

Ancestry Culture and Female Employment—An Analysis Using Second-Generation Siblings

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Abstract

We study the importance of ancestry culture for female employment. To identify the separate importance of ancestry culture and institutions is difficult, as the factors are related to each other as well as to a host of potentially omitted factors. The epidemiological approach tries to separate culture and institutions by investigating outcomes of immigrants with different cultures living in the same institutional environment. We show that estimates from studies using this approach are likely to be biased upwards. Having access to very detailed registry data on the whole Norwegian population, we are able to rely on an extended epidemiological approach whereby we compare the outcomes of different sex, second-generation immigrant siblings. We find a robust effect of ancestry culture on female employment, but it is smaller than in previous studies.

Introduction

There is a clear relationship between macro-level factors such as family policies and welfare state regimes on the one hand, and gendered outcomes such as female employment and household work on the other (Fuwa, 2004; Hook, 2006). But because societies differ along both institutional and cultural dimensions, and as these dimensions interact, we cannot infer causal relationships between e.g. welfare state regimes and female employment by simply comparing country-level outcomes. To empirically identify the importance of one of the dimensions, we need to separate the influence of culture from that of institutions. One can do so by either examining people with different cultures facing the same institutions, or individuals with similar culture facing different institutions. We investigate the causal effects of culture on female employment using second-generation immigrants in Norway.

Cultural beliefs on the appropriate role of women in society vary substantially across the globe (Inglehart and Norris, 2003), and the intergenerational transmission of such gender values is potentially important for female employment (Moen *et al.*, 1997; Vollebergh *et al.*, 2001). To identify the importance of culture for gendered outcomes, and how it is transmitted across generations, is notoriously difficult. In particular, it is not obvious how culture should be measured, or how it should be separated from other important factors, such as local labour markets or country-specific institutions. The epidemiological approach tries to separate culture and institutions by investigating outcomes of immigrants with different cultures living in the same institutional environment (see Dinesen, 2013; Nannestad *et al.*, 2014; Röder and Mühlau, 2014; Frank and Hou, 2015) for recent sociological applications and Fernández (2011) for a review of the use of the approach in economics).¹ Of particular relevance to the present study is

the contribution of Polavieja (2015), who was the first outside of economics to analyse the effects of culture on female employment using an epidemiological approach. His study included a methodological extension by using imputed traits based on country origin as instruments for cultural traits. The study sparked a debate on the crucial assumptions inherent in such an analysis (Chou, 2017; Polavieja, 2017). In particular, Chou (2017) questions whether the exclusion restriction in Polavieja (2015) is plausible in light of omitted variable bias, selection bias, and unobserved heterogeneity. Our study speaks directly to this debate, and shows that the assumptions of the epidemiological approach are more plausible when sibling fixed effects are included in the analysis.

We apply an extended version of the epidemiological approach to study the cultural impact on employment outcomes of female, second-generation immigrants in Norway. More specifically, we follow previous work on this topic on US data (Fernández, 2007; Fernández and Fogli, 2009), and study the correlation between lagged female labour force participation rates (FLFPRs) in the parents' country of ancestry and employment outcomes of second-generation immigrants.² The second-generation immigrants, as opposed to their parents, are all born and raised in Norway, and thus face the same labour market and the same institutions, but the cultural heritage from their parents is different. The FLFPRs in the parents' country of ancestry, measured at the time of the second-generation immigrants' year of birth, captures the cultural heritage. We label the cultural heritage brought from the parents' country of ancestry as 'ancestry culture' and we estimate the effects of this on female employment in the host country. The strategy necessitates plausible controls for other factors, such as the parents' level of human capital and other characteristics of the source country. Our data and novel approach allow us to control for these factors.

We have access to rich administrative data covering the whole population of second-generation immigrants, which implies that we have a substantively larger number of ancestry countries in our sample compared to in the previous literature. The high-quality register data we use are the basis for the calculation of citizens' taxes and welfare benefits, and we can link each second-generation immigrant to her parents and siblings, and thus to the employment record of her close family. A particularly useful implication is that we can compare employment of male–female sibling pairs in a sibling fixed effects model.

