Ethnic Enclaves and Cultural Assimilation

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Abstract

This paper studies whether growing up in an ethnic enclave slows down immigrants' cultural assimilation. To identify neighbourhood influence, I exploit the random allocation of asylum seekers to government housing in the Netherlands between 1996 and 2012. To assess assimilation, I examine a culturally charged consumption: the usage of hormonal contraceptives by teenage women. Using individual level administrative data on drug usage, I find that cultural assimilation is slow and cannot be accelerated by limiting the formation of ethnic enclaves. Using machine learning techniques, I do not find evidence that this baseline result hides heterogeneous effects on a relevant sub-population.

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1 Introduction

The issue of immigrants' cultural assimilation is at the centre of public debate in Europe and the United States.[1] Cultural distance and the perceived willingness to adopt mainstream culture are important factors in explaining natives' support (or lack thereof) for immigration (Tabellini, 2020). Given the importance of neighbourhoods in shaping social networks, avoiding to cluster people from the same origin is a common tool considered to speed up cultural integration (Bisin and Verdier, 2010; Bisin et al., 2016; Abramitzky et al., 2020a). The objective of this paper is to assess empirically if neighbourhood ethnic concentration causally affects cultural assimilation. This is challenging for two reasons: culture is hard to measure and people choose where they live.

I measure cultural behaviour by relying on individual-level administrative data on usage of hormonal contraceptives by teenage women. Following the sexual revolution of the 1960s, the perspective on female pre-marital sexuality changed in the West. One example of these more liberal views is the widespread usage of hormonal contraceptive among teenage women. The pill in particular is seen as an empowerment device, through its connection to gender roles (Goldin and Katz, 2002; Bailey, 2006). However, more conservative gender norms still apply in many Non-Western countries and therefore among many immigrant communities (Algan et al., 2013a). Accordingly, taking the pill is associated with having more liberal views on female pre-marital sexuality which is a cultural aspect in which natives and Non-Western immigrants are most likely to differ and for which norms cannot be reconciled.

Contraceptives are not sold over the counter; they have to be prescribed. While personal and intimate, their usage is recorded in administrative health registries.[2] The data confirms the intuitive appeal of this outcome. There are large differences in contraceptive usage between natives and Non-Western immigrants. By age 17, 65% of natives have used contraceptives at least once while only 15% of first generation immigrants with a Muslim background have. At

[1] In this paper, I use interchangeably the terms 'cultural assimilation' and 'cultural integration'. I define them as adopting a behaviour typical of natives, namely to use contraceptives as a teenager. I do not make any normative statement and rely on those terms for ease of expression.

[2] Hormonal contraceptives (such as the pill) are recorded in administrative registries. This is not true for barrier contraceptives (such as condoms). Hence, I do not focus on immigrant sexual activity per se. Starting to use contraceptives could come from two margins: (i) teenagers becoming sexually active and using hormonal contraceptives as a consequence or (ii) conditional on already being sexually active, choosing this mean of contraception. I cannot disentangle the two channels. However, in both cases, observing usage can be interpreted as adopting natives' standard on how to control their sexual life. In the case of (ii), this follows from the pill being a method of contraception fully in the hands of women and a symbol of women empowerment.

age 20, these numbers are 87% and 36%.[3] This outcome is repeated at equally spaced intervals; I observe it for every age between 15 and 20 years. I can therefore study two outcomes; (i) are young women more likely to use contraceptives by age 20 (the level of cultural assimilation) and/or (ii) do they start taking contraceptives younger (the speed of cultural assimilation).

To draw causal inference on neighbourhood characteristics, I focus on asylum seekers in the Netherlands. At arrival in the country, while their asylum application is being processed, they are taken care of and hosted by a public organization, the Central Agency for the Reception of Asylum Seekers (COA). Allocation of asylum seekers into collective centres is decided according to availability of places and not according to preferences. I use this institutional setting as a mechanism which quasi-randomly dispatches asylum seekers across the country.

COA policy is to have their centres and their residents open to the community they are located in. This means centres are not closed, children go to local schools, parents can work. For those reasons, I treat allocation to a centre as equivalent to allocation to a neighbourhood. Asylum seekers spend a significant amount of time where they have been assigned (or close-by). On average, asylum seekers remain almost two years in COA centres. Three years after assignment, roughly 30% live in the same municipality, 25% either live in the same neighbourhood (equivalent to a census tract in the US) or in one adjacent to the place they were assigned.

There is evidence of cultural assimilation; immigrants who arrive younger (and who have been more exposed to mainstream Dutch culture) are more likely to behave like natives, i.e. use contraceptives as a teenager. Arriving one year younger is associated with a 1 percentage point increase in the probability of using contraceptives by age 20. This number is not different between immigrants with and without a Muslim background.[4] First-generation immigrants who arrive very young have usage rates similar to that of second generation immigrants of the same age. However, both remain much smaller than that of natives. Therefore, I interpret this rate of

[3]Other relevant outcomes (that I observe for both men and women), would be inter-marriage (Bisin et al., 2004; Merlino et al., 2019) and giving a native sounding name to children (Algan et al., 2013b; Abramitzky et al., 2020b). The latter is not available in the data. Furthermore, exploring these outcomes generate a problematic trade-off. There are not enough people in who were part of the quasi-experiment used in this paper and who arrived young in the Netherlands (say below 15) and who have reached the age range 25 to 30 (where one observes most marriages and births). Looking at these other outcomes would require to increase the sample size and thus to increase the age at which asylum seekers arrive. However, focusing on a group who arrived below 15 is very different in terms of cultural assimilation than looking at a group who arrived after age 15. The younger immigrants arrive, the closer they are to second generation immigrants (a relevant population for the topic of this paper). Therefore, I choose to focus on hormonal contraceptives as an outcome and on asylum seekers arriving at a young age as a population.

[4]Bisin et al. (2008) find different patterns of convergence for Muslim immigrants. The evidence of this paper does not support differential rate of convergence between Muslim and non-Muslim immigrants.

convergence as being slow since it is an order of magnitude smaller than the differences between natives and immigrants.[5]

The main question addressed in this paper is whether cultural integration can be speeded up by limiting immigrants' clustering into ethnic enclaves. The most important finding is that neighbourhood ethnic concentration has no effect on cultural assimilation. Naively regressing contraceptive usage on the size of the ethnic community shows a negative correlation. However, the causal effect is a precisely estimated zero. The Intention to Treat estimate, where ethnic concentration is measured at the time of arrival (when allocation is random and neighbourhood characteristics exogenous), is very small in magnitude and statistically insignificant. The Instrumental Variable estimate, where concentration at age 15 is instrumented by concentration at assignment, is also insignificant. Using machine learning techniques, in particular generalized random forests (Athey et al., 2019), I find no evidence that this baseline result hides a significant effect on a relevant sub-population. The low rate of convergence is partially explained by the inability of the environment (at least the neighbourhood) to increase cultural assimilation. This result is robust to changing the definition of ethnic community; restricting to young immigrants or those with the same religious background.

The null effect could be due to flaws in the experimental setting. If for instance, asylum seekers spent too little time in the neighbourhoods where they were assigned, it is hard to imagine that their characteristics could have an effect. To alleviate this concern, I perform a falsification test. I choose another outcome, i.e. education level, for which the literature has found a non-null effect (Aslund et al., 2011; Danzer et al., 2018) and check that the identification strategy used in this paper captures it.[6] I find that a large ethnic community in the neighbourhood causes lower educational achievements. 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 'neighbourhood' (evidence from the falsification test). Therefore, I am confident that I could pick up an effect of 'neighbourhood' on 'contraceptive usage' (the main focus of the paper) if there was one.

[5]First generation immigrants have been at most 15 years in the Netherlands before I start looking at their drug usage. Longer exposure can increase their usage rate (on average) by 15 p.p. which is much lower than the 50 points difference between Muslim immigrants and natives at age 20.

[6]This outcome is not mentioned in the pre-analysis plan (reported in appendix) as it is used as a robustness check. I explain in detail in the robustness section of the paper why I chose this outcome for the falsification test. Aslund et al. (2011) finds a positive (marginal) effect of community size on GPA when community size is measured with all co-ethnics and positive effect (at the 5% level) of the share of highly educated within the community. Danzer et al. (2018) find that community size increase the drop-out rates of children of guest workers in Germany.

My main contribution to the vast 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 peers influence (measured at the neighbourhood level). Bisin et al. (2008) shows a correlation between the strength of religious identity and neighbourhood characteristics in the U.K. Abramitzky et al. (2020a) looks at the effect of leaving a Jewish ethnic enclave in the U.S. at the age of Mass Migration (1850-1914). They find no effect on cultural transmission (measured as the probability of giving a Jewish sounding name to their children). My findings are consistent with theirs and point to the environment having no effect on cultural integration. The canonical model of cultural transmission (Bisin and Verdier, 2000; Bisin et al., 2004) distinguishes between vertical and horizontal channels. The horizontal dimension refers to the influence of the environment, while the vertical one designates the role played by the family. This paper is the only one, to my knowledge, to provide causal evidence on the role of the horizontal channel using a large scale residential experiment.

My second contribution to these literatures is to propose a culturally charged consumption: taking contraceptives as a teenager. Immigrants' sense of identity (Bisin et al., 2008, 2016), does not give a full picture of the acculturation process. Someone who grew up in the West may still self-identify as belonging to the origin country. Yet, she could already have adapted (at least partially) to the mainstream culture. Therefore, I do not focus on identity but single out a dimension in which cultural norms are likely to differ between natives and immigrants.

Papers studying the effects of living in more or less ethnically concentrated neighbourhoods have relied on so-called "dispersal policies" implemented in the late 1980s, early 1990s mostly in Denmark (Damm and Dustmann, 2014; Damm, 2009) and Sweden (Aslund et al., 2003, 2011). The purpose of these policies was to spread out asylum seekers throughout the country once they were granted asylum status to avoid ethnic clustering. I rely on a similar natural experiment in the Netherlands, although at an earlier stage of the asylum procedure; before asylum seekers are granted refugee status (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 (Damm, 2009; Aslund et al., 2003; Beckers and Borghans, 2011), and mixed evidence on the educational achievements of their children (Aslund et al., 2011; Danzer et al., 2018).

Different to prior literature (Damm and Dustmann, 2014; Damm, 2009; Aslund et al., 2003;

Danzer et al., 2018), I study neighbourhood influence on a different dimension, i.e. cultural assimilation and at a more disaggregated level; equivalent to a census tract in the US (similar to Aslund et al. (2011); Kling et al. (2007); Chetty et al. (2016)). Using geographically disaggregated data allows to better approximate the environment one grows up in. The policy implications of my findings are that limiting ethnic clustering, through housing policies, does not affect cultural assimilation but has a positive effect on immigrants educational achievement.

The rest of the paper is organized as follows: section 2 presents the data and while section 3 shows evidence of convergence in behaviours. Section 4 presents how the placement of asylum seekers can be used as a quasi-experiment and details the identification strategy. Section 5 presents causal neighbourhood effects, while section 6 shows robustness checks. The last section discusses the findings and concludes.

2 Description of the data

I combine two sources of data: Dutch administrative registries collected and maintained by CBS (Centraal Bureau voor de Statistiek) and information on the location and operating dates of accommodations run by COA. CBS offers a very rich set of administrative datasets linkable through a unique individual identifier. This allows to put together information on various topics (medicine usage, location, family situation, etc) and to link parents to children.

2.1 Usage of contraceptives

The Dutch healthcare system fully reimburses a set of ”basic” drugs. CBS collects their usage at the individual level on a yearly basis. Contraception, for women younger than 21 is part of the basic package.[7] The data is collected on the entire population living in the Netherlands. It is not self-reported and does not suffer from measurement error.

Data is collected from dispensed (and not just prescribed) medicine. If a drug is prescribed but not collected, it does not appear in the registries.[8] This data is available for the years 2006-2018. For contraceptives, the most disaggregated entry (in the ATC4 classification) is

[7] In 2011, a reform of the health system lowered the age from 25 to 21. To maximize the number of observations, I focus on the 21 age limit throughout the period 2006-2018.

[8] 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) young woman showing up with a prescription (old or new) but wants to pay cash.

the category G03A, “Hormonal contraceptives for systemic use”. It includes the pill, patches, injections and implants.[9] Although the category G03A includes emergency contraceptives, the morning after pill is not part of the basic package and thus not recorded.[10]

Hormonal contraceptives in the Netherlands are obtainable on prescription and can be prescribed by a GP and not necessarily by a gynaecologist. Parental consent is not necessary after a girl turns 16. It is possible, however, to ask doctors to wave this obligation for younger teenagers. According to the Personal Data Protection Act (Wbp), parents cannot access information on their children's treatments when they are older than 16.

2.2 Data on COA accommodation and ethnic concentration

The Netherlands is composed of 380 municipalities. Their sizes vary between 4,000 (Ameland) and 850,000 (Amsterdam) inhabitants. A zipcode in the Netherlands is composed of 4 digits and 2 letters. The four digits divide the country in more than 4,000 areas with a median population of 2,647 inhabitants.[11] 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 however not so narrow (as ”zip 6” would be) as to miss part of social interactions. It is also more uniform (in terms of population) across the country.

I combine this administrative data with information on all accommodations listed by COA between 1995 and 2012. Asylum seekers are under the responsibility of COA while their asylum application is processed. During this period, they can either (i) stay in a collective centre 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 by COA. There is a total of 17,000 different addresses used by COA for the period 1995-2012 out of which, 15,500 could be located.

