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ESSAYS IN GENDER, EARNINGS AND GEOGRAPHY

by

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ABSTRACT

In this dissertation I study the role of local markets and firms in explaining labor market inequality across genders, and across workers. My results show that local labor markets have a relevant role in accounting for differences in labor market outcomes across genders. The dissertation is structured in three chapters, each containing a stand alone paper.

In the first chapter I show large and persistent differences exist in women's labor force participation within multiple countries. These persistent differences in employment can arise if where women grow up shapes their work choices. However, they can also arise under endogenous sorting, so that women who want to work move to places where more women work. In this chapter, I use rich data from Indonesia to argue that the place women grow up in shapes their participation in the labor market as adults. To do so, I leverage variation coming from women moving across labor markets to estimate the effect on women's labor force participation of spending more time in their birthplace. My strategy is similar to that of Chetty and Hendren (2018) and compares the labor supply choices of women who currently live in the same location, but who emigrated from their birthplace at different ages. My results indicate that birthplace has strong and persistent effects on adult women's labor supply. By the time they turn sixteen, women born in a location at the 75th of female employment

will be 5 p.p. more likely to work than those born in a 25th percentile location. Place is particularly important during the formative period between 9 and 16 years old. These results suggest that between 23 percent of the current spatial inequality in women's employment is transmitted to the next generation growing up in these locations.

The second chapter studies the relationship of big cities and gender inequality in the United States. It is well known that big U.S. cities pay higher wages, but there is growing evidence that this urban wage premium declined since the eighties (Autor, 2019). In this paper, I use data from U.S. Commuting Zones for the period between 1970 and 2020 to document that the decline in the urban wage premium affected men and women differently. While women were relatively isolated from the premium decline, men with lower education received the brunt of the impact. This caused a large relative increase in women's urban wage premium: women's premium went from being on par with men's in 1970 to being 44% larger in 2010. I go on to argue that these differential trends result from a combination of gender specialization and the evolution of urban skill premiums. Urban premiums decline the most in those skills low-education men use more intensively.

Finally, the third chapter I study the role of universities in explaining earnings inequality in U.S. academia. Previous applications from Abowd, Kramarz, and Margolis (1990) –AKM– found the best firms pay workers over and above their own productivity. These firm rents contribute to overall wage inequality. In this paper, we apply the AKM model to measure whether there are significant firm (university/college) effects on faculty earnings in academia. Specifically, we apply the model to measure the pecuniary rents associated with working as tenure-track faculty at a more prestigious university or college in the United States. To do so, we take advantage of matched employer-employee data from the Survey of Doctorate Recipients. We find

little evidence of pecuniary university premiums in the most prestigious US academic institutions. Once we control for urbanicity, the effect of university/college rankings on institutions' fixed-effects on earnings is statistically insignificant and sufficiently precisely measured that we can rule out anything larger than modest effects. We then relate our findings with those of previous literature.

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List of Abbreviations

ACS	......	American Community Survey
CZ	......	Commuting Zone
DOT	......	Dictionary of Occupational Titles
FLFP	......	Female labor force participation
IFLS	......	Indonesian Family Life Survey
IPUMS	......	Integrated Public Use Micro Samples
p.p.	......	Percentage points
SUPAS	......	Intercensal Survey
SUSENAS	......	National Socieconomic Survey

Chapter 1

The Geography of Women's Work: Evidence from Indonesia

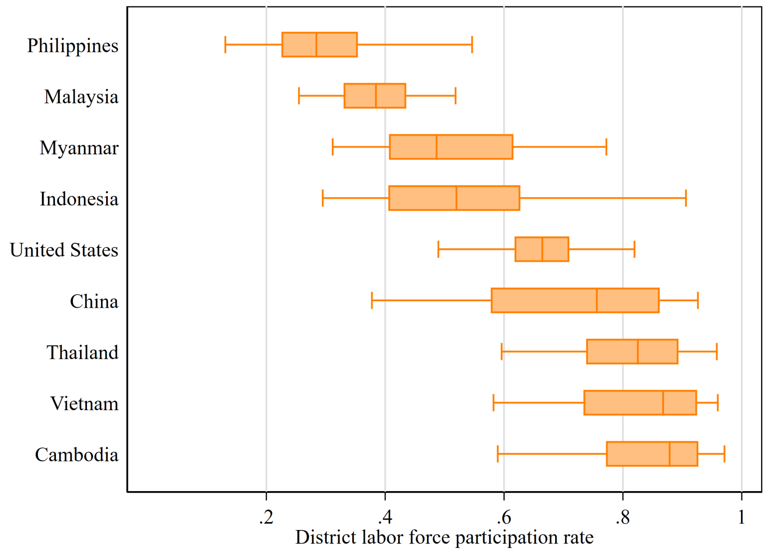
1.1 Introduction

There are surprisingly large and persistent differences in female labor force participation (FLFP) rates within multiple countries at different levels of development. I show this in figure 1·1, where I illustrate the high dispersion in subnational labor force participation within several developing countries and the United States. The FLFP rate gap between two localities within these countries can be as large as 15 percentage points (p.p.).[1] This large within-country dispersion in FLFP has generally gone unnoticed in the literature (Charles et al., 2018), and, as a consequence, we know very little about its causes and implications on women's outcomes. Particularly, there is scarce evidence of whether being born in localities with high or low participation of women in the labor market affects women's labor market participation. Consequently, we have limited insight into the extent to which current disparities are a constant feature of these localities, or whether it is transmitted across generations.

In this paper, I show that the subnational dispersion in female labor force participation has strong effects on the labor market outcomes of women born across different areas within the same country. To do so, I use rich data from internal In-

[1]Using the interquartile range as a benchmark, the gap between the localities at the 75th and the 25th percentiles of female labor force participation rates is over 15 p.p. for 6 out of the nine countries in the figure. It is 28 p.p. for China, 22 p.p. for Indonesia, and 10 p.p. in the United States.

Figure 1·1: Female labor force participation rates at the district level for selected countries



Note: The figure shows the distribution of female labor force participation rates for a large subset of Asian countries with geographic data available in IPUMS International. Countries are ordered by median district employment rate. I use the latest available sample from IPUMS International for each country. I aggregate data at the smallest geographical unit available which often corresponds to a district/county, except in the United States where I aggregate data for the US into Commuting Zones as in Autor and Dorn (2013). See table A.2 for data on a larger cross-section of countries.

donesian female migrants to show that their birthplace has a strong impact on their adult labor force participation.[2] I identify the birthplace causal effect by leveraging variation coming from women living in the same labor market as adults but who left their birthplace origin at different ages as children. Therefore, I exploit variation in the time spent in the birth location to disentangle the causal effect of the birthplace from variation driven by differences in women's unobservable characteristics or the locality's characteristics. My strategy boils down to comparing the labor force participation of women who emigrated in early childhood, versus those who left in their early teens. Then, if women born in places with higher female labor force participation are more likely to work the longer they stay there, I surmise that this is driven

[2]Migrating is a relatively common phenomenon in Indonesia, with approximately one in five Indonesians residing outside their birth locality.

by the effect of their birth location. Moreover, by comparing women living in the same location as adults, I abstract from the effect of current labor market conditions and uncover variation that is likely driven by women's labor supply. This strategy builds on that of Chetty and Hendren (2018a), and focuses it on female outcomes in a large developing country.

My results indicate that birthplace has strong and persistent effects on adult women's labor supply. I show this in two steps. First, I show that women's birthplace is highly predictive of their labor supply choices. Conditional on living in the same local labor market, women born in localities with high female employment are much more likely to work than those born in places with low-female employment. This relationship holds true even for women who migrated at a young age when it is uncommon for women to be part of the labor force. Next I show that this relationship reflects the causal effect of birthplace on women's employment by exploiting differences in the timing of emigration. I use a strategy similar to a Difference-in-Differences design, where I compare the labor force participation of women living in the same labor market but who emigrated from their birthplace at different times. Under the assumption that the omitted variable is constant for women emigrating at different ages, this strategy allows me to distinguish the causal effect from differences in women's characteristics.

I find that spending late childhood and early teen years in areas with high female employment makes women more likely to work as adults. Moreover, the longer they stay these locations, the likelier they are to enter the labor force. Under my preferred specification, staying in a place at the 75th percentile of female employment between the ages of 6 and 16 years old makes women five percentage points more likely to work than those born in a place at the 25th percentile. These magnitudes are quantitatively important as they imply that approximately 23% of the current spatial inequality in women's labor force participation is transmitted to the next generation of women through birthplace effects.

These estimates assume that omitted variable bias is constant across emigration ages; that is, the correlation between birthplace female employment rate and other unobserved determinants of women's labor supply is the same no matter the age they emigrated. Note that differences between women born in different locations in factors I do not control for are not enough to violate this assumption. For example, women from high female employment locations may be more likely to work because they had parents with higher resources to invest in their education than those born in locations with low female employment. This would generate differences between women from different origins that is not driven by birthplace effects. However, this does not necessarily violate the constant bias assumption. Instead, a violation would require the resource gap to become larger or smaller for cohorts of women who emigrated at older ages. In the paper, I provide evidence demonstrating that the gap in resources and other covariates remains fairly constant across different ages of emigration, thereby supporting the assumption underlying my identification strategy.

Why would childhood exposure to the birthplace labor market have such persistent effects on women's outcomes? Previous research has suggested three main potential mechanisms: (i) higher investment in schooling, (ii) changes in parental investment, (iii) transmission of culture and/or gender norms (Molina and Usui, 2022; Fogli and Veldkamp, 2011; Blau et al., 2011). Exposure to labor markets with a higher proportion of working women could shape the career expectations of young girls, leading to greater likelihood of staying in school. However, I find limited support for this mechanism in Indonesia. In fact, my results indicate that women's schooling is largely unaffected by longer exposure to localities with higher female employment. Furthermore, changes in parental investments are unlikely to account for my results. My findings indicate that women who had longer childhood exposure to regencies characterized by high female employment are more likely to enter the labor market as adults. If parental investments were the primary driver behind these outcomes, it would suggest that parental investment is highly sensitive to the duration of their child's exposure. Given that parents have resided in these locations for a considerable period of time, it seems unlikely that such a high level of sensitivity exists. A more plausible explanation lies in the transmission of cultural and gender norms. I provide evidence that childhood exposure to these high-employment areas also influences decisions related to fertility and marriage. Moreover, I find that the birthplace effect is particularly pronounced during the ages when children's attitudes towards gender equality are still malleable (Jayachandran, 2021).

In the paper, I take advantage of rich Indonesian data that stores people's birthplace and current location at a detailed geographic level. My main analyses source data from all waves of the Indonesian Family Life Survey (IFLS) and the 1985, 1995, and 2005 intercensal surveys (Statistics Indonesia, 2021; Minnesota Population Center, 2020). These representative and publicly available datasets track respondents' birthplace, current location, and migration history across midsized geographies. This level of detail allows studying differences in women's labor supply and birthplace effects at a level that is not possible in other countries from traditional sources (Bryan and Morten, 2019). Throughout the paper, I identify localities as Indonesian "regencies." There are medium-sized administrative geographies akin to counties in the United States. The average regency is approximately twice the size of the US state of Rhode Island and houses eight hundred thousand people.

This paper contributes to three strands of the literature. I contribute to the growing research showing that local labor markets can permanently affect women's labor supply, fertility, and human capital investment choices (Molina and Usui, 2022;

Charles et al., 2018; Boelmann et al., 2021). I make three main contributions to this literature. First, by applying techniques borrowed from the place effects literature (Chetty and Hendren, 2018a,b; Milsom, 2021), I provide causal evidence that women's birthplace has large and persistent effects on women's labor supply. This complements evidence from previous literature, which shows that exposure to current labor markets can have effects on women's expectations, labor supply, and educational investment (Molina and Usui, 2022; Boelmann et al., 2021; Milsom, 2021). Second, I also provide evidence of the ages at which birthplace is key in shaping labor supply. Although previous research has pointed out that women's childhood environment matters for their adult outcomes, this literature is mostly silent on *when* it matter (Chetty et al., 2016). Third, my results provide new evidence that where women grow up can matter more locally. Previous research emphasizes that differences in norms, culture and other factors across large geographical areas such as states, provinces, or countries can shape women's choices (Charles et al., 2018; Boelmann et al., 2021; Alesina et al., 2013). By exploiting much more disaggregated data, my results suggest these factors can act at a more local level.

Second, this paper also contributes to the literature on place effects. Primarily using evidence from developed countries, this literature shows that where people grow up and live has important implications for intergenerational mobility (Chetty and Hendren, 2018a,b), racial inequality (Chetty et al., 2020), human capital accumulation (Molina and Usui, 2022), and criminal activity (Damm and Dustmann, 2014). I add to this literature by providing new empirical evidence linking women's birthplace to their outcomes as adults in a large developing country. In this way, my findings complement existing work showing that spatial inequality is particularly important for women's human capital investment in West Africa (Milsom, 2021) and Japan (Molina and Usui, 2022).

Finally, my paper also contributes to the literature studying the determinants of women's labor supply. This literature has primarily exploited cross-country differences in female labor supply to study its determinants and its implications (Olivetti and Petrongolo, 2008, 2014; Blau et al., 2020; Blau and Kahn, 2015). In this paper, I document the existence of large and persistent differences in female labor supply within multiple developing countries and study some of its implications. In this way, my approach is closer to the recent literature documenting that factors such as commuting and sexism can help explain the geographic differences in women's labor supply within the United States and France (Charles et al., 2018; Le Barbanchon et al., 2021; Black et al., 2014; Moreno-Maldonado, 2019).

1.2 Data

1.2.1 Data sources

My main analyses use data from the Indonesian Intercensal Survey (SUPAS) and the Indonesian Family Survey (IFLS). These two datasets record detailed data on people's birthplaces, their migration histories, and their labor supply. I supplement this data with place characteristics coming from the Indonesian Census and the National Socioeconomic Survey (SUSENAS).

My primary results come from the Intercensal Survey. This is a decennial survey containing social and demographic information for approximately 0.5% of the Indonesian population. This dataset has two advantages that make it uniquely suitable to study place effects on female labor supply. First, it records people's birthplace, previous location and location of birth in midsized geographic units. The survey tracks this information at the level of the "regency", which are administrative units similar to US counties. Research on Indonesia typically uses them to identify local labor markets (Magruder, 2013; Bazzi et al., 2022), and their size allows me to study differences in women's employment across smaller geographic units than what I could observe in alternative datasets.[3] The typical regency is home to approximately eight hundred thousand people and covers an area roughly twice size of the US state of Rhode Island. Appendix figure A·1 depicts all the 268 regencies in my dataset.

Second, rich data on historical migration patterns allows me to recover the age at which individuals departed from their birthplace. Specifically, the survey records the length of time each person has lived in their current location. With this data, I can determine the age at which individuals *who have only migrated once in their lifetime* left their birthplace. These are people whose previous place of residence is the same as their birthplace. This is the key variation that I exploit in my identification strategy.

In addition to these two advantages, the Intercensal Survey also has a sizable sample size of approximately two and a half million people. Its main limitation, however, is that it contains limited demographic information. Therefore I supplement my main results with information coming from the Indonesian Family Life Survey (IFLS). Unlike the Intercensal Survey, the IFLS contains rich socioeconomic information, such as childhood conditions and proxy measures of parents' wealth, among others, that allow for the study of potential drivers of the birthplace effects. However, this comes at the cost of a smaller sample size. The IFLS is a panel survey that tracks the information of approximately 34,000 Indonesians across five survey years: 1993, 1997, 2000, 2007, and 2014. Overall, the IFLS is representative of 83% of the Indonesian population.[4]

I source place characteristics from the 1980-2010 Indonesian Decennial Censuses available in IPUMS International (Minnesota Population Center, 2020) and the 2012,

[3]Datasets available for other countries track geographic information only for states or provinces, which in most cases are either too big or too few to be interesting (Bryan and Morten, 2019)

[4]The IFLS originally sampled households from 13 of the 27 provinces that existed in 1993. These provinces account for 83% of the Indonesian population. I use retrospective work and migration history questions to construct a panel tracking the respondents' location since birth and their yearly employment history from 1988 to 2014.

2013, and 2014 National Socieconomic Surveys (SUSENAS). (Statistics Indonesia, 2019, 2020). The Censuses and SUSENAS are very similar to each other but the Census has larger sample sizes. I compute all regency characteristics by restricting the sample to people aged 18 to 64 and aggregating these datasets at the regency level. Whenever possible, I compute these aggregates from the Census.

1.2.2 Measurement

My main measure of women's labor supply is a dummy equal to one if she was employed during the year.[5] I use this variable because it is the one I can most consistently track across years and across datasets. However, as a robustness check, I also examine alternative measures such as being a paid worker, total weekly hours worked, and being a full-time worker to confirm the robustness of my findings.

In this analysis, I link women's labor supply choices to the characteristics of their birthplace. This requires having geographic units with boundaries that remain fixed over time. Unfortunately, regency boundaries in Indonesia underwent significant changes from decade to decade between 1980 and 2010, with the creation of new regencies being a common occurrence. Appendix Table A.1 shows that between 2000 and 2010 alone, 154 new regencies were established. To address this issue, I use regency aggregates with consistent boundaries that span the period from 1970 to 2010. These regency aggregates were constructed by IPUMS International (Minnesota Population Center, 2020) and consist of 268 regencies that are only slightly larger than the "original" regencies in the data. Moving forward, I will refer to these regency aggregates as regencies.

For my main analysis, I restrict my sample to one-time internal migrants because this is the population for which I can separate the current place of residence from

[5]This definition classifies unpaid and family workers as employed. The patterns I discuss look similar when I focus on paid workers only.

the birthplace. I define migration as living outside the regency of birth. Moreover, whenever I link women's employment with birthplace characteristics, such as FLFP or urbanicity, I source these from the 2010 Indonesian Census. In robustness checks, I show that my results are similar when I use information from other census years.

1.2.3 Summary statistics

In the this section, I provide an overview of my data and the Indonesian labor market using data from the pooled 1985, 1995, and 2005 Intercensal Surveys. I obtain a qualitatively similar picture if I use the IFLS. Table 1.1 provides a general description of the entire dataset, as well as statistics disaggregated by gender. This table highlights three critical features of the Indonesian labor market. Firstly, internal migration is common, with approximately one-fifth of Indonesians residing outside their birthplace. These internal migrants are the primary focus of my analysis and, as the table shows, they represent a large cross-section of the Indonesian population. Secondly, the labor market in Indonesia is predominantly informal and agrarian, with 49% of workers being self-employed and working in agriculture.Additionally, there are significant gender gaps in employment, worker type, and industry. Women are 38 percentage points less likely to work than men, which while large is consistent with patterns observed in Southeast Asia. Furthermore, women are five times more likely than men to be unpaid or salary workers. Unpaid workers are people that work or help to earn an income but are not paid a wage or salary. Most unpaid workers work in agriculture (82%) and the retail industry (10%) (Minnesota Population Center, 2020). Lastly, women are more likely than men to work in service and manufacturing industries.

Table 1.1: IFLS: summary statistics by gender and migration status

	All (1)	**Women** (2)	**Men** (3)
Age	35.54	35.36	35.72
Married	0.71	0.72	0.71
Secondary completed	0.23	0.20	0.27
Urban	0.37	0.37	0.38
Muslim	0.81	0.81	0.81
Migrant	0.21	0.20	0.22
Share left birthplace by age 25	0.13	0.13	0.12
Employed	0.66	0.47	0.85
Type of worker			
Self-employed	0.49	0.38	0.56
Salaried	0.34	0.27	0.37
Unpaid / family worker	0.17	0.35	0.07
Industry of employment			
Agriculture	0.49	0.51	0.48
Services	0.36	0.37	0.36
Manufacturing	0.09	0.11	0.08
Construction	0.05	0.01	0.07
Observations	1,317,825	667,691	650,134

Notes: data from the 1985, 1995 and 2005 Intecensal Surveys.

Table 1.2: Indonesia: women's characteristics by migration status

	Non-migrants (1)	**Emigres** (2)	**Left before 19** (3)
Age	35.50	35.43	30.35
Married	0.71	0.75	0.66
Secondary completed	0.16	0.31	0.26
Urban	0.30	0.65	0.63
Muslim	0.81	0.83	0.85
Share left birthplace by age 25	0.00	0.66	1.00
Employed	0.48	0.42	0.40
Type of worker			
Self-employed	0.39	0.34	0.33
Salaried	0.24	0.42	0.41
Unpaid / family worker	0.37	0.24	0.26
Industry of employment			
Agriculture	0.56	0.30	0.33
Services	0.32	0.59	0.53
Manufacturing	0.11	0.11	0.13
Construction	0.01	0.01	0.01
Reason for migrating[1]			
Work		0.14	0.10
Family		0.00	0.00
Education		0.06	0.08
Other		0.81	0.82
Observations	518,018	134,031	47,769

Notes: Data from the 1985, 1995 and 2005 Intercensal Surveys. Column (2) shows data for women living outside their birthplace, while column (3) does it for those who left before they turned 19.1 Uses data from the 1985 Intercensal Survey. The 1995 and 2005 surveys have data on reason for migrating for only a very restricted set of migration episodes.

In table 1.2 I zoom in on the women migrants (emigres). They are the focus of my main analysis because they are the women for whom I can distinguish between the current place of residence and the regency of birth. I present statistics for all female migrants as well as for those who migrated before they turned 19. The table highlights significant differences between migrants and stayers. They are more educated but less likely to be employed than stayers. Moreover, they are more likely to have salaried jobs and live in urban areas. This suggests that women migrants are moving to areas with more formal labor markets and less rural surroundings. Lastly, column (3) shows that other than the marriage rates and the level of education, women who left their birthplace young are generally very similar to the typical female migrant.

In the final rows of table 1.2, I provide additional details on the factors driving women's migration, which are sourced from the 1985 Intercensal Survey. Women's migration is largely motivated by reasons other than work. Specifically, over 85% of female migrations are associated with either education or "other reasons". Unfortunately, the survey does not provide a breakdown for the latter category. However, data from the IFLS indicates that the majority of these moves are likely due to family-related reasons.

The fact that migrant women are more likely to work in the service sector could suggest that migration in Indonesia is predominantly rural to urban. However, table 1.3 shows this is not case. There are substantial rural-to-rural and urban-to-urban flows. In this table, I follow Bryan and Morten (2019) and classify regencies into urban or rural according to the share of the regency's population that lives in areas that Statistics Indonesia labels as urban in the Indonesian Census. Urban regencies are those whose urban population is above 43.3%. I chose this cutoff so that the share of people living in regencies I classify as urban matches the aggregate urban share from Statistics Indonesia. I then compute migration statistics for women born in urban and rural regencies. The table shows three salient features. First, migration is not exclusive to rural regencies. Women born in urban regencies also migrate at high rates. Second, migration is not just rural-to-urban. Panel A breaks down the migration flows by origin and destination. The urban-to-rural, rural-to-rural and rural-to-rural flows are substantial. Finally, panel B shows that there is considerable heterogeneity in employment rates within each regency classification. There, I show summary statistics for the female employment rates within these categories. There is substantial dispersion in female employment *within* both of these categories. Thus, the dispersion in female employment rates I discuss in the next section is not driven only by differences between urban and rural areas.

Table 1.3: IFLS: women's migration patterns and regency characteristics by urbanicity of regency of origin

	Birth regency		
	Rural	**Urban**	**Total**
	(1)	(2)	(3)
Number of regencies	168	100	268
Share of women born in these regencies	0.39	.61	100
Migration rate	0.18	0.23	0.20
A. Share of emigres living in:			
Rural regencies	0.44	0.31	0.38
Urban regencies	0.56	0.69	0.62
B. Characteristics of origin regency			
Women's employment rate			
Average	0.57	0.46	0.53
STD	0.14	0.11	0.14

Notes: I define migration as living outside the regency of birth. Following Bryan and Morten (2019) I classify regencies as urban if the share of population living in an urban area is above a 43.3%. I choose the cutoff to match the urban share at the national level. Data from IFLS and IPUMS International.

1.3 Four facts about women's labor supply

In this section, I use data from IPUMS International and the 1980-2010 Indonesian Censuses to present four empirical facts on female employment. First, I use data from several countries to show that large geographic differences in women's employment rates within countries are pervasive across the world. Next, I zoom in on Indonesia and (i) characterize the large dispersion in female employment across regencies, (ii) document that it is highly persistent over time, and (iii) show that it is not accounted for by variation in women's demographics or labor market characteristics across regencies. Taken together, these four facts suggest that structural differences could be driving the dispersion in women's labor supply within Indonesia.

1.3.1 Fact 1: within-country dispersion in women's labor supply is pervasive across countries

In table 1.4, I provide a snapshot of the within-country variation in men's and women's employment rates for a several countries, including Indonesia and the United States. These are a subset of the countries with regional employment data available below the province or state level in IPUMS International in the appendix.[6] For all countries, I restrict the sample to people aged 18 to 64 and compute the employment rates at the smallest geographical unit available. This often corresponds to an administrative unit similar to a county or municipality. The table orders countries from highest to lowest dispersion in female employment rates, as measured by the interquartile range (IQR) in employment.

[6] Data for the full set of countries is available in table A.2 All the insights discussed in this section generalize to this larger set of countries. Further details about the cross-country data are available in section A.3 in the appendix

Table 1.4: Dispersion in regional employment rates for selected countries

	Women			Men			Avg unit	N. geographic
	IQR	SD	Mean	IQR	SD	Mean	population	units
Country	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
China	0.28	0.17	0.71	0.14	0.10	0.85	266,748	2,845
Indonesia	0.22	0.14	0.53	0.05	0.04	0.87	533,867	268
Myanmar	0.21	0.13	0.51	0.07	0.05	0.86	83,531	362
Panama	0.20	0.12	0.33	0.04	0.08	0.80	56,049	35
Vietnam	0.19	0.12	0.82	0.06	0.06	0.90	79,146	674
Brazil	0.19	0.11	0.48	0.19	0.11	0.73	59,010	2,040
Mexico	0.17	0.11	0.30	0.09	0.08	0.80	27,853	2,330
Cambodia	0.16	0.11	0.84	0.08	0.05	0.90	50,186	174
Thailand	0.16	0.11	0.81	0.08	0.06	0.88	58,290	670
South Africa	0.16	0.11	0.30	0.06	0.06	0.53	138,127	224
Argentina	0.15	0.10	0.53	0.08	0.06	0.83	75,022	312
Philippines	0.13	0.10	0.30	0.08	0.06	0.82	40,423	1,274
Chile	0.12	0.08	0.51	0.05	0.04	0.79	57,826	192
Bolivia	0.12	0.06	0.58	0.05	0.03	0.86	70,323	80
Spain	0.11	0.08	0.51	0.09	0.06	0.61	105,902	286
Malaysia	0.11	0.07	0.38	0.06	0.04	0.84	91,509	133
USA	0.09	0.07	0.67	0.10	0.07	0.77	202,635	722

Notes: SD and IQR stand for Standard Deviation and Interquartile Range. The table shows statistics from a cross-section of countries in IPUMS International with data available at a small geographic level. For all countries I use census sample from 2010 or the closest available year. Rows are ordered from highest to lowest dispersion in women's labor supply. I aggregate data at the smallest geographical unit available, except for the USA where I use Commuting Zones as in Autor and Dorn (2013). Column (7) shows the total population for the average geographic unit in each country. These are unweighted cross-locality means which –might– differ from the national-level means. See table A.2 and section A.3 in the appendix for additional details on the cross-country data.

This table highlights three insights on women's employment. First, columns 1 to 3 show that, despite the significant differences at the mean, all countries exhibit large differences in women's employment rates *within* their borders.[7] For most countries, the gap between the localities at the 75th and 25th percentiles shown in column (1) is above 15 percentage points (p.p.). A gap of 15 p.p. is fairly large even for high female employment countries such as Vietnam, Cambodia, and Thailand. Even the smaller IQR of 9 p.p. in the United States is notable, as it is equal to the change in the national US female employment rate during the last *thirty-eight years* (1984-2022).[8]

Second, the dispersion of female employment rates is a widespread phenomenon across countries at different levels of development and geographic regions of the world. Table 1.4 includes countries from various regions, including Asia, America, Africa, and Europe. It also includes middle income countries like Indonesia and Mexico, and high income countries like USA and Spain. These findings suggest that the factors driving the dispersion in female employment rates are not limited to specific regions or income levels.