The sibling fixed effects model increases the plausibility that our estimates pick up the influence of attitudes related to gender roles. That is, by comparing siblings of different sex to each other, we are controlling for all

factors that affect siblings equally, such as upbringing, parental networks, and parental resources, which are close to impossible to control for otherwise. The approach further controls for omitted characteristics correlated with ancestry female labour force participation and inherited outcomes, but uncorrelated with gender, such as work ethics and unobserved human capital through e.g. differences in school quality. In fact, the variation retained will by construction be the part of ancestry female labour force participation that affects daughters and sons differently. We show that the approach is important empirically, as ancestry female labour force participation is correlated also with second-generation males' employment. Hence, without sibling fixed effects, the measure is correlated with factors other than beliefs about female employment, such as e.g. preferences for leisure, suggesting that previous estimates of the impact of ancestry gender beliefs, attitudes, and norms are biased upwards.

Fernández (2007) and Fernández and Fogli (2009) find robust and economically important relationships between the FLFPRs in the parents' ancestry country and employment outcomes of second-generation immigrant women in the United States. Using the same method, our estimates of ancestry culture are smaller than what Fernández and Fogli (2009) find in the United States. We find that a 1 standard deviation (SD) difference in the cultural proxy amounts to a 3 percentage point difference in the probability of being employed, while the corresponding US estimate is 4 percentage points. These estimates are likely biased upwards, however. Using our preferred sibling specification, we find that a 1 SD difference in the cultural proxy amounts to a 2 percentage point difference in the probability of having earnings from employment and no effect on full-time employment. We discuss how the institutional particulars of Norway might shape the impact of culture on female employment across generations, and propose that the effect of culture is smaller due to a higher degree of social mobility, a different selection pattern of immigrants, and a strong link between labour market participation and access to welfare benefits.

Conceptualizing Ancestry Culture

There are many different definitions of culture and institutions, and there is no emerging consensus on which are the most appropriate ones (see Vaisey, 2009; Polavieja, 2015) for excellent overviews of different sociological conceptualizations of culture). We define ancestry culture as preferences and beliefs originating in the parental country of ancestry, transported to the host country, and reproduced within families via childhood socialization. Ancestry culture emerges in the country of

origin of the parent of the second-generation immigrant, and is brought to Norway and thereby separated from the institutions that caused it (such as local norms and labour markets). Transmission within the family (vertical socialization) will happen to the degree parents perceive that their children will get benefits from certain cultural traits and in so far they are willing to bear the costs associated with socialization, such as e.g. spend time and disciplining the child. The within-family socialization will, however, take place within a community which might limit or strengthen the cultural transmission (horizontal socialization). Our emphasis in this article is on within-family socialization; however, we address horizontal socialization at the end of the article.

We are particularly interested in preferences and beliefs regarding the role of women in society. We follow Fernández and Fogli (2009) and Fernández (2007), and restrict ourselves to study second-generation immigrants, and proxy for ancestry culture by the lagged FLFPRs in the parents' country of ancestry.

FLFPRs differ across countries for many reasons, such as demand for labour, family policies, availability of childcare, the types of jobs that are available, the wage differentials between men and women, and other institutional differences. But differences also stem from differences in beliefs about women's role in society and other cultural factors. When an individual moves from her place of birth, she potentially brings with her parts of the culture, but she leaves the institutions behind. The culture she brings is then partly transmitted to her children via childhood socialization. Thereby, the approach separates ancestry institutions from ancestry culture.

The main problem with the approach is that parents also pass on other things, e.g. economic, human, and cultural capital, in addition to their beliefs and attitudes on the role of women. Furthermore, parents pass on other types of beliefs and attitudes, including for instance attitudes on work ethics or different preferences for leisure (Moriconi and Peri, 2015). In addition, it is not random where people live, and immigrants from some countries may be more likely to live in areas with other immigrants, or in areas with different local labour markets. Finally, it may be that immigrants from some countries are discriminated against or face other institutional problems in the host countries. These issues loom large in the empirical literature on the effects of culture and, as will be explained in the empirical strategy, we solve them by including sibling fixed effects. By doing so, the only remaining variation is the one affecting brothers and sisters within the same families differently.

In interpreting the effects it is also important to consider the macro context. In the next section we discuss some factors of the Norwegian context that are likely to be important in our case.

Ancestry Culture and Employment in the Norwegian Context

Immigrants economic status likely depends on where they come from, the country they come to, and specific interactions between ancestry country and host country (Van Tubergen *et al.*, 2004). Hence, the effect of ancestry culture is likely contingent on macro-level factors in the host country. While economically important effects of cultural beliefs about female employment have been documented in the United States (Fernández, 2007; Fernández and Fogli, 2009; Blau *et al.*, 2013), there are a number of key aspects that might make the effects smaller in the Norwegian context.