[9] For more information, one can go to https://www.whocc.no/atc_ddd_index/?code=G03A&showdescription=yes

[10] G03A os itself subdivided into four categories, G03AA Progesterones and oestrogens, fixed combinations, G03AB Progesterones and oestrogens, sequential preparations, G03AC Progesterones and G03AD Emergency contraceptives. I do not have individual usage at this level but aggregate figures are available at https://www.gipdatabank.nl/databank#/g//B_01-basis/gebr/G03A for the period 2013-2017. The estimated number of G03AD users is around 3 out of 1000 of users of all G03A medicine. This points towards the data not accounting for emergency contraceptives.

[11] In 2017, the country was made of 4,066 zip4 areas. The median population is 2,647, while the mean is 3,413 inhabitants.

2.3 Sample restrictions

When I look at descriptive evidence on assimilation, I keep all immigrant women. When I study neighbourhood effects, I restrict the sample to asylum seekers being placed by COA. In both cases, I only focus on teenagers who have been in the Netherlands between their fifteenth to twentieth birthdays. This restriction ensures that women have spent at least five years in the Netherlands. That way, I do not capture a mechanical effect of access to healthcare.[12]

When assessing the effects of neighbourhoods, I focus on women who migrated at the latest in 2012 (last year of COA data). Therefore, I look at women who turned 15 in 2006 up until those who turned 20 in 2017. They were born earliest in 1991 and latest in 1997. I cannot use measures of behaviours (other women taking contraceptives) as the main explanatory variable. It would restrict my sample to those who migrated between 2006 and 2012 (since the data on drugs is only available from 2006). This is why I use ethnic concentration instead.

I focus primarily on *complete spells* (all observations from 15 to 20 years old). Data on contraceptives is available from 2006 onwards. Asylum seekers who arrived before age 15 and turned 15 before 2006 are not in the *complete spell* sample. For neighbourhood effect, I create a sample of *incomplete spells*. It is made of women who arrived before age 15 and whose contraceptive usage is observed from age 18 until age 20.[13] This sample is used as a robustness check. I report descriptive statistics of the sample used to investigate neighbourhood effects in tables 1 (for household heads) and 2 (for teenage women). Table A6 reports their countries of origin.

[12] I check what proportion of the samples collected a drug from the basic package during the time they were 15 to 20 years old. 96% of women followed in the neighbourhood analysis (with a *complete spell*) and 98% of women followed in the descriptive part on convergence appear in the medicine data during that period.

[13] There is a limited risk of misclassification when using the definition (18 and above) for *incomplete spell*. For instance, there would be 11% misclassification of the complete spell observations if one was to start observing them from age 18.

Table 1: Descriptive Statitics - Household Head

	Complete Sample	Non-Muslim Background	Muslim Background
	Age at migration (in years)		
25^{th} percentile	31.4	30.6	31.8
50^{th} percentile	35.4	34.5	36
75^{th} percentile	40.4	38.9	41
	Being male (in %)		
Average	64	67	63
	Education level (nb of individuals)		
Missing value	597	222	375
Basic Education	1,111	325	786
Primary School	798	312	486
Middle School	950	423	527
High School	357	123	234
Higher Education	363	88	275
	Average nb of minor children		
At arrival	2.45	2.14	2.62
When daughter turns 15	1.39	1.14	1.52
	Geographical dispersion		
Zip4 at arrival	380	283	338
Zip4 at age 15	1,497	853	1,224
Municipality at arrival	213	183	203
Nb Obs	4,176	1,493	2,683

Note: This table reports descriptive statistics of household heads. It reports the age at migration, the probability for being a male and the education level (by categories) of the household head of the *complete spell* sample (namely women observed for all years between age 15 to 20). It also reports, at migration and when the oldest daughter (followed in the sample) turns 15 y.o., the number of children (who are younger than 15 y.o.) and the number of zip4 areas and municipalities where they are registered. It also reports the number of municipalities where they are registered at arrival. Numbers are broken down between immigrants with and without a Muslim background.

Table 2: Descriptive Statitics - Experimental Population

	Complete Sample	Non-Muslim Background	Muslim Background
	Age at Migration (in years)		
25^{th} percentile	4.5	4.7	4.4
50^{th} percentile	6.4	6.6	6.3
75^{th} percentile	8.8	9	8.7
Mean	6.8	7	6.8
	Time spent with COA (in days)		
25^{th} percentile	388	407	384
50^{th} percentile	509	638	476
75^{th} percentile	871	1,094	766
Mean	709	840	652
	Nb of asylum seekers in centres (nb of individuals)		
25^{th} percentile	16	18	15
50^{th} percentile	91	99	89
75^{th} percentile	198	216	191
Mean	132	138	129
	Community size at arrival (nb of individuals)		
25^{th} percentile	15	12	16
50^{th} percentile	39	35	41
75^{th} percentile	83	72	89
Mean	86	66	80
	Community size at age 15 (nb of individuals)		
25^{th} percentile	32	27	34
50^{th} percentile	75	65	80
75^{th} percentile	166	124	200
Mean	171	103	200
	Year of arrival		
25^{th} percentile	1999	1999	1998
50^{th} percentile	2001	2001	2000
75^{th} percentile	2002	2002	2002
	Geographical dispersion		
Zip4 at arrival	372	272	331
Zip4 at age 15	1,536	845	1,285
Municipality at arrival	212	177	203
Nb Obs	4,909	1,496	3,413

This table reports descriptive statistics of women observed for all years between age 15 to 20. It reports the age at migration and the year of arrival. It also reports the number of asylum seekers registered at the COA centre at the time of arrival, the time spent with COA and the size of the ethnic community at arrival and when the girl turns 15 y.o. It also details the number of zip4 areas and municipalities where they are registered. Numbers are broken down between immigrants with and without a Muslim background.

3 Descriptive Evidence on Contraceptive Usage

3.1 Differences between natives and immigrants

Table 3 reports the probability for natives, first and second generation (Non-Western) immigrants to have used contraceptives at least once at all ages between 15 and 20 years old.[14] I split the population of immigrants between those with and without a Muslim background.[15] I report in Tables A1 and A2 the main origin countries of the observations followed in Table 3.

There is a striking difference between natives and immigrants. While almost all native females have taken contraceptives at least once by the age of 20 (87%), this proportion is much smaller for immigrants. The percentages are particularly low for those with a Muslim background, first and second generations have respectively a probability of 36% and 46%. Immigrants without a Muslim background stand in between natives and Muslim immigrants, with 2^{nd} generation immigrants having usage rates relatively close to natives (77%). The picture that emerges from Table 3 is one of slow convergence with strong differences between natives and even second generation immigrants, at least those with a Muslim background.

Table 3: Difference in usage between native and immigrant women

Age	% of contraceptives use				
	Natives	1st gen		2nd gen	
		Muslim	Non-Muslim	Muslim	Non-Muslim
15	24	4	7	8	18
16	47	9	17	16	35
17	65	15	29	25	52
18	77	22	41	33	64
19	83	29	51	40	72
20	87	36	58	46	77
Nb Obs	513,873	7,690	4,078	64,328	31,455

Note : This table reports the number of girls who were living in the Netherlands between the ages of 15 to 20 and the percentage who have used contraceptives at least once by a certain age. Girls are classified into five groups, natives, first and second generation immigrants from non-western countries with and without a Muslim background.

[14] Non-Western immigrants are those who do not come from North America, Northern and Western Europe according to the regional classification used by the M49 United Nations Statistics Division and reported in Table A9.

[15] Having a Muslim background is defining as originating from a country of the Organization of Islamic Cooperation. These countries are reported in Table A10 This follows from Bisin et al. (2008) who showed that the convergence process could be different for immigrants with a Muslim background.

3.2 Differences in behaviour by age and length of stay

There can be strong compositional differences between first and second generation immigrants. Comparing them may give a poor indication of the rate at which assimilation takes place. Instead, I estimate if usage increases the longer immigrants have been in the Netherlands. To do so, I compare first generation immigrants who migrated at different ages. Depending on how old they were when they arrived, certain immigrants have been in the Netherlands for longer when they reach their teenage years. I focus on the following model:

$$y_{i,c,t} = \alpha + \lambda_c + \theta_t + \beta_1 \, \text{age}_{i,t} \mathbb{1}\{\text{Muslim}\} + \beta_2 \, \text{age}_{i,t} \mathbb{1}\{\text{Non-Muslim}\} + X_i + \epsilon_{i,c,t} \tag{1}$$

Where $y_{i,c,t}$ is a dummy for having taken contraceptives at least once by age 20 for individual i from country c who arrived in year t in the Netherlands. λ_c are country of origin fixed effects and θ_t are year of arrival fixed effects. The variable of interest is $age_{i,t}$ the age at which individual i arrived in the country. It is interacted with a dummy variable for having a Muslim background. Individual controls X_i include categorical variables for both parents' education, number of siblings and dummy variables for municipalities where teenagers lived at age 15.[16]

In Table 4, I report the estimation of equation 1. The coefficient on age should be read as the percentage point increase in contraceptive usage from arriving one year older in the Netherlands. Column A reports the results with country of origin FE, column B adds year of arrival FE and column C adds individual characteristics to column B. There is evidence of cultural assimilation. Arriving older decreases the probability of using contraceptives. This result holds for immigrants with and without a Muslim background and complements the picture that emerges from Table 3. The starting point may differ by religious background, but the rate of convergence is similar (-1.15 p.p. and -0.91 p.p. for immigrants without and with a Muslim background.)[17] The same picture emerges from using duration models.[18] Comparing columns (B) to (C) points out an interesting element; the rate of convergence for immigrants with a Muslim background is not

[16]Equation 1 does not include the year in which individual i turns age a, since it is a linear combination of θ_t, a and $age_{i,t}$. It would limit the variation used to identify β to differences in months of arrival (since $age_{i,t}$ is calculated using birthday and day of entry in the Netherlands and is not an integer).

[17]The coefficients in columns A and B are not statistically different at the 10% level.

[18]Starting to take contraceptives can be defined as a failure in duration analysis. I estimate a Weibull MLE with the same covariates as equation 1. The coefficient on age should be read as the percentage increase in the hazard rate from arriving one year older in the Netherlands. Results are reported in Table A5. They indicate a convergence rate of 3-4% decrease in the hazard rate for arriving one year older. Contrary to Table 4, the coefficient on Non-Muslim immigrants does not change between columns B and C.

affected by the inclusion of controls for family characteristics and municipality of residence while it is for Non-Muslim immigrants. This is indicative that these characteristics (including parents' education) do not affect the rate of cultural assimilation. I report in Table A3 descriptive statistics corresponding to the sample followed in Table 4.

Table 4: Convergence Analysis - Baseline Results

Age at arrival	(A)	(B)	(C)
Immigrants (non-Muslim background)	-1.19***	-1.15***	-0.5
	(0.206)	(0.289)	(0.437)
Immigrants (Muslim background)	-0.95***	-0.91***	-0.85***
	(0.145)	(0.253)	(0.334)
Nb Obs (Total)	12,117	12,117	7,298
Nb Obs (Muslim background)	7,914	7,914	5,149
Mean (Non-Muslim background)	58	58	54
Mean (Muslim background)	36	36	35
Country FE	✓	✓	✓
Year of arrival FE	✗	✓	✓
Individual characteristics	✗	✗	✓

Note : Each column reports the results of a linear regression where the outcome is a dummy for having taken contraceptives at least once by age 20. The explanatory variables are age at arrival (interacted with a dummy for having a Muslim background) and a series of controls. The first column reports results with country fixed effects, the second adds year of arrival fixed effects and the last one adds individual characteristics (parents' education level, family size and municipality of residence fixed effects). The regressions are estimated on the sample of first generation women who were living in the Netherlands between 15 and 20 years old during the period 2006 - 2018.

3.3 Differences between siblings

To control even further for family characteristics, I estimate equation 1 with family fixed effects. This limits the comparisons to sisters who arrived at different ages in the Netherlands. The sample is restricted to families with at least two siblings whose complete spell is observed. This dramatically reduces the sample size (from 60,665 to 3,304 observations). I also add birth order dummies, as in Abramitzky et al. (2020b). Table 5 reports the results from OLS regressions at all ages from 15 to 20. Each row reports the coefficients of a regression where the outcome is having used contraceptives by a certain age.

As in Table 4, all coefficients are negative while not always significant. Coefficients for girls with a Muslim background are lower for younger ages (15 to 17 years old) but become larger after. They are in line with non-Muslim immigrants after age 17 (also an age where family control may loosen). The effect is quite large when compared to unconditional means for every age but relatively small when one considers the gap with natives. Arriving one year older decreases the probability of having used contraceptives at age 18 by 2.82 p.p. for Muslim immigrants for an unconditional mean of 22.8%. I report in Table A4 descriptive statistics corresponding to the sample used in Table 5.

