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Abstract

This paper studies how economic incentives influence cultural transmission, using a crucial expression of cultural identity: Child naming decisions. Our focus is on Arabic versus Non-Arabic names given in France over the 2003-2007 period. Our model of cultural transmission features three determinants: (i) vertical (parental) cultural transmission culture; (ii) horizontal (neighborhood) influence; (iii) information on the economic penalty associated with Arabic names. We find that economic incentives largely influence naming choices: Would the parental expectation on the economic penalty be zero, the annual number of babies born with an Arabic name would be more than 50 percent larger.

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1 Introduction

Cultural traits persist across generations, partly because individuals exhibit a preference for the transmission of their own culture to their offsprings.[1] However, external forces might be operating through both social pressure and the economic environment that restrict how the individual desire to transmit ones' culture translates into actual choices. These constraints are likely to be especially binding for minorities. This is naturally true of immigrants who live in societies in which natives tend to value conformity/assimilation and express anxiety with respect to (actual or perceived) rising cultural diversity.[2]

In this paper, we analyze how social and economic forces constrain the inter-generational transmission of culture among immigrants and their descendants with particular emphasis on the tension between the taste for the perpetuation of inherited cultural traits and the perceived economic discrimination attached to them.[3] We study the determinants of this trade-off for a specific case of transmission: The cultural type of first names parents give their children. We focus on the decision of whether to give a first name associated with Arabic/Muslim culture to babies born in France in the early 2000s. We view this decision as an appropriate object of inquiry for two main reasons: 1) First names are widely considered important markers of cultural identity—the choice of a first name is available to all parents, without material constraints, and is thus sometimes referred to as a "pure" expression of cultural identity (Lieberson, 2000); 2) There can be direct economic consequences to naming decisions.[4] Several studies have shown that first names associated with a cultural minority are perceived negatively by employers (Bertrand and Mullainathan, 2004).

In the French context, Arabic name holders are associated with both severe economic

[1]See Bisin and Verdier (2011) for a review of theoretical and empirical studies on the intergenerational tranmission of culture.

[2]For instance, Hainmueller and Hiscox (2010) provide evidence suggesting that non-economic factors are important drivers of hostility to immigration. Hainmueller and Hopkins (2014) survey the literature and highlight the dominance of concerns over cultural impacts in shaping public attitudes toward immigration.

[3]We adopt here a popular definition of culture among economists, as being the belief, behavior or identity patterns that are transmitted from one generation to another Fernández (2011). Recent surveys include Algan and Cahuc (2013) and Alesina and Giuliano (2015).

[4]Fryer Fryer and Levitt (2004) have provided additional evidence on the cultural component of first names by showing that the surge in distinctively Black names in the US since the seventies could be associated to a rise in Black cultural identity. In their study of two major waves of immigration in the United States, Abramitzky *et al.* (2020) emphasize the attractiveness of first-names as a measure of assimilation. They argue that first names are more likely to reflect preferences and less likely to reflect constraints imposed by the host society than alternative measures, such as intermarriage—which could reflect both the demand and supply determinants of assimilation opportunities.

discrimination and with cultural elements that potentially conflict with the "traditional" (native) culture: Religion, migration, political tensions, historical legacy with ex-colonies, or even consumption habits. First, the largest immigration wave since 1945 originates from former North African colonies (mostly Algeria, Morocco and Tunisia, three countries that we will refer to as Maghreb). Accounting for first-generation migrants and their descendants, this migration wave represents approximately 5.7% of the current French population INSEE (2016). Second, the decolonization process was conflictual for those countries—particularly Algerian independence which occurred after several years of violent war (1952-1964). Third, Arabic names are also a sign of the Muslim religion since most of those names come from the Quran, and the transmission of first names associated with the Quran is a natural practice for religious people. By contrast, non-Arabic names in France are mainly associated with Saints' names, i.e. coming directly from the French calendar of Christian Saints (or inspired by it). Fourth, economic prejudice against Arabic name holders has been largely documented in France. Second-generation migrants from Maghreb face the highest penalty on the French labor market among the different immigrant groups (Algan *et al.*, 2010; Duguet *et al.*, 2010). Combes *et al.* (2016) use the Labor Force survey to show that Arab immigrants working in customer-facing jobs have much higher risks of unemployment. Even closer to our main variable of interest, Adida *et al.* (2010) perform an audit study using CVs that only differ with respect to the origins of the first name (Arabic vs Christian). Vitas with an Arabic first name are 2.5 times less likely to receive a job interview callback compared to their Christian-named counterparts, everything else being equal, including the last name. Consistent with this recent work on French data, we document a large penalty attached to Arabic names on the French labor market: The average unconditional differential of unemployment between Arabic name holders and the rest of the population amounts to 13% in our sample.

Our empirical design is based on a random-utility discrete-choice model of parental naming decisions. The choice is binary and pertains to the cultural type of the child's first name, Arabic or non-Arabic. The model incorporates the two traditional vertical and horizontal channels analyzed in the literature on cultural transmission (Bisin and Verdier, 2001), to which an economic channel is added. The vertical transmission channel results from the utility gain for parents when transmitting their own cultural type. The horizontal transmission channel stems from spatial externalities associated to the cultural types of peers and neighbors. The economic channel corresponds to the expected economic penalty inflicted on one's children when giving an Arabic first name.

The French Labor Force Survey (LFS henceforth) provides a unique source of information for measuring and estimating these various cultural transmission channels. The vertical transmission channel is identified by contrasting the first names of parents and children, all being reported in the survey. Regarding the measurement of the economic channel, the LFS allows for the detailed computation across occupations of unemployment rates associated with Arabic/non-Arabic names. Finally, the LFS data collection is based on a large representative set of more than 10,000 sampling units spread all over the country, each unit consisting of a residential block of *20 adjacent households, all of which are surveyed*. This feature enables us to define a set of relevant peers at the very local level where many social interactions have been shown to occur in France (see Goux and Maurin, 2007; Maurin and Moschion, 2009). We use this set of peers for two purposes: First, to measure the horizontal transmission of naming choices from nearby neighbors. Second, we compute the *Local Information on Penalty* (LIP hereafter) as the average unemployment differential between Arabic and non-Arabic name holders across these neighbors' occupations. The idea underlying this key explanatory variable is that parents use various sources of information to form a belief about the economic penalty associated with Arabic names and our hypothesis is that one of the main sources of information is people living in the same neighborhood. This mechanism is likely to be especially relevant for migrants (and their descendants), since they should exhibit a low initial knowledge of the local labor market (see Hellerstein *et al.*, 2011; Goel and Lang, 2019, for relevant evidence on Canadian and US labor markets).

A critical issue in estimating our model relates to parents' endogenous location choices across residential neighborhoods, resulting in spatial sorting on—possibly unobserved—characteristics, correlated with the propensity to give Arabic first names to their offspring. It could be, for instance, that parents most attached to transmitting an Arabic name to their children choose to live in residential blocks with religious neighbors, who themselves tend to work in low-discrimination occupations. We mitigate this concern by restricting our estimation to a sample of households living in the French public housing sector. Due to legal and binding dispositions, state-owned apartments are allocated to households without consideration for their cultural background, mixing people indiscriminately. Furthermore, individuals rarely move since the rents are much lower than market rates. Building on Algan *et al.* (2016), we confirm, with a variety of tests, that spatial allocation within the public housing market can be considered as good as random.[5]

Our main result is that economic factors are important drivers of individual cultural transmission decisions. We find that an increase in the perceived penalty associated with Arabic name holders, as measured by our LIP variable, reduces the probability that parents will give such names. The magnitude of the effect implied by our estimates is also quite sizable: if the parental expectation of the economic penalty were brought down to zero, the annual number of babies born with Arabic names in France would be more than 50 percent larger. In terms of the two other channels, the vertical channel is by far the dominant factor in the naming decision: A French baby who has at least one parent or grandparent with an Arabic/Muslim background is twice as likely to be given an Arabic name. The horizontal channel is statistically significant in some regressions, but quantitatively much less important. While these findings hold for the sample of all households living in public housing, they are mainly driven by the behavior of first and second generations of migrants from Arabic countries. Using our theory-grounded estimates, we are also able to quantify welfare gains and losses attached to cultural transmission. Focusing on the substitution rate between the vertical and the economic cost channels, we can express the strength of cultural attachment in monetary units. For first and second generation of migrants, we find that vertical transmission of an Arabic name provides the same shift in parents' utility as a 3% rise in the child's lifetime income. Finally, we also assess the welfare effects of French policies historically aimed at restraining naming choice.

Our paper fits into several strands of research. A substantial body of work by economists studies the transmission of cultural values and the formation of identity (Akerlof and Kranton, 2000; Shayo, 2009; Atkin *et al.*, 2019). Bisin and Verdier (2001) provide a seminal cultural transmission model, distinguishing between vertical transmission by parents and oblique or horizontal transmission associated with social interactions. Tabellini (2008) and Guiso *et al.* (2008) model the interactions between norms and economic incentives in the inter-generational transmission of values like trust. Bisin *et al.* (2004) and Bisin *et al.* (2016) estimate structural models of transmission of religious values and ethnic identity. We contribute to this literature by introducing a new channel of cultural transmission through economic incentives. We also innovate in our empirical application in terms of measurement, since we observe variation in incentives at the block level as opposed to

[5] We also consider the possibility that parents retrieve information from the unemployment differential they observe in their own occupation. However, while our focus on public housing in the French context allows us to deal with spatial sorting, there is no similar device to avoid parental sorting across occupations. We therefore prefer the neighborhood-based approach which, as detailed in section 3, features fixed effects for the parental occupation and thus account for the fact that parental occupation is not random.

more aggregate units. Most important, building on Algan *et al.* (2016), we exploit the quasi-random allocation of households across blocks among public housing tenants as an identification strategy.

The stream of recent work studying the question of migrants' assimilation and how discrimination affects it is perhaps the most directly relevant to our findings. Abramitzky *et al.* (2016) analyze the co-evolution of cultural and economic assimilation during the age of mass-migration in the United States: They find significant first-name assimilation among immigrants which tends to translate into better economic outcomes for their offspring.[6] Mazumder (2019) finds that immigrants' military service in the US army during World War I increased their rate of cultural assimilation, with potentially positive economic returns. Most closely to our study, Fouka (2019) finds that immigrants from German origins responded to discrimination during WWI in the United States by increasing their assimilation efforts—partially by changing the "Americanness" of their names. Our results are consistent with Fouka (2019) since they show that minority parents are willing to undertake costly assimilation actions when exposed to more information about discrimination. This could indicate that the discrimination at play is to some extent conditional, meaning that it is likely to be lower if individuals send signals of loyalty to the dominant culture (Bisin *et al.*, 2011).[7] Our contribution with respect to this literature is twofold. First, we analyze a channel of cultural transmission directly associated with a measure of discrimination on the labor market. Second, by focusing on parents who are exogenously allocated to their neighbors within public housing, we are able to exploit fine-grained exogenous variation in neighborhood ethnic and occupational composition to estimate jointly the vertical and horizontal channels as well as the economic cost—whereas most of the previous literature focuses on the heterogeneous effects of aggregate shocks.

Our paper also relates to the literature on the link between long-run economic and cultural change. First, we document a very high preference for vertical transmission among

[6] In a subsequent version of their paper, Abramitzky *et al.* (2020) compare first-name assimilation across two periods of intense immigration in the United States.

[7] Naturally, whether the lower transmission of identity through name giving is mostly a strategy or a true shift in beliefs is an open question. In a closely related paper, Fouka (2020) shows that the second generation of German-origin Americans facing German language bans while at school in the United States following World War I displayed signs of lower assimilation. In particular, they were more likely to marry within their ethnic group (Germans) and to give German-sounding names to their children. This suggests that some form of discriminatory public policies might backlash and feed oppositional identities (consistent with the findings of Rozenas and Zhukov, 2019; Lleras-Muney and Shertzer, 2015). At any rate, given the documented labor market penalty associated with Arabic names, the response we identify is likely to have long-run real implications on the economic well-being of newly born children even if name assimilation does not reflect a pure convergence in beliefs.

parents with Arabic origins, in line with the vast literature highlighting the strong persistence of cultural norms (see Guiso *et al.*, 2016, for instance). Faced with adverse economic consequences of vertical transmission, we find, however, that parents adjust their behavior. This evidence is consistent with the notion that culture tends to be a highly persistent construct that can nevertheless evolve in the face of changing circumstances—a fact that has been well documented in the literature on long-run persistence. For instance, Nunn and Wantchekon (2011) show that the intensity of exposure to slave trade in Africa is associated with lower level of trust nowadays due to an impact on cultural norms and values. Relatedly, Voigtländer and Voth (2012) study the persistence of antisemitism in Germany from the Middle Ages age onward. They document that the persistence of antisemitism, while high on average, is much lower in cities with an history of demographic expansion and exposure to economic exchanges (e.g. trade).

Finally, our paper relates to the literature on discrimination and its consequences for economic decision and public policy. A fairly large body of research has found evidence of discrimination by employers against first names from a cultural minority. Those studies exploit either audit study methodology (Bertrand and Mullainathan, 2004; Adida *et al.*, 2010; Duguet *et al.*, 2010) or representative surveys (Heckman, 1998; Fryer and Levitt, 2004). Our paper differs from this literature in two main respects. First, our ambition is not to measure economic discrimination, but to analyze how parents react to perceived *information* on discrimination. We propose different information channels that parents can retrieve from the labor market and estimate whether they use this information in their cultural investment. Second, we analyze the *ex-ante* decision of adopting (or not) a cultural trait that could be discriminated against, while the rest of the literature focuses on the *ex-post* economic consequences of carrying this cultural trait. We show that the determinants of cultural identity, and more specifically the economic ones, have substantial welfare and public policy implications that have been overlooked so far.

The remainder of the paper is as follows. Section 2 provides a detailed description of the data we use. Section 3 presents our theoretical model of naming decision. Section 4 contains our baseline estimation results. Section 5 provides various robustness tests. Section 6 quantifies the contributions together with welfare effects of the vertical, horizontal, and economic channels on cultural transmission.

2 Data

2.1 The French Labor Force Survey

Our empirical analysis is based on the French Labor Force Survey (LFS henceforth) from 2003 to 2007. The LFS is a representative survey of the French population, stratified at levels of around 3500 residential blocks per year, with each block defined as an average of 20 adjacent households. The LFS is a rolling panel of 6 quarters and all the households within a given block are interviewed every quarter. All household members older than 15 years are interviewed, and they report information on their socio-economic characteristics, including employment status (unemployed, inactive and employed), hourly wage and occupation. The occupation variable covers seven broad categories: farmer, craftsman, unskilled blue-collar, skilled blue-collar, clerk, intermediate, and executive. But the LFS also provides a more detailed classification of 29 occupations within those categories depending on the sector and infra-skill level of the occupation. In addition, the survey records the first names of all household members, including children below 15 years old.

Since the data collection is based on (very) close neighbors, the LFS provides a unique opportunity to understand the role of horizontal factors in the transmission of names. Given that the sampling unit in the LFS consists of small groups of adjacent households, and that all the members of the households within the same block are interviewed, we get detailed information on all individuals living in the neighborhood. Another important characteristic of the LFS is that it distinguishes between the public and the private housing sectors. As discussed below, our identification strategy will be based on residential allocation of households within the public housing sector. Thus, we report on both the total sample and on the sample of public housing residential blocks.

The time span of the rolling panel is too short (6 quarters) to exploit time variation in the socio-economic composition within residential blocks. Thus, we keep one observation per member of the household, which generally corresponds to the first wave of interviews. Table 1 reports the main descriptive statistics of the full database when we use this selection criterion. Our total sample is made up of 10,541 blocks, with 1,535 blocks containing one public housing unit. The average block size is 18.31 adjacent households, and each household consists of around 3.31 members (babies, children, and adults included). Overall, the total sample includes 425,210 individuals, among whom 69,458 are living in public housing.

Table 1 – Descriptive statistics of the residential blocks

	All	Public Housing
Number of blocks	10,528	2,674
Number of blocks by department	108.54	28.45
Average number of households per block	16.31	17.59
Average number of members per household	2.43	2.51
Average number of children per household	0.50	0.65
Total number of households	173,154	26,958
Total number of individuals	425,223	69,437

2.2 Sample of babies' names

Our main variable of interest is the individuals' name type and the cultural background that is associated with it. We focus on the transmission of Arabic first names, as opposed to non-Arabic names, in French society. In our data, we code Arabic first names according to the classification of Jouniaux (2001). Arabic names are associated with the most important population of immigrants in France—Maghreb—and to a lesser extent with the Middle East (other Arabic countries and Turkey), in the aftermath of decolonization initiated in the 1960s. According to INSEE (2016), people with Maghreb origins (i.e. Algeria, Morocco, Tunisia) represent almost 60 percent of non-OECD migrants from first and second generations in France in 2008; this corresponds to 3.7 million individuals (1.7 million for the first generation and 2.0 million for the second generation) out of a total French population of 64.3 million.

We describe in Table 2 our sample of babies along three dimensions of relevance for our empirical analysis: (i) The cultural type of parents' first name; (ii) the cultural background of babies (as captured by the immigration history of the household); (iii) whether the household lives in private or public housing. We start in the upper panel with the full sample of 3,541 newborn babies over 2003-2007 for whom we have all the needed information on the parents' and blocks' characteristics. 3,216 babies (90.8%) receive a non-Arabic names.[8] Among parents with Arabic names, the naming decision is rather balanced since 51.1% of those parents give an Arabic name to their offspring. In contrast, among parents

[8] Among them, 1,879 babies (58%) are given traditional names, that is names that were already given in France in the early twentieth century. To identify those, we use INSEE's national database, "Le fichier des prénoms". Those traditional names are generally associated with Christian saint names, or names deeply ingrained in the French culture like Leo for boys or Manon for girls.

with non-Arabic names, the adoption of Arabic names is marginal, with a frequency of adoption of 2.8%.[9] The main difference when considering the sample of households living in public housing (reported in parenthesis) is that parents with Arabic names are more likely to transmit their cultural trait to their offspring.

Table 2 – Transmission of name types

	Babies with:	
	non-Arabic name	Arabic name
New Born (full sample):		
Parents with non-Arabic name	2982 (489)	80 (28)
Parents with Arabic name	234 (95)	245(132)
0-3 years old (2nd/3rd generation):		
Parents with non-Arabic name	461 (183)	111 (47)
Parents with Arabic name	658 (317)	789(461)

Note: This table reports the number of babies by name type and allocates them according to the name type of their parents. The top panel gives figure for the whole sample of babies born within the year. The bottom panel considers babies aged 0 to 3 at time of survey, born from at least one parent or grandparent with Arabic origins. In parentheses, the sample of babies in the public housing sector. The term generation refers to the generation of the babies (e.g. a second generation baby is the child of a first generation migrant).

Since we observe overall very little adoption of Arabic names by parents with non-Arabic names in the full sample of babies from all origins, our econometric analysis will mostly look at the pure transmission decision of giving an Arabic name when it is part of the original culture. To this purpose, we shall focus on households where at least one parent or grandparent is a national from Algeria, Morocco, Tunisia, Middle-East and Turkey (see Section 4.1 for details). The babies are thus born in France, but the parents/grandparents (babies of second/third generation respectively) were born in an Arabic/Muslim country. However, restricting the sample to this population would leave us with a too small sample of newborn babies, especially in the public housing sector. We therefore consider children between 0 and 3 years old instead of just newborn babies to carry out this analysis. Descriptive statistics for this sample are reported in the bottom panel of Table 2. Among children with parents having themselves an Arabic name, 45% are given a non-Arabic name

[9]The top Arabic names given by those parents are Louna for girls and Rayan for boys. Those first names are rather neutral, they are hardly selected by parents with Arabic names.

