Capturing Culture: A New Method to Estimate Exogenous Cultural Effects Using Migrant Populations

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Abstract

We know that culture influences people's behavior. Yet estimating the exact extent of this influence poses a formidable methodological challenge for the social sciences. This is because preferences and beliefs are endogenous, that is, they are shaped by individuals' own experiences and affected by the same macro-structural conditions that constrain their actions. This study introduces a new method to overcome endogeneity problems in the estimation of cultural effects by using migrant populations. This innovative method uses imputed traits, generated from non-migrating equivalents observed at the country of origin, as instruments for immigrants' own cultural traits measured at the country of destination. By construction, imputed traits are exogenous to immigrants' host social environment. The predicted power of imputed traits over observed traits in instrumental-variable estimation captures the non-idiosyncratic component of preferences and beliefs that migrants and non-migrating equivalents share as members of the same national-origin group, that is, their culture. I use this innovative method to estimate the net exogenous impact of traditional values on female labor-force participation in Europe. I find that this impact is much larger than standard regression methods would suggest.

This article provides a new approach to answer an old question: What role does culture play in shaping people's behavior? Although not a single social scientist would disagree with the assertion that culture influences individuals' actions *to some extent,* ascertaining *how much* it matters has proven an extremely difficult question to address. Estimation of cultural effects on individual behavior faces two major obstacles.

The first obstacle is theoretical and stems from the very elusiveness of the concept. There are literally hundreds of definitions of "culture"

in the social sciences; very few are parsimonious. In the past 40 years, cultural theory has become highly sophisticated, yet theoretical controversies on key issues remain unresolved (see, e.g., Charles 2008; DiMaggio 1997; Vaisey 2009). These controversies concern the

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content of culture (what is culture?), its location (where does it reside?), and its effects (what does it do?). Consensus seems unlikely to emerge soon (Small, Harding, and Lamont 2010). Culture is too often defined so broadly as to become tantamount to society itself. Broad definitions tend to blur the distinction between culture, institutions, and social structure, reducing the analytical usefulness of the concept (see Hays 1994; Small et al. 2010). Paradoxically, the shift toward increasingly sophisticated theoretical accounts of culture in sociology has made the empirical study of its effects more complicated (DiMaggio 1997).

The second major obstacle is methodological. The study of cultural effects on individual behavior confronts substantial difficulties, including issues of measurement and selection bias. Yet by far the most important methodological problem is what economists call the problem of endogenous preferences (see, e.g., Bowles 1998; Manski 1993, 2000). Endogeneity emerges because culture, institutions, and social action are mutually generative. Preferences and beliefs not only influence people's behaviors, but they are also influenced by people's own social and economic experiences. Moreover, people's preferences and beliefs, as well as their actions, can be jointly affected by institutional, social, and economic factors that operate at the macro level as largely constant contextual effects. As a result, cultural traits that appear as independent predictors of the behavior of individuals in specific social groups are typically the consequence of the social environments that the members of such groups face. This greatly complicates the estimation of causal cultural effects on individual behavior, turning an apparently simple question (i.e., what is the causal impact of people's culture?) into one of the most intricate methodological problems encountered in the social sciences.

Although sociologists have long been aware of the problem of endogenous preferences, this problem is mainly treated from a theoretical standpoint, as an essential part of "social embeddedness" (Granovetter 1985),

as a manifestation of the very complexity of cultural phenomena (see, e.g., Archer 1996; Swidler 1986, 2001), or as what Giddens (1984) calls the "duality of structure" (see also Bourdieu 1990; Sewell 1992). Unfortunately, increasing theoretical sophistication in the sociological study of culture has been accompanied by a general neglect of the methodological aspects of endogeneity. This neglect is striking considering the huge biasing potential of the problem at hand: endogeneity is not only likely to bias the *size* of regression estimates for cultural effects on individual behavior, but it could even hinder the correct identification of the *sign* of such effects. The problem of endogenous preferences poses a paramount challenge for the quantitative study of cultural influences.

Addressing the problem of endogenous preferences from a quantitative methodological standpoint requires finding innovative ways to separate out the effect of culture from other institutional, social, and economic factors that influence preferences and behavioral outcomes. This task, in turn, has important theoretical implications. Searching for methodological solutions to endogeneity problems urges sociologists to think about culture in new and more practical ways, to focus our attention on the relation between micro- and macro-level aspects of culture, and to strike the right balance between theoretical sophistication, analytic precision, and empirical applicability.

In this study, I introduce a new quantitative method to estimate the exogenous impact of culture on people's behavior, and I discuss the theoretical underpinnings and implications of this innovative approach. I apply this method to estimate the causal effect of one particular cultural trait, cultural traditionalism, on one particular outcome, women's labor-force participation (FLFP). Cultural traditionalism captures people's beliefs in the importance of behaving in accordance to the norms, values, and customs handed down by religion and family tradition. Traditional values and norms establish sharply differentiated gender roles that discourage women from

working outside the home. Traditionalism should thus curb women's labor-force participation (see, e.g., Epstein 2007; Fernandez and Fogli 2009; Read 2004). But how *large* (or small) is this effect *exactly*? Because cultural traditionalism and FLFP are endogenous processes, answering this question becomes a complex methodological task.

I address the problem of endogenous preferences using an innovative method that draws on recent epidemiological approaches developed in the emerging field of new cultural economics (see, e.g., Antecol 2000; Fernandez and Fogli 2009; Guiso, Sapienza, and Zingales 2006). The central tenet of these epidemiological approaches is to exploit the *portability* of culture to identify its exogenous impact on economic outcomes. The study of migrant populations is thus central to this line of research. Migrants take their culture with them, from one social context to another, and this provides a unique opportunity to isolate and quantify (i.e., to *identify*) the *causal* effect of culture on people's behavior.

In a particularly influential article, Fernandez and Fogli (2009) investigate how culture affects women's participation in the labor market by looking at second-generation Americans. They measure the effect of culture using past female labor-force participation rates from their countries of ancestry. Past FLFP figures capture not only the economic and institutional conditions of the countries of origin but also their prevailing *culture*. Fernandez and Fogli find that past FLFP rates at country of origin are significant predictors of the work (and fertility) behavior of second-generation women, even after accounting for a host of stringent controls for human capital heterogeneity. They conclude that it can only be through culture that these country-of-ancestry effects operate. Use of country (or region) of origin as a proxy for culture is standard research practice in this emerging field (for a review, see Fernandez 2008).

The method I propose in this study also exploits migration as a key source of identification of exogenous cultural effects. Yet rather than using country-level averages as rough proxies for culture, my method allows for estimation of the exogenous impact of *specific* cultural traits as measured through attitudinal surveys, adding precision and flexibility to previous epidemiological approaches. Briefly stated, this method consists of combining imputation and instrumental-variable techniques using cross-national samples. The central idea is to impute immigrants' preferences and beliefs using information from non-migrating equivalents at country of origin, and then to use these imputed values as instruments for immigrants' actually observed preferences and beliefs (measured in the country of destination). Non-migrating equivalents therefore act as imputation *donors* for migrants. The correlation between the imputed values generated by donors and the actual preferences and beliefs of migrants at destination captures the very essence of their cultural commonality, understood as the tendency to share similar preferences and beliefs as members of the same national-origin group. Because migrants and donors are embedded in different institutional and socioeconomic environments, this correlation (measured as the net predicted power of imputed values over observed values in instrumental-variable estimation) provides a measure of cultural traits that is *by construction* exogenous to the social context that migrants face at destination. In other words, this method provides a measure of culture that is arguably free from the social environment, and hence can be used to estimate *causal* effects. Exogenous estimates can be computed simultaneously for as many preferences and beliefs as the data allow for. This provides a particularly useful tool for opening up the black box of cultural effects on individual behavior, expanding the quantitative study of culture in action in new and promising directions.

CULTURE IN ACTION: SOME KEY CONCEPTUAL ISSUES

Contemporary approaches in cultural sociology differ with respect to the causal role

assigned to culture as a shaper of human action.[1] Literature reviews typically distinguish between two distinct conceptualizations of culture. One, associated with the Weberian and Parsonian traditions, sees culture as the repository of preferences, beliefs, values, and identities that *motivate* people's behavior (Campbell 1996; Hitlin and Piliavin 2004; Joas 2000); the other, perhaps best exemplified by the work of Swidler (1986, 2001), sees culture as a complex repertoire, or toolkit, of symbols, competences, practices, and justifications that people use strategically *to make sense* of their actions (see Swidler 1986, 2001; see also Bourdieu 1990; Lamont 1992; Sewell 1992).[2] While the former view assigns culture a strong causal role in directing people's behavior; the latter sees it mostly as a constraining force (see Kaufman 2004; Vaisey 2009).

The culture-as-motivation model offers a clear causal link connecting individuals' ideational configurations to their actions. Its appeal lies in its simplicity. Parsonian accounts, in particular, argue that people's values are acquired through socialization processes, the most important of which are assumed to take place during childhood. Once internalized, these values should remain largely stable over time (see Hitlin and Piliavin 2004; Joas 2000). This idea allows researchers to take values as if they were exogenous predictors of people's behavior, and hence to ignore the problem of endogenous preferences entirely. Parsonian approaches also depict cultural traditions as unified and largely coherent symbolic systems, widely shared by all members of a given social group. This conceptualization inevitably leads to essentialist views of national and ethnic cultures, as well as an over-socialized conception of human action (Gecas 2003; Hays 1994; Polavieja and Platt 2014). Contemporary motivational approaches have taken pains to distance themselves from the over-deterministic aspects of the Parsonian legacy, which has largely fallen out of favor in the field (see, e.g., Small et al. 2010; Smith 2003; Vaisey 2009).