Third, columns 3 to 6 reveal that the large within-country dispersion in employment is primarily concentrated among women. With the exception of Brazil, the United States, and Spain, the dispersion in women's employment rates is substantially larger than that of men's in all countries. In fact, in ten out of the seventeen countries, the dispersion in women's employment *more than doubles* that of men's. Therefore, while men work at high rates across all regions within these countries, women's rates vary significantly depending on the locality they live in.

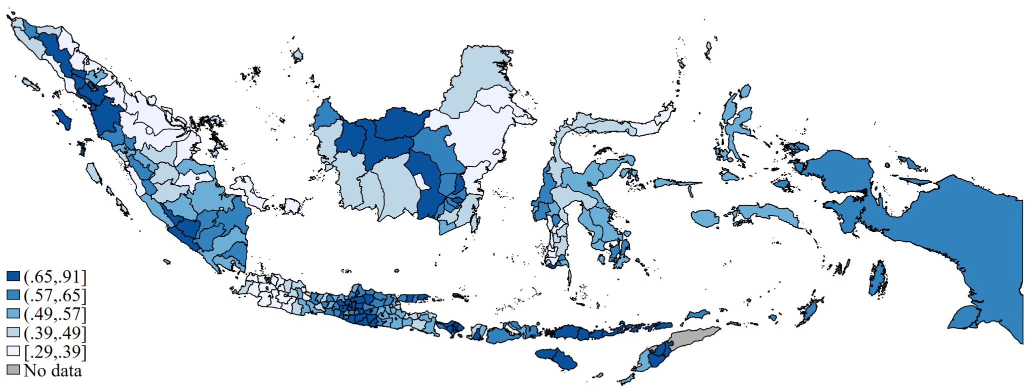
[7]Table A.3 shows that the large within-country dispersion in women's employment is not the result of regional variation in the rates of unpaid employment. For the specific case of Indonesia, 55% (IQR 12 p.p.) of the total dispersion still remains when I focus on paid employment only. This –reduced– IQR of 12 p.p. is still more than twice that of men's.

[8]This benchmark is not affected by the Covid-19 drop in women's employment. By 2022, women's employment had recovered to pre-Covid levels.

1.3.2 Fact 2: there is large within-country dispersion in women's employment rates in Indonesia

Figure 1·2 provides a detailed view of the variation in female employment rates within Indonesia. The map shows women's employment rates in all 268 regencies in my dataset, grouped by color into quintiles. Darker blues indicate higher employment rates. The map reveals that women work at vastly different rates across the country. For instance, the top quintile of regencies has employment rates above 65%. In contrast, the bottom quintile of regencies has rates below 29%, and this group includes significant population centers such as the Bogor regency and the city of Medan.[9] Notably, the map reveals that the dispersion in women's employment extends across the whole country and is not driven by any particular province, island, or group of regencies.

Figure 1·2: Indonesia: women's employment rate by regency, 2010



Note: The figure shows regency-level employment rates for women aged 18-64. It shows all the 268 regencies with consistent boundaries between 1970 and 2010. Each color groups a fifth of the regencies. The figure uses data from the 2010 Indonesian census from IPUMS international.

[9]Medan, the capital and largest city in the province of North Sumatra, is the third most populous city in Indonesia as of 2020 (Brinkhoff, 2022) Bogor, with over five million people, borders the Jakarta metropolitan area. Refer to their locations in figure A·1 in the appendix.

1.3.3 Fact 3: women's employment rates are highly persistent

The large dispersion in women's employment rates could be the result of (i) temporary economic shocks that depress women's employment in some parts of Indonesia, (ii) measurement error in the employment rates, or (iii) structural differences across regencies correlated with female employment. To understand the primary cause of the variation in employment rates, we can examine the persistence of these rates across years. If the dispersion arises mainly due to temporary shocks or measurement errors, we should expect very low persistence in the regencies' employment rates across years. This is because temporary shocks should dissipate after several years, and measurement error should be independent across time. In contrast, high cross-year persistence indicates that the variation in women's employment reflects structural differences across regencies.

Table 1.5: Indonesia: autocorrelation in regency-level women's employment rate, 1980-2010

Regressor	(1)	(2)	(3)	(4)
Female employment 10 years ago	0.80			
	(0.02)			
Female employment 20 years ago		0.72		
		(0.03)		
Female employment 30 years ago			0.70	
			(0.04)	
Same-year male employment				0.51
				(0.04)
Observations	800	534	268	1,071

Notes: The table shows the autocorrelation of regency-level employment rates across different time horizons. It also shows the simultaneous correlation between the employment of both genders. Data from 1980-2010 Indonesian Census taken from IPUMS international. Robust standard errors are in parenthesis.

In columns (1) to (3) of table 1.5, I show estimates of the autocorrelation of the regency-level employment rates across different time horizons. For this table, I standardize the regency employment rates separately by year and run regressions of the form:

$$e_{rt} = \gamma_{t-j} e_{rt-j} + \varepsilon_{rt} \tag{1.1}$$

where e_{rt} is the standardized employment rate in regency r at time t.

The estimates of autocorrelation suggest that the variation in women's employment rates is primarily driven by structural differences across regencies, and not by temporary shocks or measurement error. The autocorrelations are considerably high, starting at 80% for the ten-year horizon and staying as high as 70% for the thirty-year horizon. As a benchmark, I report the estimate of the simultaneous correlation with men's employment rates in column (4). Notably, women's employment rates are more correlated with themselves 30 years apart than with men's employment rates in the same year.[10]

1.3.4 Fact 4: dispersion in women's employment rates cannot be accounted by differences in women's characteristics alone

The highly persistent variation in female employment is likely driven by structural differences across regencies. These could be, for example, differences in the family structure or the industry mix across these labor markets. Motherhood is associated with lower female attachment to the labor market (Angelov et al., 2016; Kleven et al., 2019). Moreover, differences in the industry mix account for up to 80% of the variation in women's labor supply in developed countries (Olivetti and Petrongolo, 2016). Therefore, it is possible that the observed dispersion in female employment rates reflects underlying differences in family structure and industry mix across regencies.

In table 1.6, I test whether permanent differences in the industry mix or women's

[10] The large persistence of female employment rates is not exclusive to Indonesia. Figure A·3 shows that large 10-year auto-correlations also arise in other countries. For most countries, this auto-correlation is over 67%.

demographics can account for most of the dispersion in female employment across regencies. This table shows the R^2 from regressions of employment rates on a series of regency-level controls. They include the share of people married, the share with small children, along with measures of the age structure, the education level by gender, and the industry mix. I run the regressions separately by gender and stack data from all the 1980-2010 censuses. Additionally, I include year fixed-effects to absorb national trends in employment. If these factors accounted for most of the variation in female employment, we should expect very high R^2 values for these regressions.

Table 1.6: Indonesia: share of employment rate dispersion accounted for observed regency characteristics, 1980-2010

	Women					**Men**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
R^2	0.13	0.26	0.30	0.31	0.47	0.01	0.41	0.60	0.69	0.79
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Age structure		✓	✓	✓	✓		✓	✓	✓	✓
Women's education			✓	✓	✓					
Men's education								✓	✓	✓
Share married				✓	✓				✓	✓
With child under 5				✓	✓				✓	✓
Industry shares					✓					✓
N	804	804	804	804	804	804	804	804	804	804

Notes: The table reports the R^2 of a regression of regency employment rates on regency-level aggregates. Age structure controls are the shares of people aged 30-49 and 50-64. Education measures are the shares of people who attended at most middle school, high school, and college. When indicated, the regressions include 1-digit industry shares. Data from IPUMS International.

Table 1.6 reveals that differences in women's demographics or the industry mix account for only a moderate share of the dispersion in female employment across regencies. In column (4), controlling for women's education level and the regency's family and age structure accounts for only a third of the dispersion in employment rates. Adding a complete set of industry shares takes the R^2 to 47%. Although these factors account for a portion of the employment rate dispersion, collectively, they still leave 53% unaccounted for. In contrast, column (10) shows these same variables can account for 80% of the variation in men's employment rates. Therefore, the dispersion in female employment rates reflects variation in *other* factors that are *specific* to women. Therefore, the variation in female employment is likely driven by structural differences across regencies that are not captured by the variables included in these regressions. These could be differences in the social norms, cultural values, or institutional arrangements that shape gender roles and expectations in different contexts.

1.4 Empirical strategy and results

I start this section by showing that, conditional on the current place of residence, birthplace is highly predictive of women's labor supply in adulthood even for those that migrated before they turned 18. This persistence can reflect the causal effect of birthplace or a spurious correlation driven by women's unobserved characteristics. I then illustrate how can I use data of age at migration to separate these two sources of variation, and show evidence that the longer female early migrants stay in their birthplace, the stronger the predictive power of birthplace is. I interpret this as evidence that longer stay in birthplace has a causal effect on women's labor supply decisions.

1.4.1 Birthplace is highly predictive of women's labor supply

I start by comparing the labor supply of women who *live in the same location* but were born in different regencies. I do this by regressing a dummy equal to one if the person is employed at year t (e_{it}) on year by current-regency fixed-effects ($\delta_{c(i)t}$), women's employment rate in her regency of birth ($p_{b(i)}$), and a set of individual and regency-level controls X_{it}. These controls might include age, religion, education, number of books at home when growing up.

$$e_{it} = \delta_{c(i)t} + \boldsymbol{b} p_{b(i)} + X_{it}\kappa + \varepsilon_{it} \tag{1.2}$$

I compute the employment rate p_b using the sample of all women aged 18 to 64 living in regency b in the census of 2010. I obtain similar results when using data from previous census years.[11]

The parameter of interest in this regression is denoted by $\boldsymbol{b}$, which measures the relationship between women's labor supply and the prevailing female employment rate in their birthplace. I will refer to $\boldsymbol{b}$ as the birthplace persistence coefficient. Because the model includes regency of residency by year fixed-effects, $\boldsymbol{b}$ is primarily identified out of differences in labor supply of women who live in the same regency, in the same year, but who were born in different localities. This approach controls for permanent differences in the localities of residency, such as variations in average wages, industry mix, healthcare availability, and other factors, which are absorbed by the parameter $\delta_{c(i)t}$.

I refer to the slope of the birthplace employment rate as $\boldsymbol{b}$ to emphasize that it generally differs from the causal effect discussed in Section A.4.1. A positive value of $\boldsymbol{b}$ may not necessarily indicate a causal relationship between birthplace employment rates and women's labor force participation. Instead, it could capture differences in factors that are unrelated to birthplace characteristics, such as unobserved individual traits or preferences that make women from high-employment locations more likely to work than their counterparts from low-employment areas. For example, parents from women with high-employment areas could have invested more in their daughter's career.

[11] This is because employment rates are highly persistent.

Table 1.7: Indonesia: estimates of women's birthplace persistence on labor supply (***b***)

	(1)	(2)	(3)	(4)
Women's employment rate at birthplace (p_b)	0.30***	0.30***	0.29***	0.30***
	(0.03)	(0.03)	(0.03)	(0.03)
Mean employment rate	0.41	0.41	0.41	0.41
Implied IQR gap	0.07	0.07	0.07	0.07
Regency-year FE	✓	✓	✓	✓
Age		✓	✓	✓
Religion			✓	✓
Education				✓
Observations	62,954	62,954	62,954	62,954
R^2	0.07	0.08	0.08	0.09

Notes: This table uses data from the Intercensal Survey and restricts the sample to women who reside outside their birthplace. The implied IQR gap shows the implied employment gap between someone born at a regency at the 75th percentile of employment rate and someone born at the 25th percentile. The IQR of female employment rates across regencies is 22 percentage points. Standard errors are clustered by regency of origin. When applicable, regressions control for a quadratic polynomial in age and fixed effects for five religious and four education categories.

Table 1.7 shows estimates of the birthplace persistence coefficient ***b***. Column (1) shows results from a baseline specification that includes regency by year fixed effects only. The coefficient of 0.30 indicates that birthplace is highly predictive of women's employment. To see how large this coefficient is, let us consider two women: Putri and Amanda. Putri was born in the city of Probolinggo in East Java, which has a female employment rate of 40%. In contrast, Amanda was born in the regency of Sukoharto in Central Java, with a female employment rate of 62%. These rates places these regencies at approximately the 25th and the 75th percentiles of the distribution of female employment rates. The 0.30 coefficient implies that Putri is 7 percentage points less likely to work than Amanda. This is a difference of 17% relative to the employment rate of the average woman in my data.

The additional estimates in table 1.7 also allow me to rule out several potential drivers of the birthplace persistence. Columns (2) and (3) show that controlling for women's age and religion barely modifies the estimate. Thus, this persistence is not explained by geographic differences in age or religion. Column (4) adds education level as a control. Recent research suggests that exposure to low-employment places can affect women's labor supply through the expectations and education channel (Molina and Usui, 2022). In areas with low female employment rates, women set low labor market expectations and thus invest less in education. However, however column (4) indicates that the birthplace persistence is not driven by differences in educational investment.

Table 1.8: Indonesia: estimates of men's birthplace persistence on labor supply ($\boldsymbol{b}$)

	(1)	(2)	(3)	(4)
Women's employment rate at birthplace (p_b)	0.08***	0.08***	0.10***	0.09***
	(0.03)	(0.03)	(0.02)	(0.02)
Mean employment rate	0.87	0.87	0.87	0.87
Implied IQR gap	0.02	0.02	0.02	0.02
Regency-year FE	✓	✓	✓	✓
Age		✓	✓	✓
Religion			✓	✓
Education				✓
Observations	65,105	65,105	65,105	65,105
R^2	0.06	0.21	0.21	0.22

Notes: This table uses data from the Intercensal Survey and restricts the sample to men who reside outside their birthplace. The implied IQR gap shows the implied employment gap between someone born at a regency at the 75th percentile of employment rate and someone born at the 25th percentile. The IQR of female employment rates across regencies is 22 percentage points. Standard errors are clustered by regency of origin. When applicable, regressions control for a quadratic polynomial in age and fixed effects for five religious and four education categories.

The strong birthplace persistence in labor supply is essentially exclusive to women. I show this in table 1.8, where I show estimates from regressions where I relate men's employment in adulthood to their birthplace's *female employment rate*. Note that all these estimates are below 0.10 (about 30% the estimates in women) and imply little variation in men's employment rates across regencies. For example, the estimate in column (8) implies an IQR gap of only 2 p.p.

The persistence in women's employment rates could still be driven by variation across regencies in, for example socioeconomic or demographic factors. Unfortunately, the Intercensal Survey has limited demographic and socioeconomic information. Therefore, in Table 1.9 and Table A.6 in the appendix I take advantage of the rich data available in the IFLS to rule out additional potential drivers of the birthplace persistence.

Table 1.9: Indonesia: estimates birthplace persistence on women's labor supply (***b***)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Women's employment rate at birthplace (p_o)	0.38***	0.39***	0.35***	0.37***	0.34***	0.34***	0.29***	0.24***
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.08)	(0.08)
Mean employment rate	0.54	0.54	0.54	0.54	0.54	0.54	0.51	0.51
Implied IQR gap	0.08	0.09	0.08	0.08	0.08	0.08	0.06	0.05
Sample	Full	Full	Full	Full	Full	Full	Known mother	Known mother
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Regency FE	✓	✓	✓	✓	✓	✓	✓	✓
Age		✓	✓	✓	✓	✓	✓	✓
Religion			✓	✓	✓	✓	✓	✓
Education				✓	✓	✓	✓	✓
Childhood SES					✓	✓		
Siblings						✓		
Mother worked								✓
Observations	64,501	64,501	64,501	64,501	64,501	64,501	18,135	18,135
N individuals	6,115	6,115	6,115	6,115	6,115	6,115	2,640	2,640
R^2	0.10	0.12	0.13	0.14	0.14	0.14	0.14	0.14

Notes: Uses data from IFLS. Sample restricted to people residing outside their birthplace. Implied IQR gap shows the implied employment gap between someone born at a regency at the 75th percentile of employment rate and someone born at the 25th percentile. The IQR of the female employment rate across regencies is of 22 percentage points. Standard errors clustered by regency of origin. When indicated, the regressions control for a quadratic polynomial in age, and fixed-effects for seven religion and for education categories. Standard errors clustered by regency of origin.

First, in columns (1) to (4) of table 1.9 I reproduce the birthplace persistence estimates for the women migrants in the IFLS using the same specifications as in table 1.7. Reassuringly, these results confirm the Intercensal survey estimates, with a similar implied IQR of 8 p.p. Moreover, table A.6 shows similarly small persistence estimates for men.

Moreover, Columns (5) to (8) of table 1.9 rule out childhood socioeconomic status and maternal labor supply as drivers of my results. In columns (5) and (6), I study the role of childhood economic conditions. These variables come from a battery of questions where respondents reported information on their household when they were 12 years old. These include wealth and education proxies such as the number of books, the number of people per room, and whether their father was in formal employment, among others. Remarkably, adding these additional controls has little effect on the childhood persistence estimate. In addition, in columns (7) and (8), I rule out the possibility that the birthplace persistence is driven by differences in maternal labor supply. Previous literature shows that women with working mothers are more likely to work (Fernandez and Fogli, 2009). Therefore, the birthplace persistence might just be reflecting the fact that in places where more women work, there are higher shares of working mothers. Because of the panel nature of the IFLS, I can identify the maternal labor supply for a subset of women in my sample. Column (7) re-estimates the birthplace persistence for this sample. Column (8) shows the persistence estimate when I control for maternal labor supply. Although the point estimate is slightly smaller and noisier, I can rule out that maternal labor supply drives my results.

1.4.2 There is large persistence for those who migrated young

The birthplace persistence could be reflecting complex endogenous relationships between women's origin, their migration decision and their labor supply. Migration is a voluntary decision where the potential job opportunities at the destination are likely influence where women move to. In table 1.10, I focus my analysis on women who left their birthplace before they turned 19. Thirty eight percent of female migrants left their birthplace before this age. For these women, the migration decision is more plausibly driven by their parents' decisions. Reassuringly, I obtain similar persistence estimates for these sample.[12] Moreover, these estimates are robust to the choice of the migration age cutoff (see Figure A·5 in the appendix).

Table 1.10: Indonesia: estimates of birthplace persistence on labor supply ($\boldsymbol{b}$) for women who emigrated young

	(1)	(2)	(3)	(4)
Women's employment rate at birthplace (p_b)	0.24***	0.25***	0.24***	0.25***
	(0.03)	(0.03)	(0.03)	(0.03)
Mean employment rate	0.41	0.41	0.41	0.41
Implied IQR gap	0.05	0.05	0.05	0.06
Regency-year FE	✓	✓	✓	✓
Age		✓	✓	✓
Religion			✓	✓
Education				✓
Observations	24,178	24,178	24,178	24,178
R^2	0.08	0.08	0.08	0.09

Notes: This table uses data from the Intercensal Survey and restricts the sample to women who reside outside their birthplace and who left before they turned 19. The implied IQR gap shows the implied employment gap between someone born at a regency at the 75th percentile of employment rate and someone born at the 25th percentile. The IQR of female employment rates across regencies is 22 percentage points. Standard errors are clustered by regency of origin. When applicable, regressions control for a quadratic polynomial in age and fixed effects for five religious and four education categories.

1.4.3 The birthplace persistence is stronger the longer you stay

The strong birthplace persistence in women's employment could still reflect unobservable differences between women from different origins. Here, I address this concern by exploiting differences in the timing of migration to argue that this persistence reflects the causal effect of women's birthplace. To do so, I augment expression (1.2) by (i) allowing the coefficient on female employment rate to vary by the emigration age ($\boldsymbol{b_a}$), and (ii) adding age of emigration fixed-effects (λ_a).

[12] Table A.7 shows that the men sample shows birthplace persistence estimates similar to those of women. However, as we will see in the next section, they are mostly driven by unobserved differences between men of different origins.

As I discuss in section A.4 in the appendix, I can decompose the OLS estimates of age specific-slopes into a cumulative causal effect up age a $\boldsymbol{\sigma}_a$, and a selection term $\boldsymbol{\gamma}$:

$$\boldsymbol{b}_a = \boldsymbol{\sigma}_a + \boldsymbol{\gamma}$$

the selection term γ reflects omitted variable bias. It captures the fact that women from the same origin are likely to share characteristics that make them more (or less) likely to work, but which are not driven by a causal effect of place. For example, parents in areas with high female employment might by richer and more likely to invest in their daughters education. Under the key assumption that this omitted variable bias is constant across emigration ages, I can identify the causal effect of place at any given age (π_a) by subtracting the persistence coefficients across emigration ages:

$$\boldsymbol{\pi}_a = \boldsymbol{b}_{a+1} - \boldsymbol{b}_a$$

Moreover, the coefficient for least exposed cohort gives as estimate of the omitted variable bias: $\boldsymbol{\gamma} = \boldsymbol{b}_0$

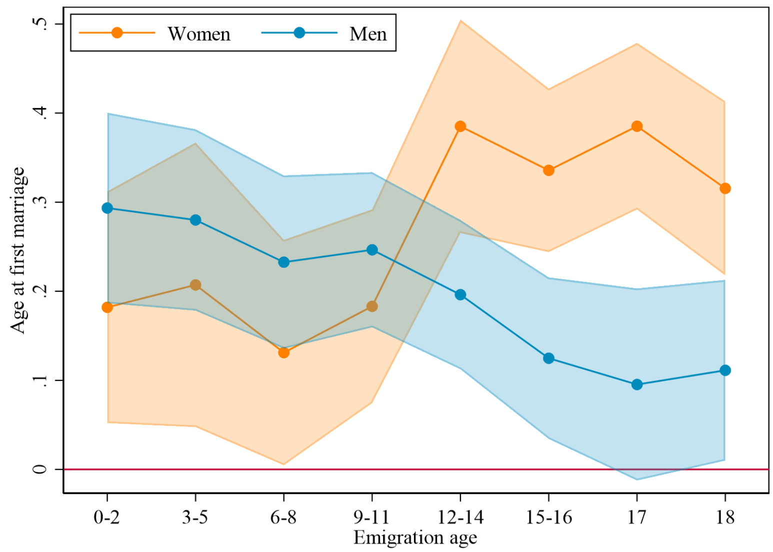
To estimate this model, I leverage age of emigration data from the Intercensal survey. Because the number of migrants at any given age is relatively small relative to the number of regencies, I bin emigration age into three-year cells.

Longer stay does make you more likely to work

Figure 1·3 displays estimates of birthplace persistence ($\boldsymbol{b_a}$) by age of emigration for both men and women. My sample remains restricted to people who left their birthplace before they turned 19. The regressions control for a quadratic polynomial in age, as well as current regency-by-year, education, and religion fixed effects.

These results show a striking pattern in the birthplace coefficients: women with

Figure 1·3: Indonesia: length of stay and marriage and fertility

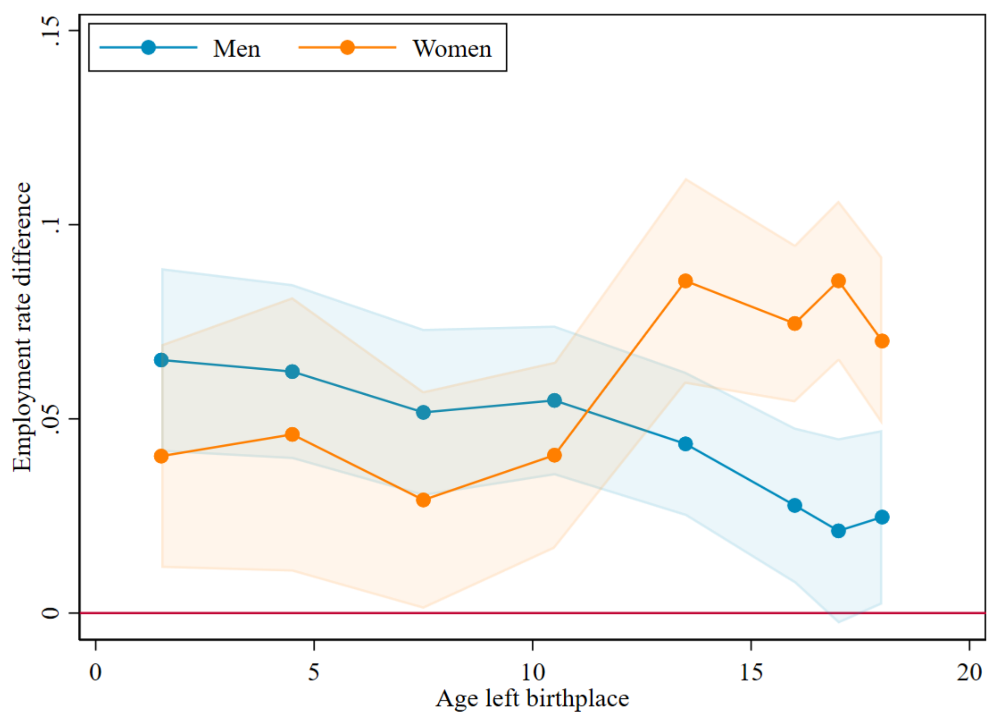


Note: The figure shows estimates of the birthplace persistence coefficients by age of emigration $\boldsymbol{b_a}$. It uses data data from 1995 and 2005 Intercensal surveys. Panel (b) uses information from the 1995 survey only, as fertility data is not available for 2005. The regression controls for current regency by year fixed-effects, a quadratic polynomial on age, and education level fixed-effects. The figure shows 90% confidence intervals.

longer exposure to high-employment locations are more likely to work. The birthplace persistence coefficients increase from 0.18 for women who left their birthplace between 0 to 2 years old, to 0.38 for those who left between 12 to 14 years old, and remain roughly constant thereafter. These patterns provides several insights. First, women from high-employment locations are likely to work from the outset. Women who left their birthplace before they turned three have very little exposure to their birthplace, and yet they are more likely to work than those coming from low-employment locations. Following the discussion in Section 1.4.1, I interpret this coefficient as reflecting unobservable differences between women from different origins (omitted variable bias). Second, longer exposure leads to higher female employment. Under the constant omitted variable bias assumption, we can attribute the increase of approximately 20 p.p. in the birthplace persistence coefficients to the effect of longer exposure to high-employment locations. Third, the birthplace effect is concentrated in late childhood and early teens, as the increase in the persistence happens between the ages of 6 to 14 years old. Staying after the age of 15 has no additional effect.

Figure 1·3 also shows persistence estimates for men. Like women, men from high-female-employment locations have traits that makes them more likely to work. The estimate for least-exposed men is of 29 p.p. However, the very gradual decline in the estimates suggest that longer exposure to these locations make men less likely to work. The coefficients decline by 18 percentage points, with a decline of roughly 1 p.p. per additional year of stay.

Figure 1·4: Indonesia: implied IQR gap in employment rate by age of emigration



Note: The figure shows estimates of $\boldsymbol{b_a}\times$ regency-level IQR in female employment (22 p.p.). This is the implied gap in employment at different ages of out-migration between a person born in a regency at the 75th percentile in female employment, and another one born in a regency at the 25th percentile of female employment. Point estimates are placed at the mean point of the respective age interval. Shaded areas show 90% confidence intervals. The figure uses data from the 1985, 1995 and 2005 Interncensal Surveys.