(658/1447).[10] A similar pattern is observed when restricting further to households living in public housing.

3 Model and Identification of Naming Decision

3.1 A simple model of baby name choice

To estimate the channels driving the transmission of name type, we build a random utility discrete choice model of baby naming decision. Our framework is rich enough to embed three different channels of interest (vertical, horizontal, economic) while remaining sufficiently tractable to highlight the underlying estimation issues. The parental decision under scrutiny is binary and relates to the cultural type attached to the baby's first name. The utility for a household i, living in residential block $k(i)$, derived from choosing a given name type for its baby born in year t is defined as $U_{it}(1)$ if the name is Arabic and $U_{it}(0)$ otherwise,

$$U_{it}(\text{Baby}) \equiv V_{it}(\text{Baby}) + \epsilon_{it}(\text{Baby}), \tag{1}$$

where Baby $\in \{0, 1\}$ denotes alternatives, $V_{it}(\text{Baby})$ is the observed part of utility and $\epsilon_{it}(\text{Baby})$ is the unobserved parental-specific random shock across alternatives.

In such a discrete-choice setting, only differences in utility over alternatives can be identified from the data. The econometrician observes a parental choice $\text{Baby}_{it} = 1$ if and only if $\Delta U_{it} \equiv U_{it}(1) - U_{it}(0) \geq 0$. Let us denote the difference in the observed part of utility as $\Delta V_{it} \equiv V_{it}(1) - V_{it}(0)$, and the difference in unobserved utility as $\varepsilon_{it} \equiv \epsilon_{it}(1) - \epsilon_{it}(0)$, such that

$$\begin{aligned} \Delta U_{it} &= \Delta V_{it} + \varepsilon_{it} \\ &= \alpha_0 + \alpha_1 \underbrace{\text{Parents}_i}_{\texttt{Vertical}} + \alpha_2 \underbrace{\mathbb{E}\left[\frac{1}{\mathcal{N}_{k(i)t}} \sum_{j \in k(i), j \neq i} \text{Baby}_{jt}\right]}_{\texttt{Horizontal}} + \alpha_3 \underbrace{\mathbb{E}[\mathcal{C}_{it}]}_{\texttt{Economic Cost}} + \varepsilon_{it}, \end{aligned} \tag{2}$$

where ΔV_{it} is specified as a three part linear function, which we label "Vertical", "Horizontal", and "Economic cost" channels of influence. Parents_i is a parental characteristic

[10] In this latter case, they rarely choose saint names, but choose instead names that are culturally less distinctive. In particular, the two non-Arabic first names that are the more frequently selected are Adam or Yanis for boys, and Ines or Sarah for girls, names that seem to be attached to different cultures and are also given by the group of parents with non-Arabic names.

equal to one when the name of one of the two parents is Arabic and zero otherwise (with alternative definitions investigated in robustness analysis). Among the $\mathcal{N}_{k(i)t}$ babies born in residential block $k(i)$ in year t, the variable Baby_{jt} codes for choices of names among babies born from other parents j living in the block.[11] Finally, $\mathbb{E}[\mathcal{C}_{it}]$ is the *perceived* economic penalty that parents i expect to be attached to their baby if they choose an Arabic name.

The `Vertical` component captures the parental desire to transmit their own cultural type (as measured by coefficient α_1). Our specification of utility is flexible as it allows both for cultural transmission and cultural adoption. Transmission is the case where the names of parents and babies belong to the same cultural type. Adoption corresponds to the two other cases: e.g. parents with Arabic names that do not transmit their cultural type to their baby or parents with non-Arabic names adopting an Arabic name for their baby. Both patterns are observed in the data although the latter is less salient (see Table 2).

The `Horizontal` component reflects social influence, i.e. the share of parents of newborn babies in residential block k expected to make the same choice as i, with parameter α_2 expected to be positive. In our data, the block k is small enough that household i is not negligible and this results in a classical Manski (1993) reflection problem. We assume that parents i form their expectations on lagged decisions of neighbors:

$$\mathbb{E}\left[\frac{1}{\mathcal{N}_{k(i)t}} \sum_{j\in k(i), j\neq i} \text{Baby}_{jt}\right] \equiv \frac{\sum_{\tau=1}^{\Upsilon} \sum_{j\in k(i),j\neq i} \text{Baby}_{jt-\tau}}{\sum_{\tau=1}^{\Upsilon} \mathcal{N}_{k(i)t-\tau}}, \tag{3}$$

that is, they expect the neighbors' current choices to be, on average, similar to the ones taken since year $t-\Upsilon$ (we will take $\Upsilon = 10$ in our application).

The third component (`Economic Cost`) relates to economic incentives: Presumably, the higher the expected penalty is, the lower the parents' desire to give their babies Arabic names. The *perceived* expected penalty, is sensitive to the parental information set and to a wide set of observed and unobserved parental characteristics influencing the future spatial and social mobility of the baby. We now explain how it is measured in our data. Our identification strategy exploits the fact that part of the parental information set is based on information on the labor market that households retrieve from social interactions and communication with their neighbors. A straightforward approach would be to consider the unemployment differential between Arabic and non-Arabic name holders in

[11] Note that in the horizontal channel, we scale by $\mathcal{N}_{k(i)t}$ (rather than $\mathcal{N}_{k(i)t} - 1$) to get a well-defined ratio in blocks where only one baby is born.

the neighborhood. However the LFS is not representative at such a fine-grained level. Instead we use the information conveyed by neighbors' occupations. The idea is that parents surrounded by neighbors working in occupations with high levels of penalty tend to update their beliefs on the extent of the penalty upwards. Formally, the *perceived* expected penalty is broken down into a block-specific informational component and an unobserved parent-specific residual component:

$$\mathbb{E}[\mathcal{C}_{it}] = \sum_{l \in \mathcal{O}} \omega_{lk(i)} \times \hat{\gamma}_l + e_{it}, \tag{4}$$

where $\mathcal{O} =$ is the set of occupations, $\omega_{lk(i)}$ is the share of neighbors in block $k(i)$ working in occupation l, $\hat{\gamma}_l$ is an occupation-specific signal on the labor market penalty (see Section 4.1) and e_{it} is the unobserved residual parent-specific part. In the remainder of this paper, $\sum_{l \in \mathcal{O}} \omega_{lk} \times \hat{\gamma}_l$ is labeled as the *Local Information on Penalty* (LIP) in block k. In Section A of the online appendix, we show how this functional form is a natural theoretical prediction in a setup where parents aim at maximizing the expected utility of their child.

In our naming decision model, parents' utility depends directly on the expected economic cost their children face. The intensity of this relationship—captured by the parameter α_3 in Equation (2)—might reflect the fact that parents are more or less likely to gather information based on their neighbors. The parameter additionally reflects the degree to which parents value their children's economic welfare. To the extent that parents discount such economic welfare heavily, this will translate into a less negative value of α_3. Accordingly, both informational sensitivity and imperfect altruism could explain parents' willingness-to-pay (in terms of the penalty experienced by their offsprings) to perpetuate their own culture. The comparison of the coefficients α_1 and α_3 reflects the parental trade-off between their own attachment to a particular cultural type and their altruistic concern toward the future economic performance of their babies.

Combining (2), (3) and (4), utility becomes

$$\begin{aligned} \Delta U_{it} &= \Delta \mathcal{V}_{it} + \delta_{it} \\ &\equiv \alpha_0 + \alpha_1 \text{Parents}_i + \alpha_2 \frac{\sum_{\tau=1}^{\Upsilon} \sum_{j \in k(i), j \neq i} \text{Baby}_{jt-\tau}}{\sum_{\tau=1}^{\Upsilon} \mathcal{N}_{k(i)t-\tau}} + \alpha_3 \sum_{l \in \mathcal{O}} \omega_{lk(i)t} \times \hat{\gamma}_l + \delta_{it}, \end{aligned} \tag{5}$$

where $\Delta \mathcal{V}_{it}$ is the observable utility and $\delta_{it} \equiv \alpha_3 e_{it} + \varepsilon_{it}$ is the new error term.

It is standard to specify δ_{it} as having a logistic distribution, with σ as its scaling parameter, in order to estimate the utility function (5). One can then express, in closedform, the probability of choosing an Arabic name—a formula that enables, in Section 6, to run counterfactuals without probabilities going out of bound:

$$\mathbb{P}(\text{Baby}_{it} = 1) = 1/[1 + \exp(-\Delta\mathcal{V}_{it}/\sigma)], \tag{6}$$

The observable utility differential $\Delta\mathcal{V}_{it}/\sigma$ is retrieved from the coefficients in (5) that can be estimated readily using standard logit.

3.2 Identification strategy

Estimation challenges. The key empirical challenge relates to spatial sorting of households. Before going into details, let us summarize the overall idea. In equations (5) and (6), a key source of identification is based on neighbors from the residential block. Neighbors are used both as: i) a source of peer-pressure for the horizontal transmission channel, and ii) a source of information for the LIP. Since individuals tend to self-segregate, e.g., most households choose their location, our estimation could be biased by endogenous residential sorting. To address this concern, our identification strategy exploits the specificity of the French context and consists in restricting estimation to a subsample of households living in the public housing sector. Previous work (Algan *et al.*, 2016) has shown that households within public housing units are essentially exogenously allocated to their residential block, thus circumventing the issue of spatial sorting.

The horizontal transmission channel raises several estimation issues that are well-known in the social interaction literature. Indeed, in equation (5), the realizations of $\text{Baby}_{jt-\tau}$ depend on $\Delta U_{jt-\tau}$. *Spatial sorting* might lead to a non-zero correlation between δ_{it} and $\delta_{jt-\tau}$ for households i and j belonging to the same residential block k. This would create a correlation between $\text{Baby}_{jt-\tau}$ and the error term in (5), δ_{it}, potentially capturing unobservable taste shocks for the considered cultural type common to households i and j. For example, it is clear that the degree of devoutness of the household, which is unobserved by the econometrician, affects positively the choice of an Arabic name for the baby; moreover, religious people tend to live in the same residential areas (e.g., close to a mosque or to halal shops). This example makes it clear that spatial clustering of Arabic names is not only driven by horizontal transmission, but might also be partly driven by unobserved characteristics of the area. Our estimates could thus be biased by the endogenous spatial sorting of households. To limit this source of bias, we identify α_2 out of regressions run on a subsample of households that are allocated across the different public housing blocks within a given *département*[12] in a plausibly exogenous way (we describe the public housing allocation process at the end of this section). The combination of *département* fixed effects with quasi-random allocation of households, within a *département*, should make our econometric estimates safely immune to spatial sorting bias.

The coefficient α_3 associated with the economic cost of a name type may also be ill-estimated due to self-selection into occupations and locations by parents. The methodological concern is that religious (Muslim) parents, attached to giving Arabic names to their offspring, tend to work in occupations with low discrimination, and are located in residential blocks with religious neighbors working in non-discriminating occupations. We address this issue first by controlling for parental occupation and education fixed effects. Although parental occupation is naturally not a random choice, including fixed effects for the parental occupation captures all time-invariant co-determinants of parental occupation and newborn naming choices. Second, rather than using the parental occupation as a source of information on the perceived expected penalty, we use the block-specific LIP. Thus the remaining issue relates to the exogeneity of the composition of occupations within the residential block. We identify the coefficient α_3 by restricting once again our estimates to the subsample of exogenously allocated households living in the public housing sector. We thus exploit exogenous variation in the composition of occupations across blocks in the public housing sector as a source of exogenous variation in the LIP.

Even when restricting the sample to households living in public housing, identification could be threatened by neighborhood-level contextual drivers of both economic penalty and naming choice. For instance, housing blocks with more unskilled workers could exhibit more social discrimination against the part of the population identified with immigration from Maghreb. This could discourage parents from giving an Arabic name, *for non-economic reasons*, biasing our estimate of the economic cost effect. We address this concern by including a set of controls at the local level, such as political and anti-Islamic attitudes by occupations (aggregated at the block level), or the degree of ethnic fractionalization. Our results show that those alternative channels are not strong determinants of naming patterns, and leave the magnitude of the economic channel estimates virtually unchanged.

A last concern is that part of the economic cost channel could operate through the horizontal channel. Indeed, our model implies that name-giving decisions by other parents in a given block should themselves be affected by the LIP. In that setting, conditioning on the local share of children with Arabic names might create a post-treatment bias for

[12] Metropolitan France is divided into 95 administrative areas, called *départements*.

our estimate of the economic cost channel. On top of using lags in the construction of the horizontal channel variable, we address this question in the robustness section 5.3 and in the online appendix Table E.3. Our analysis shows that the interaction between the horizontal and economic cost channels, while problematic in principle, is not quantitatively important for the economic channel point estimate.

Public Housing. The French public housing market is very tight, and highly regulated. We provide hereafter a short overview of the allocation process of households across public housing dwellings. The most important feature for our purpose is that households have very limited control over when and where within a *département* they will be assigned if granted public housing.[13]

The main eligibility requirements for admittance into the public housing sector are to be a legal resident of France (as a French citizen or migrant with a valid residence permit) and under a certain threshold of income per unit of consumption. This income ceiling is rather high, so that around two thirds of households living in Metropolitan France could apply for a public housing unit (Jacquot, 2007). The rents are also considerably lower in public housing than in private housing. As a result, there is a strong excess demand for public housing. In Paris for example, there were 121,937 ongoing applications, to be compared to 12,500 public housing units allocated over the year 2010. Due to those stringent constraints, other eligibility criteria are taken into account: family situation and household size (to ensure a suitable match with the characteristics of vacant dwellings), as well as the emergency of the application.[14]

The selection committees in charge of allocating households to vacant public housing dwellings are held at the *département* level.[15] Legally, applicants can refuse up to three offers but in practice they rarely do, given the large opportunity cost of declining an

[13]Algan *et al.* (2016) provide an extensive list of tests showing the absence of self-sorting along ethnic lines across public housing dwellings in France. In online appendix G we provide more details on the institutional and legal aspects and descriptive statistics; we also briefly review the set of statistical tests by Algan *et al.* (2016) and expand it to several dimensions. In particular, we show that the observed allocation is not statistically different from a random process generated though Monte-Carlo simulations.

[14]Five priority criteria—none related to nationality—are defined by law to make sure that vacant housing will first be distributed to households with obvious social difficulties (see online appendix G).

[15]At the time of our sample, Metropolitan France was divided into 22 large administrative areas, called *régions*, and into 96 smaller administrative areas, called *départements*. Each *département* is hence a subdivision of a region, and several *départements* can belong to the same region. Each *département* is administered by an elected General Council (*Conseil Général*) and its President, whose main areas of responsibility include the management of a number of social and welfare programs, junior high schools, buildings and technical staff, local roads, schools, rural buses, and municipal infrastructure.

offer. This makes it unlikely that the selected households could be really picky about the characteristics of their neighborhood and in practice very few applicants (6.6 percent) express a preference about the area they want to be allocated within the department due to the fear of being rejected on this ground. Finally, residential mobility within the public housing sector is marginal, due to the strong shortage in the supply of public housing dwellings.

4 Estimation

In this section we start with a description of how the main explanatory variables are constructed, with a special focus on the local information on penalty (LIP). We then proceed to our baseline estimation results, leaving our battery of robustness exercises to section 5.

4.1 Explanatory Variables

The vertical transmission channel is measured by two binary variables, relating to parental characteristics relevant for the transmission of their cultural traits. The first one, *One parent/grandparent with Muslim country nationality*, codes for babies born from parents or grandparents nationals from a list of countries where Arabic names are prevalent—i.e. Algeria, Morocco, Tunisia, Middle-East and Turkey.[16] The second one, *One parent with Arabic name*, codes for the type of parental first names, Arabic/non-Arabic (using the same list as for babies).

The horizontal channel is measured by the *share of Arabic-named children aged 4-10 in the block* (defined in equation (3)). We investigate the scope of the horizontal channel by also considering naming patterns among older cohorts or among larger geographical units—*département* or *sectors* that consists of 6 adjacent residential blocks.

The LIP is defined as $\sum_l \omega_{lk} \times \hat{\gamma}_l$, where ω_{lk} is the share of neighbors in block k working in occupation l and $\hat{\gamma}_l$ is an occupation-specific signal on the labor market penalty attached to an Arabic name. A difficulty here relates to the abundance of ways to measure this signal. Different assumptions—in terms of labor market structure, informational frictions,

[16]There is no obvious way to establish this list. We opted for countries proximate enough spatially and historically to account for a large share of the immigration flows that are relevant in terms of the vertical transmission channel. In our sample, this set of countries accounts for 80% of all parents that are nationals of a list of countries other than France, Europe, the American continent or former Indochina. The same ratio is 77% for parents living in private housing and 87% for public housing.

or parents' rationality—could point to different measurements.For instance, it is unclear whether the *true* penalty is the relevant variable to target in terms of measurement. Indeed, it would implicitly assume that parents are full-fledged econometricians in the sense that they would be able to isolate the true unemployment penalty imposed by giving an Arabic name to their baby when taking the decision. Without a clearly dominating option, we consider various measurement options of the penalty. In our baseline analysis, we use the *unconditional unemployment differential* between Arabic and non-Arabic name holders in occupation l. Denoting u_l^{a} and u_l^{na} the unemployment rate in occupation l for Arabic and non-Arabic name holders respectively, we set $\hat{\gamma}_l = u_l^{\mathrm{a}} - u_l^{\mathrm{na}}$. This unconditional approach is simple and compatible with a model where agents naively attribute all the unemployment penalty they observe solely to the Arabic origin of the name. In contrast, in our first robustness exercise (section 5.1), we explore the impact of measuring the signal with a *conditional* unemployment penalty retrieved from an auxiliary Mincer-type equation. This alternative approach conceptually relies on a model where sophisticated agents are able to filter out a large set of confounding factors when assessing the unemployment penalty attached to an Arabic name. Besides this demanding cognitive assumption, a caveat here is that there is no guarantee that agents use the same set of confounding factors than the one used by the econometrician (e.g. non-cognitive skills). Quite remarkably, we find that the estimations based on unconditional and conditional penalties yield similar results quantitatively. Accounting for a range of observable unemployment determinants in the auxiliary Mincer equation has little effect on our coefficients of interest when estimating our main econometric equation (6). This reassuring result suggests that our findings are robust to drastically different options for measuring the labor market penalty attached to an Arabic name.

Table 3 reports basic summary statistics on unemployment rates by occupation and name type (u_l^{a} and u_l^{na}) and the associated unconditional unemployment penalty associated with Arabic names ($\hat{\gamma}_l = u_l^{\mathrm{a}} - u_l^{\mathrm{na}}$). For the sake of exposition (in this table only), we group together the 29 different occupations listed by INSEE into 7 main categories: farmer, craftsman, unskilled blue-collar, skilled blue-collar, clerk, intermediate, and executive. On average, Arabic name holders have an unemployment rate of 20 percent, around three times as high as the unemployment rate of non-Arabic name holders (7 percent). But this average comparison hides a lot of variance across occupations. The unemployment rate of Arabic name holders among executives is only 7 percent and the unemployment gap with non-Arabic name holders falls to 3 points for this occupation. In contrast, the unemployment rate of Arabic name holders reaches 29 percent among (unskilled) blue collar workers, which represents an unemployment gap of 14 percentage points with the non-Arabic name holders in the same occupational category. The unemployment differentials for each of the 29 detailed occupations (which we use in our regressions) are presented in panel (a) of Figure E.1 in the online appendix.[17]

Table 3 – Unemployment rates by Name type and Occupation

	Unemployment rate		Unconditional Penalty
	Arabic name	non-Arabic names	
Executive	0.07	0.04	0.03
Intermediate	0.14	0.05	0.09
Clerk	0.20	0.09	0.11
Blue collar (skilled)	0.20	0.07	0.13
Blue collar (unskilled)	0.29	0.15	0.14
Craftman	0.15	0.04	0.09
Farmer	0.10	0.00	0.10
Total	0.20	0.07	0.13

Notes: The sample covers the 4 years of employment survey we have access to (2003-2007). The statistics are for adults between 25 and 55 years old.

