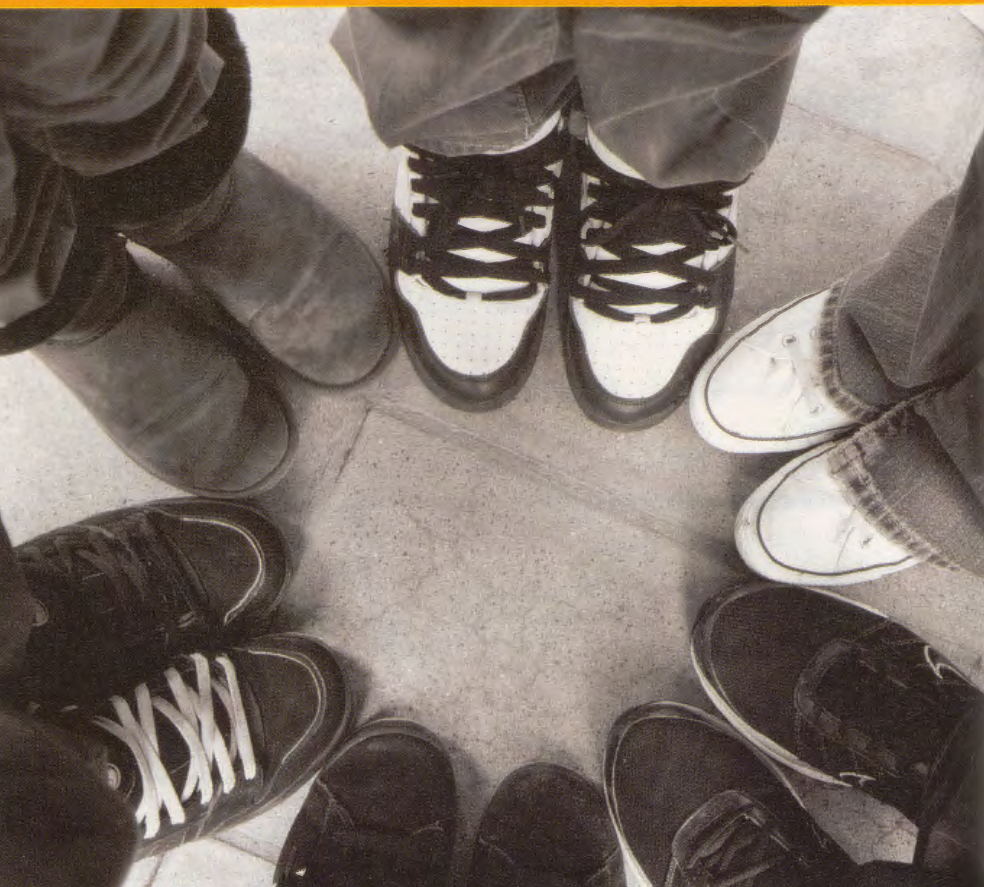


Peer Effects



On Political Attitudes of Refugees in Chicago

BY BRYAN HO WEI HAO

“No man is an island, entire of itself; every man is a piece of the continent, a part of the main.”

—Meditation XVII, *John Donne*

I. Introduction

When it comes to our decisions and identities, the popular conception today is shifting from one of individual rationality and fixed preferences to one of shifting preferences that are highly susceptible to social influences. Distinguishing peer influences from other effects is difficult because social networks tend to be highly polarized: like-minded individuals tend to cluster together, making it hard to disentangle social influences from other effects. Here, theory has advanced beyond empirical analysis by pinpointing three sources of error. The first error is due to *endogenous group membership* or *homophily*—or as the popular saying goes, “birds of a feather flock together”—individuals tend to choose with whom they want to associate and converse. Furthermore, within a relationship dyad it is often hard to say which individual is influencing the other; since these *reflection problems* are a result of the individuals’ simultaneous interactions, they are also known as *simultaneity problems*, which make it particularly hard to distinguish between

endogenous and exogenous social influences. Finally, it is likely that unmeasured *contextual effects* affect entire networks and are responsible for some of the observed correlations between network members. To what extent is it possible then to overcome these problems and answer the question: "Do our social environments influence who we are and how we view the world?"

This paper studies the effect of peer influences on refugees' political attitudes in cases where they were randomly assigned to live in Chicago. The United Nations' process of randomly assigning refugee "free cases" to host countries, followed by individual national governments assigning these refugees to different states and cities, yields a natural experiment in which refugee "free cases" are randomly assigned to new social networks, providing the theoretical conditions necessary to overcome the three econometric identification problems mentioned above. As part of their integration process, all refugees must go to one of a handful of clinics that cater specially to refugees for regular checkups, screenings, and health certifications. In this study, I gathered original field data, using a survey instrument administered to refugees at the Touhy Health Center in Chicago. I scored their responses to a series of questions about political attitudes on a six-point scale along two dimensions, authoritarian-libertarian for personal issues, and left wing-right wing for economic issues, and I asked them to imagine their peers' responses to a similar reduced set of questions for three peers that they are close to and talk to often. To determine if their political attitudes have changed since their arrival in the United States, I asked them about past political affiliations or news consumption choices (newspapers, radio channels, etc.)¹ in their countries of origin, and scored these along the same six-point scale. Finally, I ran ordinary least squares regressions of their change in political attitudes on their peers' average political attitudes and their own previous attitudes. I also compared the results with a "control group" of refugees who

1. For more on the correlation between individuals' political beliefs and their media choices, see Gentzkow & Shapiro (2006).

were not randomly assigned to Chicago but chose to come here as "family reunification cases."

I found positive and significant effects of peers' political attitudes on respondents' change in political attitudes, measured from the time they left their home countries to the time of their interview here in the United States. In particular, I found that the wider the gap between the respondents' initial beliefs and those of their peers, the greater the peer influence effect on their change in political attitudes. I will show that we can interpret the coefficients causally, as the randomized refugees' prior political attitudes are uncorrelated with those of their current peer groups, whereas those of the nonrandomized refugees were already significantly correlated with their peers' political attitudes. Chow tests also suggested that the peer effects coefficients are different for randomized versus nonrandomized refugees.

The results are particularly suggestive for the role of policy in the integration of refugee populations into American society. The evidence that refugees are influenced by their peers is qualified by the large extent to which refugees recreate and maintain largely homogenous racial and ethnic social groups. This suggests that for most refugees, their absorption of American culture and values and consequently their integration into American society may be very slow, if it happens at all. However, refugees who do not manage to recreate peer groups that preserve their prior beliefs may be deeply vulnerable to undesirable peer effects, including dangerous influences from extremist or criminal organizations. Overall, these implications support a more active public policy stance towards the social integration of refugees and perhaps immigrants in general.

This paper is organized as follows. Section II reviews the literature. Section III describes the refugee resettlement process and the data I obtain, before presenting the econometric model and survey methodology. Section IV presents and discusses the results of the peer effects model, and reexamines the statistical validity of the randomization process. Section V concludes and discusses further research directions, as well as direct policy implications and possible policy implementations.

II. Literature and Theoretical Framework

The age-old debate of “nature versus nurture” in shaping who we are has increasingly become a subject of study by social scientists. Ever more sophisticated studies have been emerging as the result of the cross-fertilization of ideas and methodologies across the various social science disciplines: sociologists borrow graph theory to map information flows across “social networks”; political scientists use econometric models to introduce quantitative rigor into political behavioral models; economists no longer taking preferences as given are beginning to investigate how social and individual preferences are formed and modified over time.

The earliest models of political preference formation focused exclusively on the individual at the expense of social interaction effects. Still one of the most widely accepted explanations of voting behavior, the Michigan voting model (Campbell et al., 1960), gives rise to the “standard Michigan causal sequence” put in terms of long- and short-term influences:

Socioeconomic state → Party identification → Issues and candidates → Vote
 [Long term]-----[Short term]

In contrast, one of the earliest cross-disciplinary empirical studies, the Columbia voting model, conceptualizes perceptions of the political world primarily in network terms (Berelson et al., 1954). The major insight of this model is that “face-to-face” contacts are essential in opinion formation, where the majority of people form homogenous political discussion networks and thus retain their political identity, while the minority of people who change their minds are socially and politically isolated, which thus makes them flexible and susceptible to political persuasion in the short-term. However, Berelson, et al. (1954) did not go much further than superficial comparisons of summary statistics in the formulation of their descriptive models.

An early attempt at blending both the Michigan and Columbia models by incorporating regression analysis to link peer effects and

voting choice was made by Knoke (1990). In particular, Knoke develops the idea that “egocentric networks,” or “egonetworks,” describe how information and influence flow from “alters” (e.g., spouses, parents, close friends) to ego (oneself). The standard approach to measuring peer effects has since been loosely based on this idea, in which observational data is used in regressions of own outcomes on peer outcomes.

