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Authors
Adida, Claire L
Laitin, David D
Valfort, Marie-Anne

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RELIGIOUS HOMOPHILY IN A SECULAR COUNTRY: EVIDENCE FROM A VOTING GAME IN FRANCE

CLAIRE L. ADIDA, DAVID D. LAITIN and MARIE-ANNE VALFORT*

Homophily—the tendency individuals have to associate with similar-others—is a powerful determinant of social networks. Yet research to date does not allow us to determine which dimension, e.g., ethnic, religious, gender, age, or class similarity, drives association. Tests demonstrating homophily are flawed by restricting the range of dimensions in the choice set. We introduce an experimental game in which we exogenously expose subjects to diverse partners to determine which dimension dominates. We find that in a socio-demographically diverse district of Paris, despite expectations of secularization, religious similarity significantly predicts homophily. Moreover, we provide tentative evidence that religious homophily is taste-based. (*JEL* C91, D03, D72, J71, Z12)

I. INTRODUCTION

Homophily—or the concept that individuals who are similar tend to come together—has long been recognized as a powerful determinant of social networks.[1] Our friends tend to be our coethnics[2] (Currarini, Jackson, and Pin 2009; Wimmer and Lewis 2010). We date those with whom we share similar levels of education (Hitsch, Hortacsdu, and Ariely 2010; Skopek, Schulz, and Blossfeld 2011), an ethnicity (Hitsch, Hortacsdu, and Ariely 2010), a religion (Hitsch, Hortacsdu, and Ariely 2010), a set of values (Bearman, Moody, and Stovel 2004), and political beliefs (Huber and Malhotra 2012; Klofstad, McDermott, and Hatemi 2013).[3] Many of these factors also end up predicting whom we marry (Kalmijn 1998). Even our hiring decisions rely on homophily of race (Bertrand and Mullainathan 2004) and religion (Adida, Laitin, and Valfort 2010).

Similarity breeds connection, but similarity of what? Basic socio-demographic characteristics such as ethnicity, class, gender, age, and religion have all been shown to underlie homophily (McPherson, Smith-Lovin, and Cook 2001). Our principal objectives in this paper are first to determine which socio-demographic characteristic emerges as the salient basis for homophily

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Adida: Assistant Professor, Department of Political Science, University of California San Diego, Social Sciences Building No. 389, 9500 Gilman Drive No. 0521, La Jolla, CA 92093-0521. Phone 858-534-3548, Fax 858-534-7130, E-mail cadida@ucsd.edu

Laitin: Department of Political Science, Stanford University, Stanford, CA 94035-6044. Phone 650-725-9556, Fax 650-723-1808, E-mail dlaitin@stanford.edu

Valfort: Paris School of Economics – Paris I Panthéon Sorbonne University, Centre d'Economie de la Sorbonne Office Room 318, 106-112 Boulevard de l'Hôpital, 75647 Paris Cedex 13, France. Phone +331 44-07-82-16, Fax +331 44-07-83-01, E-mail marie-anne.valfort@univ-paris1.fr

1. The term was originally coined by Lazarsfeld and Merton (1954) in their study of friendship. As McPherson, Smith-Lovin, and Cook (2001) reckon in their review of the literature a half-century later, the focus on the positive network implications of homophily gained less research attention than the focus on prejudice, the negative side of the same coin. Here we principally address concerns of which aspects of people's characteristics lead them to associate with one another. Thus, our focus is on homophily.

2. We follow Chandra's (2006) definition of ethnicity as a subset of identity categories in which eligibility for membership is determined by descent-based attributes; this includes race.

3. See Burgess, Sanderson, and Umana-Aponte (2011), p. 4 for a summary of this literature.

ABBREVIATION

FDR: False Discovery Rate

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when the most common social match-ups are in the mix,[4] and second to get at least a partial understanding of the mechanisms that sustain it.

We are among the first to investigate this question: research to date relies on ex post (i.e., post-selection) data, that is, on people who are already matched on certain socio-demographic characteristics. For instance, the Add Health data exploited by Currarini, Jackson, and Pin (2009) and the ALSPAC data exploited by Burgess, Sanderson, and Umana-Aponte (2011) offer information about *established* relationships: scholars measure homophily by examining what proportion of an individual's friends are "similar individuals," and comparing that to the hypothetical proportion of "similar" friends individuals would have if they were randomly matched with one another. These observational data are thus not conducive to an investigation into which socio-demographic characteristic *emerges* as the salient basis for homophily. Indeed, individuals tend to choose friends who are similar on a number of different dimensions, such that the basic characteristics enumerated above (ethnicity, class, gender, age, religion) are strongly correlated with one another in ex-post data. Such multicollinearity challenges our ability to identify the key characteristic(s) on which individuals match.

Subsequent research on homophily has relied on behavioral data that are less unambiguously ex-post. Such studies use information from social networks or dating websites (e.g., Hitsch, Hortacsdu, and Ariely 2010; Huber and Malhotra 2012; Skopek, Schulz, and Blossfeld 2011; Wimmer and Lewis 2010). These studies analyze which characteristics of a potential partner determine a user's decision to initiate contact. Yet these, too, suffer from some degree of selection bias because the set of potential partners is already the result of a selection process. Indeed, before contacting someone, a user must first browse his/her profile. This set of potential partners is thus determined either by a user's own search or by a website's suggestions—both of which are already predicated on homophily. Hence, the set of potential partners is already a set of "similar individuals." These studies therefore suffer from the same identification problem exposed above.[5]

By relying on an experimental framework where we impose an exogenous set of potential partners, our paper circumvents this identification problem. Specifically, we introduce a voting experiment in which players have limited information about, and no previous connection to, each other. Experimental subjects are assigned to a group by the investigators, and instructed to get to know one another one-by-one, within a short timespan (three minutes for each pair). They are then asked to elect a group leader among the set of partners they have just met, and which we selected. Subjects are told that the elected leader will receive a prize of €30, to be distributed among the electorate in whichever way she chooses (including keeping the entire amount for herself). Our experiment is thus designed to capture which match-up between voter and candidate best predicts the voter's choice. The most common socio-demographic categories—gender, class, ethnicity, age, religion—are in the mix. We measure which one(s) significantly correlate(s) with players' voting decisions. Additionally, our research design allows us to investigate whether the basis we uncover for homophily is a rational one. Homophily is rational if it is based on trust. In this case, the voter believes that leaders with whom she shares basic socio-demographic characteristics are more likely to be generous to her because of such similarity than are leaders who do not share these characteristics. By contrast, homophily is non-rational or taste-based if these beliefs are not at stake.

We implement the study in France in 2009, the home of a larger project on immigrant integration and a country whose recent experience with immigration has fundamentally altered its socio-economic, racial, and religious landscape.[6] This context allows us to study a population that is diverse not only on the age, gender, and class dimensions (as in many other contexts), but also by ethnicity and religion.

4. We draw this list of "most common" social match-ups (ethnicity, class, gender, age, and religion) from the set of socio-demographic characteristics identified by McPherson, Smith-Lovin, and Cook (2001).

5. Centola (2011) and Huber and Malhotra (2012) are the only exceptions of which we are aware. Huber and Malhotra's study aims to identify how individuals' political predispositions determine their romantic dating choices, and it includes an experimental portion where subjects are presented a set of potential partners determined by the investigators. In Centola (2011), participants in an internet-based network experiment were randomly assigned to homophelous versus non-homophelous networks to observe network effects on individuals' health behavior. Here, the degree of homophily is exogenously determined. However, the study does not concern itself with identifying the basis of homophily. Instead, it defines it according to gender, age and body-mass index.

6. Approximately 11% of the French population is foreign-born (Vasileva 2011).

Our empirical results are compelling: in the context of our experimental game in France, and pitting the most common socio-demographic bases for homophily against one another, we find that religious similarity emerges as the only significant determinant of participants' voting behavior when monetary stakes are involved. No other social dimension, be it gender, ethnicity, age, or class plays as significant a role on this outcome. The voting result is remarkably robust: it holds whether we focus on the full, socio-demographically diverse sample of voters, or on a smaller, more homogenous sub-sample of voters with longer inter-generational ties to France. In other words, religious homophily is not imported into France by individuals of recent immigrant background. Furthermore, no single religious belief drives the results: Judeo-Christians vote for fellow Judeo-Christians, Muslims vote for fellow Muslims, and Atheists vote for fellow Atheists. Across religions, players choose leaders with whom they share a religious identity. Finally, our results do not support a rational calculus in this voting decision. In our games, individuals give their vote to people of the same religion, though they seem not to expect that this assures them a material advantage.