Explaining the variation in unemployment differentials across occupations goes beyond the scope of this paper. We can however think of several mechanisms that have been put forward by theoretical and empirical research that could generate this type of cross-occupation variation in discrimination on which our measurement approach relies. A type of discrimination for which there is direct empirical support in the French case is customer-based: Combes *et al.* (2016) correlate the different penalty levels across occupations to how frequently employees are in direct contact with native customers. Occupations with higher levels of contact with native customers are characterized by a higher degree of employment discrimination against minority employees. Another mechanism contributing to cross-occupation variation in penalty relates to employee-based discrimination (Arrow, 1972). Applied to our context, French natives with non-Arabic names would request a higher amenity-adjusted wage to work alongside minorities with an immigration background. To the extent that prejudice/taste for discrimination among non-Arabic workers, or that the intensity of contact between workers vary across occupations, both mechanisms are expected to generate variations in unemployment penalty against Arabic names across

[17]Table F.7 in the online appendix presents evidence on the economic penalty at the name level by displaying the unemployment rate associated with the 10 most popular non-Arabic and Arabic names.

occupations. They also suggest that we should observe less discrimination in occupations where the labor market is tight and recruitment is difficult (see Baert *et al.*, 2015, for recent evidence). Finally, empirical work has shown that stereotyping and ensuing discrimination against a given type of employee (defined based on gender, ethnicity etc.) depends on how representative this type of employee is in a given occupation. For instance, gender stereotyping and discrimination against males has been shown to be particularly pervasive in female-dominated occupations (see e.g. Riach and Rich, 2006; Booth and Leigh, 2010). Along the same logic, stereotyping based on ethnicity should lead to variations across occupations in the discrimination intensity.

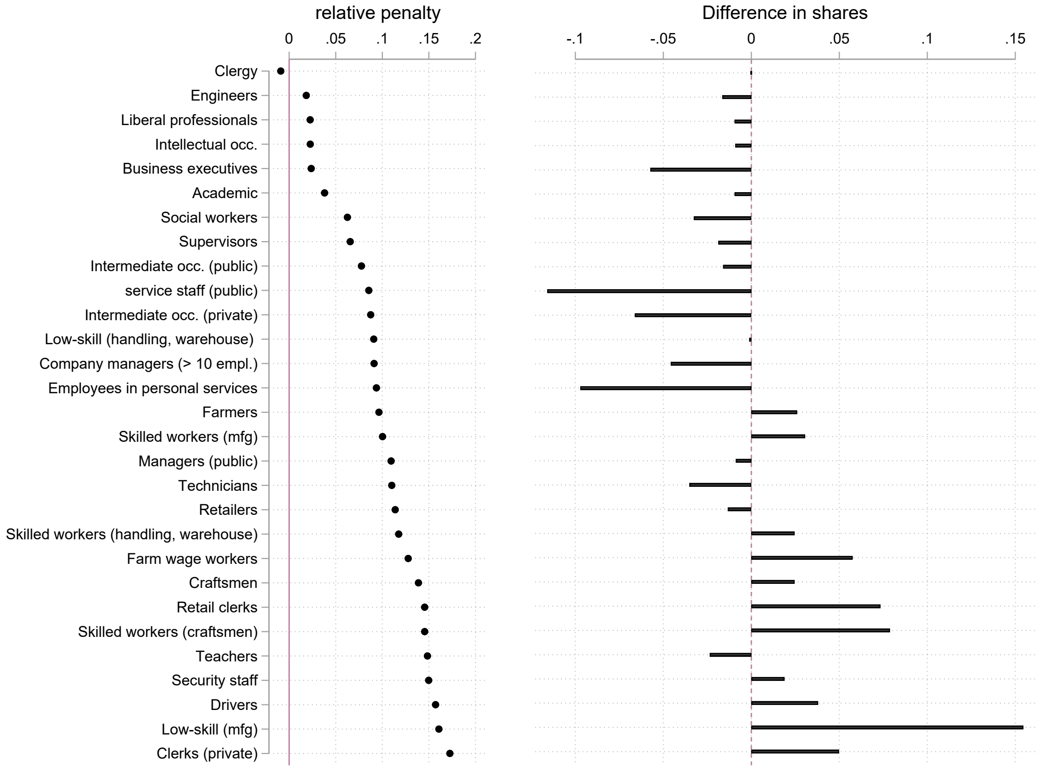
Graphical presentation of identifying variation. As detailed in Equation (4), the variation in the intensity of the LIP comes from nationwide differences in penalties across occupations interacted with differences in occupational shares across blocks. We present graphically these sources of variation in Figure 1, which contains two panels.

The left part of panel (a) reports the unconditional unemployment penalty for each detailed occupation. The right part of panel (a) shows the difference between the average share of each occupation in blocks belonging to the top (D10) and bottom decile (D1) of the distribution of the local information on penalty (LIP), computed for people living in public housing. We see that the difference between D10 and D1 is explained to a large extent by D10 blocks featuring i) lower shares of public servant and employees in personal services for instance (occupations associated with medium levels of penalties), and ii) higher shares of retail clerks, skilled craftsmen and low-skill manufacturing workers (associated with high levels of nationwide penalty). Taken together, these figures illustrate how the variation in the LIP comes from the co-movement between occupational shares and associated penalties.

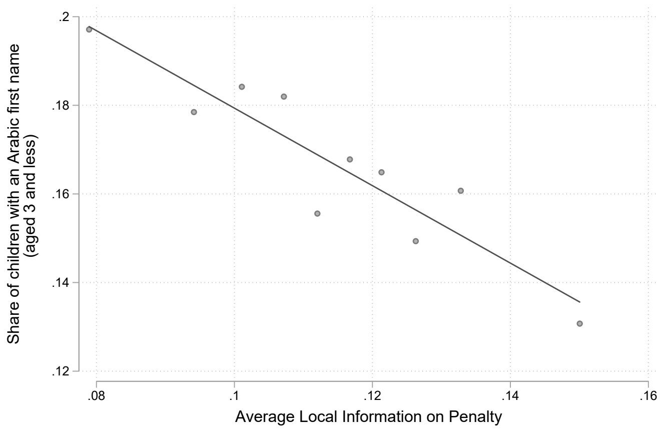
Panel (b) of Figure 1 reports a binned scatter plot (10 bins) of the relationship between the LIP and the average share of children (aged 3 and less) with an Arabic first name. The underlying sample consists in 1500 public housing blocks that are regrouped by decile of the distribution of LIP. Visual inspection reveals that the two measures are negatively related at the block-level. As such, it constitutes a preview of our main analysis that is carried out at the individual level.

Figure 1 – The measure of local information on penalty

(a) across occupations: penalties (left) / D10-D1 in block employment shares (right)



(b) LIP and the share of children (aged 3 and less) born with an Arabic first name



Notes: The left part of panel (a) reports the unconditional occupational penalty in each occupation (i.e. the difference of unemployment rate between Arabic and non-Arabic name holders). The right part of panel (a) shows the difference between the average share of each occupation in blocks in the top and bottom decile of the distribution of the local information on penalty (LIP). The LIP is the average local occupation penalty weighted by the local share of each occupation (see Equation (4) and associated text) Panel (b) reports a binned scatter plot of the relationship between the average share of childen age 3 or less with an Arabic first name and the LIP. Panel (b) and the right part of panel (a) are computed for the public housing part of the sample.

4.2 Baseline results

Table 4 displays the logit estimation results of equation (6). The dependent variable is a binary variable coding for the Arabic origins of a baby's name. Our baseline sample consists of babies aged between 0 and 3 living in public housing. All specifications add parental occupation fixed effects, parental education fixed effects, and *département* fixed effects (see Section 3.2). Standard errors are clustered at the residential block level. Average marginal effects of logit estimates are reported in all regression tables.

Columns (1) to (5) estimate regressions on babies with parents from all origins. Columns (6) to (9) restrict the sample to children born with parents or grandparents who are nationals from our list of Muslim countries described above. Those last four columns therefore condition on the first of our two vertical transmission dummy variables being turned on. The first part of the table considers the Muslim origins as a separate determinant. The second part allows for those Muslim origins to influence all determinants, and particularly how sensitive parents are with respect to the economic penalty.

The first striking result is that the coefficients on our two vertical transmission variables are positive and strongly significant in all regressions of columns (1) to (5). In the first column, we find that having a parent or a grandparent with a Muslim origin increases the probability of bearing an Arabic name by 11 percentage points. Having a parent with an Arabic name yields an even stronger effect at 28 p.p, in a sample where the baseline probability is 24%. The vertical transmission is therefore a first-order determinant, confirming the broad features of the data described in Section 2.2. The horizontal transmission channel also exhibits a strong positive effect, significant at the one percent threshold. With an average marginal effect at 0.11, and a standard deviation of this variable at 0.25, the magnitude of the effect of horizontal transmission is smaller than vertical determinants, but still important.

Column (1) presents results with a naive information structure of the economic penalty. In this specification, the two main channels of information we consider, occupation and neighbors, are entered in the regression separately. Parents are assumed to retrieve information from (Arabic vs non-Arabic name-holders) unemployment differential i/ in their own occupation (nationally), ii/ among their neighbors—independently of their occupation. Both coefficients are negative but lack statistical significance. Regarding the self-occupation measurement, one should note that it is likely to be very noisy, since parents should not systematically infer that their children will have the same occupation as them. Besides and perhaps more importantly, our empirical strategy is not dealing with the fact

Table 4 – The choice of an Arabic name - Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Var:				Arabic name for baby					
one parent/grandp. w/ Muslim country nat.	0.11^a	0.09^a	0.09^a	0.09^a	0.09^a				
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)				
one parent with Arabic name	0.28^a	0.23^a	0.23^a	0.23^a	0.23^a	0.36^a	0.36^a	0.36^a	0.34^a
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.04)	(0.04)	(0.04)
share of Arabic name in block (aged 4-10)	0.11^a	0.09^a	0.08^a	0.09^a	0.07^a	0.03	0.06	0.07	0.00
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)	(0.06)	(0.06)	(0.06)
occupational information on penalty	-0.05								
	(0.16)								
local unemployment penalty	-0.02								
	(0.02)								
local information on penalty		-0.86^a	-1.00^a	-1.06^a	-1.10^a	-2.95^a	-2.94^a	-3.19^a	-3.18^a
		(0.33)	(0.34)	(0.34)	(0.36)	(1.07)	(1.09)	(1.09)	(1.08)
local Islamophobia			0.16	0.15	0.14		0.03	0.08	-0.10
			(0.14)	(0.14)	(0.15)		(0.45)	(0.49)	(0.50)
local ELF index			0.05	0.06	0.04		-0.15	-0.13	-0.20
			(0.04)	(0.04)	(0.05)		(0.12)	(0.12)	(0.14)
share of Arabic name in sector (aged 4-10)				-0.02				-0.10	
				(0.04)				(0.11)	
share of Arabic name in dept (aged 4-10)				-0.18				0.12	
				(0.13)				(0.36)	
share of Arabic name in block (aged 11-25)					0.01				0.08
					(0.04)				(0.10)
share of Arabic name in block (aged 26-49)					0.04				0.15
					(0.04)				(0.11)
share of Arabic name in block (aged 50+)					-0.05				-0.15^c
					(0.04)				(0.08)
one parent/grandp. w/ Muslim country nat. only						Yes	Yes	Yes	Yes
Observations	2806	3829	3829	3811	3777	992	992	987	973
Pseudo R^2	0.384	0.399	0.400	0.403	0.402	0.160	0.161	0.164	0.170
Mean probability	0.24	0.19	0.19	0.19	0.19	0.50	0.50	0.50	0.51
SD LIP		0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
SD Horizontal	0.25	0.24	0.24	0.24	0.23	0.28	0.28	0.28	0.28

Note: logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.

that parents can self-sort into occupation, another source of bias. The second variable, unemployment penalty observed in the immediate neighborhood, is also likely to be a very poor proxy for the perceived penalty. The Labour Force Survey is not stratified so as to be representative at the block level, introducing considerable amount of noise in the measurement of block-level unemployment by name type.

Column (2), therefore, goes to our preferred specification of the economic cost channel, using the local information on penalty (LIP) variable. The vertical and horizontal channels keep similar magnitudes and significance levels. The coefficient associated with the LIP is negative and statistically significant at the one percent level. The estimated economic disincentive of giving an Arabic name is larger in households who live in blocks populated with individuals holding jobs most exposed to an employment penalty. How large is this effect? A natural way to proceed with quantification in this type of econometric models is to compute the predicted probability for all observations using the coefficients from column (2) twice: once using the variables at their "true" levels, and a second time after having shocked the variable of interest. Increasing LIP by one standard deviation (0.019) reduces the probability of giving an Arabic name by 1.59 percentage points (8.4% of the mean probability in this sample). It turns out that multiplying the average marginal effect by the standard deviation ($-0.86 \times 0.019 = -0.01634$) yields a very reasonable approximation of the correct quantification accounting for the non-linearity of the estimator.

One might be concerned with a set of confounding factor related to the way we measure the economic cost channel. For instance, housing blocks with more unskilled workers could be more prone to social discrimination against Muslim-origin individuals, discouraging parents from giving an Arabic name *for non-economic reasons*. Another concern is that Arabic identity could be weaker in blocks with higher ethnic heterogeneity. Column (3) tries to address this concern by controlling for two local measures of potential discrimination against Arabic name holders. We first use answers to a question in a large-scale survey about attitudes toward Muslims in France, designed to be representative at the occupation level (Sauger, 2013). The most relevant question for our inquiry reads as: "Can you tell me if ISLAM means to you something very positive, fairly positive, fairly negative, or very negative?". Following the structure of our LIP variable, we weight the occupation-specific answers by the share of each occupation in the block. As a second control, we also include a standard ELF (ethno-linguistic fractionalization) index measuring the heterogeneity of households from different countries of origin within each block. Introducing those controls in Column (3), we see that those alternative stories do not receive strong empirical support, while the impact of the economic channel remains essentially unchanged.

Columns (4) and (5) document additional features of the horizontal channel. [18] In column (4) we add the share of Arabic names for kids aged 4-10 in larger areas, either at the *sector* level or at the *département* level. Those two variables measure the spatial decay of the horizontal transmission channel by looking at wider geographical units. Neither of those two variables exhibit any significant influence, and the block-based horizontal estimate is unchanged. This points to the importance of studying those channels of transmission at a very fine-grained geographical level. Column (5) includes the share of Arabic names for older cohorts with the aim of identifying the reference group of the parents in their naming decisions. Overall, results from those two columns suggest that the horizontal channel only operates through recent choices of close neighbors, i.e. local cohorts of children under 10 years old.

In the remaining columns of Table 4, we focus on the sample of "pure transmitters" by looking at determinants of naming decisions among babies born in France while their parents or grand-parents are born with a nationality from Algeria, Morocco, Tunisia, Turkey and Middle-East countries. Columns (6) to (9) replicate columns (2) to (5) on that reduced sample, yielding noticeable changes in our three channels of interest. The vertical channel (now reduced to one variable by construction of the sample) is stronger than in the general sample, while the horizontal channel becomes weaker and insignificant. Among the set of parents with Muslim/Arabic cultural background, the naming patterns of direct neighbors is much less relevant than the transmission of ones' own cultural trait. Remarkably, the effect of LIP is about three times larger for migrants from Muslim/Arabic origins than for the full sample of parents. Renewing the quantification outlined above, we find that a one standard deviation increase in the perceived economic cost reduces the probability of giving an Arabic name to a child by around 5.5 percentage points (estimates from column 6). This is a large effect, representing more than a 10% fall in the baseline probability of this sample (around 50%). Columns (7) to (9) add control variables for local xenophobia, peer effects from older cohorts or from larger geographic localities. Like in the full sample, the coefficients of those additional variables are not statistically significant. This finding suggests that our estimates are unlikely to be pervasively contaminated by endogenous res-

[18] In our baseline analysis, we measure the horizontal channel as the share of Arabic names among 4-10 year-old children in a given block. This share is driven by the behavior of parents irrespective of whether they have Arabic names themselves. We checked whether the strength of the horizontal channel is differentiated when this share is evaluated among all parents in a block or among parents with Arabic names only. We cannot reject the hypothesis that the effects are different (see Table E.4 in the online appendix).

idential sorting even for the population most likely to transmit their cultural background (in which case the coefficients for older cohorts should also be non-zero).

We explore further the heterogeneity of the economic cost channel in our online appendix (Table E.6). We show in that appendix that the average effect of the LIP estimated in column (2) of Table 4 is entirely driven by parents with Muslim/Arabic origins. Naming decisions by other parents do not react to the economic channel. Our interpretation is that parents with a migration background have themselves probably been exposed to discrimination over their lifetimes. In contrast, other parents have not and are consequently less sensitive to the negative premium attached to Arabic names on the labor market. Accordingly, the trade-off of interest in this paper between vertical transmission and the economic cost channel seems to be relevant only for households with cultural backgrounds that make them aware and more sensitive to the economic consequences of transmitting their trait.

5 Robustness and extensions

In this section, we perform an extensive series of robustness tests. The two most important ones are presented in details; they relate respectively to an alternative measurement of the LIP and to the extrapolation of our findings (based on the sample of public housing tenants) to the rest of the population. Other robustness tests are reported in a more compact way—all additional details being relegated to the online appendix.

5.1 Conditional unemployment Differentials

In our baseline empirical analysis, the LIP is based on $\hat{\gamma}_l = u_l^{\mathrm{a}} - u_l^{\mathrm{na}}$, namely the observed *unconditional* unemployment gap between Arabic and non-Arabic name holders in each occupation l. We now investigate the robustness of our findings when the LIP is based on *conditional* unemployment gaps retrieved from an auxiliary Mincer-type equation. Our aim is to test for the stability of our main coefficients when we condition in the Mincer equation with a wide range of observables (age, sex, Maghreb nationality, and a set of fixed effects accounting in particular for education). Indeed, the unconditional unemployment gap might be an imperfect measure of the information truly used by the parents to assess the labor market penalty, leading to an attenuation bias in the estimation of α_3 in (5). In particular, parents might use additional information from the observed characteristics of their neighbors, such as education or country of origin, to assess the specific penalty associated with an Arabic name.

In Table 5 we replicate the set of regressions of Table 4, replacing the unconditional version of the LIP with the conditional one (omitting the first column of Table 4 which uses the naive and unsuccessful approach to measuring the economic cost channel). The analysis related to the auxiliary equations, the estimation of the conditional unemployment gaps and the construction of the LIP are detailed in section F of the online appendix. Comparing the estimates obtained in each table, we observe an increase in α_3 when estimated with the second measure. Conditioning on observables therefore does not weaken (and actually strengthens) the coefficient of interest.