However, Manski (1993) points out that studies of this nature are consistently hampered by three main biases that make identification of peer effects close to impossible. First, individuals generally self-select into neighborhoods, friendship groups, and other communities — Moffitt (1998) terms this “endogenous group membership.” Second, if individual i and individual j affect each other simultaneously, it becomes difficult to separate out the actual causal effect of i 's outcome on j 's outcome (“simultaneity”).² Third, it is hard to distinguish peer effects if an unobserved variable affects entire groups differentially (“contextual effects”³), and if individual's true characteristics are unobserved and correlated across individuals in the same group (“errors-in-variables”).

Researchers have attempted to address these problems in two main ways. The first group of researchers searched for either structural models or instruments that are assumed to be exogenous to analyze quasi- or natural experiments. Examples include Case and Katz (1991) and Gaviria and Raphael (1999) using the average behavior of peers' parents as an instrument for peer behavior. Similarly, in their study of the dynamic spread of happiness in a network, Fowler and Christakis (2008) attempt to control for endogenous group membership by including peers' happiness in previous medical exams. However, critics of this first group of studies rightly find it difficult to be certain about the exogeneity of the instruments or the ability of the structural models to address self-selection problems and deliver unbiased estimates of peer effects.

2. Manski terms this the reflection problem.

3. Moffitt terms this the correlated unobservables problem.

The second group of researchers follows the example of Sacerdote's (2001) study which takes advantage of the random assignment of Dartmouth College freshmen to their dormitories and roommates in order to study peer effects on grade point average, decisions to join fraternities, and choice of college major. Likewise, Kremer and Levy (2008) make use of random assignment of dorms and roommates to study peer effects of alcohol use and GPA in a large, academically strong, state university. Zimmerman (2003) studies peer effects for Williams College student GPAs, and Foster (2003) carries out a similar study at the University of Maryland. While in theory these models come as close to controlled randomized experiments as can be found in a nonexperimental setting, it must be emphasized that these natural experiments face certain constraints. First, there is often a gap between the randomization researchers like to believe occur in these colleges and what is actually practiced at the dorm level: graduates from these institutions⁴ often report that there is a less than random process by which they are assigned roommates, normally based on resident heads' assessments of individual personality traits and other idiosyncratic preferences—in other words, characteristics unobserved by the researcher. Second, most of these studies do not control for the amount of interaction that actually occurs between roommates and dorm mates. Dorm residents are, after all, not obliged to spend all or even most of their time there, and are certainly not obliged to socialize in meaningful ways with their roommates. Third, few of these studies posit how exactly the peer effects "cause" the outcomes in their study populations. A notable exception is Kremer and Levy (2008) which presents a convincing case for habit formation based on the cue theory of consumption that they borrow from Laibson (2001). Finally, the study population of generally intelligent, well-educated, middle- to upper-class youth between the ages of eighteen to twenty-two, is far from randomly selected, making generalizability beyond this study population of college students impossible.

4. A case in point being one of my own thesis preceptors, who is a graduate of Williams College.

While I explore promising avenues for the first approach in terms of exogenous instruments, this paper aims to contribute to this new and evolving body of research by focusing mainly on the second random assignment approach. I combine elements from traditional network analysis studies with natural experiment methods within the context of studying a population of particular policy interest. First, instead of assuming that subjects interact socially with people within close proximity, such as roommates, I ask subjects to name three people that they do in fact interact with frequently. Second, I make use of the random assignment of refugee “free cases” to their final resettlement destinations versus the nonrandom assignment of refugee “family reunification” cases to their resettlement locations to create the conditions of a controlled and randomized “natural experiment” in order to isolate and analyze the existence of peer effects. Finally, the issue of refugees, immigrants, and other transplanted peoples has been a renewed focus of policy debate in the face of global terrorist attacks in the United States in 2001, Madrid in 2004, and London in 2005, to name a few. These attacks were largely carried out by foreign-born individuals who had lived in these countries for some time, or in some cases, by individuals born in these countries to immigrant parents. It is widely believed that these individuals, who mostly come from unremarkable middle-class backgrounds, become radicalized only late in their lives due to influence by radical elements within their ethnic or religious groups. An understanding of how transplanted people are affected by or remain immune to their new social environments within the United States will thus be crucial in formulating better policy.

III. Empirical Framework

A. Data Description and Setting

Resettled refugees are a select group of people. Of 42 million people forcibly uprooted by conflict and persecution in 2009, 16 million are

considered “refugees or asylum seekers;”⁵ of these, about 10.5 million are recognized as refugees by the United Nations High Commissioner for Refugees (UNHCR). Finally, of all these refugees, less than 1 percent are ever resettled in one of the ten countries⁶ that collaborate with the UNHCR on refugee resettlement — the vast majority (more than 80 percent) remain in the country where they had sought initial protection, usually a bordering country, while the remainder are repatriated to their countries of origin.

The United States receives about 50 percent of all the refugees identified by the UNHCR within a given year. In recent years, this has translated into around 80,000 refugees each fiscal year, although in previous years well over 100,000 were admitted each year. Refugees qualify for an interview with the U.S. refugee program if they meet any of three “Priority Categories.” Priority One cases are individual referrals, also known as “free cases”: these are UNHCR-referred, nongovernmental agency (NGO) or embassy-identified persons of any nationality in urgent need of protection and for whom no other durable solution exists. Priority Two cases are group referrals, also known as “people in the national interest” of the United States, and can be defined as specific nationalities, clans, ethnicities, religions, locations, or combinations of such characteristics. Current groups in this category are people from the former Soviet Union, Cuba, Vietnam, and Iraq, Burmese refugees in Thailand and Malaysia, Burundians in Tanzania, Bhutanese in Nepal, Iranian religious minorities, Sudanese Darfurians in Iraq, and Eritreans in Shimelba. Priority Three cases are family reunification cases, and are only applicable

5. The legal distinction between refugees and asylum-seekers is that the latter (illegally) enter the country that they wish to resettle in; refugees are resettled by intra-governmental agencies working with foreign governments.

6. In order of refugees received, these are the United States, Canada, Australia, Norway, Sweden, New Zealand, Finland, Denmark, and the Netherlands. Several other countries accept smaller numbers of refugees: these include England, Ireland, Belgium, Germany, Spain, Brazil, Argentina, Chile, and Mexico.

to designated nationalities. This list of nationalities is determined at the start of each fiscal year, and the refugee must have an "Affidavit of Relationship" (AOR) filed on his or her behalf by an "anchor" relative residing in the United States.

The refugee resettlement process as actually executed on the ground is far from orderly. UNHCR and embassy officials walk through refugee camps and attempt to select individuals or families for interviews based on how vulnerable they appear, whether they seem likely to adapt to a foreign environment, how healthy they look, etc.⁷ As can be observed from the number of refugees that arrive in the United States with no English skills whatsoever, it is evident that no uniform standard is applied. The assignment of refugees to resettlement countries is a result of each country's prevailing policies and refugee quotas, and the consequent negotiations between the countries with each other and with the UNHCR. Finally, if the refugees are assigned for resettlement in the United States, the Refugee Council USA distributes the refugees among ten refugee volunteer agencies,⁸ or "volags," which then assign the refugees to their state-level organizations. With the exception of the Priority Three (family reunification) cases, refugees have no say at all on where they end up. Again excluding Priority Three cases, the end result of this resettlement process is, in effect, a randomly drawn refugee population that is distributed among the ten resettlement countries, and in the case of the United States, randomly distributed within states.

Since 1975, over 133,000 refugees have resettled in the Chicago area. In 2008, Chicago resettled 2,412 refugees, and in 2009, 2,800 refugees were expected. Upon arrival in Chicago, the volags receive the refugees⁹

7. Based on anecdotal evidence from resettled refugees, it seems that selection is based on a sort of reverse triage, where those most in need are seldom selected.

8. Until 2009, there were nine volags and the state of Iowa; as of 2010, a tenth volag was added, while the state of Iowa is terminating its role.

9. Again excluding "family reunification cases": these refugees are mostly received by their family members and live with them for the first couple of months.

and bring them to their new homes; over the course of subsequent months, they receive classes on local culture, customs, and the English language and are eventually found jobs. More random variation arises in two of these processes, namely in housing assignment and job assignment. Volags assign refugees to furnished homes based primarily on what is currently available; only in very rare instances, where volags have multiple housing units available, do they attempt to group the refugees by ethnicity and/or country of origin. The volag-mediated job search process is equally subject to exogenous factors. Although volags attempt to place the refugees in available jobs at the highest salary possible, ideally with health benefits, they are highly constrained in their ability to match refugees to the kinds of occupations they held in their countries of origin. Instead, volags frequently end up getting the refugees on to the lowest occupational rung, e.g., waiting tables, bagging groceries, etc. Refugees however generally attempt to make their way back up the occupational ladder by seeking recertification in their areas of expertise; for example, many of the Burmese refugees were originally teachers, and many Iraqi refugees were veterinarians, dentists, etc. Their final success on recovering their former socioeconomic status is thus highly dependent on the speed of recertification of different occupations, the state of the economy, and other market conditions.

The effects of these exogenous variations that affect the refugees' housing and job assignments form the basis of the theoretical model, and will be explored in the next section.

