Labor Market Dynamics and Development*

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Abstract

We build a dataset of harmonized rotating panel labor force surveys covering 42 countries across a wide range of development and document three new empirical findings on labor market dynamics. First, labor market flows (job-finding rates, employment-exit rates, and job-to-job transition rates) are two to three times higher in the poorest as compared with the richest countries. Second, employment hazards in poorer countries decline more sharply with tenure; much of their high turnover can be attributed to high separation rates among workers with low tenure. Third, wage-tenure profiles are much steeper in poorer countries, despite the fact that wage-experience profiles are flatter. We show that these facts are consistent with theories with endogenous separation, particularly job ladder and learning models. We disaggregate our results and investigate possible driving forces that may explain why separation operates differently in rich and poor countries.

JEL Classification Codes: O1, J6

Keywords: job flows, job-finding rate, separation rate, selection

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1 Introduction

Several recent strands of development economics have converged on poorly functioning labor markets as possible contributors to poverty. In the macro-development literature, the concern is that labor market distortions or barriers may cause workers to be misallocated across regions, industries, or firms, lowering gross domestic product (GDP) per capita.[1] A growing experimental literature has evaluated treatments that alter the search behavior of workers and firms, with the idea that they may improve outcomes such as match quality.[2] Finally, in the development policy literature, there is a belief that poor countries need to "remedy the institutional failures and market imperfections that prevent the private sector from creating more of those good jobs for development."[3]

Despite this widespread interest, there is as yet no consensus on which frictions are more severe in poor country labor markets. The search and matching literature offers a number of theories with candidate frictions, but we lack systematic empirical evidence to evaluate which is the most promising. While recent work has developed estimates of important cross-sectional objects such as hours worked or the unemployment rate, the main limitation is that little is known about labor market dynamics in poor countries (Bick et al., 2018; Feng et al., 2018).

Our first contribution is to provide systematic evidence on how labor market dynamics vary with development. We start by building a new dataset consisting of the microdata from rotating panel labor force surveys from 42 countries.[4] The countries span a broad range of development, with purchasing power parity (PPP-) adjusted GDP per capita ranging from less than \$5,000 (Nicaragua, Palestine, Philippines) to more than \$30,000 (United States, much of Europe). We use the rotating panel structure to match people's responses over time, yielding 67 million observations of people matched across two consecutive quarters.

The original microdata are rich and typically include information on labor force status, demographics, education, employer, and job. We harmonize the data to make them comparable across countries. We take particular care in harmonizing labor force status, which is used to construct empirical counterparts of key concepts such as job seekers and

[1]See Restuccia and Rogerson (2013) for an introduction to the literature and Hsieh and Klenow (2009), Gollin et al. (2014), and Young (2013) for papers that propose and evaluate misallocation of this type.

[2]For example, Abebe et al. (2019) directly subsidize transportation costs for workers who search; Abel et al. (forthcoming) provide workers with reference letters; and several researchers certify workers' skills (Carranza et al., 2019; Abebe et al., 2019; Bassi and Nansamba, 2019). See also McKenzie (2017) for a review of the literature and other related work.

[3]World Bank (2013, p. 35).

[4]We also use this dataset to study the cyclical properties of labor markets across countries (Donovan et al., 2019).

the job-finding rate. Countries do not define labor force status consistently, so we reconstruct a harmonized labor force status by applying a common definition to the original survey responses. We also implement a simplified version of the analysis of Flinn and Heckman (1983) to show that unemployment, inactivity, and self-employment are less distinct in poorer countries, even given consistent definitions. In light of this finding, we discuss several ways to map the model to the data.

We use this dataset to document three novel facts about how labor market dynamics vary with development. First, labor market flows (job-finding rates, employment-exit rates, and job-to-job transition rates) systematically decline with development, with the poorest countries in our sample having transition rates two to three times higher than the richest. This decline is strongest for the employment-exit rate but also sizable for the job-finding rate and the job-to-job transition rate.

Second, while in all countries the employment hazard function is declining with tenure, it declines much more rapidly in poorer countries. Much of the higher employment-exit rate and job-to-job transition rate in poorer countries can be attributed to very high turnover among the many workers with low tenure levels; by contrast, in any country, workers with several years' tenure are unlikely to separate from their job. This result mirrors recent findings on the decline of short job spells and labor market turnover over time in the United States (Mercan, 2017; Pries and Rogerson, 2019).

Finally, we document that the wage-tenure profile is *steeper* in poorer countries, despite the fact that we re-confirm the literature's finding that the wage-experience profile is *flatter* in poorer countries (Lagakos et al., 2018). This third fact is useful for discriminating among potential theories. It is well known that there are two possible underlying determinants of wage-tenure profiles (Topel, 1991). First, they may reflect returns to tenure – for example, through the accumulation of job-specific human capital. However, this seems unlikely to generate our cross-country patterns: it is hard to explain why workers in poor countries accumulate job-specific human capital more rapidly yet are more likely to exit from (apparently valuable) matches. Alternatively, wage-tenure profiles also reflect non-random selection of workers or matches into longer tenure spells. Motivated by this second hypothesis, we view selection via endogenous separation as the most promising mechanism for explaining and interpreting our empirical findings.

The search and matching literature provides two broad candidate theories. We present simplified versions of each and use them to highlight channels that can generate our results. In the *learning model* of Jovanovic (1979), workers and firms receive only a noisy ex ante signal of match quality; additional information is learned ex post through production. A less informative ex ante signal leads workers and firms to sample more marginal matches, generating both higher job-finding and employment-exit rates. More bad matches imply more ex post selection, which generates steeper hazard functions and tenure-wage profiles. In the *job ladder* model of Burdett and Mortensen (1998), workers can receive outside offers from other possible employers. A worse initial match or more rapid arrival rate of outside offers leads to more frequent acceptances of outside offers, generating both higher job-finding and separation rates. Both forces generate more ex post selection in terms of which matches survive to high tenure levels and so steeper hazard functions and wage-tenure profiles.

These models show how selection via endogenous separation provides a mechanism for understanding our findings. They also suggest some driving forces that may explain cross-country variation in labor market dynamics, including the amount that can learned about match quality ex ante, the arrival rate of outside offers, and workers' outside options. These forces are not directly observable. It is possible that there are universal differences across countries in, for example, the amount that can be learned about match quality. However, it is also possible that our aggregated results hide underlying compositional differences. For example, Arcidiacono et al. (2010) document that firms are better informed about more educated workers' ability in the United States, which suggests that low educational attainment in poor countries may be a promising driving force. To study this in more detail, we connect our results to observable characteristics of workers and firms.

We start by disaggregating our results to see whether observable characteristics explain selection. We find that several observable characteristics can help account for our findings but that the basic patterns hold also within narrowly defined groups. We further consider the importance of labor market institutions, motivated by a large literature that shows they are important for understanding patterns among rich countries (Ljungqvist and Sargent, 1998; Krause and Uhlig, 2012; Jung and Kuhn, 2014; Engbom, 2017). Again, we find that some labor market institutions correlate with our patterns, but controlling for institutions does not overturn the relationship between dynamics and development. We also document that many of our patterns hold when comparing poor and rich regions within several countries. We therefore conclude with a discussion of remaining possibilities, highlighting areas where other approaches in the literature are informative as well as those where more data and work are needed.

There are two important caveats to our database and the types of results we can provide. First, the poorest countries, including most of sub-Saharan Africa, generally do not collect rotating panel labor force surveys. Thus, our benchmark results do not cover such countries, although we consider some evidence from alternative sources. Second, about one-fourth of our samples cover only urban areas. We choose to focus only on urban areas for our benchmark results, although if anything our results appear to be stronger in rural areas. Additional data sources on the poorest countries, which are primarily rural, would be valuable, particularly given evidence that some labor market patterns diverge in these countries (Bick et al., 2018; Feng et al., 2018).

In addition to the work cited so far, our paper is closely related to a small literature that considers search and matching models outside the context of rich countries.[5] Three recent papers have extended the search and matching framework to allow for self-employment or informal employment, which we also find to be an important part of cross-country differences (Albrecht et al., 2009; Poschke, 2019; Bobba et al., 2018). Martellini and Menzio (2019) provide a model that accounts for the long-run (non-)trends in job-finding rates in the United States in the face of large presumed gains in matching efficiency. Finally, Rud and Trapeznikova (2018) provide the only other facts on labor market dynamics in poorer countries by looking at flows between self-employment and wage work using lower-frequency data (spanning six months to three years) for six sub-Saharan African countries. To interpret their findings, they develop a dual-economy model with labor market frictions. One goal of our work is to provide a set of systematic facts and outline candidate models that may be useful for expanding this type of analysis.

The structure of our paper is as follows. Section 2 describes the data and our work on harmonization and comparability. Section 3 provides the three new facts about labor market dynamics and development. Section 4 describes two candidate theories from the literature that rationalize our facts. Section 5 explores possible underlying driving forces. Section 6 concludes.

2 Data Description and Harmonization

The empirical results of this paper build on a new harmonized dataset constructed from the microdata of the rotating panel labor force surveys of 42 countries around the world. Our goal was for our dataset to be as comprehensive as possible. We identified the official labor force survey for all countries, meaning the survey used to generate officially reported labor force indicators, such as the unemployment rate. Many countries use or have used a rotating panel design, which includes households for multiple periods. We read documentation

[5]See Rogerson et al. (2005) for a review of the broader body of search and matching theory or Elsby et al. (2013) for recent cross-country work on flows among (relatively developed) OECD countries.

files and searched the literature to identify as many such countries as possible, including countries that have subsequently abandoned the rotating panel design.

We restrict our attention to the subset of countries with rotating panel labor force surveys that satisfy two criteria. First, we require that the country provide the original microdata with consistent identifiers so that we can match respondents over time. This restriction rules out countries that treat the microdata as confidential or that release only anonymized versions without household or individual identifiers. Second, we require that the data allow us to match people for two consecutive quarters. This restriction allows us to focus on using the largest possible comparable subset of surveys, including many countries where households are followed for only two consecutive quarters as well as some more complicated designs.[6] Our final dataset contains microdata from 42 countries. The European Union Labour Force Survey includes 17 countries with usable identifiers. Labor force surveys for the remaining countries are individually collected. See Appendix A.1 for further details as well as details on countries with rotating panel labor force surveys that we could not use.

2.1 Matching and Re-Weighting

Each observation in our dataset is an individual matched across two consecutive quarters. We match individuals in one of two ways. In most countries, we have unique and consistent household and person identifiers. In these countries, we use the pair of identifiers to match individuals over time. We validate the resulting matches by checking that each is unique and that the responses for age and gender are consistent across quarters, in line with standard practice (Madrian and Lefgren, 2000). The share of matches that fail these tests is generally low. In a few countries, we have household but not consistent person identifiers. For these countries, we match on household identifier, age, sex, and education. We keep only observations with unique, exact matches on these three variables.

All of our countries sample dwellings (physical addresses) and interview whoever inhabits those dwellings at the appropriate times. Thus, households that move dwellings between quarters cannot be matched. This fact has the potential to bias our estimates to the extent that moving (or other forms of non-response) is correlated with outcomes of interest such as finding a job. We follow the literature's typical approach of adjusting the provided sample weights so that the matched and unmatched samples have similar observable characteristics (Bleakley et al., 1999; Fujita and Ramey, 2009). We focus on education, labor force status,

[6] For example, some European countries include households for six consecutive months; the United States includes households for two four-month spells. Each allows us to create a quarterly (three-month) match.

Table 1: Sample Overview

Country	Years	Obs. (thousands)	GDP p.c. range
Albania	2012 - 2013	37	10,400 - 10,500
Argentina	2003 - 2019	794	13,400 - 19,800
Bolivia	2015 - 2018	247	6,400 - 7,000
Brazil	2002 - 2019	8,260	11,600 - 15,500
Chile	2010 - 2019	2,084	19,400 - 22,900
Costa Rica	2010 - 2019	393	12,900 - 15,700
Croatia	2007 - 2018	73	20,300 - 23,600
Cyprus	2005 - 2018	202	29,900 - 36,000
Czech Republic	2005 - 2010	532	25,800 - 29,400
Denmark	2007 - 2018	228	43,400 - 47,700
Dominican Republic	2016 - 2017	52	14,500 - 15,000
Ecuador	2007 - 2019	325	8,800 - 10,900
Egypt, Arab Rep.	2008 - 2012	205	9,500 - 10,000
Estonia	2005 - 2018	70	22,200 - 31,000
France	2003 - 2017	3,070	35,300 - 39,000
Georgia	2009 - 2016	141	6,500 - 9,300
Greece	2010 - 2018	864	23,700 - 28,700
Guyana	2017 - 2018	9	7,400 - 7,600
Hungary	2005 - 2018	1,363	22,200 - 28,200
Iceland	2005 - 2018	50	40,100 - 48,600
India	2017 - 2018	190	6,500 - 6,900
Ireland	2007 - 2018	665	42,900 - 70,400
Italy	2005 - 2018	1,669	33,900 - 38,600
Latvia	2007 - 2018	71	18,300 - 26,400
Lithuania	2005 - 2018	178	18,500 - 31,100
Malta	2009 - 2018	47	27,500 - 38,100
Mexico	1995 - 2019	17,253	13,500 - 18,100
Nicaragua	2009 - 2012	194	3,900 - 4,400
Palestine	2000 - 2015	558	2,800 - 4,600
Paraguay	2010 - 2017	45	9,700 - 11,800
Peru	2003 - 2018	248	6,900 - 12,800
Philippines	1988 - 2003	1,989	3,800 - 4,400
Poland	2005 - 2018	709	17,200 - 28,800
Romania	2005 - 2018	775	14,400 - 24,500
Slovak Republic	2005 - 2018	525	20,000 - 31,300
Slovenia	2010 - 2018	107	27,600 - 32,700
South Africa	2008 - 2018	1,228	11,800 - 12,400
Spain	2000 - 2018	6,858	30,000 - 35,100
Sweden	2005 - 2018	1,389	40,900 - 47,200
Switzerland	2010 - 2017	373	55,900 - 58,000
United Kingdom	1997 - 2017	3,591	30,300 - 39,900
United States	1979 - 2019	9,130	36,300 - 55,700
Total:			
42 countries	515 country-years	66,791	2,800 - 70,400

[a] *Table notes:* Range of PPP GDP per capita World Bank (2019), rounded to the nearest $100. An observation is an individual surveyed in two consecutive quarters.

age, and gender as the most important dimensions. See Appendix A.3 for details. The adjusted weights are generally similar to the provided weights, measured using the correlation between the two (Table A4) or the fact that standard moments are fairly similar regardless of which weight is used (Figure A1).

We de-seasonalize the quarterly data and aggregate to the country-year level; for the rest of the paper, we treat a country-year as an observation. We focus throughout on the urban population aged 16–65. Our main results of interest are even stronger when we focus on the three-quarters of samples that include both urban and rural areas; see Appendix A.5.[7] We focus on workers aged 16–65 to mitigate concerns about cross-country differences in labor market institutions such as child labor laws or retirement policies. In addition, some countries collect very limited data on people over age 65.

Table 1 identifies the countries that are covered and basic summary information. Altogether, we have about 67 million observations spanning 515 country-years. The duration of data availability varies widely, ranging from six quarters of the newly formed Guyana Labour Force Survey to 41 years in the United States. We merge our data with annual PPP GDP per capita from the World Development Indicators when discussing development trends (World Bank, 2019). Our countries cover a wide range of development, although wee lack data on the very poorest countries, where the cost of such panel surveys is generally prohibitive. We can infer dynamics from retrospective questions on employment history for a few such countries but are not able to form reliable estimates of the patterns there; see Appendix C for further details.

2.2 Comparing Labor Force Status across Countries

An essential ingredient of our paper is comparing labor force statuses and rates of transition between labor force statuses across countries. We take two steps to ensure that these comparisons are meaningful. First, we re-construct labor force status, using a standardized definition applied to the original microdata for all countries and years. This step is necessary because the provided labor force status variable is constructed using a definition that varies somewhat across countries and time.[8]

We first categorize people as employed or not employed. Formally, the employed are those who are engaged in the production of goods and services that fall inside the production

[7]See also Jeong (2019) for RCT-level evidence of frictional labor markets in rural Tanzanian village economies.