Table 5: Convergence Analysis - Family Fixed Effects

Age	Non Muslim Background	Muslim Background	Mean	Nb Obs (Total)	Nb Obs (Muslim background)
15	-1.07**	-0.34	3.5	3,304	2,455
	(0.528)	(0.405)			
16	-1.7**	-0.49	9.3	3,304	2,455
	(0.788)	(0.606)			
17	-1.57*	-0.51	15.9	3,304	2,455
	(0.952)	(0.748)			
18	-3.3***	-2.82***	22.8	3,304	2,455
	(1.096)	(0.884)			
19	-2.57**	-1.85**	30.7	3,304	2,455
	(1.171)	(0.942)			
20	-1.68	-1.65*	37.5	3,304	2,455
	(1.217)	(0.974)			

Note : Each row 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 (interacted with a dummy for having a Muslim background), family and birth order fixed effects. The regressions are estimated on the sample of first generation women who were living in the Netherlands between 15 and 20 years old during the period 2006 - 2018. It is limited to families where at least two sisters were followed for the entire spell 15 to 20 years old.

A few elements emerge from this descriptive analysis. There is evidence of cultural assimilation in the sense that immigrants behave more like the natives the longer they have been in the country. The rate of convergence is not different between immigrants with and without a Muslim background. However, the initial differences are so large that a girl who fully grew up in the Netherlands (either a second generation or a first generation who arrived very young) still behaves very differently from natives. This is why I interpret cultural assimilation to be slow.

These results establish two facts that justify ex-post using hormonal contraceptive as a measure of cultural behaviour: (i) they are differences between immigrants and natives (there is something to explain), and (ii) this behaviour changes with immigrants' length of stay in the country. This outcome is not fully inelastic. It is therefore relevant to see if the environment (i.e. neighbourhood ethnic concentration) influences it. The coefficients reported in tables 4 and 5 can also be used as a benchmark to assess the magnitude of neighbourhood effects.

4 Institutional Settings and Identification Strategy

Neighbourhood's ethnic composition could speed up or slow down cultural assimilation. Since it is easier to interact with co-ethnics; living in a neighbourhood with a large immigrant community from the same origin provides more peers and role models with customs and rules that are distinct from native mainstream culture. It also increases the probability of being identified as a non-complier if someone moves away from the norm of the community. To gather evidence on the subject, one must rely on some 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 in different environments, (iii) let them grow up in the assigned places until they become teenagers/young adults and then (iv) observe their behaviours.

The natural experiment used in this paper resembles very much the ideal setting. I observe young girls who arrived in the Netherlands as asylum seekers. While their application is being processed, they are taken care of by a public organisation in charge of welcoming asylum seekers, the Central Agency for the Reception of Asylum Seekers (COA). Since asylum seekers do not choose where they go, this first assignment provides exactly the exogenous variation required to make causal inference. I then look at their contraceptive usage later in life.

This setting departs from the ideal experiment in that asylum seekers do not stay in government provided housing indefinitely. At some point, they are out of COA responsibility and move to traditional housing. To make sure that this setting is still valid, I look at two things: (i) how long do asylum seekers stay in the neighbourhood (and municipality) in which they were first assigned? This provides evidence that assignment meaningfully influences future residential choices, (ii) how heterogeneous are baseline estimations to length of stay? Is the effect different for asylum seekers who stayed in COA centres for a long term (more than a year)?

4.1 Institutional Setting

4.1.1 Asylum placement procedure

Asylum seekers arriving to the Netherlands by plane or at another point on the border can claim asylum once in the country. After been giving 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 COA which is responsible for accommodating them. I exploit this first allocation as an exogenous variation in where asylum seekers live.

At any time, COA manages centres throughout the country. When a family needs to be hosted, COA looks for a suitable location. The allocation is done centrally by COA and the municipality where the asylum seekers will live has no say in it. Since 1996, COA is responsible for the housing of all asylum seekers in the Netherlands (Beckers and Borghans, 2011). The main allocation criteria is availability of a place for a given family composition.

There are two obvious threats to the exogeneity of this allocation. First, asylum seekers could choose to live in a specific centre or that COA could send specific families to specific locations. Although possible on paper, this scenario seems unlikely in practise. From discussions with COA personnel, the main difficulty when allocating families is to find a centre ready to host a family of a certain size when it arrives in the country. The difficulty comes from the limited supply of housing (that pre-existed the 2014 spike in asylum applications).[19]

The second threat is the existence of family ties in the Netherlands. Asylum seekers with relatives or friends already living in the Netherlands could use their pre-existing ties to influence where they will 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 at COA. Since this allocation cannot be considered as exogenous, I only consider asylum seekers living in collective housing. Because there are no definite guidelines on how the allocation is made, I perform numerous statistical tests to back up the exogeneity of first assignment.

[19] The only case in which asylum seekers (or COA) could express a preference, would be if several centres had the capacity to host the same family at the same time. It is unfortunately not possible to isolate these cases in the data but it was not pointed as a frequent scenario in discussions with COA. To alleviate any related concern, I perform numerous balancing tests to back up the exogeneity of first assignment. Asylum seekers can ask to be relocated to another centre if for instance they have first grade family members in another centre of if they have an employment opportunity somewhere else. It can also be that some centres close down and asylum seekers are sent to another centre somewhere else. The latter case is arguably random, the former not. To make sure that I capture an exogenous variation, I focus on first placement.

4.1.2 How to identify the experimental population?

I combine information on the exact addresses and operating dates of all COA accommodation (meaning centres and addresses of relatives) for the period 1996 to 2012 with administrative registries. COA operates various types of centres (for adults, for minors, etc). Table 6 lists the type of accommodation which could be located by CBS.[20]

To identify the experimental population, I look at Non-Western immigrants who were first registered in a building listed by COA.[21][22] I exclude asylum seekers whose first placement is not a collective centre. If an address is listed both as a collective and an individual type of accommodation, I take a conservative approach and exclude this observation from the sample.[23]

4.2 Is assignment a good anchor for later residential choices?

In COA centres, inhabitants are free to go outside of the centre, their children go to local schools.[24] After 6 months, they can also look for a regular job. By no means are centres closed; living there means interacting with the local community. In Table 7, I calculate the number of asylum seekers still residing in the Netherlands 1 to 8 years after having arrived in the country. I also compute how many live in the same 4 digits zipcode than the one they were assigned. The zip4 level is very small (equivalent to a US census tract) and expecting people to live so close

[20] The same address can be listed under different types of centres, for instance a regular collective centre, an *Asielzoekerscentrum*, can also be listed as one that welcomes unaccompanied minors, a *Kleinschalige Centrale Opvangeenhdn* if a wing of the building is used specifically for minors. The second column shows how unique addresses are distributed among the different types of accommodation. Columns 3 to 5 show the same distribution respectively for addresses that appear twice, three or four times.

[21] Before 2000, COA would register asylum seekers to the municipality if they had been in a centre for a year. This is when they start appearing in registries. After 2000, registration happens after 6 months. This is how I reconstitute the beginning of their stay in a centre.

[22] A certain number of observations appear as being listed first in a non COA registered accommodation (neither single, nor collective housing) for a small period of time and then in a collective housing. If an individual spent less than one month in an unregistered location and then joined a collective housing, it is included in the analysis.

[23] I perform a last check to see if asylum seekers can be identified from municipal registries (where individuals are linked to an address). Since 1996, COA is responsible for the placement of asylum seekers, so all asylum seekers should be listed at least once as living in COA registered accommodation (meaning an active address at the time). I use the information provided by IND on migration motives to see whether all people listed as asylum seekers can be found that way. 73% of all people can be traced back with this approach. This is a very high number considering that only 85% of the addresses could be identified and anonymised by CBS.

[24] It is compulsory for children to attend local schools. I check that asylum seekers can be found in school registries to which I also have access. School registries maintained by CBS are comprehensive for middle and high school from 2004. I check that young asylum seekers identified through COA addresses (from 2004 so not the sample used in this paper) who are between 12 and 18 y.o. can be found in school registries. Considering that registries are often not updated in the middle of an academic year, many asylum seekers are in school but probably do not appear in registries. Therefore checking school registries gives a lower bound of school attendance. 57% of young asylum seekers can be found in school data. This is clearly indicative that children interact with the entire neighbourhood and not just within the centre.

Table 6: Summary Information on COA accommodation

Type of centre	Nb of centre appearing at the same address 1	2	3	4	Brief description
Aanvullende opvang	183	45	13	NA	Collective housing, for emergency when no other location available
Administratief geplaatst	2,354	859	199	42	Individual housing found by a.s. with contacts in the NL
Alternatieve tijdelijke capaciteit	15	NA	NA		
Asielzoekerscentrum	66	88	45	23	Collective housing
Contingent	52	31	17	NA	
Gemeentewoning	3,698	442	83	21	Individual housing for a.s. after being granted refugee status
Kinderwoongroep	20	NA	NA	NA	Small scale location for unaccompanied minors
Kleinschalige Centrale Opvangeenhdn	1,523	182	57	26	Small scale location for unaccompanied minors
Kleinschalige wooneenheid	15	NA	NA		Small scale location for unaccompanied minors
Opvang en Onderzoekcentrum	17	NA	NA	NA	Ter Apel centre where all a.s. start the application process
Orientatie & Inburgeringslocatie	16	24	12		Centre for people being denied refugee status prior to leaving
Terugkeerlocatie	61	37	23	NA	Centre for families being denied refugee status prior to leaving
Tijdelijke Noodvoorziening	23	21	15	NA	Collective housing, for emergency when no other location available
Zelf Zorg Arrangement	7,375	2,362	442	96	Individual housing found by a.s. with contacts in the NL
Total	15,321	4,170	966	288	

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 centres for addresses that identify a single centre. The third column shows the distribution among types of centres for addresses that identify two centres. The fourth column shows the distribution among types of centres for addresses that identify three centres. The fifth column shows the distribution among types of centres for addresses that identify four centres. The entry (2,4), i.e. 199 should be read as follows: among the addresses under which three centres are listed, 199 of them are *Administratief geplaatst*, meaning the same 199 addresses appear again twice in the same column under different types of centres. NA refers to entries where the number is below 10.

to the centre 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 median population around 20,000 inhabitants.

After 3 years, 28.2% of the assigned girls are living in the same municipality, 21% in the same zip4 (most likely still living in the centres where they were assigned). 34% of the people living in the same municipality after three years thus no longer in the same zip4. This could be driven by people living in large municipalities and moving from two (distant) points within the same municipality. This does not seem to be entirely the case. More than half of those who live in the same municipality but not in the same zip 4 live in an adjacent zip4 (54%). This shows that the assignment is significant in two ways: (i) many asylum seekers stayed in a centre for a long time (on average 23 months, see Table 2) (ii) it influences future residential choices.

Table 7: Mobility of the experimental population

	Nb still living in the NL	% living		
		In zip 4	Adjacent zip 4	In municipality
After 1 year	4,559	81.4	82.1	82.8
After 2 years	4,871	36.1	39.4	41.4
After 3 years	4,848	21	24.9	28.2
After 4 years	4,830	10.9	15.6	19.4
After 5 years	4,826	6.9	11.8	15.4
After 6 years	4,838	5.3	10.1	14.3
After 7 years	4,828	4.4	9.4	13.6
After 8 years	4,767	3.7	8.9	12.8

Note : For up to 8 years after arrival, this table reports the number of people still living in the Netherlands and for three different geographical areas (zip4, adjacent zip4 and municipality), the share still living close to the location of assignment. Row 6, middle table should be read as follows: after 6 years in the Netherlands, 4838 women (of the experimental population with complete spells) were still living in the Netherlands, 10.1% of them were still living in an adjacent zip4 to the one they were assigned.

4.3 Balancing tests

I distinguish two moments in the analysis, when asylum seekers are assigned to a location and when they turn 15 years old. Since the geographical unit on which I calculate community size is very small, I group origin countries into regions (as classified in Table A9). To calculate ethnic concentration, I pool together 1st and 2nd generation immigrants living in COA facilities or not. In Table 2, I report the main quantiles and the mean of the distributions of community sizes. There is substantial variation in the size of the ethnic community at assignment and at age 15. The 75^{th} percentile of the respective distributions is more than five times larger than the 25^{th} percentile (15 vs 83 people for community size at assignment, 32 vs 166 at age 15). I follow the literature (Bertrand et al. (2000); Aslund et al. (2003); Damm (2009); Aslund et al. (2011)) and use the log of that measure as my main variable of interest.[25]

Since the precise set of variables entering the allocation decision is unknown, the results of the balancing tests are indicative of which demographic characteristics matter in deciding where asylum seekers are sent and consequently on which controls to include in the main regressions. The balancing tests work as follows: I separate variables that should enter the assignment of asylum seekers, namely demographic characteristics (Z_i) from those that should be unrelated (X_i). To test the exogeneity of assignment, I see whether ethnic concentration at the time of arrival is related to Z_i and X_i. I regress $\ln e_{i,t,h}$ (the log of ethnic community size of individual i who arrived in year t in neighbourhood h) on the characteristics of the household head (identified at the time of arrival of the young girl).[26] Z_i includes gender, age at migration, the number of children below 15, potentially country of origin fixed effects at arrival and X_i includes dummy variables for education level (keeping in mind that information on X_i is available for 85% of household heads in the sample).[27] If balancing tests are satisfied, the identifying assumption should be read as follows: a household head arriving the same year t, with the same Z_i could be sent to different types of neighbourhoods. I estimate the following equation:

$$\ln e_{i,t,h} = \alpha + \beta \underbrace{X_i}_{\text{Unrelated}} + \gamma \underbrace{Z_i}_{\text{Related}} + \theta_t + \epsilon_{i,t,h} \quad (2)$$

Results for concentration at the zip 4 level are reported in Table 8. In columns (2), I add origin country fixed effects and in (3) I add municipality of assignment fixed effects. I also report the F-test of equality of all the education dummies together with the p-value associated with

[25]In a robustness check, I define ethnic community as immigrants from the same country, I restrict the sample to asylum seekers from the main origin countries, namely Iran, Iraq, Yugoslavia, Afghanistan, Somalia, Syria, Soviet Union, Angola, Zaire, Russia, Azerbaijan, Bosnia-Herzegovina, Turkey, Federal Republic of Yugoslavia, Sudan and Turkey. To further avoid taking the log of zeros, I add 1 to each community size.