Culture-as-repertoire approaches, by contrast, tend to dominate the field of cultural sociology today. They offer a highly sophisticated account of culture, depicting it as complex, fragmented, dynamic, and internally inconsistent. Repertoire theories stress the situational aspects of culture and assign a much greater role to individual agency than do early motivational approaches (Campbell 1996). This requires recognizing that human action can shape individuals' preferences and beliefs, as well as acknowledging that socialization is a lifelong process (see also Elder 1994). The appeal of repertoire theories lies in their sophistication. As Vaisey notes (2009), however, one fundamental problem with these theories is that, by treating culture as a toolkit that people use strategically, repertoire approaches might end up ruling out the very possibility that cultural understandings act as motives for action, thus stripping culture of any real explanatory role (see also Smith 2003). A further problem is that due to their theoretical sophistication, repertoire theories are extremely difficult to operationalize in research practice (DiMaggio 1997; see also Mohr and Ghaziani 2014).

This contrast between motivational and repertoire approaches helps us identify a number of key conceptual challenges faced in the study of culture as an explanatory factor. First, to test for the effects of culture on individual action, it is essential to work with conceptions of culture that are motivational and amenable to empirical operationalization, but that can evade the over-deterministic aspects of early Parsonian approaches (Small et al. 2010).[3] Second, to be empirically tractable, such conceptions must strive for theoretical parsimony and focus on the ideational aspects of culture that can be measured at the individual level, yet parsimony should not be mistaken for theoretical oversimplification. Third, cultural explanations should be very clear about which specific aspects of culture are expected to matter for which particular phenomenon under investigation (DiMaggio 1994). In other words, scholars should aim for *precision,* a quality seldom found in

empirical research (but see, e.g., Barro and McCleary 2003). Finally, cultural explanations should be able to show that observed effects are indeed cultural and not merely idiosyncratic. This means it is not enough to find that people's ideational configurations are correlated with any given individual behavior of interest, we also need to show that such configurations, observed at the individual level, are part of a common collective trait, that is, part of a "culture" (Hitlin and Piliavin 2004:361). This, in turn, requires recognizing the multilevel character of the concept.

A Working (and Simple) Definition of Culture

I propose to view culture as the *probabilistic tendency* that members of a given social group share a given value, preference, or belief (i.e., a given trait) due to experiencing similar socialization processes. Social groups can be defined in terms of geography (e.g., nations), time (e.g., cohorts), religion (e.g., denominations), kinship (e.g., families), or social space (e.g., classes), depending on the particular question under investigation (see also Fernandez 2008). Individuals belong to various social groups simultaneously and many of these groups can have a distinctive socializing potential—that is, the potential to influence individuals' values, preferences, and beliefs.[4] This means people's values, preferences, and beliefs are typically affected by multiple social influences at once. These influences can be represented as nested in nature. For example, traditional values may vary across nations and within nations, they may also vary across generations, religious denominations, education levels, and family types.

Note that this is a simple heuristic definition that focuses on the ideational dimension of culture and stresses its multilevel character. This implies that culture can be observed at the individual level, but it is a meaningful construct only when viewed as a macro-level phenomenon. In other words, culture is "a collective phenomenon that manifests itself in people's minds" (DiMaggio 1997:272; see also Cerulo 2002). This connection between the micro and macro aspects of culture is a key aspect of the proposed definition and thus merits further comment.

One useful way of conceptualizing this connection between the micro and macro dimensions of culture is to think about any particular cultural trait as varying across individuals within social groups according to some approximately normal distribution. Viewed under this light, it is the overall distribution that defines the group's culture for the trait in question.[5]

As an illustration of this conceptualization of culture, Figure 1a shows the distribution of one particular cultural trait, traditionalism, among women in two Southern European countries, Spain and Italy. Note that these two countries share many institutional and economic similarities as well as a common Catholic heritage. Yet women in these two societies differ significantly in their average levels of traditionalism. This illustrates the importance of nations as key sources of cultural variation. Moreover, there is a high degree of within-country variation at the individual level. Such variation, represented by each country's normal density plots, can be further explored by identifying other collectivities with socialization potential that operate *within* nations (e.g., regions, generations, or levels of education). To illustrate the importance of such collectivities, Figure 1b shows the distribution of traditionalism for high- and low-educated female respondents in Spain. The density curves reveal significant average differences in traditionalism across educational subgroups but also a large degree of intra-group variance. I contend that both between-group difference and within-group variation are defining components of culture.

This conceptualization has three key theoretical implications. First, it provides a definition of culture that is always trait-specific and makes no strong assumptions as to how different traits might correlate with each other within a given collectivity. Second, it implies that if the variance curves (for a given trait) of
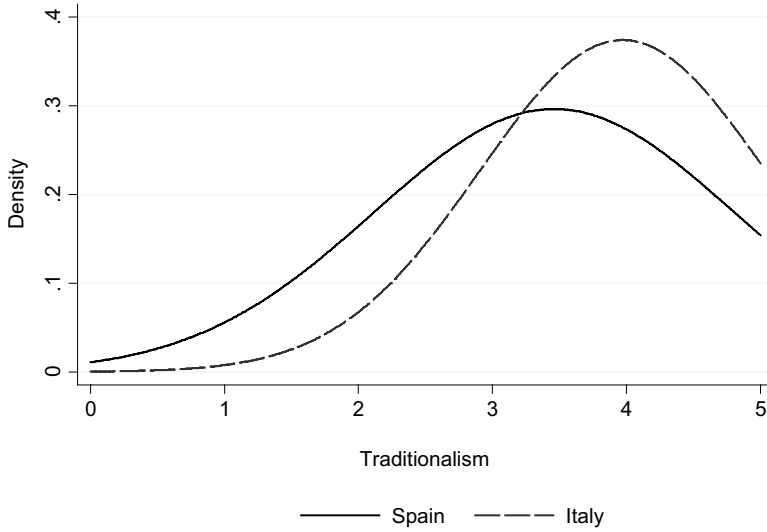
Figure 1a. Two National Cultures as Defined for One Trait: The Distribution of Traditionalism in Spain and Italy
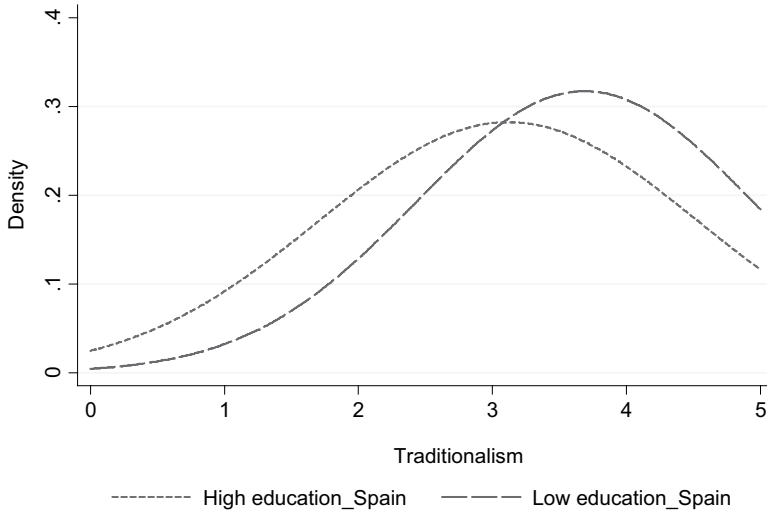
Figure 1b. Two Subcultures as Defined for One Trait: The Distribution of Traditionalism by Education in Spain
Source: European Social Survey, rounds 1, 2, and 3 combined for Spain (2002, 2004, and 2006), round 2 for Italy (2004).
Note: Normal density plots calculated using female respondents only. All group means are statistically different from each other at the 99 percent confidence level. High education is defined as tertiary education. Low education is defined as secondary education and below. Traditionalism measures how important it is for respondents to follow the customs handed down by their religion or their family using a six-interval self-placement scale.

two given populations were perfectly overlapping, there would simply be no cultural differences (for that trait) between them. In other words, cultures are always defined by comparison (see, e.g., Trompenaars and Hampden-Turner 1998). Finally, it stresses that individuals within a given collectivity always display varying values of any given cultural trait. Intrinsic cultural variance reflects the purely idiosyncratic component of all subjective phenomena. By accounting for such variation, this definition distances itself from over-socialized accounts of culture in action.

In summary, this view of culture allows us to connect *specific* individual-level traits to social groups without embracing any form of cultural essentialism; it captures the idea that culture does not form a unified or coherent ideational corpus but instead a diverse and fragmented terrain (see, e.g., Bourdieu [1979] 1984; Swidler 1986); it leaves ample room for idiosyncratic variance at the individual level; and it is even compatible with recent research on socialization processes that suggests individuals differ in their degree of malleability to cultural influences (Polavieja and Platt 2014). In other words, this conceptualization can combine theoretical parsimony, analytic precision, and empirical tractability while remaining attuned to contemporary sociological approaches. Viewing culture as the probabilistic tendency that members of a given collectivity (or nested collectivities) share a given trait also provides the theoretical foundations for the innovative estimation method that I present in the next section.