B. Methodology

Underlying my analysis is the simple framework that describes an individual's characteristic of interest as the result of his or her own background, as well as the influence of his or her peer's background and characteristic of interest. In more explicit terms, y_{ig} is the outcome variable of interest — i.e., political attitudes — for individual i in group g , x_{ig} is individual i 's own individual socioeconomic and ethnic characteristics, $y_{(-i)g}$ and $x_{(-i)g}$ are the averages of the corresponding characteristics of the other indi-

viduals in individual i 's peer group g and u_{ig} is an unobservable variable. Assuming linearity for the relationship, the true structure is thus

$$y_{ig} = \theta_0 + \theta_1 x_{ig} + \theta_2 y_{(-i)g} + \theta_3 x_{(-i)g} + u_{ig} \quad (1)$$

It can be seen from equation (1) that there are two sources of social interaction effects: θ_2 which represents the presence of *endogenous* social interactions, i.e., social interactions that arise because of the alters' characteristic of interest; and θ_3 which represents the presence of *exogenous* social interactions, i.e., those that arise out of the alters' socioeconomic and other unique characteristics. Thus all peer effects studies that claim to have identified the "effect" of one individual on another individual explicitly or implicitly claim to have identified θ_3 which is the "pure" or exogenous social effect. However the coefficients of the structural form are difficult to identify due to three problems; the structure of my approach is thus based on attempting to resolve these three problems.

i. The Simultaneity Problem

The simultaneity problem arises if person 1's actions affect person 2's actions and vice versa, such that it is impossible to determine the direction of causality: to borrow Manski's (1993) analogy in what he terms the "reflection problem," does the person cause his reflection in the mirror to move or is it the other way around? In a case of apparent simultaneity of cause and effect, this is hard to tell.

The salient points of this model can be described as follows. Firstly, assume a simple two-person model. Thus the structural equations are

$$y_{1g} = \theta_0 + \theta_1 x_{1g} + \theta_2 y_{2g} + \theta_3 x_{2g} + u_{1g} \quad (2)$$

$$y_{2g} = \theta_0 + \theta_1 x_{2g} + \theta_2 y_{1g} + \theta_3 x_{1g} + u_{2g} \quad (3)$$

For this model, we assume only that u_{1g} and u_{2g} are orthogonal to both x_{1g} and x_{2g} and that group membership is exogenous. Equations (2)

and (3) constitute a simple linear simultaneous equations problem; considering the reduced form in (4) and (5), we can see that the parameters in (2) and (3) are not identified:

$$y_{1g} = \alpha + \beta x_{1g} + \gamma x_{2g} + v_{1g} \quad (4)$$

$$y_{2g} = \alpha + \beta x_{2g} + \gamma x_{1g} + v_{2g} \quad (5)$$

such that

$$\alpha = \theta_0(1 + \theta_2)/[1 - \theta_2^2] \quad (6)$$

$$\beta = (\theta_2\theta_3 + \theta_1)/[1 - \theta_2^2] \quad (7)$$

$$\gamma = (\theta_2\theta_1 + \theta_3)/[1 - \theta_2^2] \quad (8)$$

$$v_{1g} = (u_{1g} + \theta_2 u_{2g})/[1 - \theta_2^2] \quad (9)$$

$$v_{2g} = (u_{2g} + \theta_2 u_{1g})/[1 - \theta_2^2] \quad (10)$$

Two solutions are possible. First, randomization would break the covariance of the error terms u_{1g} and u_{2g} and hence would allow us to assume that all correlations of y among individuals in a group arise from social interactions. This approach is the focus of the paper. Second, identification of parameters in simultaneous equations problems could be made possible by the use of the exclusion condition rule, i.e., excluding at least one exogenous variable from each equation permits us to identify the structural parameters. A "partial population experiment" provides such a condition. An exogenous variable or "instrument" is provided by policy variable p_{1g} that affects only individual 1 and is independent of the unobservables in the model. Then we can replace equation (2) with

$$y_{1g} = \theta_0 + \theta_1 x_{1g} + \theta_2 y_{2g} + \theta_3 x_{2g} + \theta_4 p_{1g} + u_{1g} \quad (11)$$

The absence of p_{ig} in equation (3) permits all parameters in the model to be identified. This can be seen in the new reduced form equations:

$$y_{1g} = \alpha + \beta x_{1g} + \gamma x_{2g} + \sigma_1 p_{1g} + v_{1g} \quad (12)$$

$$y_{2g} = \alpha + \beta x_{2g} + \gamma x_{1g} + \sigma_2 p_{1g} + v_{2g} \quad (13)$$

such that

$$\sigma_1 = \theta_1 / [1 - \theta_2^2] \quad (14)$$

$$\sigma_2 = \theta_1 \theta_2 / [1 - \theta_2^2] \quad (15)$$

where α , β , and γ are the same as in (6), (7), and (8).

Two measures of this difference in socioeconomic status were attempted. First is the simple difference in occupation and income before and after arrival in the United States, which is based on the assumption that refugees' relative success in obtaining their former jobs is completely based on luck. The instrument p_{ig} that affects only certain members of each peer group is thus the difference in socioeconomic status in terms of occupation and income, since some will succeed in getting an occupation and income similar to what they had in their countries of origin (in which case p_{ig} is zero), while others will fail in their efforts and effectively suffer a "tax" on their socioeconomic status (i.e., p_{ig} is negative). Furthermore empirical studies by Berelson et al. (1954) show that higher socioeconomic status is correlated with certain political leanings, e.g., being more likely to be Republican than Democrat; thus, the relative success (or lack thereof in this case) of refugees to regain their former socioeconomic status acts like a partial tax that arbitrarily affects some refugees but not others.

However it may be the case that this would bias the estimate in favor of refugees who had lower socioeconomic statuses previously, since it would be easier for them to regain their former occupations. As such, the

estimator was compared to a second instrument, namely the state of the economy as represented by the unemployment rate in the year of the refugee's arrival. The state of the economy does not discriminate against individual refugee's characteristics, and can be assumed to be orthogonal to all other variables. In this case, the state of the economy in the year of arrival will act as an instrument for the refugee's current socioeconomic status. Using this approach it is possible that not all the structural parameters will be identified however, and only the reduced form coefficients may be recovered. Still, although we will not be able to tell if the social interactions are of the endogenous or exogenous type, the reduced form coefficient γ will identify the presence of any type of social effects, for if $\theta_2 = \theta_3 = 0$ then $\gamma = 0$, which will be informative in and of itself.

Both approaches showed potential. For the first approach, I ran an ordinary least squares regression of *auth_changed* and *left_changed* (changes in authoritarian and left-wing attitudes respectively) on *se_higher* (an indicator variable for refugees' whose socioeconomic status is better now than in their home country), the coefficients are negative and significant at the 1 percent level, indicating that higher socioeconomic status is inversely correlated to being more authoritarian and left-leaning, which is consistent with Berelson et al.'s (1954) hypothesis. Similarly for the second approach, I regressed *se_lower* (an indicator variable for refugees' whose socioeconomic status is lower now than in their home country)¹⁰ on *unemp_rate* (the annual unemployment rate as provided by the Bureau of Labor Statistics), and I find that the coefficient on *unemp_rate* is positive and significant at the 5 percent level, although the F-statistic was 6.18 indicating that the instrument may still be weak.¹¹ Ultimately however I abandon this approach because although I can measure changes in subjects' exogenous characteristics, due to the limitations of the survey instrument, I could not measure changes in their

10. The omitted category is refugees' whose socioeconomic status has not changed.

11. For complete tables of regression results, please refer to Appendix I.

peers' variable of interest (i.e., their previous political attitudes), hence no further meaningful analysis could be carried out.

ii. The Contextual Effects Problem

Also known as the "correlated unobservables" problem, this problem arises if there is some group-specific component of the error term that varies across groups and is also correlated with the exogenous characteristics of the individuals (x_{ig}) (Manski 1993). There are two generic sources of such correlated unobservables: firstly, self-sorting and endogenous group membership, i.e., similar individuals grouping together; and secondly, groups of individuals being affected by unobserved common environmental factors. The first type of common unobservable can be addressed by the randomized group assignment treatment since it eliminates the intragroup correlations that arise from endogenous group membership. For the second type of common unobservable, I reach the same conclusion as Manski (1993): "Identification is not so simple and indeed, it is not even clear what the object of estimation is." Due to the small dataset (total $n=41$), I am unable to deal with this problem in a similar fashion to Sacerdote's (2001) in which he controls for dorm-level effects (such as noise pollution, etc.) by adding dorm-specific fixed effects. Ultimately however, I do not consider this a significant source of error for this study as I compare two similar groups of refugees whose main source of variation arises from their random versus nonrandom assignment to resettlement locations in Chicago, hence environmental factors should be consistent over both groups.