Table 5 – The choice of an Arabic name - Conditional penalty

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var:	Arabic name for baby							
one parent/grandp. w/ Muslim country nat.	0.09^a	0.09^a	0.09^a	0.09^a				
	(0.01)	(0.01)	(0.01)	(0.01)				
one parent with Arabic name	0.23^a	0.23^a	0.23^a	0.23^a	0.36^a	0.36^a	0.36^a	0.34^a
	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.04)	(0.04)	(0.04)
share of Arabic name in block (aged 4-10)	0.08^a	0.08^a	0.09^a	0.07^a	0.04	0.06	0.07	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)	(0.06)	(0.06)	(0.06)
local info. on penalty (Mincer-based)	-1.35^a	-1.35^a	-1.39^a	-1.44^a	-3.16^a	-3.03^b	-3.14^a	-3.33^a
	(0.37)	(0.37)	(0.37)	(0.38)	(1.17)	(1.19)	(1.18)	(1.18)
local Islamophobia		0.18	0.16	0.14		0.02	0.04	-0.17
		(0.14)	(0.14)	(0.15)		(0.45)	(0.48)	(0.50)
local ELF index		0.06	0.07^c	0.05		-0.12	-0.10	-0.18
		(0.04)	(0.04)	(0.05)		(0.12)	(0.12)	(0.14)
share of Arabic name in sector (aged 4-10)			-0.02				-0.09	
			(0.04)				(0.11)	
share of Arabic name in dept (aged 4-10)			-0.19				0.09	
			(0.13)				(0.36)	
share of Arabic name in block (aged 11-25)				0.01				0.09
				(0.04)				(0.10)
share of Arabic name in block (aged 26-49)				0.04				0.17
				(0.04)				(0.11)
share of Arabic name in block (aged 50+)				-0.05				-0.14^c
				(0.04)				(0.08)
one parent/grandp. w/ Muslim country nat. only	No	No	No	No	Yes	Yes	Yes	Yes
Observations	3829	3829	3811	3777	992	992	987	973
Pseudo R^2	0.401	0.401	0.404	0.403	0.160	0.161	0.164	0.170
Average prob.	0.19	0.19	0.19	0.19	0.50	0.50	0.50	0.51

Note: logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10% level. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.

5.2 How general are our results?

Beyond the usual econometric questions of identification and estimation, one can wonder how general the obtained results are. In our case, the baseline regressions are estimated using a specific part of the population, for which, we believe, biases linked to spatial sorting in particular are minimized: Public housing tenants. Moreover, our central trade-off between the vertical and economic cost channels is found to be mostly relevant for households with Muslim/Arabic cultural background (see Section 4.2 and online appendix Table E.6). This group is also the main population of interest for the type of cultural transmission under study. Therefore, the most important external validity question is whether we can generalize results to all households with similar cultural backgrounds, *irrespective of whether or not they live in public housing.* We provide two exercises to that effect below.

Observables in public/private housing. Table 6 contains a number of characteristics of public vs private housing tenants among our population of interest. The list of variables covers a range of labor market outcomes and skill-levels. The first point of note is contained in the last row of the table: A large fraction of households with Muslim/Arabic cultural background in France lives in public housing. In our dataset, over 50% of babies born from those households live in public housing units (992 versus 972). This implies that, independently of potential differences with the private housing sample, the sample we use for estimation and counterfactual exercises contains most of the relevant observations.

Regarding observable characteristics, Table 6 shows some expected differences in socio-economic variables: Households living in public housing units have higher unemployment propensities for both parents, and the mothers' labor force participation is lower. However, the differences are not statistically significant for two out of the three variables.

Differences in terms of occupations and education between the two groups are more marked. Private housing hosts more high-skilled and much less low-skilled occupation shares of the considered population of fathers, while the middle-skill shares are fairly similar.[19] We do see significant differences in terms of average monthly wages. However, it is also instructive to look at the overall distribution of wages among the two populations, which shows substantial overlap, as illustrated in figure 2, showing the densities of monthly wages, separately for men and women. The "bump" in the low part of the distribution for

[19] High-skill occupations refer to executives, managers, and engineers, while middle-skill refers to mid-management and technicians.

Table 6 – Characteristics of public vs private housing tenants among households with a Muslim/Arabic cultural background

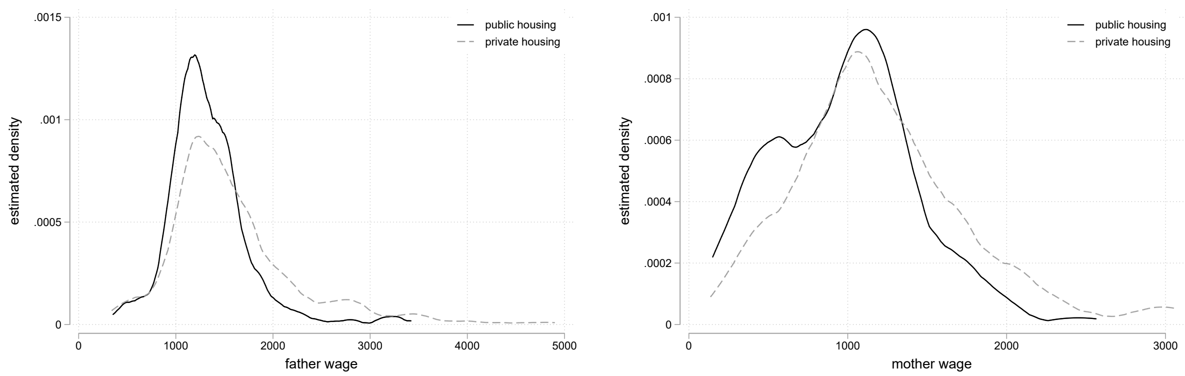
	All	Public housing	Private Housing	Private-Public
Father is unemployed	0.17	0.20	0.15	-0.06**
Mother is unemployed	0.13	0.14	0.12	-0.02
Mother LFP	0.43	0.42	0.44	0.03
Father has high-skill occ.	0.06	0.02	0.10	0.08***
Father has middle-skill occ.	0.11	0.10	0.12	0.02
Father has low-skill occ.	0.75	0.83	0.68	-0.15***
Father: higher ed	0.09	0.07	0.12	0.05***
Father's monthly wage	1504.42	1372.75	1629.11	256.36***
Mother's monthly wage	1109.38	981.20	1214.90	233.69***
Observations	1964	992	972	-20

[1] Notes: This table presents the mean of a selected list of characteristics of households where at least one parent or grandparent has a nationality from a Muslim-majority country (as defined in section 4.1). Within this population, it compares households living in public (column 2) versus in private housing (column 3) and test for the statistical significance of the difference (column 4). Significance at the 1%, 5% and 10% level is denoted by ***, ** and * respectively.

women is the most noticeable difference, and is probably due to a larger share of part-time female workers in public vs private housing.

The large overlap across households in public and private housing—even in terms of observables that are key to gaining access to public housing such as monthly earnings—might seem surprising. It is probably related to several factors driving the public housing allocation process. In particular, the large weight given to number of children in households when determining eligibility creates some overlap in overall income / education. Moreover, the well-documented public housing shortage in most areas implies long waiting lists. While the waiting time is a function of socio-economic variables, the actual access to public housing will still depend largely upon when a family first applied.

Figure 2 – Distribution of the monthly wage for fathers (left) and mothers (right) across in public and private housing.



Notes: This figure displays kernel density estimates for the distribution of public housing tenants (black solid line) and private housing tenants (gray dash line) separately among households where at least one parent or grandparent has a Muslim-majority country nationality (as defined in section 4.1).

Re-weighting regressions. While the statistics displayed above are reassuring as to the extrapolation of our results to the entire population (of Muslim/Arabic background parents), small differences in terms of observable characteristics could still translate into very different behavioral responses if those characteristics exert a strong mediating effect on the channels (vertical, horizontal and economic) underpinning parental naming decisions.

In order to investigate this possibility, we carry out an additional regression-based test. An intuitive approach would consist of replicating our baseline analysis for the overall sample (or for the sample of households living in private housing). However, this would lead us back to the first-order issue of endogenous spatial sorting of households, which is likely to yield inconsistent estimates. Therefore, in keeping with our main specification estimated on the public housing sample, we follow a different route, which involves re-weighting observations of that sample. The procedure is designed so that the re-weighted public housing sample displays similar distributions for a selected number of covariates compared to the targeted sample (either the overall set of households, or the ones living in private housing). To that effect, we adopt Hainmueller (2012)'s entropy balancing method. Those entropy weights are computed to ensure the closest possible balance between the two samples along the first moments of all the categorical variables presented in table 6.[20] This test builds on the notion that the contrast between weighted and unweighted estimates is informative about the presence of potential heterogeneous effects in behavioral

[20]We focus on categorical variables because for these matching the first moment is the natural target.

responses (Solon *et al.*, 2015). Note that the weighted and unweighted estimates could be similar for two reasons: Either there is indeed limited heterogeneity along the dimension for which we are re-weighting or the weights are fairly uniformly close to 1 as there is very limited unbalance between the public housing sample and the targeted sample among the population studied.

Table 7 – Re-weighting the public housing sample to match the distribution of all and private housing samples

	(1) Public : No weight	(2) – : Weights, private	(3) –: Weights, all	(4) Private	(5) All
Father is unemployed	0.20	0.15	0.17	0.15	0.17
Mother is unemployed	0.14	0.12	0.13	0.12	0.13
Mother lab. force part.	0.42	0.44	0.43	0.44	0.43
Father has high-skill occ.	0.02	0.10	0.06	0.10	0.06
Father: higher ed	0.07	0.12	0.09	0.12	0.09

[1] Notes: This table presents the mean of a selected list of characteristics of households with at least one parent or grandparent has a nationality from a Muslim-majority country (as defined in section 4.1). Column (1) presents statistics for the unweighted public housing sample. Column (2) presents statistics for the weighted public housing sample where weights are computed to match the private housing sample which is presented in Column (4). Column (3) presents statistics public housing sample with weights computed to match the overall sample which is presented in Column (5). The weights are obtained using Hainmuller (2012)'s entropy balancing. Entropy balancing relies produces a set of unit weights so that the re-weighted sample satisfies a large set of pre-specified balance conditions based on known sample moments (here the first moments of binary variables).

The obtained weights can be used to check the balance between the re-weighted public housing sample and the targeted sample. Those are displayed in Table 7. We compute two sets of weights, one matching moments for the subset of households with Muslim/Arabic background living in the private housing sector (column 2) and the other one matching the overall household population (column 3). We see that, in both cases, the balance is almost perfect, and t-tests (not displayed) all fail to reject any systematic differences.

We finally estimate the main specification on the re-weighted sample, using the two sets of weights contained in Table 7. Results are displayed in Table 8. Comparing columns (2) and (3), we see that, while the estimated effect of the economic channel is somewhat lower when using the weights matching the private housing sample's distribution, the difference is quite small. The difference in estimate is even smaller when using the second set of weights—which (as expected) yields results in between the unweighted sample (column 1) and the weights matching the private sample (column 2). In both cases, the coefficients' stability suggests that the results obtained from the public housing sample can be plausibly extrapolated to the whole population of Muslim/Arabic background parents.[21]

Table 8 – Re-weighting the public housing sample to match all/private housing households

	(1)	(2)	(3)
		Reweigthed	
	Baseline	Private Housing	All
one parent with Arabic name	0.36^a	0.38^a	0.37^a
	(0.04)	(0.04)	(0.04)
share of Arabic name in block (aged 4-10)	0.03	0.03	0.03
	(0.05)	(0.06)	(0.06)
local information on penalty	-2.95^a	-2.75^a	-2.88^a
	(1.07)	(1.07)	(1.07)
Observations	992	992	992
Pseudo R^2	0.160	0.182	0.168
Mean probability	0.51	0.51	0.51

[1] Notes: Logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years. Column (2) and column (3) display results from the estimation of specifications that are identical to the baseline specification displayed in column (1) except that the samples are now re-weighted based on the entropy weights computed in order for the public housing sample to match a set of first moments of the distribution of the private housing sample (column 2) or overall sample (column 3). The variable used to compute the weights are presented in Table 7, namely: Binary variables for father and mother unemployment, mother labor force participation, father's education and occupational status.

5.3 Additional robustness checks

We now provide a number of additional robustness checks. Those are run on the sample of children living in public housing, aged between 0 and 3, and born from parents with Muslim/Arabic origins. This corresponds to Column 5 of our baseline results in Table 4. We focus on this specification because it is both one of the most demanding one in term of sample restrictions, and also the most relevant one given our interest for cultural

[21] Naturally, our re-weighting procedure is based on a set of observable variables. We cannot exclude the possibility that unobservable characteristics between the two samples would drive the heterogeneity in the responses to the channels of interest and therefore would result in different responses among households living private housing. Consequently, we choose to perform our counterfactual simulations in Section 6 solely on the basis of the public housing sample, which, as mentioned above, includes a large share of the relevant population.

transmission decisions by immigrants and their descendants. The first set of robustness exercises is provided in the main text, while the remainder is relegated to our online appendix.

Weighted regressions and placebo tests. In Table 9, we perform two different exercises, both pertaining to statistical representativeness of the data. First we show that our estimation results are robust to weighting observational units by their statistical representativeness. Second, we perform two placebo tests to establish further that residential sorting is unlikely to drive our results. For the sake of comparison, we start by reporting our benchmark specification in the first column, namely the unweighted logit of Column 5 in Table 4. As discussed in Section 2, the LFS is stratified at the *département* level and representativeness is, thus, not guaranteed at the residential block level, our level of analysis. In Column (2), we report the results for a weighted logit, where the individual representativeness weights reported in the LFS are applied. We see that the 992 observed children in the estimation sample of Column (1) represent 618,314 children nationwide. More importantly, we notice that unweighted logit and weighted logit yield comparable coefficients. We conclude that imperfect stratification at the block level is not an issue for the estimation.

In the next two columns, we run placebo tests to rule out the possibility that our estimated LIP could be driven by some residual statistical bias attached to endogenous residential sorting. We replicate our benchmark specification on a fake sample of parents/neighbors, artificially reallocated to random occupations in column (3) and to random residential blocks in column (4). We see that, in both cases, the LIP coefficient, that is based on neighbors occupations (see equation 4), drops and also loses its statistical significance. This makes us confident that our identification strategy, based on the sample of children living in public housing, gets rid of endogenous residential sorting in an efficient way.

Heterogeneous effects. In Table 10, we look at heterogeneous effects by splitting the estimation sample along various relevant dimensions. In Columns (1) and (2), we document the impact of cultural background by looking at couples where both parents have Arabic origins and at mixed couples, respectively. The LIP coefficient, that captures the magnitude of the economic cost channel, is larger for the latter (though less significant), while the vertical transmission motive is stronger when both parents have Arabic origins.

Columns (3) and (4) display separately the results for 2nd generation children only

Table 9 – The choice of an Arabic name - Robustness 1

	(1)	(2)	(3)	(4)
Dep. Var:		Arabic name for baby		
one parent with Arabic name	0.36^a	0.35^a	0.36^a	0.36^a
	(0.04)	(0.04)	(0.04)	(0.04)
share of Arabic names in block (aged 4-10)	0.03	0.06	0.04	0.04
	(0.05)	(0.06)	(0.05)	(0.05)
local info. on penalty	-2.95^a	-2.23^b	1.32	0.13
	(1.07)	(1.13)	(1.10)	(0.36)
Parent/grandp. w/ Muslim country nat. only	yes	yes	yes	yes
Specifications	Bench.	Weighted	Placebo occup.	Placebo block
Observations	992	618374	992	986
Pseudo R^2	0.160	0.178	0.155	0.153
Mean pred. prob	0.50	0.51	0.50	0.51

Note: logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.

(i.e. babies with parents who migrated to France) and for 3rd generation children only (i.e. babies with parents who are born in France, but with at least one grandparent who has Muslim/Arabic origins). The economic cost channel is much larger for the latter. One interpretation is that parents with Arabic origins who are born in France are more exposed to information on discrimination. For a given level of information, the gap in the estimated effect could also be driven by different preferences, with newly arrived migrants displaying a lower willingness to adjust the vertical transmission of their culture in response to signals about the likely economic cost of such transmission for the economic well-being of their child. Parents who are first-generation migrants appear to put less weight on the economic cost of their naming decisions and attach somewhat more importance to the vertical channel. First-generation parents may be less able to use their surroundings to gather information on the labor market or less aware of the functioning of the labor market when they have kids. The parameter additionally reflects the degree to which parents value the economic welfare of their children. To the extent that parents discount such economic welfare heavily, this will translate into a less negative value of α_3. Accordingly, either difference in the discount rate attached to their children's economic welfare or differences in information sensitivity could explain first-generation parents' higher willingness-to-pay—in terms of the penalty experienced by their offspring—to perpetuate their own culture.

In columns (5) and (6) of Table 10, we study how naming decision determinants differ across genders by splitting the sample in two: Baby girls and baby boys, respectively. We see that the horizontal transmission channel is significant for girls, but not for boys. The reverse is true of the information on economic penalty whose effect is significantly negative for boys only. Previous works by sociologists who study naming patterns among minorities have documented that parents are more open to "creative names" for girls than for boys, who tend to receive more traditional names (Sue and Telles, 2007; Gerhards and Silke, 2009). This difference is likely to result in a lower rate of name convergence for boys than for girls. This is reflected in the mean predicted probability that girls receive an Arabic name which is much lower (0.44) than that of boys (0.57). Interestingly, this lower rate of assimilation for boys occurs despite the fact that the marginal effect of the economic channel (while negative for both genders) is much stronger for boys than for girls (which has a p-value of 0.101). Finally, we see that the horizontal channel is stronger for girls while the vertical channel is of similar magnitude across genders. Overall, our results suggest that parents are either more cognizant or more sensitive to the economic penalty imposed upon their baby boys than baby girls. This is consistent with parents' envisioning traditional gender roles for their children in the labor market and being more sensitive to peer-effects when choosing girl names.

Table 10 – The choice of an Arabic name - robustness 2

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var:			Arabic name for baby			
one parent with Arabic name	0.37^a	0.22^c	0.38^a	0.33^a	0.41^a	0.34^a
	(0.04)	(0.13)	(0.05)	(0.06)	(0.06)	(0.05)
share of Arabic names in block (aged 4-10)	0.06	0.17	0.09	-0.03	0.15^c	-0.02
	(0.06)	(0.17)	(0.07)	(0.09)	(0.08)	(0.08)
local info. on penalty	-2.55^b	-3.72	-1.47	-3.69^b	-2.29	-3.98^a
	(1.24)	(2.66)	(1.47)	(1.62)	(1.64)	(1.54)
Parent/grandp. w/ Muslim country nat. only	yes	yes	yes	yes	yes	yes
Specifications	Non-mixed	Mixed	2nd gen.	3rd gen.	Baby girls	Baby boys
Observations	782	143	517	432	464	470
Pseudo R^2	0.169	0.227	0.220	0.173	0.175	0.222
Mean pred. prob	0.52	0.50	0.49	0.52	0.44	0.57

Note: logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.

Intensity of vertical transmission. Our measure of the vertical transmission channel is a binary variable equal to 1 whenever one of the parents has an Arabic first name. We differentiate the effect depending on whether the mother only, the father only or both parents have an Arabic first name. Households where none of the parents have an Arabic name is the reference group. Results are presented in the online appendix Table E.5, in which we see that couples where the mother has an Arabic name are 19% more likely to give an Arabic name to their child than couples where neither of the parents carries an Arabic name. Interestingly, the father having an Arabic name is associated with a somewhat larger increase in the probability (+22%). Finally, we see that having both parents carry an Arabic name is associated with a substantially larger effect (+26%). The finding that the father's cultural marker has a larger influence on naming is consistent with previous sociological studies (see Sue and Telles, 2007, who studies naming pattern among Hispanics in the US). Once we focus on individuals with an immigration background from Muslim countries, we see the same ordering in terms of the size of the vertical channel across categories of couples.