Our results have three key implications. First, scholars have shown that homophily may constitute a threat to social cohesion. It generates segregation (Centola et al. 2007; Suen 2010), inequality (Fernández and Rogerson 2001), and compromises the ability of a society to reach a consensus on major issues (Golub and Jackson 2012). Understanding which basic characteristics are significantly correlated with homophily is thus a prerequisite to limiting homophily's potential deleterious social effects. Our study does just that. Second, our results identify a basis for discrimination against certain groups and not others, and thus they have implications for discrimination and prejudice (the other side of the coin from homophily). This body of research has already shown that racial (Bertrand and Mullainathan 2004; Findlay and Reid 1997; Monks and Robinson 2000) and religious (Adida, Laitin, and Valfort 2010) homophily are prevalent in a number of different contexts. Yet recent work challenges these findings by suggesting that discrimination does not target any one group, but targets instead any and all minorities: the threat is not Blacks or Muslims, but rather the ethnic *other* (Edo and Jacquemet 2013; Jacquemet and Yannelis 2012). Our experimental results are consistent with research showing that discrimination falls along specific identity lines: in the case of a diverse region of Paris in France in 2009, religion is significantly correlated with how individuals associate. Our results also suggest that such religion-based discrimination is taste-based. Third, by offering a direct measure of the dominant social basis for homophily, we show that religion in France remains powerful in a society where only 13% of the population explicitly claims that religion is very important in its life (World Values Survey 2006). This is true even when we restrict our analysis to those participants most rooted in France, i.e., French nationals with all four grandparents born in metropolitan France. Even after a century of government policies to undermine the influence of religion in public life through legislative means, religion remains a significant predictor of how the French associate with others living in their communities.[7]

II. THE EXPERIMENTAL SETTING

In this section, we introduce our research design. Specifically, we explain the recruitment of our subject pool, the experimental protocol, and the voting game we use to measure the salient basis for homophily.

A. *Subject Pool*

To conduct our experiment, we first brought together a semi-random sample of 80 participants recruited in Paris's diverse 19th district in March 2009. This sample was collected for a project studying Muslim immigrant integration in France. For that purpose, approximately one-third of our participants were recruited non-randomly, via social networks.[8] The rest were recruited from the 21 metro stations in the 19th district of Paris. In a fully random protocol, we assigned a weight to each metro station based on the density of the area in which it is located, with the higher density stations getting more cards in our random draw. Each recruitment team drew a metro station for each recruitment day, and then a number from 1 to 10 to determine which passer-by to invite as a game recruit. But, for the goals of our larger project, we sometimes deviated from this protocol to specifically recruit

7. This is consistent with Berger's analysis of voting determinants in Fifth Republic France (Berger 1974).

8. In our robustness checks, we run the analysis excluding those players recruited via social networks.

rooted French players, i.e., French nationals with four grandparents born in metropolitan France (in other words, participants who are at least third-generation French, and whom we thus refer to as FFF). When potential FFF subjects walked by, recruiters were instructed to ignore the sequence of selection, and to ask them to participate.[9] Table 1 provides basic socio-demographic characteristics of our sample.[10] It indicates that our candidate pool is, on average, young, well-educated, and ethnically and religiously diverse. This diversity is not surprising, given that we recruited participants from the 19th district of Paris, a highly diverse area. Indeed, according to the 1999 French Census, the percentage of individuals living in this district and born in France is 63.5—against 82.4 for all of Paris (INSEE 1999).

B. Experimental Protocol

Our protocol consisted of two phases: a registration phase and a game phase. During the registration phase, we collected basic socio-demographic data about each player and scheduled players into a game session. During the game phase 2 weeks later, we brought players together into groups of 10, and did so for 8 sessions over 2 weekends—3 of the sessions were all female, 3 were all male, and 2 were mixed in gender.[11] Each session consisted of a series of games, including our voting game, and lasted approximately 2.5 hours.[12] We introduced the experimental games to our participants as games designed to investigate "how people from Ile-de-France [Parisian region] make decisions about money."[13] At the beginning of each session, players were given a name tag to be attached to their lapels on which they wrote their first names. The only information players had about each other at the beginning of each session was therefore their looks, their manners, their dress, and their first names. None wore any

TABLE 1
Summary Statistics—Candidate Characteristics

Variable	Mean	Standard Deviation	Minimum	Maximum	N
Female	.53	.50	0	1	80
Age	36.10	12.76	18	72	80
Education	8.30	2.17	2	10	76
Family income	4.54	2.23	1	9	74
White	.36	.48	0	1	80
North African	.15	.36	0	1	80
Black	.49	.50	0	1	80
Judeo-Christian	.46	.50	0	1	78
Muslim	.37	.49	0	1	78
Atheist	.14	.35	0	1	78
Believer	.03	.16	0	1	78
Political Ideology	4.03	2.22	1	10	67

Notes: The unit of observation is the player. *Female* is a binary variable coded as "1" if the respondent is a woman and "0" if the respondent is a man. *Age* is a continuous variable that counts the respondent's age. *Education* is an ordinal variable ranging from "1" (less than primary school completed) to "10" (higher than college degree completed). *Family income* is an ordinal variable ranging from "1" (less than €500 a month) to "11" (more than €7500 a month). *White* is a binary variable coded as "1" if a majority of the respondent's four grandparents was born in a Western European country, and "0" otherwise. *North African* is a binary variable coded as "1" if a majority of the respondent's four grandparents was born in a North African country, and "0" otherwise. *Black* is a binary variable coded as "1" if a majority of the respondent's four grandparents was born in sub-Saharan Africa or non-metropolitan France, and "0" otherwise. *Judeo-Christian* is a binary variable coded as "1" if the respondent self-identifies as a member of a Judeo-Christian religion (Catholic, Jewish, Orthodox, Protestant, Christian) and "0" otherwise. *Muslim* is a binary variable coded as "1" if the respondent self-identifies as a member of the Muslim religion and "0" otherwise. *Atheist* is a binary variable coded as "1" if the respondent self-identifies as an Atheist and "0" otherwise. *Believer* is a binary variable coded as "1" if the respondent self-identifies as "Believer" or "Other" and "0" otherwise. *Political ideology* is a categorical variable ranging from "1" (most left-wing) to "10" (most right-wing).

9. The recruiting team relied on ethnic profiling, inferring from dress and facial features whether participants were potential FFF.

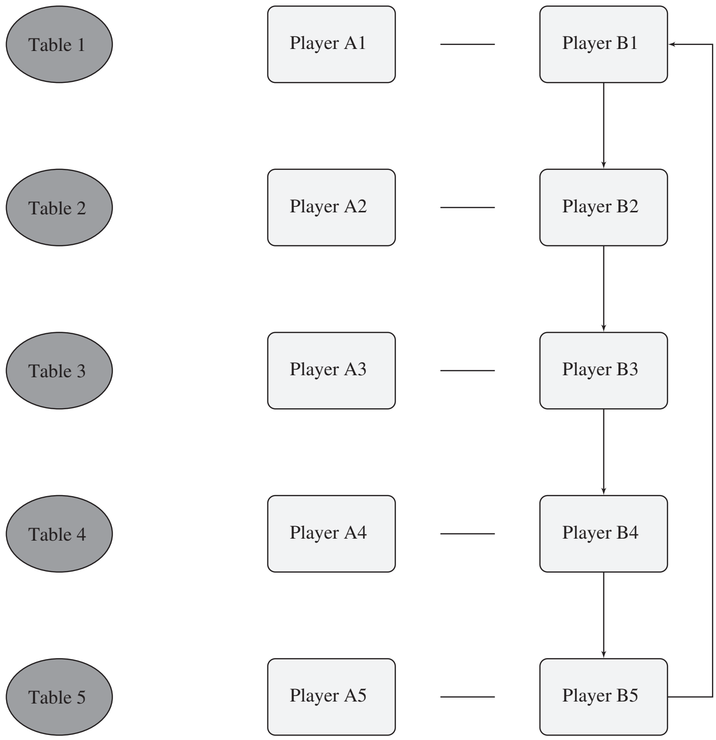
10. Table 1 indicates that *Believers*, e.g., those who answered "Believer" or "Other" in their religious self-identification, comprise only 3% of our sample, or just two respondents. In our robustness checks, we run a specification that excludes these participants. Table 1 also indicates that we have grouped Jews and Christians in a single category for the purpose of this analysis. We run a robustness check where we separate out these two categories.