[26]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 an head and thus are not included in the balancing tests.

[27]Descriptive statistics on household head's and children's characteristics are reported in Tables 1 and 2 respectively.

this test. I interpret th results of these tests as evidence of (no) sorting. It is clear from looking at Table 8 that neighbourhood composition is only correlated with demographic characteristics. More educated asylum seekers are no less likely to live in an ethnically concentrated area. The p-values of the F-tests are 0.95, 0.98 and 0.95 respectively without country FE, with them and when adding municipality fixed effects. There is no evidence that origin countries enter systematically in the allocation decision. There is also no evidence of sorting.

To give more credibility to these tests, I show that they have power against the alternative of sorting. I regress ethnic concentration in the neighbourhood where young women live when they are 15 on characteristics of the household head at that time (meaning the household head's age when their daughter turns 15, the number of children that year, etc). To make sure that individuals do sort into neighbourhoods, I focus on those (in the same sample) who do not live at age 15 in the same zip4 as they were assigned. To sum up, I run the same regressions at age 15 that I did at assignment but at a time where asylum seekers could select where to live. Regressions at age 15 should reject no sorting.

Results are reported in Table A7. The picture is very different from the regressions at assignment. There is clear evidence of sorting. In particular, highly educated people live in zip4 with smaller ethnic communities (26% smaller when estimating without municipality fixed effects, 25% when including them). The F-tests are very large, between 3.71 and 4.04 with associated p-values below 0.01. Being able to reject the null of no sorting in Table A7 gives credit to the results in Table 8 and establish the exogeneity of first assignment.

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. The results are reported in Table A11.

5 Empirical Analysis

5.1 Baseline results

I estimate the following equation:

$$y_i = \alpha + \beta \ln e_{i,h,t} + \pi X_i + \epsilon_{i,t} \tag{3}$$

Table 8: Balancing tests at the zip4 level

	(1)	(2)	(3)
Male	-0.098*	-0.152***	-0.99**
	(0.055)	(0.05)	(0.045)
Nb of children	-0.054***	-0.048**	-0.049***
	(0.02)	(0.02)	(0.017)
Age	0.004	0	-0.002
	(0.004)	(0.003)	(0.003)
Basic education	0.062	-0.029	-0.043
	(0.067)	(0.066)	(0.062)
Primary education	0.038	0.003	-0.006
	(0.072)	(0.068)	(0.063)
Middle school	0.039	-0.031	- 0.05
	(0.064)	(0.062)	(0.058)
High school	0.033	0.003	-0.033
	(0.084)	(0.08)	(0.075)
Higher education	0.01	-0.011	-0.052
	(0.09)	(0.086)	(0.079)
Arrival year FE	✓	✓	✓
Country FE	✗	✓	✓
Municipality FE (assignment)	✗	✗	✓
Nb of obs	4,176	4,176	4,176
F test	0.22	0.13	0.28
p-value	0.95	0.98	0.95

Note : This table estimates equation 2 on the sample of household heads (of women from the experimental population with complete spells). Ethnic concentration is measured as the log of the number of immigrants from the same region of the world in the zip4 the year of arrival to a COA centre. Explanatory variables include country of origin and year of arrival fixed effects together with gender of the head, age of the household head and number of children below 15 and dummies for education levels, where the baseline category is missing observation (15% of the sample). Standard errors are clustered at the country of origin and municipality level. F-test reports the test statistics of the null hypothesis: the coefficients for all education levels are zero.

where y_i is a dummy for having taken contraceptives at least once by the age of 20, $e_{i,h,t}$ is the community size of individual i in neighbourhood h at the beginning of year t. X_i are individual controls. I pursue three types of analysis: (i) the naive one in which I use concentration at age 15 as the variable of interest, (ii) an ITT strategy in which I use neighbourhood's ethnic concentration at the time of arrival and (iii) an IV strategy where I instrument concentration at age 15 with concentration at arrival.[28][29] The main result comes from the ITT specification. I report the IV specification for completeness.[30] This is a level log model and the coefficient of interest should be interpreted as follows: doubling the size of the ethnic community (increasing it by 100%) changes the probability of using contraceptives by β. The standard errors are clustered at the origin country and municipality level (Damm and Dustmann, 2014).

Results are reported in Table 9. The five columns report the results with different sets of controls. In column 1, they include origin country and year of arrival fixed effects, together with age at migration. Column 2 adds information on parents (those used in the balancing tests of Table 8) to account for family characteristics.[31] Columns 3 and 4 control for the characteristics of the environment. Column 3 includes a (time varying) neighbourhood quality index and the predicted share of contraceptive usage among 15 to 20 y.o. at the zip4 level.[32][33] The latter controls for access to hormonal contraceptives and GP practices at a very disaggregated level. Column 4 includes region fixed effects.[34]

[28]To be given a LATE interpretation, the instrument must satisfy monotonicity (a high/low ethnic concentration at arrival means a higher/lower concentration at age 15). The effect identified is the change in behaviour for those who live in a higher/lower (depending on the sense of the monotonicity) ethnic neighbourhood because they were assigned to a high/low ethnic neighbourhood when they arrived. Figures A1 shows evidence supporting monotonicity.

[29]An alternative IV strategy would be to instrument concentration at age 15 with the number of asylum seekers who have been assigned to the same zip4 in the years prior to assignment (as in Damm (2009)). Results of this strategy are reported in Table A20. The F test from the first stage are extremely low. This alternative strategy is not possible in this setting.

[30]Ethnic concentration enters linearly in the model. Currarini et al. (2009) showed that the patterns of friendship formation differ between small, large and medium sized groups. An alternative would be to include a quadratic term. The results (available upon request) also point to a null effect that is not quadratic.

[31]Note that for the naive estimation, I use information as measured the year in which a girl turns 15 y.o. and not the year of assignment (as in Table 8).

[32]I use information on the "quality" of zip4 areas provided by the Netherlands Institute for Social Research, the *status score*. Every four years, this government agency produces a ranking of all the 4 digit zipcode areas based on the average income in a neighbourhood, the percentage of people with a low income, the percentage of low-educated people and the percentage of people who do not work. These characteristics are summarized in one composite characteristic: the *status score* (fitting a line between the years).

[33]I regress individual level data on contraceptive usage for the period 2006 - 2018 on zip4 fixed effects and predict usage.

[34]These are COROP (Coördinatiecommissie Regionaal Onderzoeksprogramma) regions. They split the Netherlands into 40 areas. They are shown in figure A2. I do not use municipality fixed effects. As shown in Table 2, asylum seekers are spread over 217 municipalities and 384 zip4. Considering that zip4 is equivalent to centre level, adding municipality FE restricts the variation to within (on average) group of 1.8 centre. This is very restrictive. The COROP level, although higher, controls for potential "bible belt" effect. To account for differences at a more

In empirical applications, there is often no clear guidelines as of which variables should be picked as controls. Belloni et al. (2014) have developed a Double LASSO procedure to select control variables. In column 5, I use their algorithm to select control variables from the set used in (4). The algorithm first (i) performs LASSO of the outcome variable on the potential controls and then (ii) LASO of the main explanatory variable (log of the size of the ethnic community) on the same potential controls. The algorithm keeps the union of the variables selected in these two steps. This procedure is only available for OLS and is particularly suited for the setting of this paper. There are uncertainties surrounding which variables enter the allocation decision (and thus determines community size) and, little is known on the variables associated with the usage of hormonal contraceptives by teenage immigrants. Therefore, this is my preferred specification. Table A19 reports the variables selected as controls.[35] I use it for the ITT estimation.

The naive estimation is negative, doubling the size of the ethnic community is associated with a decrease in the probability of taking contraceptives by the age of 20, of around 1.5 percentage points. The ITT results are very small in magnitude and not statistically significant. My preferred specification is literally rounded to zero. The difference between the native and ITT estimations is indicative that sorting biases the estimates of neighbourhood ethnic concentration downwards. The null effect is precisely estimated. With the reported standard errors, the regressions would reject the null at the 5% level if the effect was of an order of +/- 1.2 percentage points. If doubling the size of the ethnic community had an effect similar to arriving 1 year and 2 months younger (result from the previous section), it would appear as statistically significant.

Low ITT estimates could be driven by the length of exposure being too small. Looking at the IV results focuses on a sub-population of compliers, who live in a larger ethnic community because of where they were assigned. It also inflates the coefficients and potentially allows to detect an effect. A reassuring element for the use of IV and the general validity of the setting is that first stage regressions are very large (F-test above 16 in all specifications). Results also point to a null effect.

Ethnic concentration is measured with all immigrants from the same origin living in the zip4. In Table 10, I reproduce the ITT analysis with alternative definitions of ethnic community.

disaggregated level, I use the controls of column (3).

[35] In addition to the variables listed in column (4) of Table 9, I add the square and a third polynomial term of the age at arrival (of the young woman and the household head) and the number of children. Dummy variables for categorical variables appear separately in the algorithm. The rational for using separate dummies is that the details of the allocation rules may have been different by years and origin countries.

Table 9: Neighbourhood Effects - Baseline Results

	(1)	(2)	(3)	(4)	(5)
			Panel A: Naive estimation		
Ethnic concentration	-0.014**	-0.014**	-0.016**	-0.013*	
	(0.0066)	(0.0066)	(0.0075)	(0.0071)	
Mean	0.427	0.427	0.426	0.427	
N Obs	4,897	4,897	4,886	4,897	
R squared	0.056	0.061	0.061	0.075	
			Panel B: ITT estimation		
Ethnic concentration	0.002	0.002	0.003	0.004	0.000
	(0.0056)	(0.0056)	(0.0058)	(0.006)	(0.006)
Mean	0.427	0.427	0.424	0.427	0.427
N Obs	4,909	4,909	4,717	4,909	4,909
R squared	0.055	0.063	0.065	0.075	0,034
			Panel C: IV estimation		
Ethnic concentration	0.027	0.026	0.034	0.054	
	(0.0655)	(0.0679)	(0.0737)	(0.0890)	
Mean	0.427	0.427	0.424	0.427	
N Obs	4,909	4,909	4,717	4,909	
R squared	0.047	0.052	0.051	0.049	
F-test	30.43	28.15	22.72	16.06	
Country FE	✓	✓	✓	✓	✗
Year of arrival FE	✓	✓	✓	✓	✗
Parental Characteristics	✗	✓	✓	✓	✗
Region FE	✗	✗	✗	✓	✗
Neighbourhood index	✗	✗	✓	✗	✗
Predicted teenage usage	✗	✗	✓	✗	✗
Double LASSO selection	✗	✗	✗	✗	✓

Note : 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 arrival fixed effects and age at migration (specification reported in column (1)). Additional controls are added successively, (2) adds household head characteristics, (3) adds the neighbourhood quality index and the predicted share of teenagers using contraceptives at the zip4 level while (4) add region fixed effects. The sample only includes teenage women with a complete spell. Standard errors are clustered at the year of arrival and municipality level. Panel A reports the "naive" estimation where ethnic concentration and control variables are measured when girls turn 15 years old. Panel B reports the ITT estimation where everything is measured at the time of assignment. Panel C reports the IV estimation where ethnic concentration at age 15 is instrumented by ethnic concentration at the time of arrival. Column (5) use a subset of the controls used in column (4), i.e. those selected according to the Double Debiased procedure (Belloni et al., 2014).

In panel A, it is measured as people from the same origin living in the same COA accomodation (zip6). This is consistent with the assumption that asylum seekers only interact with other asylum seekers in the COA centre (Beaman, 2012) and not with other immigrants in the neighbourhood. In panel B, it is restricted to immigrants with a Muslim background. The influential group is then defined as all immigrants with a Muslim background in the area (zip4). The influenced group becomes the part of the experimental population who has a Muslim background. In panel C, the ethnic community is the number of people from the same origin who are younger than 20 y.o., in panel D all immigrants from the same country in the zip4.

These additional results are also very small in magnitude and non-significant. Some specifications in panel A are different from zero. However, these results are not consistent across columns. In particular, they do not hold for the preferred specification (controls selected by Double LASSO). The different results all point to a null effect.

5.2 Heterogeneity and potential mechanisms

It can be that the null effect is the average between a positive and a negative result. The effect could be significantly positive/negative for some relevant subgroups. Looking at treatment effect heterogeneity can unveil these scenarios. It is also indicative of potential mechanisms at play.

I look at four particular dimensions of heterogeneity: (i) the age at which a young woman arrived in the Netherlands (above/below 9 years old; the closest integer to the 75^{th} percentile, see Table 2), (ii) whether she stayed in a COA accommodation for a period longer or shorter than a year, (iii) whether the centre was small/large (more than 100 asylum seekers; close to the median of centre size, see Table 2) and (iv) the education level of her household head (i.e. if it is below high school).