iii. Endogenous Group Membership Problem

One of the most familiar problems in studies of this nature, the endogenous group membership problem has alternatively been described as the problem of homophily and of self-selection into groups. Moffitt (1998) finds the simplest way to set up the model is by borrowing the framework of the two-equation switching regression model of econometrics consisting of an equation for outcomes y_{ig} conditional upon group membership

assignment of the population and a further equation for the group membership assignment itself. The first equation is familiar to us as the structural form, which is

$$y_{ig} = \theta_0 + \theta_1 x_{ig} + \theta_2 y_{(-i)g} + \theta_3 x_{(-i)g} + u_{ig}, \quad (16)$$

and the reduced form is also linear and is

$$y_{ig} = \alpha + \beta x_{ig} + \gamma x_{(-i)g} + v_{ig}. \quad (17)$$

The second equation describes the utility to individual i from locating in group g conditional on the locational decisions of the rest of the population and hence conditional on mean exogenous characteristics $x_{(-i)g}$ and mean structural residuals $u_{(-i)g}$ in each group such that these residuals are observed by individual i but not by the econometrician. The utility function is thus

$$U_{ig} = f(x_{ig}, u_{ig}, x_{(-i)g}, u_{(-i)g}) + \eta_{ig} \quad (18)$$

subject to the following decision rule:

$$\text{individual } i \text{ chooses location } g \text{ iff } U_{ig} > U_{ig'} \quad \forall g'. \quad (19)$$

Similarly to the simultaneity problem, the structure of this problem implies that a class of solutions exists, namely exclusion restrictions. In this case, to identify at least the coefficients in the reduced form equation (17), a variable is needed such that it appears in equation (18) but not equation (17). Moffitt (1998) proposes that a randomized trial of a policy intervention offering subsidies to locate in a certain group g that differs across individuals i is sufficient for this purpose. Denoting this subsidy as b_{ig} , this gives us

$$U_{ig} = f(x_{ig}, u_{ig}, x_{(-i)g}, u_{(-i)g}, b_{ig}) + \eta_{ig} \quad (20)$$

The largely randomized assignment of refugee housing in Chicago satisfies this exclusion restriction insofar as the refugees are assigned housing based on availability. To test the extent of randomization, the survey records if the respondent had family in Chicago, if the respondent received help from a volag for his or her home, as well as whether this was the respondent's first residence or not. This set of criteria provides the information needed to determine if they are "free cases" or "family reunification" cases.

The advantage of this approach appears to be that any change in the respondent's political attitudes that is significantly correlated with his or her peers can be argued to be causally related.

C. Survey Methodology

This section discusses the framework of the survey study, paying attention to the four "cornerstones" of survey research: coverage, sampling, nonresponse, and measurement (Dillman et al., 2008). As the "perfect" survey aims to minimize errors arising from these four cornerstones, I describe how I address these concerns within the constraints of this study, and the implications of the limitations of this study. Approaching the survey methodology chronologically, the first two points are covered in part i, the third point in part ii, and the final point in part iii. The survey instrument can be found in Appendix 2.

i. Sampling and Coverage

I chose the set of English-speaking refugees that visited the Touhy Health Center in Chicago, Illinois, as the sampling frame. I chose this frame after exhausting all other means of contacting refugees, including all the volags operating in Chicago. This constraint over the sampling frame has several important implications. First, it is clear that there is under coverage of the target population, since not all units of the target population are included in the sampling frame — that is to say there are some refugees in Chicago that have a zero probability of being chosen for the survey. Second, the set of refugees visiting the Touhy Health Center composes a nonprobability

or “convenience” sample. Although there is some randomness as to which refugees were contacted during the survey period of February to March 2010, it is insufficient to claim a random selection procedure. Taken together, these two facts mean that although there may be other reasons to argue for this being a good case study, there is no statistical evidence to make the findings of this study generalizable to the entire population of Chicago-based refugees; the only statistical claim that I make is that the findings are representative of the population consisting of survey respondents alone. Finally, although the choice of English-speaking refugees was originally meant to ensure exposure to American peers and culture in general, this choice may also lead to the size of the peer effects estimator being overstated for the “average” refugee, as English-speaking refugees are most likely less insulated from social influences in an Anglophone society than their non-English-speaking counterparts.

ii. Data Collection

The importance of addressing response bias is described at length by Blair and Czaja (2005). The survey instrument was developed in close consultation with various individuals that have had extensive experience in either survey methodology or working with refugee populations. Vocabulary was simplified to take into account the average refugee’s English comprehension skills, and concepts were kept simple to reduce both unit- and item-nonresponse bias due to miscomprehension. It was also found that certain relevant questions had to be omitted in the process of shortening the questionnaire to a length that refugees found acceptable. The survey length was finally reduced from ten to six pages by adapting the redesigned social network component of Dillman’s (2008) social effects questionnaire.

Recollection bias poses a special problem for this study, given the tension between the need to record changes of political attitudes, and the rapid decay of “autobiographical memory” as described by Oyserman et al. (2008). To address this, questions involving recollection were either posed in various forms to check for consistency of responses, or respondents

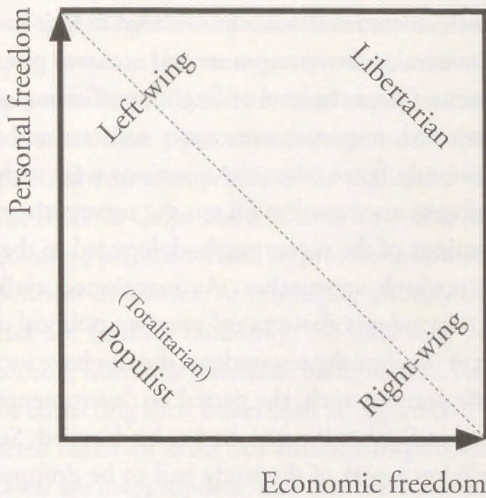


Figure 1. Nolan Chart

were asked to recall specific actions (such as which candidate they voted for, or which news source they relied on) instead of just their past sentiments (such as their political beliefs five years ago).

To create a measure of political attitudes that is comparable across different countries and cultures, I adapted questions from political-attitudes quizzes such as the Political Compass that organize political thought on the two-dimensional Nolan Chart:¹² personal freedom (authoritarian-libertarian), and economic freedom (left wing–right wing). Their responses to a set of questions were then scored on these two dimensions. Respondents were additionally asked to imagine their peers' answers to a similar set of questions of reduced length.

Before general implementation of the survey instrument, three limited survey pretests were carried out on ten refugees, with particular attention paid to the refugees' ability to understand the survey questions.

12. Named for political scientist David Nolan, who developed the concept in 1971.

The survey instrument itself was administered in face-to-face interviews in order to maximize survey responses and accuracy given time and monetary constraints. Given the level of English proficiency of the average refugee interviewed, responses were more accurate and the survey process was significantly faster when the questions were verbally posed than when the refugees attempted to fill out the surveys themselves.

Finally, limitations of the survey methodology led to the abandonment of several research approaches. As mentioned earlier, it was unfeasible to ask respondents about peers' previous political or religious attitudes, as most of the time the respondents did not have such intimate knowledge of their peers; as such, the partial tax/instrumental variables approach discussed in Section B part i. had to be dropped. Separately, a corresponding religion aspect of the study had to be dropped due to a similar problem of the survey instrument being unable to capture sufficient relevant information about respondents' and peers' religious attitudes and changes thereof. For example, changes in the importance of religion to the respondent were highly subjective and respondents could not be asked to construct a corresponding measure of religious importance for their peers; the two respondents who reported changing their religion did so for exogenous reasons, e.g., marriage to a spouse of a different religion; and finally, changes in frequency of religious observances were often attributable to the unobservable variable of accessibility, e.g., adherents of small Christian sects like the Chaldeans could not find suitable churches in Chicago.

iii. Estimation

Response rates were recorded to monitor the likelihood of nonresponse bias. Unit nonresponse rate was about 20 percent (10/51), including partially completed surveys. I do not consider unit nonresponse error to be significant, however, as nonrespondents were either refugee patients who had to go for their appointment or leave for another appointment in the middle of the survey, and hence are not significantly different from refugee patients that had the time to complete the survey; or they were not

confident of their English skills, in which case they are not part of my study sample. Errors in data entry and coding were minimal due to the small sample size. Finally, however, item nonresponse may be an issue, as there was more than 5 percent nonresponse on questions regarding previous political attitudes, which is the traditional threshold for item nonresponse (Blair & Czaja, 2005). Out of forty-one interviews, fourteen interviewees or 34 percent refused to provide information pertaining to previous political affiliations. As it is highly unlikely that this 34 percent of responses are missing randomly, the data may be biased towards refugees coming from less traumatic backgrounds. As such, traditional methods of correcting such biases such as imputation (i.e., substitution of constructed values for items that are not answered based on other cases in the dataset) are inappropriate, and this bias cannot be corrected. The implication for this study, assuming that the reason for item nonresponse is indeed extreme trauma, is that the results are not generalizable to highly traumatized refugees.