Norway belongs to the social democratic welfare state model, with universalism and egalitarianism as guiding principles (Esping-Andersen, 1990). Comparative welfare state researchers also highlight the gender aspects of the Nordic model, with a focus on dual earners and equality of outcomes between the sexes (Ellingsæter and Leira, 2006). The expectation that women should work is more prevalent in Norway, compared to in the United States,³ but it is not obvious whether this difference will affect women from low or high FLFPR cultures the most. Intergenerational social mobility is greater in Norway than in the United States (OECD, 2010, chapter 5), which implies that parents' characteristics should matter less for children's outcomes. The gender pay gap is smaller in Norway as compared to in the United States (OECD, 2013, p. 262), which should make it more attractive for (married) women to work, thus making it more likely that economic considerations out-weigh cultural considerations. Moreover, access to many important welfare benefits is tied to employment, which further strengthens the incentives to enter the labour market.

Apart from factors relating to norms and labour markets, the type of immigrants to the United States and to Norway is likely to differ. More generally, the migrants in any host country are not a random sample from the population of the source country. If immigrants from low FLFPR countries have a particularly high disutility from working compared to the average disutility level in the country of ancestry, while immigrants from high FLFPR countries have a particularly low disutility from working compared to the level in their home country, then our estimates will be biased upwards (Fernández, 2011). This is because our measure of ancestry culture

captures average beliefs in the country of ancestry. Since Norway has a comparatively egalitarian wage distribution and low returns to education, we might in contrast expect negative selection of migrants on observed characteristics like wages and education (Borjas, 1991). Furthermore, Belot and Hatton (2012) find that negative selection on skills is stronger from culturally proximate countries. If anything, these factors will induce a negative bias in the estimated impact of culture, which we consider as less serious than a positive bias, since it goes against concluding that there is an effect of culture. Nonetheless, in comparing the effects of ancestry culture across host countries it is important to note that differences in selection of immigrants are likely to affect the estimates.

Data

We rely on data from merged administrative registers, encrypted to prevent identification of individuals, which are collected, administered, and made available for research by Statistics Norway. Our data include detailed information on labour market attachment as well as country of ancestry of second-generation immigrants. The data cover the whole population, and we can link individuals to their parents and siblings. Compared to the previous literature on culture and female employment, we have higher quality data, we cover the whole population, and we have a larger number of ancestry countries in our sample.

We study the cohorts of female second-generation immigrants born in the years 1965–1980, and observe their employment outcomes in the year they turn 30.⁴ Since our empirical strategy is based on comparisons with male siblings (see below), we implicitly restrict our sample to females with male siblings.⁵ In the main analysis we define a second-generation immigrant as a person born in Norway with at least one foreign-born parent, and we do not distinguish between whether it is the mother or the father who is foreign born. To examine to what degree the broad definition of a second-generation immigrant bias our estimate of culture downwards, we also present results when we restrict the sample to those with both parents born abroad. Country of ancestry refers to the mother's country of birth if both parents are foreign born. In the Supplementary Appendix, we further examine the difference between having a foreign-born mother and a foreign-born father (see Supplementary Table A4).

We derive our key independent variable, lagged FLFPRs in the parents' country of ancestry, from the International Labour Organization's (ILO) ILOSTAT

Database (ILO, 2014). It is not obvious how far back we should lag FLFPRs to best capture the influence of culture. One might argue that FLFPRs in the country of ancestry at the time parents immigrated to Norway best captures the culture the parents brought with them. Alternatively, one might argue that FLFPRs in the ancestry country at the time of the second-generation immigrants' year of birth best proxy the values transmitted from the first to second generation. Data availability makes the former problematic, thus, we measure FLFPRs in the year of birth.⁶ As seen in Table 1, the mean FLFPRs across the countries in our sibling sample for women is 31.4 (3.3 for Log FLFPRs), with a SD of 10.6 (0.57). In the estimations, we take the natural log of FLFPRs since it makes intuitive sense that a 1 percentage point difference in FLFPRs will have a larger impact at low levels of FLFPRs. We show in the Supplementary Appendix that we get qualitatively the same conclusions if FLFPR is measured in levels.