A NEW METHOD TO ESTIMATE EXOGENOUS CULTURAL EFFECTS: THE CASE OF TRADITIONALISM AND FEMALE LABOR-FORCE PARTICIPATION

The social and economic importance of female labor-force participation is incontrovertible. FLFP varies widely across nations and ethnic groups (see, e.g., Antecol 2000; Fernandez

and Fogli 2009; Read and Cohen 2007; van Tubergen, Maas, and Flap 2004). Differences in FLFP across societies reflect differences in economic and institutional conditions as well as cultural differences, that is, differences in the distribution of certain values, preferences, and beliefs concerning women's roles in society. Such cultural differences are often evoked as a crucial factor explaining observed variation in FLFP. To date, however, the problem of endogenous preferences has hindered the direct estimation of exogenous cultural effects on FLFP for specific cultural traits.

I focus on estimation of the exogenous effect of one crucial cultural trait, namely, cultural traditionalism, which is closely implicated in the distribution of gender roles. Traditionalism captures people's respect, commitment, and acceptance of the social norms, customs, and values prescribed and handed down by religious or family tradition (Schwartz and Sagie 2000). Traditional people are characterized by an orientation to the past; they tend to show deference to religious and parental authority and assign a great importance to traditional family values and norms. This connection between traditionalism and family values is of crucial importance for women's behavior. Traditionalism establishes a sharp distinction between men's and women's roles in society. The cultural origins of sex-role distinction can be traced back to deeply rooted patriarchal values common (but not restricted) to all Abrahamic religions (Epstein 2007; Inglehart and Norris 2003; Read 2004). Traditional norms impose what Blair-Loy (2003) calls *family devotion cultural schemas*. These are cognitive (but also moral and emotional) maps that assign women the primary responsibility for home and family, encourage motherhood, and prescribe time- and emotionally-intensive care for children, consequently discouraging, in more or less subtle ways, women's participation in the public sphere on an equal footing as men (see also Folbre 1994). Cultural traditionalism thus likely curbs women's propensity to participate in the labor market through its association with traditional gender roles.[6]

The degree of traditionalism thus defined not only varies across individuals within populations but also across populations. Previous comparative research shows significant differences in the average levels of traditionalism across nations and regions throughout the world (see Inglehart 1997; Inglehart and Baker 2000; Norris and Inglehart 2004; Schwartz 2004). Such differences indicate that the distribution of traditional values is not a purely idiosyncratic phenomenon but crucially a *cultural* one—it has individual- and group-level variance dimensions. This opens up the possibility to estimate its exogenous impact on FLFP.

Traditionalism and FLFP: Addressing the Problem of Endogenous Preferences

We are interested in correctly estimating the effect of a given trait *t,* in this case traditionalism, on a given socioeconomic outcome Y, in this case FLFP. We assume this trait can be measured via survey research using a given indicator, *T*:

$$(t) \rightarrow T \rightarrow Y$$

Endogeneity is a major source of concern for two main reasons. First, women's labor-market experiences could affect their degree of traditionalism. For instance, women living in countries where there are fewer economic opportunities, or where they face particular labor-market barriers leading to lower FLFP (e.g., discrimination), could fall back on traditional values as an ex-post ideational response to such structural constraints (i.e., to make sense of them). By the same token, labor-market participation could by itself erode women's degree of traditionalism by exposing working women to new experiences, values, and norms. This is the well-known problem of reverse causality:

$$Y \rightarrow (t) \rightarrow T$$

Second, levels of traditionalism and FLFP are likely to be simultaneously influenced by a third variable, or set of variables, present in the social environment (S). More precisely, women's employment opportunities, as well as their values, preferences, and beliefs, are likely jointly affected by macro-structural variables, such as the level of industrialization, the educational system, the welfare state, labor-market institutions, processes of technological change, and macroeconomic conditions (see, e.g., Brewster and Padavic 2000; Charles and Bradley 2009; Cotter, Hermsen, and Vanneman 2011; Inglehart and Norris 2003). In other words, women's degree of traditionalism (*t*) and their employment opportunities (Y) are socially *embedded* in S:

$$S \rightarrow (t) \rightarrow T; \ S \rightarrow Y$$

The key question for causal inference in nonexperimental settings is thus how to estimate the *causal* impact of *t* on Y under conditions of endogeneity. Instrumental-variable (IV) theory can provide an answer to this question (see, e.g., Angrist and Pischke 2008; Bollen 2012). To estimate the exogenous impact of *t* (measured through *T*) on Y, we need an instrumental variable (*Z*) for *T* that is independent of Y so that we can test $(t) \rightarrow T \rightarrow Y$. For any instrument *Z* to be valid, it must satisfy the following three conditions: (1) it must be exogenous to both Y ($Y \nrightarrow Z$) and S ($S \nrightarrow Z$); (2) it must be correlated with *T*; and (3) it must have no influence on Y other than through *T* ($Z \rightarrow T \rightarrow Y \ \wedge \ Z \nrightarrow Y$) (see, e.g., Angrist and Pischke 2008). Borrowing from IV parlance, I call condition 1 the *exogeneity condition,* condition 2 *instrument relevance,* and condition 3 the *exclusion restriction*.[7]

Finding valid instruments thus becomes the most crucial question for IV estimation. Yet cultural embeddedness makes it extremely hard to find valid instruments within the bounds of a single society, because instruments are typically themselves embedded (i.e., they are influenced by the same S that simultaneously affects *T* and Y). This poses a formidable inference problem in the social sciences.

To tackle this problem, I propose a new method that combines imputation regression,

instrumental-variable estimation, and cross-national sampling to capture the exogenous effect of specific cultural traits using migrant populations. For short, I call this the SISTER method (Survey-based Imputation of Synthetic Traits used as Exogenous Regressors). The SISTER method involves two different steps, the imputation step and the estimation step.[8]

Step I: Imputing Synthetic Cultural Traits as Exogenous Instruments

Migrants are embedded in a different socio-economic context than their non-migrating counterparts (i.e., they are not influenced by the same S), and this provides a unique opportunity to identify causal cultural effects. For every migrant i residing in destination country S_D, we can find non-migrating observational equivalents $\ddot{i}$ residing in country of origin S_O.[9] Observational equivalents are individuals who likely lived through similar socialization experiences as i but who did not migrate. Non-migrating observational equivalents at origin allows us to generate what I call "synthetic" cultural traits (T'). Synthetic traits are imputed values for any given cultural trait observed at destination (T_d) that are generated using the values of observational equivalents measured at country of origin (T_o).

In practice, synthetic traits are generated by treating all values of T_d for migrants as temporarily missing and then using relevant information from non-migrating equivalents to impute these values back. This requires that migrants and their non-migrating counterparts are temporarily grouped together in what I call a *synthetic* sample, where non-migrants act as imputation *donors* for migrants (note, incidentally, that for any given migrant woman, an optimal imputation donor would be her non-migrating "sister"). Regressing donors' observed values of the trait of interest, (T_o) (in this case, traditionalism), on a list of relevant predictors allows us to estimate an imputed value of the trait for each migrant observation (T'). These imputed, or synthetic, traits can then be used as

instruments for the actually observed traits of migrants at destination (T_d) using standard IV estimation techniques. Synthetic traits have several key properties as instrumental variables.

By construction, synthetic traits satisfy the exogeneity condition ($Y \nrightarrow Z \ \wedge \ S \nrightarrow Z$) because the trait values of non-migrating donors, which provide the backdrop for imputation, are free from the destination environment (i.e., they cannot be affected by the experiences of migrants at destination). Instrument relevance is provided by the correlation between synthetic and observed traits at destination. This correlation will capture the propensity that members of the same national origin (and similar sociodemographic characteristics) display similar values of the trait of interest, that is, the very essence of their cultural commonality with respect to that particular trait—their *culture* as defined earlier.

The possibility that migrants are selected on certain cultural traits must be considered from the outset. Note, however, that if there is selection into migration on the trait of interest, instrument relevance will be low—that is, migrants' traits observed at country of destination will be only poorly predicted by the synthetic values imputed from non-migrating donors observed at origin.[10] Instrument relevance can be tested empirically using standard econometric benchmarks (see, e.g., Bollen 2012; Bound, Jaeger, and Baker 1995; Stock and Yogo 2005). Such tests will provide a measure of the cultural *closeness* between migrants and their non-migrating co-nationals.

Unlike the exogeneity condition, the exclusion restriction ($Z \rightarrow T \rightarrow Y \ \wedge \ Z \nrightarrow Y$) is not automatically ensured by the use of migrating populations. Suppose, for instance, that we used donors' age and schooling as the sole predictors of traditional values in the imputation equation. In this case, we would impute synthetic values of traditionalism for migrants by imposing on them the same age-schooling-traditionalism covariance matrix as observed for donors. The problem in this example is that synthetic values would be computed as a linear product of two variables

that do not satisfy exclusion, as they are known to influence women's LFP through paths other than culture—that is, the acquisition of labor-market skills.[11] To ensure the exclusion restriction is met, the imputation regression should include at least one regressor that is (arguably) orthogonal to the error term in the structural equation of interest (i.e., one that has no effect on Y other than through T). I propose to use regressors that measure cultural transmission as a means to build exclusion into the imputation model.