Age at arrival should capture if exposure to co-ethnics is more important during childhood or closer to teenage years. Household head's education is a key family characteristic. Focusing on durations larger than a year removes short stays.[36] Looking at centre size tells us if specific centre 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.

[36] I reproduce the balancing tests in Table A8. Although, asylum seekers who stayed longer than a year are a selected subsample, we still observe that their first assignment is as good as random.

Table 10: Baseline Results - ITT Estimates - Varying the influential peer group

	(1)	(2)	(3)	(4)	(5)
	Panel A: ZIP6 level analysis				
Ethnic concentration	0.009*	0.008	0.005	0.011**	0.002
	(0.005)	(0.005)	(0.0052)	(0.0053)	(0.0051)
Mean	0.427	0.427	0.424	0.427	0.427
N Obs	4,909	4,909	4,717	4,909	4,909
R squared	0.056	0.064	0.066	0.075	0.032
	Panel B: ZIP4 analysis for Muslims only				
Ethnic concentration	-0.000	-0.001	-0.000	0.003	0.003
	(0.0061)	(0.0061)	(0.0066)	(0.0071)	(0.007)
Mean	0.392	0.392	0.39	0.392	0.392
N Obs	3,413	3,413	3,266	3,413	3,413
R squared	0.037	0.049	0.051	0.065	0,027
	Panel C: ZIP4 level analysis with young people				
Ethnic concentration	0.004	0.003	0.004	0.004	-0.001
	(0.0059)	(0.0059)	(0.0062)	(0.0063)	(0.0063)
Mean	0.427	0.427	0.424	0.427	0.427
N Obs	4,909	4,909	4,717	4,909	4,909
R squared	0.055	0.063	0.065	0.075	0.031
	Panel D: ZIP4 level analysis with same country of origin				
Ethnic concentration	0.006	0.004	0.002	0.007	0.006
	(0.0072)	(0.0072)	(0.0073)	(0.0072)	(0.0071)
N Obs	4,167	4,167	4,006	4,167	4,167
R squared	0.037	0.047	0.05	0.06	0.038
Country FE	✓	✓	✓	✓	✗
Year of arrival FE	✓	✓	✓	✓	✗
Parental Characteristics	✗	✓	✓	✓	✗
Region FE	✗	✗	✗	✓	✗
Neighbourhood index	✗	✗	✓	✗	✗
Predicted teenage usage	✗	✗	✓	✗	✗
Double LASSO selection	✗	✗	✗	✗	✓

Note : 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 arrival fixed effects and age at migration (specification reported in column (1)). Additional controls are added successively, (2) adds household head characteristics, (3) adds the neighbourhood quality index and the predicted share of teenagers using contraceptives at the zip4 level while (4) add region fixed effects. The sample only includes teenage women with a complete spell. Standard errors are clustered at the year of arrival and municipality level. All the panels report estimation where controls and ethnic concentration are measured at the time of assignment. Panel A defines the influential group as being immigrants from the same region living in the COA centre. Panel B defines the influential group as being all other immigrants with a Muslim background. This sample is further restricted to immigrants who themselves have a Muslim background. Panel C defines the influential group as the young immigrants (younger than 20 years old). Panel D defines the influential group as being the immigrants from the same country living in the zip4. Column (5) use a subset of the controls used in column (4), i.e. those selected according to the Double Debiased procedure (Belloni et al., 2014).

Since, the heterogeneity analysis is not reported in the pre-analysis plan, 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 to overcome the curse of dimensionality, common in non-parametric estimation. 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 used in the grf algorithm are those used in specification (4) of Table 9.

Table 11: Treatment Effect Heterogeneity - Generalized Random Forests - Baseline

	Coefficient	Standard Errors	T-test	Nb Observations
		Panel A: Single characteristics		
Age migration $<$ 9 y.o.	-0.001	(0.006)	0.105	3,789
Age migration $\geq$ 9 y.o.	-0.009	(0.015)	0.565	1,120
H. Head low education	-0.002	(0.007)	0.303	4,067
H. Head high education	-0.007	(0.013)	0.510	842
Length of stay $\geq$ 1 year	0.002	(0.006)	0.252	3,933
Centre $\geq$ 100 people	-0.002	(0.012)	0.188	2,390
Centre $<$ 100 people	0.000	(0.007)	0.007	2,519
		Panel B: Combination of characteristics		
Low educ & long stay	0.001	(0.007)	0.203	3,244
High educ & long stay	-0.003	(0.014)	0.246	689
Large centre & long stay	0.001	(0.013)	0.112	1,768
Small centre & long stay	0.003	(0.008)	0.434	2,165
Young & low education	-0.001	(0.007)	0.103	3,079
Young & high educ	-0.006	(0.014)	0.429	710
Young & long stay	0.004	(0.007)	0.590	3,296
Old & low educ	-0.007	(0.017)	0.398	988
Old & high educ	-0.020	(0.034)	0.581	132
Old & long stay	-0.015	(0.019)	0.795	637
Average Effect (ITT)	-0.003	(0.006)	0.451	4,909

Note: 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 years old. The following variables are used to build causal forests: country of origin, year of arrival, age at migration, household head characteristics (including education) and region of assignment. Each row reports the effects of ethnic concentration (measured as the log of co-ethnics at the time of assignment) on a specific subgroup. Panel A reports the results on subgroups characterized by one element (on age at arrival, education level of the household head, length of stay in the centre and size of the centre). Panel B reports results on subgroups made by a combination of two characteristics. The last column reports 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 year of arrival and municipality level.

Results are reported in Table 11. Panel A reports conditional average treatment effect (CATE) for one of the four characteristics. It reports coefficients (which should be interpreted at CATE version of equation 3), standard errors together with the size of the subgroup. Panel B reports the same information when the CATE is estimated for two characteristics. 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. All of the estimates are very small in magnitude and statistically insignificant. There is no evidence that an average null result hides a larger effect on a meaningful sub-population.

6 Robustness Checks

I make four robustness checks. First, the relevant outcome may not be having used contraceptives by age 20 but starting to use them younger. To address this, I estimate a duration model rather than linear regressions. Second, the zip4 may not be the relevant level. It could be too small and miss part of the social interactions. Third, the zero result could in fact be a noisy effect that would become significant if the sample size was larger. Four, it could be that the entire strategy is not able to capture any effect for any outcome. This could be the case if, for instance, asylum seekers stay too little time in the location where they were assigned.

Using duration models In Table A12, I reproduce the baseline analysis but use a Weibull MLE on the sample stacking all the years (rather than linear models on the collapsed data at age 20). The IV estimation is a control function for duration models (Coviello et al., 2015). The results are very similar to the linear specifications. The naive estimation is negative (-5.5 to -7 % decrease in the hazard rate) while the ITT and IV point to a null effect.

Increasing the sample size Table A13 reproduces the baseline analysis on the sample of *complete* and *incomplete spells*. The picture remains dramatically the same. The naive estimates are negative and significant (-1.5 p.p.), while the ITT and IV are small and insignificant. Table A14 reproduces the heterogeneity analysis. Switching to the *complete* and *incomplete spell* sample increases the number of observations in certain subgroups and the ability to capture heterogeneous effects. Results remain small and insignificant. All but one group have t-tests below 1. Only one subgroup has a marginal negative effect, i.e. girls who arrived after 9 y.o. and whose household head has at least high school education. This group represents 5% of the entire sample. This result does not change the picture of the paper.

Doing the analysis at the municipality level Table A15 reproduces the baseline analysis when concentration is calculated at the municipality level rather than at the zip4 level. The naive estimates are negative but much smaller in magnitude and non significant (around -0.5 p.p.). This is indicative that sorting takes place at the neighbourhood rather than at the municipality level. Besides this difference, the other results are very similar, i.e. the ITT and IV point to a null effect.

Falsification test: Looking at another outcome I take an outcome on which previous research has shown a non-zero effect and I see whether this strategy can capture it. Aslund et al. (2011) have shown the effect of living in an ethnic enclave on young immigrants' educational achievements. Their evidence comes from a Swedish 'dispersal' policy, I take their work as a starting point. I want to choose an outcome as close as possible to theirs to minimize the risk of manipulation. I also want the falsification test to be as close as possible to the main analysis of this paper. In particular, I want the outcome to be a binary variable as is the case for contraceptive usage.

Therefore, I focus on an outcome has completed a certain education level by age 20. I focus on the same sample, meaning women with *complete* and *incomplete* (contraceptive) spells. The analysis by Aslund et al. (2011) focused on GPA in high school. I look at the outcome has followed an education level (in the Netherlands) as high as the level (in Sweden) for which Aslund et al. (2011) use GPA.[37]

Table A16 replicates the ITT and IV baseline analysis on both samples while tables A17 and A18 report heterogeneity analysis for the *complete* and *incomplete spell* samples respectively. A different picture emerges when looking at educational rather than cultural outcomes. The ITT is significant for the preferred specification (selecting controls using Double LASSO), -1.3 p.p. for an unconditional mean of 55%. The unconditional mean for native young women in the same age range (those followed in Table 3) is 60%. Considering that the difference between natives and asylum seekers is 5.5 percentage points, the effect estimated in Table A16 is qualitatively large. Turning to heterogeneity analysis for the small sample, the effect of increased exposure to co-ethnics is negative for meaningful subgroups. The effect of -1 p.p. is significant at the 10% level for girls who arrived before age 9 and for those with low educated parents, two relevant subgroups (each representing 75% of the sample).

The picture remains the same but seems even more pronounced when looking at the *complete* and *incomplete spell* sample. Both the ITT and IV baseline results show a negative effect, -1.4

[37]To identify the corresponding level in the Netherlands, I rely on an online document that can be found at the adress https://www.nuffic.nl/en/publications/education-system-sweden/ in particular on page 5. Compuslory education in Sweden corresponds to at least HAVO, VWO in the Netherlands. I focus on the highest education followed not completed to account for those who did not pass but would have a GPA in the Swedish example. The binary variable takes value one if the highest education followed is HAVO, VWO, HBO and WO.

p.p. for the preferred ITT specification. Heterogeneity analysis confirms the picture that emerges from Table A17. The negative effect appears for the same subgroups but at the 5 and not 10% level when a larger sample is used.[38]

Looking at another outcome does not only give 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 tables A17 and A18 provide material to understand mechanisms through which ethnic concentration operates. Early exposure matters most, which is consistent with the findings from Chetty et al. (2016). The fact that the effect is larger for household heads with low education is indicative of how family and neighbourhood characteristics interact. More educated parents can always ”make up” for the environment. As shown in Currarini et al. (2009), small groups tend to exhibit higher levels of segregation. Smaller centres may have less contact with non co-ethnics in their environment. This would explain why the effect is larger for that group.

None of these dimensions (and the potential explanations they carry with them) matter when it comes to cultural behaviour. If the main focus of the paper was educational achievements and not adoption of cultural behaviour, the conclusion would not be a (uniformly) null effect.

7 Discussion and Conclusion

This section discusses how a null effect on neighbourhood influence can be informative about the process of cultural assimilation. Abadie (2020) discusses the benefits of reporting non significant results in empirical economics. In a simple model of Bayesian updating, he shows that failing to reject a null hypothesis often brings more information about parameters location. In the case of this paper, it allows to cross out neighbourhood influence has an important mechanism. I focus on one type of horizontal channel (Bisin and Verdier, 2000; Bisin et al., 2004); neighbour peer effects. Ex ante this group was a candidate mechanism as shown by the vast research on the subject (Kling et al., 2007; Chetty et al., 2016).

[38]Note that the estimates per subgroup are very similar in tables A17 and A18. The composition of the sample is however different; the incomplete spell sample arrived older. There is a larger proportion of girls who arrived after age 9. The F-test in panels B and D of Table A16 which measure the strength of location at assignment to predict location at age 15, are larger as could be expected for an assignment closer to age 15. Since the effect is heterogeneous by age at arrival, it can be that CATE are the same in the complete and incomplete spell samples but give a slightly different unconditional effect (-0.8 p.p. versus -1.2 p.p.) due to these differences in composition. For the outcome ’taking contraceptives’ the CATE are similar by age at arrival (none is statistically significant). Therefore there are fewer differences in the unconditional effects estimated in tables 11 and A14.

By studying neighbourhoods, I pool together people from different ages who may have weaker/stronger ties to young immigrants. Future research should focus on the potential influence of different peer groups from different social activities who are potentially closer to the population of interest.

A few interesting facts about the role played by the family also emerges from this study. Family observable characteristics play a much smaller role in explaining cultural assimilation than educational achievements. For instance, the R^2 of the regressions reported in tables 9 and A13 (when contraceptive is the main outcome) is much smaller (between 2 and 3 times) than those in Table A16 (when education is the main outcome). The double LASSO algorithm (Belloni et al., 2014) selects dummies for household head education as relevant control variables when the outcome of interest is educational level but not when it is contraceptive usage (see Table A19). This means that there is no evidence that immigrants whose parents have a higher education level are more likely to assimilate culturally while their educational achievements will likely be higher.

This picture is confirmed when running a LASSO on the sample and the controls used in column C of Table 4, i.e. LASSO on the sample used to show descriptive evidence on convergence. Parental education is not selected by the algorithm as a relevant covariate. This may not be surprising for father's education but even mother's education is not associated with the probability of using contraceptives.[39] These results are in line with Bisin et al. (2008) who find no correlation between the strength of religious identity and education level. Many of the family characteristics that are associated with children doing better at school do not play in favour of strong cultural assimilation.