11. Note that, with six out of eight sessions being same-gender sessions, our design biased our results toward the emergence of gender homophily. In spite of this, we do not find a significant effect of gender homophily on vote choice.

12. Each participant played a simultaneous trust game, a speed-chatting game, a voting game, and a dictator game. For this paper, we focus on the voting and simultaneous trust games. Spill-overs between games are not a concern, as game decisions remained private and anonymous at all times.

13. We remained vague about our objective since we did not want our participants to surmise the goal of our research project and hence bias their behavior during the games. This strategy, combined with the fact that we organized the games in a setting where diversity was seen as natural (the 19th district of Paris), worked. Indeed, in exit surveys where participants were asked "Que pensez-vous que notre équipe aura appris sur vous à travers vos décisions aujourd'hui?" [What do you think our team will have learned about you from the decisions you made today?], only one subject out of 80 speculated that religion had anything to do with the purposes of the games.

FIGURE 1
Speed Chatting Quiz



Note: This diagram illustrates how players were positioned during the speed-chatting game, and the way in which they moved between rounds.

clothes or jewelry revealing religious affiliation, with the exception of one player who wore a headscarf signaling a Muslim identity.[14] Players were then assigned to one of two groups, A or B, and therefore did not choose from among the available players who would be members of their group.

C. The Voting Game

We measure the salient socio-demographic basis for homophily with a voting game designed to capture which social characteristics significantly predict how individuals associate with one another. In our experimental protocol, the voting game followed and built on the speed-chatting game, where each player met all players from the other group (henceforth their *partners*) in a speed-chatting protocol. This speed-chatting game was akin to a speed-dating scenario. Group A players sat down, each at a table, for a total of five tables positioned so as to maximize space and thus privacy. Group B players were instructed to sit down across their first A player (B1 across from A1; B2 across from A2 …). Players were then given 3 minutes to chat and, we emphasized, to “get to know one another.” At the end of those 3 minutes, players were given 1 minute to write down notes about what they had just learned. Then, all B players were instructed to stand up and move to the next table, so that player B1 now sat across A2, player B2 now sat across A3, etc..., and player B5 now sat across A1. Again, players were given 3 minutes to get to know each of their partners. This process was repeated until each A player had met all B players and vice versa. Figure 1 illustrates the speed-chatting protocol.[15]

14. In our robustness checks, we run the analysis excluding this player.

15. At the end of the speed-chatting game, players were given a quiz testing them on the age, religion, employment status, education level, country of origin, current living location, marital status, and favorite leisure activity of each partner. Players were rewarded €1 for each correct answer.

FIGURE 2

The Vote Sheet

CHOICE OF GROUP LEADER AMONG PLAYERS B

PROJECT « GAME BEHAVIOR OF PARISIAN-REGION RESIDENTS »

(Sciences-Po Paris, Université Paris 1 Panthéon Sorbonne, Stanford University)

Identification Number: ________________

1. Rank these candidates as "group leader" by order of preference:

   Rank 1 (your favorite candidate): ____________________
   Rank 2: ____________________
   Rank 3: ____________________
   Rank 4: ____________________
   Rank 5 (your least favorite candidate): ____________________

2. For which of these candidates are you voting? ____________________

Upon completion of the speed-chatting game, we introduced the voting game where each player was asked to vote for a leader among all the partners s/he had just met (see Figure 2 for a sample vote sheet). This leader would be awarded a €30 prize to divide between herself and her electorate. Then, each player was asked how she would distribute the €30 award if she were to become leader (see Figure 3 for a sample allocation sheet). At the end of the game session, the allocations of the elected leaders were added to each player's account, to be distributed after the completion of

FIGURE 3

The Allocation Sheet

GROUP LEADER ALLOCATION DECISION AMONG PLAYERS A
PROJECT « GAME BEHAVIOR OF PARISIAN-REGION RESIDENTS »
(Sciences-Po Paris, Université Paris 1 Panthéon Sorbonne, Stanford University)

Identification Number: ___________________

Imagine that you have just received 30 euros. Indicate the amount you wish to give to each participant.

 Thérèse __________	 Mamadou Lamine __________
 Marie-Laure __________	 Nabil __________
 Kals __________	

the entire experimental protocol. With this voting game, we are able to capture whom each player voted for, as well as how each hypothetical leader would divide up the reward. Each decision was tied to monetary stakes: the elected leader won €30, and players' allocation decisions were used to determine final monetary allocations once votes were tallied and leaders were elected. These instructions were carefully relayed to each group of players.

TABLE 2
Summary Statistics—Voter-Candidate Social Distance Characteristics

Variable	Mean	Standard Deviation	Minimum	Maximum	N
Different gender	.14	.35	0	1	400
Difference in age	14.52	11.71	0	52	400
Difference in education	1.99	2.00	0	7	360
Difference in family income	2.63	1.88	0	8	340
Different ethnicity	.62	.49	0	1	400
Different religion	.66	.47	0	1	380
Difference in political ideology	2.52	2.00	0	9	282

Notes: The unit of observation is the voter–candidate pair. *Different gender* is a binary variable that takes the value "1" if the pair of players is mixed gender and "0" otherwise. *Difference in age* is a continuous variable that provides the absolute difference in age between the two players. *Difference in education* is an ordinal variable that provides the absolute difference in education level between the two players. *Difference in family income* is an ordinal variable that provides the absolute difference in family income between the two players. *Different ethnicity* is a binary variable coded as "1" if the two players are of different ethnicity and "0" otherwise. *Different religion* is a binary variable coded as "1" if the two players are of different religion and "0" otherwise. *Difference in political ideology* is an ordinal variable that provides the absolute difference in political ideology between the two players.

We use players' voting decisions as a method to measure which socio-demographic characteristic emerges as the salient basis for homophily when monetary stakes are involved. This variable, we should note, captures sincere rather than strategic voting. Indeed, two questions captured voters' intent in our voting game. The first asked voters to rank, in order of preference for group leader, the five candidates they had just met. The second asked voters to vote for a single candidate as leader. We measure *Vote* as the candidate ranked first by the voter. It is important to emphasize that voters systematically chose to vote for the candidate they ranked first: voters were thus expressing their true preference, not behaving strategically in this decision. We then use players' monetary allocations as leaders along with data from our simultaneous trust game—which we present in a later section—to illuminate whether the homophily we uncover in the voting game is a rational strategy to optimize the distributive payoff.

III. THE SOCIAL BASIS FOR HOMOPHILY

A. Results

Which socio-demographic characteristic(s) drives players' vote choice in our experimental context? In Table 2, we present summary statistics for our social distance variables: these summarize, for each pair of players in the voting game, the average difference in gender, age, education level, family income, ethnicity, religion and political ideology between the players.[16]

We measure the socio-demographic basis for homophily by estimating the following model:

(1)
$$\text{Vote} = a + \mathbf{b}_1'.\,(\mathbf{Distance}) + \mathbf{b}_2'.\,(\mathbf{Candidate}) + \mathbf{b}_3'.\,(\mathbf{SessionFE}) + e$$

where Voter–Candidate pair is the unit of analysis and *Vote* captures whether or not Voter voted for Candidate. **Distance** is a vector of variables that capture the social distance between candidate and voter on a set of sociodemographic characteristics—gender, age, education, family income, ethnicity, and religion; b_1' thus summarizes the effect of the social distance between voter and candidate on vote choice. It captures the social basis underlying homophily in this context. **Candidate** is a vector of control variables (gender, age, education, family income, ethnicity, and religion) that characterize the candidate; b_2' thus summarizes the effect of candidate characteristics on the likelihood of becoming a leader. Finally, **Session FE** is a vector of game session fixed effects. Standard errors are robust and clustered at the individual voter level, since individual voter decisions on each candidate are not independent of one another.[17]

The critical aspect of our research design is that it allows us to identify the social basis

16. In robustness checks, we further control for the role that value homophily might play by controlling for political ideology. Our results persist. Discussing one's political opinions is a relatively taboo topic in France (e.g., Louarn 2012); hence the high rates of missing data for the political ideology measure.

17. We present results from logit specifications. In robustness tests, we check whether our results hold using linear estimation techniques instead.

for homophily before any selection occurs: we impose exogenously the group of players each player must vote for, and then observe which match-up best predicts vote choice. We confirm that the set of candidates each player must vote for is exogenously selected with a simple test of multicollinearity across our right-hand-side variables. If preselection occurs, we can expect the set of players in a single group to be similar in a number of ways. Yet our correlation tests across our right-hand-side variables indicate that the highest correlation we find among all social match-up variables is a correlation of $r = .21$ between the difference in education and the difference in family income. We thus have no reason to believe our specification suffers from multicollinearity.