Imputing Synthetic Traditionalism

Several imputation methods for missing data can be applied for the computation of synthetic traits using standard statistical software (see Longford 2005; McKnight et al. 2007; StataCorp. 2011a). I favor imputation methods based on multiple regression because they are simple, transparent, and allow for the consideration of many imputation predictors. This makes them particularly well-suited to reflect the stratified nature of culture, as it has been defined here. Still, the specialized literature provides several different imputation methods based on multiple regression. In the present study I use imputation by standard multiple regression, which is the simplest one. I also check the robustness of my findings to two alternative and more sophisticated methods, imputation with an added stochastic component and multiple imputation. Results are robust regardless of the imputation method applied (see section 1 of robustness tests in the online supplement).

T' for migrants of a given national origin (*o*) are imputed by regressing the observed values of traditionalism for non-migrating respondents (*i*) on a list of relevant imputation predictors. It is important that the choice of such predictors is driven by theoretical considerations. More precisely, imputation predictors should seek to capture relevant sources of socialization (in the trait of interest) that operate *within* nations of origin.[12] Age and education seem two obvious predictors because they capture cultural variation

across time and social space. These predictors influence all sorts of attitudinal phenomena, including people's degree of gender traditionalism (see, e.g., Cotter et al. 2011; Inglehart and Norris 2003). Because the family is a crucial agent of socialization for most cultural traits, it is highly advisable to include information on parental characteristics in the imputation regression. In the case of traditionalism, having predictors for parental background seems particularly germane. Parental variables are also crucial because they are likely to satisfy exclusion. Likewise, for many cultural traits it is important to account for religious denomination. This seems essential in the case of traditionalism, not only because people with no religious affiliation should logically be much less traditional, but also because previous research shows significant within-country differences in traditionalism across religious faiths (see, e.g., Inglehart and Baker 2000; Sherkat and Ellison 1999).[13]

In summary, the present study uses donors' age, years of schooling, parental education,[14] and religious denomination as predictors of traditionalism in the imputation regression. All these variables can be defended on theoretical grounds and are known to have a significant socialization impact on traditionalism. Their inclusion in the imputation regression maximizes observational equivalence because it helps us capture the nested and stratified nature of culture.

In addition to these predictors, the imputation regression includes a further parameter. This parameter computes the proportion of traditional women at country of origin in preceding cohorts ($\bar{T}_{o_{(g-1)}}$), where g denotes respondents' cohort and $g - 1$ the previous cohort (i.e., capturing roughly the parents' generation). Three large cohorts of approximately similar sample size are considered: women born before 1946 ($g = 1$); those born between 1945 and 1969 ($g = 2$); and those born after 1969 ($g = 3$) (see operationalization details in the annotated guide to implementation commands for Stata 12 included in the online supplement). The net effect of this

parameter on donors' traditionalism provides a measure of the strength of the intergenerational transmission of this cultural trait at the country of origin.[15] This measure can be used in the imputation of synthetic traditionalism for all types of migrants using cohort as the imputation matcher.[16]

The main characteristic of this latter regressor is that it is (arguably) orthogonal to the error term in the final structural equation that predicts migrants' FLFP. This is because the only way (I would argue) in which the strength of cultural transmission at country of origin can affect migrants' economic behavior at country of destination is through immigrants' own levels of traditionalism. Thus the introduction of this cultural transmitter in the imputation regression better ensures that the exclusion restriction for a valid instrument is met.[17]

In short, the imputation regression for cultural traditionalism can be expressed as follows:

$$T_{o\ddot{i},s} = \beta_{1,s}\, age_{\ddot{i},s} + \beta_{2,s}\, schooling_{\ddot{i},s}$$
$$+ \pi_{1,s}\, parental\ edu_{\ddot{i},s} + \pi_{2,s}\, religious\ denom_{\ddot{i},s}$$
$$+ \beta_{3,s}\, \overline{T}_{o(g\text{-}1)\ddot{i},s} + e_{\ddot{i},s}\, ; \ddot{i} = \{1,....N\};$$
$$s=\{1,...S\}, \tag{1}$$

where subscript $\ddot{i}$ stands for the number of non-migrating imputation donors, and subscript s stands for the number of countries of origin. Synthetic values for each immigrant respondent (T') can now be imputed using $\hat{T}_{o_{\ddot{i},s}}$, that is, the predicted values from Equation 1, which are calculated using non-migrating donors sampled at the country of origin (see implementation details in the annotated guide to implementation commands for Stata 12 included in the online supplement).

The European Social Survey (ESS) combined dataset allows us to impute synthetic values (T') for immigrant women coming from 23 different national-origin groups (see data description below). This requires fitting 23 different imputation regressions, one for each country of origin present in the ESS dataset. This output cannot be easily summarized.[18] To illustrate the overall significance of the imputation predictors included in each

of these 23 regressions, Table 1 provides average effects across countries estimated by fitting the imputation regression to the entire pool of donors.[19]

Step II: Using Synthetic Traits as Instruments for Observed Traits

T' can now be used as a valid instrument for immigrants' actual traditionalism (T_d) within a standard IV framework. Like any other instrumental variable, synthetic traits are not meant to be used as mere substitutes for the variable being instrumented. Using synthetic traits as instruments for observed traits means, in fact, using the amount of variance in T_d that can be explained by T' as a predictor of women's supply of work. Note again that this amount of explained variance will neatly capture the exogenous cultural component of traditionalism, understood as the common tendency for individuals from the same national origin who have the same sociodemographic characteristics to hold similar values of traditionalism, even when they are embedded in different social environments. In other words, using synthetic traits as instruments for observed traits allows us to *disembed* the trait of interest by isolating its exogenous cultural influence on the outcome of interest.

A final crucial property of this method is that it allows for theoretical extrapolation. This means that, to the extent immigrant acceptors and non-migrating donors are observational equivalents, IV estimates will not only be *empirically* valid for acceptors, but also *theoretically* valid for donors, that is, generalizable to the non-migrating population (I turn to this point in the conclusion). Violations of the assumption of observational equivalence, on which extrapolation rests, can be tested empirically (see Section 3 of robustness tests in the online supplement). In summary, I claim synthetic traits are theoretically grounded constructs that can, at least in principle, satisfy the three conditions for a valid instrument (relevance, exogeneity, and exclusion) and provide exogenous (and generalizable) estimates for specific cultural traits. This adds a very high level of analytic

Table 1. Predictors of Cultural Traditionalism among Imputation Donors: Average Estimates for All Countries of Origin

Variables	(1) OLS
Age	.008***
	(.002)
Years of Schooling	−.014***
	(.004)
Parental Background (ref. primary or less)	
Secondary education	−.051
	(.054)
Higher education	−.133*
	(.056)
Religious Denomination (ref. Roman Catholic)	
Protestant	−.069
	(.075)
Eastern Orthodox	.236*
	(.104)
Islam	.270***
	(.071)
Other	−.362***
	(.091)
No denomination	−.657***
	(.064)
P of Traditional Women in the Preceding Cohort at origin, $\bar{T}_{o_{(g-1)}}$	1.249***
	(.305)
Constant	2.650***
Observations	40,113
R-squared	.141

Source: Calculated by the author from the European Social Survey, rounds 1, 2, and 3 cumulative dataset (2002, 2004, and 2006) merged with the ESS national Turkey sample, round 2 (2004). Sample restricted to native women age 16 to 65 years acting as imputation donors.
Note: S.E. clustered by country. Traditionalism measures how important it is for respondents to follow the customs handed down by their religion or their family using a six-interval self-placement scale.
*p < .05; **p < .01; ***p < .001 (two-tailed test).

precision to previous epidemiological approaches.

In the present study, I estimate the causal exogenous effect of cultural traditionalism on the propensity of labor-force participation (Y_i^*) using IV-probit regression with maximum likelihood estimation. IV-probit regression fits models for dichotomous outcome variables ($Y \in \{0,1\}$) with continuous endogenous regressors and can be implemented using fairly standard statistical packages. Specifically, I use Stata 12 (StataCorp. 2011b:793–805) to estimate the following system of simultaneous equations:

$$T_{di} = \pi_{10}X_i + \pi_{11}T'_i + v_i \qquad (2)$$

$$Y_i^* = \pi_{20}X_i + \pi_{21}T'_i + u_i \qquad (3)$$

The parameter π_{11} in Equation 2 captures the first-stage effects of T'_i on T_{di} adjusting for a vector of individual covariates, X_i. These covariates are standard predictors of FLFP. The parameter π_{21} in Equation 3 captures the reduced-form effect of T'_i on Y_i^* adjusting for the same covariates, X_i. This is a recursive structural model where T'_i enters the participation equation for Y_i^*, but participation is not a predictor of T_{di}. The covariate-adjusted IV
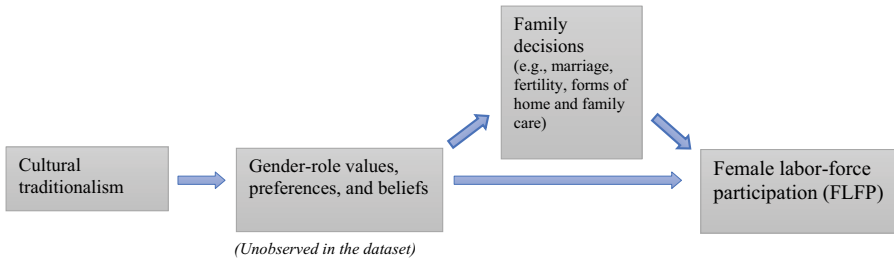
Figure 2. The Expected Effect of Cultural Traditionalism on Family and Labor-Market Participation Decisions

estimator (π_{21}) thus identifies the exogenous effect of traditionalism on the propensity to participate in the labor market (Y_i^*). I call this estimator the SISTER estimate. Note that the first-stage regression (Equation 2) is based on standard OLS estimation for continuous outcome variables, while the participation decision (Equation 3) is modeled using probit estimation for a dichotomous outcome variable:

$$Y_i \begin{cases} 0 & Y_i^* < 0 \, (not \, in \, the \, labor \, force) \\ 1 & Y_i^* \geq 0 \, (in \, the \, labor \, force) \end{cases}$$