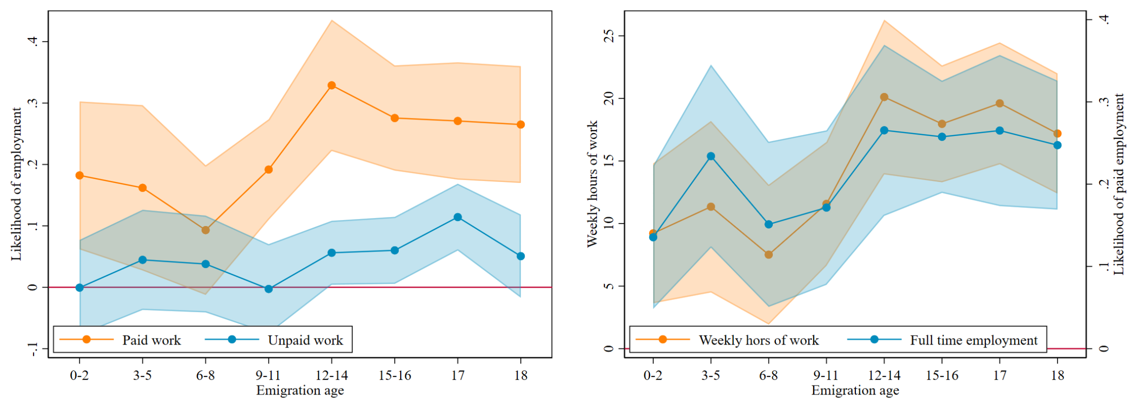
These results suggest that place effects play a crucial role in driving geographic differences in women's labor supply. This is illustrated in Figure 1·4, which displays the counterfactual gaps in employment between two women, one born in a regency at 75th percentile of the employment distribution and another born at the 25th percentiles of employment, if they had left their birthplace at different ages. I call this gap the IQR gap in employment. The figure places the gap estimates at the midpoint of each of the age brackets in figure 1·3. If both of these women had emigrated in their first year of life, I would observe a gap of 4 p.p. in their labor supply when they are adults. This initial gap is driven by unobservable differences between these two women. In contrast, if they stayed in their birthplace up to 13 years old, this gap would widen up to 9 p.p. The increase of 5 p.p. in the likelihood of employment is equivalent to 27% of the existing gap in FLFP between these regencies and is driven by the longer exposure to their birthplace. Therefore, a significant portion of the current inequality in female labor force participation is transmitted to the next generation of women growing up in these locations through birthplace effects.

Longer stay translates into more paid employment and more hours

In figure 1·5 I show that longer exposure to high-employment labor markets also translates into higher paid employment and higher working hours. Panel (a) breaks down the employment into paid and unpaid work. Unpaid work accounts for about a 35% of all female employment. The increase in employment from Figures 1·3 and 1·3 is unlikely to represent more economic independence for women if it were entirely driven by unpaid work. However, panel (a) shows that increase in the birthplace persistence between 6 to 14 years old is driven by *paid employment*. The rise in the coefficients between 0 to 14 years old translates into an increase of 3.2 p.p. IQR gap in employment.This is 64% of the effect on all employment from Figure 1·3. This contrasts with results on unpaid work. There is little effect on the likelihood of unpaid employment up to 14 years old. Although there is an uptick in the coefficients at 17, the effect is small. In all, staying up to 17 at birthplace renders an IQR gap of 1.2 p.p.

Figure 1·5: Indonesia: employment type by length of stay

(a) Type of employment **(b)** Hours and full time employment



Note: The figure show the coefficients on the interactions between age of emigration and birthplace female labor force participation. The regressions also control for (i) age of emigration dummies, (ii) birthplace female employment rate (iii) interactions between the emigration age and birthplace female employment rate. Panel (a) uses data from the 1985, 1995 and 2005 Intercensal Surveys. Panel (b) uses data from the 1985 and 1995 surveys because hours of work data is not available in 2005. Full time employment defined as working 40 hours or more in week. The figure shows 90% confidence intervals.

Panel (b) of Figure 1·5 I shows additional results on the likelihood of full-time employment and weekly hours of work. Data on weekly hours of work is not available in the 2005 Intercensal Survey, thus these results use data from the 1985 and 2005 surveys only. However, the plot shows a consistent picture: staying in high female employment places between 6 to 14 years old raises women's labor supply. The birthplace employment coefficients rise sharply at these ages and both increases are sizable. They translate into IQR gap increases of 2.5 weekly hours, and 2.86 p.p. in the likelihood of full-time employment.

So far all the evidence presents a consistent picture: longer stay in high-female employment labor markets translates into higher attachment to the labor market in adulthood. Women with more exposure to these labor markets are more likely to be paid workers, and the work longer hours. A natural question is whether they also have higher earnings. I answer this question in Figure A·6 where I show birthplace persistence coefficients in regressions with total earnings and hourly wages as dependent variables. These regressions restrict the sample to the much smaller group of migrant women with non-zero earnings. Because this is a much smaller sample, I am forced to use wider bins for the emigration age. These results are noisy, but they suggest that longer exposure to high female employment locations could lead to higher wages for women.

The data supports the constant selection assumption

The causal interpretation of the birthplace persistence coefficients hinges on the assumption that selection is independent of emigration age. More precisely, conditioning on the current location and other controls, I require that the relationship between women's unobserved characteristics and the birthplace female employment rate be the same for women who emigrated at different ages. Below, I provide results showing that selection along several observable dimensions is constant across emigration age. This suggests that the identification assumption is likely to hold in my data.

One can think of the identification assumption as an analog of the parallel trends in Difference in Differences. I expect women coming from high and low-employment regencies to be different from each other. This is not an issue. However, if there are factors correlated with female employment that change differently across emigration ages for these two groups of women, I would assign this variation to the causal effect. Thus, the lack of parallel trends could lead me to find a causal effect where there is none.

I cannot test whether the correlation between female labor force participation at birthplace and women's unobservable characteristics is constant across emigration age. However, I can test whether the correlation between the employment rate and a series of individual characteristics I do observe is the same no matter the age women migrated. To do this, I estimate the a regression where I regress a woman characteristic y_i on age of emigration fixed effects λ_a, female LFP at birthplace p_b, and interactions between age of migration and female LFP:

$$y_i = \lambda_a + \beta p_b + \sum_a \beta_a 1_a \times p_b + X_i \kappa + \varepsilon_{it} \tag{1.3}$$

in model 1.3, I chose 18 as the base category and thus the β_a can be interpreted as the difference between the slope at age a and the slope at age 18. Under constant selection across all the ages *all the interaction terms* β_a *should be jointly zero.*

In figure 1·6 I show estimates of the interaction terms β_a for different outcome variable. Panel (a) uses data from the Intercensal Survey while panels (b) to (d) take advantage of the richer demographic information available in the Intercensal Survey. All regressions control for a full set of education and religion dummies, and a quadratic polynomial in age. In addition, panels (a) to (c) include current-regency fixed effects.

In panel (a), I present estimates of the interactions between migration age and female employment at birthplace in regressions where I use migration motive dummies as dependent variables. Each series of coefficients in the panel represents a different regression. In Section 1.2.3, I showed that the reason for migration changes as women age, with older becoming more likely to migrate for work.[13] These age-related changes in the reason for migrating could pose a problem for my identification strategy if

[13] A limitation of the reason for migrating data in the Intercensal Survey is that it is unclear how respondents classify themselves among the possible options. For example, conceivably I can migrate because of *my* work, or my partner's *work*. However, migrating because of my partner's work could be classified as work-related migration, but it could also be interpreted as family-related migration. I note, however, that the IFLS provides a much more detailed –and less ambiguous– classification for migration motives and I obtain results similar to those in panel (a).

they if they differ between regencies with high and low female employment.[14] To alleviate this concern, in panel (a) I show that most of the interactions in the work-migration series are insignificant at the 5% level and, in fact, all the interactions from 3 to 17 years old are jointly insignificant. Furthermore, note the work series does not reproduce the sharp increase between 6 to 14 years from Figure 1·3.Panel (a) also displays analogous slopes for regressions with family and education migration dummies as outcomes. In both cases, I cannot reject that all these interactions are jointly zero. Therefore, there is no consistent evidence that changes in migration motives are the driving factor behind the birthplace persistence.

Panels (b) to (d) present similar exercises where I take advantage of the much richer demographic information available in the IFLS. These panels show a less detailed breakdown of migration ages than panel (a) because the IFLS (i) has a smaller sample size, and (ii) there is not age breakdown for migration episodes that happened before the people turned 12 years old. In panels (b) and (c) I show results from regressions that use parental wealth proxies and number of siblings as outcomes. There extensive evidence that parental investment is key determining children's outcomes (Baker and Milligan, 2016; Jayachandran and Kuziemko, 2011; Pande, 2003; Autor et al., 2019; Barcellos et al., 2014; DiPrete and Jennings, 2012). Moreover, family background and the number of siblings are important in determinants of these investments, specially in developing countries (Jayachandran and Kuziemko, 2011; Baker and Milligan, 2016; Pande, 2003). Therefore, in these two panels I test for evidence of changes in selection by parental wealth and number of siblings across migration age cohorts. All the interactions in these panels are jointly insignificant at the 5% level.

In panel (d), I present results from regressions where I use characteristics of the

[14] For example, changing migration motives could account for the patterns I observe if: (i) women migrating because of work are more likely to be employed at the destination, (ii) women from high-employment regencies become even more likely to migrate because of work than those from low-employment regencies

destination regency as dependent variable. There is evidence that skills acquired at the origin location are important at determining post-migration outcomes. Thus, in this panel I explore the possibility that the birthplace persistence is the result of changes of –admittedly complicated– selection patterns across migration age cohorts. The results in this panel are similar as in the other graphs in the figure, with all the interactions being jointly insignificant. Overall, the lack of clear patterns in Figure 1·6 as evidence in support of the constant omitted variable bias assumption.

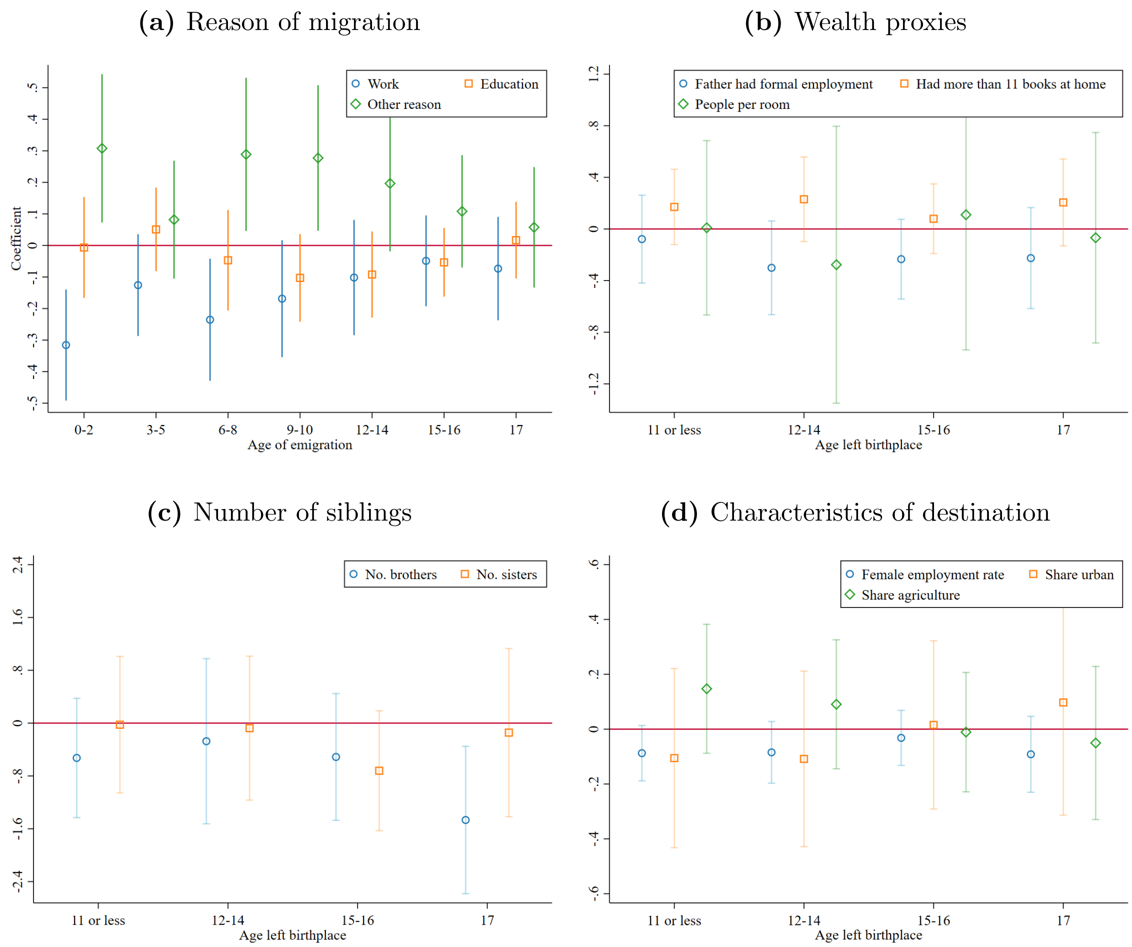
Discussion: why does birthplace matters so much?

Having established that exposure childhood exposure to birthplace has a strong effect on women's choices, the natural question is then through which mechanisms do birthplace influence women's choices. Here I examine the evidence of three mechanisms: (i) human capital accumulation, (ii) schooling quality, (iii) changes in parental investments, and finally (iv) culture and/or gender norms.

Exposure to birthplace could affect women's labor supply via their career expectations and their educational investment. Being exposed to an environment where women are actively participating in the labor force could alter their career expectations and make them more likely to invest in further education. For example, Molina and Usui (2022) that in Japanese municipalities with higher female participation rates, teenagers exhibit greater educational aspirations, leading to increased investment in schooling. If higher investment in schooling accounts for my results, I should observe higher schooling in women with higher exposure to high-female employment regencies. I test this in figure 1·7 where I show estimates of the birthplace persistence coefficients for regressions using measures of schooling as dependent variables. The figure shows results for both the years of schooling, and a dummy equal to one if the woman completed primary.[15] As in Figure 1·3, these coefficients should be read as

[15] Figure 1·3 shows the effects of birthplace are concentrated between the ages of 6 and 14. There-

Figure 1·6: Indonesia: women and selection by age of emigration



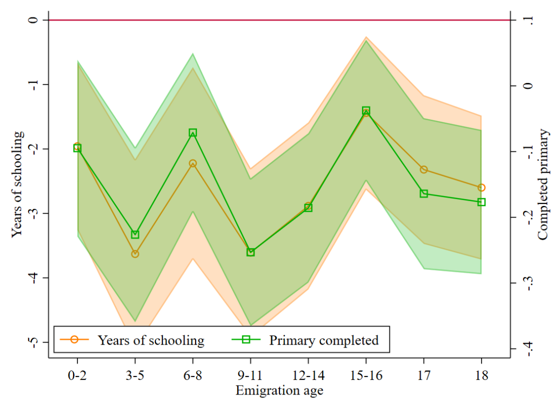
Note: The figure show the coefficients on the interactions between age of emigration and birthplace female labor force participation. The regressions also control for (i) age of emigration dummies, (ii) birthplace female employment rate (iii) interactions between the emigration age and birthplace female employment rate. I chose 18 years old as the base category so that the interactions test whether the slopes at each age are different from that for women emigrating at 18. Data on reasons for emigrating is available only for people emigrating at 12 years old or older. Error clustered by regency of birth. The figure shows 95% confidence intervals. Data from the IFLS.

the birthplace persistence accumulated up to each of the ages shown. Therefore, if staying longer in localities with higher female employment, these coefficients should be increasing up to age 18. However, the figure gives little support to education as the main channel through which the birthplace effects operate. First, the 0-2 and the 18

fore, completing primary school would be the main margin of action.

years old coefficients are very similar and thus there is no clear evidence that longer stay in high-female employment regencies leads to more overall education. Second, although there seems to be a jump in the coefficients at 15-16 years old, this not coincide with the 6-14 jump we saw in the employment results. Therefore, the timing of the jump is off.

Figure 1·7: Indonesia: education by length of stay



Note: The figure show the coefficients on the interactions between age of emigration and birthplace female labor force participation. Error clustered by regency of birth. The figure shows 90% confidence intervals. Data from the Intercensal Survey.

While women from areas with high female employment do not necessarily spend more time in school, the impact of birthplace can manifest through schooling if these locations offer education of higher quality. In this case, areas where women participate more actively in the labor market could have educational systems of higher quality that target women more effectively. Thus, even if women do not spend more time in schooling, women who spend more time in their birthplace would be exposed *longer* to an education system of higher quality. However, there are two pieces of evidence that point against this channel. First, note that all the coefficients in Figure 1·7 are *negative* and hover around -2.6. This means that women from high-employment regencies spend less time in school than their counterparts from low-employment regencies. The average coefficient of -2.6 means that women born in a regency at the 75th percentile of employment spend seven less months in school than those born at a 25th percentile regency. Second, high female employment regencies have worse overall female educational outcomes. Appendix table A.8 show that women in these regencies have worse education outcomes. In this table I split regencies at the mean of the female employment rate and compute average education outcomes for each group. On average, women in high-employment regencies stay one year less in school, and are much less likely to complete primary and secondary education. If the educational quality in high-female employment regencies were higher, one would expect that women stay in the system longer and they have better overall educational outcomes. However, the evidence does not support this possibility.

Molina and Usui (2022) suggest that exposure to local labor market opportunities influences parental investment in girls' education. However, it is unlikely that this factor is responsible for the results I obtain. My findings indicate that women who were exposed longer –as children– to regencies with high female employment are more likely to join the labor market as adults. If parental investments were the driving force behind these results, it would imply that parental investment is highly responsive to the length of their child's exposure. Given that the parents have resided in this location for a considerable period of time, it seems improbable that such a high level of sensitivity exists.

A more plausible driver of the birthplace effects is the transmission of cultural gender norms. There is a growing literature emphasizing that the transmission of culture or gender norms has permanent effects on women's labor market outcomes (Fernández et al., 2004; Alesina et al., 2013; Blau et al., 2011). Both the Intercensal Survey and the IFLS provide limited data to formally test this channel, but two pieces of evidence support it. First, I find evidence of similar birthplace effects on fertility and age of marriage, outcomes more directly linked to gender norms. In Figure 1·8, I present estimates of the birthplace coefficients for regressions using the age at first marriage and the number of children born as outcomes.[16] This figure reveals a pattern that closely aligns with the employment results, where longer childhood exposure is associated with delayed marriage and lower fertility rates. Moreover, these effects appear to be concentrated between the ages of 6 to 14 years old, although the results for the number of children are less clear due to the reversal in the coefficients after age 15. Second, the birthplace effects primarily occur during ages when gender norms are highly malleable. Late childhood and early adolescence represent a critical period when children are mature enough to form their own opinions while remaining receptive to external influences (Dhar et al., 2022). Remarkably, my findings indicate that the majority of the effects occur between the ages of 9 and 14, precisely the period when teenagers have demonstrated responsiveness to interventions targeting gender norms (Dhar et al., 2022).

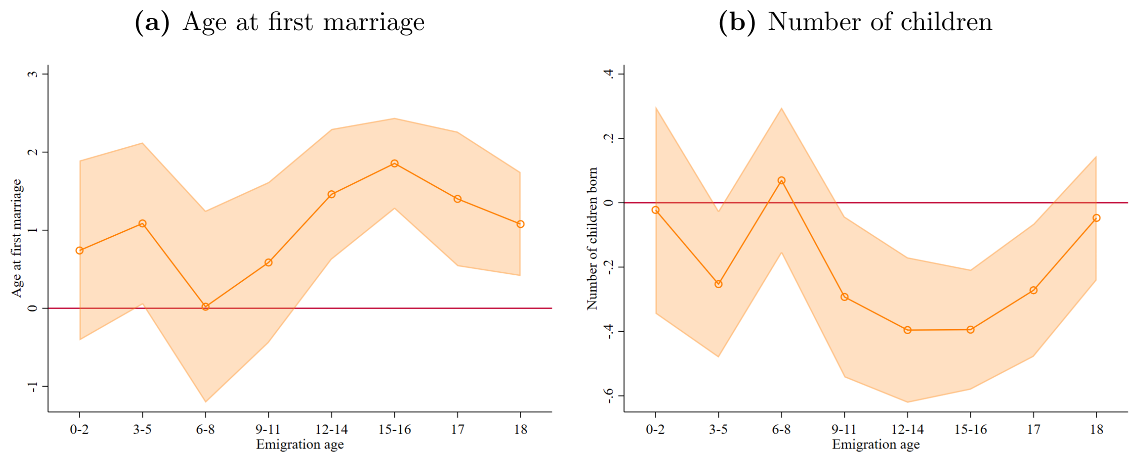
1.5 Conclusions

In this paper, I document large and persistent spatial inequality in women's labor supply in Indonesia, a country with more than 118 million women. I argue that a substantial portion of this inequality is driven by the local environment women are born into. To identify the causal effect of place, I leveraged variation coming from the age women emigrated from their birthplace. I compared the labor supply choices of women who currently live in the same location, but who emigrated from their birthplace at different ages as children. If the omitted variable bias is independent of the age of emigration, this strategy allows me to distinguish the causal effect of place from variation driven by differences in women's unobserved characteristics.

I show that women's birthplace is particularly important during the formative

[16]The Intercensal Survey only includes fertility and marriage questions for women, hence I cannot present estimates for men.

Figure 1·8: Indonesia: length of stay and marriage and fertility



Note: The figure shows estimates of the birthplace persistence coefficients by age of emigration $\boldsymbol{b_a}$. Panel (a) uses data data from 1995 and 2005 Intercensal surveys, while panel (b) uses data from the 1995 survey only. This is because marriage data is available only in 1995 and 2005, and fertility data is available for 1995 only. The regression controls for current regency by year fixed-effects, a quadratic polynomial on age, and education level fixed-effects. The figure shows 90% confidence intervals.

childhood and teen years. Staying in a location at the 75th percentile of female employment between 6 and 16 years of age makes women 5 percentage points more likely to work than those born in a location at the 25th percentile. These magnitudes mean that 23 percent of the current spatial inequality in women's employment transmits to the next generation of women. Therefore, these women-specific place effects can be an important driver of the large and persistent differences in women's labor force participation within countries.

Further research should delve into the mechanisms by which childhood exposure impacts women's choices. While my findings indicate that disparities in human capital accumulation do not account for the results, I can only suggest cultural transmission as the most likely mechanism. Future studies should concentrate on elucidating the the importance of transmission of culture and norms in driving these effects and identifying the specific ways in which this transmission occurs. Additionally, it would be intriguing to ascertain whether these results can be replicated in other countries.

Chapter 2

Gender and the Urban Wage Premium

2.1 Introduction

Do women benefit more than men from living in bigger cities? Previous research extensively documents that bigger cities pay higher wages, and recent evidence suggests that this urban wage premium is declining (Autor, 2015; Glaeser and Maré, 2001). However, there is surprisingly little evidence on whether the level and trend of this premium differ between genders. Most studies either focus on men only or do not disaggregate by gender. In this paper, I document significant gender differences in the level and trends of the premium between men and women in the United States over the last six decades. I show that women were less affected by the overall decline during this period, so that by 2016 women's premium was significantly larger than men's.

Using data from the US Decennial Censuses and the American Community Survey (ACS), I provide decadal estimates of the urban wage premium separately for men and women. I measure the premium as the elasticity of wages with respect to the locality's population (de la Roca and Puga, 2017; Baum-Snow et al., 2018). I identify localities with Commuting Zones (CZ) (Autor, 2015).

My estimates show that between 1970 and 2016 men's and women's premium evolved very differently. In 1970, women's premium was on par with men's, but by 2016, women's urban wage premium was 40% larger than men's. I call this phenomenon the rise of *women's urban advantage*.

Next, I show that the rise in women's urban advantage arises only among workers without a college degree. In line with Baum-Snow et al. (2018), I find that that the premium for non-college graduates increased up to 1990, and declined there after. However, I find women experienced faster increases and milder declines, which resulted in the broadening of the female's urban advantage among this group. Moreover, the rise in women's advantage is robust to controlling for a large set of individual and locality characteristics, which allow me to conclude that it is not driven by simple patterns of selection across Commuting Zones.

I then use data from the Dictionary of Occupational Titles to link the rise in women's advantage to changes in the urban wage gradient across tasks. Following the literature, I construct five indexes capturing the use of *non-routine cognitive*, *social*, *routine cognitive*, *non-routine manual*, and *routine manual* tasks (Autor et al., 2003; Deming, 2017). I estimate urban wage gradients for each of these tasks. These are the coefficients of interactions between each of the task indexes and CZ size in regressions having the log of hourly wages as dependent variable. Since 1970, there was a drastic decline the urban premia to non-routine manual tasks, while the premia for other tasks either remained constant or increased sharply. By virtue the occupational segregation between genders, men and women had different exposures to these changes.

This paper is most closely related to the vast literature studying the urban wage premium (Glaeser and Maré, 2001; Glaeser and Gottlieb, 2009; Duranton et al., 2013; de la Roca and Puga, 2017; Duranton and Puga, 2020; Baum-Snow et al., 2018; Autor, 2019). Evidence for women in this literature is rare, as most studies focused either on men or did not disaggregated results by gender. Notable exceptions are Bacolod (2017) and Phimister (2005). While they find evidence that women's premium is larger than men's in the US and the UK, my results highlight this is the result of the different time trends in the premia of each gender.

This paper is also related to the recent research documenting declines in the urban wage premium for non-college workers (Autor, 2019; Baum-Snow et al., 2018). The reasons behind this phenomenon are not well understood in the literature and my results highlight how it was concentrated on the jobs using certain tasks more intensively.

Finally, my paper is also related to the rich literature on the gender wage gap (Blau and Kahn, 2015; Gollin et al., 2021; Olivetti and Petrongolo, 2016). The bulk of this literature focuses on occupation, industry, and job flexibility as the main drivers of the gap (Goldin, 2014; Blau and Kahn, 2015). Until very recently geographical variation in the gender wage gap had been left mostly unexplored. Recent contributions highlight that local amenities, such as commuting time, play an important role in determining labor market outcomes across genders (Black et al., 2014; Le Barbanchon et al., 2021; Liu and Su, 2020). My findings contribute to this literature by emphasizing that there is rich variation in the gender wage gap within the US. They also point to a rich interaction between the gender wage gap and local labor market characteristics.

2.2 Data

2.2.1 Data sources

My main analysis uses data from the US Decennial Census for the years 1970, 1980, 1990, and 2000 from IPUMS, along with the five-year samples from the American Community Survey (ACS) for the years 2011 and 2018 (Ruggles et al., 2010). The 2011 ACS contains data for the years between 2007 and 2011, while the 2018 ACS contains data between 2014 and 2018. I label these ACS samples as 2010 and 2016 respectively.

My sample follows closely those commonly used in the gender-gap literature (Blau and Kahn, 2015). It consists of full-time year-round non-farm wage and salaried workers, aged between 18 and 64 years old, not attending school, and living outside of group-quarters.[1] I define full-time year-round workers as those working for at least thirty-five hours per week and at least forty weeks in the previous calendar year.

I identify local labor markets with Commuting Zones (CZs). Commuting Zones are collections of counties with strong commuting ties with other counties within the same zone, but with weak ties with counties outside the zone. I use the 1990 CZ delineation as in Autor (2019). Throughout the paper, I use the terms city and local labor market interchangeably. Moreover, I refer to specific CZs with the name of its largest city according to 1990 population counts. Cities' names and population counts come from the delineation files provided by the Economic Research Service from the US Department of Agriculture (USDA, 2019). I follow Autor and Dorn (2013) and restrict the sample to the 722 CZs in mainland USA.

I measure city size with the log of CZ's population (Baum-Snow et al., 2018; Duranton and Puga, 2020). Whenever I include industry and occupation into the analysis, I use the occupational classification crosswalk from the 1950 census provided by IPUMS. This allows me to do a simple and consistent comparison of occupational codes across several decades.

In my results, I also use data on task requirements across occupations to explore its role in accounting for rise in women's urban advantage. This data comes from the fourth edition of the Dictionary of Occupational Titles (DOT) and the extended version of the 1971 Current Population Survey (National Academy of Sciences, 1971). The DOT contains individual-level information on a series of task requirements at the job. They are recorded using discrete scales to indicate importance of each task.