Models (1) through (3) in Table 3 estimate Equation (1) above using Stata 11.0. Model (1) estimates the effect of social distance between voter and candidate on the probability of being chosen as leader. Model (2) adds controls for candidate characteristics. And Model (3) imputes missing data due largely to our *Education* and *Family income* variables.[18]

The results in Table 3 are striking: two individuals who do not know each other, but are given the chance to interact for fewer than 5 minutes, are more likely to vote for each other when they share the same religion. The coefficient on *Different religion* is consistently negative, and statistically significant at least at the 95% confidence level in all specifications. No other social dimension holds a comparably robust, statistically significant effect. Figure 4, relying only on descriptive statistics, illustrates this pattern clearly by showing the distribution of leader rankings (where Rank 1 is the voter's first choice for leader) for dyads who share a religion (in light gray) and dyads who do not share a religion (in dark gray). For example, Figure 4 clearly shows that for at least 26% of co-religious voter-candidate pairs, the voter ranked the candidate as most preferred; by contrast, the voter ranked the candidate as most preferred in only 17% of non co-religious voter-candidate pairs. Conversely, for 14% of co-religious voter-candidate pairs, the voter ranked the candidate as least preferred; by contrast, the voter ranked the candidate as least preferred in 22% of non co-religious voter-candidate pairs. To give a sense of the magnitude of the effect, the predicted probability of voting for a candidate is approximately 17% when voter and candidate do not share a religious identity, and close to 30%—a greater than 65% increase—when they do.[19]

TABLE 3
The Determinants of a Player's Vote

	DV: Vote		
Variable	**Model (1)**	**Model (2)**	**Model (3)**
(1) Different religion	−.764*	−.931**	−.740*
	(.323)	(.351)	(.312)
(2) Different gender	−.467	−.328	−.155
	(.621)	(.588)	(.476)
(3) Different ethnicity	.058	.160	.080
	(.299)	(.306)	(.265)
(4) Age difference	.005	.002	−.000
	(.014)	(.014)	(.012)
(5) Education difference	−.126*	−.099	−.126^
	(.063)	(.068)	(.066)
(6) Income difference	.023	−.004	−.018
	(.082)	(.081)	(.079)
Candidate controls	No	Yes	Yes
Imputed missing data	No	No	Yes
Pseudo R^2	.029	.064	N/A
Observations	307	307	391

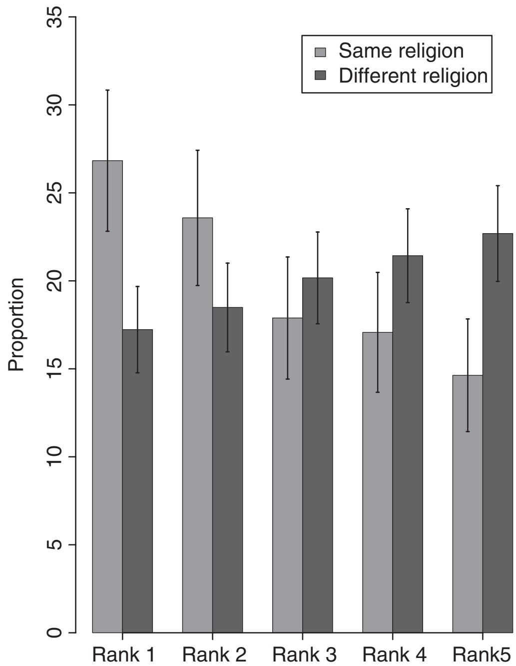
Notes: The table reports Logit estimates. The unit of observation is the voter−candidate pair. The dependent variable, *Vote*, takes the value "1" if the voter votes for the candidate, and "0" otherwise. All models include session fixed effects. All standard errors are robust and clustered at the individual voter level.

^, *, and ** denote statistical significance at the 10%, 5%, and 1% level, respectively.

18. We impute missing data relying on Stata's "mi" command, a simulation-based statistical technique that consists of (1) imputing the missing data by simulating from a Bayesian posterior predictive distribution of the missing data under the conventional prior distribution; (2) analyzing the data separately for each imputation; and (3) combining the results in step (2) into a single multiple-imputation result. For more information, readers can refer to the Stata Multiple-Imputation Reference Manual: http://www.stata.com/manuals13/mi.pdf. Additionally, we run an alternative method of addressing missing data via Manski's method of bounds (Manski 1989). Here, we make assumptions about the ways in which our missing data variables correlate with the outcome of interest. First, we focus on differences in family income, where we have missing data for $N = 60$ observations. We generate two variables for differences in family income, one where we assume that the difference is maximal when the voter does not vote for the candidate and minimal when the voter does vote for the candidate; and one where we assume the opposite. We then do the same for differences in education, where we have missing data for $N = 40$. We estimate Model (1) in Table 3 relying on these new variables, including them sequentially and then together. Our results persist.

19. These estimates are calculated using the *predict* command in Stata, applied to the logit specification for Table 3, Model (2).

FIGURE 4
Leader Rank by Same or Different Religion



Note: This histogram illustrates the distribution of leader rankings by whether or not voter and candidate share a religion. A rank of "1" is the voter's preferred candidate. A rank of "5" is the voter's least preferred candidate. For example, in at least 26% of co-religious voter–candidate pairs, the voter ranked the candidate 1 (most preferred); in 14% of co-religious voter–candidate pairs, the voter ranked the candidate 5 (least preferred). The bars are standard error bars (95% confidence level) for the estimates of proportions for each category above.

Our analysis so far reveals that, in the context of our voting game, religion is a dominant basis underlying homophily. Pitting gender, age, ethnicity, education, family income, and religion against one another, we find that more than any other factor, players who share a religious identity are more likely to vote for one another.[20]

20. In a series of Wald tests assessing the extent to which the effect of different religion significantly differs from that of each of the other social distance variables, we find that—at least at the 90% confidence level—the effect of our religious distance variable differs significantly from that of all other social distance variables except for gender. But this absence of significant difference between religious and gender homophily is not robust. In three of our six robustness checks, the Wald test comparing the effects of differences in religion and gender becomes statistically significant at least at the 95% confidence level. We elaborate on our robustness tests in Section III.B.

B. *Robustness Checks*

In Table 4, Models (1) through (6) present results from a series of robustness checks that corroborate our main empirical findings. In Models (1) and (2), we conduct our analysis on two sub-samples of voters who are more deeply rooted in France than our sample of the diverse 19th district of Paris. In Model (3) (with missing data) and (4) (with imputed data), we account for the role that political ideology-based homophily might play with controls for the candidate's political ideology as well as the distance in ideology between voter and candidate. In Model (5), we check whether our results are driven by *native* homophily, that is, by rooted French players who are turned off by foreignness (Edo and Jacquemet 2013; Jacquemet and Yannelis 2012). We do so by running the regression on our sub-sample of rooted French players, and including a control for whether the two players share typical French names.[21] Finally, in Model (6), we exclude the set of players who were recruited via social networks.[22] Our robustness checks reveal that the effect of co-religion on voting for a leader is robust: statistical significance persists in Table 4, Models (1) through (6).

In a first robustness check, we ask to what extent our results might be driven by the diverse nature of our sample. Indeed, given our sampling from the 19th district of Paris, our results could be driven by an importation of the religious cleavage by immigrants, and not at all by its salience among a population of rooted French established in France for a much longer time. We check whether this is the case with two robustness tests. First, although our sample is ethnically and religiously diverse, approximately 64% of it consists of French nationals; when we restrict our analysis to that sub-sample of voters, our results hold at the 99% confidence level. Second, when we restrict our analysis to a sample of rooted

21. A more conservative test would be to run the regression on a sub-sample of rooted French on rooted French players. However, we do not have a large enough sample of rooted French to do that.

22. We run an additional four robustness checks that are not presented here: (1) we exclude *Believers*, a small residual category for religious identification; (2) we exclude the single player who wore a headscarf, signaling a religious affiliation on top of the signals available to every other participant (first name, clothes, manner of speech); (3) we separate out Jews from Christians instead of collapsing them into a single Judeo-Christian category; (4) and we estimate Equation (1) using a linear specification. The effect of co-religion on players' voting decisions remains the only positive and statistically significant effect at least at the 95% confidence level in all cases.

