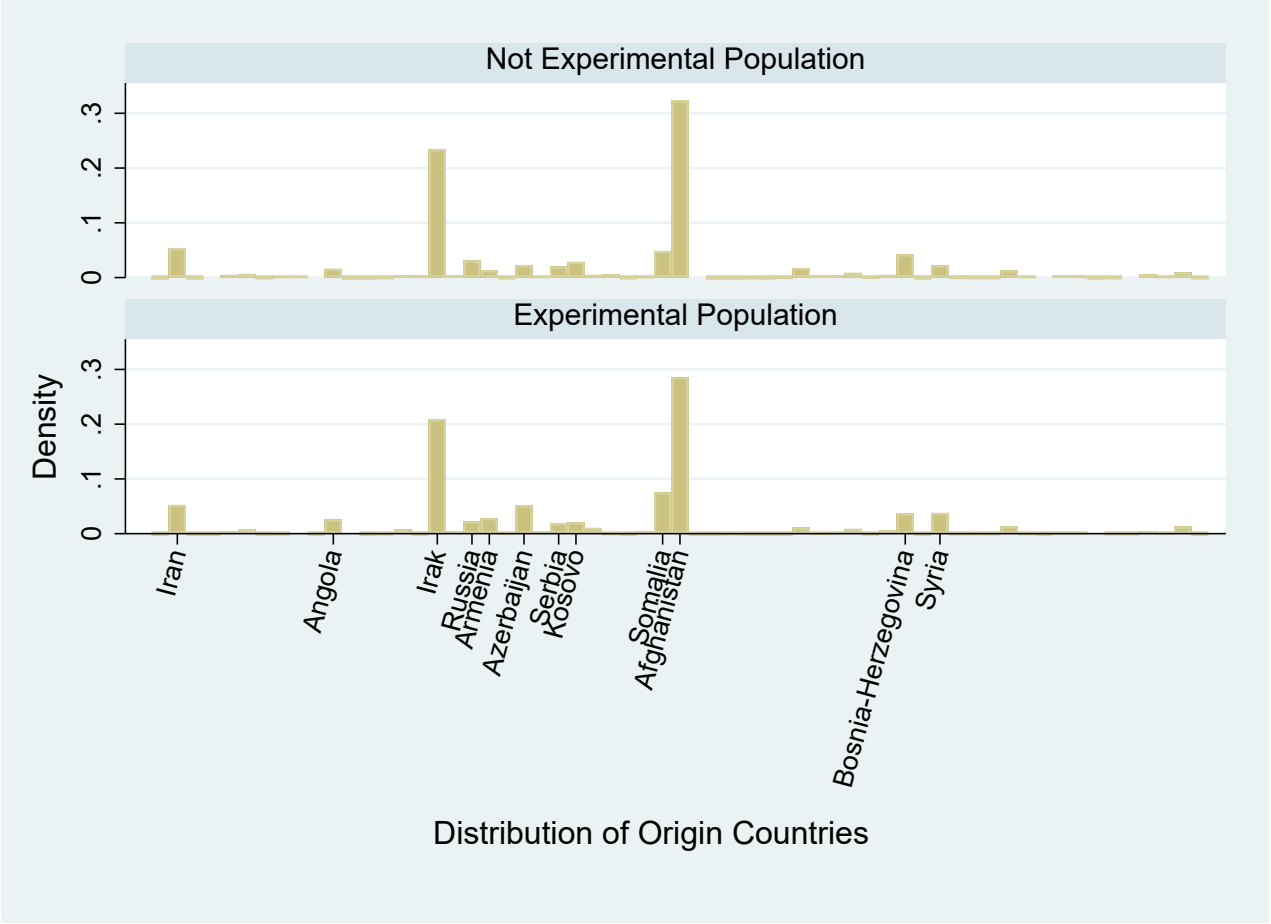
[44]To obtain this number, calculate how many people are identified as being part of the experiment ("experimental" column of Table A7) and divide by the total number of observations in Table A6.

Table A7: External Validity - Education Level of the Household Head

	Not Experimental	Experimental	Total
	Missing		
Observed	514	1,287	1,801
Expected under $\mathbb{H}_0$	476.2	1,324.8	
	Below Primary		
Observed	740	2,199	2,939
Expected under $\mathbb{H}_0$	777.1	2,161.9	
	Primary School		
Observed	496	1,506	2,002
Expected under $\mathbb{H}_0$	529.4	1,472.6	
	Middle School		
Observed	582	1,672	2,254
Expected under $\mathbb{H}_0$	596.0	1,658.0	
	High School		
Observed	280	696	976
Expected under $\mathbb{H}_0$	258.1	717.9	
	Higher Education		
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 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 A2: External Validity - Origin Countries



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 E8.[45]

[45]Note 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 E8 than in Table 10.

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 occurences	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***	-0.12***	- 0.08***	-0.68***	-0.26***	-0.02
	(0.021)	(0.017)	(0.021)	(0.084)	(0.021)	(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**	0.16***	-0.29***
	(0.048)	(0.061)	(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).

C Additional Material on Convergence in Behaviors

As explained in the main text, I use the sample of incomplete spells since it increases the number of observations by 46% as compared to complete spells. This greatly improves the precision of the estimates. Table C1 shows that the results hold when using complete spells only. The baseline coefficient is marginally significant at age 20 but significant at the 5% level for all other ages. A duration analysis, where the hazard rate is modeled as an exponential function also estimates a significant effect (at 5%) of age at migration on the probability to start using contraceptives.

Table C1: Convergence with time - Complete Spells

	Linear Regressions			
Contraceptive Usage	(1)	(2)	Mean Usage	No. of Obs
By Age 16	-0.46**	-0.48*	6.31	3,912
	(0.22)	(0.26)		
By Age 17	-0.79**	-0.94**	12.19	3,912
	(0.28)	(0.34)		
By Age 18	-0.92**	-1.19**	18.33	3,912
	(0.34)	(0.41)		
By Age 19	-0.88**	-1.35**	25.46	3,912
	(0.38)	(0.45)		
By Age 20	-0.70*	-1.11**	32.03	3,912
	(0.40)	(0.47)		
	Duration Model			
	-3.82**	-5.15**	18.87	19,560
	(1.35)	(1.69)		
Family FE	YES	YES		
1^{st} Born Dummy	NO	YES		

Notes: In the first panel, each cell reports the results of a linear regression where the outcome is a dummy variable for having taken contraceptives at least once by a certain age. The explanatory variables include age at arrival (reported coefficients), family fixed effects and in Column (2) a dummy for being the oldest child in a family. The regressions are estimated on the sample of first generation women who were living in the Netherlands between 16 and 20 years old during the period 2006 - 2019. It is limited to families where at least two sisters have a complete spell. The second panel reports coefficients of a non-linear specification where the hazard rate is modeled as an exponential function. Standard errors are clustered at the family level.

Within the sample, some families are more represented depending on the number of sisters being observed in the data. To provide evidence that this does not affect the results, I perform weighted regressions where weights are the inverse of the number of sisters. Results can be found in Table C2. They are almost identical to Table 4.

A concern could be that results are confounded by differential access to health care based

Table C2: Convergence with time - Weighted Regressions

Contraceptive Usage	(1)	(2)	Mean Usage	No. of Obs
By Age 16	-0.54**	-0.60**	6.31	3,912
	(0.22)	(0.26)		
By Age 17	-0.75**	-0.80**	11.54	4,767
	(0.23)	(0.27)		
By Age 18	-1.35***	-1.52***	17.05	5,701
	(0.24)	(0.28)		
By Age 19	-1.17***	-1.47***	24.26	5,701
	(0.28)	(0.32)		
By Age 20	-1.02***	-1.27***	30.99	5,701
	(0.29)	(0.34)		
Family FE	YES	YES		
1^{st} Born Dummy	NO	YES		

Notes: Each cell reports the results of a linear regression where the outcome is a dummy variable for having taken contraceptives at least once by a certain age. The explanatory variables include age at arrival (reported coefficients), family fixed effects and in Column (2) a dummy for being the oldest child in a family. The regressions are estimated on the sample of first generation women who were living in the Netherlands between 16 and 20 years old during the period 2006 - 2019. It is limited to families where at least two sisters were followed from the age of 18 to 20 years old. Observations are weighted such that each family equally contributes to the estimation. Standard errors are clustered at the family level.

on age at migration. For instance, older sisters may not have had time to become familiar with the health system. As already shown in Table 1, this does not seem to be the case. To provide further evidence, I reproduce the analysis restricting the sample to sisters who all arrived below the age of 12. This ensures that even the oldest had at least 8 years before they are finally observed at age 20. Results, reported in Table C3 confirm the picture emerging from Table 4.