I follow the literature and combine these measures into five different indexes that

[1]See section B.1.1 in the appendix for further details

capture different aspects of the job (Autor et al., 2003; Autor and Dorn, 2013; Deming, 2017; Cavounidis et al., 2021). To do so, I first normalize all task measures to range from 0 to 1. I emulate Deming (2017) and construct a *social* index by averaging all the social and interactive task measures available in the DOT. This index captures the importance of interacting with others at work. The remaining four indexes I take directly from the literature (Autor et al., 2003). The *non-routine cognitive* index captures analytical reasoning and managerial tasks, while the *routine cognitive* captures the repetitive cognitive tasks typical of clerical occupations. Finally the *routine manual* and *non-routine manual* capture two types of manual task content. Refer to Section B.1.2 in the appendix for further details in the construction of these indexes.

Appendix Tables B.3 and B.4 show the occupations with the highest and lowest values for each of the skill indexes. These examples generally align with expectations. For instance, the occupations with the highest social content include salesmen, professors, and HR workers. High non-routine cognitive jobs include professors, engineers, and scientists, while high routine manual jobs include clerical occupations, dentists and machinists.

2.2.2 Descriptive patterns

In Table 2.1 I provide a general summary of my data and I preview some of the patterns I will be discussing later. The table displays national-level statistics disaggregated by gender for two samples: all people in working age (panel A), and full-time year-round workers (panel B). In addition, panel C zooms in on the spatial patterns in gender inequality.

Panels A and B of table 2.1 reflect the clear progress of women in the US labor market, as highlighted in the gender literature (Blau and Kahn, 2015; Autor and Wasserman, 2013). The gender gap in labor force participation declined by 30 percentage points (71%) between 1970 and 2016. In addition to higher participation, more women worked higher hours. The share of women among full-time workers increased by 13 p.p. (42%) over the same period. At the same time, there were large declines gender wage and the gap in educational attainment. In fact, the share of women with college degrees caught up and surpassed that of men.

Table 2.1: Selected summary statistics, 1970-2016

	Census year					
	1970	**1980**	**1990**	**2000**	**2010**	**2016**
A. All people aged 18-64						
All people (000)	98,483	117,521	127,971	147,426	161,423	168,571
Share female	0.54	0.52	0.51	0.51	0.51	0.51
Labor force participation						
Men	0.92	0.90	0.89	0.85	0.86	0.85
Women	0.50	0.60	0.70	0.70	0.73	0.73
B. Full time year round-workers						
Study sample (000)	42,446	59,110	69,257	82,841	87,428	94,531
Share female	0.31	0.37	0.41	0.43	0.44	0.44
Share college graduates						
Men	0.06	0.10	0.09	0.10	0.11	0.12
Women	0.03	0.06	0.07	0.10	0.13	0.16
Average log hourly wage						
Men	3.21	3.19	3.17	3.17	3.16	3.15
Women	2.72	2.76	2.87	2.94	2.97	2.98
Gap (women − men)	-0.49	-0.43	-0.30	-0.23	-0.19	-0.17
C. Regional tends						
Men's average log(wage)						
Big CZ	3.22	3.20	3.18	3.18	3.17	3.16
Small CZ	2.99	3.02	2.95	2.97	2.96	2.98
Big CZ wage premium	0.23	0.18	0.23	0.21	0.21	0.18
Women's average log(wage)						
Big CZ	2.73	2.77	2.88	2.95	2.98	2.99
Small CZ	2.51	2.56	2.59	2.70	2.74	2.77
Big CZ wage premium	0.22	0.21	0.29	0.25	0.24	0.22
Big CZ premium gap (women − men)	-0.01	0.03	0.06	0.04	0.03	0.04

Notes: Sample restricted to full-time year-round workers in mainland US. Big CZ are those with population above the median. Small CZ have below median population. The table shows weighted means using the census sampling weight.

In Panel C I zoom in on the regional patterns obscured by the aggregate figures from panels B and C. This panel previews some of the patterns I highlight in the next sections. In the panel I split commuting zones into "big" (above median population) and "small" (below median population). You can think of the typical big CZ as a city with 360 thousand people. In contrast, the typical small CZ houses approximately 22 thousand people.

Panel C previews three interesting facts. First, as it is well documented, there is substantial urban wage premium (Baum-Snow and Pavan, 2012). Big CZs pay wages that are at least 18% higher. Second, this premium seems to have declined over this period. For men, note the that big-city premium declined by 5 percentage points (22%) between 1990 and 2016. This decline is in line with the evidence documented in recent literature (Autor, 2015). Third, there are important differences across genders the evolution of the premium, which resulted in women gaining an urban advantage over men. This is highlighted in the last row of the table, where I compute the difference in the big city-premium across genders. The was little difference in men's and women's premium in 1970, but by 2016 women's premium was 4 percentage points (22%) higher than men's.

In Table B.2 I show selected statistics at the CZ-level. Most CZs are relatively small. Only about 25% of them have populations over 150 thousand people. Moreover, there is a great deal of variation in the gender wage gap across CZs. By 2016, the interquartile range for the gender wage gap is 6 percentage points. Note that this is a large difference, of about one third the national-level gap from table 2.1. I leverage this variation in the next section when I compute the changes in women's urban advantage.

2.3 Empirical specification

In the next section I will present several facts on the evolution of the urban wage premium by gender, and the relationship between city size and the gender wage gap over time. As it is standard in the literature, I estimate the urban wage premium by Feasible Generalized Least Squares (FGLS) (de la Roca and Puga, 2017; Ananat et al., 2018). In a first stage, I use individual-level data to regress the log of hourly wages (w_{it}) on Commuting Zone fixed-effects (λ_{rt}), and (possibly) a series of individual level controls:

$$w_{it} = \lambda_{grt} + X_{it}\kappa_{gt} + \varepsilon_{it} \tag{2.1}$$

I estimate regression (2.1) separately by gender (thus the gender g subscript) and by decade. In a second stage, I take the gender-specific estimates of the CZ fixed effects and regress them on CZ-level controls:

$$\hat{\lambda}_{grt} = \delta_{gt} + \beta_{gt}s_{rt} + Z_{rt}\gamma_t + \eta_{rt} \tag{2.2}$$

where s_{rt} is log of the CZ population. I estimate this model separately by decade and weight observations by the inverse of the estimated variance of $\hat{\lambda}_{grt}$.

In regression (2.2), I am interested in the gender-specific coefficients β_{gt}. These can interpreted as estimates of the gender-specific urban wage premium. They measure the elasticity of wages with respect to the CZ population. I am interested both in level and the trend of these premia for each gender. In my results, I progressively expand the set of controls in the first (X_{it}) and second (Z_{rt}) stages to explore potential factors driving changes in the gender-specific premiums. Alternatively, by subtracting (2.2) across genders we can obtain a relationship between the city-specific gender wage gap and the city size:[2]

$$\hat{\lambda}_{frt} - \hat{\lambda}_{mrt} = \pi_t + \omega_t s_{rt} + Z_{rt}\rho_t + \nu_{rt} \tag{2.3}$$

in this alternative specification, ω_t shows the slope between city size and the gender wage gap. It captures women's relative urban advantage. For example, a positive ω_t indicates that women have an urban wage premium that is higher than men's, and thus, bigger cities feature lower gender wage gaps.

[2] I estimate (2.3) weighting by the inverse of the estimated variance of the CZ gender wage gap.

2.4 Women gradually gained an urban advantage over time

In panel (a) of figure 2·1 I show estimates of the urban wage premium from model 2.2 for both men and women, panel (b) zooms in on the gap between women's and men's premia using model 2.3. These estimates do not include any additional control variables in either the first or second stages. As a result, the figure shows estimates of the *unconditional* urban wage premium and the *unconditional* female urban advantage.

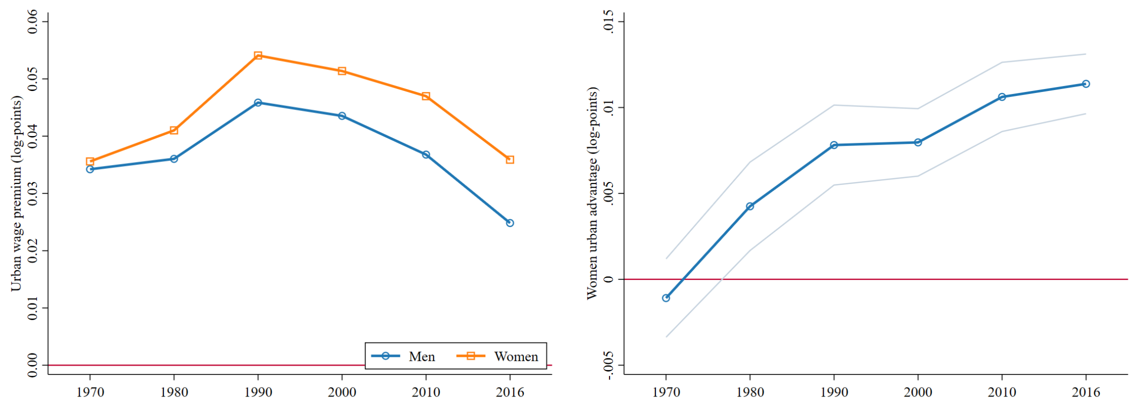
Figure 2·1 shows that women have gradually gained an urban wage advantage over men. Panel (a) shows that in 1970 there was little difference in the urban wage premium across genders. At that time, a doubling of city size was associated with an increase of roughly 3.5% in both men's and women's wages. However, over time, a gap emerged and widened between these premia. By 2016, a doubling of city size was associated with an increase of 3.6% in women's wages, compared to only 2.5% for men's wages. Panel (b) zooms in on this gap and shows that the widening in the female advantage is slow and steady, going from 0 to 1.1 log-points over the whole period.

The gradual increase in the female urban advantage underlies two clear and distinct periods in the evolution of the urban wage premium. Between 1970 and 1990, the urban premia grew an accelerated pace, increasing by 1.2 log-points (34%) for men and 1.9 log-points for women (52%). Therefore, the widening of the urban advantage between 1970 and 1990 is driven by the much faster growth in women's premium. After 1990, there was a fast decline in the premium for both genders, but the female advantage continued to widen due to a slower decline in the female urban premium. Men's premium declined by 2.1 log-points from its 1990 peak (46%), while women's declined by 1.8 log-points (33%).

These trends in wage premiums imply economically relevant changes in the wages

Figure 2·1: Women's urban advantage, 1970-2018

(a) Urban wage premium **(b)** Female urban advantage



Note: The figure shows estimates of the coefficient on the log of the CZ population. In panel (a) the dependent variable is the log of hourly wages, and thus panel shows the evolution of the urban wage premium by gender. In panel (b) the dependent variable is the CZ-gender wage gap, and the panel plots the evolution of the female urban advantage. I estimate both panels by FGLS, with no additional controls in the regressions. First stage standard errors clustered at the CZ level. Panel (b) shows 90% confidence intervals..

across geographies and genders. Table 2.2 shows the wage differences implied by the estimates in Figure 2·1 for CZs of different sizes. Let us consider Kerrville, TX and Boston, MA as examples. Kerrville is a small Texan town that was home to just thirteen thousand people in 1970. This placed it in the 25th percentile of the CZ size distribution for that year. In contrast, Boston was –and still is– the seventh largest CZ of the US. My estimates imply that the gap in wages between Boston and Kerville was the same for men an women in 1970. However, by 2016 women had an edge of 5 percentage points over men. Boston men's wages were 10% larger than Kerville's, while women's were 15% larger.

Table 2.2: Predicted differences in average wages by gender for selected CZ

			Men		Women	
	Percentiles (1970)	**Population**	**Wages**	**%Δ from p5**	**Wages**	**% Δ from p5**
1970						
Philip, SD	5.00	2,637	2.95	0.00	2.46	0.00
Burlington, CO	10.00	5,498	2.98	0.03	2.49	0.03
Kerrville, TX	25.00	13,883	3.01	0.06	2.52	0.06
Wilmington, NC	75.00	97,798	3.08	0.12	2.59	0.13
Saginaw, MI	90.00	242,349	3.11	0.15	2.62	0.16
Youngstown, OH	95.00	446,832	3.13	0.18	2.65	0.18
Boston, MA	100.00	2,183,803	3.18	0.23	2.70	0.24
2016						
Philip, SD	5.00	2,793	2.94	0.00	2.71	0.00
Burlington, CO	10.00	6,295	2.96	0.02	2.74	0.03
Kerrville, TX	25.00	41,654	3.01	0.07	2.80	0.10
Wilmington, NC	75.00	259,195	3.05	0.11	2.87	0.16
Saginaw, MI	90.00	266,016	3.05	0.11	2.87	0.16
Youngstown, OH	95.00	386,753	3.06	0.12	2.88	0.18
Boston, MA	100.00	2,928,351	3.11	0.17	2.96	0.25

Notes: Table shows predicted differences in the average wages on a regression like (2.3). I run the regressions separately by gender. Percentiles based on 1970 population distribution.

I also note that the rise in women's advantage depicted in Figure 2·1 is robust to changes in the estimating sample, changes to my measure of CZ size, and it arises *within* multiple demographic groups. In figure B·1 in the appendix, I show that women's advantage also increases for samples that include self-employed and part-time workers. Moreover, figure B·2 shows that the patterns are similar if I use population density as regressor in the second stage.[3] Moreover, Figures B·3 and B·4 show that women's advantage rises within marital statuses and age groups. This evidence rules out the possibility that these patterns are the result of straightforward changes in demographic composition across CZ of different sizes.

[3]The urban literature emphasizes agglomeration economies coming from concentration of economic activity as a source of the urban wage premium (Glaeser and Gottlieb, 2009; Duranton and Puga, 2020). It is then natural to use population density as a measure of population concentration.

2.5 Women's rising advantage is driven by workers without a college degree

Table 2.3 splits the sample by education level into workers with and without a college degree. I then generated separate estimates for the women's *unconditional* urban advantage within each education group which I show in the top row of each panel. The differences by education level are striking. While the female advantage for workers without a college degree reproduces quite closely the patterns from the previous section, women with a college degree display *urban disadvantage* that was more or less constant throughout the whole period.

Table 2.3: Women's unconditional urban advantage and gender-specific urban wage premia

	1970 **(1)**	**1980** **(2)**	**1990** **(3)**	**2000** **(4)**	**2010** **(5)**	**2016** **(6)**
A.Workers without college degree						
Female urban advantage	0.002	0.005	0.008	0.009	0.013	0.014
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Urban wage premia						
Women	0.032	0.038	0.050	0.047	0.039	0.029
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Men	0.030	0.033	0.041	0.037	0.026	0.015
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
B. Workers with college degree						
Female urban advantage	-0.022	-0.010	-0.017	-0.015	-0.018	-0.016
	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Urban wage premia						
Women	0.028	0.042	0.048	0.043	0.042	0.030
	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Men	0.049	0.052	0.065	0.059	0.060	0.046
	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)

Notes: The table splits the sample by education level and presents estimates of the urban wage premium and female urban advantage for each education level. The regressions do not control for any other covariate.

Table 2.3 also presents estimates for the gender-specific urban wage premium for each education level. These premia exhibit a similar pattern to the ones depicted in Figure 2·1. They increased over the first 20 years, followed by a general decline thereafter. However, the decline for men without a college degree is particularly large, going from 4.1 log-points to only 1.5. Remarkably, this is the only group for which the decline is so large that the value in 2016 is only half that of 1970. For all the other groups I cannot reject that the values of 1970 and 2016 are the same. Therefore, the increase in the advantage for women without a college degree is driven by women experiencing larger increases and milder declines in their premium relative to men.

2.6 Differences in worker and CZ characteristics do not account for the rise in women's advantage

In table 2.4 I focus on workers without a college degree and show estimates of the urban advantage that condition on a series of individual and CZ-level characteristics. The table displays the advantage estimate for 1970 and 2018, as well as the change between these two years. Column (1) shows the *unconditional* estimates, and columns (2) to (8) display *conditional* estimates that progressively control for larger set of variables.

In this table, I am particularly interested in how conditioning on any of these control modifies the overall change in the conditional estimates between 1970 and 2018. I show this change in the third row of the table. If any of these controls accounted for the increase in the female advantage, the change in the conditional estimates should be close to zero. For example, if the increase in the advantage were driven by a faster entry of women into highly paid occupations in bigger cities, then there should not be any change in the advantage once I condition on occupation-by-gender dummies.

Table 2.4: Women's conditional urban advantage for non-college workers, 1970-2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1970	0.001	0.003	0.000	-0.001	-0.002	-0.002	-0.006	-0.006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2016	0.014	0.015	0.015	0.013	0.011	0.011	0.011	0.011
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Advantage change: $\Delta_{2016-1970}$	0.013	0.013	0.015	0.014	0.013	0.013	0.017	0.017
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	4,326	4,326	4,326	4,326	4,326	4,326	4,326	4,326
First stage controls (interacted with gender)								
Age		✓	✓	✓	✓	✓	✓	✓
Race		✓	✓	✓	✓	✓	✓	✓
Marital status			✓	✓	✓	✓	✓	✓
Number of children			✓	✓	✓	✓	✓	✓
Industry				✓	✓	✓	✓	✓
Occupation					✓	✓	✓	✓
Mean CZ commuting time						✓	✓	✓
Second stage controls								
Gender specific labor force participation rates							✓	✓
CZ wage inequality								✓

Notes: The table shows the second stage coefficients from regressions of the CZ wage gap on the CZ size. All regressions weight by the inverse of the estimated variance of the CZ gender wage gap.

Columns (2) and (3) show estimates that condition on interactions between gender and age, race, marital status, and number of children. The presence of children plays prominent rule in explaining the gender gap at the national level (Kleven et al., 2019; Cortés and Pan, 2020). However, conditioning on children and the other demographics has little effect on my estimates of women's advantage.

In columns (4) and (5), I examine whether the industrial or occupational structure of employment plays a significant role in the growth of women's advantage. It is well-established that men and women tend to work in different industries and occupations, which contributes to a significant portion of the overall gender earnings gap (Blau and Kahn, 2015). Additionally, there have been substantial changes in the industrial and occupational composition of employment since the 1970s (Autor and Dorn, 2013; Autor et al., 2015). However, surprisingly, the impact of these variables on the change in advantage is minimal.

In columns (6) to (7), I show the effect of conditioning on several CZ characteristics that serve as tests for potential explanations derived from the gender gap literature. Note that to account for the change in the advantage, these factors must exhibit different trends across CZs of varying sizes. Having said that, including these controls does not modify, or actually increases, the change in the advantage estimates.

In column (6) I show the effect of including interaction between gender and the average CZ commuting time as a first stage regressor. The CZ commuting time is computed as the average time spent in going to work among all the workers residing in the CZ. Le Barbanchon et al. (2021) and Black et al. (2014) show that commuting time is an important determinant of gender differences in labor supply across space because women are more sensitive to commuting time than men. In column (7), I introduce as second stage regressors the CZ-level labor force participation rate of men and women to account for the large variation in the growth rates of female labor force participation across the US (Black et al., 2014). In a simple model with positive self-selection, this variation implies spatial differences in the evolution of the gender wage gap.[4]. Lastly, in columm (8), I show estimates that include the overall CZ wage inequality, measured as the gap between the 90th and the 10th percentile in wages in the CZ. This specification is motivated the observation that increases in wage inequality tend to increase gender wage gaps because women are often located at the bottom of the pay distribution (Blau and Kahn, 1997). In all, none of the explanations in columns (6) to (8) is able to reduce the change estimates and, in fact, conditioning on the labor force participation rates *rises* significantly the change in the advantage estimates.

2.7 The rise in the advantage disappears once I account for the urban task gradient

In Table 2.5 I show the result of adding interactions between the five DOT task indexes and CZ size in the first stage. Note that I *do not interact* them with gender. These indexes capture the intensity of five types of tasks: *non-routine cognitive*, *social*, *routine cognitive*, *routine manual*, non-routine manual. Autor (2019), Rossi-Hansberg et al. (2019) and Bacolod et al. (2009) show that there are strong city size gradients in the employment and wage of occupations that use some of these tasks more intensively.

The third row Table 2.5 shows that the rise in women's urban advantage disappears once I condition on the interactions between the five tasks indexes and CZ size. Column (2) shows results that condition in the first stage on interactions between

[4]To illustrate this point, let us consider an extreme example where male LFP is the same across two cities: city 1 and city 2. Moreover, there is positive selection so that only the most productive people are participating in the labor market. These two cities only differ in the trend of their female LFP. Female FLP is growing in city 1, while it is constant in city 2. Under these conditions, the gender wage gap will be *increasing* in city 1 because women with less productivity are entering the labor market.

the five tasks indexes and CZ city size, along with interactions between gender and age, race, marital status and number of children. For reference, column (1) presents estimates that exclude the task controls. Remarkably, conditioning on the task index interactions reduces the advantage from 1.5 log-points to zero.

Table 2.5: Women's urban advantage and occupational task intensity of workers without a college degree

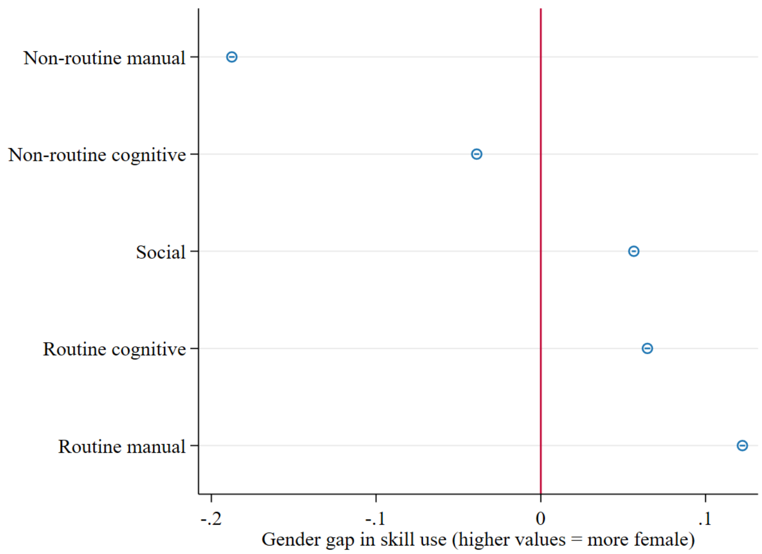
	(1)	(2)
1970	0.000	0.005
	(0.001)	(0.001)
2016	0.015	0.005
	(0.001)	(0.001)
Advantage change: $\Delta_{2018-1970}$	0.015	0.000
	0.002	0.002
First stage controls		
Task indexes		✓
Task indexes × CZ size		✓
Observations	4,326	4,326

Notes: The table shows second stage coefficients of regressions of the CZ gender wage gap on CZ size. The first stage regressions include interactions between gender and age, race, marital status and the number of children.

2.7.1 What makes tasks different?

There are two reasons that make tasks different: (i) men and women use tasks at different intensities, and (ii) there are large differences in how bigger cities are rewarding these tasks over time. It is well established that there is gender segregation across occupations (Blau, 2015). By virtue of this segregation, men and women perform different tasks at their jobs. In figure 2·2, I show the national gender gap in task use for each the five task indexes I consider, where lower values denote higher task use by men. Overall, women perform social, routine cognitive, and routine manual tasks more intensively than men. In contrast, non-college men perform non-routine manual tasks particularly intensively.

Figure 2·2: Gender gap in skill use in workers without a college degree, 1970



Note: The figure shows the coefficients on a female dummy in regressions having the skill index as dependent variable. The figure shows 95% CI.

Table 2.6: Coefficients on the interactions between the task indexes and CZ size

	Non-routine cognitive (1)	**Social** (2)	$\boldsymbol{\Delta_{2016-1970}}$ (3)
Non-routine cognitive	0.0090	0.0810	0.0720
	(0.0019)	(0.0010)	
Social	0.0444	0.0383	-0.0061
	(0.0027)	(0.0016)	
Routine cognitive	0.0187	-0.0149	-0.0336
	(0.0027)	(0.0014)	
Non-routine manual	0.0158	-0.0216	-0.0374
	(0.0015)	(0.0008)	
Routine manual	-0.0231	-0.0061	0.0170
	(0.0023)	(0.0011)	

Notes: The table shows the first stage coefficients on the interaction between the task indexes and CZ size from column (2) of Table 2.5. Sample restricted to workers without a college degree.

Moreover, there are large differences in evolution of the urban wage gradient of each of these tasks among non-college graduates. In Table 2.6 I show the first stage coefficients on the interactions between the task indexes and CZ size for 1970 and 2016. These coefficients correspond to the specification in column (2) of Table 2.5. I will refer to these coefficients as the urban task premia, but I recognize that they can have multiple interpretations.[5] The evolution of the urban premia for each of these tasks is markedly different. While the premia for to non-routine manual and routine cognitive tasks disappeared, and, in fact, became *negative*, there is little change in the premia of social and routine manual tasks and a dramatic increase in the premia to non-routine cognitive tasks.[6] Therefore, women had higher exposure to the steady social and routine manual gradients, while men were more exposed to the plummeting of the non-routine gradient. This differential exposure explains why the urban wage premium on non-college men declined sharply, while women's did so more mildly.

2.8 Conclusions

Using US data for the period between 1970 and 2016, this paper shows new facts on the evolution of the urban wage premium by gender. I show that women are less affected by the overall decline in the urban wage premium from recent decades. In 1970, there was no difference between men's and women's urban wage premium but by 2016, women's premium was 40% larger than men's. This apparent female urban

[5]Because I link tasks to workers using the occupational title, I cannot capture variation in task use within occupations across different labor markets. This means the task gradients in Table 2.6 can have multiple interpretations. For example, they could reflect that bigger cities reward certain tasks differently, but they could also reflect that the same occupation is qualitatively different across labor markets of different sizes. In the first interpretation, the social skills of the manager have a larger reward in bigger CZs, while in the second interpretation managers in big CZs perform a job that is more sophisticated than the managers in the smaller CZs. Distinguishing between these stories is beyond the scope of this paper.

[6]Although table 2.6 shows that between 1970 and 2016 the premia to routine manual tasks increased by 1.7 log-points, the totality of this increase occurred between 1970 and 1980, with little change thereafter.

advantage arises only among workers without a college degree and it is robust to my sample selection and my specification choices.

I then use data from the Dictionary of Occupational Titles to link the rise in women's advantage to changes in the urban wage gradient across tasks. Over this period, there was a drastic decline the urban premia to non-routine manual, while the premia for other tasks either stayed constant or increased sharply. By virtue the occupational segregation across genders, men and women had different exposures to these changes.

Chapter 3

Do Elite Universities Overpay Their Faculty?

César Garro-Marín[1], Shulamit Kahn[2], and Kevin Lang[3]

3.1 Introduction

This chapter measures the relation between faculty salaries (net of faculty quality) and university or college prestige. We find no evidence that more prestigious institutions pay premiums above the competitive salary for the quality of the faculty they hire. Indeed, using an AKM (Abowd et al., 1999) model, we find little evidence of any institution effect on salaries, although institutions in more urban areas pay higher salaries.

The absence of institution effects in the AKM model is striking because their absence implies that, aside from a random factor, faculty would receive the same salary at any university. We authors find it implausible that Oakland University would be willing to match the salaries Stanford pays its tenure-track faculty. Readers are free to draw their own conclusions.

Our evidence is based on the Survey of Doctorate Recipients (SDR), a panel survey of individuals with U.S. doctorates in fields covered by the National Science Foundation. Thus, our results apply to STEM and the social sciences but not necessarily to the humanities or faculty with professional degrees. We merge the SDR with the

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Times Higher Education (*THE*) 2017 rankings of (research) universities and the *Wall Street Journal* – *THE* 2017 college rankings, supplemented with rankings from the *U.S. News and World Report* (*UNSWR*) rankings for some universities and colleges not in the other rankings and well as IPEDS institutional data.

We begin by applying a standard AKM model to the data. The variance of the institution fixed effects is as little as .006, depending on the correction we use. When we regress the estimated fixed effects on institution characteristics, the effect of university or college prestige is always small and generally insignificant. We find some evidence that institutions with larger endowments per student pay premiums, but the magnitude of the premium is modest.