To sum up, I study if immigrants' cultural behaviour converges with natives' and if this process is influenced by neighbourhood ethnic composition. I measure culture with contraceptive usage by young women. I show that there is evidence of convergence in behaviours: immigrants who arrived younger are more likely to use contraceptives. However, the rate of assimilation is low. There are very large differences between natives and immigrants who grew up in the same country. I see if assimilation can be speeded up by limiting the formation of ethnic enclaves. I exploit the placement of asylum seekers in the Netherlands as an experiment which brings exogenous variation in neighbourhood characteristics. I do not find that growing up in a environment

[39]The LASSO algorithm, using the parameters specified in (Belloni et al., 2014), selects eight origin countries, family size, age at migration and having a Muslim background as meaningful variables.

with a large ethnic community has as effect on the probability of using contraceptives in the ages 15 to 20. This result is indicative that the role played by the environment (horizontal channel) is weak. Unobservable family characteristics are more likely to be of first order importance to understand cultural assimilation.

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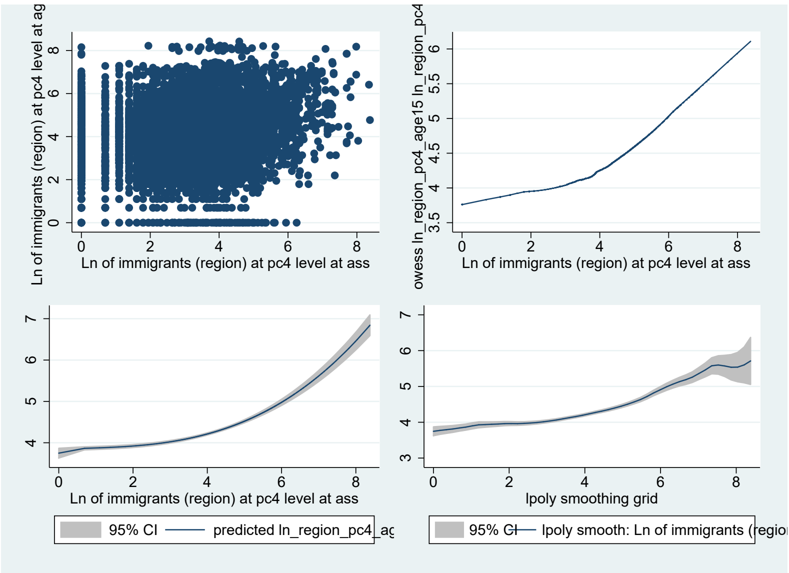
8 Appendix

Figure A1: Community size at assignment and age 15



Note : I plot the log of ethnic concentration at age 15 against the log of ethnic concentration at the time of assignment. Ethnic concentration is defined as immigrants from the same region and neighbourhood refers to zip4. The figure is split into four quadrants, in the upper left one, I scatter the points, in the upper right one I approximate the scatter of points with a locally weighted regressions, in the lower left one I use a quadratic approximation and in the lower right one a kernel-weighted local polynomial.

Figure A2: Map of the COROP

Table A1: Origin countries of the first generation immigrants reported in table 3

	Nb Obs	% of sample	% cumulated
Afghanistan	2,137	12.44	12.44
Irak	1,692	9.85	22.28
Morocco	1,340	7.80	30.08
Turkey	1,326	7.72	37.80
Germany	998	5.81	43.61
Surinam	841	4.89	48.50
Dutch Antilles	826	4.81	53.31
Yougoslavia	608	3.54	56.85
Iran	488	2.84	59.69
Poland	451	2.62	62.32
Somalia	297	1.73	64.04
Russia	268	1.56	65.60
Syria	251	1.46	67.06
Bosnia-Herzegovina	240	1.40	68.46
U.S.A.	234	1.36	69.82
Federal Republic of Yougoslavia	210	1.22	71.05
Azerbaijan	196	1.14	72.19
South-Africa	188	1.09	73.28
China	188	1.09	74.37
Pakistan	164	0.95	75.33
Indonesia	160	0.93	76.26
Armenia	151	0.88	77.14
Portugal	150	0.87	78.01
Soviet Union	139	0.81	78.82
Spain	136	0.79	79.61
Aruba	130	0.76	80.37
Egypt	129	0.75	81.12
Italy	119	0.69	81.81
Brasil	118	0.69	82.50
Colombia	116	0.68	83.17
Zaire	113	0.66	83.83
Bulgaria	112	0.65	84.48
Sri Lanka	108	0.63	85.11
Angola	104	0.61	85.72
Philippines	101	0.59	86.31

Note : This table reports the main origin countries of the first generation immigrants followed in table 3. The first column reports the absolute numbers, the second one the share of a particular origin and the third one the cumulative share.

Table A2: Origin countries of the second generation immigrants reported in table 3

	Nb Obs	% of sample	% cumulated
Turkey	21,143	22.07	22.07
Morocco	18,988	19.82	41.90
Surinam	13,289	13.87	55.77
Indonesia	4,679	4.89	60.66
Dutch Antilles	3,854	4.02	64.68
Germany	3,350	3.50	68.18
Yougoslavia	2,136	2.23	70.41
China	1,374	1.43	71.84
Australia	1,260	1.32	73.16
Poland	1,153	1.20	74.36
United States of America	1,078	1.13	75.49
Spain	1,000	1.04	76.53
Italy	975	1.02	77.55
Egypt	974	1.02	78.57
Duth New Guinea	944	0.99	79.55
Cape Verde	880	0.92	80.47
Vietnam	870	0.91	81.38
Pakistan	827	0.86	82.24
South-Africa	776	0.81	83.05
Ghana	723	0.75	83.81
Iran	690	0.72	84.53
Hong-Kong	664	0.69	85.22
Philippines	594	0.62	85.84
Irak	585	0.61	86.45
Brasil	565	0.59	87.04
Portugal	531	0.55	87.60

Note : This table reports the main origin countries of the second generation immigrants followed in table 3. The first column reports the absolute numbers, the second one the share of a particular origin and the third one the cumulative share.

Table A3: Descriptive Statistics of the Sample followed in table 4

Age at Arrival	
25th percentile	3.9
Median	6.3
75th percentile	9.4
Mean	6.9

Year of Arrival	
10th percentile	1996
25th percentile	1998
Median	2000
75th percentile	2003
90th percentile	2007

Origin Country			
Afghanistan	2,072	17.08	17.08
Irak	1,614	13.30	30.38
Turkey	1,048	8.64	39.02
Morocco	966	7.96	46.98
Iran	443	3.65	50.63
Poland	413	3.40	54.03
Germany	409	3.37	57.41
Yougoslavia	381	3.14	60.55
Surinam	380	3.13	63.68
Somalia	280	2.31	65.99
Russia	240	1.98	67.96
Syria	235	1.94	69.90
Bosnia-Herzegovina	215	1.77	71.67
Federal Republic of Yougoslavia	201	1.66	73.33
Azerbaijan	196	1.62	74.94
China	168	1.38	76.33
Armenia	146	1.20	77.53
Soviet Union	135	1.11	78.65
Portugal	128	1.05	79.70
Pakistan	108	0.89	80.59
Bulgaria	106	0.87	81.46

Note : This table reports descriptive statistics of the teenage women followed in table 4. Panel A reports elements of the distribution of age at arrival and panel B of year of arrival. Panel C reports the main origin countries (absolute numbers, raw and cumulated shares).

Table A4: Descriptive Statistics of the Sample followed in table 5

Age at Arrival	
25th percentile	4.4
Median	6.6
75th percentile	9.3
Mean	7

Year of Arrival	
10th percentile	1997
25th percentile	1999
Median	2000
75th percentile	2003
90th percentile	2006

Origin Country			
Afghanistan	836	25.30	25.30
Irak	538	16.28	41.59
Morocco	312	9.44	51.03
Turkey	233	7.05	58.08
Yougoslavia	110	3.33	61.41
Germany	107	3.24	64.65
Somalia	101	3.06	67.71
Federal Republic of Yougoslavia	84	2.54	70.25
Suriname	71	2.15	72.40
Syria	69	2.09	74.49
Azerbaijan	64	1.94	76.42
Poland	56	1.69	78.12
Iran	53	1.60	79.72
Russia	52	1.57	81.30

Note : This table reports descriptive statistics of the teenage women followed in table 5. Panel A reports elements of the distribution of age at arrival and panel B of year of arrival. Panel C reports the main origin countries (absolute numbers, raw and cumulated shares)

Table A5: Convergence Analysis - Baseline Results

Age at arrival	(A)	(B)	(C)
Immigrants (non-Muslim background)	-0.031***	-0.0366***	-0.034***
	(0.00419)	(0.0069)	(0.01021)
Immigrants (Muslim background)	-0.0344***	-0.0397***	-0.0363***
	(0.00506)	(0.00744)	(0.00992)
Nb Obs (Total)	60,665	60,665	36,968
Nb Obs (Muslim background)	39,580	39,580	26,053
Mean	28.1	28.1	25.7
Country FE	✓	✓	✓
Year of arrival FE	✗	✓	✓
Individual characteristics	✗	✗	✓

Note : Each column reports the results of a Weibull regression where the outcome is the point in the spell (15 to 20 years old) where young women have used contraceptives for the first time. The explanatory variables are age at arrival (interacted with a dummy for having a Muslim background) and a series of controls.The first column reports results with country fixed effects, the second adds year of arrival fixed effects and the last one adds individual characteristics (parents' education level, family size and municipality of residence fixed effects). The regressions are estimated on the sample of first generation women who were living in the Netherlands between 15 and 20 years old during the period 2006 - 2018.

Table A6: Grouping countries into regions

Country	Nb obs	% of the sample
Afghanistan	1,275	23.68
Iraq	938	17.42
Somalia	366	6.80
Iran	299	5.55
Yugoslavia	211	3.92
Azerbaijan	200	3.71
Russia	199	3.70
Syria	196	3.64
FR Yugoslavia	156	2.90
Angola	150	2.79
Armenia	147	2.73
Bosnia Herzegovina	127	2.36
Soviet Union	106	1.97
Turkey	97	1.80
Comores	92	1.71
Sudan	87	1.62
Burundi	57	1.06
Ethiopia	52	0.97
Ukraine	42	0.78
Georgia	36	0.67
Sri Lanka	36	0.67
Pakistan	34	0.63
Colombia	32	0.59
Myanmar	21	0.39
Rwanda	20	0.37

Note : This table shows the distribution of origin countries for asylum seekers who are part of the experimental population (complete spells).

Table A7: Balancing tests at the zip4 level - Power of the test

	(1)	(2)	(3)
Male	-0.037	-0.126***	-0.11***
	(0.057)	(0.048)	(0.042)
Nb of children	-0.037*	-0.015	0.004
	(0.02)	(0.018)	(0.017)
Age	0.013***	0.008**	0.009***
	(0.004)	(0.004)	(0.003)
Basic education	0.136*	-0.018	-0.005
	(0.78)	(0.068)	(0.06)
Primary education	0.119	0.044	0.05
	(0.078)	(0.068)	(0.062)
Middle school	0.065	-0.028	- 0.05
	(0.071)	(0.064)	(0.059)
High school	-0.003	-0.086	-0.068
	(0.093)	(0.08)	(0.077)
Higher education	-0.204**	-0.26***	-0.248***
	(0.087)	(0.077)	(0.072)
Arrival year FE	✓	✓	✓
Country FE	✗	✓	✓
Municipality FE (age 15)	✗	✗	✓
Nb of obs	3,607	3,607	3,607
F test	3.97	3.71	4.04

Note : This table estimates equation 2 on the sample of household heads (of women from the experimental population with complete spells). Ethnic concentration is measured as the log of the number of immigrants from the same region of the world in the zip4 the year the oldest daughter turns 15. Explanatory variables include country of origin and year of arrival fixed effects together with gender of the head, age of the household head and number of children below 15 and dummies for education levels, where the baseline category is missing observation (15% of the sample). Standard errors are clustered at the country of origin and municipality level. The sample is restricted to household heads who do not live in the same zip4 (when the oldest daughter turns 15) where they were assigned. F-test reports the test statistics of the null hypothesis: the coefficients for all education levels are zero.