I also reproduce the baseline analysis for the outcome having used any drug by the age of 20. This would capture differential access at the extensive margin. Results are insignificant both economically and statistically. They are reported in Table C4 for specifications without a dummy for being first born child (arguably not relevant when looking at all drugs).

Table C3: Convergence with time - Arrived Before Age 12

Contraceptive Usage	(1)	(2)	Mean Usage	No. of Obs
By Age 16	-0.45*	-0.47*	6.67	3,192
	(0.26)	(0.31)		
By Age 17	-0.80**	-0.81**	12.29	3,840
	(0.28)	(0.32)		
By Age 18	-1.40***	-1.54***	17.98	4,471
	(0.29)	(0.33)		
By Age 19	-1.32***	-1.61***	25.23	4,471
	(0.33)	(0.38)		
By Age 20	-1.18***	-1.47***	31.74	4,471
	(0.35)	(0.41)		
Family FE	YES	YES		
1^{st} Born Dummy	NO	YES		

Notes: Each cell reports the results of a linear regression where the outcome is a dummy variable for having taken contraceptives at least once by a certain age. The explanatory variables include age at arrival (reported coefficients), family fixed effects and in Column (2) a dummy for being the oldest child in a family. The regressions are estimated on the sample of first generation women who were living in the Netherlands between 16 and 20 years old during the period 2006 - 2019. It is limited to families where at least two sisters were followed from the age of 18 to 20 years old. Standard errors are clustered at the family level. They must have all immigrated before the age of 12.

Table C4: Convergence Over Time - Any Drug

	Baseline	Weighted	Before 12 y.o.
Age at Migration	-0.0017	-0.0018	-0.0019
	(0.00155)	(0.00159)	(0.00182)
No. of Obs	5,701	5,701	4,471
Mean Outcome	0.94	0.94	0.94
R squared	0.513	0.528	0.505

Notes: Each cell reports the results of a linear regression where the outcome is a dummy variable for having taken any drug by age 20. The explanatory variables include age at arrival and family fixed effects. The regressions are estimated on the sample of first generation women who were living in the Netherlands between 16 and 20 years old during the period 2006 - 2019. It is limited to families where at least two sisters were followed from the age of 18 to 20 years old. The first column reports baseline regressions similar to Table 4, the second column reports weighted regressions (see Table C2) and the third restricts the sample to girls who migrated before 12 years old (see Table C3) . Standard errors are clustered at the family level.

D Additional Material on the Institutional Setting and the Identification Strategy

Table D1: Main Countries of Origin

Country	Share
Afghanistan	22.4
Irak	16.3
Somalia	7.1
Iran	5.9
Azerbaijan	4.3
Russia	4.3
Syria	3.7
Bosnia-Herzegovina	3.6
Armenia	3.3
Angola	2.8
Kosovo	2.1
Serbia	2
Turkey	1.8
Sudan	1.6
Zaire	1.5

Table D2: Types of COA Accomodation

Type of accommodation	Nb of centre appearing at the same address 1	2	3	4	Brief Description
Aanvullende opvang	206	42	12		Collective housing, for emergency when no other location available
Administratief geplaatst	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 Opvangeenhdn	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 geplaatst*, meaning the same 205 addresses appear again twice in the same column under different types of accommodation.

Table D3: Correlation between measures of concentration

	Share Natives	Share Muslims	Ln country	IHS country
Share Natives	1.000			
Share Muslims	-0.931	1.000		
Ln country	-0.264	0.275	1.000	
IHS country	-0.262	0.272	0.998	1.000

D.1 More on balancing

Power of the test To give more credibility to the tests reported in Table 8, 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.). To make sure that individuals do sort into neighborhoods, I focus on those (in the same sample) who do not live at age 16 in the same zip4 as they were assigned. To sum up, I run the same regressions at age 16 that I did at assignment but at a time where asylum seekers could select where to live. Regressions at age 16 should reject the absence of sorting.

Results are reported in Table D5. The picture is very different from the balancing regressions of Table 8. There is clear evidence of sorting. In particular, highly educated people live in neighborhoods with smaller ethnic communities. 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 8 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.

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

The results are reported in Table D6.

Table D4: Balancing Tests with Alternative Samples

	Share Natives		Share Muslims		Log of Country	
	With FE	Without FE	With FE	Without FE	With FE	Without FE
			Incomplete Spells			
F Test	0.91	0.62	1.82	1.31	1.18	1.78
p value	0.48	0.69	0.11	0.26	0.32	0.12
N Obs	6,033	6,033	6,033	6,033	4,984	4,984
			First Assignment			
F Test	1.14	1.12	2.18	1.70	1.43	1.75
p value	0.34	0.35	0.05	0.13	0.21	0.12
N Obs	5,227	5,227	5,227	5,227	4,288	4,288
			Variation 1			
F Test	1.56	0.98	2.51	1.62	1.90	2.24
p value	0.17	0.43	0.03	0.15	0.09	0.05
N Obs	5,487	5,487	5,487	5,487	4,482	4,482
			Variation 2			
F Test	1.07	0.85	2.20	1.63	1.60	1.87
p value	0.38	0.52	0.05	0.15	0.16	0.10
N Obs	5,482	5,482	5,482	5,482	4,497	4,497

Notes: This table estimates equation 2 on the sample of household heads (of women from the experimental population with complete spells). Ethnic concentration is measured in three different ways: share of natives, share of Muslims and log of the number of immigrants from the same country in the neighborhood. It is measured the year of arrival to a COA accommodation. Explanatory variables include country of origin and year of assignment fixed effects together with gender of the head, age of the household head and number of children below 16 and dummies for education levels, where the baseline category is missing observation (15% of the sample). For each measure of concentration (in columns) and each variation of the baseline sample (in rows), I report specifications with and without regional fixed effects. Standard errors are clustered at the country of origin and regional level. I only report F-test of the null hypothesis: the coefficients for all education levels are zero, together with the associated p-value.

Table D5: Balancing Test - Power of the test

	Sh Natives		Sh Muslims		Ln Country	
Male	1.273*	1.441**	-0.864	-0.952**	0.023	0.007
	(0.691)	(0.570)	(0.532)	(0.453)	(0.047)	(0.044)
Nb of children	0.574**	0.327*	-0.378**	-0.218	0.039**	0.047**
	(0.220)	(0.180)	(0.163)	(0.137)	(0.016)	(0.015)
Age	0.009	-0.016	-0.006	0.012	0.003	0.004
	(0.043)	(0.037)	(0.032)	(0.028)	(0.003)	(0.003)
Primary School	-1.089	-1.085*	0.921*	0.920*	0.063	0.050
	(0.729)	(0.616)	(0.530)	(0.477)	(0.061)	(0.057)
Lower Secondary	0.719	1.103*	-0.285	-0.462	0.010	0.001
	(0.795)	(0.620)	(0.567)	(0.458)	(0.059)	(0.051)
Upper Secondary	0.909	0.724	-0.396	-0.293	0.007	0.014
	(0.767)	(0.620)	(0.556)	(0.459)	(0.050)	(0.046)
College	0.574	0.459	0.050	0.116	-0.014	0.006
	(0.941)	(0.811)	(0.724)	(0.636)	(0.067)	(0.065)
Above College	1.074	1.067	-0.342	-0.364	-0.120**	-0.124**
	(0.876)	(0.814)	(0.686)	(0.656)	(0.061)	(0.059)
N Obs	4,532	4,532	4,532	4,532	3,720	3,720
R squared	0.05	0.32	0.05	0.29	0.25	0.33
F Test	2.33	3.77	1.67	2.46	1.90	2.19
P Value	0.04	0.00	0.14	0.03	0.09	0.05
Assignment Year FE	YES	YES	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES	YES	YES
Regional FE	NO	YES	NO	YES	NO	YES

Notes: This table estimates equation 2 on the sample of household heads (of women from the experimental population with complete spells who do not live at age 16 in the neighborhood where they were assigned). Ethnic concentration is measured in three different ways: share of natives, share of Muslims and log of the number of immigrants from the same country in the neighborhood. It is measured the year girls turn 16. Explanatory variables include country of origin and year of assignment fixed effects together with gender of the head, age of the household head and number of children below 16 and dummies for education levels, where the baseline category is missing observation (15% of the sample). For each measure of concentration, I report specifications with and without regional fixed effects. Standard errors are clustered at the country of origin and regional level. F-test reports the test statistics of the null hypothesis: the coefficients for all education levels are zero.