[8]Hussmanns (2007) reviews the ideal definitions and some of the conceptual and practical issues that arise and lead countries to deviate. The most important deviation is that many countries do not require workers to meet the "search" criteria to be counted as unemployed.

boundary as defined by the System of National Accounts (Hussmanns, 2007). They include those who work for someone else (wage and salary workers) and the self-employed, which in turn includes employers, own-account workers, and unpaid family workers. We follow the United States' convention of requiring at least 15 hours of unpaid family work to be counted as employed, in order to minimize concern that we might artificially inflate "flows" between employment and non-employment among such workers in poor countries. Most surveys in poorer countries include a battery of questions designed to ensure that they capture people who are engaged in self-employment inside the production boundary. For example, it is typical to have separate questions about whether the respondent raises crops or livestock for his or her own consumption, operates a small business, or produces small handcrafts, rather than a single question asking whether he or she is self-employed.

Those who are not employed are categorized either as unemployed or inactive (out of the labor force). We define unemployment consistently as people who are not employed but who satisfy the standard three-part test: i) they want a job; ii) they have actively searched for a job in the last four weeks; and iii) they are available to start a job.[9] People who fail any of these three questions are labeled inactive.

The first step ensures that labor force status is consistently defined across countries. The second step considers the mapping between data and theory in the cross-country context. The heart of the search and matching literature is the matching function, which governs the number of matches formed as a function of the number of job seekers and vacancies. The convention in most work that takes this function to the data is to equate job seekers with the unemployed, at least as a first pass.[10] This mapping may not be appropriate in our context, which includes much poorer countries.

Two specific concerns have received attention in the literature. First, the search criterion for unemployment may be less salient in poorer countries, where workers may either know or easily be able to learn about the relevant set of job opportunities without much active search (Hussmanns, 2007). In this case, some people whom our methodology classifies as inactive may actually be job seekers. Second, recent work has suggested that self-employment in poor countries acts in part as a substitute for missing unemployment insurance, allowing workers to earn some income while searching for better work (Albrecht et al., 2009; Schoar, 2010; Poschke, 2013, 2019). Both these concerns motivate us to re-examine who should be included in the set of job seekers.

[9] The exact search period varies slightly but is generally four weeks, 30 days, or a month. India is the only outlier; it asks about search over the last week.

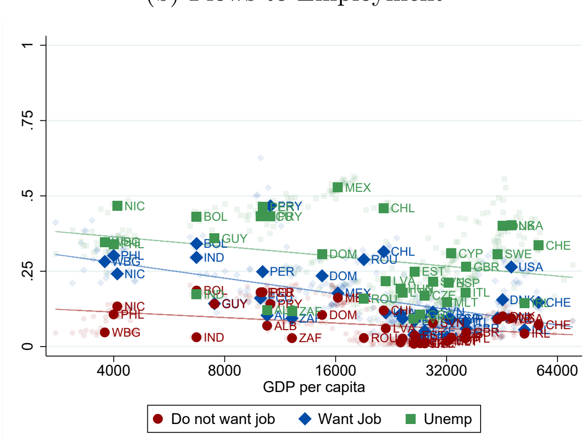
[10] There are exceptions; Elsby et al. (2015) show that cyclical variation in labor market outcomes such as the unemployment rate is affected by movements in and out of the labor force.

We conduct a test of whether unemployment and inactivity are distinct, in the spirit of Flinn and Heckman (1983). They propose that the two statuses are distinct to the extent that they have different job-finding hazards. Conversely, if people who have been unemployed or inactive for the same length of time are equally likely to find work, then there is no meaningful behavioral difference between the two statuses. Although our data do not allow us to construct the entire job-finding hazard, we can construct the relative quarterly job-finding rates. In Figure 1a we plot the relative job-finding rate of the unemployed as compared to the inactive against GDP per capita.

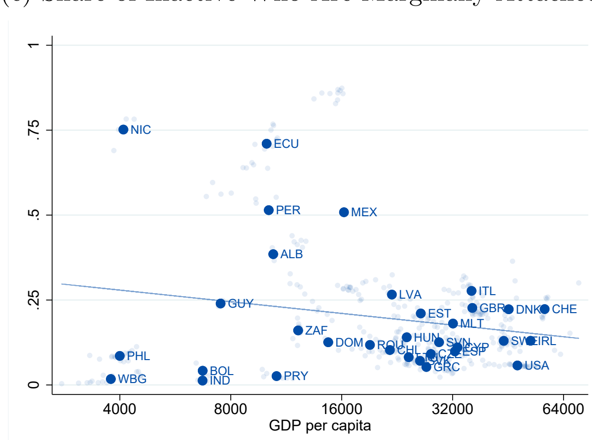
Figure 1: Inactivity and Development

(a) Relative Job-Finding Rate (Unemployed/Inactive)

(b) Flows to Employment



(c) Share of Inactive Who Are Marginally Attached



This figure adopts the common format we use throughout the paper, so some explanation is in order. First, we always plot outcomes of interest against PPP GDP per capita. Each observation is the average for a country-year constructed as discussed above. We also compute and plot the cross-year average for each country, which we label with three-digit country codes. Given the clustering by country, this label often helps distinguish within-country patterns from those between countries. Finally, we include in all scatter plots a best-fit line of a regression of the data points against log PPP GDP per capita.

Figure 1a shows two main results. The unemployed are more likely than the inactive to move to employment in all countries and years. However, there is a strong positive trend with development. In the poorest countries, the unemployed are only twice as likely to find a job; in the richest countries, the proportion grows to a factor of around 4–12.

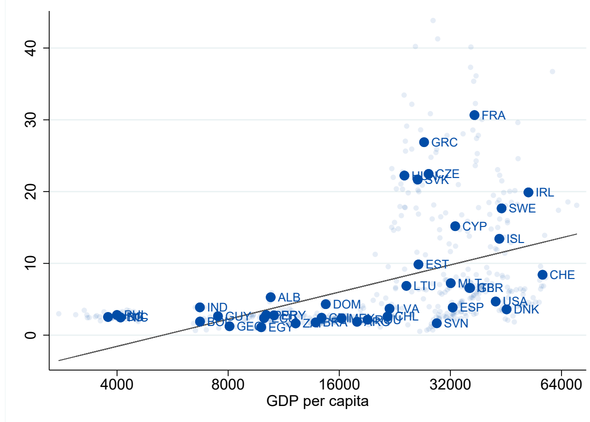
We use the microdata to investigate why so many workers in poor countries transition between inactivity and employment. Figure 1b unpacks the job-finding rate of inactive workers coded based on self-reported reason for not seeking work. We code workers who report being unable to find suitable work (wrong skills, too young or old, no work currently available, etc.) as marginally attached, while those who are unable to work or uninterested in work (sick, disabled, in school, retired, caring for the household or family) are coded as no attachment.[11] As expected, in all countries, the marginally attached are more likely to move to employment. However, the correlation of job-finding rates with development is weak for each of the groups. Figure 1c shows that large cross-country variation in the fraction of people who are marginally attached to the labor force accounts for much of our findings. While as much as 75 percent of the inactive in poor countries are marginally attached to the labor force, a much lower share of workers in rich countries are. These results indicate that unemployment and inactivity are less distinct in poor countries.

We conduct a similar analysis for the self-employed. In this case, we compare the rates of transition to wage work from unemployment versus self-employment. The results are shown in Figure 2. In most rich countries, the ratio is high, meaning that it is relatively rare for the self-employed to transition to wage work. On the other hand, the self-employed are only half as likely as the unemployed to find wage work in poorer countries. This result is consistent with the cross-country literature that asks workers why they are self-employed. The finding there is that the self-employed in poorer countries are less likely to report that they had an idea for a business or wanted to be their own boss and more likely to report that they could not find other work (Schoar, 2010; Poschke, 2013).

These findings suggest that the set of job seekers is broader than the pool of unemployed

[11] For a subset of countries, we can utilize instead a direct question about whether the respondent "wants to work"; similar results apply.

Figure 2: Relative Wage-Work Finding Rate (Unemployed/Self-Employed)



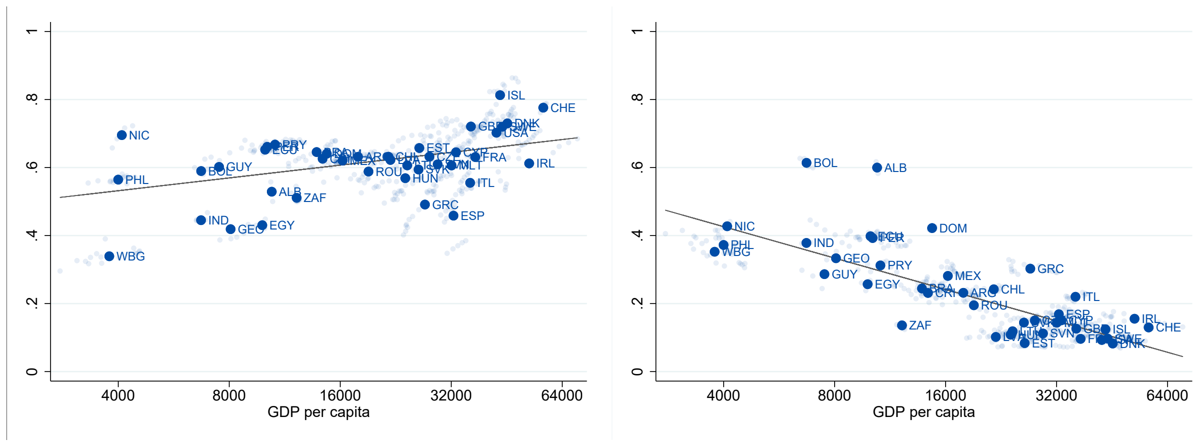
people in poorer countries. Our baseline approach for the remainder of the paper is to pool workers into employed and non-employed and to study flows between them: the job-finding rate and the employment-exit rate. Doing so acknowledges that unemployment and inactivity are less distinct in poorer countries. It also alleviates the most important form of *classification error*, which results when workers misreport their labor force status. Abowd and Zellner (1985) and Poterba and Summers (1986) draw on re-interview surveys from the Current Population Survey to document that by far the most common problem is misclassification between unemployment and inactivity. By aggregating the two, we eliminate the concern that we are imputing spurious labor market transitions and hence inflating estimates of labor market flows. Following on our second results, we keep track of two different types of transitions within employment: between wage jobs (the job-to-job transition rate) and from self-employment to wage work.

2.3 Basic Cross-Sectional Facts

Given these conventions, it is useful for context to check cross-sectional facts about the stocks of workers. Figure 3 plots against GDP per capita the employment-to-population ratio and self-employment as a share of total employment. We find an upward trend for the employment-to-population ratio. This is in line with results from Bick et al. (2018) after conditioning on similar countries (we are missing the very poorest countries, which they find have higher ratios). The self-employment share is strongly negative correlated with development, with self-employment accounting for half of employment in our poorest countries, in line with existing work (Gollin, 2008).

Figure 3: Cross-Sectional Labor Force Facts

(a) Employment-to-Population Ratio (b) Self-Employment as a Share of Employment



3 Labor Market Dynamics and Development

In this section, we document three new findings about how labor market dynamics vary with development. We show that flows are higher in poorer countries; high turnover in poor countries is concentrated among workers with low tenure levels; and wage-tenure profiles are steeper in poorer countries even though wage-experience profiles are flatter. We discuss theories of these findings in Section 4, and in Section 5, we use the microdata to disaggregate them and look for possible driving forces.

3.1 Labor Market Flows

We start by documenting how four labor market flows vary with development. We use reported changes in labor force status to measure three of the four labor market flows. The job-finding rate is constructed as the share of initially non-employed workers who transition to employment in the subsequent quarter.[12] The employment-exit rate is constructed as the share of initially employed workers who transition to non-employment in the subsequent quarter. We also construct as one form of job transition the share of initially self-employed workers who transition to wage work.

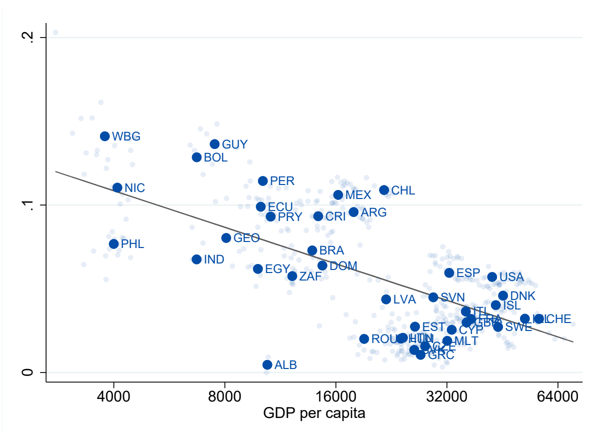
The fourth labor market flow – the job-to-job transition rate among wage workers – is measured differently, using reported tenure on the job. For the set of countries where

[12]We generally do not observe and abstract from workers who have transitions within the quarter. Standard corrections to produce the implied hazards assume that hazard rates are constant over the intervening quarter and hence do not affect the relative trends by development that we focus on (Shimer, 2012).

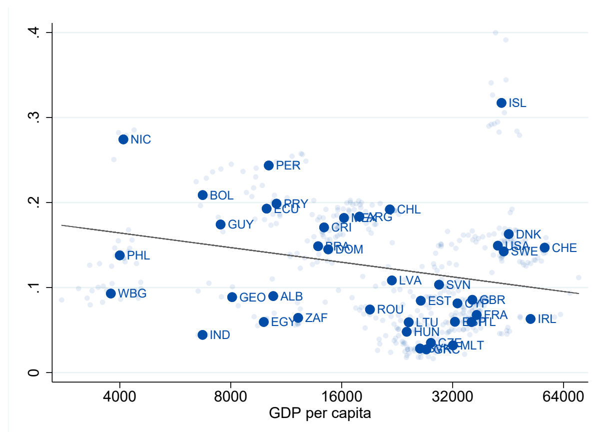
short tenure spells are reported in weeks or months, we define the job-to-job transition rate as the share of initially employed wage workers who remain employed at wage work with tenure less than three months in the subsequent quarter.[13] Tenure is available only for wage workers, and we use it to define only the wage job to wage job transition rate, which we simply call the job-to-job transition rate.

Figure 4: Quarterly Transition Rates

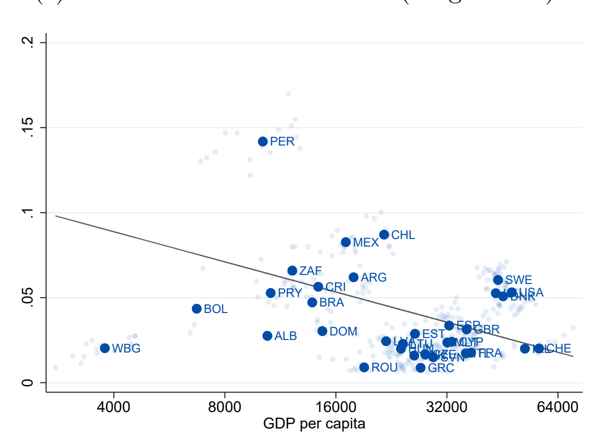
(a) Employment-Exit Rate



(b) Job-Finding Rate



(c) Job-to-Job Transition Rate (Wage Work)



(d) Self-Employment to Wage Job Transition Rate

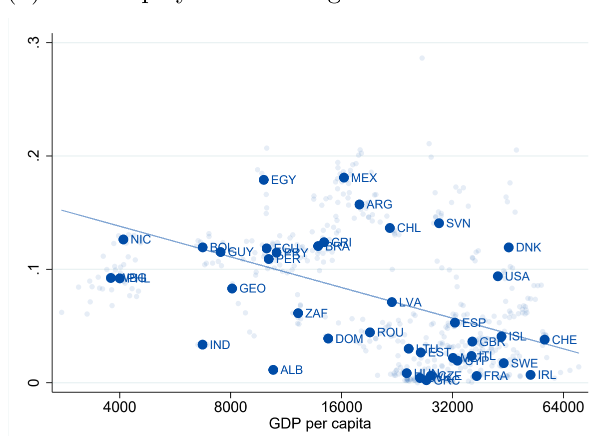


Figure 4 plots the transition rates against development. The main finding is that all four transition rates decline with development. The effect is economically significant, with transition rates two to three times higher in the poorest as compared with the richest

[13]The U.S. CPS data are an outlier. We use the dependent coding available since 1994 to classify job-to-job transitions as workers who work for wages in months 1 and 4, but report changing employer during months 2–4, following Fallick and Fleischman (2004).

countries. Panel A of Table 2 shows the results for the regressions underlying the trend lines in Figure 4. All of the point estimates are negative and statistically significant at conventional levels. The reported R^2s confirm the visual impression that the fit is tightest for the employment-exit rate.