Table A8: Balancing tests at the zip4 level - Long stays in C0A centres ($\geq$ 1 year)

	(1)	(2)	(3)	(4)	(5)	(6)
Male	-0.58	-0.001	-0.145**	-0.104**	-0.094*	-0.094**
	(0.63)	(0.64)	(0.059)	(0.053)	(0.053)	(0.047)
Nb of children	-0.054**	-0.031	-0.05**	-0.016	-0.049**	0.002
	(0.23)	(0.22)	(0.023)	(0.02)	(0.02)	(0.19)
Age	0.003	0.011	-0.003	0.007*	-0.004	0.007*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Basic education	0.004	0.0154*	-0.076	0	-0.65	-0.004
	(0.078)	(0.086)	(0.077)	(0.075)	(0.071)	(0.068)
Primary education	0.044	0.08	0.023	0.03	0.02	0.041
	(0.081)	(0.087)	(0.077)	(0.078)	(0.71)	(0.71)
Middle school	0.006	0.039	-0.059	-0.045	-0.073	-0.007
	(0.074)	(0.076)	(0.072)	(0.07)	(0.067)	(0.066)
High school	-0.022	0.023	0.006	-0.006	-0.054	0.004
	(0.098)	(0.102)	(0.092)	(0.092)	(0.086)	(0.085)
Higher education	-0.016	-0.291***	-0.001	-0.326***	-0.044	-0.284***
	(0.1)	(0.098)	(0.095)	(0.089)	(0.085)	(0.082)
Country FE	✓	✓	✓	✓	✓	✓
Arrival year FE	✗	✗	✓	✓	✓	✓
Municipality FE (assignment)	✗	✗	✗	✗	✓	✗
Municipality FE (age 15)	✗	✗	✗	✗	✗	✓
Nb of obs	3,336	2,870	3,336	2,870	3,336	2,771
F test	0.15	5.07	0.72	4.45	0.73	4.13
p-value	0.98	0	0.61	0	0.6	0

Note : This table estimates equation 2 on the sample of household heads (of women from the experimental population with complete spells who stayed longer than a year at the place of assignment). Ethnic concentration is measured as the log of the number of immigrants from the same region of the world in the zip4. In columns (1), (3) and (5) concentration is measured the year of arrival to a COA centre, in columns (2), (4) and (6), it is measured the year the oldest daughter turns 15. Explanatory variables include country of origin and year of arrival fixed effects together with gender of the head, age of the household head and number of children below 15 (at assignment in (1) and (3), when the oldest daughter turns 15 in (2) and (4)) and dummies for education levels, where the baseline category is missing observation (15% of the sample). Standard errors are clustered at the country of origin and municipality level (municipality at assignment for (1), (3) and (5), at age 15 for (2), (4) and (6)). In columns (2), (4) and (6), the sample is restricted to household heads who do not live in the same zip4 (when the oldest daughter turns 15) where they were assigned. F-test reports the test statistics of the null hypothesis: the coefficients for all education levels are zero.

Table A9: 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: Country groupings according to the M49 standard description used by the United Nations Statistics Division.

Table A10: 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 A11: Reallocating asylum seekers between centres

Year	χ^2 - Statistics	Degrees of freedom	Critical values 95^{th} percentile
1996	131.6	2,388	2,963
1997	158	3,234	3,367
1998	189.5	4,422	4,577
1999	264.3	5,430	5,603
2000	322.9	7,554	7,757
2001	365.1	7,230	7,429
2002	155.3	5,862	6,041
2003	96.5	3,342	3,477
2004	34	1,614	1,708
2005	43	1,182	1,263
2006	22	966	1,039
2007	16	714	777
2008	21	858	927
2009	35	1,038	1114
2010	24	1,110	1189
2011	13	822	890
2012	7	354	399

For each arrival year, I randomly reallocate household heads (who arrived that year) to COA centres (opened that year) 500 times. I calculate the average number of household heads with a particular education level in each COA centre in 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).

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

Where $n_{c,j}$ is the number of household head with education level j in centre c (where C is the total number of centres opened that year) and $\hat{n}_{c,j}$) is the predicted number 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. Following (Ammermueller and Pischke, 2009):

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

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. 6) and the number of COA centres 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.

Table A12: Neighbourhood Effects - Duration Analysis

	(1)	(2)	(3)	(4)
	Panel A: Naive estimation			
Ethnic concentration	-0.055**	-0.059**	-0.069**	-0.057**
	(0.0229)	(0.0229)	(0.0258)	(0.0247)
Mean	0.397	0.397	0.397	0.397
N Obs	4,660	4,660	4,650	4,660
	Panel B: ITT estimation			
Ethnic concentration	0.000	-0.001	-0.003	0.002
	(0.0185)	(0.0189)	(0.00199)	(0.0214)
Mean	0.397	0.397	0.395	0.397
N Obs	4,672	4,672	4,492	4,672
	Panel C: IV estimation			
Ethnic concentration	0.008	-0.01	-0.026	0.05
	(0.2192)	(0.2303)	(0.2501)	(0.3109)
Mean	0.397	0.397	0.395	0.397
N Obs	4,672	4,672	4,492	4,672
F-test	30.43	28.15	22.72	16.06
Country FE	✓	✓	✓	✓
Year of arrival FE	✓	✓	✓	✓
Parental Characteristics	✗	✓	✓	✓
Region FE	✗	✗	✗	✓
Neighbourhood index	✗	✗	✓	✗
Predicted teenage usage	✗	✗	✓	✗

Note : This table reports estimations of equation 3 using Weibull ML estimation. The outcome variable is the point in the spell (15 to 20 years old) where young women have used contraceptives for the first time. All specifications control for country of origin, year of arrival fixed effects and age at migration (specification reported in column (1)). Additional controls are added successively, (2) adds household head characteristics, (3) adds the neighbourhood quality index and the predicted share of teenagers using contraceptives at the zip4 level while (4) add region fixed effects. The sample only includes teenage women with a complete spell. Standard errors are clustered at the year of arrival and municipality level. Panel A reports the ”naive” estimation where ethnic concentration and control variables are measured when girls turn 15 years old. Panel B reports the ITT estimation where everything is measured at the time of assignment. Panel C reports the IV estimation where ethnic concentration at age 15 is instrumented by ethnic concentration at the time of arrival.

Table A13: Neighbourhood Effects - Increasing sample size

	(1)	(2)	(3)	(4)	(5)
			Panel A: Naive estimation		
Ethnic concentration	-0.015**	-0.016**	-0.015**	-0.016**	
	(0.0056)	(0.0055)	(0.0063)	(0.0060)	
Mean	0.4	0.4	0.4	0.4	
N Obs	6,794	6,794	6,771	6,794	
R squared	0.057	0.062	0.062	0.07	
			Panel B: ITT estimation		
Ethnic concentration	0.005	0.004	0.004	0.005	0.003
	(0.0045)	(0.0045)	(0.0047)	(0.005)	(0.0051)
Mean	0.4	0.4	0.398	0.4	0.4
N Obs	6,866	6,866	6,588	6,866	6,866
R squared	0.056	0.063	0.065	0.071	0,037
			Panel C: IV estimation		
Ethnic concentration	0.053	0.048	0.057	0.074	
	(0.0515)	(0.0529)	(0.0579)	(0.0758)	
Mean	0.4	0.4	0.398	0.4	
N Obs	6,866	6,866	6,588	6,866	6,866
R squared	0.031	0.04	0.035	0.026	
F-test	42.621	39.68	33.06	20.54	
Country FE	✓	✓	✓	✓	✗
Year of arrival FE	✓	✓	✓	✓	✗
Parental Characteristics	✗	✓	✓	✓	✗
Region FE	✗	✗	✗	✓	✗
Neighbourhood index	✗	✗	✓	✗	✗
Predicted teenage usage	✗	✗	✓	✗	✗
Double LASSO selection	✗	✗	✗	✗	✓

Note : 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 arrival fixed effects and age at migration (specification reported in column (1)). Additional controls are added successively, (2) adds household head characteristics, (3) adds the neighbourhood quality index and the predicted share of teenagers using contraceptives at the zip4 level while (4) add region fixed effects. The sample includes teenage women with a complete spell and those who are observed from age 18 to age 20. Standard errors are clustered at the year of arrival and municipality level. Panel A reports the "naive" estimation where ethnic concentration and control variables are measured when girls turn 15 years old. Panel B reports the ITT estimation where everything is measured at the time of assignment. Panel C reports the IV estimation where ethnic concentration at age 15 is instrumented by ethnic concentration at the time of arrival. Column (5) use a subset of the controls used in column (4), i.e. those selected according to the Double Debiased procedure (Belloni et al., 2014).

Table A14: Treatment Effect Heterogeneity - Generalized Random Forests - *Incomplete spell* sample

	Coefficient	Standard Errors	T-test	Nb Observations
		Panel A: Single characteristics		
Age migration $<$ 9 y.o.	-0.001	(0.006)	0.193	4,433
Age migration $\leq$ 9 y.o.	-0.009	(0.009)	0.956	2,433
H. Head low education	-0.002	(0.006)	0.314	5,667
H. Head high education	-0.011	(0.011)	1.002	1,199
Length of stay $\leq$ 1 year	0.001	(0.005)	0.250	5,663
Centre $\leq$ 100	-0.005	(0.010)	0.465	3,156
Centre $<$ 100	-0.002	(0.006)	0.377	3,710
		Panel B: Combination of characteristics		
Low Educ & long stay	0.002	(0.006)	0.404	4,657
High educ & long stay	-0.011	(0.013)	0.839	1,006
Large centre & long stay	0.005	(0.012)	0.434	2,440
Small centre & long stay	0.001	(0.007)	0.098	3,223
Young & low education	-0.001	(0.007)	0.093	3,588
Young & high educ	-0.003	(0.013)	0.244	845
Young & long stay	0.003	(0.006)	0.504	3,928
Old & low educ	-0.004	(0.010)	0.407	2,079
Old & high educ	-0.037*	(0.021)	1.741	354
Old & long stay	-0.005	(0.011)	0.430	1,735
Average Effect (ITT)	-0.003	0.005	0.539	6,686

Note: 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 years old. The following variables are used to build causal forests: country of origin, year of arrival, age at migration, household head characteristics (including education) and region of assignment. Each row reports the effects of ethnic concentration (measured as the log of co-ethnics at the time of assignment) on a specific subgroup. Panel A reports the results on subgroups characterized by one element (on age at arrival, education level of the household head, length of stay in the centre and size of the centre). Panel B reports results on subgroups made by a combination of two characteristics. The last column reports 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. The sample is made of young women observed from the age of 18. Standard errors are clustered at the year of arrival and municipality level.

Table A15: Neighbourhood Effects - Municipality level

	(1)	(2)	(3)	(4)	(5)
		Panel A: Naive estimation			
Ethnic concentration	-0.005	-0.005	-0.006	-0.006	
	(0.0035)	(0.0035)	(0.0039)	(0.0039)	
Mean	0.427	0.427	0.426	0.427	
N Obs	4,897	4,897	4,886	4,897	
R squared	0.056	0.06	0.06	0.075	
		Panel B: ITT estimation			
Ethnic concentration	-0.004	-0.004	-0.002	-0.006	-0.006
	(0.0034)	(0.0034)	(0.0036)	(0.0042)	(0.043)
Mean	0.427	0.427	0.424	0.427	0.427
N Obs	4,909	4,909	4,717	4,909	4,909
R squared	0.055	0.063	0.065	0.075	0.03
		Panel C: IV estimation			
Ethnic concentration	-0.020	-0.021	-0.011	-0.031	
	(0.0173)	(0.0175)	(0.0183)	(0.0202)	
Mean	0.427	0.427	0.426	0.427	
N Obs	4,909	4,909	4,717	4,909	
R squared	0.051	0.055	0.061	0.061	
F-test	87.44	83.89	69.17	66.21	
Country FE	✓	✓	✓	✓	✗
Year of arrival FE	✓	✓	✓	✓	✗
Parental Characteristics	✗	✓	✓	✓	✗
Region FE	✗	✗	✗	✓	✗
Neighbourhood index	✗	✗	✓	✗	✗
Predicted teenage usage	✗	✗	✓	✗	✗
Double LASSO selection	✗	✗	✗	✗	✓

Note : 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 arrival fixed effects and age at migration (specification reported in column (1)). Additional controls are added successively, (2) adds household head characteristics, (3) adds the neighbourhood quality index and the predicted share of teenagers using contraceptives at the zip4 level while (4) add region fixed effects. The sample only includes teenage women with a complete spell. Standard errors are clustered at the year of arrival and municipality level. Panel A reports the ”naive” estimation where ethnic concentration and control variables are measured when girls turn 15 years old. Panel B reports the ITT estimation where everything is measured at the time of assignment. Panel C reports the IV estimation where ethnic concentration at age 15 is instrumented by ethnic concentration at the time of arrival. Ethnic concentration is measured at the municipality level. Column (5) use a subset of the controls used in column (4), i.e. those selected according to the Double Debiased procedure (Belloni et al., 2014).

Table A16: Neighbourhood - ITT Estimates - Education outcome

	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	Panel A: ITT - Complete Spell					Panel C: ITT - Observed from age 18				
Ethnic concentration	-0.005	-0.006	-0.007	-0.008	-0.013**	-0.009**	-0.009**	-0.011**	-0.012**	-0.014**
	(0.0055)	(0.0053)	(0.0057)	(0.0059)	(0.0058)	(0.0047)	(0.0045)	(0.0047)	(0.005)	(0.0052)
Mean	0.552	0.552	0.553	0.552	0.552	0.566	0.566	0.568	0.566	0.566
N Obs	4,601	4,601	4,421	4,601	4,601	6,237	6,237	5,985	6,237	6,237
R squared	0.12	0.163	0.167	0.169	0.119	0.105	0.149	0.152	0.156	0.121
	Panel B: IV - Complete Spell					Panel D: IV - Observed from age 18				
Ethnic concentration	-0.065	-0.078	-0.089	-0.115		-0.103*	-0.106*	-0.131**	-0.176**	
	(0.0668)	(0.068)	(0.0762)	(0.0913)		(0.0563)	(0.0566)	(0.0633)	(0.0845)	
Mean	0.552	0.552	0.553	0.552		0.566	0.566	0.568	0.566	
N Obs	4,601	4,601	4,421	4,601		6,237	6,237	5,985	6,237	
R squared	0.095	0.122	0.116	0.09		0.04	0.073	0.042		
F-test	26.41	24.82	19.61	14.25		38.66	36.51	30.06	19.13	
Country FE	✓	✓	✓	✓	✗	✓	✓	✓	✓	✗
Year of arrival FE	✓	✓	✓	✓	✗	✓	✓	✓	✓	✗
Parental Characteristics	✗	✓	✓	✓	✗	✗	✓	✓	✓	✗
Region FE	✗	✗	✗	✓	✗	✗	✗	✗	✓	✗
Neighbourhood index	✗	✗	✓	✗	✗	✗	✗	✓	✗	✗
Predicted teenage usage	✗	✗	✓	✗	✗	✗	✗	✓	✗	✗
Double LASSO selection	✗	✗	✗	✗	✓	✗	✗	✗	✗	✓

Note : 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 arrival fixed effects and age at migration (specification reported in column (1)). Additional controls are added successively, (2) adds household head characteristics, (3) adds the neighbourhood quality index and the predicted share of teenagers using contraceptives at the zip4 level while (4) add region fixed effects. Standard errors are clustered at the year of arrival and municipality level. Panels A and C report the ITT estimation where ethnic concentration and controls are measured at the time of assignment. Panels B and D report the IV estimation where ethnic concentration at age 15 is instrumented by ethnic concentration at the time of arrival. Panels A and B restrict the sample to complete spells (of contraceptive usage), panels C and D to teenagers whose contraceptive usage are observed every year from age 18 onwards. The fifth column uses a subset of the controls used in the fourth one, i.e. those selected according to the Double Debiased procedure (Belloni et al., 2014).