IV. Analysis of Data

A. Results

Information about refugees' peers is generated as follows: survey respondents are asked to list three peers that they talk to frequently about important matters, and are asked to imagine their responses to certain political questions. Peers attitudes are then scored as binary values on two dimensions: authoritarian attitudes and left-wing attitudes. These peer attitudes are then averaged across the three peers to generate average attitudes of each refugee's peer group.

The subgroup of "randomized" refugees was generated by selecting all the refugees that satisfied the following criteria: 1) they were living in their first residence, 2) *and* they reported receiving help from volags in getting only their homes, 3) *or* both their homes and jobs; 4) *or* they had no family in Chicago prior to their arrival here. This generates the list of

Table I. Summary Statistics for Sample of Chicago Refugees

Variable	Obs	Mean	Std. Dev.	Min	Max
Male	41	.6341463	.4876524	0	1
Age	41	36.34146	11.33933	18	67
Years in U.S.	41	2.803659	4.14262	.1	16
Iraq	41	.6341463	.4876524	0	1
Benin	41	.0243902	.1561738	0	1
Ethiopia	41	.0487805	.2180848	0	1
Liberia	41	.0243902	.1561738	0	1
Bhutan	41	.097561	.3004062	0	1
Bangladesh	41	.0243902	.1561738	0	1
Bosnia	41	.0731707	.2636517	0	1
Socioecon status lower	41	.6341463	.4876524	0	1
Coworkers same race	41	.0731707	.2636517	0	1
Neighbors same race	41	.2682927	.448575	0	1
Volag helped with home	41	.3414634	.4800915	0	1
Volag helped with job	41	.0243902	.1561738	0	1
Volag helped with both	41	.1219512	.3312946	0	1
Chose Chicago	41	.7073171	.4606464	0	1
Family here	41	.5121951	.5060608	0	1
Authoritarian (=0,1)	41	.902439	.3004062	0	1
Authoritarian (6-points)	41	4.585366	1.071812	2	6
Left wing (=0,1)	41	.7804878	.4190582	0	1
Left wing (6-points)	41	4.512195	1.227232	2	6
Auth previously (=0,1)	27	.6296296	.4921029	0	1
Auth previously (6-points)	27	4.333333	1.270978	2	6
Left previously (=0,1)	27	.7777778	.4236593	0	1
Left previously (6-points)	27	3.925926	1.238048	1	6
Average authoritarian peers	39	.7350427	.4404907	0	1
Average left-wing peers	39	.5555556	.4545632	0	1
Auth attitudes changed	27	.4814815	1.649484	-2	4
Left attitudes changed	27	.8888889	1.694637	-2	5
Randomly assigned	41	.6341463	.4876524	0	1

refugees that were randomly assigned to the United States, then to Chicago, then to their respective neighborhoods and jobs, i.e., the “free cases” as described in section IIIA. This set of criteria was used in favor of refugees’ own responses to whether or not they received “help in getting a home” and whether they had “family here” as stand-alone criterion respectively, as refugees were not always clear on what constituted “help” or if they were being asked about family that was already in Chicago or family that came along with them. Since volags are bound by the United Nations’ convention to provide housing and entry-level job opportunities for all refugees arriving as “free cases,” refugees reporting “no family in Chicago” must necessarily have received direct housing assistance from a volag; conversely, if they received help finding a home from a volag, they did not have any prior family here, as their relatives would otherwise have primary responsibility for housing them initially. Thus, this set of criteria generates a reliable list of refugees who were randomly assigned to their final destinations.

As a further check on randomization, I regress the various measures of respondents’ *previous* political attitudes on their peers’ average *current* political attitudes. Regression (1) is an ordinary least squares regression of previous authoritarian beliefs (i.e., beliefs held in their home countries before coming to the United States as refugees) for the randomized subset of refugees on the averaged authoritarian beliefs of their peers. The *t*-statistic on averaged peers’ authoritarian beliefs is .86, indicating that there is no significant relationship between respondents’ prior authoritarian attitudes and peers’ current authoritarian attitudes for the randomized group. Regression (2) repeats the same OLS regression, but for nonrandomized (i.e., “family reunification”) refugees: here, the *t*-statistic is 3.62, indicating that their prior authoritarian beliefs were already significantly correlated with those of their peers.

A similar set of regressions was carried out for randomized versus nonrandomized refugees’ previous economic attitudes on their peers’ averaged economic attitudes. Here the *t*-statistic on peers’ left-wing attitudes is insignificant for both randomized and nonrandomized

Table II. Evidence of Random Assignment 1

Own Pretreatment Authoritarian Attitudes Regressed on Peers' Current Authoritarian Attitudes

	①	②	③	④
	Authoritarian Previously		Authoritarian Now	
	Random	Nonrandom	Random	Nonrandom
Average Authoritarianism of Peers (0-1)				
	0.224	0.714*	0.375*	-0.0769
	(0.260)	(0.197)	(0.179)	(0.0794)
Constant				
	0.466*	0	0.630**	1
	(0.216)	(.)	(0.176)	(.)
Observations				
	18	8	24	15

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

refugees, even after controlling for changes in the refugees' socio-economic status since they arrived in the United States, suggesting that both randomized and nonrandomized refugees did not take their peers' economic beliefs into account when establishing relations with them.

These two tables of regressions also provide some initial evidence for the existence of peer influence effects. Comparing the set of regressions (1) in Table II, and (1), (2), (5) and (6) in Table III, with the set of regressions (3) in Table II, and (4), (7) and (8) in Table III, refugee's whose previous authoritarian and left-wing attitudes were uncorrelated with those of their peers' have current attitudes that are correlated with those of their peers at the 5 percent level, suggesting that they have modified their attitudes based on those of their peers. The coefficients cannot be interpreted causally, however, as the changes in political attitudes are not captured in these regressions.

Table III. Evidence of Random Assignment 2

Own Pretreatment Left-wing Attitudes Regressed on Peers' Current Left-wing Attitudes

	①	②	③	④	⑤	⑥	⑦	⑧
Left-wing Attitudes								
	Previously		Now		Previously		Now	
	Random	Nonrandom	Random	Nonrandom	Random	Nonrandom	Random	Nonrandom
Average Left-wing attitudes of Peers (0-1)								
	-0.297	-0.383	-0.218	-0.545*	-0.351	-0.273	-0.322*	-0.545*
	(0.165)	(0.670)	(0.118)	(0.248)	(0.183)	(0.600)	(0.160)	(0.255)
Socioeconomic status lower (=0,1)								
					0.297	0.288	0.281	-0.389
					(0.243)	(0.417)	(0.182)	(0.208)
Constant								
	0.987***	0.830**	1.026***	0.782***	0.811**	0.591	0.911***	1.015***
	(0.0678)	(0.201)	(0.0200)	(0.158)	(0.200)	(0.417)	(0.0909)	(0.0973)
Observations								
	18	8	24	15	18	8	24	15

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table IV shows the heteroskedasticity robust ordinary least squares regression results for the following two peer effects equations:

$$\text{change}_i = \beta_0 + \beta_1 * \text{peers}_i + u_i, \quad (1)$$

$$\text{change}_i = \beta_0 + \beta_1 * \text{peers}_i + \beta_2 * \text{peers}_i * \text{past}_i + u_i, \quad (2)$$

where $\text{change}_i \in [-6, 6]$ is the change in political attitude for individual i , calculated as the difference between current political attitude and past

political attitude, both measured on a six-point scale for two dimensions (authoritarian-libertarian, and left wing-right wing); $peers_i \in [0, 1]$ is the average political attitude of individual i 's current peer group calculated as the average of three binary values equaling 1 if the peer is authoritarian and zero otherwise (for the authoritarian-libertarian dimension), and equaling 1 if the peer is left-wing and zero otherwise (for the left wing-right wing dimension); $past_i$ is own previous political attitude; and u_i is the error term resulting from unobservable variables whose mean conditional on average peers' attitudes and own previous attitudes is assumed to be zero, due to the randomization process.

The least squares assumptions are generally satisfied in these regressions. First, due to the random assignment, the error term u_i can be taken to be uncorrelated with the x_i terms. Second, although the refugees interviewed constitute a convenience sample, for the purposes of the study they can be taken to be independently and identically distributed, as all Chicago-based refugees have to visit one of three health clinics for certification at periodic intervals, and the particular clinic and time period chosen to interview refugees should not result in a biased subset of the population. Third, large outliers are unlikely, as political attitudes were scored on a six-point scale, hence both y_i and x_i terms have nonzero finite fourth moments. Fourth, the x_i terms have full column rank, i.e., no perfect multicollinearity, as average peers attitudes are binary whereas own previous attitudes are on a six-point scale. Fifth, heteroskedasticity-robust errors were used in all regressions, so the standard errors should be accurate.