Table 2: Labor Market Flows and Development

Panel A: All countries	Exit Rate	JFR	S.E. - Wage	Job-Job
Log GDP per capita	-0.035***	-0.017***	-0.033***	-0.012***
	(0.002)	(0.004)	(0.003)	(0.002)
Observations	486	486	486	409
R-squared	0.460	0.029	0.173	0.061
Sample Average	0.057	0.120	0.071	0.040
Panel B: Rich countries	Exit Rate	JFR	S.E. - Wage	Job-Job
Log GDP per capita	0.019***	0.105***	0.015	0.034***
	(0.003)	(0.012)	(0.009)	(0.003)
Observations	286	286	286	271
R-squared	0.098	0.207	0.009	0.366
Sample Average	0.035	0.098	0.044	0.030

Table Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 'S.E. - Wage" are all flows from self-employment to wage work. "Job-Job" is all wage work to new wage work jobs.

Our findings are somewhat at odds with the conventional wisdom in the existing literature on cross-country comparisons of labor market flows. That literature focuses primarily on developed countries. Within this set of countries, the typical finding is that richer countries have higher transition rates: the United States, United Kingdom, and northern Europe have higher transition rates than central and southern Europe. This finding applies also in our sample. Panel B of Table 2 provides the regression estimates from a sample that includes only EU countries, Switzerland, the U.K., and the United States. For this sample, we also find a positive relationship between labor market flows and development. These countries span a fairly narrow range of development, suggesting caution is required when attempting to extrapolate trends related to development from analyses conducted among rich countries.

These results hold for our preferred approach of aggregating employment and non-employment. However, we show in Appendix B.1 that similar findings apply for alternative approaches. Intuitively, this is because each of the four original labor force statuses is more persistent in richer countries (Figure B1) and most of the possible flows among these statuses are less frequent in richer countries (Figure B2). Hence, we find also that flows are negatively correlated with development if we take the conventional approach of equating job seekers with the unemployed and disregarding the inactive, if we include the marginally attached but not the non-marginally attached inactive workers in the pool of job seekers, or if we include the self-employed in the pool of job seekers. We conclude that the finding that flows are negatively correlated with development is robust. This finding by itself invites many possible explanations. We now provide two additional facts on cross-country labor market dynamics that can help discriminate among explanations.

3.2 Employment Hazard Functions

Our second fact involves the cross-country comparison of employment hazard functions. We find that much of the correlation between labor market flows and development is driven by high hazard rates for workers with low levels of job tenure. In essentially all countries, exit rates are low for workers with high levels of job tenure. This finding is consistent with recent work that documents that much of the decline in turnover over time in the United States is accounted for by a reduction in very short employment spells (Mercan, 2017; Pries and Rogerson, 2019).

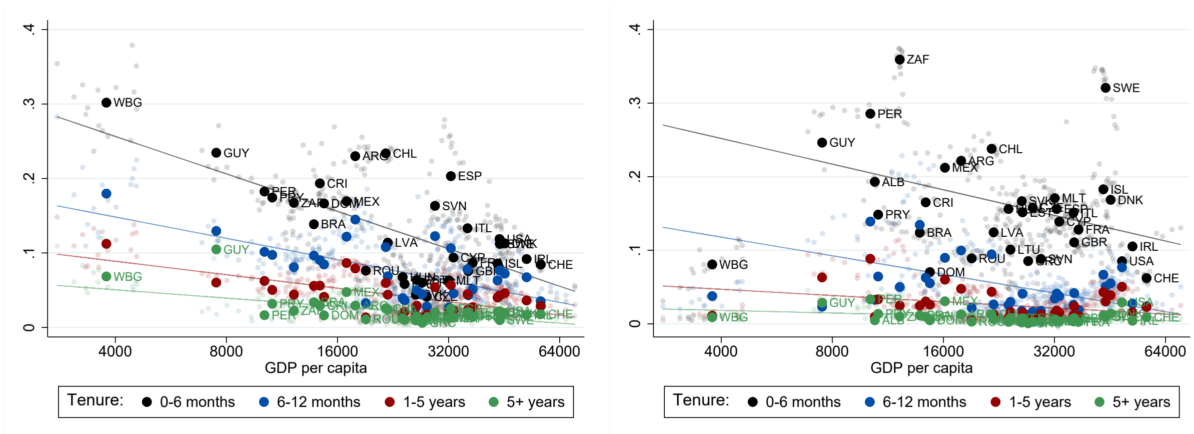
To show this point, in all countries for which we have the data, we construct as a function of initial tenure the probability that a worker transitions to non-employment or to a new job. Tenure is available only for wage workers, so the facts in this and the next subsection apply only to this population. We group wage workers into four tenure bins for visual clarity: those on the job for less than six months, six to twelve months, one to five years, and five years or more.

Figure 5a shows the results for transitions to non-employment, and Figure 5b shows the results for job-to-job transitions. In both cases, there is an important role for tenure. There is a strong trend in exit rates and job-to-job transition rates for workers who have been on the job for less than a year. By contrast, there is essentially no trend for workers who have five or more years of tenure; such workers in any country are unlikely to switch jobs or exit employment.

These results are quantitatively significant. If we counterfactually endow all countries with the world average distribution of tenure duration for wage workers, then the estimated effect of development on employment-exit rates and job-to-job transition rates falls by 36–40 percent. This figure turns out to be much larger than any other observable characteristic

Figure 5: Transition Rates by Job Tenure

(a) Transition to Non-Employment (b) Transition to New Job



we explore, as we will show in Section 5.1. Of course, tenure is an endogenous feature of the match. However, these results suggest that theories of flows that prioritize a meaningful role for tenure are likely to be useful. In the next section, we provide a third fact that further supports this view.

3.3 Wage-Tenure Profiles

Our last fact concerns cross-country variation in the shape of the wage-tenure profile. To estimate this, we use reported labor earnings to construct an hourly wage. The original questions vary somewhat by country, but this is most commonly constructed by dividing monthly labor earnings by 4.33 times the hours worked in the reference week. Since our interest in tenure leads us to focus anyway on wage workers, we disregard some cases where we have reported income for the self-employed.

We estimate an augmented Mincer wage equation motivated by Topel (1991) and Lagakos et al. (2018). We pool all available years for a country and regress:

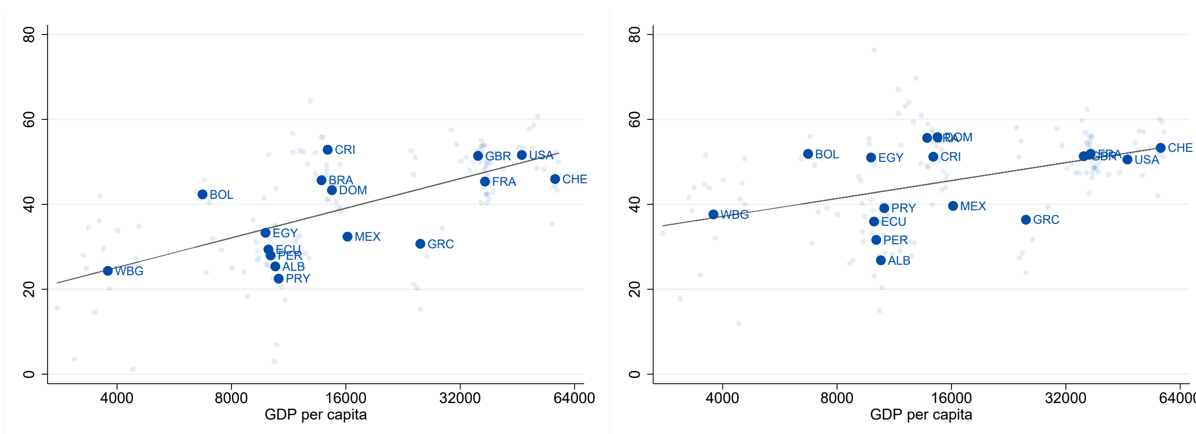
$$\log(w_{it}) = \alpha + \phi_x + \xi_\tau + \rho_{edu} + \gamma_t + \varepsilon_{it}. \tag{1}$$

w_{it} is the hourly wage of individual i observed at time t. The vector ϕ_x consists of dummies for potential experience groups {2–4 years, 5–9 years, 10–19 years, 20+ years}, with 0–1 years of potential experience serving as the omitted reference group. Potential experience is constructed as age minus expected years of schooling minus six. The vector ξ_τ consists of dummies for tenure group {6–12 months, 1–5 years, 5+ years}, with 0–6 months of tenure serving as the omitted reference group. The vector ρ_{edu} is a set of dummies for education (harmonized to the categories of Barro and Lee (2013)), and γ_t is a vector of year dummies. ε_{it} is a mean-zero error term.

We start with the wage-experience profile. Previous work has that it is flatter in poorer countries (Lagakos et al., 2018). Figure 6 plots against PPP GDP per capita the estimated percentage wage difference between workers with 10–19 or 20 or more years of experience, each compared to workers with 0–1 years of experience. We obtain the same result as previous work: the returns to experience rise with development. This finding holds independently of whether we control for tenure.

Figure 6: Wage-Experience Profiles

(a) Wage Difference, 10–19 Years Experience (b) Wage Difference, 20+ Years Experience

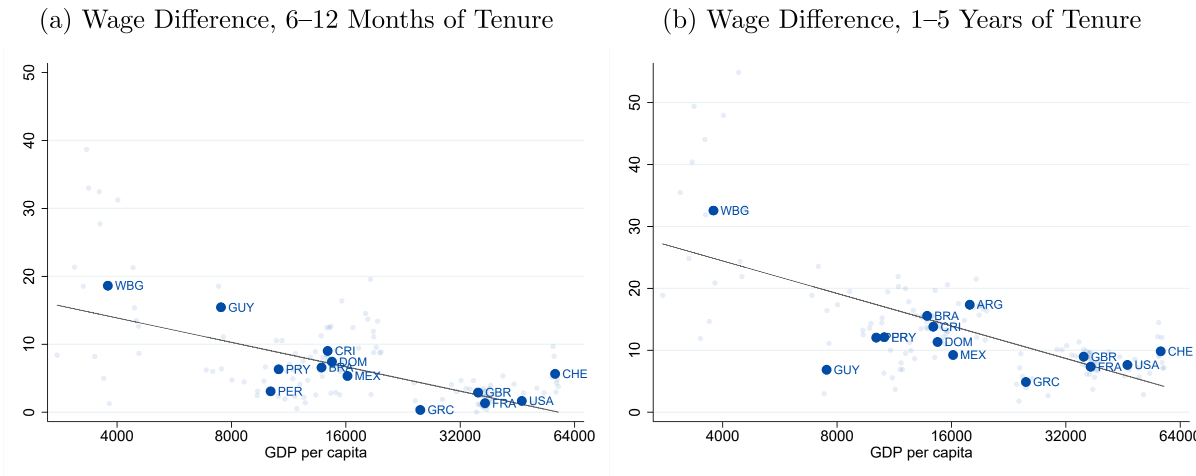


We now turn to the estimated wage-tenure profiles shown in Figure 7. We plot against PPP GDP per capita the estimated percentage wage difference between workers with 6–12 months or 1–5 years of tenure, each compared to workers with less than six months of tenure. The striking finding is that wage differences for highly tenured workers are *higher* in poorer countries, implying that the wage-tenure profile is *steeper*.[14]

It is well known that there are two determinants of wage-tenure profiles (Topel, 1991). They reflect the accumulation of job-specific human capital. However, this factor seems unlikely to generate our cross-country patterns: it is hard to explain why workers in poor countries accumulate job-specific human capital more rapidly yet are more likely to exit

[14]In the Appendix, we provide various alternative bins for tenure, along with different controls in the regression. The results are robust.

Figure 7: Wage-Tenure Profiles



from (apparently valuable) matches and have flatter wage-experience profiles. Wage-tenure profiles also reflect the selection of which workers or matches survive to longer tenure spells. This factor offers a more promising explanation of our cross-country patterns: it requires that workers with high job tenure in poor countries be more selected on wages or productivity, which seems plausible in light of the finding that hazard rates are much higher at low levels of job tenure in poor countries. In the next section, we outline two theories from the literature that formalize this mechanism.

4 Theory

Two classes of search and matching theories feature endogenous separation and hence selection of which matches survive to high levels of tenure. Each is capable of generating our three key facts: higher flows, steeper tenure hazards, and steeper tenure-wage profiles in poorer countries. In learning models, workers and firms are imperfectly informed about match productivity but learn more by producing (Jovanovic, 1979, 1984; Menzio and Shi, 2011). If they learn that a match is unproductive, they endogenously (jointly) choose to separate. In job ladder models, workers receive outside employment offers (Burdett and Mortensen, 1998). Attractive offers induce workers to quit their current job and move. The key insight from these models is that endogenous selection of high-productivity or high-wage matches to longer tenures is an avenue for understanding our findings.[15]

[15] By contrast, the textbook search and matching model has little to say about tenure (Pissarides, 1985). Nonetheless, in Appendix E we analyze that model for completeness. It can generate patterns consistent

4.1 Learning

Our version of the learning model draws on Menzio and Shi (2011), although the predictions of interest also hold in the original model of Jovanovic (1979). We consider a match between an unemployed person and a vacancy generated by the standard matching function. Upon meeting, the pair draw a match-specific productivity x from distribution $F(x)$ that has mean μ. However, they do not necessarily know this productivity. Instead, the worker and the firm draw a signal s that is equal to x with probability p and is an independent draw from F with probability $1-p$. In the limit case $p=1$, matches are said to be *inspection goods*, whose quality can be perfectly determined in advance. In the limit case $p=0$, matches are said to be *experience goods*, whose quality can be learned only through production.

The worker and the firm are both risk neutral, and they have outside options b and 0, respectively. They first decide whether to engage in production. If they do so, they produce x. We assume that they produce if joint expected surplus exceeds the combined outside option b. Each period of production reveals true match quality with probability λ. Matches that are revealed to have negative surplus ($x<b$) are endogenously destroyed. Matches are also exogenously destroyed with probability δ.

This simple model makes predictions about labor market flows, employment hazards, and wage-tenure profiles. We derive each in turn.

Labor Market Flows First, the model allows for the possibility that some matches (with sufficiently low match-quality signals) endogenously do not produce, which in turn affects the job-finding rate. In the model, the share of matches that generate expected surplus above the outside option and so lead to production is given by

$$1-F\left(\frac{b-(1-p)\mu}{p}\right). \tag{2}$$

If we assume that $\mu>b$ (the average match generates surplus exceeding the outside option), then the share of matches that lead to production and thus the job-finding rate is decreasing in both b and p. For example, a share $1-F(b)$ of matches leads to production in the inspection-good case, but all matches lead to production in the experience-good case.

The model also generates predictions about the employment-exit rate. Define the share of matches that engage in production despite having (unobserved) match productivity below

with our findings on labor market flows only if we allow for cross-country variation in unobserved parameters (the cost of posting a vacancy or the productivity of the matching function).

the reservation level as

$$\nu \equiv (1-p)F(b) + p\left[F(b) - F\left(\frac{b-(1-p)\mu}{p}\right)\right].$$

The two terms capture type-1 and type-2 errors, respectively: the probability of using an inaccurate signal from a bad match, plus the probability of failing to reject a marginally bad match because of signal imprecision.

This is the share of matches that will be endogenously destroyed after receiving the λ shock. It varies with p and b, which implies that the model can generate variation in the employment-exit rate. The share is decreasing in p; it ranges from $F(b)$ in the experience-good case to 0 in the inspection-good case.

Employment Hazard The model generates a hazard function given by

$$d_\tau = \delta + (1-\lambda)^{\tau-1}\lambda\nu. \tag{3}$$

The function is strictly declining in tenure if $\lambda > 0$ and $p < 1$. The model can generate variation in the shape of the hazard function either through the arrival of the learning shock λ or through ν, which in turn depends on p and b.