Our first outcome is a binary indicator of whether the individual is employed, defined as being registered with positive earnings in the administrative registers. This is a liberal definition of being employed, as it implies that only 1 hour of paid work during the year is sufficient to be defined as employed. We also use a variable representing whether the individual is employed full time. This definition implies that the person has to be registered as working 37.5 hours a week. In addition, we employ the number of days the individual has been employed last year (according to his/her contract) as an indicator. As mentioned, all outcomes are measured in the year they turn 30.

We see in Table 1, that 72 per cent of the sample of female, second-generation immigrants in our sibling sample have earnings from employment, while 50 per cent are full-time employed. The average across second-generation

Table 1. Summary statistics for women and men in the sibling sample

Variables	(1)		(2)	
	Women		Men	
	Mean	SD	Mean	SD
Employed	0.72	(0.45)	0.75	(0.44)
Full time	0.50	(0.50)	0.64	(0.48)
Days	222.88	(162.60)	243.21	(159.91)
FLFPR	31.36	(10.61)	31.36	(10.61)
Log FLFPR	3.34	(0.57)	3.34	(0.57)
N	9626		9626	

Table 2. Country summary statistics for women in the sibling sample

Country	FLFPR	Observations	Employed	Employed full time
Algeria	2.30	13	0.77	0.54
Argentina	19.92	33	0.67	0.52
Australia	28.73	46	0.65	0.46
Austria	30.95	99	0.73	0.51
Belgium	24.82	55	0.75	0.51
Brazil	20.31	15	0.67	0.47
Canada	29.77	81	0.72	0.53
Cape Verde	26.89	23	0.83	0.57
Chile	17.17	51	0.65	0.35
Colombia	14.38	14	1	0.86
Denmark	36.70	1,878	0.73	0.51
Egypt	4.55	20	0.50	0.35
Faroe Islands	20.35	115	0.70	0.48
Finland	39.13	403	0.71	0.48
France	29.94	114	0.74	0.56
Germany	30.96	897	0.74	0.52
Great Britain	33.75	1,157	0.73	0.51
Greece	21.23	32	0.72	0.59
Hong Kong	30.85	16	0.81	0.56
Hungary	38.06	146	0.74	0.53
Iceland	29.87	106	0.80	0.57
India	18.11	138	0.65	0.54
Indonesia	25.23	30	0.70	0.40
Ireland	20.28	33	0.67	0.52
Iran	7.69	14	0.57	0.57
Italy	21.46	103	0.76	0.45
Japan	36.83	48	0.81	0.60
Madagascar	46.90	29	0.83	0.79
Morocco	10.08	76	0.64	0.51
New Zealand	25.39	17	0.88	0.65
Nigeria	19.31	12	0.75	0.58
Pakistan	4.40	552	0.54	0.36
Poland	45.38	97	0.69	0.43
Portugal	27.99	18	0.67	0.39
Russia	45.49	26	0.77	0.50
Serbia	31.61	185	0.76	0.61
Singapore	25.09	11	0.64	0.55
South Africa	20.60	45	0.73	0.53
Spain	14.35	141	0.67	0.45
Sri Lanka	19.20	20	0.85	0.65
Sweden	37.53	1,400	0.76	0.52
Switzerland	32.24	89	0.76	0.57
Thailand	40.24	12	0.58	0.42
The Czech Rep	43.51	74	0.70	0.55
The Netherlands	20.02	297	0.72	0.54
The Philippines	14.68	65	0.74	0.55
Trinidad and Tobago	17.13	23	0.61	0.43
Turkey	31.55	110	0.51	0.31
United States	44.39	495	0.72	0.49

immigrants is however less interesting than the huge variation across ancestry countries. We show a list of all ancestry countries and respective FLFPRs in Table 2, which shows that the vast majority of second-generation immigrants have a background from West-European countries. Pakistan is the non-western ancestry country with the highest number of second-generation immigrants, with about 6 per cent of the female sample. Among countries with at least 10 female, second-generation immigrants in the sibling sample, 50 per cent from Egypt ($n = 20$) are registered with positive earnings, compared to 100 per cent ($n = 14$) from Colombia.⁷ We have a small number of observations from several of the ancestry countries; however, conclusions are robust to excluding e.g. countries with less than 40 observations.

Empirical Strategy

In estimating the effects of ancestry culture, a natural starting point is to follow the previous literature and estimate the correlations between our outcomes and FLFPRs for the total population of second-generation immigrants born in 1965–1980. We show these for women in Table 3, Panel A, and for men in Panel B. The results in Table 3 show a significant correlation between FLFPRs for females, but also for males, which tells us that the ancestry FLFPR picks up more than just beliefs about female employment. These results strongly suggest that previous estimates of the impact of gendered culture are biased upwards.