We repeat the exercise but replace the two-step estimation with a single step in which we include institution characteristics rather than institution fixed effects. The results are similar, as expected, since both approaches provide consistent estimates of the same parameters.

We also examine the relation between institution prestige (as measured by rank) and faculty quality, as measured by the individual fixed effect. Consistent with our expectations (and probably the expectations of most faculty at research universities), the correlation is positive.

We briefly discuss how to reconcile the absence of a prestige premium, the positive match between prestige and faculty quality, and our sense that faculty at prestigious institutions would earn less at less prestigious institutions. In essence, we develop a toy hedonic model in which faculty transition only among similarly ranked institutions. We conclude with some thoughts about why our results differ from AKM models of the broad labor market.

3.2 AKM in the Academic Context

AKM uses a standard two-way fixed-effect model

$$\ln w_{ijt} = X_{it}\beta + \alpha_i + \gamma_j + \varepsilon_{ijt} \tag{3.1}$$

where w_{ijt} is annual salary, X_{it} is a vector of time-varying individual characteristics ε_{ijt} is an i.i.d. mean-zero error term.

The institution fixed effect γ_j captures the tendency of the institution to pay all faculty a different salary than they would receive elsewhere. It may reflect compensating differentials for institution characteristics that the econometrician does not measure or institutional rents shared with faculty.

The individual fixed effect, α_i, captures whatever factors tend to raise a faculty member's wage relative to other faculty in the same (or similar) institutions. In the AKM model, α is typically interpreted as a measure of worker quality or productivity, but it captures any other factor that affects pay, such as discrimination or, in our case, differentials across fields. We will largely follow tradition and treat this fixed effect as capturing worker (faculty) quality. However, we note that in an early AKM application, Eeckhout and Kircher (2011) firms pay high wages to workers they hire whether high or low skill. This induces these workers to apply despite being unlikely to receive an offer (see also Abowd et al. (2019)).

It is well known that problems arise if we treat the variance of estimated γ ($\widehat{\gamma}$) as the variance of γ. We correct this bias using Andrews et al. (2008).

It is evident that (3.1) makes strong assumptions. First, AKM assumes that mobility is random. (Formally, ε and γ are uncorrelated.) Applied to academia, AKM assumes that faculty do not change university because the profession has changed its belief about their value or because they are particularly valuable at their new university. Instead, moves reflect changes in personal preferences, etc. Second, the log-linear functional form is highly restrictive; the institution effect is proportional: a given university pays a constant percentage premium to all faculty it hires, except for the random error term ε_{ijt}. Similarly, an individual who earns 20 percent more than the norm at one university would also earn 20 percent more elsewhere, again, except for ε_{ijt}. These assumptions and the logarithmic form imply that better (higher α) faculty gain more in absolute terms by working at a higher-paying university (higher γ).

Under these assumptions, the AKM model allows us to answer several questions:

1. How important are firms for determining salaries? (What is the variance of *gamma* in the estimated AKM model?)

2. How important are differences between individuals (variance of α_i) for determining salaries?

3. Do the best workers go to the best (highest salary) firms? (What is the covariance of α and γ in the estimated (and corrected) AKM model?)

Unlike most applications of AKM, we can measure firm quality directly. We use published rankings and measures such as endowments, potentially correlated with a university's eliteness, to measure prestige. Thus, we address the above questions for academia and relate them to eliteness measures.

Having estimated the university fixed effects by (3.1), we can regress $\widehat{\gamma}$ on university characteristics. This reveals the characteristics associated with university salary premiums.

$$\widehat{\gamma}_j = Z_j\Lambda + \eta_j + \nu_j \tag{3.2}$$

where Z is a vector of university characteristics, η is a random error term uncorrelated with Z consisting of unmeasured university characteristics, and ν is measurement error ($\widehat{\gamma}_j = \gamma_j + \nu_j$).

Alternatively, we can estimate (3.1) and (3.2) in a single step by substituting for γ_j in the AKM equation (3.1) to get

$$\ln w_{ijt} = X_{it}\beta + Z_j\Lambda + \alpha_i + \nu_j + \varepsilon_{ijt} \tag{3.3}$$

As Amemiya (1978) shows, if the variance components of (3.2) and (3.3) are estimated in the same way, generalized least squares (GLS) estimation of the two equations is numerically identical. However, we will estimate (3.2) by feasible GLS but only correct the standard errors in (3.3), thus producing somewhat different results.

3.3 Data

Our primary data come from the restricted-use version of the Survey of Doctorate Recipients (SDR) from the National Center for Science and Engineering Statistics (NCSES). The SDR is a representative longitudinal panel of individuals with doctorates in natural or social sciences, engineering, or health from a U.S. academic institution. Every 2-3 years, the survey collects data on their salaries, employers, and demographic characteristics. It also identifies all U.S. academic employers using the IPEDS institution codes, enabling us to identify the work histories of the academics trained and working in the United States.

We use all SDR waves of data beginning with the 1993 major SDR restructuring through the last year currently available (1993, 1995, 1997, 1999, 2001, 2003, 2006, 2008, 2010, 2013, 2015, 2017, 2019). In most years, the SDR included most survey participants from previous waves. It added participants from newly granted PhD's (identified from the NSF's Survey of Earned Doctorates) and dropped those who aged out. However, in 2015, the SDR created a new larger panel that included only a minority of the original sample. Therefore, most participants have data only before 2015 or from 2015 and later.

The SDR response rate among individuals in the U.S. is quite high. Typically, fewer than 5% of eligible respondents fail to respond. Including those who could not be found, are missing a key item, or live abroad, lowers the rate, but it remains high (75%-85%).

We restrict the sample to individuals employed full-time (35 hours/week for at least 40 weeks/year) in a tenure-stream (tenured or tenure-track) position at a U.S. 4-year college or university, medical school attached to a university, or university research institute. We thus exclude 2-year colleges, junior colleges, technical institutes that do not confer regular degrees, and non-educational institutions. We drop observations where respondents were in a post-doctoral position, earned less than the minimum National Institutes of Health post-doctoral stipend ($53,760 in 2020), or worked outside the U.S. (whether in academia or not).

Unfortunately, using the SDR to study moves requires considerable data cleaning, which we describe in detail in the appendix section C.4. For example, there were 2,916 observations where the IPEDS university code changed, but the respondent reported not changing institutions. These frequently involved two institutions with similar names. Thus, a Boston University faculty member in multiple waves might be miscoded as Boston College faculty for one wave, while not reporting changing institutions. Academics know the difference; some data coders did not.

We also drop observations with large one-time salary changes within the same institution that are subsequently reversed (see appendix for details). The online appendix (Tables A1-A4) contains all tables replicated using these observations, so readers can verify that it has little effect on the results. Since these are within a person/university match, dropping them leaves the number of movers and moves unchanged.

We supplement the SDR data with the rankings from the *Times Higher Education* 2017 World University Rankings and the *Wall Street Journal – Times Higher Education* 2017 College Rankings (Times Higher Education, 2017a,b), hereafter the rankings. We use the *USNWR* Best Colleges rankings (US News, 2021) to impute ranks for institutions without a *THE* rank (see online appendix).

As is well-known, we can only include information on institutions in the connected set in AKM estimation. Institutions may be connected directly or indirectly. If one faculty member moves from university A to university B and another from B to C, A and C are connected. We limit ourselves to the largest connected set, which consists of 679 institutions. Other connected sets were very small. One-step estimation does not require a connected set, but we use the same data to maintain consistency between the two approaches.

We matched 585 (86% of the total) of the 679 institutions to a *THE* ranking. Of the remaining 94, we imputed a rank for 59 schools ranked in USNWR, using the relation between *USNWR* and *THE* ranks, leaving 35 unranked schools (5% of the total). We define *research universities* as those in the *THE* university rankings or imputed from the *USNWR* National University rankings. This group is broader than R1 institutions. We define *colleges* as those included in the *THE* college rankings or imputed from any other *USNWR* ranking. Many of the *colleges* are not liberal arts colleges but simply institutions not included among the *THE* research universities or *USNWR* National Universities. Within each type of institution, we normalize the best rank to 1 and the worst to 100.[4]

The top-ranked research universities are Stanford, Harvard, Cal Tech, and MIT. Those at the bottom include Western Michigan University, Texas State University, Oakland University, and the University of North Carolina, Wilmington. The top-

[4]Due to ties, the lowest ranked college is at the 99$^{\text{th}}$ percentile.

ranked colleges are Amherst, Williams, Wellesley, and Pomona. The worst-ranked include Grambling State University, Southern University of New Orleans, Georgia Southwestern State University, and the University of Rio Grande. The unranked institutions include Texas A&M at San Antonio, Brigham Young University at Idaho, and the University of Texas at Brownsville.

Our data on institution characteristics come from the Integrated Postsecondary Education Data System (IPEDS) surveys. We obtain total enrollment, number of faculty, endowment, and dummy variables for large city, urban fringe/mid-size city/suburb, private institution, and undergraduate-only institution from 1998, 2005, 2012, and 2017. We measure endowment by the average of the beginning and ending values for nonprofit institutions and the average of the beginning and ending equity for for-profit institutions.

Panel A of Table 1.2.3 shows the frequency of moves. We have 64,537 observations on 26,614 individuals, an average of roughly 2.4 observations each. 1,868, or about 7% of individuals, changed institutions at least once. Unsurprisingly, movers are disproportionately those we observe in more waves. Movers account for roughly 13% of our observations.

Panel B shows we observe only one move for most movers. We have 2,196 transitions involving 679 institutions and 1,868 movers, or 1.2 moves per mover and 3.2 moves per institution. Transitions by institutions are highly skewed, with a minimum of 2 and a maximum of 53.

When surveyed, 45% of the faculty observations were full professors and 29% associate professors (see Panel C). A few faculty (1%) report being on the tenure track but holding a title other than assistant, associate, or full professor. About one-third of faculty are female; five-sixths are married when surveyed.

Panel D gives information on the 679 institutions in the connected set, of which 152 are ranked universities and 492 ranked "*colleges*," with the remaining 35 unranked. They vary dramatically in size and endowment. 41% are private, and 22% serve only undergraduates.

Table 3.1: Summary Statistics

A: Number of movers in the sample

	All	Movers	Share of total
Total observations	64,537	8,091	0.13
Number of people	26,614	1,868	0.07
Average obs./person	2.42	4.33	

B: Number of transitions in the sample

	Total		Max
Transitions	2,196		
Number of movers	1,868		
Number of institutions	679		
Transitions/mover	1.18	1	*
Transitions/institution	3.23	2	53

C: Summary statistics: Individuals

Characteristics	N	Mean	Std
Years since Ph.D.	64,537	18.12	10.65
Has tenure	64,537	0.73	0.45
Faculty rank			
Assistant Prof.	64,537	0.25	0.43
Associate Prof.	64,537	0.29	0.45
Professor	64,537	0.45	0.50
Lecturer	64,537	0.00	0.03
Instructor	64,537	0.00	0.04
Other	64,537	0.01	0.09
Female	64,537	0.32	0.47
Married	64,537	0.83	0.38
Has child under 6	64,537	0.18	0.38
Has child aged 6-11	64,537	0.20	0.40
Has child aged 12-18	64,537	0.20	0.40
Has child aged 19+	64,537	0.10	0.30

D: Summary statistics: University-level characteristics

	Mean	Std	Min	Max
Research university rank	48	28	1	99
College rank	46	25	1	100
Log total enrollment	8.75	1.05	5.09	10.89
Log total endowment ($2020)	18.03	2.13	10.90	24.32
Log endowment/student	9.32	1.97	2.89	14.84
Log faculty size	5.79	0.96	0.92	8.04
Log faculty/student	-3.14	0.55	-5.21	-1.69
Share in large city	0.23	0.42	0.00	1.00
Share in medium city	0.34	0.47	0.00	1.00
Share in small city	0.43	0.50	0.00	1.00
Share private	0.41	0.49	0.00	1.00
Share undergraduate	0.22	0.41	0.00	1.00

Note: There are 152 research universities and 492 colleges. 35 institutions are unranked and not classified as colleges or universities. * Suppressed for confidentiality. Exceeds 4.

3.4 Results

3.4.1 How important are the institutions for determining wages? Not much!

We first estimate the AKM model with only individual and institution fixed effects. Table 3.2 shows the overall variance of log salaries is 0.141; the variance of the individual fixed effects with no correction is .131 (93% of the overall variance). In contrast, the variance of the institution fixed effects is .029 (21% of the overall variance), in line with the 20% typically found in AKM models Card et al. (2018). Thus, their sum exceeds the total variance.

Table 3.2: Fixed-effect variance estimates in AKM model

	Uncorrected (1)	**Corrected** Andrews et al. method
Individual by year level		
Variance log(salary)	0.141	0.141
Variance of Fixed-effects		
Individual	0.131	0.105
Institution	0.029	0.012
Correlation	-0.332	-0.397
Collapsed at the spell level		Bonhomme et al. method
Variance log(salary)	0.140	0.140
Variance of Fixed-effects		
Individual	0.128	0.078
Institution	0.026	0.006
Correlation	-0.310	0.081

However, it is well known that we over-estimate these variances, especially in situations like ours where many institutions in the sample experience little turnover (Andrews et al 2012, Kline et al. 2020, Bonhomme et al forthcoming). While $\widehat{\gamma}$ is a consistent estimate of γ, the *variance* of $\widehat{\gamma}$ is not a consistent estimate of the variance of γ. For a simple insight into the problem, consider an extreme case where all the γ are 0 (so $\sigma^2 = 0$) and the $\widehat{\gamma}$ are i.i.d. with variance $\sigma_{\widehat{\gamma}}{}^2$. Then, $\sigma_{\widehat{\gamma}}{}^2$ is completely measurement error. In addition, AKM negatively biases the covariance between the two sets of fixed effects. To see this, note that if we overestimate the institution fixed effect, we will (partially) subtract that overestimate from the individual fixed effect, leading to a negative correlation between the two sets of fixed effects.

When we use the Andrews et al. (2008) correction,[5] the variance of the individual fixed effects falls to .105 or 74.5% of the overall salary variance, while the variance of the institution fixed effects is only .012 or 8.5% of the overall variance (Table 2). Thus, institutions account for little of the total variance. This proportion is about half the estimate in Kline et al. (2020) for Northern Italian workers but in line with Bonhomme et al. (forthcoming) for a Swedish sample with little turnover (similar to our sample) when using the Andrews variance correction.

When we collapse the data to the spell level to reduce measurement error, as in Bonhomme et al. (forthcoming), the total variance of ln(salaries) by spell is .140, similar to the overall variance. As Table 2 shows, the uncorrected variance of the individual fixed effects is .128, but the corrected variance is .078 or 56% of the overall salary variance, somewhat smaller than with the uncollapsed spells. The uncorrected variance explained by institution fixed effects (.026) is similar to what we found without collapsing spells. After correction, this variance is negligible, .006 or only 4% of the salary variation, and somewhat lower as a proportion of variance than Bonhomme et al. (forthcoming) find for five countries, and substantially lower than Kline et al. (2020) report using their preferred correction.

We thus conclude that institution effects explain almost no variation in faculty salaries. Instead, individual faculty (worker) fixed effects explain most of the variance.

Our estimates of the correlation between faculty and institution fixed effects is sensitive to collapsing to the spell level. The uncorrected correlations are negative, as is common in AKM models due to mismeasurement bias, and equal approximately

[5]Given our data, it is not feasible to use the approaches developed by Kline et al. (2020) and Bonhomme et al. (forthcoming).

-.3 (Table 3.2). Since the individual fixed effects may partially be due to field, we also netted out field differences from the individual fixed effects before we calculated the correlations. As is clear, it makes little difference.

However, without collapsing, the corrected correlation is -.40; after collapsing, it is .08. We note that Andrews et al. find little effect of their variance correction unless they restrict the sample to movers and large firms. Since after collapsing, the correction shows institution fixed effects are negligible, it is difficult to interpret the small correlation even though it is positive.

3.4.2 Time-varying individual characteristics: It's mostly rank and experience

Appendix Table C.1 shows the coefficients on the time-varying faculty characteristics in the full AKM model (3.1). Adding these variables decreases the unexplained variance from 8.5% to 4.8%. The coefficients in Table C.1 correspond to our expectations and/or past studies of academic salaries. Salaries increase with post-PhD experience, although at a declining rate. Nevertheless, the point estimate of the slope remains positive at all experience levels in the data. Academic rank, rather than tenure status, affects salaries. The small number of tenure-track lecturers and instructors earn salaries comparable to assistant professors. Associate professors earn a slight premium (5%) relative to assistant professors. In comparison, full professors earn about 10-11% more than comparable associate professors. The small "other" group lies between associate and full professors.

Family composition has little effect on male or female earnings, conditional on rank and experience. The sole exception is that men, but not women, earn about 1% more if they have teenage children. Prior research suggests that children make women less likely to take tenure-track jobs (Ginther and Kahn, 2006; Cheng, 2020; Wolfinger et al., 2008; Martinez et al., 2007). However, among women who do take tenure-stream STEM jobs, children and marriage are positively associated with women's salary in academia (Kahn and Ginther, 2017), as are men's. Yet for both, positive association is likely due to selection which our model captures through the individual fixed effects.

We cannot meaningfully add time-varying *institution* characteristics (such as rankings) to our model because they change very slowly. When they do change, long and uncertain lags in their impact would prevent us from associating salary and institutional changes.

3.4.3 Institution characteristics have little impact on salaries

Despite the near absence of firm fixed effects in the AKM model, we ask whether institution characteristics, and particularly the rank of the institution and university endowments, explain salaries. Because rank and endowment are very highly correlated, we include rank in Table 3.3 and Endowments in Table 3.4. We first regress the 679 institutional fixed effects, γ, from our AKM model (small as they are) on institution type and other characteristics as in equation (3.2). Results are shown in columns 1-3. Then, we instead include these characteristics directly in the ln salary equation as in equation (3.3) (columns 4-6).

Column 1 of Table 3 shows the results of the two-stage estimates, regressing firm effects on institution type and rank. This explains only 1.6% of the variation in institutional fixed effects γ, which itself is only 21% of log(salary) variation (see Table 3.2). Table 3.2 also showed that 60% of the variance in γ's is measurement error (using Andrews et al.), so 1.6% of the (uncorrected) variance of the γ's corresponds to about 4% of the non-measurement error γ variance.

The point estimates in Table 3.3 column 1 imply that the most prestigious university ($\ln$ rank $= 0$) pays a 15% premium and the most prestigious college pays a 10% premium relative to an unranked institution, although neither is significant at standard levels. Comparing among ranked universities, the most prestigious pay premiums of about 10% relative to the least prestigious ($.0212 * \ln(100)$), and similarly for colleges. However, the coefficient on university rank is clearly not significant and the coefficient on college rank has a p-value of .07, while jointly, their p-value is .11 (F=2.23). Yet for all 4 variables, the joint effect of type and rank is significant (p=.024). Column 2 includes urbanicity, that not surprisingly significantly affects salaries. However, controlling for urbanicity reduces the already small effect of the two rank variables individually and jointly (F=1.63, p=.20) and reduces the joint significance of the four institution type and rank variables (F=1.74, p=.14). Column 3 adds several additional institutional characteristics, which hardly changes the other coefficients but further decreases the significance of institution type and rank.

Columns (4)-(6) show the results using one-step estimates. The estimates are generally somewhat smaller but more precise. Consequently, we *can* reject the hypotheses that the research university dummy and university rank have no effect on earnings in column (4). Again, the most prestigious university pays about an 10% premium relative to unranked institutions and also relative to the least prestigious university (which pays roughly the same salaries unranked institutions). However, (ranked) colleges pay only a tiny premium (.009) relative to unranked institutions and there is no difference between colleges of best and worst ranks. Moreover, if we compare the R-squared of .946 in column (4) with the R-squared of .952 explained by the individual fixed effects and individual time-varying variables (Table B.1), it is clear that the 679 institution dummies add very little to the model's explanatory power.

In Table 3.4, we redo the estimation of Table 3.3, replacing the rank of universities and colleges with the (log of) the endowment per student of the university, which measures the resources available to the institution and the rents it can share.

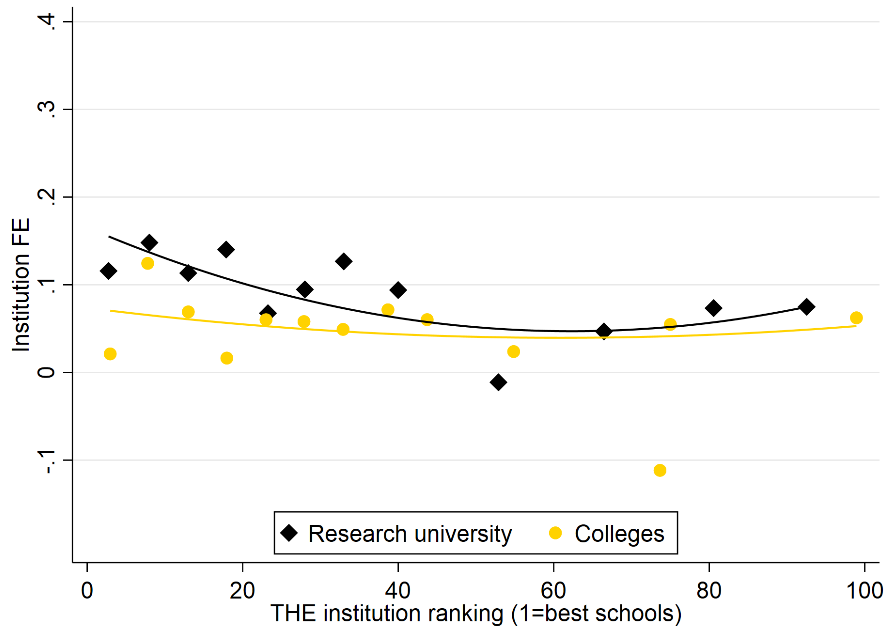
Endowment does have a statistically significant effect. Nevertheless, its impact remains small. Endowment together with university type still explains less than 2% of the variation in university effects (column 1). Moreover, the difference between the largest and smallest endowment per student predicts only a 14% difference in the institution salary effects (γ) in column 1. We have estimated similar models with both endowment *and* rank variables; this lowers the size and significance of both, and R-squared remains less than 4% with all institutional factors included.

The insignificance of rank does not depend on our choice of functional form. Figure 3·1 plots binned institution fixed effects against institution rank separately for universities and colleges and fits quadratics. Both plots are somewhat U-shaped, so that better ranks are not monotonically better. For universities (shown as circles), the gap between the peak (at top ranks) and bottom institutions is noticeable but small (less than ten log points). For colleges (shown with yellow circles), even the difference between the peak and trough is negligible. This is very different from Figure C·1 in the online appendix which shows a definitive negative relationship between binned average *salary* and institution rank.

Appendix Figures C·2 and C·3 show that the institution fixed-effects figure is robust. In C·2 we choose bins to equalize the number of movers across bins. In C·3 we combine institutions with adjacent ranks until each institution or pseudo-institution has at least five movers. This primarily affects colleges because most universities are sufficiently large to have enough movers. The resulting patterns are largely unchanged.

We have also estimated simple correlations between the university logged rank and the university fixed-effects, which range from ρ = -.22 to -.26 in the two-step and one-step estimates, respectively (bottom Table 3). Recall that more prestigious institutions have a lower rank, so this indicates a substantial positive relationship be-

Figure 3·1: Institution pay premium and rank



tween institution and individual quality. The correlations between college institution effects are smaller and more dependent on which estimate is used, with $\rho =$ -0.10 and -.16 for colleges in the 2-step and 1-step models, respectively.

Part of the variation in individual effects may be due to the faculty being from different fields. The most prestigious universities may be willing to pay both anthropologists and economists more than they would earn at less prestigious institutions but do not pay anthropologists and economists equal salaries. However, the correlations between university rankings and the individual fixed effects net of field is the same to 2 decimal places, the correlation between college rank and net individual effects is about .01 greater than reported in Table 3.3.

Table 3.3: Do rankings increase institution fixed effects?

	Two-Step Estimates			One-Step Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
*Institution type * log of rank*						
Research university	-0.021	-0.018	-0.019	-0.0216**	-0.020*	-0.0147
	(0.021)	(0.021)	(0.023)	(0.0098)	(0.011)	(0.011)
College	-0.0218*	-0.0188	-0.0219	-0.006	-0.006	-0.0017
	(0.0119)	(0.0119)	(0.0141)	(0.009)	(0.009)	(0.011)
Institution type (omitted=unranked)						
Research university	0.148*	0.114	0.107	0.098**	0.081	0.0357
	(0.086)	(0.087)	(0.109)	(0.0446)	(0.047)	(0.048)
College	0.096	0.047	0.079	0.009**	0.000	-0.0281
	(0.059)	(0.057)	(0.066)	(0.040)	(0.040)	(0.0412)
Institution characteristics						
Large city		0.076	0.068**		0.047	0.043
		(0.023)	(0.025)		(0.015)	(0.015)
Medium city		0.025	0.022		0.016	0.012
		(0.021)	(0.021)		(0.012)	(0.013)
ln (total enrollment)			-0.008			0.010
			(0.014)			(0.009)
Undergrad only			-0.055**			-0.034
			(0.024)			(0.018)
Private institution			-0.011			0.026
			(0.030)			(0.019)
Joint significance of 2 rank variables						
F statistic	2.23	0.982	0.329	2.079	1.892	0.554
p-value	0.108	0.375	0.720	0.126	0.151	0.575
Joint significance of university type and rank variables						
F statistic	2.8	1.741	1.3	3.22	2.648	1.320
p-value	0.024	0.139	0.27	0.012	0.032	0.261
Correlation between individual fixed-effects and ln(rankings)						
Universities		-0.223		-0.264	-0.259	-0.259
Colleges		-0.095		-0.162	-0.157	-0.152
Observations	679	679	679	64,537	64,537	64,537
R squared	0.013	0.029	0.038	0.946	0.946	0.946

Notes: Standard errors in parenthesis. Institution rank ranges from 1 (best) to 100. Additional controls: individual fixed-effects, years since PhD,rank(lecturer, instructor, associate, full, other), tenured, female, married, children (<6, 6-11, 12-18, 19+), female*married, female*children. Two-step estimates are obtained by including institution fixed effects and regressing the institution coefficients on the explanatory variables. One-step estimates replace the institution fixed effects with the institution characteristics. Research universities are mainly R1 but include some R2 institutions. Colleges include all remaining post-secondary institutions granting four-year degrees. Large, medium, and small cities have populations above 250k, between 100k and 250k, and under 100k repectively. In columns (5)-(9) standard errors are clustered at the institution level.

Table 3.4: Does endowment increase institution fixed-effects?

	Two-Step Estimates			One-Step Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
ln (endowment per student)	.0102**	.0096**	0.0108**	0.0083**	.0089**	0.0059
	(0.0047)	(0.0047)	(0.0066)	(0.0033)	0.0032	(0.0041)
Institution type (omitted=unranked)						
Research university	0.0493	0.029	0.0107	-0.0080	-0.0223	0.0392
	(0.0451)	(0.0454)	(0.0509)	(0.0257)	(0.0254)	(0.0276)
College	0.0057	0.0026	-0.0127	-0.0288	-0.0406*	-0.0472*
	(0.0413)	(0.0412)	(0.0420)	(0.0240)	(0.236)	(0.0243)
Institution characteristics						
Large city		0.0713***	0.0679***		0.0504	0.0439***
		(0.0235)	(0.0253)		(0.0147)	(0.0150)
Medium city		0.0317	0.0286		0.0165	0.0133
		(0.0207)	(0.0212)		(0.0125)	(0.0126)
ln (total enrollment)			-0.0053			0.0120
			(0.0128)			(0.0090)
Undergrad only			-0.0552**			-0.0323*
			(0.0237)			(0.0173)
Private institution			-0.0099			0.0266
			(0.0296)			(0.0193)
Observations	679	679	679	64,537	64,537	64,537
R squared	0.017	0.030	0.038	0.946	0.946	0.946

Notes: Standard errors in parenthesis. Institution rank ranges from 1 (best) to 100. Additional controls: individual fixed-effects, years since PhD, rank (lecturer, instructor, associate, full, other), tenured, female, married, children (<6, 6-11, 12-18, 19+), female×married, female×children. Two-step estimates are obtained by including institution fixed effects and regressing the institution coefficients on the explanatory variables. One-step estimates replace the institution fixed effects with the institution characteristics. Research universities are mainly R1 but include some R2 institutions. Colleges include all remaining post-secondary institutions granting four-year degrees. Large, medium, and small cities have populations above 250k, between 100k and 250k, and under 100k respectively. In columns (5)-(9) standard errors are clustered at the institution level.