Wage-Tenure Profile Finally, the model generates a wage-tenure profile. To derive this prediction, we need to specify a wage-setting rule, taking care to ensure that it is consistent with our assumption that all matches with expected match quality above b lead to production. One analytically convenient wage rule that satisfies this property is to assume that workers and firms equally split the surplus in each period. Surplus depends on match quality if it is known and expected match quality if not. Then, average wages for matches with known and unknown quality are given by

$$\begin{aligned} w^k &= \frac{\mathbb{E}(x|x>b)}{2} - \frac{b}{2} \\ w^u &= \frac{\left[p\mathbb{E}\left(x|x>\frac{b-(1-p)\mu}{p}\right) + (1-p)\mu\right]}{2} - \frac{b}{2}. \end{aligned}$$

Note that $w^k \geq w^u$ unless matches are inspection goods, in which case match quality is known ex ante and wages never change.

With this notation, we can characterize the wage-tenure profile. The average log-wage of workers with tenure τ relative to new workers with no tenure is given by

$$\log(w_\tau) - \log(w_0) = \log\left[(1-\lambda)^\tau + (1-(1-\lambda)^\tau)\frac{w^k}{w^u}\right]. \quad (4)$$

The wage-tenure profile is increasing and concave if $0 < \lambda < 1$ and $p < 1$, but it is again flat for the inspection-good case. The wage-tenure profile is steeper if p is lower.

Cross-Country Implications The learning model can help understand cross-country differences in labor market dynamics if learning and ex post selection are correlated with development. For example, it can account for our findings if p is higher in richer countries, meaning that rich countries are closer to the inspection-good case and poor countries closer to the experience-good case. Along the same lines, it can account for our findings if λ is higher in poor countries. Finally, it accounts for several of the findings if b is lower in poorer countries, because this induces workers and firms to engage in more marginal matches, which are more likely to be revealed to be unproductive.In Section 5, we consider evidence for these hypotheses in the data and the literature. First, we show that similar predictions arise in a job ladder model.

4.2 Job Ladder

Our job ladder model draws on Ridder and van den Berg (2003). We assume that there are two discrete types of jobs: low-wage jobs and high-wage jobs, paying $w_L < w_H$, respectively.[16] The supply of vacancies of each type is exogenous and fixed, with π denoting the share of low-wage vacancies. The non-employed have an outside option b drawn from a distribution with cdf B and support $[\underline{b}, \overline{b}]$ satisfying $\underline{b} \leq w_L \leq \overline{b} < w_H$.

Non-employed people who match with a vacancy decide whether to accept the exogenous wage offer. After production in each period, matches are subject to two shocks. First, they can be exogenously destroyed with probability δ. Second, workers can receive outside offers. These offers generate endogenous separation in a manner similar to learning in the previous model, so we use the notation λ to denote the probability of receiving an outside offer to emphasize the commonality. Workers who receive an offer from a higher-paying job switch. Workers who receive an offer from a job that pays the same as their existing job are indifferent and are assumed to remain with their current employer. Given this simple setup,

[16] Burdett and Mortensen (1998) show how to get wage heterogeneity in equilibrium even with ex ante identical firms and workers. Ridder and van den Berg (2003) get similar results in a model with a continuum of exogenous firm types; we focus on two types for simplicity.

workers switch jobs if they can ascend one rung up the job ladder, from the low-paying to the high-paying job.

This simple model also makes predictions about labor market flows, employment hazards, and wage-tenure profiles, which we again derive.

Labor Market Flows First, the model again allows for the possibility that some matches (between low-wage firms and high-opportunity cost workers) do not lead to production, which affects the job finding rate. This share of matches is given by $\pi B(w_L) + 1 - \pi$, which is increasing in the share of workers with low outside options, $B(w_L)$.

The model also generates predictions about the job-to-job transition rate, which depends on the rate at which outside offers are received λ and the share of workers with low-wage jobs in equilibrium. In steady state, the share of matches with tenure τ that pay low wages is given by

$$\ell_\tau = \frac{[1-\lambda(1-\pi)]^\tau}{[1-\lambda(1-\pi)]^\tau + \frac{1-\pi}{\pi B(w_L)} + \frac{\lambda(1-\pi)(1-\delta)}{1-(1-\delta)[1-\lambda(1-\pi)]}}.$$

The numerator captures the mass of low-wage matches that have not received a high-wage outside offer.[17] It varies with parameters such as offer arrival rate and the share of workers with low outside options.

Employment Hazard Second, this model again generates a declining hazard function. The probability that a match is destroyed after achieving tenure τ is given by

$$d_\tau = \delta + \ell_\tau \lambda(1-\pi).$$

This probability is strictly declining in tenure if $\lambda > 0$ and $0 < \pi < 1$. The initial level depends on the rate of arrival of outside offers and the initial share of workers in low-wage jobs, which in turn depends on outside options through $B(w_L)$.

As with the learning model, these results reflect selection via endogenous separation. The unemployed initially match with both low-wage and high-wage jobs. As tenure increases and outside offers accumulate, a growing share of the workers who initially worked low-wage jobs will have received a high-wage outside offer. Hence, increasing tenure implies a growing share of workers with high-wage jobs.

[17] The two extra terms in the denominator capture the mass of workers who initially matched with a high-wage job and remain at tenure τ as well as the share of workers who ascended the job ladder to a high-wage job and achieved tenure τ.

Tenure-Wage Profile This selection process once again has implications for the wage-tenure profile. We again characterize the log-wages of a worker with tenure τ relative to a new worker:

$$\log(w_\tau) - \log(w_0) = \log\left[\frac{\ell_\tau w_L + (1-\ell_\tau)w_H}{\ell_0 w_L + (1-\ell_0)w_H}\right],$$

which is positive and concave if $\lambda > 0$ and $0 < \pi < 1$.

Cross-Country Implications The job ladder model can help understand cross-country differences in labor market dynamics if the rate at which workers climb the ladder is correlated with development. For example, it can account for our findings if λ is higher in poorer countries, meaning that workers are more likely to receive outside offers there. It can also account for our findings if workers in poorer countries have worse outside options (higher $B(w_L)$), which in effect starts them on a lower rung of the job ladder and gives them more room to climb.

4.3 Summary

We have shown that two theories in the search and matching literature can rationalize our findings. Both emphasize endogenous selection of matches to generate declining employment hazards and rising wage-tenure profiles. They also provide possible driving forces that may explain why this mechanism works differently in poor countries. Within the simple theories considered so far, those mechanisms include the precision of ex ante information about match quality, the rate at which match quality is learned, the rate at which workers receive outside offers, and the quality of workers' outside options.

Of course, we cannot directly observe these forces. This leaves two broad possibilities. The first is that there is indeed some technological difference across countries that makes it more difficult to, for example, screen workers in poorer countries. The second is that the difference in "average" screening difficulty is driven by underlying compositional differences in workers or firms or differences in (observable) labor market policies. For example, while we cannot measure directly the precision of ex ante information about match quality, Arcidiacono et al. (2010) documents that in the United States, firms are better informed about more-educated workers' ability. This suggests disaggregating our cross-country results to see whether they vary systematically by education and whether the composition of the labor force by education can account for our results. Likewise, while we cannot measure workers' outside options, we have measures of labor market institutions that affect those outside options.

To study these questions, we therefore use a combination of disaggregated results and measures of labor market institutions. We then return to the direct forces emphasized in the theories presented here, highlighting related results from the literature and areas where additional data and analysis are needed.

5 Evidence on Driving Forces

In this section, we explore possible driving forces that might explain our new empirical facts. We disaggregate our results to study the importance of composition effects. We link them to available measures of labor market institutions and study within-country variation. Finally, we discuss related findings from the literature and avenues where additional data are needed.

5.1 Disaggregated Results and Composition Effects

Our first approach is to disaggregate our results by worker and firm characteristics. Doing so allows us to understand how labor market dynamics vary within and between groups, with a focus on whether they are accounted for by composition effects. The standard disclaimer about accounting methods applies: they capture only proximate differences and cannot capture any possible spillovers or general equilibrium effects.

We return to our microdata and harmonize widely available characteristics of workers and firms. Not all variables are available in all countries or years; Table A3 provides details on availability. For workers, we harmonize age in 10-year bins (16–25, 26–35, and so on). Gender is available for all countries. We code education into a binary variable for whether a worker has completed high school. More detailed classifications can be constructed for subsets of countries but yield similar results. We harmonize occupations at the one- and two-digit level, following the International Standard Classification of Occupations (ISCO) 08 standards. Most countries directly use ISCO occupational schemes or adapt their own schemes from the ISCO, which makes harmonization somewhat easier.

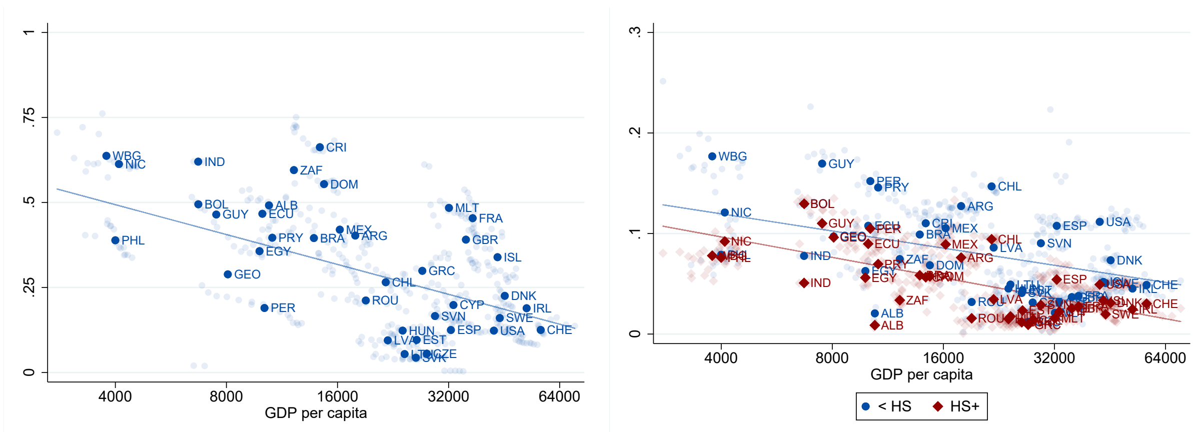
For firms, we categorize industry using an industry coding scheme with 15 possible codes suggested by Minnesota Population Center (2019). We measure establishment size using three bins: small (1–9 employees), medium (10–50 employees), and large (51+ employees) establishments. Most surveys were careful to distinguish establishment from firm, but in some poorer countries, the distinction was not so clearly made. The rarity of multiestablishment firms in poor countries makes this less important. Finally, in many countries, we know whether the firm employs the worker on a formal basis, where a formal job is one where the employer makes payments into social programs (such as pensions) on the worker's behalf.

We disaggregate employment shares and employment-exit rates along each of these dimensions. Figure 8 shows the disaggregated results by education, motivated by the work of Arcidiacono et al. (2010) discussed in the previous section. Figure 8a plots the share of workers with less than a high school degree. It varies widely, from one-half in the poorest countries to about one-tenth in the richest. Figure 8b disaggregates the employment-exit rate by education. Educated workers are less likely to exit employment essentially everywhere, with an average gap of 2–3 percentage points in quarterly exit rates.

Figure 8: Accounting for Education

(a) Employment Share, <HS

(b) Exit Rate by Education



Figures with disaggregated results for other characteristics are available in Appendix D. We find several other characteristics with large between-group variation in exit rates and cross-country variation in employment composition. These characteristics include age, occupation, establishment size, and formal status. These findings raise the question of whether our aggregate trends can be explained by systematic variation in labor force composition by development.

We use an accounting exercise to address this question. Recall that our benchmark estimate of the trend relationship between labor market flows and development in Table 2 is derived from a simple regression:

$$T_{ct} = \alpha + \beta \log(y_{ct}) + \varepsilon_{ct}.$$

We construct counterfactual labor market flows that fix at a common level the composition of workers or firms, isolating only the variation in flows by type. If we decompose the overall transition rate T_{ct} into the transition rate by group $g \in G$, T_{gct} and the share of group g in the relevant population ω_{gct}, then our counterfactual transition rate is

$$\tilde{T}_{ct} = \sum_{g \in G} \bar{\omega}_g T_{gct},$$

where $\bar{\omega}_g$ is the average share for group g in our cross-country sample.

We estimate the relationship between this counterfactual flow and development:

$$\tilde{T}_{ct} = \tilde{\alpha} + \tilde{\beta} \log(y_{ct}) + \tilde{\varepsilon}_{ct}.$$

We say that accounting for group G is important if it substantially attenuates the estimated relationship between flows and development. Formally, we say that group G accounts for

$$share = 1 - \frac{\tilde{\beta}}{\beta}$$

of the overall flows-development trend.

Table 3 summarizes our accounting results for employment-exit rates.[18] The columns give the results for total employment or for wage workers alone. The main reason for studying wage employment is that the self-employed in most countries do not provide information on their industry, occupation, or formal status, so we can account for these characteristics only for wage workers.

We start with Panel A, which considers each factor in isolation. As expected, several of the observed characteristics help account for the flows-development trend. The education findings shown in Figure 8 account for 19 percent; age, occupation, establishment size, and formal status account also for non-negligible shares. However, our findings hold even within disaggregated categories. The easiest way to see this is that no single factor accounts for even one-fifth of the overall flows-development trend.

Panel B of Table 3 considers interactions of factors and their ability to account for the trend in labor market flows. Broadly, the findings are consistent with a small role for observable characteristics. Two factors account for, at most, one-third; accounting

[18]We focus on the employment-exit rate because in this case, $\bar{\omega}_g$ is the sample average share of group g matches, which is always observed. By contrast, accounting for the job-finding and job-to-job transition rates for firm characteristics requires us to fix the sample average share of group g vacancies, which is not observed. In Appendix D, we present accounting results for worker characteristics and the other flows.

Table 3: Accounting for Employment-Exit Rates

	Share Accounted for (%)	
Panel A: One Factor	Wage Employment	Total Employment
Sex	-0.042	-0.096
Sector	0.023	–
Firm Size	0.110	0.213
Informal	0.120	–
Occupation	0.140	–
Age	0.155	0.079
Edu	0.188	0.195
Panel B: Multiple Factors		
Occupation + Firm Size	0.223	–
Firm Size + Age	0.242	0.291
Occupation + Sector	0.250	–
Occupation + Age	0.282	–
Occupation + Edu	0.324	–
Age + Sex + Edu	0.267	0.157
Occupation + Age + Edu + Firm Size	0.493	–

Table notes: All figures capture the share of the exit rate-development relationship accounted for by the worker or firm characteristics given in the rows. The share accounted for is constructed as explained in the text. Columns give the corresponding figure for total employment or wage employment; "–" indicates that the figure cannot be computed.

for interactions of three or four terms can push the figure up to, at most, one-half. We conclude that although disaggregating our results and allowing for composition effects can help explain our findings, at least half remains unaccounted for.

5.2 Labor Market Institutions

An existing literature cites the importance of labor market institutions – the set of regulations, rules, and norms that affect employment relations in a country – for explaining cross-country differences in labor market dynamics among rich countries (Ljungqvist and Sargent, 1998; Krause and Uhlig, 2012; Jung and Kuhn, 2014; Engbom, 2017). In the context of our models, labor market institutions are likely to affect separation rates (through employment protection laws) and workers' outside options (through minimum wages). We consider two forms of evidence on the importance of labor market institutions.

We start by conducting accounting exercises using the measures of cross-country labor market institutions available from the World Bank's Doing Business survey. The index measures the institutions governing a fixed benchmark case: the employment of a cashier at a supermarket in the retail sector. The data are available from 2014 to 2018, although the exact indicators available vary by year.

We investigate the relationship between labor market flows and development after controlling for labor market institutions using the regression

$$T_{ct} = \beta \log(y_{ct}) + \psi z_{ct} + \gamma_t + \varepsilon_{ct}, \tag{5}$$

where T_{ct} is a measure of flows in country c in year t, y_{ct} is GDP per capita, and z_{ct} is one of the various measures provided of labor market regulations and institutions. We also include year fixed effects γ_t. Our main question of interest is whether controlling for labor market institutions substantially attenuates the estimate of β.

Table 4 shows the results. We focus here on the employment-exit rate, but results for the job-finding rate are similar and can be found in Appendix D. The first column confirms that the exit rate declines with income. We then proceed to introduce various labor market indicators. We control for the extent of severance pay requirements; paid leave requirements; the existence of labor courts for resolving labor disputes; the legal status of fixed-term contracts; the minimum wage, expressed as a ratio of value added per worker; and the duration of a probationary period for new workers. Finally, in column (8) we use principal component analysis to extract the common factor among all the measures of institutions.