The effect of a subject being assigned to an authoritarian (or a left-wing) network is thus equivalent to setting $peers_i = 1$, such that

$$change_i = \beta_0 + \beta_1 + \beta_2 * past_i + u_i \quad (3)$$

whereas being assigned to a libertarian (or a right-wing) network is equivalent to setting $peers_i = 0$, giving us

$$change_i = \beta_0 + u_i \quad (4)$$

The difference in change in political attitude, or "treatment magnitude," of being assigned to an authoritarian versus libertarian (or a left-wing versus a right-wing) network is thus the difference between (3) and (4), which is

$$\text{treatment}_i = \beta_1 + \beta_2 * \text{past}_i \quad (5)$$

Looking first at the authoritarian dimension, without controlling for own previous authoritarian attitudes, the coefficient on average peers' authoritarian attitudes is negative and significant at the 5 percent level. Once the interaction term for own previous authoritarian attitudes multiplied by peers average authoritarian attitudes is added however, the coefficient of average peers' attitudes on regression (2) is positive and significant at the 1 percent level, while the coefficient on the interaction term is negative and significant at the 0.1 percent level. Thus for a subject assigned to an authoritarian peer group instead of a libertarian one, the peer effect on an extreme libertarian ($\text{auth_prev}=0$) as given by equation (5) is a shift towards authoritarianism of $3-.936(0) = 3$ out of 6 points; the peer effect on a milder libertarian ($\text{auth_prev}=1$) is $3-.936(1) = 2.014$; and the peer effect on an even milder libertarian ($\text{auth_prev}=2$) is $3-.936(2) = 1.128$; and finally, the peer effect on a very mild libertarian ($\text{auth_prev}=3$) is $3-.936(3) = .192$. This result is consistent with the hypothesis that subjects receiving a larger "treatment" magnitude (i.e., being assigned to peers whose beliefs are more different from the subject's) experience a greater degree of peer influence.

Looking at the left-wing dimension yields a similar set of results. The coefficient of average peers' left-wing attitudes in regression (3) is insignificant, but once the interaction term is added, the coefficient on average peers' left-wing attitudes is positive and significant at the 5 percent level, while the interaction term is negative and significant at the 1 percent level. Thus for a subject assigned to a left-wing group instead of a right-wing one, the peer effect on an extreme right-winger ($\text{left_prev}=0$) is a shift towards the left of $3.94-1.072(0) = 3.94$ out of 6 points; the peer effect on

a milder right-winger (*left_prev*=1) is $3.94 - 1.072(1) = 2.868$; and the peer effect on a milder right-winger (*left_prev*=2) is $3.94 - 1.072(2) = 1.796$; and finally, the peer effect on a very mild right-winger (*left_prev*=3) is $3.94 - 1.072(3) = .724$. Again, greater "treatment" magnitudes correspond to greater changes in economic attitudes. Adding a variable to control for changes in economic left-wing attitudes due to changes in own socioeconomic status lowered the magnitudes of both coefficients very slightly, but did not affect their significance, as shown in regression (5).¹³

Finally, I ran Chow tests on the coefficients to check if the independent variables have different impacts on randomized versus nonrandomized refugees. I do this by generating dummies for randomized ($d1=1$ if *random*=1) and nonrandomized refugees ($d2=1$ if *random*=1), and interacting them with the two explanatory variables of average authoritarian attitudes and the interaction term of average authoritarian attitudes multiplied by own previous authoritarian attitudes. I then run the following regression:

$$\begin{aligned} \text{change} = & d1 * \beta_1 * \text{peers}_1 + d2 * \beta_2 * \text{peers}_2 + d1 * \beta_3 * \text{peers}_1 * \text{past}_1 \\ & + d2 * \beta_4 * \text{peers}_2 * \text{past}_2 + d1 * u_1 + d2 * u_2. \quad (3) \end{aligned}$$

The F-statistic for the coefficient on *Avg_auth* (average peers authoritarian) for randomized versus nonrandomized refugees is 3.51, allowing us to reject the hypothesis that the two coefficients are equal at the 7.5 percent level. The coefficient for the interaction term of *avg_auth*auth_prev* (average peers authoritarian times own previous authoritarian attitudes) was only significant at the 18 percent level yielding a F-statistic of 1.92. Finally, the joint hypothesis that both coefficients

13. For both *auth_prev* and *left_prev* in these equations, notice that the peer effects approach zero when these variables approach the value of 4; this is because for values greater than 4, the subjects are coded as authoritarian and right-wing respectively, so the effective gap between their beliefs and those of their peers becomes zero. For this reason, I do not show the predicted results of these values, as they are irrelevant.

Table IV. Peer Effects on Change in Political Attitudes

OLS Results for Randomized Refugees

	①	②	③	④	⑤
	Change in Authoritarian attitudes		Change in Left-wing attitudes		
Peers Authoritarian (0-1)					
	-1.335*	3.000**			
	(0.619)	(0.984)			
Peers Authoritarian x Past Authoritarian					
		-0.936***			
		(0.200)			
Peers Left wing (0-1)					
			-0.0517	3.940*	3.905*
			(0.751)	(1.349)	(1.481)
Peers Left wing x Past Left-wing Attitudes					
				-1.072**	-1.043**
				(0.303)	(0.310)
Socioeconomic status lower (=1,0)					
					-0.398
					(0.686)
Constant					
	1.310*	1.256*	1.259*	1.308*	1.543*
	(0.480)	(0.479)	(0.494)	(0.451)	(0.526)
Observations					
	18	18	18	18	18
R2					
	0.180	0.583	0.000	0.542	0.555

Standard errors in parentheses * p < 0.05, ** p < 0.01, *** p < 0.001

are the same for randomized versus nonrandomized refugees yielded a F-statistic of 2.36, allowing us to reject it at the 12 percent level. It thus seems likely that randomized refugees are affected by peer influences differently from nonrandomized refugees; we cannot however conclude that one group is more affected by peer influences than the other, even though the magnitudes of coefficients for the nonrandomized refugees are larger than those for the randomized refugees, due to the fact that we cannot attribute any causal relationship between peers' attitudes and own attitudes for the nonrandomized refugees.

The F-statistic for the coefficients on *Avg_left* (average peers left-wing) for randomized versus nonrandomized refugees is .06, while the F-statistic for *Avg_left*left_prev* (average peers left-wing times own previous left-wing attitudes) is 0.28; the joint test yields the F-statistic of 0.17. The Chow test hence does not let us reject the hypothesis that the coefficients are the same for randomized versus nonrandomized refugees. This may be more to do with the small sample size ($n=26$ for all refugees reporting both current and previous political beliefs) however than any true similarity between the two groups: for example, the coefficients are different for the two groups, e.g., the coefficient on *Avg_left* is 4.84 for the randomized refugees, and 5.20 for nonrandomized refugees, while the coefficient on *Avg_left*left_prev* is -1.06 for randomized refugees, and it is -1.34 for nonrandomized refugees.

To summarize, there is convincing evidence that the effect of the random assignment process on otherwise similar refugees is significant, thus allowing us to interpret the coefficients on their peers' attitudes causally, i.e., that there exist positive and significant peer effects on the political attitudes of the randomized refugees.

B. Discussion

The extent to which the results of this study can be taken to prove the existence of peer effects on changes in political attitudes depends critically on two conditions: first, the degree to which the refugees were randomly assigned to their final work and residential locations; and

second, the degree to which this random assignment significantly constrained their control of who was in their peer group. If either of these conditions was violated, the three theoretical problems of simultaneity, contextual effects, and endogeneity, will arise, as described in the theoretical framework.

The first concern appears to be fairly well addressed based on the institutional procedures for assignment of refugees to countries, states, cities, and finally neighborhoods, as described by State Department documents, interviews with government officials, volag officials, etc., in section IIIA, and confirmed by survey interviews with refugee "free cases" who reported having no particular desire to come to the United States.

The second concern has dogged all previous studies involving randomization of subjects. The traditional method to address this concern has been to run regressions of subjects' initial or previous characteristics against peers' average characteristics and showing zero correlation between the two (Kremer & Levy, 2008; Sacerdote, 2001). I ran a similar test in the previous section, and showed that for the randomized refugees, this zero-correlation condition holds, whereas for the nonrandomized refugees, this condition does not hold with regard to their previous authoritarian attitudes, i.e., their previous authoritarian attitudes are significantly correlated with that of their peers. This suggests that the randomized refugees were indeed significantly constrained in their choice of peers to the extent that they ended up "choosing" peers with political attitudes unrelated to their own.

Does this prove that randomized refugees are completely helpless in their choice of peers? From Table V, the answer appears to be "no": comparing the average number of peers of the same country of birth, peers of the same race, and relatives listed as peers, randomized refugees only list 3 percent fewer countrymen and 9 percent fewer peers of the same race than their nonrandomized counterparts; conversely, randomized refugees list 5 percent more relatives as peers than nonrandomized refugees. Furthermore, the t-statistics on all these differences do not let us reject the hypothesis that they are the same. This shows that at the very

Table V: Comparison of Randomized versus Nonrandomized Refugees' Peers

t-test of mean differences results

	① Nonrandom	② Random	③ Nonrandom — Random
Average number of peers from same country of birth			
	2.33	2.25	.0833
	(0.211)	(0.219)	(.304)
Average number of peers of same race			
	2.47	2.21	.2583
	(.215)	(.217)	(.306)
Average number of relatives listed as peers			
	1.27	1.417	-.15
	(.267)	(.182)	(.364)

Standard errors in parentheses

[^]p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

least refugees always attempt to control their peers. Looked at from a different angle however, although the t-statistic on the difference in means between randomized versus nonrandomized refugees in the various t-tests is not significant, this is likely to be due to small sample size (26 randomized observations versus 15 nonrandomized observations) and the resulting large standard errors. Ultimately, the randomized refugees do list fewer countrymen and peers of the same race, and more relatives than the nonrandomized refugees, all of which suggest that they are significantly more constrained over their choice of peers than the nonrandomized refugees.