TABLE 4

Robustness Checks on Determinants of a Player's Vote

Variable	Model (1) French Natl.	Model (2) FFF	Model (3) Pol. Id.	Model (4) Imputed Pol. Id.	Model (5) Same Name	Model (6) No Soc. Net.
(1) Diff. religion	−1.419**	−1.793^	−0.896*	−.779*	−2.027*	−.931**
	(.503)	(.964)	(.385)	(.320)	(.994)	(.351)
(2) Diff. gender	.248	27.804**	−.094	−.159	28.869**	−.328
	(.759)	(2.263)	(.618)	(.463)	(2.794)	(.588)
(3) Diff. ethnicity	−.119		−.018	.131		.160
	(.410)		(.348)	(.267)		(.306)
(4) Age diff.	.008	.027	−.011	−.005	.027	.002
	(.018)	(.032)	(.017)	(.013)	(.035)	(.014)
(5) Education diff.	−.203^	−.677*	−.156	−.116	−.741^	−.099
	(.113)	(.336)	(.131)	(.075)	(.399)	(.068)
(6) Income diff.	.076	−.119	−.058	.002	−.120	−.004
	(.131)	(.243)	(.091)	(.079)	(.241)	(.081)
(7) Pol. ideology diff.			−.215*	−.143		
			(.102)	(.092)		
(8) Same French name					1.182	
					(1.345)	
Imputed missing data	No	No	No	Yes	No	No
Pseudo R^2	.148	.239	.090	N/A	.254	.064
Observations	190	84	234	391	84	307

Notes: The table reports Logit estimates. The unit of observation is the voter–candidate pair. The dependent variable, *Vote*, takes the value "1" if the voter votes for the candidate, and "0" otherwise. Model (1) restricts the sample to French national voters only. Model (2) restricts the sample to FFF voters only. Model (3) analyzes the full sample and includes a control for political ideology. Model (4) imputes missing data from Model (3). Model (5) restricts the sample to rooted French voters only and controls for both players bearing typical French names. Model (6) excludes from the sample those players recruited via social networks. All models include session fixed effects and candidate controls. All standard errors are robust and clustered at the individual voter level.

^, *, and ** denote statistical significance at the 10%, 5%, and 1% level, respectively.

French voters, i.e., participants who are French citizens and whose four grandparents were born in metropolitan France, our results persist at the 90% confidence level, with a larger substantive effect [see Table 4, Models (1) and (2)]. Note also that when we run the analysis on the full sample and include an interaction term between *Different religion* and being a rooted French voter, we find that this interaction term is negative and statistically significant at the 95% confidence level, indicating that, although rooted French consider themselves to be secularized, they play a particularly active role in the emergence of religious homophily.

Second, we test whether our results persist when further controlling for value homophily. Scholars have shown that, beyond the socio-demographic and behavioral bases for homophily, people come together based on shared values. Cohen (1977), for example, shows that friendship networks in an American school are based on shared values such as risk behavior, academic interest, and aspirations. Many of our distance variables already account for value homophily: religion, for example, strongly conditions individuals' values (e.g., Hillman 2007). Yet, it seems important to control for another source of value homophily, viz. distance in political ideology. Klofstad, McDermott, and Hatemi (2013) and Huber and Malhotra (2012), for example, show that political beliefs play a key role in individuals' dating preferences. We account for the role of value homophily in Models (3) and (4) with a control for the candidate's political ideology, as well as the distance between candidate and voter ideology. Again, we find that the effect on *Vote* is robust, retaining significance at the 95% confidence level whether or not we impute missing data. Furthermore, religious distance remains the only statistically significant effect in our model: difference in political ideology, while negative and statistically significant in Model (3), loses its statistical significance once we impute missing data in Model (4).

Model (5) tests the native homophily hypothesis (e.g., Edo and Jacquemet 2013), purporting that a common rooted French identity—rather than religion—drives homophily. We run the regression on our sub-sample of rooted French players, and include a control for whether both players have typical French names. Our results indicate that the effect persists; note, also, that the coefficient for the *Same French name* dummy is positive—as would be expected by the native homophily hypothesis—but not statistically significant.

In the final robustness test of Table 4, we exclude from our sample the players we recruited via social networks. The concern here is that these players may already know each other or belong to homophilous social networks. Since the goal of our experiment is to gauge which socio-demographic dimension emerges as a dominant basis for homophily in a context where individuals do not know each other and are not already associating with one another, we must be sure that our pool of players does not already belong to homophilous networks outside our experimental context.[23] In Model (6), our effect holds at the 99% confidence level.

Finally, we offer an even-more conservative robustness check by correcting for multiple comparisons.[24] We do so first by assessing how likely it would be to find the number of statistically significant results we find by chance if all the nulls were true. Furthermore, we specify—relying on the Benjamini and Hochberg correction for multiple comparisons (Benjamini and Hochberg 1995)—which of our findings hold up to corrections for multiple comparisons.

We check first for a possible multiple comparison problem by asking what is the probability that we might have observed the significant effect of co-religion on vote choice by chance if all the nulls we tested were true? Our paper tested which of six possible social distance variables—religion, gender, ethnicity, age, education and income—become significant predictors of the voter's choice. At an α of .05, the probability of wrongly rejecting the null hypothesis for one test is 5%. Hence, the probability of wrongly rejecting one of the six null hypotheses we test is $6\times[(.05)\times(.95)^5]=.23$. Put differently, on average, we wrongly reject the null hypothesis for 1 out of our 6 tests. Using a Poisson distribution of parameter 1 (the average number of tests for which the null hypothesis is wrongly rejected) allows us to calculate the likelihood of observing our religious homophily result by chance if all the nulls were true. We find that this likelihood is equal to .37.[25]

This possibility compels us to correct for a possible multiple comparisons problem. To do so, we rely on the Benjamini and Hochberg correction (1995), which is an application of the false discovery rate criterion.[26] This false discovery rate constitutes the threshold, or the largest p value that holds up to the Benjamini and Hochberg correction. It is equal to $\text{FDR}=(m_0/m)\times\alpha$, where m is the number of null hypotheses that are being corrected for, m_0 is the number of statistically significant results (or the number of null hypotheses rejected before the correction), and α is the level of statistical significance.

For our purposes, at an alpha of .05, with 6 null hypotheses that are being corrected for and only 1 statistically significant result, the false discovery rate is $\text{FDR}=(1/6)\times\alpha=.0083$.[27] This FDR acts as a threshold p value: any p value equal to or less than .0083 holds up to the Benjamini and Hochberg correction. Given that the p value we actually observe for the test of statistical significance of *Different religion* in Table 3, Model (2), is .0079, we conclude that our result holds up to the Benjamini and Hochberg correction.[28]

C. Discussion

In the context of our experimental games in France in 2009, religious similarity is a significant predictor of vote choice between players who do not otherwise know each other. No other social dimension we capture plays such a role. This result is consistent and robust. When the most common social matchups can emerge as the basis for homophily, it is

23. Note, however, that the experimental design raises little concern for this, since the divide between the group of voters and the group of candidates was exogenous (i.e., imposed by the experimenter).

24. We thank an anonymous reviewer for this suggestion.

25. We use the following equation to determine this: $p=(e^{-\lambda}\times\lambda^x/x\,!)=(e^{-1}\times 1^1/1\,!)=.37$.

26. The false discovery rate, or FDR, is the expected proportion of incorrectly rejected null hypotheses.

27. Note that this assumes independence between our tests, and is thus a more conservative threshold than if this assumption did not hold.

28. Note that an even more conservative test, the Bonferroni correction (Dunn 1961)—which controls for the family wise error rate, or the probability of making at least one false rejection in the set of comparisons—is equivalent to the test we run here because we are testing only one significant p value. The Bonferroni correction indicates that we must only consider a result to be robust using an α of .05 if the p value for the test is less than α/m=.05/6=.0083. Again, given that the p value we observe is p=.0079, our result holds up to the most conservative correction for multiple comparisons.