Use of measure of relative penalty rather than in levels. Our analysis is based on a measure of occupation-specific penalties expressed in *levels*, i.e. the difference in percentage point of unemployment rates between Arabic and non-Arabic name holders. In section E.1 of our online appendix, we show that using *relative* unemployment penalties, i.e. the ratio of unemployment rates of Arabic name holders versus that of individuals with non-Arabic names, leads to quantitatively similar results (see in particular Table E.2 of that appendix). We further show that our results are unchanged when using the measure of penalty in levels and controlling for baseline unemployment rate, i.e. unemployment among non-Arabic-name workers, as predicted by the occupations of neighbors (see Table E.1 in particular).

Interaction between the horizontal and economic cost channels. A concern that might arise regarding the economic channel is that part of it could operate through the horizontal channel. Indeed, the intensity of Arabic name-giving in a given block is the result of individual decisions that are themselves a function of the LIP (since the naming decision is also the LHS variable, this results in a potential manifestation of the reflection problem). In that setting, conditioning on the share of children with Arabic names might bias our estimate of the marginal effect of the LIP due to post-treatment bias (Imai *et al.*, 2011). A feature of our benchmark regressions, which should mitigate the possibility that the horizontal channel is a product of the LIP, is that we measure the horizontal channel as the *lagged* block-level share of Arabic names given to children. There is, thus, a minimum of 1 year between the naming decision of households we are analyzing (as the LHS variable) and the realization of the same outcome among peers in our measure of the horizontal channel (as a RHS variable).

In order to further assess how the magnitude of our economic channel estimates depends upon the presence of the horizontal channel, we provide robustness regressions, where we re-estimate our baseline specification without measuring the horizontal channel. The results, reported in Table E.3 of the online appendix, show that omitting the horizontal channel variable leaves the estimated coefficient associated with the economic cost virtually unchanged. This suggests that, while interactions between the horizontal and economic cost channels could, in principle, be an issue for our estimation, they do not seem to matter in practice.

6 Quantification and Welfare Analysis

We now turn to quantifying the effects of vertical, horizontal, and economic channels in the naming decision. We first analyze the short-run contributions of each channel. We then perform a welfare analysis. In section D of the online appendix, we also quantify the long-run effects taking into account the dynamics of inter-generational cultural transmission. All the analysis is based on estimates from our baseline Table 4.

6.1 Short-run effects

In Table 4, coefficients are reported as average marginal effects over choices in our sample. Therefore, the change in the baseline probability of an independent change in each channel is easy to interpret (see section 4.2). An alternative, and interesting, way to quantify those effects relative to each other is to look at the model's predicted numbers of babies born with an Arabic name when we shut down each of the three channels in turn. In order to calculate such counterfactuals, we adopt the following strategy: We start by running our benchmark regression to estimate the coefficients of interest, which gives us the benchmark probability of transmitting an Arabic first name in the sample. Then we run the counterfactual by changing the values for one or more explanatory variables. For instance, we shut down the economic cost channel by forcing the LIP variable to be zero for the whole sample. The logit formula (6) provides the counterfactual naming probability for each observation.

Summing those over the sample gives the counterfactual number of babies born with an Arabic name in each experiment. This procedure ensures that the probability remains within the admissible range while doing a "what if" experiment.

Results are reported in Table 11, where different lines present different scenarios. We focus on the sample of babies born from parents with a Muslim/Arabic background and living in public housing (the point estimates of column (6) in Table 4). The first line reports the true number of babies with an Arabic name, 501 in this sample; they represent 320,851 babies nationally when survey weights are applied. The second line is the number of babies born with Arabic names as predicted by the benchmark regression of column (6). We then remove (in the third row) the vertical channel associated with parental name. The predicted number of Arabic naming decisions falls to 221 in that case, that is, 44% of true births. This is a quite drastic cut, especially when compared to the horizontal channel, where a similar thought experiment removes only 2% of Arabic naming decisions from the benchmark. The economic channel has a much stronger effect than the horizontal one: removing the economic penalty completely increases the number of babies receiving an Arabic name by 56%. The line "no ghetto" shows the results from a slightly different experiment. In this scenario, all blocks in the country had the same neighborhood composition and the same information on unemployment penalty. This amounts to considering the predicted number of babies when averaging the horizontal and penalty variables, which induces effects that almost cancel out in naming choices on average.

Table 11 – Quantification of the 3 channels

	# babies with arabic name		Mean Δ Welfare wrt benchmark	
Scenario:	count	weighted count	change	weighted change
true figure	501	320851	.	.
benchmark	501	316292	.	.
no vertical (parental name)	221	139636	-.555	-.567
no horizontal	491	310516	-.026	-.024
no penalty	783	490945	1.098	1.109
no ghetto	500	316275	.002	.005
no foreign names	0	0	-.822	-.838

Note: This table uses logit estimates (col 6 of Table 5) based on the sample of 992 babies (0-3 years old) with Arabic origins and living in public housing (representing 618374 nationally). Each line presents a scenario, removing in turn one of the channels of influence in the regression.

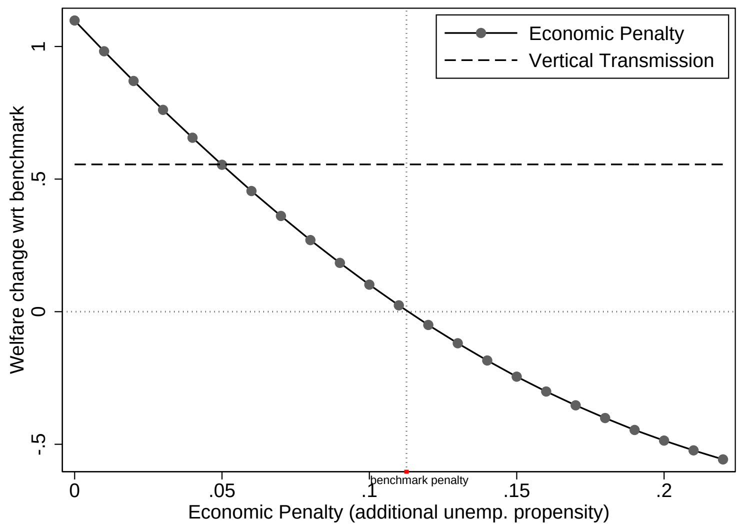
6.2 Welfare analysis

Our random-utility, discrete-choice model of naming choice allows for a quite simple characterization of welfare changes associated with the different thought experiments of Table 11. A natural metric for welfare in this model is the expected value of *parental* maximum utility between naming choices (the theoretical analysis of welfare analysis in our model is provided in online appendix B). This varies across households, and we average this welfare over our sample (a simple average in the third column of Table 11, and a weighted one in the fourth). Note that we consider welfare from the parental perspective—a natural approach in this short-run analysis, where parents are the decision makers in term of naming choice. When it turns to modeling inter-generational dynamics (Online appendix D), the approach has to be qualified because our underlying model of cultural transmission is based on an assumption of imperfect altruism (see our theoretical discussion in section 3.1): This implies that children's welfare and parents' welfare cannot be treated as one and the same.

The absolute level of welfare has no meaningful unit in the logit model—as noted by Anderson *et al.* (1992) and Train (2009)—and a natural way to quantify welfare changes is first to take the difference between welfare in each scenario and the benchmark case and then compare across scenarios. Looking at the last two columns of Table 11, we see, for instance, that the negative impact of removing the vertical transmission motive would be more than twenty times larger than the effect of removing the horizontal one. Considering economic penalty, the effect naturally varies according to the cut in the additional unemployment rate associated with Arabic names. If this penalty were brought to zero, the gain in welfare would be about twice as large as the one arising from the utility boost linked to vertical transmission. Figure 3 spans over a wider set of changes in economic penalty and compares it to the welfare changes associated with vertical transmission. The x-axis reports the counterfactual economic penalty (percentage point differences in unemployment rates). The y-axis measures welfare change differences with the benchmark level (with zero change occurring at the sample average of economic penalty, around 11 percentage point difference). We also represent the utility gain (with respect to benchmark) associated with vertical transmission. An interesting conclusion from this figure is that we are now able to gauge cultural attachment strength in monetary units. Indeed the vertical transmission motive of one's cultural trait is equivalent in terms of welfare gains to a cut by around half the perceived economic penalty associated with that trait. Since, in this sample, this amounts to cutting the penalty by around 6 percentage points, using the estimates of unemployment-related income loss reported in section F of the online appendix, the vertical transmission channel is found to be on the same indifference curve as a 3% upward shift in lifetime income of one's child.

It is possible to look for differences in the monetary valuation of cultural attachment across households. Probably the most meaningful source of heterogeneity is related to whether the parents choosing a name for their baby are themselves first-generation migrants from Arabic countries versus being born in France from migrating Arabic parents. This distinction is done in columns 3 and 4 of Table 10, and we reproduce in online appendix C the equivalent of figure 3 for the sub-samples of 1st and 2nd generation parents separately (Figure C.1). Interestingly, we find a much stronger monetary equivalent of the vertical transmission motive for first-generation migrants. For those, the vertical transmission channel is on the same indifference curve as a 6.2% upward shift in lifetime income of one's child while for second generation parents this figure amounts to a 2.3% upward shift in lifetime income.

Figure 3 – Welfare in the short-run, economic penalty. and vertical transmission



Finally, we consider an experiment where France would return to historical naming regulations. Between 1803 and 1993, the choice of first names was essentially restricted to Saints' names, names from ancient Greece and Rome, and names from the Bible. The legal procedure was that a civil officer had to state whether the name proposed by the parents respected the 1803 Napoleonic law. If the answer was negative, the parents had to challenge the decision in court. Foreign names were hardly tolerated at all before a 1987 revision explicitly asked civil officers to be more liberal with names coming from a "foreign or French tradition, whether national or local". Note that the computation of this scenario involves shutting down not only all three channels emphasized in our paper, but also the occupation, regional, and educational controls we have in the regression, in order to generate a predicted number of babies with Arabic names of 0. The mean welfare loss from this return to a strict ban on foreign—and therefore Arabic—names would be substantial, around 50% larger than the cut of the vertical channel alone.

Long-run implications. In Section D of the online appendix, we explore the long-run implications of our structural model on naming patterns. To this purpose we consider a simple extension of the static model described in Section 3 that accounts for inter-generational cultural transmission dynamics. We restrict our focus to a partial equilibrium setup where any potential feedback effect of naming patterns on the economic penalty is ignored—admittedly an important simplification in a long-run perspective. We come up with two main findings. First, our quantification shows that the long-run share of Arabic name-holders predicted by our structural model for the population of Muslim/Arabic background individuals living in public housing should converge to 9%, which is much smaller than the actual one in our sample (48%). Hence, the actual share is still far from its steady-state value and transitory dynamics are expected to bring it down in the future. This feature might be explained by the fact that migration from Arabic countries is still a quite recent phenomenon in France, and most babies born in the 2003-2007 period belong to the third generation of migrants only. Second, our analysis confirms that the economic cost channel is also a key driver of cultural transmission in the long run. In a counter-factual scenario, where the labor-market penalty attached to Arabic names is artificially brought to zero, the long-run share of Arabic name-holders should converge to a much larger steady-state level, namely 36% instead of 9%. Finally, we refrain from drawing any strong conclusion with respect to long-run welfare effects. As discussed above, defining a welfare criterion in an inter-generational model of cultural transmission would require taking a stance on the degree of parental altruism. This would imply extending the model beyond the scope of current paper.

7 Conclusion

While it might seem natural to consider culture as a deep individual characteristic, our paper shows that the cultural choices made by a person cannot be completely insulated from the economic context in which he/she operates. We focus on one cultural trait that has the advantage of being easily measurable, identified by social sciences as a key marker of cultural identity, and has economic consequences: First names.

Our results show that the information about economic factors available to parents deeply shape individual decisions of cultural transmission. While the vertical channel plays a key role in the cultural transmission process, parents do account for the information about the economic cost of their cultural trait in their naming decisions. Counterfactually reducing the economic penalty on Arabic names to zero, the annual number of babies born with an Arabic name in France would be more than 50 percent larger. The horizontal channel, which has been the focus of much attention in the social interaction literature, is found to be much less important in our case. Our theory-based estimates allow us to perform a welfare analysis where we gauge cultural attachment strength in monetary units. We find that allowing for a vertical transmission channel provides the same shift to parents' utility as a 3% raise in lifetime income of the child. We also show that a return to an old regulation banning choice of names of foreign origins would cause very important losses to the well-being of parents.

While we have focused on naming decisions, our paper opens new questions on the welfare effects of public policies aiming at promoting or restraining expressions of cultural identity, such as wearing religious signs in public areas. It also raises questions about the use in academic papers of cultural traits as determinants of economic outcomes. It seems clear from our results that at least some aspects of culture cannot be considered as exogenous to what happens in the economic sphere.

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ONLINE APPENDIX
Theoretical appendix

A Local Information Penalty: Theoretical Motivation

In this section, we motivate the the use of the difference in unemployment rate as the measure of penalty at the occupational level. We show how the *difference* enters naturally the objective function of parents maximizing their child's expected utility. To see this, it is useful to consider the *level* of the utility of parents i living in block $k(i)$ in a simplified setting where it depends solely on a cultural taste for vertical transmission and an economic penalty on children (note that in a discrete choice model, only differences in utility over alternatives can be identified from the data, but it is important to examine the utility level to see how the penalty should be modeled.) We omit the time subscript for simplicity.

The parents' utility obtained from choosing an Arabic name for their child is denoted $U_i(1)$ as specified in equation (1). It is the sum of cultural component $C(1)$ and an altruistic component which is their child's expected utility $E(U_i^{\text{child}}(1))$, that is we have:

$$U_i(1) = \underbrace{C(1)}_{\substack{\text{parental utility from} \\ \text{giving an Arabic name}}} + \beta \times \underbrace{\mathbb{E}(U_i^{\text{child}}(1))}_{\substack{\text{expected utility of child} \\ \text{when given an Arabic name}}} + \epsilon_i(1), \tag{A.1}$$

where β account for the fact that the parents might be imperfectly altruistic and discount their child's utility and $\epsilon_i(1)$ is an error term.

We consider the case where an Arabic name affects the probability of being unemployed in a given occupation but not the utility when employed. This assumption is in line with the evidence showing that discrimination affects the employment probability but not the wage of individuals with an immigrant background (Aeberhardt *et al.*, 2010; Rathelot, 2014).[22] We denote $u_l(1)$ the probability of being unemployment in occupation l if given an Arabic name. We can then write $\mathbb{E}(U_i^{\text{child}}(1))$ as:

$$\mathbb{E}(U_i^{\text{child}}(1)) = \underbrace{\sum_l \omega_{l,k(i)} \times u_l(1) \times U_{l,\text{unemp}}}_{\text{child expected utility if unemployed}} + \underbrace{\sum_l \omega_{l,k(i)} \times (1 - u_l(1)) \times U_{l,\text{emp}}}_{\text{child expected utility if employed}},$$

[22]The finding of substantial employment ethnic gap combined with little ethnic wage gap conditional on employment is not specific to the French setting (for an analysis of the Black-White wage gap in the United-States case see Neal and Johnson (1996))

where $U_{l,\text{emp}}$ and $U_{l,\text{unemp}}$ refer to the level of utility when employed and when unemployed respectively and $\omega_{l,k}$ are – like in the body of the paper – the share of neighbors in block k with occupation l. From the point of the view of the parents, what matters for the decision is the *difference* in a child's expected utility between the case where she/he is given Arabic name or not. We can write this difference as:

$$\begin{aligned}\underbrace{\mathbb{E}[\mathcal{C}_{it}]}_{\texttt{Economic Cost}} &\equiv \mathbb{E}(U_i^{\text{child}}(1)) - \mathbb{E}(U_i^{\text{child}}(0)) \\ &= \sum_l \omega_{l,k(i)} \times \underbrace{(u_l(1) - u_l(0))}_{\Delta \text{ in unemp. rate}} \times (U_{l,\text{unemp}} - U_{l,\text{emp}}) \\ &= \sum_l \omega_{l,k(i)} \times \gamma_l \times (U_{l,\text{unemp}} - U_{l,\text{emp}}).\end{aligned}$$

We see that the unemployment penalties in this formulation enter as differences in unemployment probabilities and not as ratios of unemployment probabilities. Note that this result is independent of how utilities in the state of employment and unemployment are specified.

B Welfare Analysis

The absolute level of utility of a discrete choice model exposed in the main draft cannot be identified. Hence, without loss of generality, we can express individual i's welfare in our binary model specified in terms of cross-alternative utility differentials:

$$W_i \equiv \mathbb{E}\{\max[0; \Delta\mathcal{V}_i + \delta_i]\} \tag{B.1}$$

With δ_i logistically distributed and σ its scaling parameter, this expectation becomes (Small and Rosen 1981, Anderson et al. 1992)

$$W_i = \sigma \ln[1 + \exp(\Delta\mathcal{V}_i/\sigma)] + \lambda \tag{B.2}$$

where λ is a term that is constant across individuals and alternatives. Note that both λ and σ cannot be identified from the data and are unknown. This reflects the fact that the absolute value of utility cannot be interpreted in the logit, and that we can only put meaningful numbers on *relative* welfare.

The change in individual welfare between the benchmark condition `B` and a counterfactual condition A1 is given by

$$\Delta W_i^{\mathtt{A1}} = \sigma \times \left(\ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{A1}}/\sigma)\right] - \ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{B}}/\sigma)\right]\right) \tag{B.3}$$

where $\Delta\mathcal{V}_i^{\mathtt{A1}}$ and $\Delta\mathcal{V}_i^{\mathtt{B}}$ correspond to utility differentials (of choosing Arabic vs non-Arabic names) in the benchmark and counterfactual conditions respectively.

The change in aggregate welfare is obtained by averaging (B.3) across the sampled population

$$\Delta\mathcal{W}^{\mathtt{A1}} = \frac{\sigma}{\mathcal{N}}\left(\sum_i \ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{A1}}/\sigma)\right] - \ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{B}}/\sigma)\right]\right), \tag{B.4}$$

which still has no meaningful unit because of the unknown scaling parameter σ. The solution consists in computing the relative change in aggregate welfare between two different counterfactuals A1 and A2

$$\frac{\Delta\mathcal{W}^{\mathtt{A1}}}{\Delta\mathcal{W}^{\mathtt{A2}}} = \frac{\sum_i \left(\ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{A1}}/\sigma)\right] - \ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{B}}/\sigma)\right]\right)}{\sum_i \left(\ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{A2}}/\sigma)\right] - \ln\left[1+\exp(\Delta\mathcal{V}_i^{\mathtt{B}}/\sigma)\right]\right)}. \tag{B.5}$$

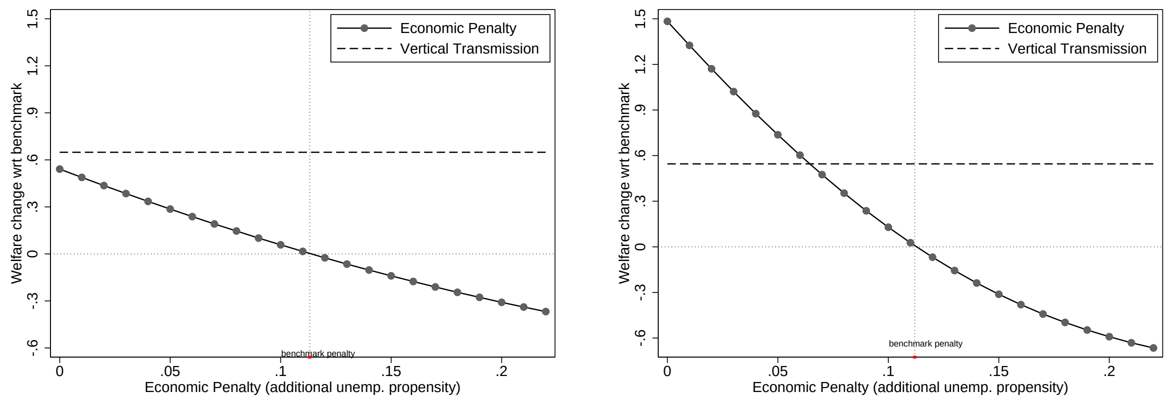
Equation (B.5) involves a double comparison (i.e. a ratio of welfare change) and its interpretation is subtle. For the sake of exposition, in the main text, Table (11) and Figure 1 report, for each counterfactual A, the welfare change $\Delta\mathcal{W}^{\mathtt{A}}$ under the standard scaling assumption $\sigma = 1$. However, when discussing the effects, we pay attention to analyzing only the ratio of welfare change between counterfactuals.