3.4.4 Why does institutional affiliation matter so little?

We find the absence of institution effects counterintuitive. Consider the University of Wisconsin, Madison, and the University of Wisconsin, Oshkosh. Both schools are in our data set and have publicly available salaries. In academic year 2021, the median full professor of economics at UWM earned \$370,954 – almost three times as much as the median economics full professor at UWO, who earned \$126,193. Now, imagine the

UWM professor with the median earnings ($370,954) moving exogenously to UWO and vice versa. What salary do you think they would receive? This exchange is hard to imagine, but our results suggest that it would not change their salaries since there are no meaningful university effects. We find it unlikely that UWM would hire anyone as a tenured Professor of Economics whom it was only willing to pay $126,193. It is equally unlikely that UWO would be willing to hire an economics professor with tenure for almost $100,000 more than it pays its Chancellor. Readers are, of course, free to disagree with our intuition.

There are no clear patterns in salary changes upon moving

In the online appendix Table C.2, we show salary changes as people move from and to institutions, by quintile rank of universities and colleges. On average, all transitions raise salaries, which is unsurprising since we expect most people to move to better-paid jobs. However, there are few, if any, other clear patterns.

In particular, faculty do not receive larger raises when moving to a better institution (as they would if elite institutions paid more) or when moving to a worse institution (as they would if they received a compensating salary differential). If we focus on research universities, those exiting jobs in the top or second quintile see the largest gains if they end up in the second quintile; but those exiting the third quintile institution do best if they end up in the fourth quintile and worst in the third. Those starting in the fourth do slightly better ending up in the second than the fourth but noticeably better than ending in the first or third. If the AKM model is correct, the effects of moving from A to B and B to A should be equal and of opposite sign, net of any mobility premium. Instead, in our data, salary changes are independent of the direction of movement, consistent with more prestigious institutions not paying rent.

Movement among institutions is not random

The thought experiment at the beginning of the subsection is challenging because we rarely observe movements across institutions differing so wildly in prestige. To be consistent, the AKM model requires that mobility be random; the error term must be uncorrelated with the explanatory variables, most notably the individual and faculty fixed effects. We will see that movement is not random, although not necessarily in a manner that challenges the AKM assumptions.

The tendency of faculty to move to institutions of relatively similar eliteness is clear from the transition matrix, Table C.3 in the online appendix. We suspect that the table probably overstates mobility across prestige levels since the prestige of individual departments is not always similar to overall institution prestige. Nevertheless, when tenure-stream faculty leave a university in the top quintile, almost half (45%) remain in the top university quintile, 66% within the top two quintiles, and 76% within the top two quintiles of universities *or* colleges (not shown). There is only a 0.5% chance of them moving to the lowest-quintile university and almost no chance of moving to a lowest-quintile college.

Similarly, roughly 70% of moves from a university that end in a top-tier university come from first or second-tier universities, and another 6% from top colleges. The likelihood of moving to the best university from either the lowest quintile universities, the bottom 2 quintile *colleges*, or unranked institutions is tiny. However, movements involving the most-elite university quintile are somewhat atypical in their degree of insularity. For other quintiles and for colleges, movement to proximate quintiles is more common. Movements originating in the highest quintile universities are also more common than those originating in other quintiles or in colleges. Still, regardless of an academic institution's type and rank, there is limited movement to very different institutions. 72.6% of those starting in universities move within +/- 1 university quintile or to a more highly ranked college.

Moreover, there is relatively little movement from universities to colleges (31% of university movers, even though 44% of destination jobs are in colleges) and particularly little movement from colleges to universities (35% of college movers, even though 54% of destination jobs are in universities). Finally, of those who start and end in universities, the same percentage (21%) go to worse-ranked jobs as go to better-ranked jobs. However, of those who start and end in colleges, far more (26%) go to worse-ranked jobs than better-ranked jobs (13%).

Hedonics may explain wages and mobility

We found a substantial positive correlation between faculty fixed effects and university and, to a lesser extent, college prestige (shown at the bottom of Table 3.3). Simultaneously, we find no evidence that more prestigious institutions pay salary premiums. Consistent with this, there is considerable mobility between institutions. However, salary changes do *not* show a pattern where moving to higher-prestige institutions increases salaries. We suggest that a hedonic model augmented with idiosyncratic tastes fits our results well.

There is a continuum of institutions with prestige, p. The salary an institution is willing to pay for a particular match, w_m, depends on the potential faculty member's quality, $q \in Q$ and p:

$$w_m = w_m(q, p), \; \frac{\partial w_m}{\partial p} > 0 \tag{3.4}$$

We assume that w_m is continuous in p. We further assume that for any $p' > p''$, there is a q^* such that

$$w_m(q^*, p') = w_m(q^*, p'') \tag{3.5}$$

and

$$w_m(q, p') > w_m(q, p'') \iff q > q^* \tag{3.6}$$

This ensures that institutions' willingness-to-pay curves cross exactly once. Under these assumptions, there will be a unique p that maximizes an individual's compensation.

To take a simple example, let

$$w_m = -p^2 + pq \tag{3.7}$$

Then salary is maximized at $p = 0.5q$, and salary is $w_m = 0.25q^2$ at the maximum.

With perfect matching, the observed salary is the upper envelope of the individual institution willingness-to-pay curves. While, in the example, each institution's willingness-to-pay is linear, the equilibrium salary is convex in worker quality as in Roy (1951).

With perfect matching, we cannot distinguish between individual and worker effects. Earnings can be fully explained by either p or q. For instance, in the above example, the maximizing salary, w_m can also be expressed as $w_m = p^2$.

Moreover, suppose individuals deviate slightly from their optimal institutions. Then, the effect on their earnings is only second-order since the derivative of earnings with respect to prestige is 0 at the optimum. On the other hand, the difference between the imperfectly-matched faculty's q relative to other faculty at that institution is first order. Therefore, individual fixed effects and not institution fixed effects explain wages.

Consider an individual with $p = p^*$ has and, therefore, $q = 2p^*$ at their highest-pay institution. Consider a second institution $p' = p^* + \varepsilon$. The individual earns $-(p' - \varepsilon)^2 + 2(p' - \varepsilon)(p' - \varepsilon)$ when matched to p^*, but only $-p'^2 + 2p'(p' - \varepsilon)$ when imperfectly matched to p'. Taking the difference gives the tiny difference ε^2. However, comparing the well-matched individual at p^* with a well-matched individual at p' who earns $-p'^2 + 2p'(p')$, the difference is a larger $2p'\varepsilon$.

Therefore, we do not observe our University of Wisconsin economists exchanging campuses because both would take significant salary cuts since they are poor matches at the other institution.

Intuitively, in the example, the mismatch between faculty and institution is not very different among proximate universities. Neither earns rents because there are similar institutions that would offer them essentially the same salary.

Online appendix C.3 develops this example. In the appendix, the variance of log salaries is .14, as in our data. If the highest and lowest quality faculty were both matched with the most prestigious institution, a highly improbable event for the latter, the ratio of their earnings would be 11. The example allows for a significant degree of mismatch. For example, the median quality faculty has a 6-7% chance of ending up in each of the top and bottom quintiles. Nevertheless, the variance of the institution effects is trivial.

3.5 Discussion and conclusion: is academia different?

Applying standard AKM techniques to tenure-track academic jobs, we find no evidence that prestigious institutions pay rents to their STEM faculty. Individual faculty members do differ considerably in their salaries, even when netting out field effects. Moreover, the individual effects are quite correlated with the institution's rank. However, when we use AKM methods to separate out the firm and person effects based on movements of individuals between institutions, we find that practically all of the variation is in the person effects. We present a simple model suggesting that if faculty and institution are optimally matched, AKM estimation can lead to seemingly small institution effects.

Whether our results differ from findings for broader labor markets depends somewhat on which study we compare our results with. Nevertheless, the finding that establishment effects are small to nonexistent puts our results at the low end of the range of estimated effects. We can only speculate as to why our findings for faculty differ from the broader labor market. The most likely explanation is that the labor markets are simply different.

For instance, many dimensions along which faculty success is measured – publications in prestigious journals, appointments to prestigious societies, editorships etc. – are the same dimensions that feed into the success or prestige of the universities as well. Moreover, these dimensions are visible both inside and outside the institutions. This alone is likely to make rents unlikely in academia. In contrast, in other labor markets, individuals' contributions to productivity are often difficult for both the firm and the worker to measure, and are not visible at all outside the firm, making rents feasible.

It is also possible that the technologies are different. Faculty positions are quintessential star jobs (Baron and Dreps, 1999); successes are rare and valuable; failures are common and not very costly. In contrast, (Bose and Lang, 2017) argue that most nonacademic jobs are guardian jobs in which the gains from an especially good performance are small, but the costs of a bad performance are very large. In such settings, firms with high costs of failures would only hire workers who had demonstrated their competence and would pay those workers a premium. Mobility would primarily be upward; workers moving to high-wage firms would earn a premium. However, the premium would not be rent but a payment for their revealed high quality.

In some labor markets, higher firm salaries may be due to compensating wage differentials (Sorkin, 2018). However, in academia, non-wage aspects of the job (e.g.

light workloads, more research support, better students) are highly positively correlated with prestige, so we would not see high salaries compensating for low levels of other job characteristics.

Nothing in our results allows us to distinguish among these explanations and perhaps others that may occur to readers. However, we believe that our results, while perhaps interesting in their own right, should encourage us to reflect more on the interpretation of the AKM model.

Appendix A

Appendix to chapter 1

A.1 Tables

Table A.1: Indonesia: number of existing rengencies by year, 1980-2010

	1980	**1990**	**2000**	**2010**
Number of regencies	286	295	339	493

Notes: These regencies have chaning borders across decades. In my analysis, in each year, I aggregate these unis into 268 consistent-boundary regencies. Data IPUMS international.

Table A.2: Dispersion in regional employment rates within countries

Country	Women			Men			Pop.	Obs.
	IQR	SD	Mean	IQR	SD	Mean		
Benin	0.35	0.19	0.44	0.08	0.06	0.76	57,764	77
Zimbabwe	0.30	0.19	0.59	0.13	0.08	0.77	70,597	88
Guinea	0.29	0.19	0.52	0.11	0.09	0.84	22,567	209
China	0.28	0.17	0.71	0.14	0.10	0.85	266,748	2,845
Nepal	0.26	0.17	0.63	0.05	0.03	0.81	191,443	72
Ecuador	0.24	0.13	0.43	0.03	0.03	0.83	104,465	78
Zambia	0.23	0.15	0.50	0.09	0.07	0.64	108,098	55
Indonesia	0.22	0.14	0.53	0.05	0.04	0.87	533,867	268
Myanmar	0.21	0.13	0.51	0.07	0.05	0.86	83,531	362
Panama	0.20	0.12	0.33	0.04	0.08	0.80	56,049	35
Tanzania	0.20	0.12	0.69	0.09	0.05	0.82	178,632	113
Vietnam	0.19	0.12	0.82	0.06	0.06	0.90	79,146	674
Brazil	0.19	0.11	0.48	0.19	0.11	0.73	59,010	2,040
Mexico	0.17	0.11	0.30	0.09	0.08	0.80	27,853	2,330
South Africa	0.16	0.11	0.30	0.06	0.06	0.53	138,127	224
Cambodia	0.16	0.11	0.84	0.08	0.05	0.90	50,186	174
Thailand	0.16	0.11	0.81	0.08	0.06	0.88	58,290	670
Costa Rica	0.16	0.08	0.37	0.05	0.04	0.73	48,673	55
Nicaragua	0.16	0.09	0.31	0.10	0.06	0.81	38,849	68
Argentina	0.15	0.10	0.53	0.08	0.06	0.83	75,022	312
Kenya	0.15	0.10	0.68	0.06	0.06	0.79	513,569	35
Sierra Leone	0.15	0.11	0.71	0.15	0.09	0.75	27,333	126
Togo	0.14	0.10	0.72	0.08	0.05	0.80	75,345	37
Philippines	0.13	0.10	0.30	0.08	0.06	0.82	40,423	1,274
Mauritius	0.13	0.20	0.53	0.03	0.06	0.83	16,626	50
Bolivia	0.12	0.06	0.58	0.05	0.03	0.86	70,323	80
Chile	0.12	0.08	0.51	0.05	0.04	0.79	57,826	192
Spain	0.11	0.08	0.51	0.09	0.06	0.61	105,902	286
Malaysia	0.11	0.07	0.38	0.06	0.04	0.84	91,509	133
Greece	0.10	0.06	0.43	0.05	0.04	0.66	42,492	156
Uganda	0.10	0.10	0.83	0.05	0.05	0.89	111,479	136
USA	0.09	0.07	0.67	0.10	0.07	0.77	202,635	722
Ghana	0.08	0.05	0.76	0.06	0.05	0.78	122,422	102
Senegal	0.06	0.05	0.19	0.09	0.06	0.58	233,811	27
Bangladesh	0.02	0.03	0.06	0.04	0.03	0.87	1,335,491	60

Notes: SD and IQR stand for Standard Deviation and Interquartile Range. The table shows statistics for all countries in IPUMS International with geographic data below the state/province level. Rows are ordered from the highest to the lowest IQR in women's employment rates. For all countries I use census sample from 2010 or the closest available year. I aggregate data at the smallest geographical unit available, except for the USA where I use Commuting Zones as in Autor and Dorn (2013). Column (7) shows the total population for the average geographic unit in each country. I show the unweighted cross-locality means which –might– differ from the national-level means.

Table A.3: Dispersion in employment and paid employment rates for selected countries

Country	All employment IQR	All employment Mean	Paid employment IQR	Paid employment Mean	Observations
Benin	0.35	0.44	0.37	0.41	77
Zimbabwe	0.30	0.59	0.30	0.59	88
Guinea	0.29	0.52	0.24	0.43	209
Nepal	0.26	0.63	0.27	0.62	72
Ecuador	0.24	0.43	0.23	0.42	78
Zambia	0.23	0.50	0.06	0.27	55
Indonesia	0.22	0.53	0.12	0.34	268
Panama	0.20	0.33	0.21	0.33	35
Tanzania	0.20	0.69	0.21	0.67	113
Vietnam	0.19	0.82	0.11	0.72	674
Brazil	0.19	0.48	0.20	0.46	2,040
Mexico	0.17	0.30	0.16	0.27	2,330
Thailand	0.16	0.81	0.09	0.69	670
South Africa	0.16	0.30	0.16	0.30	224
Costa Rica	0.16	0.37	0.16	0.37	55
Nicaragua	0.16	0.31	0.16	0.31	68
Argentina	0.15	0.53	0.15	0.53	312
Kenya	0.15	0.68	0.15	0.68	35
Sierra Leone	0.15	0.71	0.16	0.66	126
Togo	0.14	0.72	0.17	0.59	37
Philippines	0.13	0.30	0.12	0.28	1,274
Mauritius	0.13	0.53	0.13	0.52	50
Bolivia	0.12	0.58	0.12	0.56	80
Chile	0.12	0.51	0.12	0.51	192
Malaysia	0.11	0.38	0.11	0.38	133
Spain	0.11	0.51	0.11	0.50	286
Greece	0.10	0.43	0.10	0.43	156
Uganda	0.10	0.83	0.12	0.76	136
Ghana	0.08	0.76	0.08	0.61	102
Senegal	0.06	0.19	0.05	0.17	27
Bangladesh	0.02	0.06	0.02	0.06	60

Notes: IQR stands for Interquartile Range. The table shows data from all countries in table A.2 with data that distinguishes unpaid and family workers from other worker types.

Table A.4: Female labor force participation rages by country: IPUMS vs ILO-STAT

Country	**IPUMS (ages 18-64)**	**ILOSTAT (ages 15+)**	**Difference**
Cambodia	0.82	0.81	0.01
China	0.74	0.64	0.10
Indonesia	0.50	0.51	-0.01
Malaysia	0.43	0.43	-0.00
Myanmar	0.50	0.53	-0.03
Philippines	0.33	0.48	-0.15
Thailand	0.77	0.64	0.13
United States	0.67	0.58	0.10
Vietnam	0.79	0.72	0.07

Notes: Uses data from IPUMS international and ILOSTAT. I restrict the sample in IPUMS to people aged between 18-64 years old.

Table A.6: Indonesia: estimates birthplace persistence on men's labor supply (***b***)

	(1)	(2)	(3)	(4)
Women's employment rate at birthplace (p_o)	0.01	0.04	0.05*	0.04
	(0.03)	(0.03)	(0.03)	(0.03)
Mean employment rate	0.90	0.90	0.90	0.90
Implied IQR gap	0.00	0.01	0.01	0.01
Year FE	✓	✓	✓	✓
Regency FE	✓	✓	✓	✓
Age		✓	✓	✓
Religion			✓	✓
Education				✓
Observations	60,126	60,126	60,126	60,126
N individuals	6,293	6,293	6,293	6,293
R^2	0.05	0.17	0.17	0.18

Notes: Uses data from IFLS. Sample restricted to people residing outside their birthplace. Implied IQR gap shows the implied employment gap between someone born at a regency at the 75th percentile of employment rate and someone born at the 25th percentile. The IQR of the female employment rate across regencies is of 22 percentage points. Standard errors clustered by regency of origin. When indicated, the regressions control for a quadratic polynomial in age, and fixed-effects for seven religion and for education categories. Standard errors clustered by regency of origin.

Table A.5: Source IPUMS samples for cross-country data

Country	Geographic unit	Years of sample	
Argentina	Department	2010	2001
Bangladesh	Upazila	2011	2001
Benin	Commune	2013	2002
Brazil	Municipality	2010	2000
Cambodia	District	2013	2008
Chile	Department	2017	2002
China	Prefecture	2000	
Costa Rica	Cantón	2011	2000
Ecuador	Cantón	2010	2001
Ghana	District	2010	2000
Greece	Municipality	2011	2001
Guinea	Sub-prefecture	2014	
Indonesia	Regency	2010	2000
Kenya	District	2009	1999
Malaysia	District	2000	1991
Mauritius	Municipal ward	2011	2000
Mexico	Municipality	2010	2000
Myanmar	Township	2014	
Nepal	Municipality	2005	1995
Panama	District	2010	2000
Philippines	Municipality	2010	2000
Senegal	Department	2013	2002
Sierra Leone	Sierra Leone	2015	2004
South Africa	Municipality	2011	
Spain	Municipality	2011	2001
Tanzania	District	2012	2002
Thailand	District	2000	1990
Togo	Prefecture	2010	
Uganda	County	2014	2002
USA[1]	Commuting zone	2012	
Vietnam	District	2009	2001
Zambia	Constituency	2010	2000
Zimbabwe	District	2012	

Note: the table details the source samples from the cross-country data in IPUMS International. All cross-country comparisons are based on the most recent sample. The less recent samples are used only for cross-country comparison of employment rate persistence. [1]USA data for 2010 comes from the 5-year ACS sample for 2012.

Table A.7: Indonesia: estimates of birthplace persistence on labor supply (***b***) for men who emigrated young

	(1)	(2)	(3)	(4)
Women's employment rate at birthplace (p_b)	0.19***	0.19***	0.22***	0.19***
	(0.05)	(0.04)	(0.04)	(0.03)
Mean employment rate	0.87	0.87	0.87	0.87
Implied IQR gap	0.04	0.04	0.05	0.04
Regency-year FE	✓	✓	✓	✓
Age		✓	✓	✓
Religion			✓	✓
Education				✓
Observations	19,537	19,537	19,537	19,537
R^2	0.09	0.25	0.25	0.28

Notes: This table uses data from the Intercensal Survey and restricts the sample to women who reside outside their birthplace and who left before they turned 19. The implied IQR gap shows the implied employment gap between someone born at a regency at the 75th percentile of employment rate and someone born at the 25th percentile. The IQR of female employment rates across regencies is 22 percentage points. Standard errors are clustered by regency of origin. When applicable, regressions control for a quadratic polynomial in age and fixed effects for five religious and four education categories.

Table A.8: Indonesia: high female employment regencies have worse educational outcomes

Regency group	**Years of schooling** (1)	**Primary completed** (2)	**Secondary completed** (3)
Low female employment	7.86	0.78	0.30
	(0.13)	(0.01)	(0.01)
High female employment	6.82	0.70	0.21
	(0.13)	(0.01)	(0.01)
Observations	258	258	258

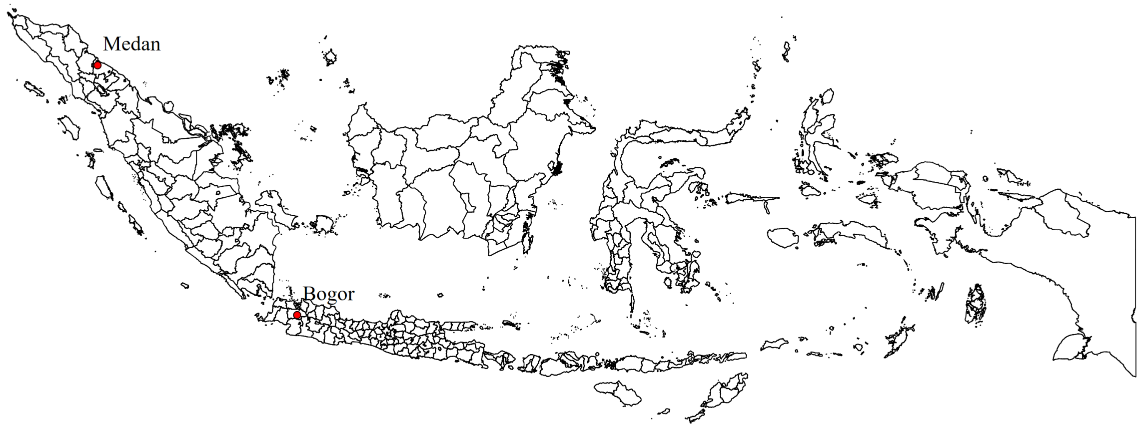
Notes: This table uses data from the 2005 Intercensal Survey. I split regencies at the median of the female employment rate.

A.2 Figures

A.3 Cross-country data

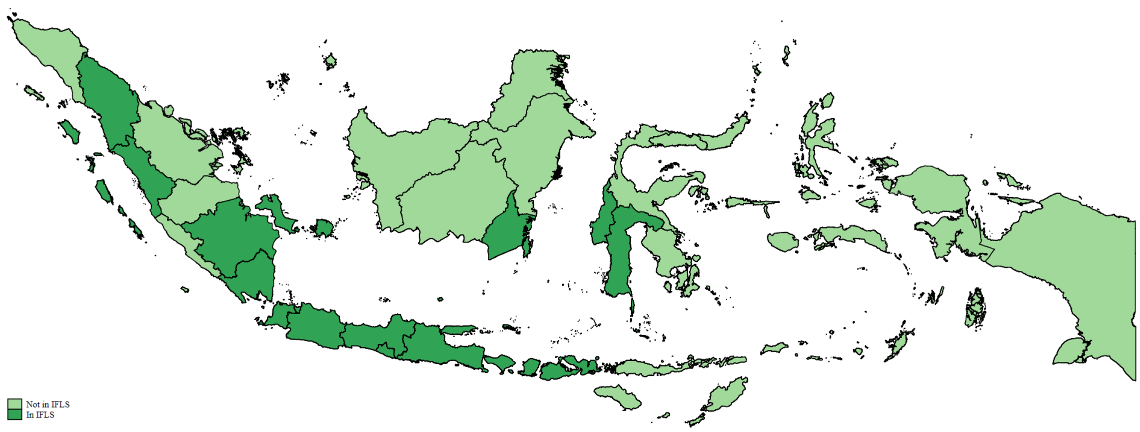
I use harmonized data from IPUMS International to build figure 1·1 from the introduction and table 1.4 from section 1.3. They show local employment rates for men and women aged 18-64 for a cross-section of countries. For all of them, I use the latest decennial census sample available. In most cases, this corresponds to 2010 or a year close to it.

Figure A·1: Indonesian regencies



Note: The figure shows the 268 regency aggregates with consistent boundaries between 1970 and 2018. Boundaries obtained from IPUMS International. It highlights with red dots the locations of the city of Medan and Bogor regency. Medan, the capital and largest city in the province of North Sumatra, is the third most populous city in Indonesia as of 2020 (Brinkhoff, 2022). Bogor, with over five million people, borders the Jakarta metropolitan area.

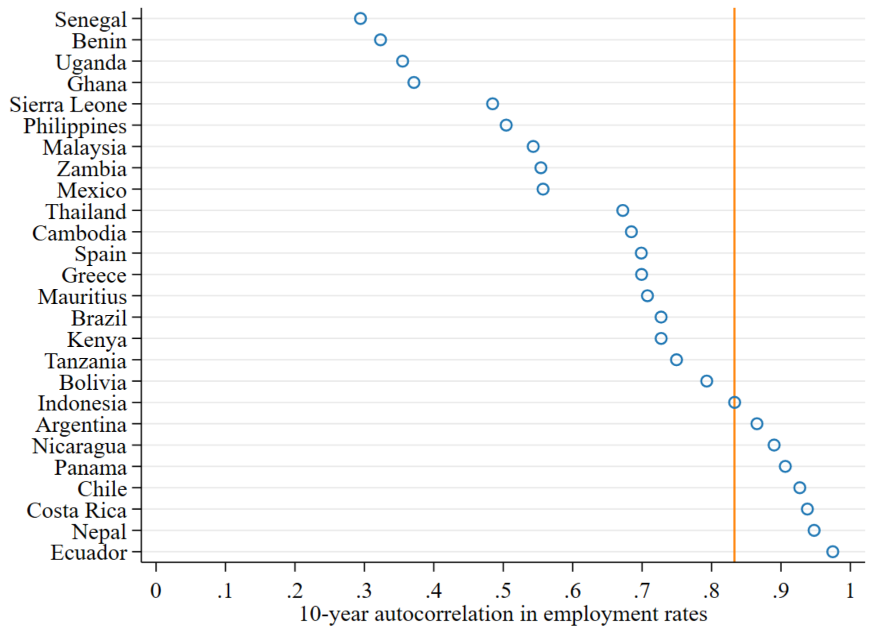
Figure A·2: Provinces in the original 1993 IFLS sample



Note: The provinces from which the original 1993 IFLS sampled households. Because of migration, subsequent years can include individuals living outside these provinces. *Source:* RAND corporation.

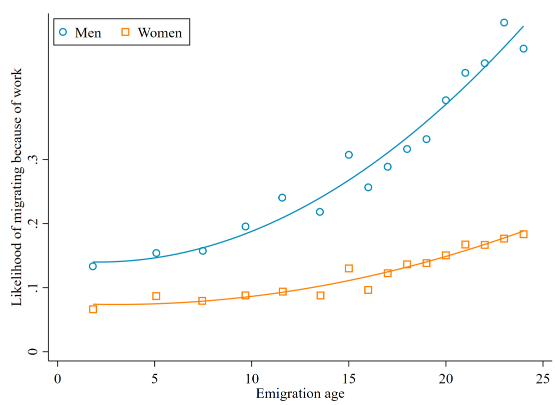
I define employment using the harmonized employment status (`empstat`). When this variable is not available, if the class of worker is available (`classwkr`), I say a person is employed if they report being self-employed, a salaried worker, or an unpaid worker in the variable. In China, employed workers are those who reported working at least 1 day in the past week. Despite these slight definition differences, table A.4 shows that the employment rates I obtain are in line with the female labor force

Figure A·3: 10-year autocorrelation in female employment rates at the district level for selected countries



Note: The figure shows the 10-year autocorrelation in female employment rates. I aggregate data at the smallest geographical unit available which often corresponds to a district/county. Data from IPUMS international.