FIGURE 5
The Speed Chatting Quiz

SPEED CHATTING QUESTIONNAIRE
PROJECT : « GAME BEHAVIOR OF PARISIAN-REGION RESIDENTS »
(Sciences-Po Paris, Université Paris 1 Panthéon Sorbonne, Stanford University)

Identification Number: __________________

Simone

Questions	Did you guess this or learn it during the speed-chatting game?	Indicate on a scale of 1 to 10 your degree of confidence in your answer (1 being the lowest degree of confidence, 10 the highest)
1. How old is this person? ________	Guessed/Learned	1 2 3 4 5 6 7 8 9 10
2. What is her religion? ____________	Guessed/Learned	1 2 3 4 5 6 7 8 9 10
3. Is she self-employed? YES / NO	Guessed/Learned	1 2 3 4 5 6 7 8 9 10
4. Does she have the Baccalauréat? YES / NO	Guessed/Learned	1 2 3 4 5 6 7 8 9 10
5. What is her country of birth? ____________	Guessed/Learned	1 2 3 4 5 6 7 8 9 10
6. In what arrondissement does she live? If she does not live in Paris, mark "NP": ____________________________	Guessed/Learned	1 2 3 4 5 6 7 8 9 10
7. Is she married? YES / NO	Guessed/Learned	1 2 3 4 5 6 7 8 9 10
8. What is her favorite leisure activity? __________________________________	Guessed/Learned	1 2 3 4 5 6 7 8 9 10

Note: This figure illustrates the speed-chatting quiz that players took immediately after the speed-chatting game.

religious similarity—not ethnic, gender, class, or age similarity—that provides a foundation for association.

This result has important implications for the context in which we ran the experiment because it challenges the social ideology in France according to which religion should have no impact on an individual's behavior in the public sphere. Hence, our results uncover a tension between the norm of French *laïcité* and the way in which individuals associate with one another. This tension might explain why religion has become so taboo in French society, a reality we were confronted with in our own experiments, namely in our speed-chatting game.

Our speed-chatting game provided players an opportunity to get to know one another. At the end of the game, players were given a quiz testing them on the information they were able to collect from their partners. This quiz, illustrated in Figure 5, asked players to identify their partners' age, religion, employment status, education level, country of origin, district of residence, marital status, and favorite leisure activity. Players were allowed to consult the notes they had taken during their speed-chats. For each

TABLE 5
Speed-Chatting Quiz Summary Statistics

Label	Question Asked	Mean	SD	Min.	Max.	*N*	Response Rate	Proportion Learned	Proportion Guessed
q1	How old is this person?	.373	.484	0	1	381	.953	.373	.488
q2	What is his/her religion?	.686	.465	0	1	344	.885	.113	.670
q3	Does s/he work for him/herself?	.863	.345	0	1	357	.978	.705	.175
q4	Does s/he have the Baccalauréat?	.786	.410	0	1	365	.945	.225	.643
q5	In what country was s/he born?	.823	.382	0	1	389	.973	.520	.340
q6	In what district does s/he live?	.590	.493	0	1	356	.903	.495	.303
q7	Is s/he married?	.824	.381	0	1	364	.960	.483	.373
q8	What is his/her favorite leisure activity?	.326	.470	0	1	285	.753	.223	.448
Grade	Sum of q1 through q8	5.207	1.364	1	8	217			

Notes: The unit of analysis is the voter–candidate pair. The table above provides summary statistics for how well voters performed on their quizzes. For each question, voters are coded as "1" if they answered correctly, "0" if they did not, and as missing data otherwise. The response rate provides the frequency with which players answered each question (as opposed to leaving it blank). *Proportion learned* provides the frequency with which a given answer was learned during the speed-chat, according to the quiz-taker. *Proportion guessed* provides the frequency with which a given answer was guessed by the quiz-taker (and thus not learned during the speed chat). *Proportion learned* and *Proportion guessed* do not add up to the response rate because of missing data on these variables, for which respondents were not monetarily incentivized to respond.

question, players provided their response (or selected "Don't Know"), and indicated whether they had learned the information during the speed-chat, or simply guessed the answer. For each correct answer, players were rewarded €1. This reward did not depend on whether players had guessed or learned the information, and we conveyed this to each participant.

We uncover important insights from these data, which we provide in Table 5. First, Table 5 indicates that, while players' performance varies tremendously by question (with a low of 32% correct response for the partner's favorite leisure activity to a high of 86% correct response for whether the partner is self-employed), response rates were high throughout (dipping, at its lowest, to 75% for the partner's favorite leisure activity). In other words, players were responding positively to the monetary incentive we offered. Second, Table 5 highlights which topics were more likely to be discussed during the speed chat by providing the proportion of answers that were learned via the speed-chatting game and the proportion of answers that were guessed by the player. Here, an interesting trend appears. The partner's religion was the most guessed answer in this game, rivaled only by whether the partner obtained her Baccalauréat (the exam that completes French secondary education). Indeed, 67% of answers about the partner's religion were guessed, while a mere 11% were learned during the speed-chat. No other question was so little likely to be discussed during the speed-chat.

The fact that most players guessed their partner's religion underscores the power of French *laïcité* but it also raises a question: might our religious difference variable be a mere proxy for something else? In other words, could difference in religion be correlated with another, more observable and less taboo characteristic?

The most obvious alternative explanation for our result is that differences in religion are correlated with racial or ethnic differences, characteristics which are more observable and—in France—less taboo. Yet the research design for this project called specifically for the decoupling of ethnicity and religion, meaning that different-ethnicity dyads are not necessarily likely to be different-religion dyads. To wit, the correlation between the two is r=.162. Furthermore, based on our interactions with players during the game's registration phase, we did not observe religion-specific accents or differences in speech that might explain our result.

We further investigate whether our *Different religion* variable actually captures social distance in religion by looking at players' guesses about their partner's religious identity. In other words, we examine whether our players correctly guessed whether or not their game partner shared their religion. In Table 6, we show a simple tabulation of correct (in bold) and incorrect guesses by our players. We find that players were correct close to 80% of the time. In other words, although religion was not widely discussed during the speed-chatting exercise, players correctly guessed whether or not their partner shared their religion. Despite laïcité and the accompanying

TABLE 6
Player Guesses about Partner's Co-Religious or Non Co-Religious Identity

	Guessed Same Religion	Guessed Different Religion	Total
Is same religion	**73 (65%)** **(72%)**	39 (35%) (17%)	112 (100%) (34%)
Is different religion	29 (13%) (28%)	**189 (87%)** **(83%)**	218 (100%) (66%)
Total	102 (31%) (100%)	228 (69%) (100%)	330 (100%) (100%)

Notes: The numbers in each cell provide a raw count. The numbers in parentheses to the right of each count provide the row frequency (rounded to the nearest integer). The numbers in parentheses below each count provide the column frequency (rounded to the nearest integer). The numbers in bold above indicate instances where the player guessed correctly that his/her partner is or is not co-religious. In total, players were correct close to 80% of the time.

social taboo, our experiment reveals the tacit importance of religion in French public life.

D. *Is Homophily Rational?*

Is religious homophily in the context of our voting game rational? In other words, do voters rely on religious similarity to elect a leader because they expect greater material reward from a co-religious leader, as is assumed in the ethnic voting literature (Chandra 2004; Horowitz 1985)? To address this question, we analyze two other aspects of our experimental protocol: the leader's allocation decision and the simultaneous trust game.[29]

The Leader's Allocation Decision. After voting for a leader, we asked all players to imagine that they are elected leader, and to provide their allocation decision. On average, players donated €4 to each member of their electorate (or $4 * 5 = 20$ to the entire voting pool) out of their €30 bonus. But a wide variation exists across members of the electorate: some received nothing, others received €20.

We estimate the determinants of leaders' monetary allocations with Equation (2) below:

(2)

$$\text{Allocation} = a + \mathbf{b}_1'.\,(\mathbf{Distance}) + \mathbf{b}_2'.\,(\mathbf{Candidate}) + \mathbf{b}_3'.\,(\mathbf{Voter}) + \mathbf{b}_4'.\,(\mathbf{SessionFE}) + e$$

where Voter–Candidate pair is the unit of analysis and *Allocation* captures the amount Candidate would donate to Voter if s/he were elected leader. **Distance** is a vector of variables that capture the social distance between candidate and voter on a set of sociodemographic characteristics—gender, age, education, family income, ethnicity, and religion; b_1' thus summarizes the effect of the social distance between voter and candidate on the allocation decision. **Candidate** is a vector of control variables (gender, age, education, family income, ethnicity, and religion) that characterize the candidate; b_2' thus summarizes the effect of candidate characteristics on the allocation decision. **Voter** is the same vector of control variables applied to the voter, such that b_3' summarizes the effect of voter characteristics on the allocation decision.[30] Finally, **Session FE** is a vector of game session fixed effects. Standard errors are robust and clustered at the individual voter level, since individual candidate allocation decisions are not independent of one another.[31]

In Table 7, Models (1) through (4), we estimate Equation (2) for each player's allocation decision. The results are clear: religious homophily is not a significant predictor of a

29. Although the experimental protocol also included a dictator game, we unfortunately cannot analyze its results for our purposes here because the recipients in this game were presented pictorially to our players, were not part of our sample, and were thus not players for whom we had collected socio-demographic information such as age, education or income. Furthermore, we did not elicit our players' beliefs about these recipient characteristics.