To test for the robustness of my findings to alternative model specifications, I also fitted two-stage least squares linear probability models, where the first stage is identical to Equation 2, but Y_i^* in the second stage is treated directly as a linear function of the explanatory variables using standard OLS regression estimation.[20] Both IV-probit and linear probability models are easy to implement using standard statistical packages. Reassuringly, results are consistent regardless of the estimation method applied (see section 1 of robustness tests and the annotated guide to implementation commands in Stata 12 in the online supplement).

ESTIMATING THE IMPACT OF TRADITIONALISM ON FLFP IN EUROPE

As explained earlier, I expect cultural traditionalism to curb women's supply of work due to its association with traditional family values and gender norms. Using the SISTER method, I identify the exogenous impact of this cultural trait and test for its effects before and after controlling for family composition. Because traditionalism should exert a joint influence on women's family and labor-market decisions, it seems reasonable to expect that a sizeable share of its statistical effect on FLFP is accounted for by introducing family structure variables in the second stage of the IV regression (see Figure 2). Note, however, that a model including both traditionalism and family structure covariates will be misspecified, because family and labor-market decisions are co-determined by cultural traditionalism and hence endogenous to each other. In other words, only a specification without family controls can provide *exogenous* estimates for the full *causal* impact of traditionalism on FLFP.

Data, Variables, and Descriptive Statistics

I use the first three rounds of the European Social Survey (ESS) cumulative dataset (2002, 2004, and 2006) (see European Social Survey 2010). The analysis is restricted to working-age migrant-origin women with observed values of traditionalism at country of destination and whose national origins are other European countries that are also part of the ESS. The final analytic sample includes immigrant women from 23 different European national origins (including Ukraine and Turkey) who reside in 25 different European

destinations ($N \approx 3,200$).[21] Note that most immigrants in the analytic sample are European Union (EU) citizens who come from, and reside in, rich countries.[22] To impute synthetic traits for traditionalism, immigrants from the 23 different national origins are temporarily grouped with their corresponding non-migrating donors. Each imputation group forms a synthetic sample. So, for instance, the French synthetic sample ($n = 1,925$) includes all native non-migrating French women sampled in France (imputation donors, $n = 1,676$) plus all immigrant women from French origins sampled in other countries of the ESS (immigrant acceptors, $n = 249$). Only native women age 16 to 65 years are used as imputation donors. A total of 40,485 donors were used to impute synthetic traits for all immigrants in the analytic sample. Table 2 presents synthetic sample statistics (for further implementation details, see the annotated guide to implementation commands in Stata 12 included in the online supplement).

Table 3 shows descriptive statistics for key variables in the analytic sample by immigrant group. The dependent variable, labor-force participation, has a value of 1 if the respondent is in the labor force (either working or seeking work) and 0 if she is out of the labor force but not in full-time education.[23] The average participation rate thus defined for the whole analytic sample of migrant women is 63 percent, but there are marked differences by country of origin. Scandinavian migrants show the highest participation rates (around 80 percent); Turkish-origin migrants report the lowest (43 percent). The average rates of FLFP of the immigrant groups considered in this study are significantly correlated with the FLFP rates of their countries of origin/ancestry, both when country-of-origin rates are calculated using contemporary figures ($r = .65$, $p < .001$) and when they are calculated using lagged figures measured in 1980 ($r = .41$, $p < .001$).[24] We obtain only slightly lower correlations when looking at second-generation immigrants alone ($r_{contemp.} = .60$, $p < .001$; $r_{lagged} = .33$, $p < .001$). This already suggests that the FLFP behavior of immigrant

women in Europe is indeed influenced by country of ancestry, confirming Fernandez and Fogli's (2009) findings for immigrants in the United States.

In accordance with standard practice in migration research, I distinguish between first-, 1.5-, and second-generation migrants and account for language assimilation (see, e.g., Portes and Rumbaut 2006; Ryabov 2009).[25] The average age for the analytic sample is 42 years, and average schooling is roughly 13 years. Finally, 53 percent of all migrant respondents are married and 72 percent have children.

Cultural traditionalism (T_d) is measured using a six-interval self-placement scale. The ESS asks respondents to report how much they think they are like the person in the following description: "Tradition is important to her. She tries to follow the customs handed down by her religion or her family." Respondents are given six ordered options, ranging from "very much like me" to "not like me at all." Responses are reversed so value 0 corresponds with the lowest reported value of traditionalism and value 5 with the highest. To test the robustness of my findings to alternative measures of traditionalism, I also used respondents' self-reported degree of religiosity, which is measured using a full 11-point self-placement interval scale ranging from 0 to 10. Reassuringly, results are fully consistent regardless of the measure used. All the descriptive statistics and the findings presented here refer to the traditionalism scale. Results using self-reported religiosity are presented in the Appendix.

The average value of observed traditionalism for the entire analytic sample is 3.2. Yet again, there are marked differences by country of origin: the highest values are found among Turkish-origin migrant women (4.1), followed by Portuguese (3.6), Polish (3.4), and Irish (3.37); the lowest values are found among Danish (2.77), Finnish (2.79), Swedish (2.88), and Spanish (2.92) migrants. Table 3 also shows the average values of synthetic traditionalism (T') for each of the 23 ethnic groups for which the ESS has both origin and

Table 2. Acceptors, Donors, and Synthetic Samples

Origin	Immigrant Acceptors		Non-migrating Donors		Synthetic Sample		Ratio Acc./ Don.
	N	%	*N*	%	*N*	%	
Austrian	95	2.95	2,547	6.29	2,642	6.04	.04
Belgian	79	2.45	1,773	4.38	1,852	4.24	.04
British	276	8.57	2,032	5.02	2,308	5.28	.14
Czech	128	3.97	1,413	3.49	1,541	3.53	.09
Danish	53	1.65	1,612	3.98	1,665	3.81	.03
Dutch	97	3.01	2,277	5.62	2,374	5.43	.04
Finnish	138	4.28	2,069	5.11	2,207	5.05	.07
French	249	7.73	1,676	4.14	1,925	4.40	.15
German	572	17.76	2,897	7.16	3,469	7.94	.20
Greek	34	1.06	1,703	4.21	1,737	3.97	.02
Hungarian	79	2.45	1,871	4.62	1,950	4.46	.04
Irish	59	1.83	1,831	4.52	1,890	4.32	.03
Italian	290	9.00	569	1.41	859	1.97	.51
Norwegian	45	1.40	1,760	4.35	1,805	4.13	.03
Polish	215	6.67	2,231	5.51	2,446	5.60	.10
Portuguese	171	5.31	2,258	5.58	2,429	5.56	.08
Slovakian	101	3.14	1,218	3.01	1,319	3.02	.08
Slovenian	22	.68	1,480	3.66	1,502	3.44	.01
Spanish	99	3.07	1,856	4.58	1,955	4.47	.05
Swedish	95	2.95	1,615	3.99	1,710	3.91	.06
Swiss	34	1.06	1,651	4.08	1,685	3.86	.02
Turkish	197	6.12	862	2.13	1,059	2.42	.23
Ukrainian	93	2.89	1,284	3.17	1,377	3.15	.07
Total	3,221	100.00	40,485	100.00	43,706	100.00	.08

Source: European Social Survey, rounds 1, 2, and 3 cumulative dataset (2002, 2004 and 2006) merged with the ESS national Turkey sample, round 2 (2004). Only women age 16 to 65 years act as imputation donors.

destination data. Synthetic values for traditionalism were imputed using information for non-migrating donors as explained earlier. The overall correlation between synthetic and observed levels of traditionalism for the whole analytic sample is .35 ($p < .001$) (correlations by origin are presented in the last column of Table 3).

RESULTS

Table 4 shows standard probit and SISTER estimates (IV-probit) for the effect of traditionalism on FLFP. Standard errors are clustered at country of destination, and all models include information about the type of destination location of migrants (i.e., large city, small city, or countryside village) to account for unobserved destination-specific effects. Models 2 and 4 include controls for the following family characteristics: marital status, whether respondents have children, and the number of children living in the household. These controls are absent from Models 1 and 3. Four main findings are worth reporting.