C Welfare effects for different generations of migrants

Figure C.1 - Appendix reproduces the Figure 1 - Main text for two samples of baby naming decisions. The (a) panel takes all babies born from parents living in French public housing themselves being born in an Arabic country. The (b) panel constrains the set of parents to have Arabic origins, but being born on French territory (the coefficients used come from the last two columns of Table (10) from the main draft. The x-axis axis reports the counterfactual economic penalty (percentage point differences in unemployment rates). The y-axis measures welfare changes (with respect to the benchmark level). The horizontal bar represents the utility gain (wrt benchmark) associated with vertical transmission.

Figure C.1 – Welfare in the short run, economic penalty and vertical transmission

(a) 2nd generation babies (b) 3rd generation babies



D Long run effects

D.1 The dynamic version of the model

In this section we assess the long run implications of our structural model on the naming patterns. We run various counterfactual experiments based on our estimates of the naming decision of descendants of migrants from Arabic countries (Column 6 of Table 4).[23] To this purpose we consider a simple dynamic extension of the static model described in Section 3. We restrict our focus to a partial equilibrium analysis where any potential feedback effect of naming patterns on the economic penalty is ignored—obviously an important simplification in a long run perspective.

Let us consider that blocks are populated by a large number of agents $\mathcal{N}$. To keep the model tractable, we consider that agents differ only in their name type. We denote m_t the share of Arabic name holders at date t. Time is discrete. Abstracting from demographic and fertility issues, we impose a constant $\mathcal{N}$ by assuming that just before death each agent gives birth to a unique child whose name is chosen by his parent. Mortality is ruled by a Poisson process with parameter θ. The naming decision follows the model described in Section 3. We denote $(\mathbb{P}_{0,t}, \mathbb{P}_{1,t})$ the probability of giving an Arabic name for, respectively, a non-Arabic parent and an Arabic parent. Those probabilities potentially differ because of the vertical transmission channel. The law of motion of the share of Arabic name holders

[23]Simulations for population of all origins are available upon request from the authors

is given by

$$m_{t+1} = (1-\theta) \times m_t + \theta[(1-m_t) \times \mathbb{P}_{0,t} + m_t \times \mathbb{P}_{1,t}]. \tag{D.1}$$

Labeling μ the steady state value of m_t, we have

$$\mu = (1-\mu) \times \mathbb{P}_0 + \mu \times \mathbb{P}_1, \tag{D.2}$$

where the steady state probabilities of transmission, $(\mathbb{P}_0, \mathbb{P}_1)$, are characterized by equation (6). Those can be conveniently rewritten as

$$\mathbb{P}_A = [1 + \tanh(\Delta\mathcal{V}_A/2\sigma)]/2, \tag{D.3}$$

where $\tanh(x) \equiv (e^x - e^{-x})/(e^x + e^{-x})$ and $\Delta\mathcal{V}_A$ is the observable utility differential (with $A \in \{0,1\}$). In $\Delta\mathcal{V}_A/\sigma$, the steady state value of the horizontal component is equal to $\mathbb{E}(m_t) = \mu$ and the parameters $(\hat{\alpha}_0, \hat{\alpha}_1, \hat{\alpha}_2, \hat{\alpha}_3)$ correspond to the point estimates retrieved from our empirical analysis. We thus have

$$\Delta\mathcal{V}_A/\sigma = \hat{\alpha}_0 + \hat{\alpha}_1 A + \hat{\alpha}_2\mu + \hat{\alpha}_3\mathcal{C} \text{ with } A \in \{0,1\}, \tag{D.4}$$

where $\mathcal{C}$ corresponds to an exogenous economic penalty attached to Arabic name holders.

Combining (D.2), (D.3) and (D.4), we obtain μ as a solution to the following fixed point equation

$$\mu = \frac{1}{2} + \frac{1-\mu}{2} \times \tanh\left(\frac{\hat{\alpha}_0 + \hat{\alpha}_2\mu + \hat{\alpha}_3\mathcal{C}}{2}\right) + \frac{\mu}{2} \times \tanh\left(\frac{\hat{\alpha}_0 + \hat{\alpha}_1 + \hat{\alpha}_2\mu + \hat{\alpha}_3\mathcal{C}}{2}\right), \tag{D.5}$$

A first noticeable point is that this equation does not depend on the value of the Poisson parameter θ. This makes us confident in the innocuity of our dynamic, albeit simple, demographic structure as long as we focus our analysis strictly on the steady-state, abstracting from any consideration on the transition dynamics. Second, while existence of μ follows directly from the Brouwer fixed-point theorem, uniqueness is not guaranteed and the previous equation may have multiple solutions. Contrary to (Brock and Durlauf, 2001, proposition 2), our dynamic setting with a non-homogenous population of agents forbids us to simply characterize the presence of multiplicity as a function of parameter values.[24] We consequently rely on numerical computations of (D.5) to characterize the set of solutions.

D.2 Long run steady states and counterfactual experiments

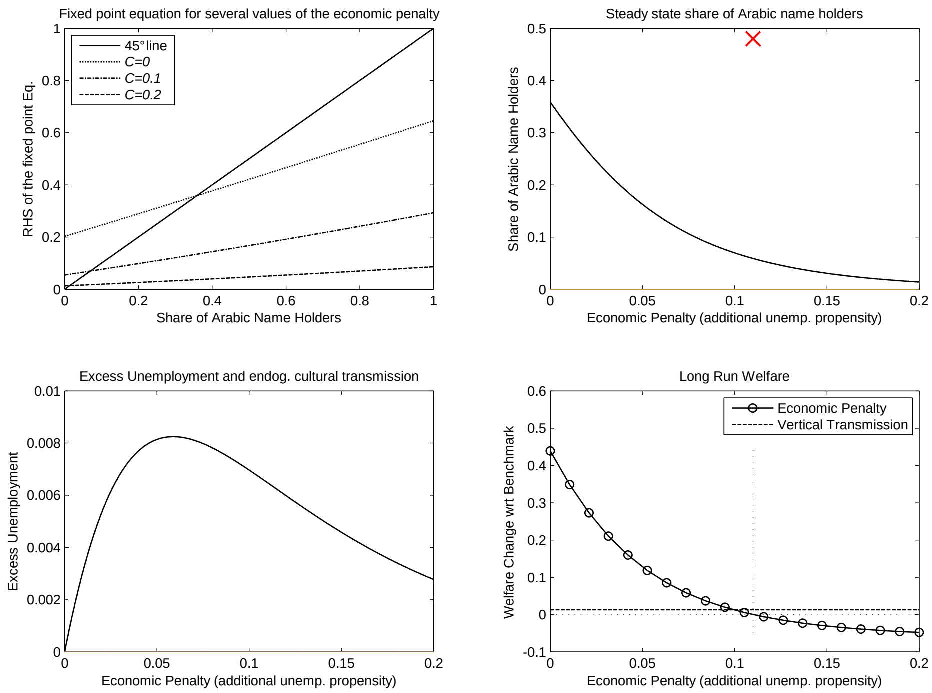
The parameters $(\hat{\alpha}_0, \hat{\alpha}_1, \hat{\alpha}_2, \hat{\alpha}_3)$ correspond to the point estimates of a specification similar to our benchmark where the sample is restricted to descendants from Arabic migrants in public housing (Column 6, Table 4). A difference is that we remove all added covariates and fixed-effects which are not directly related to the three channels of our model in equation (5). The resulting values for our parameters are $\hat{\alpha}_0 = -1.37, \hat{\alpha}_1 = 1.8, \hat{\alpha}_2 = 0.17$, and $\hat{\alpha}_3 = -14.8$. We solve numerically the fixed-point equation (D.5) for values of $\mathcal{C}$ spanning the range $[0, 0.2]$; for each value of $\mathcal{C}$ this gives us the steady-state share of Arabic name holders $\mu(\mathcal{C})$. We then compute our second variable of interest, $\mathcal{U}$, which corresponds to the steady-state value of excess-unemployment due to discrimination toward Arabic name holders

$$\mathcal{U}(\mathcal{C}) \equiv \mu(\mathcal{C}) \times \mathcal{C}, \tag{D.6}$$

The results are displayed on figure D.1. The upper left panel depicts the fixed-point equation (D.5) for various values of economic penalty $\mathcal{C}$ within the range of 0 to 20 percent. We can check visually in this panel that the equilibrium is unique confirming that the horizontal channel is not large enough to generate multiple social equilibria. The upper right panel reports the steady-state value of $\mu(\mathcal{C})$. The red cross represents the actual values of the unemployment penalty (equal to 0.11) and the actual share of Arabic name holders (equal to 0.48) observed in our sample of first and second generations of migrants from Arabic countries living in public housing over the 2003-2007 period. We observe that for $\mathcal{C} = 0.11$, the steady-state share of Arabic name-holders predicted by our structural model is $\mu = 0.086$ which is much smaller than the actual one. This feature might be explained by the fact that migration from Arabic countries is still a quite recent phenomenon in France and that most babies born in the 2003-2007 period belong to the third generation of migrants only. Hence the actual share is still far from its steady-state value and transitory dynamics are expected to bring it down in the future. We also see that in absence of discrimination ($\mathcal{C} = 0$), this steady-state share is predicted to be much larger, at 0.36.

[24] Without vertical transmission, i.e. with $\hat{\alpha}_1 = 0$, our model would be included in the class of model analyzed by Brock and Durlauf (2001). Indeed, in that case, the population of babies has homogenous characteristics with respect to the naming process and our equation (D.5) is equivalent to their main equation (12).

Figure D.1 – Long-Run Effects - Descendants from Arabic Migrants in Public Housing



This confirms that the economic cost channel is also a key driver of cultural transmission in the long run.

On the bottom left panel we report the predicted excess-unemployment $\mathcal{U}(\mathcal{C})$. We observe a non-monotonous relationship. This shows how a change in the degree of penalty (potentially resulting from public policy) may be partially counteracted by endogenous naming choices. Indeed, when the perceived penalty intensity ($\mathcal{C}$) falls, parents tend to raise their propensity to give Arabic names, everything else equal. This counteracting effect results in an ambiguous effect on the overall level of discrimination in the economy (total number of unemployed Arabic name holders because of the estimated penalty). Our simulation shows the interesting result that the overall discrimination starts by rising when the underlying penalty (the intensity of discrimination) decreases from a high initial level.

The bottom right panel replicates, for the long-run, the welfare analysis of figure 3. Considering the same benchmark and counterfactual conditions, we compute, for each, the steady-state value of aggregate welfare, taking into account the full dynamic impact of the counterfactual change in parameters. As explained in the main text (Section 6.2), defining an aggregate welfare criterion in an inter-generational model of cultural transmission is problematic because of imperfect altruism. We follow a simplistic approach here. It consists in assessing the long-run outcome from the welfare perspective of the first-generation of parents who experience the counterfactual change in the economic penalty. Consequently, the magnitude of the reported effects should be considered an upper bound of long-run welfare effects. With respect to the short run, we see that the welfare impact of the vertical channel experiences a fivefold reduction: This stems from the long-run predicted fall of the share of Arabic name holders in the population (of comparable magnitude), which are the only individuals for which the vertical channel is relevant. The vertical transmission motive is now equivalent in terms of welfare gains to a cut by 1.2 percentage points of the penalty, namely a 0.6% upward shift in lifetime income of one's child. Naturally, the same figure calculated on the subsample of the population that keeps an Arabic name in the long run (8.6% of the considered population) would be much higher, at 6.9% of lifetime income.

Empirical appendix

E Additional empirical results

E.1 Robustness to use of *relative* of penalty.

We assess the robustness of our results to the use of unemployment penalties that are measured in relative terms as opposed to absolute differences as in the main analysis of the paper.

First, in order to ensure that our baseline results based on difference in the absolute value of unemployment rate penalties (reproduced in columns 1 and 4 of Table E.1) is not driven by variation in unemployment across occupations among non-Arabic name holders, we introduce the average occupation-specific unemployment rate among non-Arabic name holders within each block as a control . We see in columns (2) and (4) of the same table that the negative effect of the LIP based on absolute occupational penalties is not affected by the introduction of this control. We see this robustness test as attenuating the concern that our results could be entirely driven by the baseline level of unemployment in neighbors' occupations.

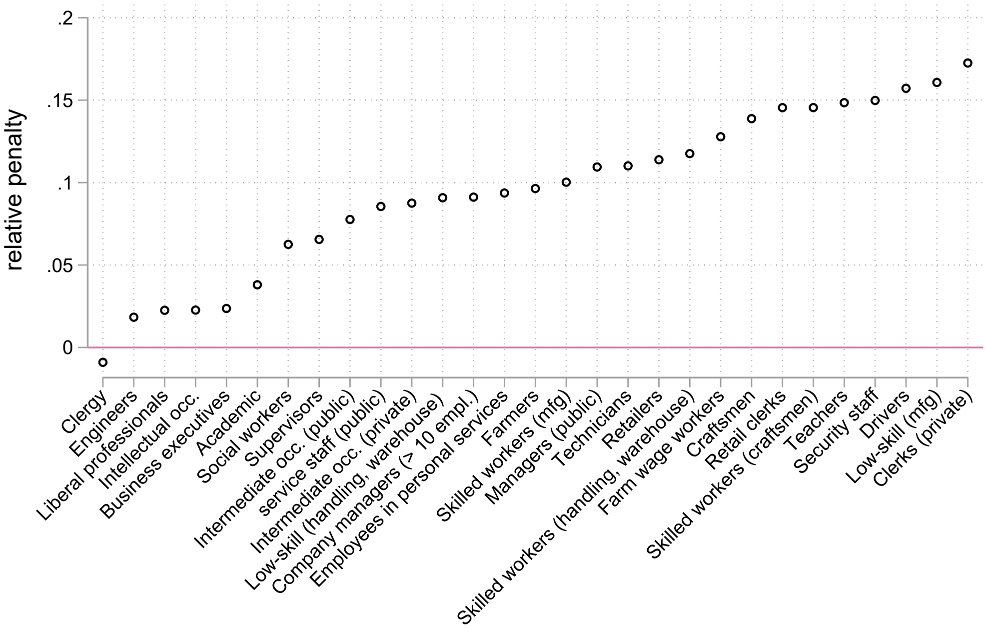
Second, we run the analysis with unemployment rate differential measured in relative terms ($\hat{\gamma}_l = (u_l^{\mathbf{a}} - u_l^{\mathbf{na}})/u_l^{\mathbf{na}})$). An issue with the proposed measure based on the relative penalty is that it is very volatile because measured unemployment in some occupations is very small among non-Arabic name holders—in particular for farming where it is very low (0.26%) in comparison with Arabic name holders (9.9%). Note however that the number of Arabic name holders in that profession is very low. While this gap is not very influential to our baseline measure in levels it tends to affect the ratio despite the low number of farmers among neighbors (around 0.5%). To diminish the role of outliers in determining the value of the LIP based on relative penalties, we "winsorize" the LIP variables at the 5th and 95th percentiles. Results are presented in Table E.2. They do show that the LIP based on *relative* penalties has a negative effect on the probability of giving Arabic first names (columns 2 and 5). We notice that winsorizing the LIP based on *absolute* penalties barely affects the baseline estimates (columns 1 and 4).

As mentioned above, after some investigation into the reasons for this excessive dispersion of the relative LIP measure, we found that farmers (a very small percentage—less than 0.5%— of the sample) have an unusually high relative penalty ratio (the penalty differences and ratios across occupations that we use in the construction of the LIP variable are provided in figure E.1). As a result, the very small gap among farmers between Arabic and non-Arabic name holders is inflated through a division by (nearly) zero. Excluding farmers from the computation of the LIP leads to results that are very to the winsorization as can be seen in columns 3 and 6 of Table E.2.

While the coefficients of columns (2)-(3) and (5)-(6) are much smaller than in our baseline estimation (1) and (4), the economic implication of LIP's effect of is very close. Using the interquartile range of the LIP variable provided in the last row of the table, we can calculate the effect of going from the bottom to the top quartile of the LIP variable of our sample. This reduces the probability of giving an Arabic name by $.86 \times .02 = 1.72$ percentage points in column (1) and by $.03 \times .57 = 1.71$ percentage points in columns (2) and (3). In the social housing sample of the last 3 columns, the baseline reduction amounts to $3.35 \times .02 = 6.7$ percentage points in columns (4), while it is 2.75 and 3.24 percentage points in columns (5) and (6).

Figure E.1 – Distribution of the penalty in *levels* (top) and penalty in *relative* terms (bottom) across detailed occupations.

(a) Penalty in levels: $\gamma_l = u_l^{\mathrm{a}} - u_l^{\mathrm{na}}$



(b) Relative penalty: $\gamma_l = u_l^{\mathrm{a}}/u_l^{\mathrm{na}} - 1$

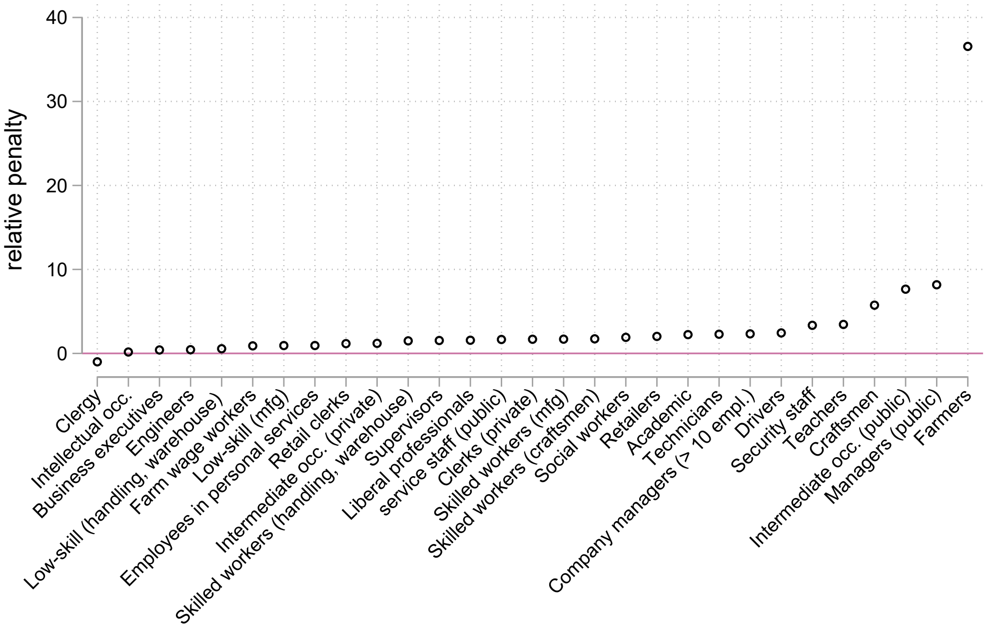


Table E.1 – U[Arabic]-U[non Arabic] and controlling for U[non Arabic]

	(1) arabic	(2) arabic	(3) arabic	(4) arabic
one parent/grandp. w/ Muslim country nat.	0.09^a	0.09^a		
	(0.01)	(0.01)		
one parent with Arabic name	0.23^a	0.23^a	0.36^a	0.36^a
	(0.01)	(0.01)	(0.04)	(0.04)
share of Arabic name in block (aged 4-10)	0.09^a	0.09^a	0.03	0.04
	(0.02)	(0.02)	(0.05)	(0.05)
local information on penalty	-0.86^a	-0.89^a	-2.95^a	-3.18^a
	(0.33)	(0.33)	(1.07)	(1.09)
UR in nghbors' occ. among N-A name holders ($\sum_l \omega_{l,k} U(0)_l$)		-0.07		-0.38
		(0.12)		(0.34)
Observations	3829	3829	992	992
Pseudo R^2	0.399	0.399	0.160	0.161
Mean probability	0.19	0.19	0.50	0.50
Parent/grandp. w/ Muslim country nat. only			✓	✓

[1] Notes: Logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.