Table D6: Reallocating asylum seekers between addresses

Year	Chi Saquarred Test	Degrees of freedom	Critical Values
1996	160.92	2982	3110
1997	194.67	3198	3331
1998	215.58	4530	4688
1999	274.50	5862	6041
2000	376.88	8850	9070
2001	444.48	8490	8705
2002	228.25	7518	7721
2003	181.72	3810	3955
2004	32.50	2406	2521
2005	54.25	1218	1300
2006	40.64	1290	1375
2007	25.79	966	1039
2008	50.62	1218	1300
2009	67.63	1218	1300
2010	37.33	1470	1560
2011	24.75	1038	1114
2012	28.08	750	815

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 95^{th} 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.

E Additional Material with Contraceptive Usage

Table E1: ITT Estimation - Contraceptive Usage - Duration Analysis

Share of Natives			
Concentration	-0.0004	-0.0002	-0.0005
	(0.00157)	(0.00160)	(0.00161)
No. of Obs	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41
Share of Muslims			
Concentration	-0.0006	-0.0006	-0.0003
	(0.00221)	(0.00224)	(0.00225)
No. of Obs	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41
Log of Country			
Concentration	0.0046	- 0.0002	- 0.0002
	(0.01888)	(0.01887)	(0.01882)
No. of Obs	5,391	5,391	5,391
Mean Outcome	0.4	0.4	0.4
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	NO	YES	YES
Average Contraceptive Usage	NO	NO	YES
LASSO Selection	NO	NO	NO

Notes: This table reports estimations of equation 3. The outcome variable is the age young women, between the ages 16 to 20 start using hormonal contraceptives. All specifications control for country of origin, year of assignment and regional 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 origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

Table E2: IV Estimation - Contraceptive Usage

Share of Natives			
Concentration	-0.0017	-0.0012	-0.0029
	(0.00718)	(0.00749)	(0.00933)
No. of Obs	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41
R squared	0.066	0.074	0.066
F-test 1^{st} Stage	12.89	12.26	8.07
Share of Muslims			
Concentration	-0.0025	-0.0026	-0.0012
	(0.01160)	(0.01194)	(0.01521)
No. of Obs	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41
R squared	0.070	0.076	0.077
F-test 1^{st} Stage	6.99	6.90	4.10
Log of Country			
Concentration	0.0352	0.0205	0.0203
	(0.09849)	(0.10145)	(0.10347)
No. of Obs	5,391	5,391	5,391
Mean Outcome	0.40	0.40	0.40
R squared	0.046	0.059	0.059
F-test 1^{st} Stage	12.14	11.29	10.71
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	NO	YES	YES
Average Contraceptive Usage	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. All specifications control for country of origin, year of assignment and regional 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 model is estimated by instrumental variable where concentration at age 16 is instrumented by concentration at assignment. The sample only includes teenage women with a complete spell. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

Table E3: IV Estimation - Contraceptive Usage - Duration Analysis

Share of Natives			
Concentration	-0.0053	-0.0026	-0.0080
	(0.02211)	(0.02327)	(0.02851)
No. of Obs	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41
F-test 1^{st} Stage	12.89	12.26	8.07
Share of Muslims			
Concentration	-0.0102	-0.0107	-0.0068
	(0.03585)	(0.03699)	(0.04731)
No. of Obs	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41
F-test 1^{st} Stage	6.99	6.90	4.10
Ln of Country			
Concentration	0.0667	-0.0080	-0.0099
	(0.30268)	(0.31351)	(0.31895)
No. of Obs	5,391	5,391	5,391
Mean Outcome	0.4	0.4	0.4
F-test 1^{st} Stage	12.14	11.29	10.71
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	NO	YES	YES
Average Contraceptive Usage	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is the age young women, between the ages 16 to 20 start using hormonal contraceptives. All specifications control for country of origin, year of assignment and regional 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 model is estimated as a control function. In a first stage concentration at age 16 is regressed on concentration at assignment and controls. Residuals are computed and added to a second stage estimation whose results are reported in the table. The sample only includes teenage women with a complete spell. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

Table E4: ITT Estimation - Contraceptive Usage - Concentration at Municipality

Share of Natives				
Concentration	0.0016	0.0018	0.0018	0.0012
	(0.00127)	(0.00128)	(0.00133)	(0.00122)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41	0.41
R-squared	0.071	0.077	0.077	0.032
Share of Muslims				
Concentration	-0.0028	-0.0030	-0.0034	-0.0015
	(0.00205)	(0.00207)	(0.00228)	(0.00194)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41	0.41
R-squared	0.071	0.077	0.077	0.029
Log of Country				
Concentration	0.0002	-0.0011	-0.0015	0.0035
	(0.00592)	(0.00597)	(0.00593)	(0.00551)
No. of Obs	5,391	5,391	5,391	5,391
Degree of Freedom	5316	5301	5300	5359
R-squared	0.056	0.064	0.064	0.041
Regional FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	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 of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. All specifications control for country of origin, year of assignment and regional 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 origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country. All of these measures are made at the municipality level.

Table E5: ITT Estimation - Different Levels of Clustering - Contraceptive Usage

Share of Natives							
Concentration	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003
	(0.00051)	(0.00057)	(0.00052)	(0.00055)	(0.00052)	(0.00054)	(0.00049)
No. of Obs	6,525	6,525	6,525	6,525	6,525	6,525	6,525
No. of Clusters	1,114	40	2,175	222	2,588	449	6,525
Share of Muslims							
Concentration	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
	(0.00069)	(0.00079)	(0.00068)	(0.00080)	(0.00067)	(0.00076)	(0.00067)
No. of Obs	6,525	6,525	6,525	6,525	6,525	6,525	6,525
No. of Clusters	1,114	40	2,175	222	2,588	449	6,525
Log of Country							
Concentration	-0.0009	-0.0009	-0.0009	-0.0009	-0.0009	-0.0009	-0.0009
	(0.00617)	(0.00593)	(0.00614)	(0.00675)	(0.00608)	(0.00674)	(0.00555)
No. of Obs	5,391	5,391	5,391	5,391	5,391	5,391	5,391
No. of Clusters	525	40	1,431	220	1,808	423	5,391

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. The specification and sample definition correspond to those of the fourth column of Table 10. Standard errors are clustered at different level: region and origin country, region only, municipality only, municipality and origin country, neighborhood only, neighborhood and origin country and robust standard errors. For each level of clustering, I report the number of clusters and the number of observations.