30. Note that in Equation (1), we controlled only for candidate characteristics but not for voter characteristics. We would want to control for voter characteristics if the vote were optional and we were trying to account for turnout. However, the vote was not optional and all voters voted. This is not true for the leader's allocation, since leaders could choose not to allocate any money at all and keep the entire €30. Therefore, in Equation (2), we include a vector of controls for candidate characteristics to account for this decision. Note that, in Equation (1), if we do include voter characteristics, our results hold.

31. Because our outcome variable is continuous, we present results from linear specifications. In robustness tests, we specify an ordered probit model instead; our results do not change.

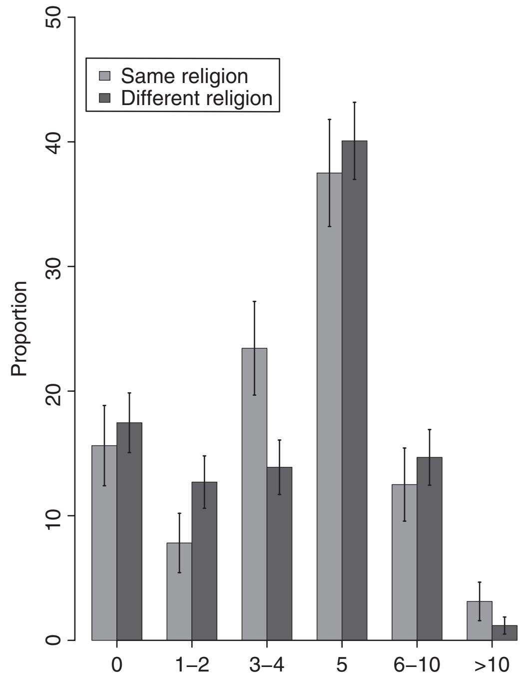
TABLE 7
The Determinants of a Leader's Donations

	DV: Donation			
Variable	**Model (1)**	**Model (2)**	**Model (3)**	**Model (4)**
Different religion	−.027	−.198	−.310	−.484
	(.399)	(.410)	(.435)	(.369)
Different gender	−.283	−.300	−.043	−.967
	(.966)	(.901)	(.757)	(.872)
Different ethnicity	.363	.253	.401	.268
	(.307)	(.258)	(.264)	(.228)
Age difference	−.004	−.013	−.005	−.004
	(.017)	(.017)	(.019)	(.015)
Education difference	.096	.137	.109	.091
	(.088)	(.104)	(.135)	(.110)
Income difference	−.100	−.098	−.107	−.075
	(.091)	(.091)	(.097)	(.087)
Candidate controls	No	Yes	Yes	Yes
Voter controls	No	No	Yes	Yes
Imputed missing data	No	No	No	Yes
R^2	.021	.089	.117	N/A
observations	308	308	308	395

Notes: The table reports OLS estimates. The unit of observation is the voter-candidate pair. The dependent variable, Donation, counts the number of Euros donated by Candidate (if Candidate were to be elected leader) to Voter. All models include session fixed effects. All standard errors are robust and clustered at the individual voter level.

$^{\wedge}$, *, and ** denote statistical significance at the 10%, 5%, and 1% level, respectively.

FIGURE 6
Leader Donation by Same or Different Religion



Note: This histogram illustrates the distribution of leader donations by whether or not voter and leader share a religion. "0" represents a donation of €0, while "5" represents a donation of €5. For example, 16% of leaders in co-religious pairings gave €0, while .8% of leaders in co-religious pairings gave more than €10. The bars are standard error bars (95% confidence level) for the estimates of proportions for each category above.

leader's allocation decision. Although the coefficient is consistently negative, suggesting that leaders are less likely to donate to voters who do not share their religion, it does not reach conventional levels of statistical significance. Figure 6, based solely on descriptive statistics, illustrates the absence of a clear pattern of leader donations toward co-religious versus non co-religious voters. In fact, Figure 6 shows that one focal point emerges: splitting equally between one's constituents and oneself (e.g., giving €5 on average).[32]

Our results show that religious homophily is at stake in the voting behavior but not in the allocation decision. If the same player, as a voter, elects a leader based on religious similarity, but as a leader, rewards voters not based on religious similarity, then the religious homophily we observe in a player's voting decision is unlikely to be rational. Indeed, if voters believe that co-religious leaders are more likely to be generous toward them than non co-religious leaders, then we should observe these leaders allocating more to co-religious voters if they behave in a way that is consistent with their beliefs. Nonetheless, it may be that individuals do not behave in accordance with their beliefs. If this is the case, showing that religious homophily is not at stake in the leader's allocation decision is not enough to claim that religious homophily is taste-based. To better elicit the mechanisms underlying religious homophily, we therefore turn to the analysis of the simultaneous trust game.[33]

The Simultaneous Trust Game. The 2009 simultaneous trust game was the first game participants played in our experimental setup. In this game, subjects sat quietly in a waiting room (they were

32. We thank two anonymous referees for pointing us in this direction in an effort to better understand the absence of results in Table 7.

33. We thank two anonymous referees for pushing us toward leveraging the other games in our experimental protocol to better understand the mechanisms underlying religious homophily.

supervised such that they could not communicate with one another), and were called to a table in pairs. For each pair, one player was assigned the role of "sender" and the other "receiver."

Sender was allotted €3 in her account and could send any amount {0, 1, 2, 3} to receiver by marking this amount on a sheet that receiver would never see. As part of the instructions, senders and receivers learned that the amount sent by the sender would be tripled. For example, if sender sent €1 to receiver, receiver would receive €3; if sender sent €2, receiver would receive €6; and if sender sent €3, receiver would receive €9. Simultaneously (and without ever knowing how much was actually sent by the sender), the receiver offered to return a fraction {0, 1/3, 2/3, 1} of the amount received.[34] For example, if receiver sends back 1/3 after sender offers €1, sender ends up with €2 from the initial allocation plus one third of €3 from the transaction, for a total of €3; receiver, for her part, ends up with €3 from the sender minus €1 sent back, for a total of €2. After each play, sender and receiver returned to the waiting room, not knowing if they would be called again, or in what role.

Experimental economists commonly consider the amount sent by the sender in the trust game as a sign of trust, hence the game's name. Indeed, if the sender trusts the receiver, i.e., if the sender believes that the receiver will be generous enough to send back a strictly positive share of the amount received, then the sender should send the totality of the initial endowment (€3) so as to maximize the amount returned. At the very least, if receiver sends back the lowest strictly positive option, the sender ends up with the same amount she originally had. However, if the sender does not trust the receiver, i.e., if she believes that the receiver will not send anything back, then the sender is better off keeping the €3. These instructions were carefully relayed to our players: the sender's decision reflects her trust toward the receiver.

The amount sent back by the receiver in this simultaneous trust game, on the other hand, is most plausibly interpreted as a signal of unconditional altruism (receiver's generosity regardless of sender's action) and/or belief-based reciprocal altruism (receiver's generosity conditional on receiver's beliefs about sender's action).

Our simultaneous trust game therefore allows us to further disentangle between rational versus non-rational motivations underlying religious homophily. If religious homophily is rational, we should observe it in both sender and receiver behavior. If, on the other hand, religious homophily is non-rational—or taste-based—we should observe it only in the receiver's behavior.