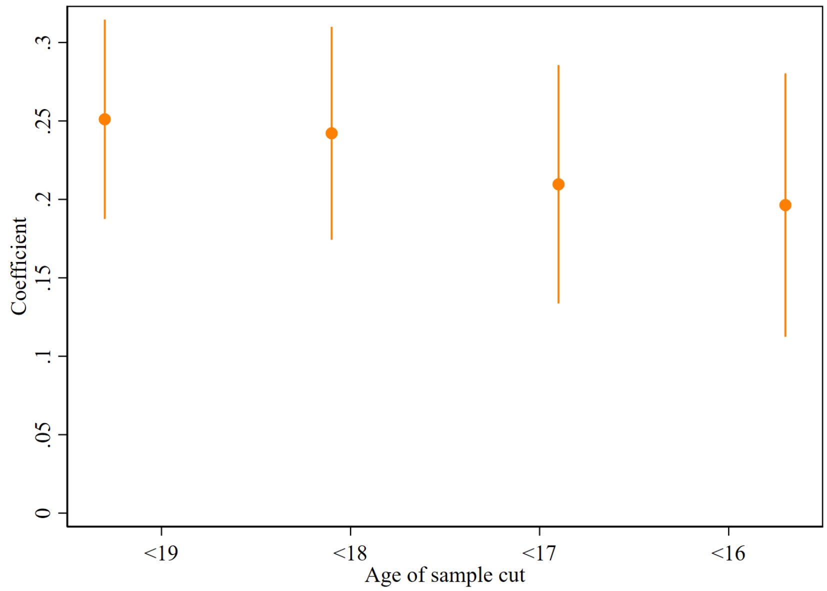
Figure A·4: Indonesia: likelihood of work-related migration by emigration age



Note: The survey does not distinguish whose work generated the move. Thus, the move can be related to parents' job, own job, or husband's / wife's job. Data from 1985 intercensal survey. The 1995 and 2005 surveys only list cause of migration for migration 5 years ago, and a very limited number of observations are available for people younger than 19. Figure generated on 1 Mar 2023 at 15:35:52.

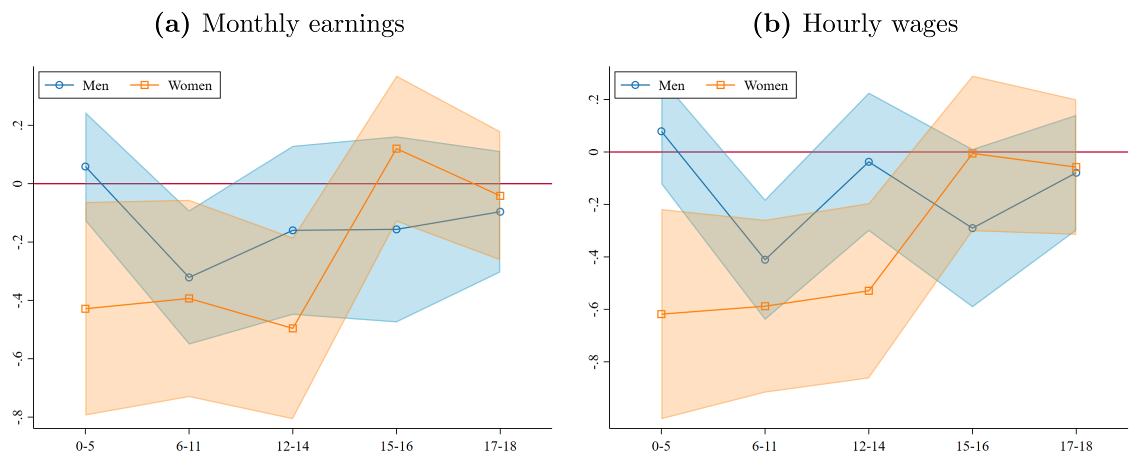
participation rates reported by the International Labor Organization and the World

Figure A·5: Estimates of birthplace persistence for different emigration age cutoffs



Note: This figure uses data from the Intercensal Survey and restricts the sample to women who reside outside their birthplace and who left before they turned 19. Standard errors are clustered by regency of origin. All regressions control for a quadratic polynomial in age and fixed effects for five religious and four education categories. The figure shows 95% confidence intervals.

Figure A·6: Indonesia: earnings and length of stay at birthplace



Note: Data from 1995 intercensal survey. The regression controls for current regency fixed-effects, a quadratic polynomial on age, and education level fixed-effects. The figure shows 90% confidence intervals.

Bank (International Labour Organization, 2021).[1] The differences in the age ranges I consider drive the discrepancies for the United States, Vietnam, Thailand and China.

For all countries, I compute subnational employment rates at the lowest geographic unit available. For most countries, this corresponds to a geographic area akin to a district, a county, or a municipality. The only exception is the United States, where I compute these rates by commuting zone (Autor and Dorn, 2013). Table A.5 provides further details on the unit of aggregation and samples used. I winsorize the employment rates at the 5th and 95th percentiles by country. This reduces the possibility that very small regions drive the dispersion I observe within countries.

A.4 The empirical strategy

A.4.1 Place and women's labor supply: the identification challenge

The place of residence can, directly and indirectly, affect women's labor supply. Direct effects affect the labor supply of all the current female residents. There is considerable empirical evidence documenting these effects. These might arise, for example, from factors such as the levels of childcare availability (Compton and Pollak, 2014), commuting costs (Le Barbanchon et al., 2021; Farre and Ortega, 2021), the industry makeup of employment (Olivetti and Petrongolo, 2014), or the level of gender discrimination in the local labor market (Charles et al., 2018). Differences across localities in any of these factors will cause geographic differences in women's labor supply. However, place can also affect women indirectly by affecting their preferences and their skills. Women born and brought up in locations where many women work can internalize these norms and thus be more likely to work as adults (Charles et al.,

[1]The only exception is the Philippines, where the data from IPUMS International implies much lower employment rates. In my data, I obtain a female employment rate of 33% for women aged 18-64. The ILOSTAT database reports a female labor force participation rate of 48% for 15+ women in 2010. The gap between these two figures cannot be accounted for by female unemployment which is of the order of 4%. That said, I am interested in within-country dispersion, these discrepancies are second order as long data collection is consistent within the country.

2018; Boelmann et al., 2021). Moreover, environments with high female employment may encourage women to invest in the skills they need to participate in the labor market (Molina and Usui, 2022). These permanent indirect effects will create differences in labor supply across women born in different locations *irrespective* of where they currently reside. Evidence on these indirect effects is much more scarce in the literature (Charles et al., 2018).

The omitted variable problem

In this paper, my main interest lies in determining what women's labor supply would be if, conditional on the current place of residence, she was born in an area where more women work. This counterfactual exercise keeps the woman, her family, and her place of residence fixed and varies only her childhood experience. To answer this question, I study the labor supply of women residing outside their birthplace. Because for these women, the place of residence is different from their birthplace, I can separate the indirect effects from the direct impact of place. More formally, let us consider the following model for women's probability of employment e_{it},

$$e_{it} = \delta_{c(i)} + \boldsymbol{\sigma} p_{b(i)} + \eta_{it} \tag{A.1}$$

In this model, women's employment choices depend on three main factors. First, an effect form her place-of-residence captured by the fixed effect $\delta_{c(i)}$ which captures all the direct effects of location c on her labor supply. These might include commuting costs, childcare availability, and gender discrimination. Second, the female employment rate of her location of birth $p_{b(i)}$ is intended to capture the causal effect of growing up in a location where $p_b(i)$ percent of the women work. Finally, the error term η_{it} captures all other factors making some women migrants more likely to work than others.

Model (A.1) follows closely the tradition brought forth by the "epidemiological" approach literature (Fernández and Fogli, 2006; Fernández et al., 2004; Fernández, 2013). Women's birthplace could have multiple impacts on women's behavior as adults. Including the prevailing female employment rates as the main regressor in equation (A.1) relies on the idea that these rates capture the place-driven factors vital in determining women's employment choices. Moreover, focusing on the exposure in the origin location, allows to isolate variation potentially driven by environmental factors –culture, institutions–, from variation driven by purely economics factors, such as wages, and income. This specification also facilitates testing whether alternative channels are driving the relationship with the birthplace employment rates (Fernández, 2013).

In model (A.1), $\boldsymbol{\sigma}$ captures the birthplace effects. It gives the counterfactual increase in women's employment if they had been born in a place with a one p.p. higher female employment rate. In the ideal, but unfeasible experiment, I would reassign women's birthplace randomly while keeping their family and the current residency fixed. Random assignment would guarantee that women's birthplace is uncorrelated with the error term. Thus an OLS regression of (A.1) would give a consistent estimate of $\boldsymbol{\sigma}$. In observational data, however, it is likely that the unobserved factors imbued in the error term are correlated with birthplace labor supply. Therefore, the OLS estimates of employment rate slope will conflate the causal effects of birthplace with omitted variable bias:

$$\begin{aligned} \text{plim } \hat{\boldsymbol{\sigma}} &= \boldsymbol{\sigma} + \frac{\text{cov}(\tilde{p}_{b(i)}, \tilde{\eta}_{it})}{\text{var}\left(\tilde{p}_{b(i)}\right)} \\ &= \boldsymbol{\sigma} + \boldsymbol{\gamma} \end{aligned} \tag{A.2}$$

where tilde accents denote variables that are residualized from regency fixed effects (Angrist and Pischke, 2009). Expression (A.2) shows that the OLS coefficient reflects two factors: first, the causal effect of birthplace $\boldsymbol{\sigma}$, but also differences in unobservable characteristics across women from different origins $\boldsymbol{\gamma}$. The critical identification challenge is separating the selection term $\boldsymbol{\gamma}$ from the birthplace effect $\boldsymbol{\sigma}$.

The selection term $\boldsymbol{\gamma}$ highlights that even in the absence of a causal effect, birthplace could capture characteristics about a person or their family that are relevant to their work decision. In the paper, I argue that the causal effect of place is positive ($\boldsymbol{\sigma} > 0$). That is, being born in a place where more women work, makes you more likely to work. In these circumstances, I am be more concerned with omitted variable –or selection– bias making women from high-employment birthplaces more likely to work than their low-employment counterparts. For example, previous research shows that daughters from working mothers are more likely to work (Fernández, 2007). Even in the absence of a causal effect, a positive $\hat{\boldsymbol{\sigma}}$ could simply be reflecting that, in places where more women work, girls are more likely to be raised up by a working mothers.

Using emigration age data to identify causal effects

Under additional assumptions, data on the age of emigration allows me to distinguish selection from the causal effect of place. The argument is similar to that of Chetty and Hendren (2018a). I assume that place effects are stronger the longer women stay there. Thus, the employment choice for women who emigrated at age a is determined as follows:

$$e_{it} \quad = \quad \delta_{c(i)} + \lambda_a + \boldsymbol{\sigma}_a p_{b(i)} + \eta_{it} \tag{A.3}$$

Here $\boldsymbol{\sigma}_a$ captures the cumulative effect of birthplace up to age a[2]. The age of emigration fixed-effects λ_a absorb differences in labor force participation across women who emigrated at different ages. The causal impact of staying in the birthplace at age a

[2]The causal effect $\boldsymbol{\sigma}$ in the previous subsection can be interpreted as a weighted average of age-specific causal effects.

is then $\boldsymbol{\pi}_a = \boldsymbol{\sigma}_a - \boldsymbol{\sigma}_{a-1}$.

By an argument analogous to that in expression (A.2), the OLS estimates will conflate the causal effects of birthplace $\boldsymbol{\sigma}_a$ with the omitted variable bias for women migrating at age a $\boldsymbol{\gamma_a}$:

$$\text{plim } \hat{\boldsymbol{\sigma}}_a = \boldsymbol{\sigma}_a + \boldsymbol{\gamma}_a \tag{A.4}$$

Assumption A.4.1. ***Constant omitted variable bias***
Omitted variable is the same no matter the age of emigration, that is $\boldsymbol{\gamma}_a = \boldsymbol{k}$

This assumption requires that, conditional on the fixed effects of location and age of emigration, the correlation between the birthplace employment rate and the error term is the same for women who emigrated at different ages. To make this point more concrete, let's consider work-related migration as an example. It is conceivable that women who migrated with work in mind would be more likely to be employed in their destination, and women in their 20s would be more likely to migrate because of work. At first glance, this would seem to invalidate the identification strategy. However, my strategy does not require that women migrating at different ages have the same likelihood of migrating for work. Rather, it requires a much weaker condition: that the correlation between birthplace FLFP and the likelihood of work migration is the same for women migrating at different ages. Therefore, even though older teenagers are more likely to migrate for work (see Figure A·4 in the appendix), this does not violate the identification assumption.

Under the constant omitted variable bias assumption, I can isolate the birthplace causal effect from the omitted variable bias. By subtracting the OLS estimates across different emigration ages, the constant selection term $\boldsymbol{\gamma}$ goes away, leaving only the causal effects:

$$\begin{aligned} \text{plim } \boldsymbol{\hat{\sigma}_a} - \boldsymbol{\hat{\sigma}_{a-1}} &= \boldsymbol{\sigma}_a - \boldsymbol{\sigma}_{a-1} \\ &= \boldsymbol{\pi}_a \end{aligned} \tag{A.5}$$

this expression also shows that identification does not necessarily require constant bias across all *all* emigration ages. If, instead, bias is constant only within some age ranges, I can still identify the effects within those ranges. For example, suppose there is reason to believe that the bias for women who emigrated between 0 to 6 years old is different than for those who emigrated between the ages of 7 to 15. If constant selection holds *within* these ranges, I can still identify the place effects within the 0 to 6 and 7 to 15 ranges.

Appendix B

Appendix to chapter 2

B.1 Data

B.1.1 Sample definitions

I restrict the sample to salaried workers aged between 18 and 64 years old, not attending school and who live outside of group-quarters. My classification of non-farm workers is based on the 1950 census occupational classification. It excludes all workers working in the following occupations:

Table B.1: Farm workers, 1950 census occupational classification

Occupation code	Description
100	Farm owners and tenants
123	Farm managers
810	Farm foremen
820	Farm laborers, wage workers
830	Farm laborers, unpaid family workers
840	Farm service laborers, self-employed

B.1.2 Task requirements

Data on occupational task requirements comes from the US Dictionary of Occupational Titles (DOT) and the extended version of the 1971 Current Population Survey (National Academy of Sciences, 1971). The DOT contains multiple task measures of mainly two types: (i) discrete scales based on how high the task requirements are relative to the population, or (ii) dummies indicating whether the job requires a given aptitude. I normalize all these measures to range from zero to one, and construct five skill indexes:

- **Social index:** is the average of the interactive task measures in the DOT. These are: *adaptability to situations involving the interpretation of feelings, ideas, or facts* (FIF), *adaptability to influencing people* (INFLU), *dealing with people* (DEPL).
- **Non-routine cognitive:** is the average of *direction, planing and control* (DCP), and the *GED math requirements.*
- **Routine cognitive:** is given by *adaptability to set limits, tolerances, or standards* (STS).
- **Non-routine manual:** measured by the variable *eye-hand-foot coordination* (EYEHAND).
- **Routine manual:** measured by *finger dexterity* (FINDEX).

I assign these task indexes to each of the occupations in the 1950 occupation classification (occ1950). To do so I create a cross-walk between the augmented CPS and each Census as follows:

1. First, I calculate the averages for each 3-digit occupation by gender cell in the augmented CPS dataset.
2. Next, I merge these averages with the 1970 census using the census occupational classification (occ) and gender as linking variables.
3. Finally, I impute the values for any occupation-gender cell in the census that does not have task data by assigning them the average of their respective 2-digit occupation code by gender cells.

B.2 Tables

Table B.2: Summary statistics at the CZ-level, 1970-2016

	Census year					
	1970	**1980**	**1990**	**2000**	**2010**	**2016**
Observations	722	722	722	722	722	722
A. CZ population (000)						
p25	14.65	19.25	18.08	20.50	21.62	21.36
Mean	139.78	165.83	179.83	206.58	225.90	236.18
p75	103.11	132.47	133.32	151.91	167.36	172.92
B. CZ gender wage gap (men's - women's)						
sd	0.07	0.07	0.08	0.05	0.06	0.06
p25	0.44	0.39	0.31	0.23	0.18	0.16
Mean	0.48	0.44	0.35	0.27	0.22	0.20
p75	0.52	0.49	0.38	0.30	0.25	0.23

Notes: Sample restricted to full-time year-round workers in mainland US. The table shows means weighted by the census sampling weight.

Table B.3: Occupation with the highest task requirements by task index

Position	A. Social	B. Non-routine cognitive
1	Auctioneers	Agricultural sciences-Professors and instructors
2	Editors and reporters	Metallurgical, metallurgists-Engineers
3	Buyers and dept heads, store	Psychology-Professors and instructors
4	Advertising agents and salesmen	Chemistry-Professors and instructors
5	Salesmen and sales clerks (nec)	Misc. natural scientists
6	Subject not specified-Professors and instructors	Social sciences (nec)-Professors and instructors
7	Librarians	Mathematics-Professors and instructors
8	Personnel and labor relations workers	Chemical-Engineers
9	Watchmen (crossing) and bridge tenders	Engineering-Professors and instructors
10	Musicians and music teachers	Biological sciences-Professors and instructors

Position	C. Routine cognitive	D. Routine manual
1	Auto mechanics apprentice	Bank tellers
2	Furriers	Artists and art teachers
3	Machinists and toolmakers apprentice	Veterinarians
4	Electricians apprentice	Chiropractors
5	Bricklayers and masons apprentice	Dentists
6	Dancers and dancing teachers	Physicians and surgeons
7	Misc. natural scientists	Machinists and toolmakers apprentice
8	Millwrights	Pharmacists
9	Boilermakers	Machinists
10	Upholsterers	Jewelers, watchmakers, goldsmiths, and silversmiths

Position	D. Non-routine manual
1	Airplane pilots and navigators
2	Sailors and deck hands
3	Roofers and slaters
4	Apprentices, other specified trades
5	Excavating, grading, and road machinery operators
6	Sports instructors and officials
7	Bricklayers and masons apprentice
8	Firemen, fire protection
9	Dancers and dancing teachers
10	Bus drivers

Notes: The table shows the top 10 occupation by task requirements in the 5-year ACS of 2018. The table uses 3-digit occupation codes from the 1950 census.

Table B.4: Occupation with the lowest task requirements by task index

Position	A. Social	B. Non-routine cognitive
205	Jewelers, watchmakers, goldsmiths, and silversmiths	Sawyers
206	Stationary firemen	Sailors and deck hands
207	Bricklayers and masons apprentice	Ushers, recreation and amusement
208	Sawyers	Messengers and office boys
209	Misc. natural scientists	Elevator operators
210	Oilers and greaser, except auto	Cranemen,derrickmen, and hoistmen
211	Machinists and toolmakers apprentice	Oilers and greaser, except auto
212	Laundresses, private household	Chainmen, rodmen, and axmen, surveying
213	Mathematicians	Weavers, textile
214	Molders, metal	Laundresses, private household

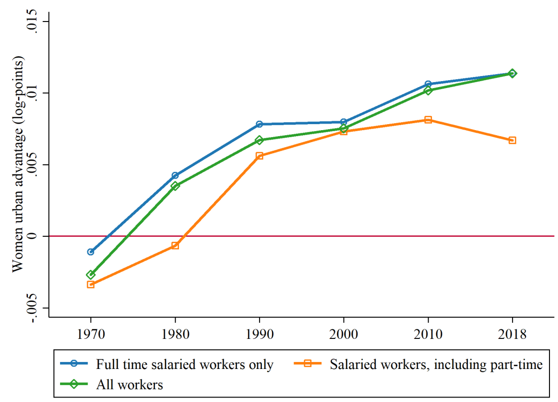
Position	C. Routine cognitive	D. Routine manual
205	Ushers, recreation and amusement	Natural science (nec)-Professors and instructors
206	Psychology-Professors and instructors	Agricultural sciences-Professors and instructors
207	Postmasters	Buyers and shippers, farm products
208	Mathematics-Professors and instructors	Auctioneers
209	Optometrists	Psychology-Professors and instructors
210	Collectors, bill and account	Laundresses, private household
211	Sheriffs and bailiffs	Watchmen (crossing) and bridge tenders
212	Social sciences (nec)-Professors and instructors	Personnel and labor relations workers
213	Mail carriers	Policemen and detectives
214	Chiropractors	Lawyers and judges

Position	D. Non-routine manual
205	Physics-Professors and instructors
206	Funeral directors and embalmers
207	Chemistry-Professors and instructors
208	Postmasters
209	Auctioneers
210	Dietitians and nutritionists
211	Authors
212	Dispatchers and starters, vehicle
213	Agricultural sciences-Professors and instructors
214	Pharmacists

Notes: The table shows the bottom 10 occupation by task requirements in the 5-year ACS of 2018. The table uses 3-digit occupation codes from the 1950 census.

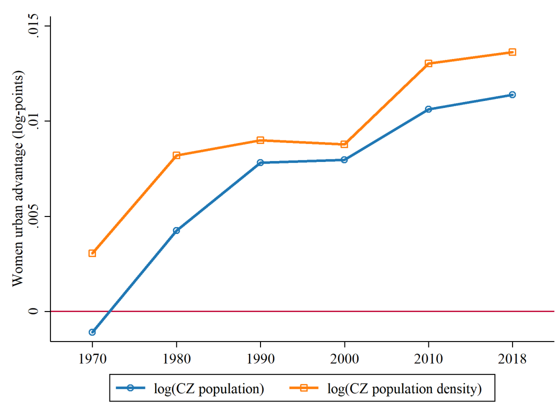
B.3 Figures

Figure B·1: Women's urban advantage for alternative sample selections, 1970-2018



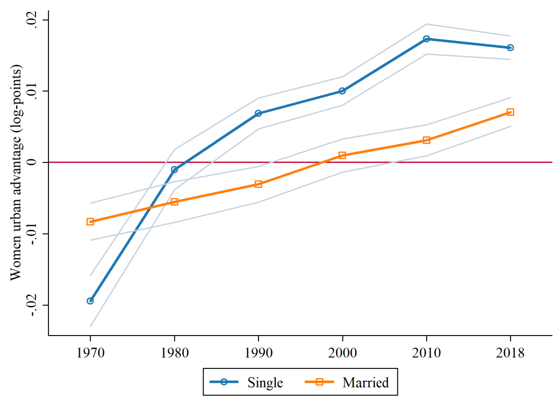
Note: The figure shows the unconditional urban wage advantage. All workers includes self-employed and part-time workers. Observations are weighted by the inverse of the estimated variance of the CZ-gap. Regressions are run separately each year. Figure shows 90% CI.

Figure B·2: Women's urban advantage for alternative CZ size measures, 1970-2018



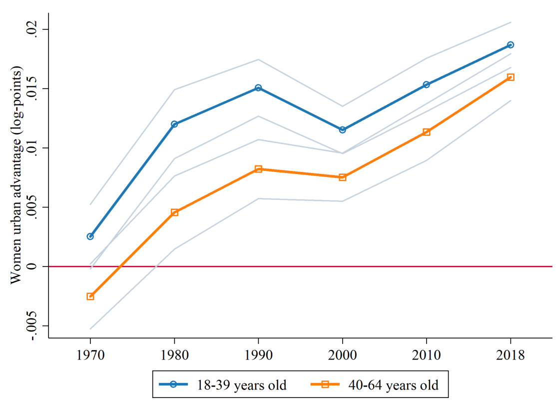
Note: The figure shows the unconditional urban wage advantage. All workers includes self-employed and part-time workers. Observations are weighted by the inverse of the estimated variance of the CZ-gap. Regressions are run separately each year. Figure shows 90% CI.

Figure B·3: Women's urban advantage by marital status, 1970-2018



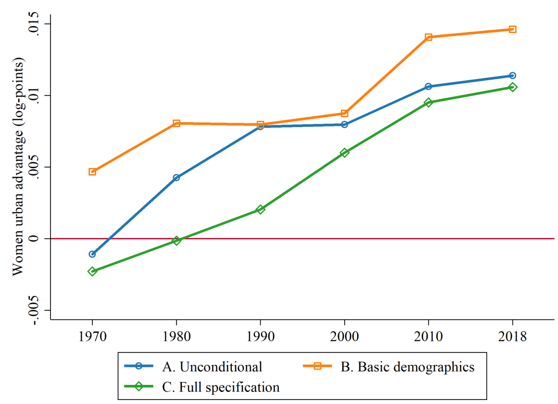
Note: The figure shows the unconditional urban wage advantage. The estimates come from a regression of CZ-specific gender wage gaps on the log of CZ population. Observations are weighted by the inverse of the estimated variance of the CZ-gap. I don't include any controls in the first stage. . Regressions are run separately each year and marital status.Figure shows 90% CI.

Figure B·4: Women's urban advantage by age group, 1970-2018



Note: The figure shows the unconditional urban wage advantage. I do not include any controls in the first stage. The estimates come from a regression of CZ-specific gender wage gaps on the log of CZ population. Observations are weighted by the inverse of the estimated variance of the CZ-gap. . Regressions are run separately each year and age group.

Figure B·5: Women's urban advantage for alternative conditioning on covariates, 1970-2018



Note: Sample restricts to full-time year-round workers in mainland USA. First stage regressions condition on individual-level covariates. Basic demographics controls age, race and education. Full specification controls for age, race, education, and a full set of occupation and industry dummies. Observations are weighted by the inverse of the estimated variance of the CZ-gap. First stage regressions run separately each year. Basic demographics controls for age, race, and education. Full specification controls for age, race, education, and a full set of occupation and industry dummies. Observations are weighted by the FGLS weight. First stage regressions are run separately by year. Figure shows 90% CI.

Appendix C

Appendix to chapter 3

C.1 Tables

Table C.1: Effect of time-varying characteristics

	(1) Full sample	(2) No outliers
Years since PhD	0.0374	0.0356
	(0.0071)	(0.0068)
Years since PhD squared	-0.0003	-0.0002
	(0.0000)	(0.0000)
Is tenured	0.0060	0.0067
	(0.0087)	(0.0069)
Faculty rank (omitted=assistant professor)		
Lecturer	-0.0279	0.0142
	(0.0775)	(0.0407)
Instructor	-0.0038	-0.0069
	(0.0376)	(0.0368)
Associate professor	0.0496	0.0456
	(0.0100)	(0.0079)
Professor	0.1587	0.1459
	(0.0124)	(0.0098)
Other	0.0882	0.0818
	(0.0211)	(0.0187)
Married	0.0081	0.0052
	(0.0076)	(0.0057)
Married × female	0.0032	0.0022
	(0.0115)	(0.0087)
Children below 6	-0.0010	0.0018
	(0.0055)	(0.0041)
Children below 6 × female	0.0012	-0.0063
	(0.0089)	(0.0075)
Children between 6 and 11	0.0021	0.0039
	(0.0047)	(0.0038)
Children between 6 and 11× female	-0.0090	-0.0096
	(0.0073)	(0.0061)
Children between 12 and 18	0.0118	0.0102
	(0.0041)	(0.0035)
Children between 12 and 18× female	-0.0159	-0.0181
	(0.0078)	(0.0065)
Children between 19+	0.0034	0.0030
	(0.0044)	(0.0036)
Children between 19+× female	-0.0086	-0.0078
	(0.0097)	(0.0081)
Individual FE	✓	✓
Year FE	✓	✓
Observations	65893	64537
Number of movers	1868	1868
R^2	0.9123	0.9516

Notes: Standard errors in parenthesis. Column (1) uses the full sample. Column (2) excludes extreme within-institution wage changes.