Table 4: Employment-Exit Rates and Labor Market Institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log GDP per capita	-0.042***	-0.031***	-0.042***	-0.037***	-0.043***	-0.041***	-0.043***	-0.022***
	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.007)
Severace pay (weeks of salary)		0.008***						
		(0.002)						
Annual paid leave required (days of work)			-0.017***					
			(0.003)					
Existence of labor court				0.017**				
				(0.007)				
Legal to have fixed-term contracts					-0.002			
for permanent work?					(0.005)			
Min Wage/VA per worker						0.014		
						(0.014)		
Probationary period (months)							0.000**	
							(0.000)	
1st principal component								0.010***
								(0.002)
Observations	139	139	139	87	139	110	129	51
R-squared	0.450	0.508	0.542	0.433	0.450	0.466	0.525	0.627
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Sample Average	0.051	0.051	0.051	0.050	0.051	0.051	0.049	0.046
Coeff, GDP per capita (no institutions)	-0.042***	-0.042***	-0.042***	-0.039***	-0.042***	-0.043***	-0.044***	-0.041***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)
R-squared (no institutions)	0.450	0.450	0.450	0.398	0.450	0.461	0.505	0.487

Table notes: All regulations are taken from the World Bank Doing Business survey. Severance and annual paid leave are measured as inverse hyperbolic sines, to approximate a log specification while allowing zeros. The row labeled “coeff, GDP per capita (no institutions)” is the coefficient from the regression of exit on log GDP per capita in the restricted sample without including any labor market indicators. “R^2 (no institutions)” is the associated R^2.

These results show that labor market institutions affect employment-exit rates, consistent with the existing literature. Many of the relationships are economically and statistically significant. However, the negative relationship between labor market flows and development remains after controlling for labor market institutions. Even in the most extreme case (controlling for the bundle of labor market institutions produced by principal component analysis) the estimate is cut by less than half. The estimates in the appendix show that the negative relationship between the job-finding rate and development often strengthens after controlling for labor market institutions.

Intuitively, labor market institutions do not explain our findings because they are only weakly correlated with development. While many European Union countries have restrictive labor market institutions, so do Egypt, Palestine, and South Africa; on the other hand, while many poorer countries have less stringent institutions, so do the United States and the United Kingdom.

This first approach relies on evidence from cross-country regressions. As a second, complementary approach, we explore whether the relationship between labor market flows and development holds also across regions within a country, where differences in labor market institutions and other possible confounding factors are presumably smaller.

We focus on three countries with consistently defined regions, large regional income variation, and a large number of observations per region: India, Mexico, and the United States. For each, we re-compute transition rates by region (state, including administrative regions in India) and year. We merge this data with annual regional real GDP per capita.[19] In each case, GDP is adjusted for inflation but not for cross-regional price disparities; we are not aware of systematic regional accounts that include such a correction.

For each country, we pool regions and years and regress transition rates on log GDP per capita. Table 5 shows the results. The estimates are negative and statistically significant for five of the six possible cases; only the job-finding rate in India shows a positive (but statistically insignificant) trend. The magnitudes also compare well with the aggregate cross-country evidence in Panel A of Figure 2. The effect of income on the exit rate is about one-third of the cross-country estimate (-0.035), while the effect of income on the job-finding rate is quite similar (-0.017). This evidence suggests again that labor market institutions

[19] United States: per capita real GDP from the regional accounts, available at https://apps.bea.gov/iTable/index_regional.cfm. Mexico: 2013 fixed price GDP from https://www.inegi.org.mx/programas/pibent/2013/default.html#Tabulados divided by population from https://datos.gob.mx/busca/dataset/proyecciones-de-la-poblacion-de-mexico-y-de-las-entidades-federativas-2016-2050/resource/c3a55508-2678-4018-bf5b-bf1f45745ae7. India: per capita net state domestic product from http://mospi.nic.in/data.

Table 5: Labor Market Flows and Income Across Regions

	USA		Mexico		India	
	Exit Rate	JFR	Exit Rate	JFR	Exit Rate	JFR
Log GDP p.c.	-0.011***	-0.014***	-0.012***	-0.023***	-0.010*	0.002
	(0.001)	(0.003)	(0.001)	(0.002)	(0.006)	(0.004)
Observations	1,982	1,982	741	741	66	66
R-squared	0.075	0.011	0.133	0.143	0.047	0.003
Sample Average	0.055	0.162	0.104	0.186	0.052	0.038

Table Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

represent a plausible channel for cross-country variation in labor market dynamics but that they are unlikely to account for all of the differences.

5.3 Evidence from the Literature and Areas for Future Work

Our database is useful because it yields systematic evidence on labor market dynamics for a large number of workers and a wide range of countries. However, the underlying microdata consist of surveys of workers, which limits the types of data they contain and the types of hypotheses we can directly address. The goal of this section is to consider the evidence from alternative approaches used in the literature.

For example, a growing experimental literature has considered the role of limited ex ante information in labor market matches. This approach has the benefit of being able not only to isolate information but also potentially to disentangle *who* is missing the information.[20] Recent work has tested such issues and finds an important role for signaling worker skills to firms (Bassi and Nansamba, 2019; Abebe et al., 2019). The effects of such an intervention are larger when certificates are provided to firms in addition to workers, suggesting that joint information about match quality is important for these effects (Carranza et al., 2019). More broadly, limited ex ante information is consistent with the findings that referrals and reference letters are important mechanisms for filling vacancies and finding jobs in developing countries (Beaman and Magruder, 2012; Abel et al., forthcoming). The combination of our cross-country results and these well-identified micro studies across multiple countries suggests that the learning model presented in Section 4 is a promising benchmark for studying equilibrium labor market policies.

One limitation we face is that labor force surveys typically ask questions only about the search activity of people who are classified as unemployed, while our results suggest that the

[20]Recall the learning theory in Section 4 relies on match-specific information. In practice, this could come from workers being uninformed about the firm, firms being uninformed about workers, or both.

search behavior of the employed and the inactive would also be of interest. Banerjee and Bucci (1995) document that in urban India, on-the-job search activity declines significantly with tenure, consistent with what one would expect in job ladder models. They show also that the self-employed and informally employed are more likely to engage in such search than wage workers, especially public-sector wage workers, which is consistent with recent work that models self-employment in poor countries as being a mixture of a substitute for missing unemployment insurance and search (Schoar, 2010; Poschke, 2013, 2019). It would be useful to more broadly incorporate these types of questions.

Finally, our microdata contain limited information on employers. This limitation makes it difficult to address hypotheses that attribute the driving forces to employer characteristics. For example, Koren and Tenreyro (2007) document a negative correlation between aggregate economic volatility and development. If aggregate economic volatility generates firm-level employment volatility, it could help explain our findings.[21] The limited employer data available in labor force surveys do not allow us to directly address this hypothesis.

We highlight these results to show that despite the undertaking to collect our data, more work is clearly required on these topics. In addition to the RCT evidence above, a promising channel is the growing use of matched employer-employee databases, as Cornwell et al. (2019), Engbom and Moser (2018), and Morchio and Moser (2019) utilize to study various aspects of the Brazilian labor market. Moreover, the results highlight the clear complementarity between cross-country studies of labor market outcomes and more detailed studies of individual labor markets in building a body of evidence on the functioning of labor markets around the world.

6 Conclusion

We build a new cross-country dataset of harmonized rotating panel labor force surveys covering 42 countries with widely varying average income. We document three new empirical findings on how labor market dynamics vary with development. First, labor market flows (job-finding rates, employment-exit rates, and job-to-job transition rates) are two to three times higher in the poorest as compared to the richest countries. Second, employment hazards in poorer countries decline more sharply with tenure; much of their high turnover

[21] While plausible, this relationship is not obvious. McKenzie and Paffhausen (2019) document that small-firm exit rates are positively correlated with GDP per capita among poor and middle income countries, which implies that the extensive margin of firm employment volatility has the wrong sign with respect to development. Moreover, to the extent this is correlated with establishment size, we find little impact when fixing the size distribution.

can be attributed to high exit rates among workers with low tenure. Finally, wage-tenure profiles are much steeper in poorer countries.

These facts can be rationalized by theories that feature endogenous separation and hence selection of which matches survive to long tenure spells. We use simple versions of two existing theories of this type to demonstrate how the selection mechanism works. These theories also suggest some candidate driving forces that may explain why selection works differently in poor versus rich countries.

Finally, we use our data to empirically investigate driving forces. We disaggregate our results and find several interesting patterns, but none that account for more than one-half of our basic facts. Likewise, labor market institutions correlate with our patterns, but controlling for them does not eliminate the correlation between development and labor market dynamics. Finally, we consider several alternative hypotheses that are more difficult to address with our data. We show several areas where additional survey questions or new types of data would be useful.

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Table of Contents for Appendix

A Data Construction Details

A.1 Data Sources

We are aware of a large number of countries that have instituted a rotating panel labor force survey for at least some years (many countries switch between rotating and non-rotating designs). All European Union countries have labor force surveys with such a design, organized and collected under the European Union Labour Force Survey. Additionally, at least 35 other countries have instituted a rotating panel labor force survey at some point. At least basic information for most countries' labor force surveys can be found under the name given at the website of the International Labour Organization at https://www.ilo.org/surveydata/index.php/home.

We have been able to clarify with the national statistical agencies of most countries the conditions (if any) under which they will make available for research purposes the microdata with individual identifiers. Table A1 shows the samples included in our dataset. It lists for each country the name of the underlying dataset (with preference for the English name, if in common usage) and a brief description of how we acquired the data. *Available online* indicates that the data can be easily accessed online. In some cases they can simply be downloaded, but we also include countries that have a short and minimal registration or application process. *Application required* indicates that data can be accessed under somewhat stricter conditions. This typically includes submitting a formal application and research proposal to the relevant national statistical agency. It might also include assurances or plans to protect and not disseminate the data, or a fee. *Personal correspondence* indicates that the data were acquired through direct communication with the national statistical office.

The European Union Labour Force Survey is a complicated case. Eurostat does not make available to researchers the data with longitudinal identifiers. However, roughly half of EU countries use consistent household and person identifiers within each year, which makes it possible to match people over time within a calendar year.[22] For France and the United Kingdom, we are also able to access microdata with longitudinal identifiers directly from the national statistical office (via Quételet PROGEDO Diffusion and the Office for National Statistics, respectively). We use these data instead so that we can also match individuals across calendar years and because they include additional information about

[22]We thank Nik Engbom for bringing this point to our attention. We were able to confirm that these identifiers are consistent for some countries with Eurostat. We determined which countries could be matched in this way through experimentation; the relevant countries have extremely high rates of agreement over time on age and sex, while others do not.

Table A1: Rotating Panel Labor Force Surveys – Included

Country	Name[a]	How Acquired[b]
Albania	Labour Force Survey	Available online
Argentina	Encuesta de Hogares y Empleo	Available online
Bolivia	Continuous Employment Survey	Available online
Brazil	Continuous National Household Sample Survey (PNAD)[c]	Available online
Chile	National Employment Survey (ENE)	Available online
Costa Rica	Continuous Employment Survey (ECE)	Available online
Croatia	European Union Labour Force Survey	Application required
Cyprus	European Union Labour Force Survey	Application required
Czech Republic	European Union Labour Force Survey	Application required
Denmark	European Union Labour Force Survey	Application required
Dominican Republic	Mercado de Trabajo Encuesta Continua (ENCFT)	Personal correspondence
Ecuador	Encuesta de Empleo	Available online
Egypt, Arab Rep.	Labour Force Sample Survey	Application required
Estonia	European Union Labour Force Survey	Application required
France	Enquete Emploi en Continu	Application required
Georgia	Monitoring of Household Survey	Available online
Greece	Labour Force Survey	Application required
Guyana	Labor Force Survey	Available online
Hungary	European Union Labour Force Survey	Application required
Iceland	European Union Labour Force Survey	Application required
India	Periodic Labor Force Surveys	Available online
Ireland	European Union Labour Force Survey	Application required
Italy	European Union Labour Force Survey	Application required
Latvia	European Union Labour Force Survey	Application required
Lithuania	European Union Labour Force Survey	Application required
Malta	European Union Labour Force Survey	Application required
Mexico	Encuesta Nacional de Empleo	Available online
Nicaragua	Encuestas de Hogares	Personal correspondence
Palestine	Labor Force Survey	Application required
Paraguay	Encuesta Permanente de Hogares Continua	Available online
Peru	Encuesta National de Hogares	Available online
Philippines	Labour Force Survey	Application required
Poland	European Union Labour Force Survey	Application required
Romania	European Union Labour Force Survey	Application required
Slovak Republic	European Union Labour Force Survey	Application required
Slovenia	European Union Labour Force Survey	Application required
South Africa	Quarterly Labour Force Survey	Available online
Spain	Encuesta de Poblacion Activa	Application required
Sweden	European Union Labour Force Survey	Application required
Switzerland	Swiss Labour Force Survey	Application required
United Kingdom	Labour Force Survey	Available online
United States	Current Population Survey	Available online

[a] Name of dataset, in English if the national statistical office designates such a name.
[b] Brief description of how data were acquired. See text for details.
[c] Data for 2002–2011 come from the Brazilian Monthly Employment Survey (PME), which samples six urban areas in Brazil. Patterns of interest are similar to those from urban areas for more recent data so we keep both.

Table A2: Rotating Panel Labor Force Surveys – Excluded

Country	Name[a]	Status[b]
Armenia	Labour Force Survey	Wrong rotation scheme
Australia	Labour Force Survey	Restricted access
Bangladesh	Labour Force Survey	Confidential
Canada	Labour Force Survey	Restricted access
Indonesia	National Labor Force Survey (Sakernas)	Only alternating quarters released
Israel	Labour Force Survey	Restricted access
Japan	Labour Force Survey	Wrong rotation scheme
Korea	Economically Active Population Survey	Restricted access
New Zealand	Household Labour Force Survey	Confidential
Nigeria	Household Labour Force Survey	No response
Russia	Labor Force Survey	Wrong rotation scheme
Saudi Arabia	Labor Force Survey	Confidential
Taiwan	Manpower Survey	Wrong rotation scheme
Thailand	Labour Force Survey	Restricted access
Turkey	Household Labour Force Survey	Confidential

[a] Name of dataset, in English if the national statistical office designates such a name.
[b] Brief description of why data cannot be acquired or are not useful for our purposes. See text for details.

certain variables of interest. In the European Labour Force Survey it is not possible to match data for Greece or Spain, but we acquired the data separately from the national statistical offices (Hellenic Statistical Authority [ELSTAT] and the Instituto Nacional de Estadistica [INE], respectively).

A number of countries appear to have rotating panel labor force surveys that we cannot access or that are not useful for our research design. One prominent example is the remaining countries in the European Union Labour Force Survey that randomize identifiers across quarters within a year. Table A2 gives the remaining countries we are aware of, again with the name of the survey and the reason why the data are not included.

Restricted access indicates data that are available under one or more of three restrictive conditions: researchers have to be citizens/nationals of the country; they have to be affiliated with a university or research institute of the country; or they have to travel to a secure location in the country. *Confidential* indicates that data are not available to researchers, to the best of our knowledge. *Wrong rotation scheme* indicates that the workers can be matched at a different frequency, typically monthly or annually. Indonesia operates a quarterly rotating panel labor force survey but makes only semi-annual data available, which for our purposes is the same as the wrong rotation scheme. Finally, *no response* indicates that the country appears to collect the appropriate data, but we were unable to find the data or secure a response from the national statistical agency despite numerous attempts to do so.

A.2 Variable Availability

Not all countries collect or share all requisite data. For example, the EU LFS does not include earnings (only earnings deciles), thus eliminating its use in some parts of the paper. The table below specifies which countries include which data.