These results are striking given the surface level characterizations of refugees' peer networks, which would seem to suggest that refugees show a remarkable ability to recreate homogenous ethnic or racial peer groups. In spite of this, the evidence from this study suggests that regardless of the degree of homophily or self-selection, the refugees are not insulated from social effects as popular conceptions might indicate.

The results of this study are thus generally in line with expectations: (1) there appears to be a significant and positive correlation between peers' political attitudes and changes in the subjects' political attitudes, suggesting the existence of peer influence effects; and (2) the extent of peer influence effects is directly proportional to the initial difference in political attitudes between the subject and his or her peers. In particular, these findings closely resemble several other peer effects studies that find larger initial peer differences corresponding to larger peer effects, such as Sacerdote's (2001) finding that top students have the greatest effect on roommates' GPA when they are assigned to roommates from the bottom of the distribution, with the effect diminishing when they are assigned to middle students. Sacerdote (2001) does not however offer a causal explanation for why this correlation between initial differences and peer effects exists. Kremer and Levy (2008) on the other hand offer two possible causal explanations: firstly, the model of habit disruption, and secondly, the model of peer influence on preferences and on habit formation.¹⁴ While the second model of peer influence on preferences best describes the peer effects described in this study, the results of this study

14. In Kremer and Levy's (2008) paper, the *model of disruption* hypothesizes that hardworking students suffer most from disruptive "peer effects" as they are being constantly disturbed by their less industrious alcohol-consuming peers. On the other hand, the *model of habit formation* hypothesizes that students who are already at the bottom of the GPA curve are affected most by roommates' drinking, while students at the top are affected least. The first model is ultimately rejected in favor of the second model.

are in direct opposition to Kremer and Levy's (2008) findings in which peer influence effects seem to exacerbate preexisting tendencies but have little effect on individuals who are very different from their peers. The lack of "sticky preferences" regarding political issues in this study, however, may be explained by the unique situation of the refugees' forced relocation and resulting social isolation: to the extent that randomly relocated refugees lose their preference-reinforcing original peer groups, they are more likely to have unstable political preferences that are susceptible to influence from their new peer groups, thus resulting in greater initial peer differences leading to greater peer influence effects as observed in this study. Verification of this theory would require further research into the factors affecting differential peer influence effects and could yield further interesting insights into why certain individuals or preferences appear more susceptible to peer effects than others.

V. Conclusion and Policy Implications

In this paper, I present the results of a natural experiment in which the random assignment of refugees to countries, states, cities, and finally neighborhoods permits the identification of peer effects by overcoming longstanding problems in peer effects studies, namely the problems of simultaneity, contextual effects, and endogenous or self-selected group membership. The data show that although refugees do attempt to and often succeed in recreating homogenous racial or ethnic peer groups, they are influenced by their peers' political attitudes. Furthermore, instead of extreme prior attitudes making subjects more resistant to peer influence, peer groups have a greater effect on subjects whose initial attitudes were more different from those of their peers. In short, having greater differences in attitudes appears to correspond to greater peer effects.

Further research could explore if different individual characteristics correspond to different levels of susceptibility to peer influence, or if other deeply held beliefs such as religious beliefs are also subject to peer influence. Furthermore, it would be very interesting to see if the instrumental

variables approach yields the same peer effects results — as demonstrated in this paper, exogenous instruments that directly affect one individual's political beliefs but not those of his or her peers do exist and should be exploited in a study that can afford to keep track of changes in peers' political attitudes as well as those of the respondent. Finally, more qualitative studies could examine the exact mechanisms of how peer influence changes political perceptions or preferences, e.g., if active persuasion or passive imitation plays a more significant role.

In terms of direct policy implications, this study offers two levels of insights pertaining to the integration of refugees into American society. Regarding the content of refugees' peer groups, the success of refugees in surrounding themselves with peers of the same original nationality despite their scarcity relative to the general American population, indicates that on the one hand, refugees need no assistance from cultural or ethnic associations in seeking out their countrymen; on the other hand, volags' stated goal of expediting refugees' integration into American society is meeting with clear difficulties. It is uncertain from this study whether this is due to "weak demand" for American peers on the part of the refugees, or "weak supply" of American peers who are willing to befriend refugees, although the answer is likely to be a combination of both factors.

This point is closely related to the second level of insights addressing the existence of peer effects that could influence refugees' deeply held beliefs on how societies and economies should be managed. This paper provides some evidence to support the theory that immigrants and refugees who had lost their natural support networks were more isolated and vulnerable to influence by militant extremist organizations (Dunbar, 1996). This theory is supported in two ways by this study: first, refugees are subject to peer influences on deep ideological issues; and second, refugees who lose their "natural support networks" to a greater extent, for example through the process of random assignment to a host country, find it harder to self-select into peer groups that will preserve their prior beliefs. In particular, these refugees may be more susceptible to dangerous peer influences than refugees who are better able to rebuild their

original peer groups, for example, those who arrive as “family reunification” cases. This is especially so if we assume that the average refugee has moderate political attitudes to begin with: then, if the refugee’s new peer group is radically different from him or her in terms of political attitudes, the results of this paper suggest that the refugee is very likely to close the opinion divide by becoming more radical too.

Taken together, these insights suggest that a more proactive U.S. policy toward integration may be preferable to the current *laissez-faire* stance. Pickus and Skerry (2007) note that immigrants often find it impossible to integrate into American society precisely because the terms of belonging to an American community are never defined for them:

Indeed, nonimmigrants tend to overlook the confusing signals this diverse society sends out to newcomers. Certainly, in recent decades, we have taken a decidedly *laissez-faire* approach to the integration of immigrants. As one astute immigrant organizer in Chicago put it, “I wish to hell someone would make it clear how we’re supposed to act here!”

Separately, Hansen (2007) finds that although the British and the French are tied for the most positive attitudes toward Muslim immigrants in Europe, only in France are these positive sentiments reciprocated by the Muslims toward their non-Muslim hosts; this discrepancy is explained by the difference between French and British attitudes toward integration. While the French government actively addresses the incorporation of migrants as part of its national policy of “assimilation”, perhaps most famously in rejecting claims for religious or cultural differences in public institutions by implementing a hijab/kippa/cross ban, the British, like their American counterparts, have been more willing to accept looser policies on school dress and religious schools as part of their underlying policy of “multiculturalism,” and in general choose to leave the immigrants to integrate — or not — at their own pace and on their own (often ill-defined) terms. The key differences between French

“assimilation” versus British “multiculturalism” deserve to be highlighted here. Where the model of integration is based on the idea that a distinct national identity exists and must be adopted by all who claim citizenship, the model of multiculturalism allows immigrants to preserve their own cultural and linguistic traditions while living in the host country. Defenders of multiculturalism point to its pragmatic value, but accept that segregation by ethnicity is often the result; on the other hand, while they acknowledge that assimilation succeeds in providing a clear national identity that can be readily referred to, they often accuse such policies of being oppressive and inviting backlash (Langellier 2005). Hansen (2007) however notes that while both British and American media furiously denounced France’s hijab/kippa/cross ban, claiming that it would inflame moderate Muslim opinion and pander to racism, the ban “has done neither.” In short, the integration model of assimilation appears to be more successful in generating goodwill amongst immigrants and preventing ethnic segregation than that of multiculturalism.

The results of this study thus support the more proactive stance of government-led assimilation of refugees into American society. The aim of government policies should be twofold: first, to protect refugees from extremist or criminal influences; and second, to encourage the absorption of American political and cultural values. Put differently, the first aim can be interpreted as having a “negative” objective—the prevention of undesirable peer influences—while the second aim can be interpreted as having a “positive” objective—the promotion of desirable peer influences.

Programs that address the first “negative” objective (preventing undesirable peer influences) should essentially target known or suspected extremist organizations within the United States for close monitoring, especially religious organizations. As it may not be feasible or ideologically desirable to outlaw all of these organizations, however, the best way to discourage membership in these organizations may be to subsidize more moderate elements within the United States instead. To take the example of combating militant Islamic organizations, it may prove to be in America’s national interest to subsidize moderate mosques or Islamic

nonprofits such as the Inner-City Muslim Action Network (IMAN) of Chicago,¹⁵ in order to assist them in expanding their reach and organizational capacity. While the idea of using state funds to support Islamic organizations may prove politically unpalatable, the payoff is not insignificant: besides the aforementioned goal of replacing militant strains of Islam with more moderate versions, a considerable amount of goodwill from Islamic nations stands to be gained, all of which would contribute in large measure to national security.