Table E6: ITT Estimation - Contraceptive Usage - Non Linear Effect

Share of Natives				
Below Median	-0.0003	-0.0002	-0.0003	-0.0004
	(0.00053)	(0.00054)	(0.00055)	(0.00053)
Above Median	-0.0005	-0.0005	-0.0006	-0.0005
	(0.00063)	(0.00064)	(0.00065)	(0.00062)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41	0.41
R-squared	0.071	0.076	0.077	0.035
Share of Muslims				
Below Median	0.0000	0.0000	0.0001	0.0002
	(0.00076)	(0.00077)	(0.00077)	(0.00073)
Above Median	0.0047	0.0041	0.0040	0.0068
	(0.00430)	(0.00430)	(0.00431)	(0.00428)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.41	0.41	0.41	0.41
R-squared	0.071	0.076	0.076	0.034
Log of Country				
Below Median	0.0018	0.0009	0.0008	-0.0004
	(0.00610)	(0.00613)	(0.00611)	(0.00604)
Above Median	-0.0025	-0.0037	-0.0038	-0.0009
	(0.01113)	(0.01113)	(0.01112)	(0.01118)
No. of Obs	5,391	5,391	5,391	5,391
Mean Outcome	0.40	0.40	0.40	0.40
R-squared	0.056	0.064	0.064	0.039
Regional FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	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 of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. All specifications control for country of origin, year of assignment and regional 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 origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country. Concentration is interacted with a dummy for being above the respective (to each measure) medians of concentration.

Table E7: ITT Estimation - Robustness to Alternative Samples - Contraceptive Usage

	Share of Natives	Share of Muslims	Log of Country
	Incomplete Spells		
Concentration	-0.0005	0.0000	0.0041
	(0.00048)	(0.00063)	(0.00537)
No. of Obs	7,838	7,838	6,569
Mean Outcome	0.40	0.40	0.38
R-squared	0.036	0.035	0.042
	First Assignment		
Concentration	-0.0004	0.0001	-0.0002
	(0.00047)	(0.00065)	(0.00579)
No. of Obs	6,525	6,525	5,391
Mean Outcome	0.41	0.41	0.40
R-squared	0.035	0.032	0.038
	Variation 1		
Concentration	-0.0002	-0.0002	-0.0034
	(0.00049)	(0.00067)	(0.00550)
No. of Obs	6,830	6,830	5,620
Mean Outcome	0.41	0.41	0.40
R-squared	0.039	0.039	0.037
	Variation 2		
Concentration	-0.0003	-0.0003	0.0003
	(0.00049)	(0.00066)	(0.00556)
N Obs	6,838	6,838	5,650
Mean Outcome	0.41	0.41	0.40
R-squared	0.036	0.036	0.037
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	YES	YES	YES
Average Contraceptive Usage	YES	YES	YES
LASSO Selection	YES	YES	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. All specifications use the Double Debiased procedure (Belloni et al., 2014b) to chose control variables among the following: country of origin, year of assignment and regional fixed effects together with age at migration, household head characteristics, predicted share of teenagers using contraceptives at the municipality level. For each measure (in columns) and each variation of the baseline sample (panels), the sample only includes teenage women with a complete spell. Standard errors are clustered at the origin countries and regions level.

Table E8: ITT Estimation - Contraceptive Usage - External Validity - Re weighting

Share of Natives				
Concentration	0.0000	0.0000	-0.0000	-0.0000
	(0.00056)	(0.00057)	(0.00057)	(0.00056)
No. of Obs	5,756	5,756	5,756	5,756
Mean Outcome	0.40	0.40	0.40	0.40
R-squared	0.059	0.065	0.065	0.034
Share of Muslims				
Concentration	-0.0004	-0.0003	-0.0003	-0.0007
	(0.00080)	(0.00080)	(0.00080)	(0.00076)
No. of Obs	5,756	5,756	5,756	5,756
Mean Outcome	0.40	0.40	0.40	0.40
R-squared	0.059	0.065	0.065	0.034
Log of Country				
Concentration	0.0026	0.0016	0.0016	-0.0005
	(0.00617)	(0.00624)	(0.00623)	(0.00619)
No. of Obs	5,327	5,327	5,327	5,327
Mean Outcome	0.40	0.40	0.40	0.40
R-squared	0.055	0.063	0.063	0.041
Regional FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	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 of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. All specifications control for country of origin, year of assignment and regional 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 origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country. Observations are weighted as to give more importance to combinations of origin country/arrival year which are under-represented from what they could be, see Section A.4.

Table E9: Treatment Effect Heterogeneity - Generalized Random Forests - Contraceptive Usage

	Share of Natives			Share of Muslims			Log of Country		
	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs
Age migration < 9	0.0004	(0.00075)	4,901	-0.0005	(0.00116)	4,901	-0.0045	(0.00715)	4,157
Age migration > 9	-0.0006	(0.00072)	4,005	0.0002	(0.00104)	4,005	(0.0050)	0.00735	3,371
Father low education	0.0001	(0.00065)	7,368	-0.0006	(0.00101)	7,368	-0.0012	(0.00622)	6,243
Father high education	-0.0017	(0.00125)	1,538	0.0022	(0.00181)	1,538	-0.0002	(0.01074)	1,285
Length of stay > 1 year	-0.0001	(0.00064)	7,125	-0.0006	(0.00098)	7,125	0.0004	(0.00599)	6,136
Center larger than 100	-0.0001	(0.00069)	4,076	0.0004	(0.00118)	4,076	0.0010	(0.00878)	3,322
Centre smaller than 100	-0.0002	(0.00080)	4,830	-0.0013	(0.00116)	4,830	-0.0031	(0.00682)	4,206
Low education & long stay	0.0005	(0.00072)	5,865	-0.0013	(0.00108)	5,865	-0.0009	(0.00662)	5,076
High education & long stay	-0.0022*	(0.00128)	1,260	0.0033*	(0.00193)	1,260	0.0047	(0.01251)	1,060
Large centre & long stay	-0.0001	(0.00087)	2,942	0.0002	(0.00129)	2,942	0.0007	(0.00999)	2,498
Small centre & long stay	0.0002	(0.00081)	4,183	-0.0013	(0.00119)	4,183	-0.0012	(0.00737)	3,638
Young & low education	0.0008	(0.00082)	3,987	-0.0013	(0.00122)	3,987	-0.0052	(0.00802)	3,381
Young & high education	-0.0029	(0.00179)	914	0.0030	(0.00264)	914	-0.0064	(0.01450)	776
Young & long stay	0.0002	(0.00078)	4,265	-0.0005	(0.00121)	4,265	-0.0047	(0.00756)	3,656
Old & low education	-0.0010	(0.00079)	3,381	0.0005	(0.00111)	3,381	0.0043	(0.00815)	2,862
Old & high education	0.0003	(0.00144)	624	0.0001	(0.00199)	624	0.0069	(0.01388)	509
Old & long stay	-0.0004	(0.00083)	2,860	-0.0005	(0.00117)	2,860	0.0072	(0.00866)	2,480
Average Effect (ITT)	-0.0003	(0.00059)	8,906	-0.0002	(0.00095)	8,906	-0.0006	(0.00557)	7,528

Notes: This table reports the conditional average partial effects estimated using Generalized Random Forests (Athey et al., 2019). The outcome variable is having used contraceptives at least once by the age of 20. The following variables are used to build causal forests: country of origin, year of assignment, age at migration, household head characteristics (including education), region of assignment and predicted municipal usage rate. Each row reports the effects of ethnic concentration (measured in three different ways) on a specific subgroup. For each measure and dimension of heterogeneity, I report CATE, standard errors and the number of people in the sample who belonged to each specific subgroup. The last row reports the average partial effect on the entire sample (girls whose contraceptive usage can be measured from age 18 to 20), which has a similar interpretation to the baseline ITT estimates. Standard errors are clustered at the origin countries and regions level.