Table A3: Variable Availability by Sample

Country	Employment Status	Age	Education	Gender	JJ Flows	Marginally Attached	Sector	Occupation	Formality	Establishment Size	Tenure	Earnings	Hours	Rural
Albania	x	x	x	x	x		x	x	x	x	x	x	x	
Argentina	x	x	x	x	x	x	x	x	x	x	x	x	x	
Bolivia	x	x	x	x	x	x	x	x	x	x	x	x	x	
Brazil	x	x	x	x	x	x	x	x	x	x	x	x	x	x
Chile	x	x	x	x	x	x	x	x			x		x	x
Costa Rica	x	x	x	x	x				x	x	x	x	x	x
Croatia	x	x	x	x	x	x	x	x		x	x		x	x
Cyprus	x	x	x	x	x	x	x	x		x	x		x	x
Czech Republic	x	x	x	x	x	x	x	x		x	x		x	x
Denmark	x	x	x	x	x	x	x	x		x	x		x	x
Dominican Republic	x	x	x	x	x	x	x	x	x	x	x	x	x	
Ecuador	x	x	x	x		x	x	x	x	x	x	x	x	
Egypt, Arab Rep.	x	x	x	x			x	x	x	x	x	x	x	x
Estonia	x	x	x	x	x	x	x	x		x	x		x	x
France	x	x	x	x	x		x	x		x	x	x	x	x
Georgia	x	x	x	x	x		x	x	x			x	x	
Greece	x	x	x	x	x	x	x	x		x	x	x	x	x
Guyana	x	x	x	x		x	x	x	x	x	x	x	x	x
Hungary	x	x	x	x	x	x	x	x		x	x		x	x
Iceland	x	x	x	x	x	x	x	x		x	x		x	x
India	x	x	x	x		x	x	x	x			x	x	
Ireland	x	x	x	x	x	x	x	x		x	x		x	x
Italy	x	x	x	x	x	x	x	x		x	x		x	x
Latvia	x	x	x	x	x	x	x	x		x	x		x	x
Lithuania	x	x	x	x	x	x	x	x		x	x		x	x
Malta	x	x	x	x	x	x	x	x		x	x		x	x
Mexico	x	x	x	x	x	x	x	x	x	x	x	x	x	
Nicaragua	x	x	x	x		x	x	x		x			x	x
Palestine	x	x	x	x	x	x	x	x	x	x	x	x	x	x
Paraguay	x	x	x	x	x	x		x	x	x	x	x	x	
Peru	x	x	x	x	x	x	x	x		x	x	x	x	
Philippines	x	x	x	x		x	x	x				x	x	x
Poland	x	x	x	x	x	x	x	x		x	x		x	x
Romania	x	x	x	x	x	x	x	x		x	x		x	x
Slovak Republic	x	x	x	x	x	x	x	x		x	x		x	x
Slovenia	x	x	x	x	x	x	x	x		x	x		x	x
South Africa	x	x	x	x	x	x	x	x		x	x		x	
Spain	x	x	x	x	x	x	x	x		x	x		x	x
Sweden	x	x	x	x	x	x	x	x	x	x	x		x	x
Switzerland	x	x	x	x	x	x	x	x	x	x	x	x	x	x
United Kingdom	x	x	x	x	x	x	x	x		x	x	x	x	
United States	x	x	x	x	x	x	x	x		x	x	x	x	x

[a] x = variable included for at least one year.

A.3 Longitudinal Weights

All of our countries provide sample weights so that cross-sectional moments are representative of the population of interest (typically the labor force or the urban labor force). However, the provided weights are not sufficient when constructing longitudinal moments such as the job-finding rate. The underlying problem is what is called *margin error* in the literature, or the failure to match workers with complete information across periods. This failure could arise because of attrition, temporary absence from the sample, inability to create a unique match, or nonresponse to the relevant outcomes in either period. If we drop all such observations and use the cross-sectional weights, then we are assuming that these variables are *missing at random*, while substantial evidence suggests that attrition is correlated with labor market transitions (Abowd and Zellner, 1985; Bleakley et al., 1999; Fujita and Ramey, 2009).

Multiple solutions to this problem have been proposed in the literature (see, for example, Bleakley et al. (1999) or Fujita and Ramey (2009)). We post-stratify our weights so that the population distribution is the same in the matched and unmatched samples along dimensions of interest. For example, if unemployed people are more likely to move to find work and drop out of the sample, then they will be underrepresented in the longitudinally matched sample. Post-stratification increases the weight of unemployed workers in the longitudinal sample so that the implied unemployment rate is the same in the longitudinally matched sample as in the cross section.

An important question with post-stratification is which dimensions to use in re-weighting the data. Adding more dimensions and fitting joint distributions rather than marginal distributions allows for a better match of longitudinal and cross-sectional data and reduces concern about attrition bias. On the other hand, adding too many dimensions generates practical problems as cell sizes become small and the adjustments to the original weights become large. At the extreme, post-stratification breaks down in cases where the unmatched sample has observations in a cell but the matched sample does not.

We focus on four dimensions that are available in all countries and are important for understanding labor force dynamics: labor force status (wage workers, self-employed, unemployed, and inactive), age (in 10-year bins), gender, and education (Barro-Lee categories). Post-stratifying on labor force status is important for fitting cross-sectional moments such as the unemployment rate. After that, we focus on demographics and education because we find that they are observable factors that account for a lot of variation in labor force status and labor force flows.

We cannot fit the full joint distribution of these characteristics. Our compromise is to

Table A4: Impact of Re-Weighting

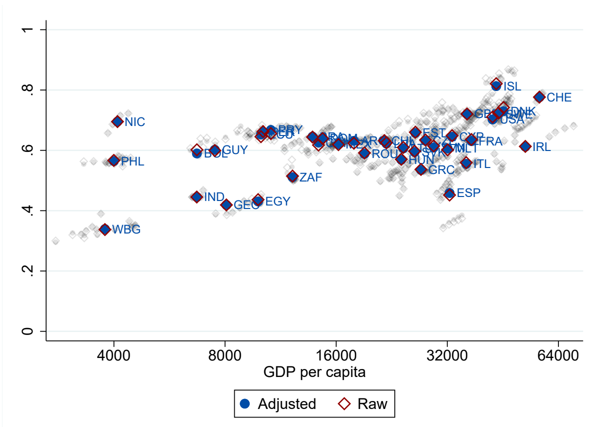
Country	Weight Correlation	Median Absolute Change
Albania	0.997	0.038
Argentina	0.998	0.031
Bolivia	0.893	0.200
Brazil	0.999	0.025
Chile	0.999	0.027
Costa Rica	0.998	0.037
Croatia	0.919	0.018
Cyprus	0.989	0.027
Czech Republic	0.999	0.009
Denmark	0.989	0.048
Dominican Republic	0.999	0.011
Ecuador	0.980	0.056
Egypt, Arab Rep.	0.981	0.041
Estonia	0.997	0.026
France	0.998	0.026
Georgia	0.999	0.013
Greece	1.000	0.008
Guyana	0.972	0.069
Hungary	1.000	0.009
Iceland	0.950	0.040
India	1.000	0.004
Ireland	0.989	0.032
Italy	0.999	0.015
Latvia	0.997	0.032
Lithuania	0.998	0.022
Malta	0.991	0.038
Mexico	1.000	0.020
Nicaragua	0.997	0.020
Palestine	0.998	0.015
Paraguay	0.990	0.040
Peru	0.994	0.038
Philippines	0.993	0.044
Poland	0.924	0.016
Romania	0.999	0.011
Slovak Republic	0.997	0.010
Slovenia	0.998	0.026
South Africa	0.994	0.036
Spain	0.997	0.031
Sweden	0.998	0.027
Switzerland	0.999	0.012
United Kingdom	1.000	0.000
United States	0.995	0.042

Table notes: Weight correlation is the correlation between the original cross-sectional weights and post-stratified weights. Median absolute change is the median of the absolute log deviation between cross-sectional weights and post-stratified weights.

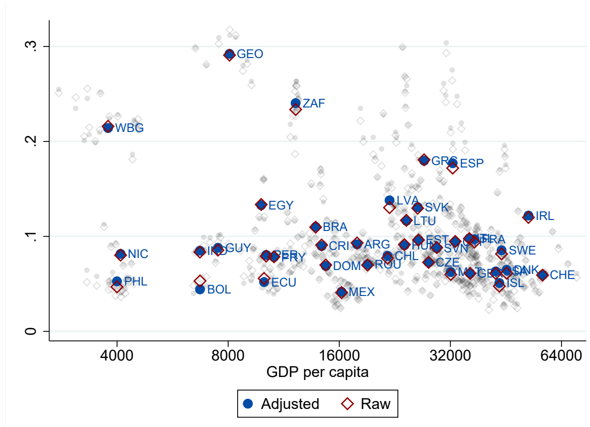
rake the weights so that the matched and unmatched samples for each country-year have the same density by education-labor force status cells and age-gender cells. We focus on these dimensions because they are available and comparable across all countries and because matching them is important for the overall results. In some cases, we have to aggregate categories slightly before raking. For example, the number of unemployed workers with tertiary education in poorer countries or primary education in rich countries can be quite small; in such cases, we merge educational categories.

Figure A1: Labor Market Facts (Adjusted vs Raw Data)

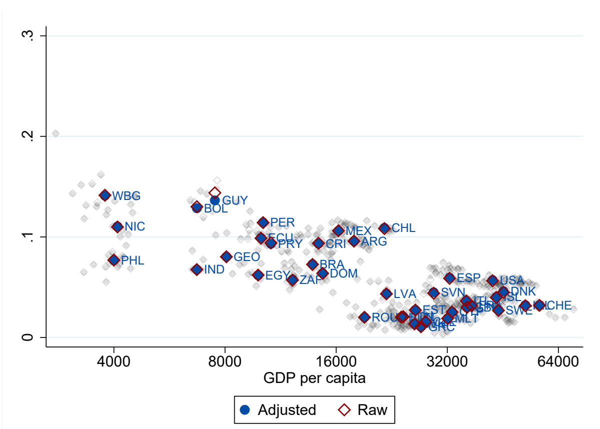
(a) Employment-to-Population Ratio



(b) Unemployment Rate



(c) Employment-Exit Rate



(d) Job-Finding Rate

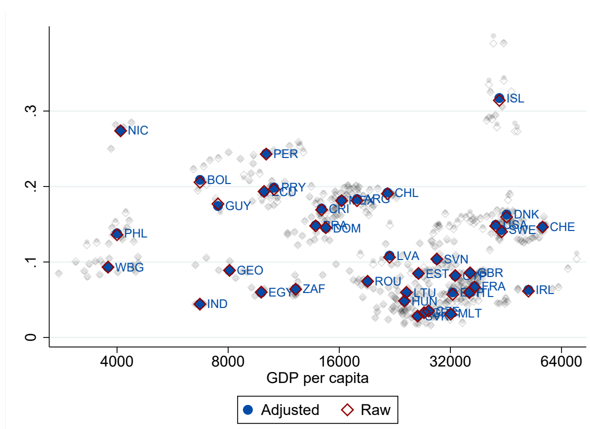


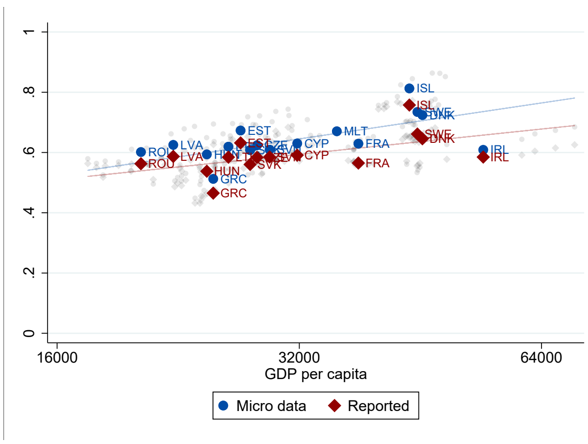
Table A4 shows the impact of re-weighting by comparing the original and adjusted weights. The two are highly correlated for all countries. The median absolute deviation is generally small, on the order of 0–20 percent. Another way to make the same point is to use original versus longitudinal weights to construct key moments. Figure A1 reproduces some of the main figures in the text but compares the raw versus adjusted data. Re-weighting has a visible effect on the unemployment rate (Figure A1b) but a negligible effect on the employment-to-population ratio or the implied flows.

A.4 Comparison of EU Microdata versus Reported Flows

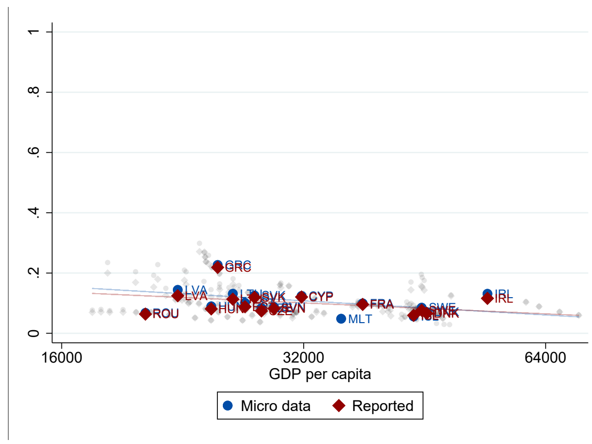
The EU directly reports flow data. Their reported flows differ from ours in several ways. First, they report flows among the population aged 15–74, while we cut off at 65 to remain consistent across various countries. Second, while the EU uses a similar raking procedure to adjust weights, it differs in that they use only age group, sex, and labor force status. We additionally include education.[23] The figures below show how our data differ from theirs, in both stocks and flows.

Figure A2: Stocks

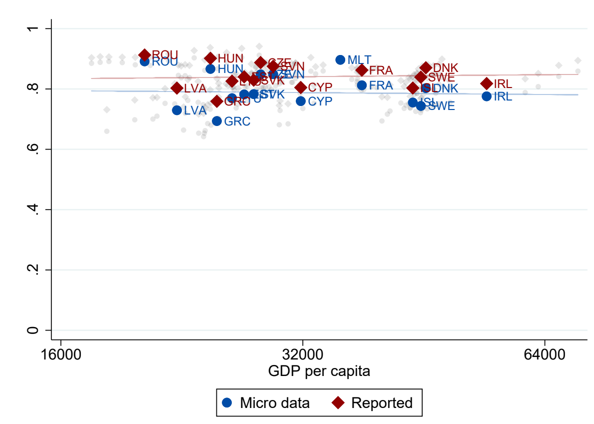
(a) Employed Population Share



(b) Unemployment Rate

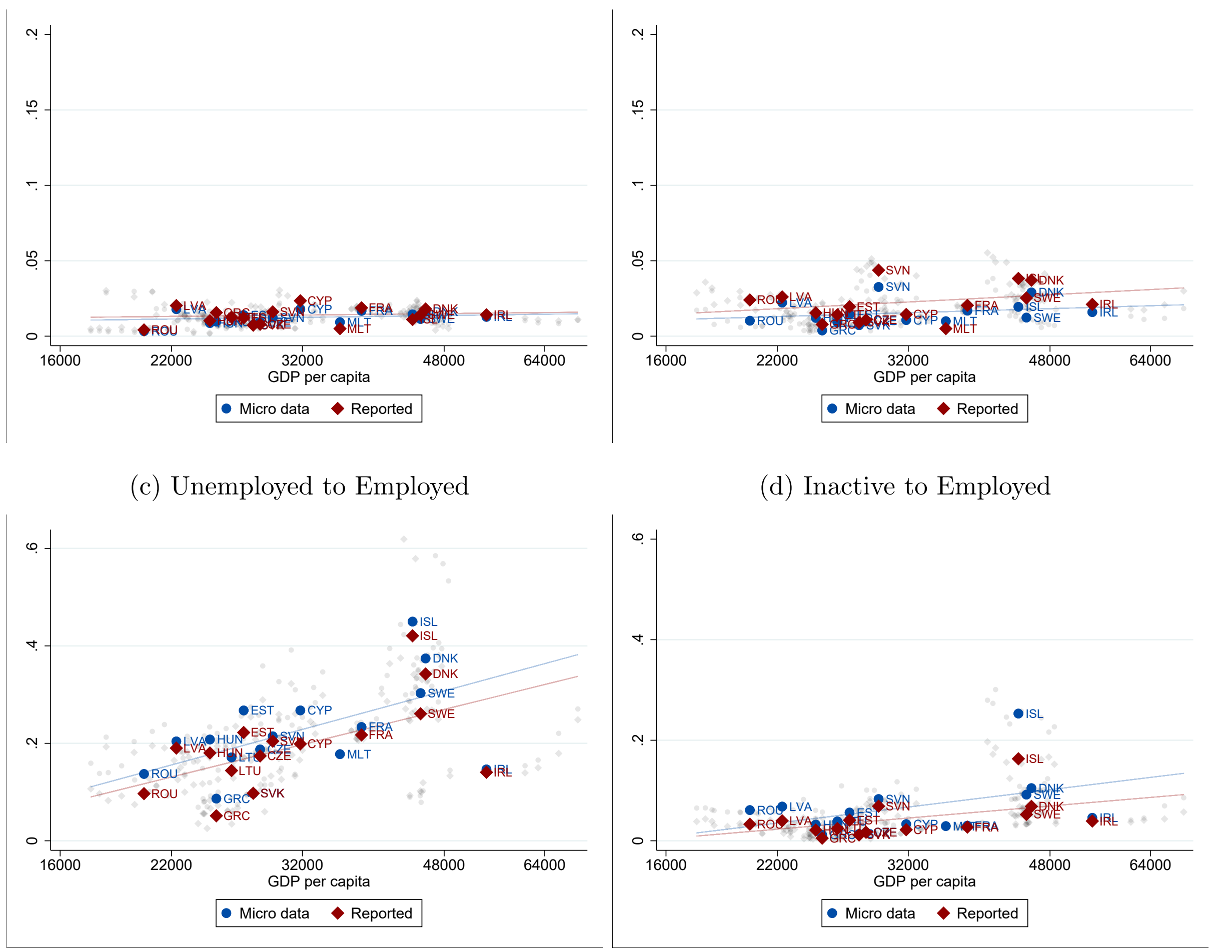


(c) Inactive Share of Non-Employed



[23]More details of the EU procedure are available online at https://ec.europa.eu/eurostat/statistics-explained/index.php/Labour_market_flow_statistics_in_the_EU.