Table C.2: Salary wage changes by transition type

	Universities					Colleges					
	Best	**2**	**3**	**4**	**Worst**	**Best**	**2**	**3**	**4**	**Worst**	**Unranked**
Origin	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Universities											
Best	0.330	0.393	0.296	0.177	N.D.	0.190	0.123	0.037	0.094	N.D.	N.D.
2	0.356	0.408	0.282	0.226	0.229	0.245	0.103	0.165	0.072	N.D.	0.253
3	0.275	0.256	0.089	0.320	N.D.	0.340	0.289	0.241	0.093	N.D.	N.D.
4	0.210	0.337	0.219	0.305	0.265	0.295	0.265	0.229	0.163	N.D.	0.183
Worst	N.D.	0.272	0.269	0.063	N.D.	N.D.	0.382	0.275	0.287	N.D.	N.D.
Colleges											
Best	0.356	0.218	0.297	0.347	N.D.	0.311	0.290	0.196	0.173	N.D.	N.D.
2	0.331	0.236	0.400	0.233	N.D.	0.193	0.178	0.250	0.164	N.D.	-0.015
3	0.278	0.245	0.201	0.208	0.101	0.188	0.181	0.209	0.162	N.D.	0.218
4	N.D.	0.135	0.251	0.166	N.D.	0.476	0.123	0.182	0.190	N.D.	N.D.
Worst	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.
Unranked	0.331	N.D.	N.D.	0.233	N.D.	N.D.	0.178	0.250	0.164	N.D.	N.D.

Notes: Data from cells with less than 5 individuals were suppressed to preserve confidentiality. We denote these cells with N.D.

Table C.3: Transition probability by ranking quintile and institution type

	Universities					**Colleges**					**Unranked**
	Best	**2**	**3**	**4**	**Worst**	**Best**	**2**	**3**	**4**	**Worst**	
Origin	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Universities											
Best	0.446	0.219	0.084	0.062	N.D.	0.044	0.047	0.062	0.03	N.D.	N.D.
2	0.238	0.181	0.151	0.090	0.019	0.058	0.099	0.079	0.055	N.D.	0.025
3	0.146	0.190	0.241	0.091	N.D.	0.062	0.077	0.117	0.051	N.D.	N.D.
4	0.067	0.172	0.124	0.129	0.033	0.053	0.086	0.153	0.115	N.D.	0.057
Worst	N.D.	0.113	0.097	0.097	0.081	N.D.	0.113	0.210	0.145	N.D.	N.D.
Colleges											
Best	0.151	0.086	0.086	0.059	N.D.	0.092	0.217	0.184	0.079	N.D.	N.D.
2	0.084	0.154	0.044	0.062	0.022	0.141	0.163	0.167	0.132	N.D.	0.026
3	0.049	0.070	0.115	0.101	0.049	0.070	0.098	0.259	0.15	N.D.	0.035
4	N.D.	0.050	0.078	0.177	0.035	0.071	0.17	0.199	0.163	N.D.	N.D.
Worst	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	0.333	N.D.	N.D.	N.D.
Unranked	N.D.	N.D.	N.D.	0.271	N.D.	N.D.	0.305	0.102	0.102	N.D.	N.D.

Notes: Data from cells with less than 5 individuals were suppresed to preserve confidentiality. We denote these cells with N.D.

Table C.4: Summary statistics including wage outliers

A: Number of movers in the sample

	All	Movers	Share of total
Total observations	65,893	8,192	0.12
Number of people	26,873	1,868	0.07
Average obs./person	2.45	4.39	

B: Number of transitions in the sample

	Total	Min	Max
Transitions	65,893	8192	0.12
Number of movers	26,873	1868	0.07
Number of institutions	2.45	4.39	
Transitions/mover	1.18	1	*
Transitions/institution	3.23	2	53

*Suppressed, exceeds 4

C: Summary statistics: Individuals

Characteristics	N	Mean	Std
Years since Ph.D.	65,893	18.18	10.66
Has tenure	65,893	0.73	0.45
Faculty rank			
Assistant Prof.	65,893	0.25	0.43
Associate Prof.	65,893	0.29	0.45
Professor	65,893	0.45	0.5
Lecturer	65,893	0	0.03
Instructor	65,893	0	0.04
Other	65,893	0.01	0.09
Female	65,893	0.32	0.47
Married	65,893	0.83	0.38
Has child under 6	65,893	0.18	0.38
Has child aged 6-11	65,893	0.2	0.4
Has child aged 12-18	65,893	0.2	0.4
Has child aged 19+	65,893	0.1	0.3

D: Summary statistics: University-level characteristics

	Mean	Std	Min	Max
Research university rank	48	28	1	99
College rank	46	25	1	100
Log total enrollment	8.75	1.05	5.09	10.89
Log total endowment ($2020)	18.03	2.13	10.90	24.32
Log endowment/student	9.32	1.97	2.89	14.84
Log faculty size	5.79	0.96	0.92	8.04
Log faculty/student	-3.14	0.55	-5.21	-1.69
Share in large city	0.23	0.42	0	1
Share in medium city	0.34	0.47	0	1
Share in small city	0.43	0.5	0	1
Share private	0.41	0.49	0	1
Share undergraduate	0.22	0.41	0	1

Note: There are 152 research universities and 492 colleges. 35 institutions are unranked and not classified as colleges or universities.

Table C.5: Fixed-effect variance estimates in AKM model including wage outliers

	Uncorrected (1)	**Corrected** Andrews et al. method
Individual by year level		
Variance log(salary)	0.148	0.148
Variance of Fixed-effects		
Individual	0.140	0.110
Institution	0.029	0.012
Correlation	-0.325	-0.398
Collapsed at the spell level		
Variance log(salary)	0.140	0.140
Variance of Fixed-effects		
Individual	0.129	0.077
Institution	0.027	0.006
Correlation	-0.318	0.058

Table C.6: Do rankings increase institution fixed effects (including wage outliers)

	Two-Step Estimates			One-Step Estimates		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
*Institution type * log of rank (low ranks best)*						
Research university * ln(rank)	-0.0203	-0.0174	-0.0192	-0.0179	-0.0167	-0.0104
	(0.0208)	(0.0208)	(0.0213)	(0.0100)	(0.0108)	(0.0111)
College * ln(rank)	-0.0197	-0.0169	-0.0220	-0.0056	-0.0062	-0.0006
	(0.0122)	(0.0122)	(0.0145)	(0.0095)	(0.0095)	(0.0108)
Institution type (omitted=unranked)						
Research university	0.1502*	0.1190	0.1113	0.0873	0.0724	0.0233
	(0.0871)	(0.0878)	(0.0937)	(0.0455)	(0.0479)	(0.0491)
College	0.0902	0.0716	0.0814	0.0115	0.0043	-0.0281
	(0.0605)	(0.0607)	(0.0676)	(0.0413)	(0.0401)	(0.0420)
Institution characteristics						
Large city		0.0703***	0.0661**		0.0472***	0.0403***
		(0.0242)	(0.0258)		(0.0149)	(0.0150)
Medium city		0.0262	0.0220		0.0118	0.0086
		(0.0215)	(0.0218)		(0.0124)	(0.0124)
ln (total enrollment)			-0.0069			0.0113
			(0.0134)			(0.0089)
Undergrad only			-0.0581**			-0.0330*
			(0.0248)			(0.0181)
Private institution			-0.0089			0.0378**
			(0.0278)			(0.0170)
Observations	679	679	679	65,893	65,893	65,893
R squared	0.016	0.028	0.036	0.906	0.906	0.907
Joint significance of 2 rank variables						
F statistic	1.781	1.294	1.426	1.665	1.312	0.444
p-value	0.169	0.275	0.241	0.190	0.270	0.641
Joint significance of university type and rank variables						
F statistic	2.726	1.684	1.285	2.541	2.106	0.888
p-value	0.028	0.152	0.274	0.039	0.079	0.471
Correlation between individual fixed-effects and ln(rankings)						
Universities		-0.22		-0.261	-0.256	-0.255
Colleges		-0.093		-0.156	-0.152	-0.146

Note: See footnotes Table 3

Table C.7: Does endowment increase institution fixed effects? (including wage outliers)

	Two-Step Estimates			One-Step Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
ln (endowment per student)	0.0094*	0.0089*	0.0118*	0.0076**	0.0084**	0.0046
	(0.0049)	(0.0049)	(0.0068)	(0.0033)	(0.0033)	(0.0042)
Institution type (omitted=unranked)						
Research university	0.0560	0.0357	0.0120	-0.0028	-0.0163	-0.0310
	(0.0463)	(0.0466)	(0.0523)	(0.0258)	(0.0257)	(0.0280)
College	0.0082	-0.0002	-0.0118	-0.0247	-0.0356	-0.0405*
	(0.0424)	(0.0423)	(0.0431)	(0.0240)	(0.0237)	(0.0245)
Institution characteristics						
Large city		0.0725***	0.0709***		0.0499***	0.0426***
		(0.0241)	(0.0260)		(0.0148)	(0.0151)
Medium city		0.0286	0.0266		0.0124	0.0094
		(0.0213)	(0.0218)		(0.0125)	(0.0126)
ln (total enrollment)			-0.0038			0.0130
			(0.0132)			(0.0092)
Undergrad only			-0.0519**			-0.0298*
			(0.0244)			(0.0174)
Private institution			-0.0179			0.0321
			(0.0304)			(0.0196)
Observations	679	679	679	65,893	65,893	65,893
R squared	0.016	0.029	0.036	0.906	0.906	0.907

Note: See footnotes Table 3.

C.2 Figures

Figure C·1: Average faculty log salary and institution rankings

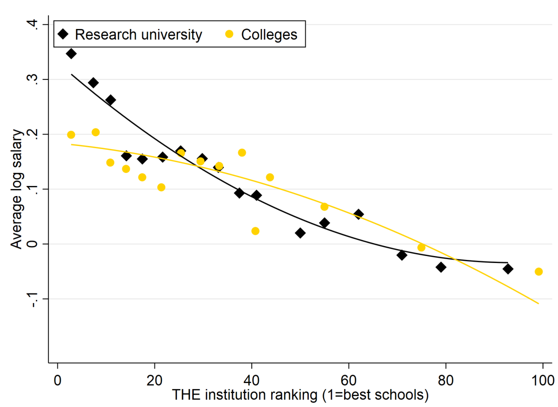
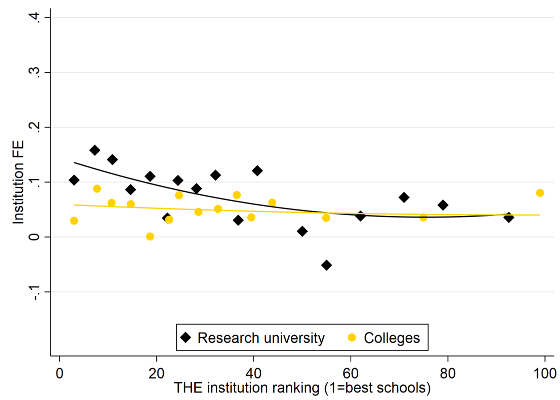
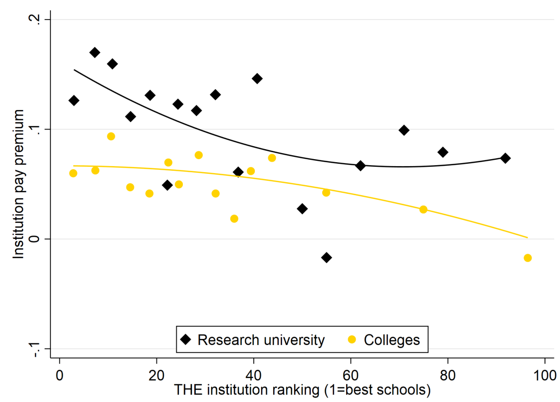


Figure C.2: Institution premium and institution rankings (weighted by number of movers)



Note: the figure weights observations so that each cell has the same number of movers.

Figure C.3: institution premiums and rankings weighted for grouped institutions



Note: the figure shows institution premium estimates for grouped institutions. We group institutions with similar rankings so that each institution "pseudo-institution" has at least five movers.

C.3 A simple example

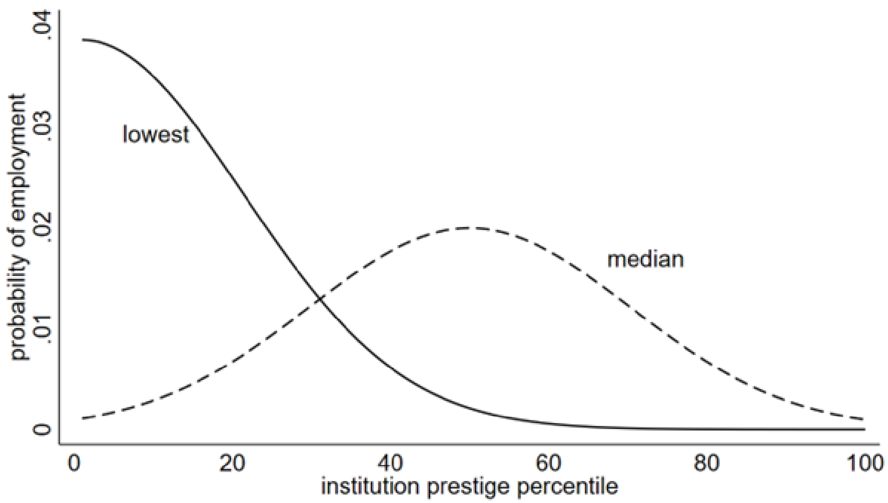
We choose functional forms to generate a realistic example but do not attempt to calibrate the example fully. We have 100 universities with prestige, p, given by {.211, .222, .233, . . . , 1.30}. Similarly, we 100 faculty-quality types with quality, q, given by {.422, .444, .466, . . . , 2.60}. Universities pay a faculty member $\ln w(p, q) = -p2 + pq$. These assumptions ensure that each faculty member maximizes their salary by choosing the university with the prestige rank equal to their quality rank.

We choose these numbers so that if both are perfectly matched, the highest type earns about five times as much as the lowest type but the highest type would earn about 17 times as much as the lowest type if they were both at the most prestigious university but would only earn about two-thirds more if they were both at the least prestigious. The utility the faculty receives from an appointment at a given university is $u = lnw + \eta$ where η is type 1 extreme value with scale parameter .1. Then the probability that a worker of quality, q, is in the job with prestige p' is given by

$$P(p', q) = \exp(10 * \ln w(p', q)/(\sum p(10 * \ln w(p, q)))$$

The AKM model fits the data well in the sense that it explains 99% of the variance. Of course, the example has no idiosyncratic errors, but the ability of the AKM model to fit the data is still striking. Although the university fixed effects are jointly significant, they are relatively unimportant with an uncorrected standard deviation of less than .01. Faculty fixed effects alone explain 83% of the variance. Appendix Figure C·4 shows the distribution of the lowest and median quality faculty. Although the lowest quality faculty is most likely matched with the lowest prestige university, they still have a nontrivial chance of ending up in the third quintile. Similarly, the median quality faculty is mostly likely to be matched with the median prestige university but has a nontrivial chance of being in either the top or bottom quintiles. The 10th percentile faculty (not shown) has a 55% chance of being in a bottom quintile university, 35% in the fourth quintile, and 9% in the fifth quintile.

Figure C·4: Probability of prestige level: lowest and median quality faculty



C.4 Data

In this paper, we combine data from three sources: individual-level data from the restricted-use version of the Survey of Doctorate Recipients (SDR) from the National Center for Science and Engineering Statistics (NCSES); university and college rankings data from the *Times Higher Education* 2017 World University Rankings, the *Wall Street Journal – Times Higher Education* 2017 College Rankings, and the 2021 *US News and World Report* University and college rankings; and university characteristics from Integrated Postsecondary Education Data System (IPEDS) surveys.

Our analysis required three main steps: build a work history panel for tenure-track faculty, construct a dataset with institution characteristics, and associate each school to a unique ranking. Below we detail the main steps we used to build our final dataset.

C.4.1 Building the work history panel

We first combine the information from all the SDR waves available between 1993 and 2017 (inclusive). We restrict the sample to people employed full-time (35 hours/week for at least 40 weeks/year) in a tenure-stream (tenured or tenure-track) position at a US 4-year college or university, medical school attached to a university, or university research institute. We also drop observations where respondents were in a post-doctoral position, earned less than the minimum National Institutes of Health post-doctoral stipend ($53,760 in 2020), or worked outside the US (whether in academia or not). We identify employers using the IPEDS institution code reported by the SDR. We transform all salary figures into 2020 dollars using the yearly CPI for all urban consumers (U.S Bureau of Labor Statistics, 2023). This leaves us with an unbalanced panel tracking the work history of tenure-track faculty across US academic institutions.

Determining faculty moves in the SDR

We pay special attention to ensuring that we track the moves of faculty across academic institutions correctly. The AKM model identifies the pay-premiums out of variation coming from people moving across institutions. Thus, it is crucial that we record moves correctly.

We say an academic changed employer whenever we observe a change in the IPEDS code of the current employer, except when these changes result from a leave of absence or a likely coding error. We identify leaves of absence as *temporary moves* out of a primary or home institution. These are moves satisfying three conditions:

(i) we observe the academic in three *consecutive* SDR waves;

(ii) the academic starts in an institution (home) and moves to a *host* institution for one period;

(iii) to then return to their home institution.

We identify 59 leaves-of-absence in our data. We exclude the host school observation for them, keeping the observations in their home school only.

We also identified and manually corrected moves that were likely the result of a coding error. There were 2,916 observations where the IPEDS university code changed, but the respondent reported not changing institutions. These frequently involved two institutions with similar names. Thus, a Boston University faculty member in multiple waves might be miscoded as Boston College faculty for one wave, while not reporting changing institutions. We manually checked these moves and corrected those we deemed likely mistakes.

Because we are interested in institution-level premiums, we merged IPEDS codes that identify units of the same university. IPEDS divides some large universities across different codes. For example, ASU-Tempe and ASU-Phoenix have different codes even though they belong to the same institution. We did not count these as moves in our dataset, since all are within ASU. Therefore, we assigned all university units to a single code in such cases. (It is possible that we missed some moves in this process but wanted to be conservative in what we considered to be moves.) Whenever we determined university campuses were independent of each other, we kept them as separate IPEDS codes. For example, we keep University of Wisconsin-Madison and University of Wisconsin Oshkosh as separate institutions.

We tried to be as conservative as possible in this process, only combining 40 institution codes into 24 codes. We can provide the list of merged codes upon request.

C.4.2 Salaries

In addition to excluding observations that we determined to be leaves of absence, we excluded salary observations with very large one-time salary changes that were subsequently reversed *within the same institution.* We identify these outliers as follows:

1. First, we computed the growth in the log of salary adjusted for job experience ($\Delta\widetilde{w}_t$):

$$\Delta\widetilde{w}_t = \Delta w_t - \Delta\widehat{w}_t \tag{C.1}$$

   where Δw_t is the log change in the individual salary, and $\Delta\widehat{w}_t$ is the expected change in the log salary due to experience. This expected change comes from a regression of log salaries on years of experience, and years of experience squared:

$$w_t = \alpha_o + \alpha_1 y_t + \alpha_2 y_t^2 + \nu_t$$

   where y_t denote years since Ph.D. Then we define the expected change as:

$$\Delta\widehat{w}_t = \widehat{\alpha}_1 \Delta y_t + \widehat{\alpha}_2 \Delta y_t^2$$

   The expression in C.1 measures how much actual salary growth deviates from what we should expect based on the experience profile alone.

2. We flag a *within-institution* log salary change as a *potential outlier* if, after adjusting for experience, it is larger than 0.4 in absolute value:

$$|\Delta\widetilde{w}_t| = |\Delta w_t - \Delta\widehat{w}_t| > 0.4$$

   We note that 0.4 is a conservative threshold, in the 97$^{\text{th}}$ percentile of adjusted salary growth.

3. We then focus on the *potential outliers* and exclude observations as follows. We drop all observations from people with only two observations in the dataset

and who worked for only one institution. For people having at least three observations and who worked for several institutions, we apply the following procedure:

4. If $|\Delta\widetilde{w}_t| > 0.4$, then either w_t or w_{t-1} may be the outlier. We exclude w_t if its distance from any other salary observation for that person is greater than 0.2[1]. That is,

$$\text{Drop } w_t \text{ if } \min_j \{d_j | d_j = |w_j - w_t|\,,\ j \neq t\} > 0.2$$

5. If $|\Delta\tilde{w}_t| > 0.4$ but its minimum distance is less than 0.2, we apply additional sequential filters (i.e., if an observation survives filter (i) below, then we applied (ii)):

   i. We excluded all observations where the individual's primary work activities were not teaching or research. These people are likely to be in administrative positions[2].

   ii. We excluded all salaries that were out of line with the individual's salary trend. This judgment was made on a case-by-case basis. All these modifications were codified into the do file *code/build_database/outlier_exclusion_list.do*

C.4.3 Building the institution characteristics dataset

All university characteristics other than the rankings are extracted from IPEDS. We use the modules of institution characteristics, fall enrollment, finance, and salaries for the years 1998, 2005, 2012, and 2017. All nominal figures are converted into 2020 dollars using the CPI for all urban consumers. As we say in the paper, we cannot

[1] 0.2 is the 90th percentile of the adjusted wage growth.

[2] In later waves, the SDR asked if the person working in an academic institution was (a) a president, provost or chancellor or (b) a dean, department head or department chair. However, this question was not asked in most SDR waves in our study so we do not use it.

meaningfully add time-varying institution characteristics to our model because they change very slowly, and when they do change, long and uncertain lags in their impact would prevent us from associating salary and institutional changes to salary shifts. Thus, we average all continuous variables across the four survey waves. For all dummy variables, we assign the maximum value across the four years. For example, we classify a university as granting a Ph.D. Degree if it ever granted a Ph.D. Degree during any of the four survey waves.

We extract the following variables from IPEDS:

- **University location:** we classify the university's location into small, medium, and large city. This variable is a recoding of IPEDS' locale variable. Table C.8 details the mapping between both variables.
- **Private university:** dummy equal to one if the university is private.
- **Undergrad-only:** dummy variable equal to one if the institution only offers undergraduate degrees.
- **Total enrollment:** sum of undergraduate and graduate enrollment, averaged over the four survey years.
- **Total faculty:** total faculty size, average of the four survey years.
- **Value of endowment:** IPEDS reports finance information separately for public institutions, private not-for-profit, and private for profit. Our endowment variable corresponds to:
  - **Public universities and private non-profits:** we average the value of endowment assets at the beginning and the end of the fiscal year.
  - **Private for-profits:** we average the value of equity at the beginning and the end of the year.

We use the average of the endowment across the four survey waves.

C.4.4 University rankings

Our primary sources for the institution rankings are the *Times Higher Education* 2017 World University Rankings, the *Wall Street Journal – Times Higher Education* 2017 College Rankings. The *THE* rankings consist of a list of institution names along with their position in the ranking and the state in which they are located. We linked these rankings to a unique IPEDS code using the institution name and location. In most cases, the names in *THE* and IPEDS were similar, and the linkage was straightforward. For the few cases where the linkage was not obvious, we followed the following rules:

1. Whenever names only differed in the word "college" or "university," we use a Google search and the location information to determine if they were the same institution. For example, if the IPEDS label was "Concordia College" and the *THE* ranking name was "Concordia University". We linked both names if and only if:

   a. The institution state is the same in both datasets.

   b. A search for the term "[...] college" gives "[...] university" as the first search result (or vice versa).

1. Different campuses in a university system have different IPEDS codes. Sometimes *THE* provides only one rank for a university system without reference to the campus. In this case, we associated the rank to the flagship campus. For example, the *THE* rank for "Penn State University" was associated to the IPEDS code for "Penn State University, University-Park."

The procedure above was applied to both the *THE* World University and the *WSJ/THE* College rankings. Based on the result of the matching, we classify institutions into three mutually exclusive categories. These categories determine the value of the *institution ranking* variable we use in our regressions.

1. **Research universities:** these are institutions we matched to the *THE* World University Rankings. For these institutions, the value of *institution rank* is their position in the World University Ranking.

1. **Colleges:** there are institutions (i) not matched to the World University Ranking but (ii) matched to the College Ranking. Their *institution rank* is their position in the College Rankings. Note that many institutions in this category are not solely undergraduate institutions.

2. **Unranked universities:** these are institutions we could not match to any of the rankings. We assign a value of zero to their *institution rank*.

We matched 585 (86% of the total) of the 679 institutions to a *THE* rank. Of the remaining 94, we imputed a rank for 59 schools ranked in *USNWR*, using the relation between *USNWR* and *THE* ranks (see below), leaving 35 unranked schools (5% of the total).

C.4.5 Imputing of the THE ranks

The THE rankings are our primary source of university performance information. However, we were unable to match 94 institutions to a THE rank. For 59 of these insitutions, we were able to impute a *THE* rank using U.S. News and World Report (*USNWR*) rankings as follows:

1. First, we merge the *THE* rankings with each of the ten available *US News* ranking lists (national, liberal arts colleges, and regionals). Merging was done

by institution (university or college) name. Names were manually checked to ensure consistency.

2. For universities ranked by both *THE* and *US News* (in any of the six lists), we run an OLS regression of their position in the *THE* list on their position in the *US News* list:

$$THE\_ranking_i = \alpha + \beta US\_news\_ranking_i + \varepsilon_i$$

We run a separate regression for each of the *US News* lists (national, liberal arts colleges, and regionals). Table C.10 shows the results of each of these auxiliary regressions.

3. We infer the position in the *THE* rankings for universities unranked by *THE* but ranked by *US News* using the predicted values of the regression in 2. That is:

$$\widehat{THE}_{\,ranking_i} = \alpha + \widehat{\beta} US\_news\_ranking_i$$

Note that all ten *US News* rankings are mutually exclusive. Therefore, the imputed *THE* position is unique. We treat institutions in the *national US News* ranking as *research universities,* and institutions in all other *US News* rankings (liberal arts colleges, regional universities, and regional colleges) as *colleges.* Table C.11 provides a breakdown of the imputed ranks according to the *US News* ranking list we used for the imputation.

Table C.8: University location classification

1998 IPEDS locale classification		Recoding used	
Codes	**Labels**	**Codes**	**Labels**
1	Large city	1	Large city
2 3, 4	Mid-size city Urban fringe of large / mid-size city	2	Mid size city / suburb
5, 6, 7 9	Large town, small town, rural Not assigned	3	Small city / rural town

2005-2017 IPEDS locale classification		Recoding used	
Codes	**Labels**	**Codes**	**Labels**
11	Large city	1	Large city
12 21, 22, 23	Mid-size city Suburbs	2	Mid-size city / suburbs
13 31 - 43	Small city Towns, rural	3	Small city / rural town

Table C.9: Description of location codes

Location	Description
Large city	Urban area, population above 250k
Mid-size city / suburbs	Urban area, population between 100k and 250k, or suburbs
Small city / rural town	Urban areas with population below 100k, rural areas

Table C.10: Ranking imputation regressions

	National rankings		Regional universities				Regional colleges			
	(1) **National**	**(2)** **Liberal**	**(3)** **North**	**(4)** **South**	**(5)** **Midwest**	**(6)** **West**	**(7)** **North**	**(8)** **South**	**(9)** **Midwest**	**(10)** **West**
US News ranking	1.762	3.115	3.101	2.671	2.883	3.872	3.681	1.715	7.927	7.938
	(0.132)	(0.139)	(0.237)	(0.361)	(0.293)	(0.395)	(1.901)	(0.623)	(1.130)	(5.318)
Constant	82.21	-20.90	326.0	550.3	456.5	439.7	624.2	694.0	382.2	585.5
	(17.665)	(15.120)	(21.710)	(24.234)	(23.468)	(25.014)	(50.416)	(23.008)	(37.954)	(68.844)
r2	0.582	0.771	0.554	0.386	0.477	0.530	0.211	0.296	0.629	0.182
F	179.3	502.0	171.4	54.61	96.79	96.04	3.748	7.571	49.24	2.228
N	131	151	140	89	108	87	16	20	31	12

Notes: The dependent variable in column (1) is the THE research university ranking. The dependent variables for all the columns is the THE college university ranking.

Table C.11: Number of schools imputed by ranking type

Ranking type	**Number of schools**
National rankings	
Universities	10
Liberal arts colleges	13
Regional Universities	
North	6
South	4
West	2
Midwest	8
Regional colleges	
North	2
South	5
West	2
Midwest	2
Total	54

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Fields of Interest

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