F Additional Material with Marriage with a Native

Table F1: ITT Estimation - Marriage with a Native

Share of Natives				
Concentration	0.0015	0.0012	0.0014	0.0014
	(0.00090)	(0.00091)	(0.00096)	(0.00087)
No. of Obs	902	902	902	902
Mean Outcome	0.14	0.14	0.14	0.14
R-squared	0.198	0.229	0.230	0.086
Share of Muslims				
Concentration	-0.0016	-0.0012	-0.0015	-0.0016
	(0.00123)	(0.00124)	(0.00134)	(0.00118)
No. of Obs	902	902	902	902
Mean Outcome	0.14	0.14	0.14	0.14
R-squared	0.197	0.229	0.230	0.085
Log of Country				
Concentration	-0.0080	-0.0103	-0.0104	-0.0101
	(0.01063)	(0.01053)	(0.01056)	(0.01047)
No. of Obs	775	775	775	775
Mean Outcome	0.13	0.13	0.13	0.13
R-squared	0.145	0.178	0.178	0.099
Family FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Status Score (ZIP4)	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having been married with a native Dutch (neither first, nor second generation immigrants). All specifications control for country of origin, year of assignment and regional 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 status score, a neighborhood quality index. 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 includes women followed in Table 10 who ever were married. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

Table F2: ITT Estimation - Robustness to Alternative Samples - Marriage with a Native

	Share of Natives	Share of Muslims	Log of Country
	Incomplete Spells		
Concentration	0.0006	-0.0005	0.0026
	(0.00086)	(0.00107)	(0.00948)
No. of Obs	1266	1266	1102
Mean Outcome	0.13	0.13	0.12
R-squared	0.076	0.076	0.073
	First Assignment		
Concentration	0.0012	-0.0012	-0.0051
	(0.00091)	(0.00119)	(0.01109)
No. of Obs	902	902	775
Mean Outcome	0.14	0.14	0.13
R-squared	0.081	0.080	0.086
	Variation 1		
Concentration	0.0016	-0.0019	-0.0106
	(0.00099)	(0.00133)	(0.01062)
No. of Obs	940	940	807
Mean Outcome	0.14	0.14	0.13
R-squared	0.083	0.078	0.082
	Variation 2		
Concentration	0.0013	-0.0014	-0.0128
	(0.00097)	(0.00131)	(0.01006)
No. of Obs	941	941	810
Mean Outcome	0.13	0.13	0.13
R-squared	0.083	0.080	0.071
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	YES	YES	YES
LASSO Selection	YES	YES	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having been married with a native. All specifications use the Double Debiased procedure (Belloni et al., 2014b) to choose control variables among the following: country of origin, year of assignment and regional fixed effects together with age at migration, household head characteristics, predicted share of teenagers using contraceptives at the municipality level. For each measure (in columns) and each variation of the baseline sample (panels), the sample only includes women who were ever married (from the respective samples). Standard errors are clustered at the origin countries and regions level.

G Additional Material with Dutch Citizenship

Table G1: ITT Estimation - Dutch Citizenship

Share of Natives				
Concentration	0.0002	0.0000	-0.0000	-0.0006
	(0.00045)	(0.00044)	(0.00044)	(0.00042)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.74	0.74	0.74	0.74
R-squared	0.231	0.246	0.246	0.181
Share of Muslims				
Concentration	-0.0004	-0.0002	-0.0001	0.0007
	(0.00059)	(0.00058)	(0.00058)	(0.00054)
No. of Obs	6,525	6,525	6,525	6,525
Mean Outcome	0.74	0.74	0.74	0.74
R-squared	0.231	0.246	0.246	0.180
Log of Country				
Concentration	0.0017	0.0029	0.0033	0.0035
	(0.00504)	(0.00497)	(0.00493)	(0.00481)
No. of Obs	5,391	5,391	5,391	5,391
Mean Outcome	0.76	0.76	0.76	0.76
R-squared	0.227	0.240	0.240	0.203
Regional FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Status Score (ZIP4)	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having the Dutch citizenship by the age of 20. All specifications control for country of origin, year of assignment and regional 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 status score, a neighborhood quality index. 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 includes women followed in Table 10. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

Table G2: ITT Estimation - Robustness to Alternative Samples - Dutch Citizenship

	Share of Natives	Share of Muslims	Log of Country
	Incomplete Spells		
Concentration	-0.0009**	0.0012**	0.0021
	(0.00039)	(0.00050)	(0.00460)
No. of Obs	7,838	7,838	6,569
Mean Outcome	0.73	0.73	0.74
R-squared	0.203	0.202	0.220
	First Assignment		
Concentration	-0.0004	0.0006	0.0030
	(0.00041)	(0.00054)	(0.00503)
No. of Obs	6,525	6,525	5,391
Mean Outcome	0.74	0.74	0.76
R-squared	0.179	0.179	0.204
	Variation 1		
Concentration	-0.0007	0.0007	0.0039
	(0.00040)	(0.00052)	(0.00480)
No. of Obs	6,830	6,830	5,620
Mean Outcome	0.75	0.75	0.76
R-squared	0.182	0.181	0.206
	Variation 2		
Concentration	-0.0005	0.0005	0.0043
	(0.00042)	(0.00054)	(0.00459)
No. of Obs	6,838	6,838	5,650
Mean Outcome	0.75	0.75	0.76
R-squared	0.185	0.184	0.205
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	YES	YES	YES
Status Score (ZIP4)	YES	YES	YES
LASSO Selection	YES	YES	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having the Dutch citizenship by the age of 20. All specifications use the Double Debiased procedure (Belloni et al., 2014b) to choose control variables among the following: country of origin, year of assignment, regional fixed effects, age at migration, household head characteristics, the status score (a neighborhood quality index). For each measure (in columns) and each variation of the baseline sample (panels), the sample only includes teenage women with a complete spell. Standard errors are clustered at the origin countries and regions level.

Table G3: ITT Estimation - Different Levels of Clustering - Dutch Citizenship

Share of Natives							
Concentration	-0.0006	-0.0006	-0.0006	-0.0006	-0.0006	-0.0006	-0.0006
	(0.00042)	(0.00061)	(0.00042)	(0.00045)	(0.00042)	(0.00045)	(0.00039)
No. of Obs	6,525	6,525	6,525	6,525	6,525	6,525	6,525
No. of Clusters	1,114	40	2,175	222	2,588	449	6,525
Share of Muslims							
Concentration	0.0007	0.0007	0.0007	0.0007	0.0007	0.0007	0.0007
	(0.00054)	(0.00078)	(0.00057)	(0.00060)	(0.00056)	(0.00054)	(0.00054)
No. of Obs	6,525	6,525	6,525	6,525	6,525	6,525	6,525
No. of Clusters	1,114	40	2,175	222	2,588	449	6,525
Log of Country							
Concentration	0.0033	0.0033	0.0033	0.0033	0.0033	0.0033	0.0033
	(0.00496)	(0.00453)	(0.00497)	(0.00466)	(0.00503)	(0.00461)	(0.00434)
No. of Obs	5,391	5,391	5,391	5,391	5,391	5,391	5,391
No. of Clusters	525	40	1,431	220	1,808	423	5,391

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having the Dutch citizenship by the age of 20. The specification and sample definition correspond to those of the fourth column of Table G1. Standard errors are clustered at different level: region and origin country, region only, municipality only, municipality and origin country, neighborhood only, neighborhood and origin country and robust standard errors. For each level of clustering, I report the number of clusters and the number of observations.

Table G4: ITT Estimation - Dutch Citizenship - External Validity - Re weighting

Share of Natives				
Concentration	0.0000	-0.0001	-0.0001	-0.0005
	(0.00046)	(0.00045)	(0.00045)	(0.00047)
No. of Obs	5756	5756	5756	5756
Mean Outcome	0.76	0.76	0.76	0.76
R-squared	0.235	0.247	0.247	0.192
Share of Muslims				
Concentration	-0.0002	-0.0001	-0.0000	0.0006
	(0.00058)	(0.00057)	(0.00058)	(0.00057)
No. of Obs	5756	5756	5756	5756
Mean Outcome	0.76	0.76	0.76	0.76
R-squared	0.235	0.247	0.247	0.192
Log of Country				
Concentration	0.0027	0.0036	0.0039	0.0033
	(0.00505)	(0.00498)	(0.00496)	(0.00493)
No. of Obs	5327	5327	5327	5327
Mean Outcome	0.76	0.76	0.76	0.76
R-squared	0.233	0.245	0.245	0.216
Regional FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Status Score (ZIP4)	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having taken contraceptives at least once by the age of 20. All specifications control for country of origin, year of assignment and regional 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 status score (a neighborhood quality index). 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 includes teenage women followed in Table G1. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country. Observations are weighted as to give more importance to combinations of origin country/arrival year which are under-represented from what they could be, see section A.4.