Figure A3: Flows



(a) Employed to Unemployed (b) Employed to Inactive

(c) Unemployed to Employed (d) Inactive to Employed

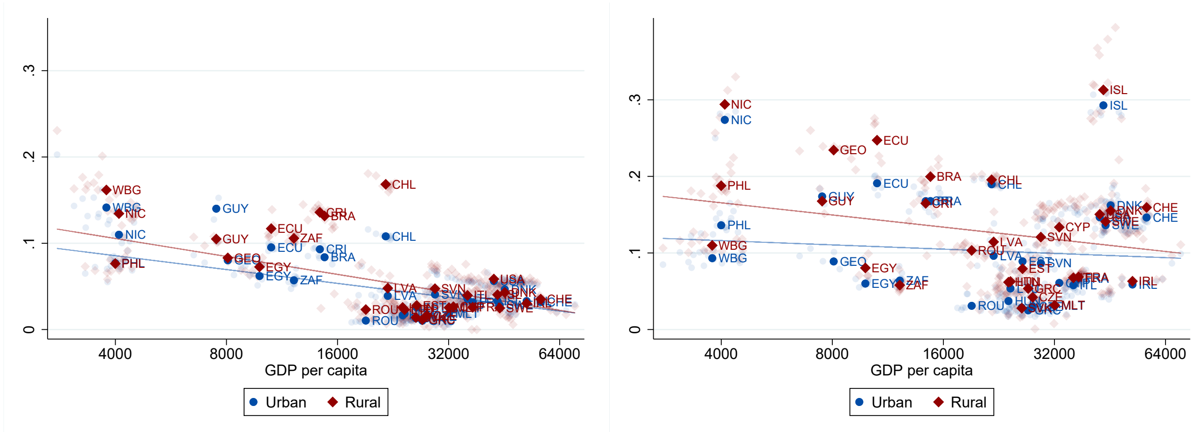
A.5 Urban-Rural

Our baseline analysis focuses on urban labor markets because some of our datasets do not sample rural labor markets. In this appendix, we compare the patterns for urban and rural labor markets for the countries where we have data on both.

Figure A4 plots against GDP per capita employment-exit rates and job-finding rates separately for rural and urban workers. Transition rates are similar for the two types of workers in the richest countries, but elsewhere rural workers have higher transition rates. Poorer countries also have higher rural population shares. Put together, these findings imply that the relationship between labor market flows and development is probably stronger than what we estimate using only urban workers. For this sample of countries, the estimated

Figure A4: Quarterly Transition Rates: Rural versus Urban Workers

(a) Employment-Exit Rate (b) Job-Finding Rate



coefficient from a regression of flows on PPP GDP per capita is 36 percent higher for employment-exit rates in rural relative to urban areas and 34 percent higher for job-finding rates.

B Additional Results on Stocks and Flows

B.1 Detailed Transition Rates

Figure B1 plots against PPP GDP per capita the quarterly persistence of each of the four labor force statuses. Each status is more persistent in richer countries, on average.

Figure B1: Quarterly Probability of Remaining in Same Status

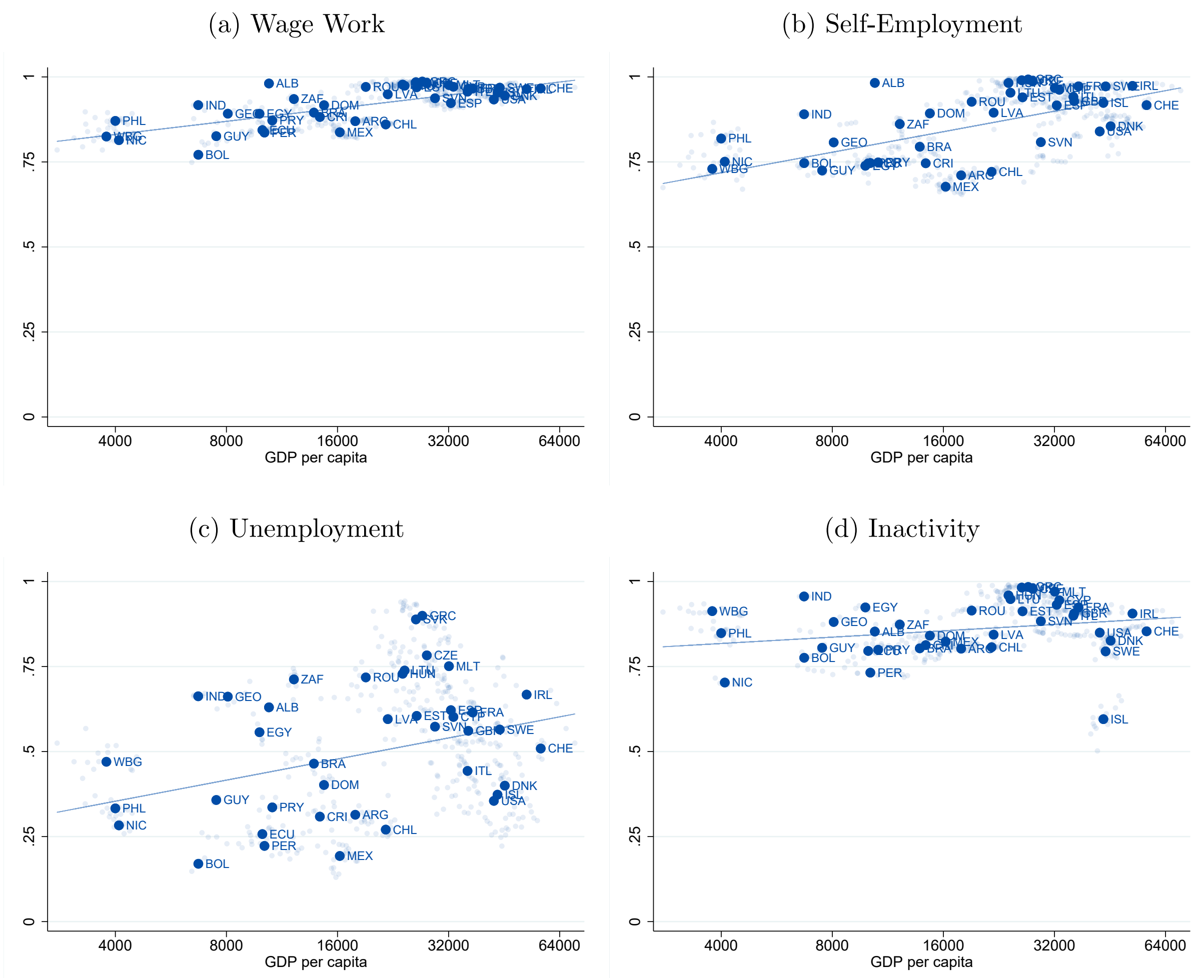
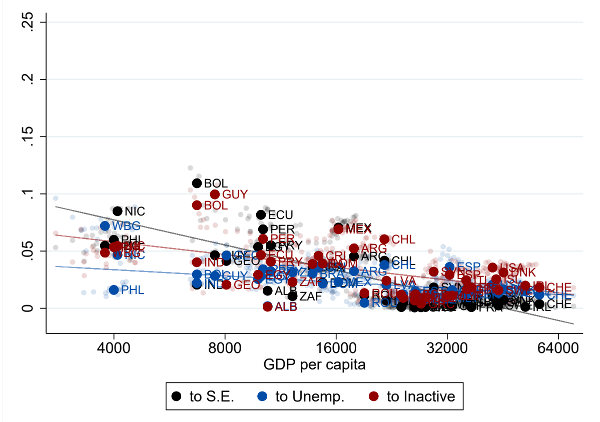


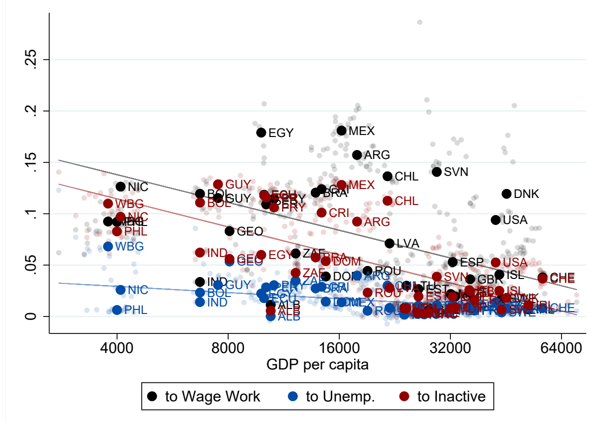
Figure B2 shows the detailed transition rates between states. Estimated regression lines of transition rates against log GDP per capita are included in all figures. The trend is clearly negative for nine of the twelve transitions; the probability of transitioning from unemployment or inactivity to wage work is the important outlier in terms of increasing with respect to development.

Figure B2: Detailed Quarterly Transition Rates

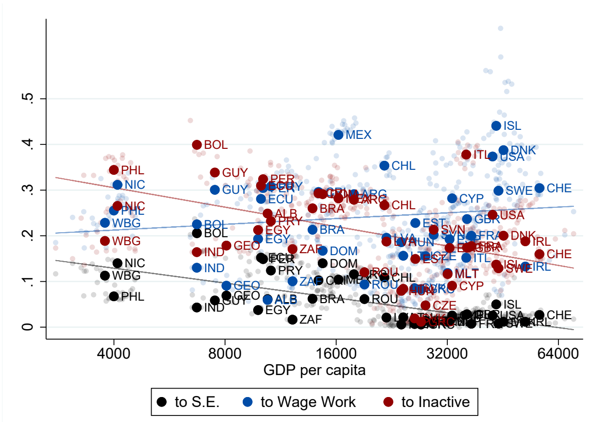
(a) From Wage Work



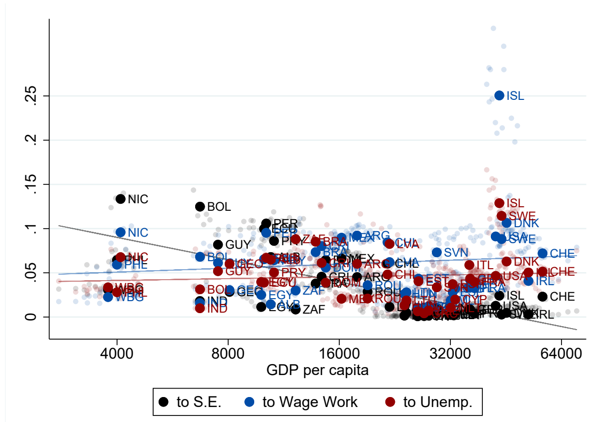
(b) From Self-Employment



(c) From Unemployment



(d) From Inactivity



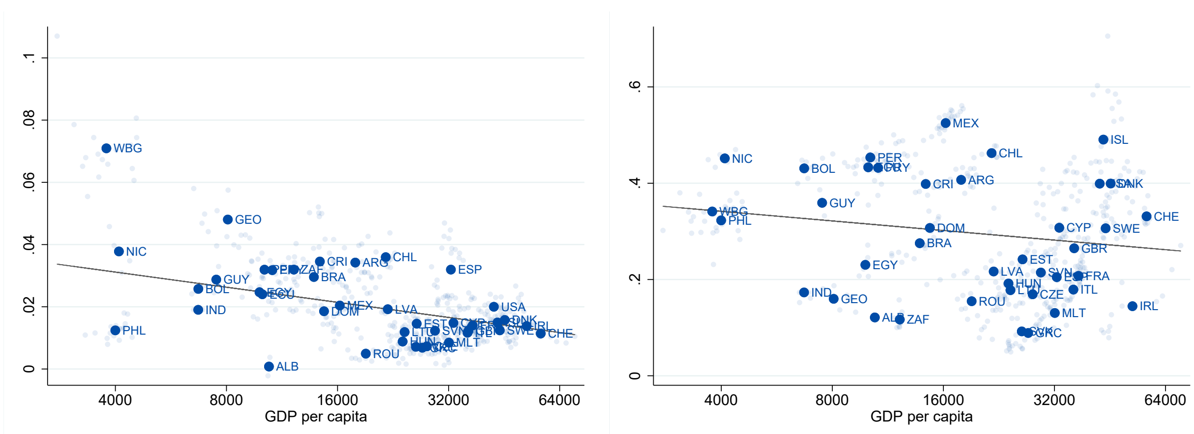
B.2 Alternative Aggregations of Labor Force Status

Section 2 of the paper shows that unemployment, inactivity, and self-employment are less distinct in poorer countries, in the sense that there are smaller differences in job-finding or wage work-finding rates for workers with these statuses. Given this finding, there is no clear way to map job seekers in search models to the data. Our benchmark approach is to focus on transitions between employment and non-employment, pooling unemployment and inactivity. Here, we explore two plausible alternatives.

First, in Figure B3, we consider the more traditional approach of focusing on movements between employment and unemployment, entirely disregarding the inactive. There is still a negative relationship between labor market flows and development.

Figure B3: Labor Market Results: Excluding Inactivity

(a) Employment-Exit Rate (b) Job-Finding Rate

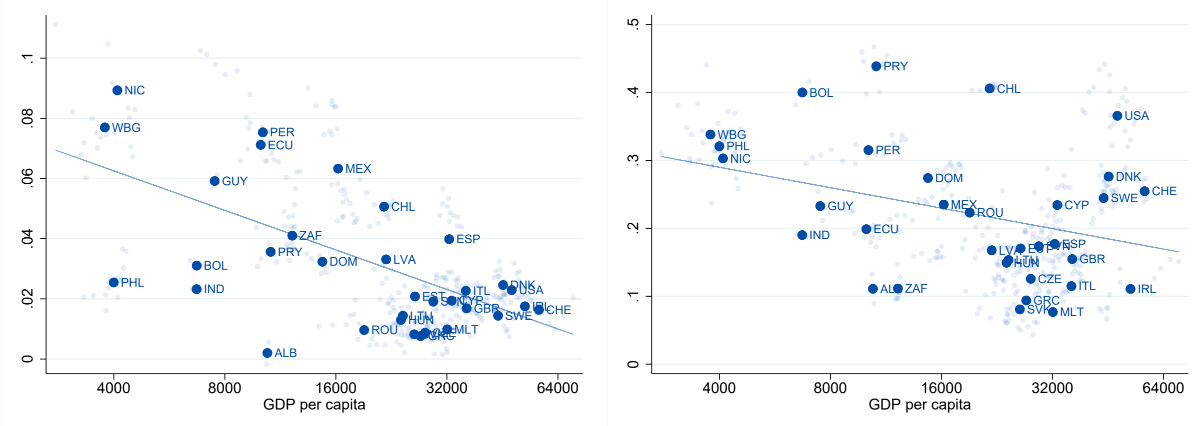


We also consider a hybrid between the baseline and previous approach, where we focus on flows between employment and a broader measure of unemployment that includes the marginally attached inactive workers. We define people as marginally attached if they are inactive but their self-reported reason for not seeking work indicates that they are unable to find suitable work (wrong skills, too young or old, no work currently available, etc.). Those whose responses indicate that they are unable to work or uninterested in work (sick, disabled, in school, retired, caring for the household or family) are excluded from the analysis. We then study the employment-exit rate (to broadly defined unemployment) and the job-finding rate (from broadly defined unemployment).