Programs addressing the second “positive” objective (integration into American society) could be patterned after the French “integration contract” model, in which new arrivals are essentially provided with a clear roadmap to being American (in terms of norms, culture, etc.), and are rewarded for following this roadmap toward integration with generous social services (Simon, 2003). Such programs could be administered indirectly via the volags, since they are currently in charge of easing refugees’ transition into life in the United States. A model program could be “The Golden State Residency Program,” proposed by California’s Little Hoover Commission, in which refugees are rewarded with benefits such as a driver’s license, in-state tuition at public colleges and universities, and eligibility for public health insurance, in return for demonstrating an active attempt at becoming responsible members of American society through proficiency in English (e.g., by actual skills or enrollment in appropriate programs), participation in civic affairs (e.g., in volunteer and community-based programs), and responsibility to the local community (e.g., maintaining a history of paying taxes, remaining in good standing with law enforcement). Such a policy adjustment would require only a fairly modest budget increase, as many of the benefits are already provided to the refugees but with no strings attached.

15. IMAN is a large community-based nonprofit founded in 1995 by Muslim students, community residents, and leaders; part of their mission includes working for social justice by delivering a range of social services such as career development and medical attention to Chicago-area residents, regardless of race or religion.

Another potentially cost-effective strategy may involve a mentoring arrangement organized by volags, in which local American volunteers could be paired with refugees much in the same way a foreign-exchange program pairs foreign students with local students to provide a safe and friendly environment for cultural learning and adaptation to take place.

To summarize, to the extent that the results of this paper indicate the existence of peer influence effects that increase with the difference of initial attitudes between individuals and their peers, individuals that are socially isolated or have fragmented social networks such as refugees are at particular risk of influence by extremist ideologies. These individuals may be best helped by policies that firstly, encourage their contact with moderate ideologies, and secondly, promote their social integration into American society in terms of culture, norms, and values. Although such policies may come with political costs in a society of predominantly liberal attitudes, the payoff in terms of social cohesion and national security may well be worth it.

Ultimately, however, it must be emphasized that the purpose of this study has been to demonstrate the theoretical existence of peer effects. In reality, refugees and people in general will always exercise control over who comprises their peer groups, they will always mutually affect each other, and they will always be affected by broader changes in their environments. As such, policymakers hoping to exploit peer influences should remain wary not to be over reliant on their ability to use peer effects to influence GPA outcomes, binge-drinking habits, or in this case, even to avert undesirable political attitudes. ■

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Appendix 1: Regression Tables

OLS Regression Results for Potential Exogenous Instruments:
Changes in Socioeconomic Status and Unemployment Rate in
Refugee's Year of Arrival

	①	②	③
	left_changed (change in left-wing attitude, six-point scale)	auth_changed (change in authoritarian attitude, six-point scale)	se_lower (socioeconomic status lower, =1,0)
se_higher (socioeconomic status higher dummy =1,0)			
	-3.000***	-2.577***	
	(0.325)	(0.321)	
unemp_rate (%)			
			0.0898*
			(0.0361)
Constant			
	1**	0.577	-0.113
	(0.325)	(0.321)	(0.311)
Observations			
	27	27	41

Standard errors in parentheses

* p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 2: Survey Instrument

Hello! This survey should take about 10 minutes to complete. This study is for my undergraduate BA thesis at the University of Chicago, to gain more information about refugees, immigrants, asylees, and how they assimilate into Chicago.

Nobody else will read this survey beside myself and your responses will **not** be shared with anyone. Your responses will be pooled with others and will not be singled out for separate study.

Please check or fill in your response if blanks are provided.

1. What is your gender? Male Female

.....

2. What is your age? ____ years

.....

3. Are you a refugee, asylum-seeker, or immigrant?

Refugee Asylum-seeker/Asylee Immigrant

.....

4. How long have you been living in the United States? ____ years

.....

5. What country were you born in? _____

.....

6. Did you live in another country before you came to

the United States? Yes, I lived in _____ No

.....

7. What is your race/ethnicity (e.g., Arab, Nepali, etc.)? _____

.....

8. What is your highest level of education?

Elementary school High school: Graduated? Yes No

College: Graduated? Yes No

If yes, what degree? Bachelors Masters PhD

.....

9. What is your job here in the United States
(i.e., where do you work)? _____

.....

10. What was your job in your home country? _____

.....

11. In terms of money and job only, do you feel like you are
doing better, worse, or the same as you were before you came to
the United States?

Better The same Worse

.....

12. If you are working, what race are your coworkers?
(Or, if you are studying, what race are your classmates?)

Mostly my race Several different races: Which races? _____
 I am not working or studying

.....

13. In which neighborhood do you live?

Albany Park Skokie Uptown
 Other: _____

.....

14. Is this your first residence in the United States?

Yes No

.....

15. What race are most of your neighbors?

Mostly my race Several different races: Which races? _____
 I don't know

.....

16. Did a refugee or immigrant agency help you in finding your
current **home** or **job**?

Yes, for my home Yes, for my job Yes, for both
 No, I received no assistance

.....

17. Did you choose to come to Chicago, in the United States, to live?

Yes No, I wanted to go to _____.

.....

18. When you arrived here in the United States, did you already have family here? Yes No

.....

I. Social Network Questions

Think of **three** people (e.g., friends, relatives, etc) with whom you've discussed politics or religion recently. Write the initials of their name— for example, Bob Lee would be "B.L."

Write each persons name or initials (*optional*)

Person 1 _____ Person 2 _____ Person 3 _____

.....

1. What is your relation to this person?

Write: Relative / Friend / Colleague / Neighbor / Other (*describe*)

Person 1 _____ Person 2 _____ Person 3 _____

.....

2. How many years have you know this person?

Person 1 _____ Person 2 _____ Person 3 _____

.....

Imagine you are this person. Would this person...

3. ...want government taking more money from rich people to give to poor people? Write: Yes / No / Maybe

Person 1 _____ Person 2 _____ Person 3 _____

.....

Imagine you are this person. Would this person...

4. ...want flag burning to be illegal? Write: Yes / No / Maybe

Person 1 _____ Person 2 _____ Person 3 _____

.....

5. What is this person's religion?

Person 1 _____ Person 2 _____ Person 3 _____

.....

6. How often does this person attend religious services?

Write: Daily / Once or more weekly / Yearly / Rarely/Never

Person 1 _____ Person 2 _____ Person 3 _____

.....

7. Where was this person born? Write the country

Person 1 _____ Person 2 _____ Person 3 _____

.....

8. What is this person's race?

Person 1 _____ Person 2 _____ Person 3 _____

.....

Do any of these three people know one another? Check all that apply.

Person #1 knows Person #2

Person #2 knows Person #3

Person #1 knows Person #3

They do not know each other

II. Political & Religious Identification Questions

Personal Issues

Should people be allowed to buy drugs?

Yes Maybe/Don't Know No

.....

Should burning the national flag be allowed?

Yes Maybe/Don't Know No

.....

Should all citizens be made to join the army?

Yes Maybe/Don't Know No

.....

Economic Issues

Should every working person receive at least \$1,000?

Yes Maybe/Don't Know No

.....

Should unemployed people receive money from the government?

Yes Maybe/Don't Know No

.....

Are higher taxes okay if the government spends the money wisely?

Yes Maybe/Don't Know No

.....

Political Issues

1. On a scale of 1 to 5, how important is politics to you now?

1 = Not Important > 5 = Extremely Important 1 2 3 4 5

.....

2. On a scale of 1 to 5, how important was politics to you
in your country-of-birth?

1 = Not Important > 5 = Extremely Important 1 2 3 4 5

.....

3. Which political party did you support in your country-of-birth?

4. On a scale of 1 to 5, how much did you agree with this political party?

1 = Not at All > 5 = Absolutely 1 2 3 4 5

Religious Issues

1. On a scale of 1 to 5, how important is religion to you now?

1 = Not Important > 5 = Extremely Important 1 2 3 4 5

2. What is your religion now?

Protestant-Christian Catholic Orthodox-Christian Jewish
 Muslim Buddhist Hindu Other: _____

3. How often do you attend religious services (e.g., go to your church/ mosque to pray) now?

Daily Once or more weekly Monthly Yearly Rarely/Never

4. On a scale of 1 to 5, how important was religion to you in your country-of-birth?

1 = Not Important > 5 = Extremely Important 1 2 3 4 5

5. What was your religion before you came to the U.S.?

Protestant-Christian Catholic Orthodox-Christian Jewish
 Muslim Buddhist Hindu Other: _____

6. How often did you attend religious services (e.g., go to church/ mosque to pray) before you came to the U.S.?

Daily Once or more weekly Monthly Yearly Rarely/Never

.....

Thank you for completing this survey! J

Please turn over to last page for contact details and more information about this study.