Table G5: Treatment Effect Heterogeneity - Generalized Random Forests - Dutch Citizen

	Share of Natives			Share of Muslims			Log of Country		
	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs
Age migration < 9	-0.0003	(0.00063)	4,519	0.0005	(0.00088)	4,519	0.0069	(0.00567)	3,800
Age migration > 9	0.0007	(0.00098)	2,006	-0.0014	(0.00133)	2,006	0.0103	(0.01129)	1,591
Father low education	0.0001	(0.00061)	5,416	-0.0005	(0.00090)	5,416	0.0098	(0.00604)	4,479
Father high education	0.0002	(0.00097)	1,109	0.0000	(0.00132)	1,109	0.0029	(0.00898)	912
Length of stay > 1 year	0.0001	(0.00062)	5,042	-0.0004	(0.00092)	5,042	0.0059	(0.00591)	4,267
Center larger than 100	0.0003	(0.00077)	3,219	0.0001	(0.00106)	3,219	0.0268***	(0.00987)	2,564
Centre smaller than 100	-0.0002	(0.00075)	3,306	-0.0008	(0.00110)	3,306	0.0007	(0.00715)	2,827
Low education & long stay	0.0001	(0.00068)	4,151	-0.0002	(0.00099)	4,151	0.0093	(0.00657)	3,528
High education & long stay	0.0008	(0.00108)	891	-0.0010	(0.00147)	891	-0.0080	(0.00974)	739
Large centre & long stay	0.0002	(0.00092)	2,202	0.0007	(0.00120)	2,202	0.024**	(0.01120)	1,840
Small centre & long stay	-0.0002	(0.00081)	2,840	-0.0013	(0.00117)	2,840	0.0001	(0.00763)	2,427
Young & low education	-0.0003	(0.00069)	3,676	0.0004	(0.00097)	3,676	0.0084	(0.00650)	3,091
Young & high education	-0.0006	(0.00100)	843	0.0013	(0.00126)	843	0.0047	(0.00878)	709
Young & long stay	-0.0001	(0.00068)	3,899	0.0003	(0.00091)	3,899	0.0062	(0.00579)	3,313
Old & low education	0.0005	(0.00104)	1,740	-0.0012	(0.00144)	1,740	0.0121	(0.01237)	1,388
Old & high education	0.0018	(0.00243)	266	-0.0013	(0.00308)	266	-0.0045	(0.02485)	203
Old & long stay	0.0003	(0.00128)	1,143	-0.0011	(0.00182)	1,143	0.0046	(0.01537)	954
Average Effect (ITT)	0.0002	(0.00056)	6,525	-0.0005	(0.00084)	6,525	0.0083	(0.00538)	5,391

Notes: This table reports the conditional average partial effects estimated using Generalized Random Forests (Athey et al., 2019). The outcome variable is the Dutch citizenship by the age of 20. The following variables are used to build causal forests: country of origin, year of assignment, age at migration, household head characteristics (including education), region of assignment and neighborhood quality index (statusscore). Each row reports the effects of ethnic concentration (measured in three different ways) on a specific subgroup. For each measure and dimension of heterogeneity, I report CATE, standard errors and the number of people in the sample who belonged to each specific subgroup. The last row reports the average partial effect on the entire sample, which has a similar interpretation to the baseline ITT estimates. Standard errors are clustered at the origin countries and regions level.

H Additional Material with Educational Outcomes

Table H1: ITT Estimation Educational Outcome

Share of Natives				
Concentration	0.0013***	0.0013**	0.0013***	0.0017***
	(0.00049)	(0.00049)	(0.00049)	(0.00052)
No. of Obs	5,963	5,963	5,963	5,963
Mean Outcome	0.56	0.56	0.56	0.56
R squared	0.149	0.181	0.181	0.131
Share of Muslims				
Concentration	-0.0017**	-0.0016**	-0.0018***	-0.0020***
	(0.00066)	(0.00065)	(0.00066)	(0.00067)
No. of Obs	5,963	5,963	5,963	5,963
Mean Outcome	0.56	0.56	0.56	0.56
R squared	0.149	0.181	0.181	0.130
Log of Country				
Concentration	-0.0156**	-0.0150**	-0.0152**	-0.0189***
	(0.00606)	(0.00617)	(0.00616)	(0.00616)
No. of Obs	4,919	4,919	4,919	4,919
Mean Outcome	0.57	0.57	0.57	0.57
R squared	0.133	0.171	0.171	0.141
Regional FE	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Status Score (ZIP4)	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having followed the above HAVO education (i.e., a binary variable taking value 1 if the highest education followed is HAVO, VWO, HBO, and WO) by the age of 20. All specifications control for country of origin, year of assignment and regional fixed effects, together with age at migration (a specification reported in the first column). Additional controls are added successively, the second column adds household head characteristics, and the third adds the status score, a neighborhood quality index. 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 observations from Table 10. Standard errors are clustered at the origin-country and regional level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as the log of immigrants from the same country of origin.

Table H2: ITT Estimation - Educational Outcome - Ordered Probit

Share of Natives			
Concentration	0.0032***	0.0032***	0.0032***
	(0.00122)	(0.00120)	(0.00122)
No. of Obs	5,963	5,963	5,963
Mean Outcome	3.62	3.62	3.62
Share of Muslims			
Concentration	-0.0039**	-0.0040**	-0.0040**
	(0.00173)	(0.00169)	(0.00177)
No. of Obs	5,963	5,963	5,963
Mean Outcome	3.62	3.62	3.62
Log of Country			
Concentration	-0.0328**	-0.0306**	-0.0301**
	(0.01350)	(0.01412)	(0.01423)
No. of Obs	4,919	4,919	4,919
Mean Outcome	3.63	3.63	3.63
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	NO	YES	YES
Status Score (ZIP4)	NO	NO	YES
LASSO Selection	NO	NO	NO

Notes: This table reports estimations of equation 3 using an ordered probit MLE. The outcome variable is an ordered variable for educational attainment by the age of 20 (see Table A3). All specifications control for country of origin, year of assignment and regional 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 status score (a neighborhood quality index). The sample only includes teenage women followed in Table 10. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

Table H3: ITT Estimation - Educational Outcome - Years of Education

Share of Natives				
Concentration	0.0047**	0.0046***	0.0045**	0.0068***
	(0.00188)	(0.00179)	(0.00183)	(0.00205)
No. of Obs	5,963	5,963	5,963	5,963
Mean Outcome	17.35	17.35	17.35	17.35
R-squared	0.161	0.201	0.202	0.143
Share of Muslims				
Concentration	-0.0055**	-0.0055**	-0.0053**	-0.0075***
	(0.00270)	(0.00255)	(0.00267)	(0.00271)
No. of Obs	5,963	5,963	5,963	5,963
Mean Outcome	17.35	17.35	17.35	17.35
R-squared	0.160	0.201	0.201	0.141
Log of Country				
Concentration	-0.0484**	-0.0436**	-0.0426*	-0.0608***
	(0.02167)	(0.02181)	(0.02200)	(0.02165)
No. of Obs	4,919	4,919	4,919	4,919
Mean Outcome	17.39	17.39	17.39	17.39
R-squared	0.143	0.186	0.186	0.159
Regional FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Status Score (ZIP4)	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is years of schooling by the age of 20 (see Table A3). All specifications control for country of origin, year of assignment and regional 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 status score (a neighborhood quality index). 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 followed in Table 10. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country.