We suggest a sibling comparison approach to improve the plausibility of FLFPRs picking up attitudes specifically related to gender roles. That is, by comparing siblings of different sex, we are controlling for all factors affecting siblings equally, such as childhood environment, parental networks, time since immigration, and local labour markets, to the extent they affect siblings equally. These common factors are otherwise impossible to control for. In addition, the sibling fixed effects control for factors at the contextual level such as the share of immigrants in the area where the family lives and local labour market conditions. The approach further controls for characteristics correlated with FLFPRs and inherited outcomes, but uncorrelated with gender, such as work ethics and ancestry level human capital.⁸ In fact, the variation retained will by construction be the part of FLFPRs that affects daughters and sons differently. The linear models we estimate are of the following form⁹:

Table 3. OLS regressions of our outcomes on ancestry FLFPR

Variables	Employed	Full time	Days
Panel A: Women			
Log FLFPR	0.06*** (0.02)	0.05*** (0.02)	21.91*** (5.74)
Observations	20,976	20,976	20,976
Panel B: Men			
Log FLFPR	0.04*** (0.01)	0.04*** (0.01)	14.66*** (3.14)
Observations	22,276	22,276	22,276

*** $P < 0.01$, ** $P < 0.05$, * $P < 0.1$.

Note: Robust standard errors adjusted for clustering on country of ancestry. All regressions include a constant and a control for year of birth. The samples are the total population of second-generation immigrants born in the years 1965–1980. Outcomes are measured in the year they turn 30.

$$Y_{i,s} = \alpha_s + \beta_1 \text{FLFPR}_s \times \text{FEMALE}_{i,s} + \beta_2 \text{FEMALE}_{i,s} + \beta_3 \text{YEARBORN}_{i,s} + \beta_4 \text{YEARBORNSQ}_{i,s} + \epsilon_{i,s}$$

where i refers to individuals, s to sibling pairs, and α_s to sibling fixed effects. One individual can appear several times in the data set if s/he has multiple siblings. The inclusion of α_s implies that identification is from within-sibling pair variation. This approach is powerful since the sibling fixed effects effectively control for all the family- and country-level variation which affects brothers and sisters similarly. β_2 captures the average difference between the female and male sibling at a level of zero FLFPRs, while β_1 —our key estimate of interest—captures how the sibling differences vary depending on FLFPRs in the country of ancestry. The inclusion of α_s makes the assumption that β_1 captures the effect of cultural beliefs more plausible, compared to the estimates in Table 3. Note that FLFPR does not vary within sibling pair, thus it is perfectly collinear with the sibling fixed effects, and the ‘main effect’ of FLFPR is absorbed by the sibling fixed effects. We consistently control for year of birth and its square term. We estimate robust standard errors adjusted for clustering at country of ancestry since FLFPR varies at this level.¹⁰

Sibling fixed effects may, however, introduce a new set of problems (Frisell *et al.*, 2012). While the sibling fixed effects control for all factors that are time invariant within the families, a worry may be that there is some time-varying unobservable factor that affects our results. Since the siblings are born at different times, possible confounders could be changes in the parents’ networks over time, upbringing practices, or improvements of living standards. Unless there is sex-selective

Table 4. Sibling fixed effects results

Variables	Employed	Full time	Days
Log FLFPR \times female	0.04*** (0.01)	−0.00 (0.01)	16.01*** (3.43)
Female	−0.17*** [−0.05]	−0.14*** [−0.15]	−76.79*** [−28.78]
Observations	19,252	19,252	19,252

*** $P < 0.01$, ** $P < 0.05$, * $P < 0.1$.

Note: Robust standard errors adjusted for clustering on country of ancestry are presented in parentheses. All regressions include a constant and a control for year of birth and its square term. The brackets under the female coefficient shows the coefficient after a regression where we centre the FLFPR variable at the mean. Outcomes are measured in the year they turn 30.

abortion, however, the birth order of siblings is random.¹¹ In addition we control for year of birth. Thereby the internal validity of the results is likely to be unaffected. Whether the effects are generalizable to other types of families is, however, not certain. One factor could be that gender is more salient in families with both sons and daughters. We find comfort in the fact that the results are qualitatively similar if we subtract the male coefficient from the female coefficient in the cross-sectional specification. Furthermore, as shown in Supplementary Table A1, the sibling sample and the total sample are similar on observable characteristics but need not be so on unobservable characteristics.