Table E.2 – Relative Penalty: U[Arabic]-U[non-Arabic])/U[non-Arabic]

	(1) level win.	(2) rel. win.	(3) no farm	(4) level win.	(5) rel. win.	(6) no farm
one parent/grandp. w/ Muslim country nat.	0.09^a	0.09^a	0.09^a			
	(0.01)	(0.01)	(0.01)			
one parent with Arabic name	0.23^a	0.23^a	0.23^a	0.36^a	0.36^a	0.36^a
	(0.01)	(0.01)	(0.01)	(0.04)	(0.04)	(0.04)
share of Arabic name in block (aged 4-10)	0.09^a	0.09^a	0.09^a	0.03	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.05)
Local information on penalty (LIP) – winsorized	-0.86^a			-3.35^a		
	(0.33)			(1.11)		
LIP based on relative pen. $\left(\sum_l \omega_{l,k} \frac{U(1)_l - U(0)_l}{U(0)_l}\right)$		-0.03^a	-0.03^a		-0.05^c	-0.06^b
		(0.01)	(0.01)		(0.03)	(0.03)
Observations	3829	3829	3829	992	992	992
Pseudo R^2	0.399	0.400	0.401	0.161	0.157	0.159
Mean probability	0.19	0.19	0.19	0.50	0.50	0.50
Parent/grandp. w/ Muslim country nat. only				$\surd$	$\surd$	$\surd$
Interquartile range in measure of LIP	0.02	0.57	0.51	0.02	0.55	0.54

[1] Notes: Logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with [a], [b] and [c] respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.The winsorization is set at the 5th and 95th percentile for both the baseline LIP (columns 1 and 4) and the LIP based on relative penalties (columns 2 and 5). Percentiles are computed based on the HLM sample. The correlation between the two measures is 0.37. In columns (3) and (6), the measure of LIP based on relative penalties exclude farmers. The unemployment rate for non Arabic name holders is very low (3.8%) while it is relatively high for Arabic names (31%), thus resulting in an outlier. Results are similar than when using winsorization.

E.2 Omitting the horizontal channel

Table E.3 – Omitting measures of the horizontal channel does not affect the estimated effect of the economic channel

	(1)	(2)	(3)	(4)
	Arabic name for baby			
one parent/grandp. w/ Muslim country nat.	0.09^a	0.10^a		
	(0.01)	(0.01)		
one parent with Arabic name	0.23^a	0.24^a	0.36^a	0.35^a
	(0.01)	(0.01)	(0.04)	(0.04)
share of Arabic name in block (aged 4-10)	0.09^a		0.03	
	(0.02)		(0.05)	
local information on penalty	-0.86^a	-0.90^a	-2.95^a	-2.98^a
	(0.33)	(0.32)	(1.07)	(1.03)
Observations	3829	3958	992	1020
Pseudo R^2	0.399	0.397	0.160	0.157
Mean probability	0.19	0.19	0.50	0.51
Parent/grandp. w/ Muslim country nat. only			√	√

[1] Notes: Logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.

E.3 Other robustness checks and additional results

Table E.4 – Naming patterns among all and among Arabic households in the block

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Arabic name for baby							
1. one parent/grandp. w/ Muslim country nat.	0.09^a	0.12^a	0.12^a	0.12^a				
	(0.01)	(0.02)	(0.02)	(0.02)				
2. one parent with Arabic name	0.23^a	0.31^a	0.31^a	0.31^a	0.36^a	0.37^a	0.37^a	0.37^a
	(0.01)	(0.02)	(0.02)	(0.02)	(0.04)	(0.05)	(0.05)	(0.05)
3. local information on penalty	-0.86^a	-1.99^a	-2.11^a	-1.98^a	-2.95^a	-4.16^a	-4.20^a	-4.20^a
	(0.33)	(0.60)	(0.59)	(0.59)	(1.07)	(1.31)	(1.31)	(1.32)
4. share of Arabic name in block (aged 4-10)	0.09^a	0.10^a		0.10^b	0.03	0.03		0.01
	(0.02)	(0.03)		(0.04)	(0.05)	(0.07)		(0.09)
5. – (as above) among Arabic-named parents			0.05^b	-0.00			0.03	0.02
			(0.02)	(0.03)			(0.05)	(0.07)
Observations	3829	1988	1988	1988	992	741	741	741
Pseudo R^2	0.399	0.371	0.369	0.371	0.160	0.163	0.163	0.163
Mean probability	0.19	.2	.28	.28	0.50	.52	.55	.55
SD horizont. channel	.24	.25	.38	.	.28	.26	.35	.
Parent/grandp. w/ Muslim country nat. only					√	√	√	√

[1] Notes: Logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.This table assesses whether the horizontal channel differs when it is measured by the share of Arabic names in block (aged 4-10) among all households – as in the main analysis, see variable whose marginal effect is presented in line 4 – or when it is measured as the same share among parents with Arabic names – line 5. Column (1) produces the baseline analysis on the public housing sample (identical to column (1) of Table 4). Column (2) presents the same specification estimated on the subset of blocks for which the share of children 4 to 10 with Arabic name among children born to parents with Arabic names is well defined. Column (3) replaces the baseline measure with the share among Arabic-named parents. Column (4) includes both variables. Columns (5) to (8) reproduce the analysis focusing on 2nd and 3rd generation babies. The line "SD horizont. channel" refers to the standard deviation of the variable capturing the horizontal channel. It is not well defined when two variables are used to measure this channel (columns 4 and 8).

Table E.5 – Decomposition of the vertical channel: Differentiated effect depending on whether only the mother, only the father or both parents have an Arabic name – public housing sample

	(1)	(2)	(3)	(4)
	Arabic name for baby			
one parent/grandp. w/ Muslim country nat.	0.09^a	0.08^a		
	(0.01)	(0.02)		
one parent with Arabic name	0.23^a		0.36^a	
	(0.01)		(0.04)	
Mother has Arabic name only		0.19^a		0.27^a
		(0.02)		(0.05)
Father has Arabic name only		0.22^a		0.29^a
		(0.02)		(0.05)
Both parent with Arabic name		0.26^a		0.42^a
		(0.01)		(0.04)
share of Arabic name in block (aged 4-10)	0.09^a	0.09^a	0.03	0.02
	(0.02)	(0.02)	(0.05)	(0.05)
local information on penalty	-0.86^a	-0.83^b	-2.95^a	-2.71^a
	(0.33)	(0.32)	(1.07)	(1.05)
Observations	3829	3829	992	992
Pseudo R^2	0.399	0.405	0.160	0.173
Mean probability	0.19	0.19	0.50	0.50
Parent/grandp. w/ Muslim country nat. only			√	√

[1] Notes: Logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.

Table E.6 – Heterogeneity within public housing depending on immigration background.

	(1) All PH	(2) No Imm. Muslim	(3) Imm. Musl.	(4) Native French	(5) Residual
one parent/grandp. w/ Muslim country nat.	0.09^a				
	(0.01)				
one parent with Arabic name	0.23^a	0.16^a	0.36^a	0.17^a	0.20^a
	(0.01)	(0.01)	(0.04)	(0.03)	(0.01)
share of Arabic name in block (aged 4-10)	0.09^a	0.12^a	0.03	0.13^a	0.16^a
	(0.02)	(0.02)	(0.05)	(0.04)	(0.03)
local information on penalty	-0.86^a	-0.01	-2.95^a	-0.27	0.60
	(0.33)	(0.32)	(1.07)	(0.60)	(0.57)
Observations	3829	2517	992	908	1052
Pseudo R^2	0.399	0.387	0.160	0.286	0.511
Mean probability	0.18	0.07	0.25	0.09	0.10
Mean prob. Parent Arabic Name	0.27	0.09	0.77	0.05	0.15
Parent/grandp. w/ Muslim country nat.			Yes		
2nd/3rd gen. babies	Yes — No	No	Yes	No	Yes
1st/2nd gen. parents	Yes — No	No	Yes	No	Yes

[1] Notes: Logit estimates (average marginal effects). Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%. All regressions include dummies for parental education level, parental occupation group, *département* of residence, and years.In column (3), immigrant from a Muslim country is defined as having one parent or grand parent from our list of Muslim majority country (see Section 4.1). Native French is defined as having no Muslim immigration background and at least one parent of French nationality. The Residual category in column (5) refers to households not contained in the sub-samples of columns (3) or (4) which contains individuals with some immigration background but not from a country included in our list of majority Muslim. The table shows that the average effect estimated on the entire sample of public housing tenants (column 1) is entirely driven by the response of households with some immigration background from a Muslim/Arabic country.

F Unemployment penalty associated with Arabic names

The goal of this section is to document further the unemployment penalty attached to Arabic first names across occupations. As explained in the main text, our goal is not to identify perfectly the discrimination associated with an Arabic name. Therefore we keep our estimation method voluntarily simple. It is however interesting to note that our estimates of the penalty attached to Arabic first names are very much in line with the existing findings based on more elaborate econometric methods.[25].

Table F.7 displays the *unconditional* unemployment rate associated with the 10 most popular non-Arabic and Arabic names. The data derives from the population aged 25-55 in the LFS 2003-2007. The unemployment rate of men with popular Arabic names is between four to eight times as high as the unemployment rate of men with popular non-Arabic names. A striking example is given by men named Abdelkader, whose unemployment rate reaches 37 percent against 5 percent for individuals named Philippe.[26] The unemployment gap is even more pronounced among the female population. Women named Fatma (ranked 6th in the list of the most popular Arabic names for women) have an average unemployment rate of 42 percent, against 10 percent for women named Sandrine or Patricia (who have the highest unemployment rate among the most popular non-Arabic names). The cross-name heterogeneity is also much larger for Arabic names but this is probably driven by small sample issues.

Table F.8 documents the *conditional* unemployment penalty by running a standard Mincer-type equation estimated on the LFS subsample of active persons aged between 25 and 55. The left-hand-side variable is the employment status, equal to 1 if the respondent is employed, and 0 if unemployed. We consider the set of standard controls, including

[25] In particular, Duguet *et al.* (2010) use pair auditing to test access to job interviews of individuals who share the same characteristics, except Arabic and non-Arabic names. They find that the probability to get interviewed is 7 percentage points lower for Arabic name holders in the French labor market, which is really close to our results. Adida *et al.* (2010) isolate the source of discrimination by identifying the effect of being Muslim on the French labor market. Using a large-scale survey on immigrants from Senegal, they are able to identify typical first names from the Muslim and the Christian parts of this population, which they report to be otherwise quite similar on all measurable aspects. The authors then ran an audit survey with CVs identical in all dimensions, but with a different type of first name. The CVs would in particular have the same family name, for instance Diouf, a typical Senegalese family name, but one CV would have a typical Muslim first name (e.g. Khadija for women) and the other a well-known Catholic first-name (e.g. Marie). Adida *et al.* (2010) find a statistically significant difference of 13 percentage points in the response's rates to job applications between the holders of Catholic first names and those with Muslim first names.

[26] Interestingly, Emir Abdelkader was the military leader who led the struggle against the French colonial invasion of Algeria in the mid-19th century (we thank Nour Meddahi for pointing that out during a presentation of this paper).

Table F.7 – Unemployment rate by name

	Name type			
Rank of name	Non-Arabic		Arabic	
1	Philippe	0.05	Mohamed	0.19
2	Alain	0.05	Said	0.30
3	Christophe	0.07	Rachid	0.20
4	Frederic	0.07	Ali	0.20
5	Patrick	0.07	Abdelkader	0.37
6	Michel	0.05	Karim	0.17
7	Thierry	0.05	Ahmed	0.18
8	Pascal	0.05	Mustapha	0.18
9	Laurent	0.06	Kamel	0.20
10	Stephane	0.08	Farid	0.29
1	Nathalie	0.08	Nadia	0.18
2	Sylvie	0.07	Fatima	0.26
3	Isabelle	0.08	Malika	0.24
4	Catherine	0.07	Aicha	0.26
5	Christine	0.06	Naima	0.15
6	Martine	0.08	Fatma	0.42
7	Valerie	0.08	Khadija	0.17
8	Sandrine	0.10	Rachida	0.37
9	Veronique	0.08	Samira	0.31
10	Patricia	0.10	Yamina	0.36

Notes: The sample covers the 4 years of employment survey we have access to (2003-2007). The statistics are for adults between 25 and 55 years old.

nationality at birth of the respondent and parent's respondent, individual characteristics (age, age squared, gender, marital status and number of children), educational, occupational, spatial and year fixed effects. Our variable of interest is *Arabic name*, a binary variable coding for a first name from Arabic origins. Column (1) reports the unemployment penalty associated with an Arabic name, without controlling for the nationality at birth of the respondent and of the parents' respondent. Holding an Arabic name decreases the probability to be employed by 10 percentage points and the effect is statistically significant at the 1 percent level. However, most of Arabic name holders being first or second generation migrants, the previous correlation captures both the discriminating impact on the labor market of foreign origins and of foreign names; while closely related, the latter dimension is manipulable by parents but the former is not. To isolate the specific penalty from a name that sounds culturally distinctive, we also control for other attributes of the country of origin. Column (2) includes a dummy variable equal to 1 if the nationality at birth of the respondent or of the parents' respondent is from an Arabic country, and 0 otherwise. The estimated unemployment penalty associated with an Arabic name remains fairly high at 7 percentage points and remains highly statistically significant. The estimated unemployment penalty associated with an Arabic name is of the same order of magnitude as the one associated with having an Arabic-related nationality, suggesting that a specific employment penalty is attached to the first name.[27] In the next two columns, the sample is restricted to individuals living in the public housing sector (on which our main econometric analysis will be based). Column (3) shows that the conditional unemployment penalty is identical for this subsample and is robust to the inclusion, in Column (4), of a variable coding for the number of children *with* Arabic name in the household. This last variable is likely to be correlated to a bundle of *unobservable* characteristics related to the degree of individual attachment to Arabic culture (e.g. religiosity) that may simultaneously influence the penalty. Column (5) estimates the conditional unemployment penalty for each broad occupational category. The reference category is executives.

How large is the implied loss in lifetime expected income associated with an Arabic name? A simple "back of the envelope" calculation suggests that it is substantial. Breuil-Genier (2001) provides detailed estimates of income variations induced by a transition from employment to unemployment on the French labor market (including in particular social benefits, that we do not observe in the LFS). She finds an average income loss of 50 percent. From Table F.8 - Column (2), we know that, in every period, the conditional unemployment gap of Arabic name holders is 7 percentage points relative to non-Arabic name holder. This means that the total income loss of typical Arabic name holder during his/her active life is $0.07 \times 0.5 = 3.5\%$ of expected income. Since the average participation to the active population is 39 years in France, this is equivalent to $39 \times 0.035 = 1.365$ years, i.e. around 16 months of income.

[27] This might reflect the absence of clear morphological markers of ethnicity (e.g. skin color, size) for individuals with Arabic origins, living in France. Thus the first name conveys meaningful information on ethnic background.

Table F.8 – The penalty of an Arabic name

	(1)	(2)	(3)	(4)	(5)
Dep.Var:		emp. / unemp. status			
Arabic name	-0.10^a	-0.07^a	-0.07^a	-0.06^a	0.00
man	0.01^a	0.01^a	0.03^a	0.03^a	0.01^a
age	0.01^a	0.01^a	0.01^a	0.01^a	0.01^a
age squared	-0.00^a	-0.00^a	-0.00^a	-0.00^a	-0.00^a
nationality from Maghreb/Middle-East		-0.06^a	-0.07^a	-0.06^a	-0.06^a
count of kids with Arabic name				-0.01^b	
Arabic × intermediate					-0.06^a
Arabic × clerk					-0.08^a
Arabic × blue collar (skilled)					-0.09^a
Arabic × blue collar (unskilled)					-0.09^a
Arabic × craftsman					-0.08^a
Arabic × farmer					-0.04
Observations	148582	148582	90693	90693	148582
R^2	0.041	0.042	0.047	0.047	0.049

Note: Column (5) has executives as the baseline occupation group. All regressions include dummies for education level, occupation group, *département* of residence, years, as well as number of children, and marital status. The sample includes active persons aged between 25 and 55. The unconditional unemployment rate in this sample is 8%. Standard errors are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%

Institutional context

G Public Housing

G.1 Formal Allocation Process in Public Housings

Due to a strong "Republican ideal", the French public housing system allocates state-planned moderate cost rental apartments (HLMs - *Habitations à Loyer Modéré*) to natives and immigrants without concern for their cultural and ethnic background, mixing people indiscriminately. The allocation process across public housing blocks is mainly inspired by theories from the famous architect Le Corbusier (1887-1965). Le Corbusier insisted that France must avoid the homogeneous ghettoes of the urban landscapes elsewhere, and should therefore allocate housing blind to ethnicity, not permitting family networks to grow within housing establishments. These ideas were later translated into state regulation (Bernardot, 2008). This Appendix documents the legal framework for the residential allocation in the public housing sector. We show that the exogeneity of the allocation with respect to salient characteristics, such as ethnicity, is built into the law.

We first describe the eligibility criteria and the formal selection process. The only eligibility requirements for admittance into the public housing sector are to be a legal resident of France (as a French citizen or migrant with a valid residence permit) and to be live under a certain threshold of income per unit of consumption. This income ceiling is usually rather high: in 2009, this threshold was between 36,748 and 50,999 Euros per year for a four-person family, depending on the region of residence. As a consequence, the population eligible for public housing is on average three to four times as large as the available space in vacant dwellings. However, the situation is even tighter in the most crowded areas, such as Paris. According to the *Observatoire du Logement et de l'Habitat de Paris* (2011), as of January 2010, there were 186,017 public housing dwellings in Paris. Public housing buildings are scattered across all Parisian areas, with a high concentration (69 percent) in six districts (the 13th, 14th, 15th, 18th, 19th, and 20th *arrondissements*). Within Paris, 48.7 percent of households are under the income ceiling and could, theoretically, be eligible. In practice, only households with very modest incomes apply (71 percent have an income lower than the minimum ceiling for all France, equivalent to 2345 euros per month for a household with two children). On the 31st of December 2010, there were 121,937 ongoing applications, to be compared to 12500 public housing units allocated over the year 2010. Due to those stringent housing supply constraints, other eligibility criteria are taken into account.[28] In addition to household income, the family situation, and household size are taken into account to ensure a suitable match with the characteristics of vacant dwellings, as well as the emergency of the application. Those latter criteria have recently become the main criteria the commission uses due to the boom in housing prices in the private sector during the mid-90s and the 2000s. In particular, five priority criteria are defined by law (Article L441-1 of law relative to construction and housing - *Code pour la Construction et l'Habitat*) at the national level to ensure that vacant housing will first be distributed to households with obvious social difficulties. Households satisfying these priority criteria are those in which there is a (mentally or physically) disabled person, those living in precarious or hazardous shelter due to financial constraints, those living in a temporary accommodation, individuals living in a precarious shelter who recently found a job after a long unemployment spell, and victims of domestic violence.