We designed the simultaneous trust game to maximize interactions among a sub-sample of players whose behavior we studied in our larger project on immigrant integration. Namely, we focused on interactions between FFF, Senegalese Muslims (SM), and Senegalese Christians (SX). Our Senegalese Muslim and Christian players allow us to draw clean inferences about the effects of co-religion: these participants hail from immigrant families with similar socio-economic characteristics, who arrived in France at the same time for the same economic reasons. They share a national identity and an ethnicity. What differentiates them is their religious identity, a factor that FFF could infer from players' first names—which were pasted on their name tags on their chests. In what follows, we compare the behavior of FFF players toward co-religious SX to that of FFF players toward non co-religious SM.[35]

We estimate the following model:

(3)

$$\begin{aligned}\text{Amount} = a &+ b_1 . (\text{FFF} \rightarrow \text{SM}) \\ &+ b_2 . (\text{SM} \rightarrow \text{FFF}) + b_3 . (\text{SX} \rightarrow \text{FFF}) \\ &+ \mathbf{b'_4} . (\mathbf{Player1characteristics}) \\ &+ \mathbf{b'_5} . (\mathbf{Player2characteristics}) \\ &+ \mathbf{b'_6} . (\mathbf{Distance}) \\ &+ \mathbf{b'_7} . (\mathbf{SessionFE}) + e\end{aligned}$$

where the Sender–Receiver pair in the simultaneous trust game is the unit of analysis. *Amount*

34. The novelty of our simultaneous trust game with respect to the original trust game introduced by Berg, Dickhaut, and McCabe (1995) is in the simultaneity of the decisions made by the sender and the receiver. In the original trust game, the amount sent by the sender is revealed to the receiver before the receiver decides how much to send back. We opted for the simultaneous trust game over the original trust game for several reasons. Our objective was to treat each trust game played by our subjects as a one-shot game in order to mimic random everyday life encounters between strangers. We thus needed to avoid reputation effects. Furthermore, the simultaneity brings a touch of realism since most interactions in real life happen under incomplete information. In this respect, removing sequentiality in the decision process seems less artificial. Finally, we needed to avoid contagion between games: players' behavior in subsequent games in our experimental protocol would have been biased by information gleaned from the trust game.

35. All FFF players were of Christian tradition.

represents the amount transacted, i.e., the amount sent or the amount returned. FFF → SM represents a transaction between FFF as Player 1 and SM as Player 2. SM → FFF represents a transaction between SM as Player 1 and FFF as Player 2. SX → FFF represents a transaction between SX as Player 1 and FFF as Player 2. The omitted category is the transaction between FFF as Player 1 and SX as Player 2. When analyzing the amount sent by the sender, Player 1 is the sender and Player 2 is the receiver. When analyzing the amount returned by the receiver, Player 1 is the receiver and Player 2 is the sender. **Player 1 characteristics** is a vector of controls for Player 1, namely gender, age, family income, and education. **Player 2 characteristics** is a vector of the same controls for Player 2. **Distance** is a vector of social distance variables for gender, age, family income, and education between Player 1 and Player 2.[36] **Session FE** is a vector of session fixed effects. Standard errors are robust and clustered at the individual player level.

In Table 8, Model (1) estimates Equation (3) for the sender's decision and Model (2) estimates Equation (3) for the receiver's decision. Our results indicate that, as senders, FFF do not discriminate between co-religious SX and non co-religious SM. However, as receivers, FFF send back significantly less to non co-religious SM than to co-religious SX. We thus observe co-religious homophily in the amount sent back but not in the amount sent.[37] The fact that FFF do not discriminate as senders reveals that FFF do not expect that players who do not share their religion will behave differently, e.g., send back less money, than players with whom they share a religion. This logic extends to the voting game: FFF are unlikely to be motivated by the belief that leaders with whom they share a religion will reward them more than leaders with whom they do not share a religion. The analysis of the simultaneous trust game allows us to rule out the possibility that the voter's decision in the voting game might be based on her expectations about the allocation decision of the candidates once these candidates are elected. Our analysis of the simultaneous trust game provides evidence consistent with a taste-based mechanism underlying hemophilic preferences.

TABLE 8
The Simultaneous Trust Game

Variable	Amount Sent Model (1)	Amount Sent Back Model (2)
FFF → SM	−.301	−.177^
	(.205)	(.099)
SM → FFF	−1.103**	−.176
	(.408)	(.182)
SX → FFF	−1.049**	−.129
	(.272)	(.147)
Sender controls	Yes	Yes
Receiver controls	Yes	Yes
Distance controls	Yes	Yes
Session FE	Yes	Yes
R^2	.384	.176
Observations	83	82
Wald test: SM → FFF = SX → FFF	$p = .886$	$p = .811$

Notes: The table reports OLS estimates. The unit of observation is the sender–receiver pair in the simultaneous trust game. The omitted category is the transaction from FFF to SX. The dependent variable in Model (1) is the amount sent by the sender and in Model (2), the amount sent back by the receiver. All models include session fixed effects. All standard errors are robust and clustered at the individual voter level.

^, *, and ** denote statistical significance at the 10%, 5%, and 1% level, respectively.

We infer from this that while voters prefer a leader of their own type, the monetary implications of this preference, either in expecting to receive or in distributing to others, are insignificant. Thus, we see a taste for co-religionists without any material implications. Bringing together our results from the simultaneous trust game and from the voting game, it seems that taste-based discrimination emerges in the form of lower unconditional altruism before our speed-chatting game. But after the speed-chatting game, which involves some form of socialization, such discrimination appears to be more subtle: players simply seek to associate with people with whom they share a religion, though they are no longer more altruistic toward them (as our absence of results for the leader's allocation decision indicates). We note, however, that this interpretation can only be suggestive: it is based on only a subsample of FFF, SM, and SX players and thus on fewer than 100 observations.

36. Note that we do not control for distance in ethnicity; this is because, by construction, distance in ethnicity is held constant. Since we analyze only interactions between FFF and SM on one hand, and FFF and SX on the other, the Sender–Receiver pair is always a different-ethnicity pair.

37. Furthermore, we observe that SM do not behave differently toward FFF than do SX, either as senders or as recipients (see the Wald test for whether SM → FFF = SX → FFF at the bottom of Table 8). Yet taste-based religious homophily would predict that SM send back less. This null result may be driven by the fact that, as we reported in our robustness check, religious homophily is driven primarily by FFF players.

IV. CONCLUSION

This paper offers a new method for measuring which social dimension matters most in individuals' decisions to associate with one another. It introduces a voting game in which participants do not previously know one another. Investigators exogenously impose the set of partners with whom each participant must interact, and the measured outcomes capture how individuals associate with one another through a vote choice. Elected leaders then receive money from the experimenter and decide whether and how much of this amount they want to share with their electorate. The paper then gauges which social dimension—ethnic, class, religious, gender, or age—is significantly associated with vote choice. Furthermore, it measures whether or not this association is rational, i.e., whether players rely on the same social dimension to vote and to allocate their monetary reward. Our experiment reveals that, in the context of our voting game in the 19th district of Paris in 2009 and pitting the most common socio-demographic bases for homophily against one another, religion is the sole significant predictor of participants' vote choice: voters are only 17% likely to vote for a non co-religious candidate, but close to 30% likely to vote for a co-religious candidate. This effect stands even after controlling for candidate characteristics, and it persists through a number of robustness checks. No other social matchup, be it gender, age, education, ethnicity, or socio-economic similarity, has similar predictive power. Moreover, our results suggest that religious homophily is taste-based: while religious similarity drives vote choice, it does not determine allocation decisions, nor does it affect players' beliefs about others' altruistic behavior. In short, voters seem to vote for those with whom they share a religious belief, without any expectation of garnering a material benefit from it.

Our results have implications, both substantive and methodological. Substantively, our results have three implications. First, scholars have shown that homophily can threaten social cohesion (Centola et al. 2007; Fernández and Rogerson 2001; Golub and Jackson 2012); understanding which social dimension significantly predicts homophily is an important prerequisite to limiting homophily's potential deleterious effects. We have shown one way in which this can be measured, and uncovered the salience of religious similarity in one context. Second, our results confirm that discrimination need not be a general orientation toward difference; rather, it can target specific groups. In our French case the target is based on religious heritage, which offers a strong basis for homophily on one side, and for discrimination on the other. In other words, if religion is a significant predictor for how individuals associate, it can also predict how individuals discriminate (e.g., Adida, Laitin, and Valfort 2010). Finally, we show that religious homophily dominates even in a country that prides itself on its *laïcité*, and which imposes—both legislatively and culturally—strict boundaries between religion and public life. Indeed, religion has always been a strong undercurrent in French society: the voting patterns in the Fifth Republic, for example, have been well-predicted by the Church-going behavior of French voters (Berger 1974). In a country that has, for centuries, attempted to legislate its way to freedom from religion, religion remains one of the main organizing principles in at least one slice of French society today.

On the methodological front, we offer an individual-level, experimental method for measuring the ex ante salient socio-demographic basis of homophily. Research has shown that ethnicity, age, class, gender, and religion all matter in one context or another (McPherson, Smith-Lovin, and Cook 2001). Yet most previous work on homophily cannot identify which socio-demographic characteristic emerges as the basis for homophily. Indeed, by measuring either established relationships or individual choices of partners based on a set of options that has already undergone some selection process (typically relying on homophily), research to date has suffered from selection bias. Our method, which is easily replicable in other contexts, overcomes this problem by relying on an experimental framework where investigators exogenously impose the set of partners to each individual player. In doing so, we can identify the social basis for homophily in one context.

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