Results

Main Results

Table 4 reports the main results. We estimate the exact regression displayed in the equation above, but only display the output of the key coefficients. The results in the first column show that sisters have a lower probability of being employed than brothers. In brackets we show the results after we mean-centre FLFPRs so that the coefficient captures the average difference between the female and male sibling at the mean level of FLFPRs. We see that, at average level of FLFPRs, sisters are 5 percentage points less likely to be employed. The precisely estimated interaction term says that the sister–brother difference varies with FLFPRs in the country of ancestry. In our sample, (mean-centred) Log FLFPR varies between −2.4 and 0.94, implying that the estimated sibling gap varies between −14.6 percentage points and −1.2 percentage points. A 1 SD difference in the cultural proxy (0.57) amounts to a 2 percentage point difference in the probability of having earnings from employment. This is of course a non-negligible impact of ancestry

culture, but smaller than estimates in e.g. Fernández and Fogli (2009).

If we move from examining the impact on the probability of being employed to examining full-time employment, we find less clear results. We find a large gender gap in the probability of full-time employment, but the full-time gender gap does not vary with ancestry FLFPRs. There is also a correlation between ancestry culture and the probability of working full time, but this correlation is present for both sexes as seen in Table 3. However, if we examine contracted days of work (column 3), we again find a gender gap which varies according to ancestry culture. At the lowest observed FLFPR, the gap is estimated to 67 days, compared to 14 days at the highest observed FLFPR.

In the Supplementary Appendix we present a set of robustness checks of the main specification. If we measure FLFPRs in levels (see Panel A in Supplementary Table A3), we find that the estimated gender gap on any earnings from employment is between 5 and 13 percentage points¹² and between 15 and 57 on number of days employed. Results are very similar if we rely on alternative definitions of second-generation immigrants, that is, if we define a second-generation immigrant based on the mother's or the father's country of ancestry. This is potentially important as it could be that skills are more easily transferred from father to son and mother to daughter. If that is true, and if mothers worked less for institutional reasons in the source country, we may have attributed the effect to ancestry culture while it would have been lack of transferable skills for institutional reasons. The results are also similar if we restrict the sample to those who have two foreign-born parents (see Supplementary Table A4).

Finally, in Supplementary Table A6 we present results using the proportion agreeing that men should have more right to a job than women if jobs are scarce (from the World Values Survey) and FLFPRs measured in the year 2000 as alternative proxies for culture. As seen in Supplementary Figures A1 and A2 there is a very close relationship between these two measures and our main measure. Using FLFPRs measured in the year 2000 produces almost identical results to those in Table 4; the coefficients are roughly twice as big, but since the SD on this variable is about half of the FLFPRs used in the main results (0.34 versus 0.57), a 1 SD difference amounts to a similar difference in the outcomes. When we rely on the attitudinal proxy we get results that point in the same direction; however, the estimated effect is somewhat smaller (the SD for *jobs for men* is 0.19) and less precisely estimated ($P = 0.11$ in the employment regression). Less precise estimates in this regression is to

be expected since the number of ancestry countries is smaller using the attitudinal proxy, and because measurement error might be larger since this proxy is based on survey data.

It should be noted that the effect we identify is an average effect for many cultures and many time periods. This does not imply that we assume the effect of culture to be stable over time. Neither do we think that the effects are deterministic in the limited sense that all second-generation women with ancestors from countries with lower FLFPRs will have lower employment in Norway. There is obviously a large degree of heterogeneity at the individual level. Even at the ancestry country level there is a lot of heterogeneity in the effects. In Supplementary Figures A3–A5 we divide the sample into 22 equal-sized bins and plot the average country-level gender difference from separate sibling fixed effects regressions against the average log FLFPRs by source country. We fit regression lines based on the underlying data and find that while the effects seem to be linear with respect to FLFPRs for employment and days worked, they are by no means monotonic.¹³

Co-ethnic Networks

Next we explore whether the relationship between ancestry culture and outcomes depends on co-ethnic networks in the county of upbringing. There are many reasons for why the number of co-ethnics in the community may affect the effect of culture. For instance, a large share of immigrants may affect the local labour market and the local schools. The share of immigrants has also been argued to affect the level of discrimination as larger groups may pose more of a threat or, conversely, larger groups may fare better if there is more hiring from co-ethnics (Van Tubergen *et al.*, 2004). Relatedly, there may be gains from having a large share of co-ethnics in terms of 'ethnic capital', especially if they are highly educated (Borjas, 1995). An important benefit of the sibling fixed effects approach is that such factors are controlled for to the extent that co-ethnic networks affect the siblings similarly.