First, the SISTER estimates (IV-probit) are statistically relevant in that they show a sufficiently high correlation with migrants' observed traits. The effects of synthetic traits on observed traits are captured by the first-stage coefficients shown at the bottom of

Table 3. Descriptive Statistics for Immigrant Women by Cultural Origin (Selected Variables)

Cultural Origin	N	% in the Labor Force	Age Mean	Age SD	Years of Schooling Mean	Years of Schooling SD	Immigrant Generational Status % 1st	% 1.5	% 2nd	% Speak Host Lang.	% Married	% with Children	Observed Tradition. (T_d) Mean	SD	Synthetic Tradition. (T') Mean	SD	Pairwise Correlat. T_d and T'
Austrian	95	58	44.60	(12.9)	11.95	(3.1)	39	04	57	99	55	71	3.25	(1.15)	3.05	(.53)	.31**
Belgian	79	65	41.67	(13.0)	14.06	(4.4)	59	08	33	94	46	65	3.05	(1.42)	3.15	(.56)	.20
British	276	59	40.20	(12.2)	13.79	(3.4)	63	12	25	96	53	69	2.93	(1.33)	2.95	(.57)	.41***
Czech	128	57	42.09	(14.7)	12.50	(3.6)	35	10	55	94	56	75	3.29	(1.37)	3.35	(.43)	.15
Danish	53	76	42.21	(12.1)	13.96	(3.7)	53	06	42	98	57	77	2.77	(1.31)	3.33	(.34)	.21
Dutch	97	60	42.73	(13.1)	13.52	(4.1)	55	03	42	93	70	77	3.10	(1.27)	3.21	(.42)	.32**
Finnish	138	80	44.56	(12.2)	13.25	(3.9)	59	04	38	93	36	77	2.79	(1.40)	3.01	(.27)	.08
French	249	62	39.95	(12.8)	13.09	(4.1)	50	10	39	94	53	67	2.99	(1.34)	2.89	(.68)	.32***
German	572	61	43.71	(12.8)	13.46	(4.0)	46	10	44	97	51	70	3.05	(1.39)	2.94	(.49)	.39***
Greek	34	53	37.79	(12.1)	13.68	(4.9)	35	12	53	76	50	56	3.15	(1.74)	3.91	(.51)	.36*
Hungarian	79	59	46.63	(12.6)	12.58	(3.4)	34	04	62	91	52	82	3.13	(1.37)	3.61	(.40)	.42***
Irish	59	59	43.17	(12.4)	13.51	(3.8)	42	05	53	98	61	80	3.37	(1.47)	3.29	(.49)	.29*
Italian	290	60	43.17	(12.0)	11.86	(3.9)	32	04	64	91	51	72	3.25	(1.37)	3.73	(.46)	.31***
Norwegian	45	82	42.64	(15.2)	13.58	(3.8)	31	02	67	89	44	71	3.29	(1.24)	3.12	(.33)	.54***
Polish	215	57	42.29	(13.5)	13.16	(3.4)	55	06	40	90	51	74	3.43	(1.32)	3.61	(.40)	.34***
Portuguese	171	74	36.58	(12.7)	9.07	(4.3)	61	16	22	68	54	71	3.60	(1.33)	2.95	(.47)	.26***
Slovakian	101	64	43.45	(12.7)	12.42	(3.4)	46	06	49	93	50	81	3.32	(1.32)	3.32	(.38)	.31**
Slovenian	22	53	43.73	(14.3)	13.55	(4.6)	64	05	32	86	45	73	3.05	(1.36)	3.22	(.50)	.44*
Spanish	99	77	40.78	(13.0)	12.47	(4.2)	42	05	53	89	47	65	2.92	(1.45)	3.11	(.48)	.39***
Swedish	95	84	35.77	(11.7)	14.56	(3.6)	46	29	24	96	47	64	2.88	(1.31)	3.01	(.40)	.33***
Swiss	34	62	38.38	(13.2)	14.58	(5.0)	38	24	38	94	47	66	3.00	(1.28)	2.90	(.63)	.46**
Turkish	197	43	37.73	(13.8)	10.07	(4.2)	44	20	36	71	68	75	4.10	(1.16)	4.15	(.50)	.38***
Ukrainian	93	70	41.66	(13.3)	13.64	(3.0)	52	09	40	92	61	76	3.31	(1.11)	3.52	(.41)	.23*
All	3,221	63	41.66	(13.1)	12.76	(4.0)	48	10	43	91	53	72	3.19	(1.37)	3.24	(.61)	.35***

Source: European Social Survey, rounds 1, 2, and 3 combined (immigrant women age 16 to 65 years from ESS-sampled origins).
Note: Synthetic traditionalism for Turkish-origin migrants calculated using the ESS national Turkey sample, round 2 (2004).
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed test).

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Table 4. The Impact of Cultural Traditionalism on Migrant Women's Labor-Force Participation, Probit and SISTER (IV-Probit) Estimates

Variables	(1) Probit	(2) Probit	(3) SISTER	(4) SISTER
Traditionalism	−.056*	−.021	−.240***	−.137
	(.023)	(.020)	(.071)	(.080)
Age	−.037***	−.043***	−.033***	−.041***
	(.003)	(.003)	(.003)	(.002)
Age^2	−.002***	−.003***	−.002***	−.003***
	(.000)	(.000)	(.000)	(.000)
Schooling	.047***	.044***	.037***	.039***
	(.007)	(.009)	(.007)	(.007)
Generation 1.5	.093	.091	.122	.113
	(.102)	(.106)	(.106)	(.113)
2nd generation	.105*	.117**	.085*	.109**
	(.041)	(.043)	(.041)	(.042)
Speaks host language at home	.144	.039	.087	.013
	(.106)	(.109)	(.096)	(.100)
Married		−.475**		−.419***
		(.079)		(.083)
Has children		−.475**		−.456***
		(.089)		(.099)
N of children in household		−.115**		−.114**
		(.042)		(.040)
Constant	−.001	.882***	.770*	1.298***
Observations	2,915	2,893	2,915	2,893
1st-stage effect T′ on T (z)			20.57***	20.33***
Wald test of exogeneity (Chi^2[1])			7.32**	2.21

Source: Calculated by the author from the European Social Survey, rounds 1, 2, and 3 combined, restricted migrants sample (1st-, 1.5-, and 2nd-generation immigrant women age 16 to 65 years from ESS-sampled origins and not in education).
Note: S.E. clustered at destination. SISTER = IV-probit models. All models control for type of destination location.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed test).

Table 4. Typically, *z*-values over 10 are considered a sign of instrument strength in the IV literature (see Stock and Yogo 2005). First-stage regressions yield *z*-values just above 20 ($p < .001$) in all models. This means that synthetic values of traditionalism computed on the basis of non-migrating donors are good predictors for observed traditionalism at destination. We can thus reject the null hypothesis of weak instruments. I interpret this finding as strong evidence that traditionalism has a significant cultural component.

Second, as the Wald tests reported in the last row of Table 4 show, the exogeneity condition is also statistically met before controls for family structure are introduced in the system of simultaneous equations. For one single endogenous variable, the Wald test checks whether the error terms in the structural equation and the reduced-form equation are correlated on the assumption that the instrument is indeed exogenous (see Wooldridge 2002). The Wald test rejects the null hypothesis of endogenous instruments in Model 3 but not in Model 4. This suggests that family formation and fertility decisions are crucial factors mediating the exogenous impact of traditionalism on FLFP, an interpretation that is further supported by the drastic reduction observed in the IV-coefficient for

traditionalism once family structure controls are accounted for (see below).

Third, and most important, cultural traditionalism has a very large and highly significant curbing impact on women's labor-force participation. SISTER estimates (IV-probit) are roughly four times as large as the standard probit coefficients in the best-specified model (Model 3). To better gauge the magnitude of these estimated effects, I calculated net predicted probabilities of FLFP for various levels of traditionalism and years of schooling. Predicted probabilities based on Model 3 show that the exogenous effect of traditionalism is remarkably strong. According to the model's estimates, a one-standard-deviation increase in the traditionalism scale would reduce women's participation propensity by as much as 32 percentage points. This notable drop is comparable in size to the effect of reducing education by as much as eight years—or two standard deviations.[26] The cultural component of traditionalism thus appears to exert a very powerful influence on women's labor-force participation. This influence is underestimated by standard approaches that do not account for the problem of endogenous preferences. I return to this point in the conclusion.

Finally, and as expected, the effect of traditionalism on women's supply of work is greatly reduced when family structure variables capturing family formation and fertility decisions are introduced in the model. Specifically, the size of the IV-coefficient for traditionalism is reduced by over 40 percent, falling below the 95 percent level of statistical significance, once family characteristics are accounted for (see Model 4). These findings tell us that more traditional women are *simultaneously* more likely to be married, to have children, and to be out of the labor force than are their less traditional counterparts. In other words, traditionalism imposes strong gender norms about women's roles in both the public and the private sphere. This also explains why Model 4 fails to pass the exogeneity test: Model 4 is not a good causal model because family-formation and labor-market decisions are endogenously co-determined by traditional values. The full

causal impact of traditionalism on FLFP is thus only captured by Model 3.