Table A17: Generalized Random Forests - Education - *Complete spell* sample

	Coefficient	Standard Errors	T-test	Nb Observations
		Panel A: Single characteristics		
Age migration $<$ 9 y.o.	-0.010*	(0.006)	1.686	3,571
Age migration $\geq$ 9 y.o.	-0.002	(0.011)	0.174	1,030
H. Head low education	-0.010*	(0.006)	1.677	3,794
H.Head high education	0.005	(0.013)	0.399	807
Length of stay $\geq$ 1 year	-0.007	(0.006)	1.219	3,688
Centre $\geq$100	0.010	(0.010)	1.005	2,231
Centre $<$ 100	-0.011	(0.007)	1.564	2,370
		Panel B: Combination of characteristics		
Low Educ & long stay	-0.011	(0.007)	1.620	3,022
High educ & long stay	0.011	(0.013)	0.828	666
Large centre & long stay	0.018	(0.013)	1.413	1,649
Small centre & long stay	-0.012	(0.007)	1.551	2,039
Young & low education	-0.013*	(0.007)	1.861	2,887
Young & high educ	0.007	(0.013)	0.517	684
Young & long stay	-0.010	(0.007)	1.475	3,103
Old & low educ	0.000	(0.012)	0.003	907
Old & high educ	-0.001	(0.041)	0.029	123
Old & long stay	0.007	(0.016)	0.405	585
Average Effect (ITT)	-0.008	(0.005)	1.498	4,601

Note: This table reports the conditional average partial effects estimated using Generalized Random Forests (Athey et al., 2019). The outcome variable is 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 years old. The following variables are used to build causal forests: country of origin, year of arrival, age at migration, household head characteristics (including education) and region of assignment. Each row reports the effects of ethnic concentration (measured as the log of co-ethnics at the time of assignment) on a specific subgroup. Panel A reports the results on subgroups characterized by one element (on age at arrival, education level of the household head, length of stay in the centre and size of the centre). Panel B reports results on subgroups made by a combination of two characteristics. The last column reports 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 year of arrival and municipality level.

Table A18: Generalized Random Forests - Education - *Complete* and *Incomplete spell* sample

	Coefficient	Standard Errors	T-test	Nb Observations
		Panel A: Single characteristics		
Age migration $<$ 9 y.o.	-0.013**	(0.006)	2.311	4,125
Age migration $\geq$ 9 y.o.	-0.006	(0.008)	0.765	2,112
H. Head low education	-0.012**	(0.005)	2.167	5,105
H. Head high education	-0.002	(0.011)	0.142	1,132
Length of stay $>$ 1 year	-0.009	(0.005)	1.737	5,133
Centre $\geq$ 100	0.011	(0.009)	1.196	2,861
Centre $<$ 100	-0.015**	(0.006)	2.344	3,376
		Panel B: Combination of characteristics		
Low Educ & long stay	-0.010	(0.006)	1.609	4,178
High educ & long stay	0.000	(0.012)	0.004	955
Large centre & long stay	0.018	(0.012)	1.500	2,201
Small centre & long stay	-0.014**	(0.007)	2.082	2,932
Young & low education	-0.015**	(0.007)	2.299	3,315
Young & high educ	-0.001	(0.012)	0.083	810
Young & long stay	-0.013**	(0.006)	2.111	3,645
Old & low educ	-0.006	(0.009)	0.641	1,790
Old & high educ	-0.008	(0.022)	0.370	322
Old & long stay	0.002	(0.011)	0.168	1,488
Average Effect (ITT)	-0.012**	(0.005)	2.389	6,237

Note: This table reports the conditional average partial effects estimated using Generalized Random Forests (Athey et al., 2019). The outcome variable is 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 years old. The following variables are used to build causal forests: country of origin, year of arrival, age at migration, household head characteristics (including education) and region of assignment. Each row reports the effects of ethnic concentration (measured as the log of co-ethnics at the time of assignment) on a specific subgroup. Panel A reports the results on subgroups characterized by one element (on age at arrival, education level of the household head, length of stay in the centre and size of the centre). Panel B reports results on subgroups made by a combination of two characteristics. The last column reports 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. The sample is made of young women observed from the age of 18. Standard errors are clustered at the year of arrival and municipality level.

Table A19: Variables selected by LASSO in ITT analysis

Table Numbering	Specification	Variables Selected
Table 9	Contraceptives - Complete Spell Peers are Region in the same zip4	Nb of children, Age at migration (and square), Years 1996, 2002, 2003, 2004 Overig Groningen, Zuidoost-Drenthe, Zuidwest-Drenthe, Alkmaar en omgeving, Groot-Amsterdam, Agglomeratie 's-Gravenhage, Delft en Westland, Oost-Zuid-Holland, Groot-Rijnmond, Zeeuwsch-Vlaanderen, Midden-Limburg, Flevoland Iran, Somalia, Zaire, Azerbaijan, Kazakhstan, Afghanistan, Syria, Bulgaria and Panama
Table 10	Contraceptives - Complete Spell Peers are Country in the same zip4	nb of children, (Square) age at migration, Gender of head, Years 1996, 2002, 2003, 2004 Oost-Zuid-Holland, Zeeuwsch-Vlaanderen, Flevoland Iran, Somalia, Zaire, Azerbaijan, Syria, Sudan, Angola, Yugoslavia, Iraq, Russia, Bosnia-Herzegovina, Fed. Rep. Yugoslavia
Table 10	Contraceptives - Complete Spell Peers are Muslims in the same zip4	Age at migration (and square), Gender of head, Years 1996, 2002, 2003, 2004 Noord-Drenthe, Zuidoost-Drenthe, Zuidwest-Drenthe, Arnhem/Nijmegen, Zuidwest-Gelderland, Alkmaar en omgeving, Agglomeratie Haarlem, Zaanstreek, Groot-Amsterdam, Agglomeratie 's-Gravenhage, Delft en Westland, Oost-Zuid-Holland, Groot-Rijnmond, Zeeuwsch-Vlaanderen, Noord-Limburg, Flevoland, Iran, Afghanistan
Table 10	Contraceptives - Complete Spell Peers are Region in the same zip6	Nb of children, Gender of head, (Third power) number of children, Years 1996, 2002, 2003, 2004, Zuidoost-Drenthe, Zuidwest-Drenthe, Noord-Overijssel, Groot-Amsterdam, Het Gooi en Vechtstreek, Delft en Westland, Oost-Zuid-Holland, Noordoost-Noord-Brabant, Midden-Limburg, Zuid-Limburg, Flevoland, Iran, Somalia, Zaire, Kazakhstan, Syria, Laos, Zambia, Colombia, Spain, Sudan, China, Malta, Thailand, Vietnam
Table A15	Contraceptives - Complete Spell Peers are Region in the same municipality	Nb of children, Age at migration (and square), Years 1996, 2002, 2003, 2004, Oost-Groningen, Overig Groningen, Noord-Friesland, Zuidwest-Friesland, Noord-Drenthe, Zuidoost-Drenthe, Zuidwest-Drenthe, Zuidwest-Overijssel, Veluwe, Arnhem/Nijmegen, Alkmaar en omgeving, Agglomeratie Haarlem, Zaanstreek, Groot-Amsterdam, Agglomeratie Leiden en Bollenstreek, Groot-Rijnmond, Midden-Limburg, Iran, Somalia, Zaire, Azerbaijan, Kazakhstan, Afghanistan, Syria, Libya, Australia
Table 10	Contraceptives - Complete Spell Peers are young Region in the same zip4	Nb of children, Age at migration, Years 1996, 2002, 2003, 2004 Zuidoost-Drenthe, Zuidwest-Drenthe, Alkmaar en omgeving, Groot-Amsterdam, Agglomeratie 's-Gravenhage, Delft en Westland, Oost-Zuid-Holland, Groot-Rijnmond, Zeeuwsch-Vlaanderen, Flevoland Iran, Somalia, Laos, Angola, Iraq, Zaire, Azerbaijan, Kazakhstan, Australia, Spain, Syria, Panama, Afghanistan, Nigeria
Table A13	Contraceptives - Large Sample Peers are Region in the same zip4	Age at migration (and square), Nb of children, Years 1996, 1997, 2002, 2003, 2004, 2010, 2014, Overig Groningen, Noord-Drenthe, Zuidoost-Drenthe, Zuidwest-Drenthe, Noord-Overijssel, Alkmaar en omgeving, Agglomeratie Haarlem, Groot-Amsterdam, Agglomeratie 's-Gravenhage, Delft en Westland, Oost-Zuid-Holland, Groot-Rijnmond, Zeeuwsch-Vlaanderen , Midden-Limburg, Flevoland , Iran, Iraq, Saudi-Arabia, Angola, Zaire, Azerbaijan, Kazakhstan, Afghanistan, Spain, Uzbekistan, Sierra-Leone, Syria, Honduras, Bulgaria, Sudan
Table A16	Education - Complete Spell Peers are Region in the same zip4	Years 2006, 2008, 2009, 1996, 2002, 2003, 2004, Age at migration (and square), All (but basic) education levels Overig Groningen, Zuidoost-Drenthe, Zuidwest-Drenthe, Alkmaar en omgeving, Groot-Amsterdam, Agglomeratie 's-Gravenhage, Delft en Westland, Oost-Zuid-Holland, Groot-Rijnmond, Zeeuwsch-Vlaanderen, Midden-Limburg, Flevoland Afghanistan, Saudi-Arabia, Angola, Iraq, Zaire, Syria, Poland, Guinea, Azerbaijan, Kazakhstan, Uzbekistan
Table A16	Education - Large Sample Peers are Region in the same zip4	Nb of children (third polynomial), Years: 1996, 2006, 2008, 2009, 2010, 1997, 2002, 2003, 2004, 2014, All (but basic) education levels Overig Groningen, Noord-Drenthe, Zuidoost-Drenthe, Zuidwest-Drenthe, Noord-Overijssel, Agglomeratie Haarlem, Groot-Amsterdam, Agglomeratie 's-Gravenhage, Delft en Westland, Oost-Zuid-Holland, Groot-Rijnmond, Zeeuwsch-Vlaanderen, Midden-Limburg, Flevoland, Iran, Soviet Union, Saudi-Arabia, Angola, Iraq, Zaire, Azerbaijan, Spain, Sierra-Leone, Afghanistan, Honduras, Bulgaria, Sudan, Syria, Kazakhstan, Uzbekistan

Note : This table reports the variables selected by the double LASSO algorithm. The first two columns report the specification for which the algorithm is performed and to which table it corresponds. The third column reports the variables which are selected: in red when LASSO is performed on the outcomes, in black when it is performed on the main explanatory variables. Controls which are underlined appear for both. Years, education level, origin countries and COROP names refer to dummy variables.

Table A20: Alternative IV estimation

	(1)	(2)	(3)	(4)
Ethnic concentration	-10.308	121.531	0.525	1.385
	(229.2515)	(32372.9414)	(1.4157)	(3.2853)
N Obs	4,909	4,909	4,717	4,909
Mean Outcome	0.427	0.427	0.424	0.427
F test	0.002	0.000	0.220	0.189

Note : The table reports IV estimates of equation 3 for the outcome having taken contraceptives at least once by the age of 20. All specifications control for country of origin, year of arrival fixed effects and age at migration (specification reported in column (1)). Additional controls are added successively, (2) adds household head characteristics, (3) adds the neighbourhood quality index and the predicted share of teenagers using contraceptives at the zip4 level while (4) add region fixed effects. The endogenous variable is log of the number of immigrants from the same origin living in the zip 4 when the young woman turns 15. The instrument is the log of the number of asylum seekers who have been assigned to the same zip4 between 1996 and the year of assignment. The F test is the test statistics for zero effect of the instrument in the first stage. The sample only includes complete spells. Standard errors are clustered at the year of arrival and municipality (at assignment) level.

Appendix: 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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Appendix: Pre Analysis Plan

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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Appendix: Pre Analysis Plan

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