However, it may be the case that co-ethnic networks affect the local community or the effects of culture in a gender-specific way, and that such influences differ for immigrants with different ancestry culture. The effects of ancestry culture may be reinforced or inhibited as co-ethnic networks might make gender-specific issues more salient in the community and hence make parents treat boys and girls differently. We here conduct a complementary analysis where we explore whether the effect of ancestry culture differs in different type of areas depending

Table 5. Share of co-ethnic immigrants in the county in 1992

Co-ethnics Dep. Variable	High Employed	Low Employed	High Full time	Low Full time	High Days	Low Days
Log FLFPR × Female	0.04*** (0.01)	0.04** (0.02)	−0.02 (0.01)	0.01 (0.02)	15.61*** (2.08)	17.11** (6.62)
Female	−0.18*** (0.02)	−0.17*** (0.06)	−0.12*** (0.04)	−0.18*** (0.05)	−80.62*** (7.48)	−74.98*** (22.07)
Observations	9,855	9,397	9,855	9,397	9,855	9,397

*** $P < 0.01$, ** $P < 0.05$, * $P < 0.1$.

Note: Robust standard errors adjusted for clustering on country of ancestry are shown in parentheses. All regressions include a constant and a control for year of birth and its square term. Ancestry FLFPR is in log form. Outcomes are measured in the year they turn 30.

on the share of immigrants in the community. Our analysis will be able to tell us whether the gender difference in the effects of culture is related to the share of co-ethnics in the community. As such, it speaks to the distinction between direct vertical and horizontal transmission of culture (Bisin and Verdier, 2005, 2010), referring to socialization within and outside of the family.

To analyse the role of co-ethnics for the effects of culture, we construct a measure of whether the county of upbringing is one with relatively many co-ethnics. This is done by constructing a dummy that equals 1 if there were more than the median share of co-ethnics living in the county in 1992 and zero otherwise.¹⁴

In Table 5 we see that the differences are generally small between different areas depending on the number of co-ethnics living there in 1992, and none of the differences in the Log FLFPR X Female coefficients between the two samples are statistically significant. We further show in Supplementary Table A7 that the results are similar if we instead split on the number of co-ethnics in the municipality, or if we count the number of highly educated co-ethnics in the county (Supplementary Table A8). Hence, it does not seem like co-ethnic networks affect the gendered effects of culture in a substantive manner. This may suggest that for gender and employment outcomes, vertical transmission is more important than horizontal transmission. It should be noted, however, that identification of the effects of co-ethnic networks is not as strong as the identification of the effects of ancestry culture, since immigrants self-select into areas of residence. Parents may choose where to live as part of their vertical socialization effort and there are many other factors that differ across areas.

Concluding Discussion

The striking cross-national differences in women's employment across the world is likely a function of both differences in culture and differences in institutions.

Measuring the impact of any of these dimensions is challenging, not least since their variation is correlated with differences in the other dimension. In an attempt to identify the causal effects of culture on a number of outcomes, recent research has employed the so-called epidemiological approach (Fernández, 2007; Fernández and Fogli, 2009; Polavieja, 2015). By investigating outcomes for immigrants from different source countries in the same institutional environment, this approach can potentially separate culture from institutions. Immigrants are, however, already affected by the institutions in their home country, so, when possible, the previous literature circumvents this issue by studying behaviour of second-generation immigrants, as they have not experienced ancestry institutions.

Even though focussing on second-generation immigrants is better than investigating the migrants themselves, it is no panacea. Most importantly, it introduces the worry that we are picking up other factors that are transmitted through generations. That is, in addition to passing on their culture, parents also transmit social-, economic-, and human-capital to their children. Moreover, it is difficult to pinpoint what aspect of the ancestry culture that is transmitted. In addition to differences in gendered beliefs, the approach may pick up differences in e.g. work ethics and quality of schools.