Robustness Tests

I performed numerous tests to check the robustness of these findings and to test for different potential sources of estimation bias. I checked the effect of using different imputation methods in the construction of synthetic traits; I used linear probability models instead of probit models in the estimation of the second stage; I clustered standard errors by country of origin, while using destination fixed effects; I clustered standard errors by each observed combination of origin and destination countries; I controlled for differences in the ratio of acceptors to donors across the different origin groups and restricted the analytic sample to only migrant groups that appear in all three ESS rounds; I re-estimated the SISTER models to country-of-origin subsamples selected according to the ratio of acceptors to donors, as well as to instrument relevance; I removed immigrants from the poorest and the richest countries, as well as from the most and least traditional origins; I removed immigrant groups with FLFP rates sizably different from the rates of their respective countries of origin; and, finally, I re-estimated the models excluding first-generation migrants. Results are robust to all these stringent tests, even when many result in a substantial loss of observations (see robustness tests in the online supplement).

SUMMARY AND DISCUSSION

Culture influences people's behavior. Very few social scientists would dispute this. Yet estimating the exact extent of this influence has proven a particularly difficult question to address in the social sciences. How can one measure the impact of culture if preferences and beliefs are themselves shaped by individuals' own experiences and affected by the same macro-level institutions that constrain their actions? The problem of endogenous preferences casts a long shadow over the empirical study of cultural effects.

In this article, I introduced a new method to estimate the exogenous impact of culture on people's behavior. This method combines imputation techniques, instrumental-variable regression, and cross-national sampling to produce exogenous estimates for specific cultural traits. The SISTER method offers an innovative take on the problem of endogenous preferences.

To illustrate the actual functioning and the explanatory potential of this technique, I applied it to estimation of the exogenous impact of one single cultural trait, traditionalism, on one particularly relevant socioeconomic outcome, women's labor-force participation. Using the SISTER method, I was able to identify the cultural component of traditionalism, defined as the common probabilistic tendency that migrant women and their non-migrating equivalents display similar values of this trait even when they are embedded in different social contexts. I argued that this common tendency can be nicely captured by the first-stage effect of synthetic values over immigrants' observed values in IV regression, that is, by the instrument relevance of synthetic traits over observed traits. The fundamental property of synthetic traits is that they are *by construction* exogenous to the destination environment, because they are imputed using information from observational-equivalent women who did not migrate.

Using this innovative technique I showed that cultural differences in traditionalism associated with the country of origin have a large *causal* effect on the LFP of immigrant women. The impact of this cultural trait on FLFP is so strong that it doubles the effect of education when both variables are tested jointly. I interpret this finding as a reflection of the association between traditionalism and traditional gender norms (unobserved in the ESS cumulative dataset) that lead to labor-market participation decisions. I further showed that standard approaches that do not account for the problem of endogenous preferences could be grossly underestimating the impact of traditional values on FLFP. This latter finding deserves further comment.

I explained that there are two possible sources of endogeneity bias in standard estimates for cultural effects: reverse causality, that is, the socializing impact of the outcome of interest on the cultural trait of interest $(Y \rightarrow (t) \rightarrow T)$; and social embeddedness, that is, the simultaneous impact of the social environment on both the outcome and the trait $(S \rightarrow (t) \rightarrow T; S \rightarrow Y)$. Note that reverse causality, in this particular case, should yield upwardly biased estimates. This is because the most probable socializing impact of FLFP is one of reducing (not increasing) women's traditionalism, whereas women outside of the labor force should, if anything, become more (not less) traditional. Reverse causality bias should therefore lead to an increase in the slope of standard coefficients and hence yield larger (not smaller) absolute effects. Because we find exactly the opposite, we must conclude that the most probable source of bias in standard estimates is not reverse causality but social embeddedness—that is, the simultaneous effect of the social context on both women's supply of work and their degree of traditionalism. I argued that embeddedness poses a major challenge to the exogenous estimation of cultural effects because instrumental variables are often embedded themselves—they are affected by the same social context as the variables they are supposed to instrument. This problem can be solved by use of synthetic traits for migrant populations and this, I believe, is the reason why SISTER estimates unveil much stronger cultural effects than do standard regression methods.

This study has significant implications for theory and research. First, it has implications for cultural theory. I claimed that the quantitative study of culture in action demands theoretical parsimony, analytic precision, and empirical tractability. The SISTER method rests on a working definition of culture that provides all three. This definition bypasses the over-socialized and over-deterministic aspects of early motivational theories by accommodating several key features characteristic of contemporary and more sophisticated sociological approaches (see, e.g., Charles 2008; Kaufman 2004; Vaisey 2009).

These features include the assertion that culture is a complex, fragmented, and stratified phenomenon; the idea that individuals are subjected to socialization influences throughout their lives; and the notion that culture is at the same time a collective and an individual-level process. Viewing culture as the probabilistic tendency that members of a given collectivity (or nested collectivities) share similar values of a given trait can thus help us conciliate motivational and repertoire approaches to culture, while providing a key conceptual tool to tackle the tortuous problem of endogenous preferences.

Second, the question of how best to estimate cultural effects under conditions of social embeddedness is highly relevant to many contemporary debates outside the field of cultural sociology, including debates in key disciplinary subfields such as economic sociology (e.g., DiMaggio 1994, 1997; Granovetter 2005; Zelizer 2010), gender stratification (e.g., Charles and Bradley 2009; Correll 2004; Ridgeway and Correll 2004), political sociology (e.g., Bernstein 2005; Dinesen 2013; Inglehart 1997), social psychology (e.g., Hitlin and Paliavin 2004; Hofstede 2001), the sociology of religion (e.g., Barro and McCleary 2003; Sherkat and Ellison 1999), immigration research (e.g., Platt 2014; Portes and Rumbaut 2006; Read and Cohen 2007), social stratification (e.g., Charles 2008; Desmond and Turley 2009), and the sociology of poverty (e.g., Lareau 2003; Small and Newman 2001; Wilson 2009). Although not always framed in methodological terms, all these different literatures confront similar dilemmas when it comes to assessing the *causal* role of culturally influenced preferences and beliefs. The SISTER method holds promise to contribute significantly to all these disciplinary subfields, as well as to fields outside the sociological discipline, because it provides a flexible and precise tool to test for cultural explanations of all kinds.

Third, the SISTER method can potentially be applied to investigate the impact of virtually any measurable cultural trait (including various traits at once) on virtually any measurable form of social behavior, and it can do so using quite simple regression and instrumental-variable estimation commands that are widely available in standard statistical packages. Indeed, any sociological explanation that involves the existence of macro-level cultures, typically of a national or ethno-linguistic kind, is in principle amenable to empirical test using this innovative technique. All we need is cultural measures for people who stay embedded in their origin cultures and people who migrate to different social environments.[27] Particularly relevant applications in the social sciences would include, for example, estimation of the causal effect of key cultural variables, such as generalized trust, religiosity, civic culture, or self-enhancement orientations, on key social outcomes such as, for instance, pro-social behavior, political engagement, processes of educational and occupational attainment, marital and fertility decisions, consumption and saving patterns, or risk-taking practices, to name but a few. In short, the scope of applications of this technique to the empirical study of cultural effects is potentially quite expansive.

Finally, such a wide scope of applications is ultimately granted by a fundamental property of this method, namely, allowing for what I call theoretical extrapolation. I argued that, insofar as migrating acceptors and non-migrating donors are observational equivalents, SISTER estimates can be interpreted as having *general* validity, that is, as empirically valid for acceptors and theoretically valid for donors. This claim to generality is common to all epidemiological approaches. When medical epidemiologists began to study migrant populations, their aim was not to establish the determinants of immigrants' health *in particular* but the determinants of human health *in general*. The study of migrant populations offered a unique opportunity to separate out the genetic contribution to disease from the contribution of environmental factors (including cultural influences). Likewise, epidemiological approaches to the study of culture in

the social sciences, including the present method, aim for generality and study migrant populations because migration provides a crucial source of identification of *causal* effects (see also Fernandez 2008). This is why the contribution of epidemiological approaches to the study of culture goes far beyond the specific field of immigration research. Their aim is much bigger and so is their promise.

Finding innovative solutions to the problem of endogenous preferences is a critical enterprise of great sociological import. Epidemiological approaches to the study of culture open up new and promising roads. A wide range of potential applications across the social sciences are waiting to be explored.

APPENDIX

Religiosity as an Alternative Measure of Traditionalism

I tested for a different operationalization of traditionalism based on respondents' self-reported religiosity. Religiosity is measured using a standard 0 to 10 scale. Gender traditionalism is an essential component of the core values that all Abrahamic religions transmit, and this justifies using this scale as an alternative measure (the correlation between religiosity and the traditionalism scale is .44 at the individual level and .67 at the national-origin level). Results are fully robust to using this alternative operationalization, regardless of the model specification (see Table A1).