Table H4: ITT Estimation - Robustness to Alternative Samples - Educational Outcome

	Share of Natives	Share of Muslims	Log of Country
	Incomplete Spells		
Concentration	0.0015***	-0.0018***	-0.0148**
	(0.00047)	(0.00061)	(0.00588)
No. of Obs	7,040	7,040	5,889
Mean Outcome	0.56	0.56	0.57
R-squared	0.127	0.126	0.130
	First Assignment		
Concentration	0.0016***	-0.0019***	-0.0152**
	(0.00054)	(0.00072)	(0.00636)
No. of Obs	5,963	5,963	4,919
Mean Outcome	0.56	0.56	0.57
R-squared	0.127	0.126	0.137
	Variation 1		
Concentration	0.0017***	-0.0020***	-0.0194***
	(0.00049)	(0.00063)	(0.00581)
No. of Obs	6,242	6,242	5,127
Mean Outcome	0.56	0.56	0.57
R-squared	0.128	0.127	0.128
	Variation 2		
Concentration	0.0016***	-0.0019***	-0.0193***
	(0.00051)	(0.00064)	(0.00585)
No. of Obs	6,244	6,244	5,150
Mean Outcome	0.56	0.56	0.57
R-squared	0.130	0.129	0.132
Regional FE	YES	YES	YES
Origin Country FE	YES	YES	YES
Year of Assignment FE	YES	YES	YES
Individual Controls	YES	YES	YES
Family Controls	YES	YES	YES
Status Score (ZIP4)	YES	YES	YES
LASSO Selection	YES	YES	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having followed above HAVO education (i.e. a binary variable taking value 1 if the highest education followed is HAVO, VWO, HBO and WO) by the age of 20. All specifications use the Double Debiased procedure (Belloni et al., 2014b) to choose control variables among the following: country of origin, year of assignment and regional fixed effects together with age at migration, household head characteristics, the status score (a neighborhood quality index). For each measure (in columns) and each variation of the baseline sample (panels), the sample only includes teenage women with a complete spell. Standard errors are clustered at the origin countries and regions level.

Table H5: ITT Estimation - Educational Outcome - External Validity - Re weighting

Share of Natives				
Concentration	0.0014***	0.0014***	0.0015***	0.0016***
	(0.00054)	(0.00053)	(0.00053)	(0.00057)
No. of Obs	5,245	5,245	5,245	5,245
Mean Outcome	0.56	0.56	0.56	0.56
R-squared	0.143	0.178	0.178	0.135
Share of Muslims				
Concentration	-0.0021***	-0.0022***	-0.0023***	-0.0021***
	(0.00073)	(0.00071)	(0.00072)	(0.00075)
No. of Obs	5,245	5,245	5,245	5,245
Mean Outcome	0.56	0.56	0.56	0.56
R-squared	0.143	0.178	0.179	0.135
Log of Country				
Concentration	-0.0163***	-0.0160**	-0.0159**	-0.0196***
	(0.00611)	(0.00621)	(0.00622)	(0.00608)
No. of Obs	4,860	4,860	4,860	4,860
Mean Outcome	0.57	0.57	0.57	0.57
R-squared	0.132	0.170	0.170	0.141
Regional FE	YES	YES	YES	YES
Origin Country FE	YES	YES	YES	YES
Year of Assignment FE	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES
Family Controls	NO	YES	YES	YES
Status Score (ZIP4)	NO	NO	YES	YES
LASSO Selection	NO	NO	NO	YES

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having followed above HAVO education (i.e. a binary variable taking value 1 if the highest education followed is HAVO, VWO, HBO and WO) by the age of 20. All specifications control for country of origin, year of assignment and regional 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 status score (a neighborhood quality index). 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 includes teenage women followed in Table H1. Standard errors are clustered at the origin countries and regions level. The first panel measures concentration as share of natives, the second as share of Muslims and the third as log of immigrants from the same origin country. Observations are weighted as to give more importance to combinations of origin country/arrival year which are under-represented from what they could be, see Section A.4.

Table H6: ITT Estimation - Different Levels of Clustering - Educational Outcome

Share of Natives							
Concentration	0.0017***	0.0017***	0.0017***	0.0017***	0.0017***	0.0017***	0.0017***
	(0.00052)	(0.00059)	(0.00051)	(0.00047)	(0.00051)	(0.00051)	(0.00048)
No. of Obs	5,963	5,963	5,963	5,963	5,963	5,963	5,963
No. of Clusters	1,080	40	2,075	218	2,455	437	5,963
Share of Muslims							
Concentration	-0.0020***	-0.0020***	-0.0020***	-0.0020***	-0.002***	-0.0020***	-0.0020***
	(0.00067)	(0.00061)	(0.00066)	(0.00065)	(0.00068)	(0.00066)	(0.00064)
No. of Obs	5,963	5,963	5,963	5,963	5,963	5,963	5,963
No. of Clusters	1,080	40	2,075	218	2,455	437	5,963
Log of Country							
Concentration	-0.0189***	-0.0189***	-0.0189***	-0.0189***	-0.0189***	-0.0189***	-0.0189***
	(0.00616)	(0.00614)	(0.00589)	(0.00545)	(0.00593)	(0.00574)	(0.00554)
No. of Obs	4,919	4,919	4,919	4,919	4,919	4,919	4,919
No. of Clusters	519	40	1,377	213	1,723	408	4,919

Notes: This table reports estimations of equation 3. The outcome variable is a dummy for having followed above HAVO education (i.e. a binary variable taking value 1 if the highest education followed is HAVO, VWO, HBO and WO) by the age of 20. The specification and sample definition correspond to those of the fourth column of Table H1. Standard errors are clustered at different level: region and origin country, region only, municipality only, municipality and origin country, neighborhood only, neighborhood and origin country and robust standard errors. For each level of clustering, I report the number of clusters and the number of observations.

Table H7: Treatment Effect Heterogeneity - Generalized Random Forests - Educational Outcome

	Share of Natives			Share of Muslims			Log of Country		
	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs	Coeff	S-E	No. of Obs
Age migration < 9 y.o.	0.0012	(0.00079)	4,189	-0.0027**	(0.00123)	4,189	-0.0088	(0.00664)	3,509
Age migration > 9 y.o.	0.0019*	(0.00096)	1,774	-0.0029**	(0.00131)	1,774	-0.0065	(0.01002)	1,410
Father low education	0.0014*	(0.00073)	4,924	-0.0028**	(0.00113)	4,924	-0.0046	(0.00631)	4,065
Father high education	0.0020	(0.00155)	1,039	-0.0025	(0.00204)	1,039	-0.0239**	(0.01155)	854
Length of stay > 1 year	0.0013*	(0.00074)	4,608	-0.0025**	(0.00115)	4,608	-0.0062	(0.00648)	3,887
Center larger than 100	0.0011	(0.00085)	2,943	-0.0018	(0.00119)	2,943	0.0030	(0.00950)	2,340
Centre smaller than 100	0.0014	(0.00098)	3,020	-0.0034***	(0.00140)	3,020	-0.0114	(0.00803)	2,579
Low education & long stay	0.0010	(0.00082)	3,771	-0.0026**	(0.00128)	3,771	-0.0070	(0.00729)	3,195
High education & long stay	0.0021	(0.00159)	837	-0.0033	(0.00241)	837	-0.0084	(0.01321)	692
Large centre & long stay	0.0008	(0.00099)	2,015	-0.0008	(0.00137)	2,015	0.0041	(0.01252)	1,678
Small centre & long stay	0.0011	(0.00100)	2,593	-0.0030*	(0.00152)	2,593	-0.0074	(0.00857)	2,209
Young & low education	0.0012	(0.00090)	3,392	-0.0027*	(0.00140)	3,392	-0.0061	(0.00758)	2,839
Young & high education	0.0022	(0.00159)	797	-0.0027	(0.00220)	797	-0.0217*	(0.01276)	670
Young & long stay	0.0009	(0.00081)	3,604	-0.0025**	(0.00125)	3,604	-0.0083	(0.00698)	3,052
Old & low education	0.0019*	(0.00103)	1,532	-0.0029**	(0.00138)	1,532	-0.0000	(0.01098)	1,226
Old & high education	0.0020	(0.00257)	242	-0.0032	(0.00354)	242	-0.0258	(0.02544)	184
Old & long stay	0.0018	(0.00122)	1,004	-0.0023	(0.00175)	1,004	-0.0023	(0.01533)	835
Average Effect (ITT)	0.0014**	(0.00067)	5,963	-0.0030***	(0.00104)	5,963	-0.0079	(0.00560)	4,919

Notes: This table reports the conditional average partial effects estimated using Generalized Random Forests (Athey et al., 2019). The outcome variable is a dummy for having followed the above HAVO education (i.e. a binary variable taking value 1 if the highest education followed is HAVO, VWO, HBO, and WO) by the age of 20. The following variables are used to build causal forests: country of origin, year of assignment, age at migration, household head characteristics (including education), region of assignment, and the status score (a neighborhood quality index). Each row reports the effects of ethnic concentration (measured in three different ways) on a specific subgroup. For each measure and dimension of heterogeneity, I report CATE, standard errors and the number of people in the sample who belonged to each specific subgroup. The last row reports the average partial effect on the entire sample, which has a similar interpretation to the baseline ITT estimates. Standard errors are clustered at the origin-country and regional level.