Regarding the selection process, the commissions of selection in charge of allocating households to vacant public housing dwellings are held at the *département* level (or at the city level in the case of Paris which is both a city and a *département* due to its size). The commissions' composition is regulated by law: it includes six members of the public housing offices board, a representative of associations for social and economic insertion (appointed by the head of the *département*), mayors of the cities (or districts) in which vacant housings are to be attributed, as well as a representative of any association defending tenant rights. In addition, another *département* representative may attend the commission meeting. For each vacant housing unit, at least three households must be considered by the commissioners, who finally decide which household will be allocated to which housing unit, according to the eligibility and priority criteria detailed above. Other criteria such as the number of children in the household are also taken into account in order to allocate suitable dwellings.

Despite this legal allocation process, one might still be worried about the possibility of self-sorting of households that refuse the residential allocation proposed by the commission.

[28]To apply for an apartment in the public housing sector, one has to submit a form showing one's identity, family situation, and employment status as well as the resources of one's household; the reasons for applying to the public housing sector (e.g. currently or soon to be homeless, or reasons related one's health situation, family situation, job situation, and inappropriate current housing or unpleasant environment); the type of housing that is being sought, and whether the applicant is disabled and whether it is the first application. It is important to stress the fact that the application form contains very limited information about the ethnicity of the applicant: he or she only needs to state his or her nationality, which is limited to three possible categories (French, European Union, or non European Union).

In theory, households can refuse up to three offers. However, self-sorting, especially on ethnic characteristics, seems unlikely to be a common practice. Residential mobility within the public housing sector is very low, due to the current strong shortage of supply of public housing dwellings. This makes it unlikely that the selected households could be really picky about neighborhood diversity (see the study by Simon, 2003). In addition, rents are considerably lower in public housing than in private housing, increasing the opportunity cost of moving, so that turnover is very low. More specifically, the mobility rate in the public housing sector is even lower than for recent owners. Public housing allocation in Paris serves as a useful concrete example. The mobility rate (defined as the ratio of new entrants over the total number of public housing dwellings) is particularly low: it reaches 5.5 percent in 2010. It is formally possible to indicate a precise neighborhood in the application form, but in practice, very few applicants (6.6 percent) do provide this information. More than half of the 121,937 applicants (52.9 percent) did mentioned no particular area at all, probably due to the fear of being rejected on this ground. Among those who indicated an area of preference, 91.2 percent mentioned the area where they were already living. People who move within the public housing sector are people who moved for larger space following an increase in their household size (only 12 percent of the public housing dwellings have more than three rooms).

G.2 Descriptive statistics on public housing

Table G.1 reports the descriptive statistics of the sample of individuals over 15 years old and who are consequently interviewed in the LFS. There is an over-representation of Arabic name holders in public housings: the share reaches 14 percent in this type of housing against 4 percent in the total sample. Similarly, the proportion of individuals who have Arabic origins is 16.8 percent in public housings. This proportion is three times as high as in the total sample (5.61 percent). Individuals who enter the public housing sector have also lower socio-economic backgrounds than the rest of the population. The share of unemployed is almost twice as high in public housings (9.6 percent) than in the total sample (5 percent). There is an over-representation of blue collar workers and clerks (78 percent in the public housing, and 55 percent in the whole sample) and an under-representation of executives (3 percent in public housing, 12 percent in the whole sample). This table shows that there is a clear selection of individuals into public housings, since eligibility is based on the socio-economic characteristics. However, our key identification strategy relies on the exogeneous spatial allocation of individuals within the public housing sector.

Table G.1 – Descriptive statistics of individuals

	Total sample Mean (std)	Public housing Mean (std)
Age	45.22 (19.22)	40.93 (18.19)
Gender (Male)	0.48 (0.49)	0.46 (0.49)
Married	0.61 (0.48)	0.49 (0.50)
Arabic names	0.04 (0.21)	0.14 (0.35)
Arabic origins	0.05 (0.23)	0.16 (0.37)
Employed	0.52 (0.49)	0.50 (0.49)
Unemployed	0.05 (0.20)	0.09 (0.27)
Inactive	0.43 (0.49)	0.41 (0.49)
Hourly wage (euros)	9.70 (4.29)	8.24 (3.00)
Occupation: executive	0.12 (0.33)	0.03 (0.18)
Occupation: intermediate	0.20 (0.40)	0.14 (0.34)
Occupation: clerk	0.30 (0.46)	0.40 (0.49)
Occupation: blue collar (skilled)	0.15 (0.36)	0.20 (0.40)
Occupation: blue collar (unskilled)	0.10 (0.30)	0.18 (0.39)
Occupation: craftman	0.06 (0.24)	0.02 (0.14)
No education	0.21 (0.41)	0.36 (0.48)
Elementary school	0.44 (0.49)	0.42 (0.49)
High school	0.14 (0.34)	0.11 (0.31)
College	0.09 (0.28)	0.05 (0.22)
Graduate	0.10 (0.30)	0.04 (0.20)

G.3 Exogenous spatial residential allocation in public housing

We now proceed to more formal statistical tests for the exogenous spatial allocation of households across public housing residential blocks within a *département*.

In their analysis of the impact of fractionalization on local public goods, Algan et al. (2016) have already provided an extensive list of tests showing the absence of self-sorting along ethnic lines across public housing dwellings. We briefly summarize those tests here. First, they run placebo tests on whether, at the block level, diversity correlates with measures of the distribution of exogenous public housing buildings characteristics. They showed that block-level ethnic diversity does not correlate with any fixed housing characteristics, in the sense that residents have no control over them (such as the size of the building), while it does on outcomes that can be influenced by residents (such as voluntary degradations). Second, they test the exogeneity of the different steps in the allocation process during the application and the refusal decision process. They show there is no self-sorting along ethnic lines focusing on movers into public housing blocks. Since self-selection could still occur prior to the move, they also focused on households that have refused a public housing dwelling offer. They show that households declining an offer end up living in public housing blocks that display the same level of diversity as those who directly accepted their first offer. Thus, even if households try to be choosy with respect to the ethnic composition of their neighborhoods, they eventually do not self-segregate in the public housing sector due to the allocation process and the tight supply constraints of dwellings.

Here, we complement those previous findings with two direct tests of the allocation process. Since the allocation is at the department level, we investigate whether households are allocated exogenously across the different public housings within a given *département*.

Table G.2 provides a first approach where, for various observable household characteristics, we test for the difference in means between residential blocks. We regress, for each *département* taken separately, each observable on a battery of fixed effects associated with the different residential blocks located in this *département*. Those regressions are performed on the subsample of household heads who are living in public housing. In case of endogenous residential sorting in some public housing blocks, the fixed effects associated with those blocks should be statistically significantly correlated with the household characteristics, and the F-test will be rejected. We consider the two main characteristics used in our model of spatial information, e.g the ethnic and occupation composition of the residential block. We focus on the Arabic origin of the respondent's name, the nationality at birth (set to one if the nationality of the respondent or of the respondent's parent is from France, and zero otherwise) and the respondent's occupation (coded as a binary variable equal to one for blue collars). Column (1) of Table G.2 reports, for each observable, the share of *départements* for which the F-test is not rejected at the 10 percent level (a more conservative criterion in our context than the standard 5 percent level). For the sake of comparison, we run, in Column (2), the same F-test on the full sample of household heads, including both those who live in the public and private housing sectors. In this case, as expected, endogenous residential sorting is much more salient.

Table G.2 – F-Test of Residential Sorting

% departments without residential sorting relative to households' characteristics		
	Public Housing	Total Sample
Household's characteristics:		
Nationality from Maghreb/Middle East	77.35	55.55
French Nationality at Birth	81.88	59.52
Occupation: blue collar	80.32	54.22

Note: The table reports the share of *département* for which F-tests (at the 10 percent level) do not reject the null-hypothesis of a null correlation between observable characteristics and residential block fixed effects. The F-test are based on a logistic regression of household characteristic on public housing fixed effect within each *département*. The sample includes household heads aged over 15 years old.

Our previous approach to testing exogenous spatial allocation has the advantage of simplicity but might not be ideally suited to our empirical context where the spatial units under consideration are small. Indeed, in the LFS, the average residential block is composed of only 18.31 household heads. In this context, as first pointed out by Ellison and Glaeser (1997), parametric test of spatial allocation/concentration (that assume independent location choices) might be ill-defined to test the null hypothesis of exogenous allocation.[29]

[29]The allocation of households across public housing blocks takes place at the *département* level. If the members of the public housing committee strictly follow the legal criteria and do not take into account the ethnic characteristics in the allocation process, we should find a uniform distribution of households of a given nationality across the various public housing residential blocks *only if the size of each block is large enough*. For the sake of illustration, let us assume that 10 percent of individuals with Arabic origins live in the public housing sector in Paris. We would find the same share of 10 percent within each Parisian housing block if the allocation is truly exogenous with respect to ethnic characteristics only if we have a sufficiently large number of individuals within each housing block; otherwise we will observe patterns of spatial concentration in some blocks.

Thus, we propose an alternative approach. We perform a Monte Carlo simulation generating artificial random allocations that we later compare to the observed allocation. For each *département*, we pool the public housing population and reallocate it randomly, without replacement, across the different residential blocks of the corresponding *département*, maintaining unchanged the actual size of each block. We get a simulated random distribution of individuals with a given characteristic across blocks. We then run a Kolmogorov-Smirnov (KS) test of equality of distribution with the actual spatial distribution. The final step calculates the percentage of *départements* for which the actual and simulated distributions across housing blocks are similar, i.e. those for which we cannot reject the null hypothesis of equality of the distribution at the 10 percent level. We run 100 draws of the Monte Carlo simulation. For each draw we compute the tests for the equality of distributions.

Table G.3 shows the values of those tests averaging over 100 Monte Carlo draws. Column (1) shows that the equality of spatial distribution of ethnic origins and occupations between the randomly simulated distribution and the observed one is accepted in most departments in the public housing sector. In particular, the equality of distribution with respect to Arabic origin (French origin) is not rejected in 80.08 percent (70.23 percent) of the *départements* in the public sector. Similarly, the distribution of individuals according to their occupation across the different housing blocks is close to the simulated distribution. In contrast, Column (3) shows that in the full sample, the equality of distribution is not rejected in 54.3 percent of the departments for the characteristic Arabic origin and in 24.8 percent of the department for the characteristic French origins. Thus in the whole sample, French households do self-segregate a lot, and even more than Arabic-origin households thanks probably to their social and economic capital. However this strong spatial self-sorting no longer holds in the public housing sector.

G.4 Additional tests on the Exogeneous Spatial Allocation Process in Public Housing

This Appendix G.4 provides a variety of additional tests. First, we focus on movers and show that, in the public housing sector, movers do not select new neighborhoods where their ethnic and socio-economic characteristics (education and occupation) is over-represented. Since self-selection could occur prior to the move, we also look at the characteristics of households that have refused a public housing dwelling offer. We show that they display the same characteristics as those who accepted their first offer. Thus, even if households try

Table G.3 – Monte-Carlo Test of Random Allocation

% departments without residential sorting relative to households' characteristics		
	Public Housing	Total Sample
Household's characteristics		
Nationality from Maghreb/Middle East	80.08	54.36
French Nationality at Birth	70.23	24.89
Occupation: blue collar	97.02	60.95

Note: Comparison between the actual and simulated distributions by ethnic groups shares, education and occupations across public housing blocks (Col. 1) and across the whole sample of housing blocks (Col. 2). Percentage of *départements* where equality is not rejected at the 10 percent level using a Kolmogorov-Smirnov test.

to be choosy with respect to the ethnic and socio-economic composition of their neighborhoods, they cannot self-segregate in the public housing sector due to the allocation process and the tight supply constraints of dwellings. Overall, those tests are supportive of our identifying assumption that the allocation of households across the public housing blocks can be considered as exogenous with respect to their ethnic and occupational backgrounds.

G.4.1 Absence of self-sorting on ethnic backgrounds

Our first set of alternative tests consists in showing that while households tend to self-segregate in the unconstrained private housing market, there is no such evidence in the public housing market. We test this using the LFS and focusing on individuals who recently moved into an area (within the previous year).

We first estimate the correlation between the origin (nationality) of individuals moving into a new area and the share of the area's "long term" population of the same origin.[30] We expect a significant relationship in the private housing market where location choice is relatively unconstrained but not in the public housing sector. Table G.4 reports the results from an OLS regression of the share of neighbors from the same origin as new movers on new movers' characteristics: nationality group, public housing dummy, quadratic function of age, hourly wage (log) education, socio-economic category, *département* fixed effects, and interaction of individual characteristics with the public housing dummy. We consider

[30] A similar test was proposed by Goux and Maurin (2007) to show that the educational achievement of the children of newcomers in public housing is uncorrelated with that of the current residents. Individuals do not self-select in public housing neighborhoods according to the educational achievement of the neighbors' children. By contrast, the authors find a strong self-selection on educational characteristics in the private housing sector.

seven different nationality groups: native French, naturalized French, Europeans, Arabic, other Africans, Asians, and other nationalities, which is taken as the reference group.

Three facts are worth noting here. First, there is indeed evidence that on average native French are significantly more likely to move in neighborhoods where the share of natives is higher, compared to households from other nationalities. This is not surprising given the fact that natives make up a large majority of the French population. The second interesting point is that the coefficient for living in the public housing sector is negative and significant at the 5% level. More precisely, it reveals that HLM households move in areas where the share of individuals from the same origin is on average 18.4% lower than for households in the private housing sector. This result strengthens the idea that the extent to which households in the public housing sector live close to those in similar ethnic groups is lower than in the private sector. Finally, when we interact nationalities with the public housing dummy, none of the coefficients are statistically significant. This comforts us with the idea that there is no particular self-segregation along ethnic lines in the public housing sector. We have run the same kind of test on other individual characteristics, and reach similar conclusions. We find that in the private sector, highly educated or low skilled workers are very likely to move into neighborhoods with higher levels of highly educated (respectively low skilled) people. This is not surprising and illustrates self segregation along education level in the private sector. On the contrary, such segregation does not appear in the public housing sector.

G.4.2 Tests on the refusal rate of public housing offers

The previous tests point out the absence of self-selection along ethnic lines among the movers, but self-selection could occur prior to the move. In this case the sample of movers that we observe in the database would be biased. We address this issue by looking at households that have refused a public housing dwelling offer. We show that even if households declined at least one offer, possibly due to the ethnic diversity or the socio-economic composition of the neighborhood, they were still unable to choose the ethnic and socio-economic composition of the housing block in which they ended up living. In this section, we report the results for the refusal rate of public housing offers depending on the neighborhood's ethnic composition (measured with a standard fractionalization index; see Alesina *et al.*, 1999). Similar (unreported) results are obtained when looking at the composition of the neighborhood by educational levels or occupations.

We run this analysis on an alternative database, the 2002 Housing Survey, to bring

Table G.4 – Correlation between new movers' nationality and the residential share of neighbors from the same nationality

Dep Var: % of neighbors from the same nationality	
Nationality (ref.: Other nat.)	
Native	0.067^{b}
	(0.030)
Naturalized French	-0.037^{b}
	(0.012)
European	-0.007
	(0.011)
Arabic	0.007
	(0.013)
Asian	-0.019
	(0.047)
Public Housing (HLM)	-0.184^{b}
	(0.066)
Nationality × HLM	
HLM × Native	0.040
	(0.033)
HLM × Naturalized	0.051
	(0.036)
HLM × European	0.010
	(0.037)
HLM × Arabic	0.024
	(0.036)
HLM × Asian	0.027
	(0.078)
Intercept	0.070**
	(0.035)
R-squared	0.864
N	11519

Note:The dependent variable is the share of neighbors from the same ethnic group as new movers in a given housing block. Additional controls are a quadratic function of age, gender, hourly wage (in log), education, occupation, housing block socio-economic characteristics and department fixed effects. Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%.

additional evidence on the absence of sorting. The Housing Survey (HS) shares exactly the same structure as the Labor Force survey, with information collected at the housing block level with adjacent neighbors (an average of 18.2 in the public housing sector). The HS additionally reports specific questions on household satisfaction with housing quality and if they have turned down public housing offers.

If there were self-selection upon diversity, we should expect households that turned down proposals before being allocated to their current public housing dwelling to end up living in less diverse neighborhoods. To test this conjecture, we run OLS regressions of a variable indicating whether the household declined at least one offer (during the latest application process) on the level of ethnic diversity of the neighborhood in which it now lives. Panel A-I of Table G.5 shows various estimates of the effect of ethnic diversity on the probability of having turned down offers. Column 1 shows the correlation without any additional control variables. In Column 2, we control for household characteristics. We add housing project characteristics in Column 3. Column 4 finally includes neighborhood characteristics and *département* fixed effects since the allocation of a public housing dwelling takes place at the *département* level. In each specification, the coefficient on ethnic diversity is not significantly different from zero, showing that households having declined offers during their past allocation process do not end up living in neighborhoods with significantly different levels of diversity.

Table G.5 – Rejection of Public housing offers and Ethnic diversity

	Coeff. associated with Ethnic Diversity			
Rows: Dependent Variables	(1)	(2)	(3)	(4)
Panel A: Sample of households who currently live in public housing:				
I. Probability of having declined at least one public housing offer during the previous application process	0.058 (0.058)	0.069 (0.063)	0.017 (0.067)	0.123 (0.0886)
N	1,779	1,779	1,748	1,744
R^2	0.001	0.021	0.023	0.089
II. Probability that the reason for having declined a public housing offer during the previous application was "unpleasant environment"	0.162 (0.144)	0.061 (0.158)	0.017 (0.171)	-0.0310 (0.258)
N	417	417	415	414
R^2	0.003	0.035	0.050	0.308
Panel B: Sample of households who are currently applying to public housing:				
I. Probability of having declined at least one public housing offer during the current application process	-0.063 (0.057)	-0.043 (0.064)	-0.088 (0.071)	-0.116 (0.103)
N	1,192	1,192	1,173	1,171
R^2	0.001	0.014	0.024	0.121
II. Probability that the reason for having declined a public housing offer during the current application was "unpleasant environment"	0.004 (0.194)	-0.007 (0.237)	-0.104 (0.250)	-0.122 (0.506)
N	198	198	195	194
R^2	0.000	0.083	0.115	0.590

Note:Each of the coefficients is estimated from a separate regression of each of the four dependent variables described in the first column on an ethnic fractionalization index at the housing block-level. Column 1 does not include any control. Column 2 includes households characteristics (gender, age, education, employment status and nationality of the head of household, total income (in log) of the household per unit of consumption, and household size). Column 3 adds up the characteristics of the building (number of apartments (in log) and construction date). On top of that, column 4 includes neighborhood characteristics (socio-economic background (Tabard index), and local unemployment rate), as well as *département* fixed effects. In addition, a dummy variable indicating whether the household already lives in the public housing sector is included in specifications 2 to 4 of Panel B. Standard errors, clustered at the residential block level, are in parentheses with ^{a}, ^{b} and ^{c} respectively denoting significance at the 1%, 5% and 10%.