Table A1. The Impact of Cultural Traditionalism Measured as Religiosity on Migrant Women's Labor-Force Participation: Standard and SISTER (IV) Estimates for Probit and Linear Probability Model (LPM) Specifications

Variables	(1) Probit	(2) Probit	(3) SISTER	(4) SISTER	(5) LPM	(6) LPM	(7) SISTER	(8) SISTER
Traditionalism as Religiosity (R)	−.029***	−.019*	−.071***	−.040	−.010***	−.007*	−.024***	−.014*
	(.008)	(.009)	(.019)	(.020)	(.003)	(.003)	(.006)	(.006)
Constant	−.041	.873***	.252	.999***	.498***	.771***	.598***	.819***
Observations	3,162	3,131	3,162	3,131	3,162	3,131	3,162	3,131
R-squared					.135	.185	.127	.183
Family Controls	No	Yes	No	Yes	No	Yes	No	Yes
First-Stage Effect R′ on R(z)			12.48***	12.21***			24.72***	24.5***
Exogeneity Tests			5.64*	1.06			5.29*	1.49

Source: Calculated by the author from the European Social Survey, rounds 1, 2, and 3 combined, restricted migrants sample (1st-, 1.5-, and 2nd-generation immigrant women age 16 to 65 from ESS-sampled origins and not in education).
Note: S.E. clustered at destination. SISTER estimates are IV-probit in Models 3 and 4, and IV-LPM in Models 7 and 8. All models control for age, age², years of schooling, generational status, language assimilation, and type of destination location. Exogeneity tests are Wald's test for IV-probit models and Wooldridge's test for IV-LPM models.
*p < .05; **p < .01; ***p < .001 (two-tailed test).

Acknowledgments

Notes

1. For comprehensive reviews of the sociological literature on culture see, for example, Charles (2008); DiMaggio (1997); Kaufman (2004); and Vaisey (2009).

2. Swidler (2001:148) notes that people tend to "trim their philosophy to fit their action commitments." Because people can assign various different cultural meanings to any given action, she argues, such meanings cannot be taken as providing the motivation for action.

3. This requires questioning the old paradigm of value-internalization upon which essentialist accounts of culture ultimately rest. Individuals can act in accordance with prevailing cultural norms without internalizing them, as a means of avoiding the expected sanctions that breaking such norms would bring (see, e.g., Elster 1989; Sherkat and Ellison 1999).

4. Socialization processes include learning about the existing social norms and the sanctions that are likely to follow if such norms are not observed, as well as forming expectations about the future by observing the behavior of others (Polavieja 2012).

5. This view of culture as a normal distribution is implicit in all quantitative studies that infer national cultural orientations by averaging individual values. For an explicit discussion see, for example, Trompenaars and Hampden-Turner (1998) and Hofstede (2011).

6. Although I focus on traditionalism, I do not mean to imply that this trait is the sole possible cultural influence on FLFP. Yet cultural traditionalism is crucial in shaping traditional sex-roles from which traditional behavior follows. The European Social Survey cumulative dataset lacks any direct indicator of beliefs in gender roles.

7. Most IV-theory manuals treat conditions 1 and 3 as one and the same (i.e., exclusion). It is assumed that if the instrument Z has an effect on the outcome Y through channels other than the instrumented variable T (i.e., through omitted variables), then Z must be *necessarily* correlated with S (S→Z). Yet this conclusion no longer holds when using migration as a source of identification because migration *necessarily* implies that the outcome of interest (Y) takes place in a different social environment from S. This

is why it is important to keep conditions 1 and 3 separate to explain the logic of my method.

8. To facilitate the implementation of this method, as well as to encourage its replication and validation, I present annotated syntax commands for Stata 12 in the online supplement (http://asr.sagepub.com/supplemental).

9. For the second generation, S_D is their country of birth and S_O is their country of ancestry (i.e., their parents' country of birth).

10. The potential biasing impact of selection is unknown. It could be great, small, or null. This impact could depend on the reasons governing the migration decision. For instance, more traditional women could show a higher propensity to emigrate when emigration is triggered by traditional family arrangements. Conversely, the least traditional women could be more likely to emigrate if emigration is triggered by purely individual considerations. I discuss selection bias further in sections 2 and 3 of robustness tests in the online supplement.

11. One could argue that this problem can be easily remedied by using age and schooling as predictors in the second-stage of the IV estimation step. Yet whether this suffices to solve the exclusion problem turns out to be a debatable question among IV specialists (Sophocles Mavroeidis, personal communication, June 15, 2012; Laura Mayoral, personal communication, November 7, 2013). There is no statistical test for exclusion.

12. Note that this automatically excludes the possibility of introducing other preferences and beliefs (as measured through attitudinal variables) as imputation predictors for the trait of interest. Attitudes might yield strong statistical correlations with the trait, but they are clearly endogenous and have no socialization potential.

13. Inglehart and Baker (2000) show that differences in traditionalism across religious denominations within religiously mixed countries are relatively small when compared to differences across countries. This suggests that the impact of religious traditions on cultural traditionalism today is transmitted through nationwide institutions to all members of society, including those who have little or no contact with religious institutions.

14. Parental education is measured using the educational level of donors' fathers. For donors with missing information on fathers' education, I use mothers' education. Results do not change if mothers' education is used throughout.

15. When younger cohorts face a very different socializing environment (S) from that of their predecessors, their attitudes might diverge significantly from their elders'. That is why cohort replacement is a key driver of cultural change (see, e.g., Brewster and Padavic 2000; Brooks and Bolzendahl 2004).

16. An obvious alternative measure for cultural transmission for first-generation migrants would be

parental traditionalism, insofar as parents remained in the country of origin (exogeneity condition). Indeed, if these data provided information on the degree of traditionalism of non-migrating parents, such information could be used directly as an instrument. Note, however, that this would still leave us with no instrument for migrants whose parents also reside in the country of destination, because in this latter case parents' traditionalism would also be endogenous to the destination environment.

17. Note, however, that none of the findings of this study depend on the inclusion of this variable in the imputation regression, and removing it from Step I hardly changes the IV estimates for FLFP (available on request). Having parental variables in the imputation step, while controlling for immigrants' age and education in the estimation step, likely suffices to comply with the exclusion restriction.

18. The imputation regression can alternatively be expressed as one single interacted model fitted to the full combined ESS dataset of non-migrating donors. In this interacted model, all imputation predictors in Equation 1 are multiplied by country of origin. This yields 22 different interacted terms for each imputation predictor, which is, again, a very extensive output to summarize.

19. The goal of the imputation regression is not to obtain a large R-squared but to reflect, for each country of origin, intra-national sources of variation in the trait of interest (i.e., subnational collectivities with a socialization potential). Because such sources should be captured using objective (i.e., non-attitudinal) variables, and given that high idiosyncratic variance is an essential component of all cultural traits, the R-squared is unlikely to be large. What matters is that imputed values are relevant instruments—that is, they are sufficiently correlated with immigrants' actual values so as to capture the cultural (i.e., non-idiosyncratic) component of the trait.

20. Albeit mostly discredited in sociology and political science, the use of linear probability models for binary outcome variables is currently widespread in applied economics, where the respective merits and demerits of linear and nonlinear specifications continue to be the subject of intense debate (see, e.g., Friedman 2012).

21. I use version 1.0 of the ESS cumulative dataset. To impute synthetic values of traditionalism to Turkish-origin migrants I merged the cumulative dataset with the Turkish national sample. Most countries appear in rounds 1, 2, and 3 but not all: Greece appears only in rounds 1 and 2; Italy and Luxemburg participated in the first two rounds but the question on traditionalism was asked only in round 2; Slovakia and Ukraine appear only in rounds 2 and 3; and the merged Turkish sample corresponds to round 2. Removing countries that are not present in all three rounds from the dataset does not alter my findings. Although Ireland participated in all three rounds, in round 2 the human value module of the core questionnaire, which contains the item on traditionalism, was administered only to half the sample. I excluded immigrants from Estonian and Luxembourgian origin from the analysis due to small sample size ($n < 20$).

22. The largest sampled group is German-origin migrants ($n_o = 572$), followed by Italian-origin ($n_o = 290$), British-origin ($n_o = 276$), and French-origin ($n_o = 249$) migrants. These four countries of origin are among the eight richest economies in the world. Roughly 70 percent of all migrants in the sample come from countries with gross GDP per capita higher than 32,000 U.S. dollars; 90 percent come from EU countries.

23. Immigrants in full-time education are excluded to avoid the possible biasing impact that differences in enrollment rates by migrant group could have on the estimates. This cuts roughly 300 observations from the final IV regression analyses.

24. FLFP correlations are calculated using the ESS cumulative dataset for immigrant groups and World Bank figures for their countries of origin. For each country of origin, I calculated contemporary FLFP rates by averaging World Bank figures for the period 2002 to 2006, which corresponds to the period covered in the first three rounds of the ESS. Lagged FLFP rates are World Bank figures for 1980, as this is the earliest year for which there is information for all countries of origin included in the ESS dataset. The World Bank defines FLFP as the proportion of women age 15 and older who are in the labor market.

25. Note that 48 percent of the sample is first generation (defined as respondents born abroad to foreign-born parents and arriving at the host country at ages older than 13 years); roughly 10 percent is generation 1.5 (defined as respondents born abroad to foreign-born parents but arriving younger than age 13); and 43 percent is second generation (defined as respondents born in the destination country to foreign-born parents of the same origin country). The vast majority of immigrant respondents in the sample (91 percent) speak the language of the host country frequently at their homes, as either the first or second most-frequently spoken language.

26. Exogenous estimates are somewhat smaller when traditionalism is measured using self-reported religiosity. In this case, a one-standard-deviation increase in the religiosity scale produces a drop in women's participation propensity of 21 percentage points. This drop is comparable in size to reducing education by five years (available upon request).

27. Use of comparative surveys, such as the World Values Survey or the European Social Survey, greatly facilitates the implementation of this method but it is not a necessary condition. Researchers can construct synthetic samples by matching immigrants to donors using single national surveys.

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