I Additional Material with Alternative Strategy

Sample Characteristics I provide a list of the main origin countries in Table I1 and descriptive statistics of the sample in Table I2.

Table I1: Main Origin Countries

Country	Share
First-Generation Immigrants	
Afghanistan	23.8
Irak	16.8
Morocco	15.0
Turkey	11.8
Somalia	4.9
Second-Generation Immigrants	
Morocco	40.1
Turkey	34.6
Indonesia	5.8
Pakistan	1.7
Egypt	1.5

Results and Robustness Results are shown in Table I3 for the three measures used to assess ethnic concentration. No coefficient is individually significant at the 5% level, even though the sample size is very large (close to 40,000 observations when concentration is measured as a share). I also report three F-test for $\mathbb{H}_0$ all coefficient are zero and all coefficients below and above 10 years old are zero. I fail to reject any of the nulls with very large p-values.

As a robustness check, I estimate the same equation on a sample of incomplete spells (meaning girls who have been observed at least from the age of 18 instead of all ages from 16 y.o.), the two coefficients that were statistically significant at the 10% level in Table I3 become insignificant although the sample size changed little, see Table I4. This alleviates concerns that there was a small but significant effect.

Table I2: Descriptive Statistics - Alternative Strategy

	First-Generation	Second-Generation
Panel A: Individual Characteristics		
Age at arrival (in years)		
25^{th} percentile	4.28	
Median	6.78	
75^{th} percentile	9.94	
Mean	7.29	
Year of arrival		
25^{th} percentile	1998	
Median	2000	
75^{th} percentile	2003	
Educational Attainment (no. of individuals)		
Missing Value	446	1,084
Lower Secondary	2,592	15,043
Upper Secondary	2,325	13,198
College and Above	616	2,734
N Obs	5,979	32,059
Panel B: Family Characteristics		
Difference between oldest and youngest sister (in years)		
25^{th} percentile	2.00	2.09
Median	3.33	3.50
75^{th} percentile	5.25	5.17
Mean	3.72	3.78
Educational Attainment of the Father (no. of individuals)		
Missing value	985	6,232
Primary School	812	2,859
Lower Secondary	513	2,099
Upper Secondary	541	2,666
College	238	944
Above College	280	703
Difference in Contraceptive Usage between sisters (no. of individuals)		
Both using	554	3,222
None using	1,629	7,180
Some using, some not	1,186	5,101
N families	3,369	15,503

Notes: This table reports descriptive statistics on young women followed in the alternative strategy explained in Section 6.3.3. Panel A details individual characteristics and Panel B characteristics of their family. Panel A reports the age at migration, year of arrival and the education level (by categories, see Table A3). Panel B reports information on the difference in age between the oldest and youngest sibling in families, the difference in contraceptive usage between sisters and the education level of the father. Categories in Panel B correspond to those reported in Table A3, where Primary School is anything below Lower Secondary.

Table I3: Alternative Strategy - Contraceptive Usage - Complete Spells

	Share Natives	Share Muslims	Log of Country
Age 5	-0.0001	0.0003	0.0051
	(0.00050)	(0.00062)	(0.00449)
Age 6	0.0009	-0.0013*	-0.0033
	(0.00063)	(0.00081)	(0.00547)
Age 7	-0.0006	0.0007	-0.0013
	(0.00066)	(0.00083)	(0.00575)
Age 8	0.0009	-0.0012	-0.0004
	(0.00068)	(0.00085)	(0.00615)
Age 9	-0.0003	-0.0001	0.0027
	(0.00064)	(0.00079)	(0.00599)
Age 10	0.0004	-0.0001	-0.0030
	(0.00066)	(0.00084)	(0.00621)
Age 11	-0.0001	-0.0001	0.0023
	(0.00068)	(0.00088)	(0.00623)
Age 12	0.0007	-0.0005	-0.0052
	(0.00068)	(0.00084)	(0.00685)
Age 13	-0.0002	-0.0001	0.0071
	(0.00076)	(0.00092)	(0.00752)
Age 14	-0.0005	0.0008	0.0035
	(0.00080)	(0.00097)	(0.00782)
Age 15	0.0001	-0.0003	-0.0121*
	(0.00070)	(0.00083)	(0.00731)
No. of Obs	38,038	38,038	32,914
Mean Outcome	0.36	0.36	0.32
R-squared	0.642	0.642	0.622
F-Test	0.90	1.13	0.48
p-value	0.543	0.332	0.920
F-Test before 10	1.19	1.64	0.32
p-value before 10	0.311	0.146	0.901
F-Test after 10	0.40	0.30	0.66
p-value after 10	0.877	0.939	0.683

Notes: This table reports the results of a linear regression where the outcome is a dummy variable for having taken contraceptives at least once by age 20. The explanatory variables include family fixed effects and ethnic concentration experienced at the neighborhood level between the ages 5 to 15. The regressions are estimated on the sample of first- and second-generation women (from Non-Western countries) who were living in the Netherlands between 16 and 20 years of age during the period 2006 - 2019. It is limited to families where at least two sisters were followed from the age of 16 to 20.

Table I4: Alternative Strategy - Contraceptive Usage - Incomplete Spell

	Share Natives	Share Muslims	Log of Country
Age 5	-0.0000	0.0003	0.0044
	(0.00049)	(0.00061)	(0.00441)
Age 6	0.0009	-0.0013	-0.0030
	(0.00062)	(0.00079)	(0.00534)
Age 7	-0.0006	0.0007	-0.0006
	(0.00064)	(0.00081)	(0.00558)
Age 8	0.0009	-0.0011	0.0001
	(0.00066)	(0.00084)	(0.00600)
Age 9	-0.0002	-0.0002	0.0020
	(0.00063)	(0.00078)	(0.00587)
Age 10	0.0004	-0.0001	-0.0021
	(0.00064)	(0.00082)	(0.00606)
Age 11	-0.0003	0.0001	0.0032
	(0.00066)	(0.00085)	(0.00609)
Age 12	0.0008	-0.0008	-0.0070
	(0.00067)	(0.00083)	(0.00646)
Age 13	-0.0003	0.0001	0.0061
	(0.00071)	(0.00087)	(0.00688)
Age 14	-0.0003	0.0006	0.0047
	(0.00074)	(0.00090)	(0.00704)
Age 15	-0.0002	-0.0000	-0.0075
	(0.00063)	(0.00076)	(0.00611)
No. of Obs	38,826	38,826	33,579
Mean Outcome	0.36	0.36	0.32
R-squared	0.642	0.642	0.622
F-Test	0.96	1.16	0.37
p-value	0.477	0.307	0.968
F-Test before 10	1.20	1.73	0.23
p-value before 10	0.305	0.123	0.949
F-Test after 10	0.51	0.32	0.52
p-value after 10	0.799	0.927	0.791

Notes: This table reports the results of a linear regression where the outcome is a dummy variable for having taken contraceptives at least once by age 20. The explanatory variables include family fixed effects and ethnic concentration experienced at the neighborhood level between the ages 5 to 15. The regressions are estimated on the sample of first- and second-generation women (from Non-Western countries) who were living in the Netherlands between 16 and 20 years of age during the period 2006 - 2019. It is limited to families where at least two sisters were followed from the age of 18 to 20.

J 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.

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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 Asielzokers) 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.

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