We are interested in norms and beliefs about women's work, and we apply an extended version of the epidemiological approach to study the cultural impact on employment outcomes for female, second-generation immigrants in Norway. Having access to very detailed registry data on the whole Norwegian population we are able to extend the epidemiological approach and exploit variation within cross-sex sibling pairs. That is, we can include sibling fixed effects in our estimation framework and thereby improve the plausibility that our estimates pick up the influence of attitudes related to gender roles. The approach allows us to control for all factors that affect siblings equally, such as upbringing, parental

networks, and parental resources, as well as characteristics correlated with ancestry FLFPRs and inherited outcomes, but uncorrelated with gender, such as work ethics and school quality. Hence, the resulting measure will only capture differences in ancestry culture that are correlated with ancestry FLFPRs, but that affects men and women differently in the host country.

We find that ancestry culture matters for female, second-generation employment but that previous estimates are likely biased upwards. We further find that ancestry culture has less persistent effects on female employment in Norway, than in comparable studies from the United States (Fernández, 2007; Fernández and Fogli, 2009). The method we use can be applied to other sociological questions regarding the effects of culture on other outcomes, in particular we advice scholars interested in the gender dimensions of culture to always differentiate out the more general cultural aspects by taking the difference between men and women. We especially encourage future studies using the same approach in other settings. By having many empirically trustworthy measures from different contexts we will increase our understanding of factors for social change.

Separating the effects of culture on female employment by using the epidemiological approach only gives a partial answer on the role of culture, however. The approach, by construction, purges away any impact that institutions have as moderators for the effects. There are reasons to suspect that the impact of ancestry culture on female employment will vary across institutional settings. First, since institutions affect the rewards and benefits of cultural action and as institutions and policies affect the gendered division of labour (Kotsadam and Finseraas, 2011, 2013). Second, it is likely that the effects differ with respect to female employment, as the experiential perspective has been shown to be important for social trust (Uslaner, 2011; Dinesen, 2013; Helliwell *et al.*, 2014; Nannestad *et al.*, 2014), and as gender roles are particularly malleable to social relational contexts (Ridgeway and Correll, 2004). We hope that future scholarship can investigate these interactions by combining the extended version of the epidemiological approach with well-identified effect estimates of institutional change.

Notes

- 1 More broadly, the question of the effects of cultural heritage has a long history in sociology with prominent contributions analysing variation in violence (Gastil, 1971; Nisbett and Cohen,

1996) and more recently civic culture (Rice and Feldman, 1997) in the United States.

- 2 Van Tubergen *et al.* (2004) also document a positive association between female employment in the country of origin and female employment in the country of destination, but only for first generation immigrants.
- 3 For instance, in the 2005–2009 round of the World Values Survey, 56 per cent of Norwegians agree or agree strongly that ‘being a housewife is just as fulfilling as working for pay’, compared to 78 per cent in the United States.
- 4 The register data we have cover our outcomes for the years 1995–2010, which is why we study the 1965–1980 cohorts.
- 5 In Supplementary Table A1 we present the summary statistics for the entire sample and we note that the samples are very similar with respect to all variables.
- 6 The ILO database reports annual FLFPRs, but with gaps in the time series. These gaps vary across countries, but the majority of the countries are observed in 1970 and in 1980. We interpolate between observations and use the interpolated value if the true value is missing.
- 7 We exclude countries with less than 10 observations from the table for reasons of anonymity.
- 8 A simpler correction of this last problem is to subtract the male coefficient from the female coefficient in Table 3. This produces results more in line with the sibling fixed effects model.
- 9 In Supplementary Table A2 we present fixed effects logit models of the two binary outcomes.
- 10 The standard errors are similar if we cluster on sibling pair (see Panel B in Supplementary Table A3).
- 11 Recent research has used the epidemiological approach to investigate the cultural component of biased sex ratios, and find that immigrants from China, India, South Korea, and Taiwan display signs of sex selective abortion in the United States and the United Kingdom (Dubuc and Coleman, 2007; Almond and Edlund, 2008; Abrevaya *et al.*, 2009). Excluding second-generation immigrants from these countries does not affect our results (see Supplementary Table A5).
- 12 The FLFPR variables are first mean-centred and then divided by 100 for ease of presentation. The maximum/minimum on FLFPR X Female is

0.285/0.212 and the max on the square terms is 15.6/10.9.

- 13 In Supplementary Figures A6–A10 we present the gender difference in employment from sibling fixed effects by ancestry country. Although many of the coefficients are negative, most are statistically insignificant.
- 14 Unfortunately, we do not have exact information on county of upbringing. However, we know where parents lived in 1992 and assume that this is the county of upbringing.

Supplementary Data

Supplementary data are available at ESR online.

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