The results are shown in Figure B4. As with the other approaches, we find a negative relationship between labor market flows and development. That relationship is stronger for

Figure B4: Labor Market Results: Unemployed plus Marginally Attached

(a) employment-exit rate (b) Job Finding Rate



the employment-exit rate than for the job-finding rate, again consistent with the previous checks.

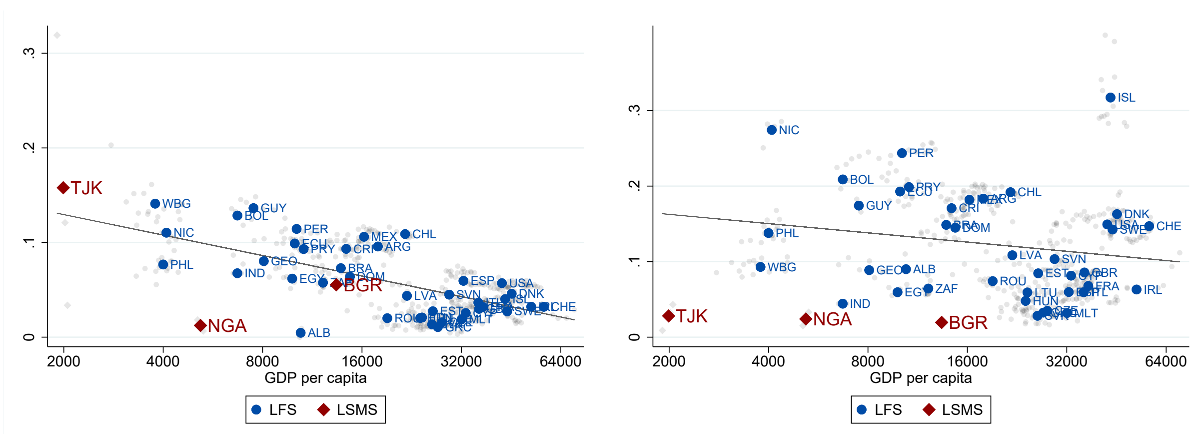
Finally, we have considered alternative approaches to the treatment of the self-employed, such as counting them as unemployed. Doing so has a large impact on cross-sectional moments such as the unemployment rate but maintains the negative relationship between labor market flows and development. Results are available upon request.

C Inclusion of Other Data

As we note in the text, we are missing nearly all sub-Saharan African countries. Thus, we are unable to answer whether these same patterns hold when we include the poorest countries in the world. To attempt to study this question, we turned to the Living Standard Measurement Surveys (LSMS) released by the World Bank. These are cross-sectional surveys, but some include labor market modules that include the length of employment (which, in principle, could be used to back out a job-finding rate) or the length of non-employment (for the employment-exit rate). Unfortunately, only a small set of the 121 surveys include the proper questions, and even when they do, most do not properly map to our measure of the job-finding rate or exit rate.[24] However, four surveys include retrospective monthly panels of labor market indicators.[25] They are Bulgaria-2007, Nigeria-2010, Nigeria-2012, and Tajikistan-2009. We include them below.

Figure C1: Flows

(a) Employment Exit (b) Job Finding Rate



Overall, the employment-exit rates (Figure C1a) seem to line up with our data. The job-finding rates (Figure C1b) are quite low, though the rationale for this result is difficult to come by. Feng et al. (2018) and Bick et al. (2018) highlight how including such countries

[24] For example, the Ghanaian survey asks "How many years or months have you been doing this work, all together?" thus including the entire length of any E–U–E flows in the same occupation/job. This makes this question inconsistent with the definition of a job finding rate. The Serbian survey asks, "When did you cease to perform your last job?" but records only years, thus making it impossible to measure at the frequency desired an employment-exit rate.

[25] We considered all country-surveys available on the LSMS website (121 surveys, available here: https://microdata.worldbank.org/index.php/catalog/lsms). These four had documented retrospective panels.

may change the overall shape of cross-sectional labor market patterns. Given the extra cost in collecting (even short) panel data, however, labor force surveys are unfortunately unavailable in such countries. We view this as an important question for future work.

D Additional Accounting Results

This section provides results on the ability of labor market institutions and worker characteristics to account for job-finding rates and job-to-job transition rates.

D.1 Accounting for Job-Finding Rates

Table D1 shows the accounting results for the job finding rate. As explained in the text, results are available only after controlling for worker characteristics, which account for a small share of job-finding rates.

Table D1: Accounting for Job-Finding Rates

	Share Accounted for (%)	
	Total Employment	Wage Employment
Age	0.288	-0.532
Edu	-0.005	0.122
Sex	-0.212	0.207
Age + Edu + Sex	-0.226	0.140

Table notes: All figures capture the share of the JFR-development relationship accounted for by the characteristics given in the rows. The share accounted for is constructed as explained in the text. Columns give the corresponding figure for total employment or wage employment; n/a indicates that the figure cannot be computed.

Table D2 shows how labor market institutions account for job finding rates. Column (1) shows the relationship between GDP per capita and the job-finding rate for years 2014–2018 (the only years the regulation data is available). The remaining columns show that controlling for labor market institutions has little effect on the estimated relationship between job-finding rates and development. In fact, in many cases, the relationship becomes stronger, suggesting that labor market institutions confound the underlying relationship between job-finding rates and development.

Table D2: Job-Finding Rates and Labor Market Institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log GDP per capita	-0.026**	-0.027**	-0.026**	-0.024*	-0.032***	-0.026**	-0.031***	-0.012
	(0.010)	(0.013)	(0.010)	(0.014)	(0.012)	(0.013)	(0.011)	(0.024)
Severace pay (weeks of salary)		-0.001						
		(0.006)						
Annual paid leave required (days of work)			-0.021**					
			(0.009)					
Existence of labor court				-0.004				
				(0.020)				
Legal to have fixed-term contracts for permanent work?					-0.013			
					(0.013)			
Min Wage/VA per worker						0.004		
						(0.035)		
Probationary period (months)							-0.000	
							(0.000)	
1st principal component								0.010
								(0.008)
Observations	139	139	139	87	139	110	129	51
R-squared	0.049	0.049	0.086	0.037	0.056	0.052	0.071	0.088
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Sample Average	0.127	0.127	0.127	0.123	0.127	0.128	0.126	0.121
Log GDP per capita (no institutions)	-0.026**	-0.026**	-0.026**	-0.024*	-0.026**	-0.026**	-0.030***	-0.031*
	(0.010)	(0.010)	(0.010)	(0.014)	(0.010)	(0.012)	(0.011)	(0.018)
R-squared (no institutions)	0.049	0.049	0.049	0.036	0.049	0.052	0.066	0.060

Table notes: All regulations are taken from the World Bank Doing Business survey. Severance and annual paid leave are measured as inverse hyperbolic sines, to approximate a log specification while allowing zeros. The last two rows are the estimated coefficient and R^2 of the regression of the JFR on log GDP per capita on whatever sample is used in that column.

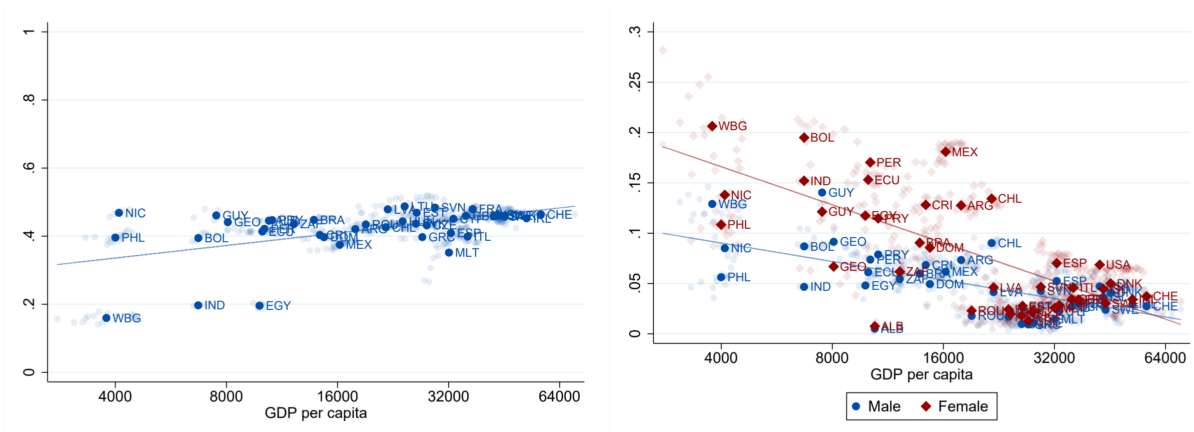
D.2 Detailed Accounting Results: Gender

Figure D1 provides detailed information on gender's role in accounting for labor market flows. Figure D1a shows the employment share of women against GDP per capita. In poorer countries, women generally have a lower share of employment. Figure D1b plots the exit rate by gender against GDP per capita. In most countries, women are more likely to exit employment. However, given that the employment share of women is lower in poorer countries, this contributes negatively to understanding high overall labor market flows there.

Figure D1: Accounting for Gender

(a) Employment Share of Women

(b) Exit Rate by Gender



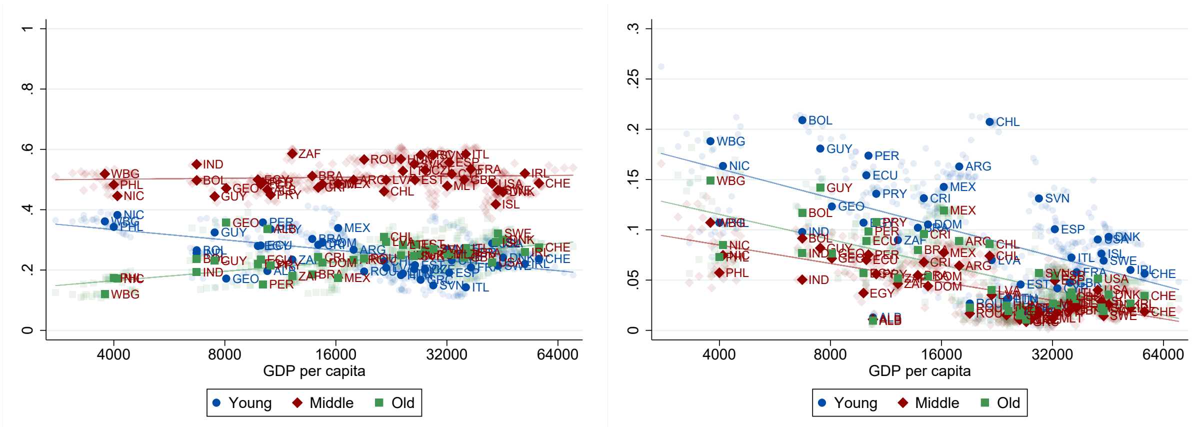
D.3 Detailed Accounting Results: Age

Figure D2 provides detailed information on the role of age in accounting for labor market flows. For visual clarity, we divide the population into three groups, young (16–29 years of age), middle aged (30–49 years) and old (50–65 years). Figures D2b and D2a show the exit rate and employment share by GDP per capita. Although there are large differences in transition rates by age category, the population shares do not differ enough by age to account for much of the estimated relationship between labor market flows and development.

Figure D2: Accounting for Age

(a) Employment Share by Age

(b) Exit Rate by Age

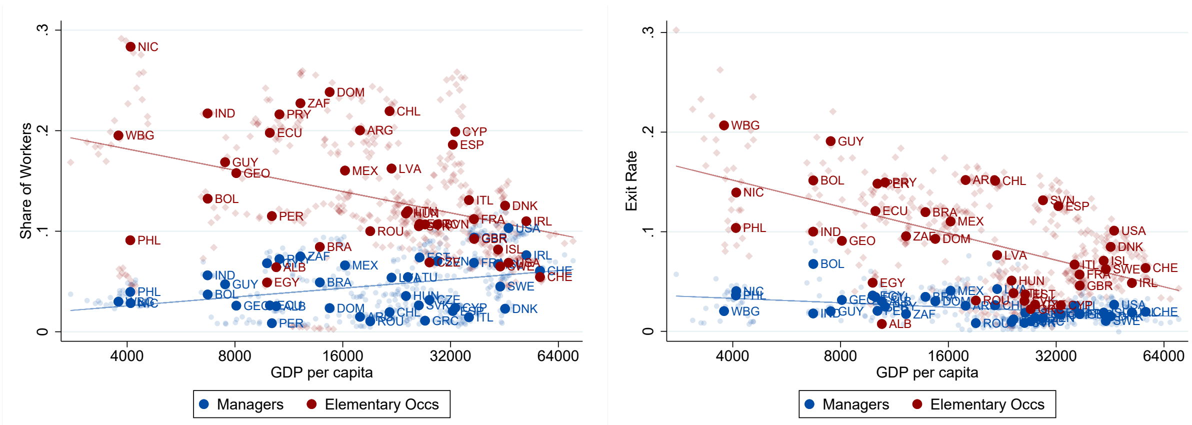


D.4 Detailed Accounting Results: Occupations

Figure D3 provides detailed information on the role of occupation in accounting for labor market flows. For visual clarity, we focus on the two extreme ends of the occupational distribution: managers (the most skilled category in ISCO) and elementary workers (the least skilled). There are clear differences in the employment shares of these occupations between poor and rich countries (Figure D3a) and large differences in exit rates (Figure D3b). Overall, occupation accounts for somewhat less of the overall picture than education, because the other occupations (ISCO one-digit groups 2–8) offer a less clear pattern than the extremes.

Figure D3: Accounting for Occupation

(a) Employment Share, Select Occupations (b) Exit Rate, Select Occupations



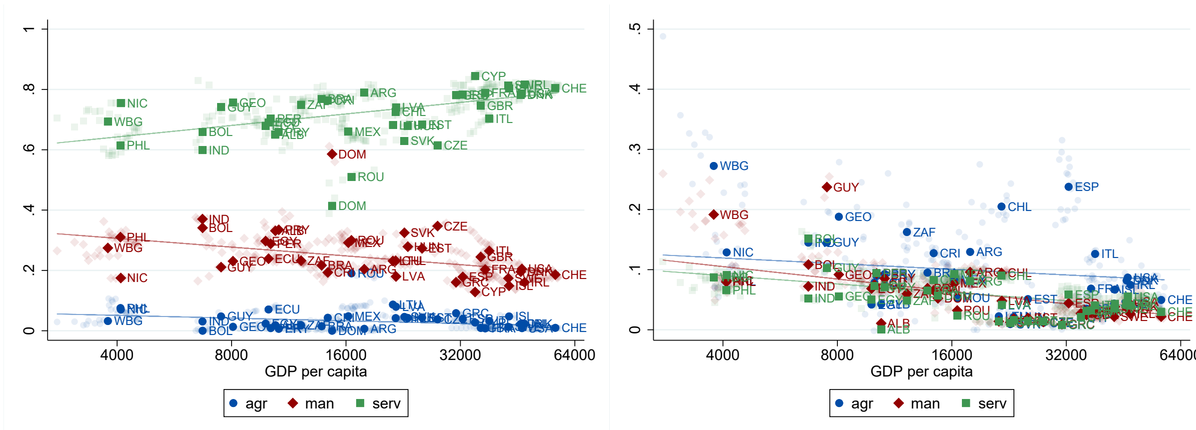
D.5 Detailed Accounting Results: Sectors

Figure D4 breaks down exit and finding rates by broad non-agricultural sectors. Interestingly, there is almost no difference in exit rates across these sectors, which echo the more detailed results in the main text. Figure D4a shows the share of employment in services and manufacturing. As expected, richer countries have more employment in services.[26]

Figure D4: Accounting for Sectors

(a) Employment Share by Sector

(b) Exit Rate by Sector



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[26]There is a small amount of agriculture even in urban areas here. The sampling unit is a dwelling, so some urban workers may still work in agriculture.

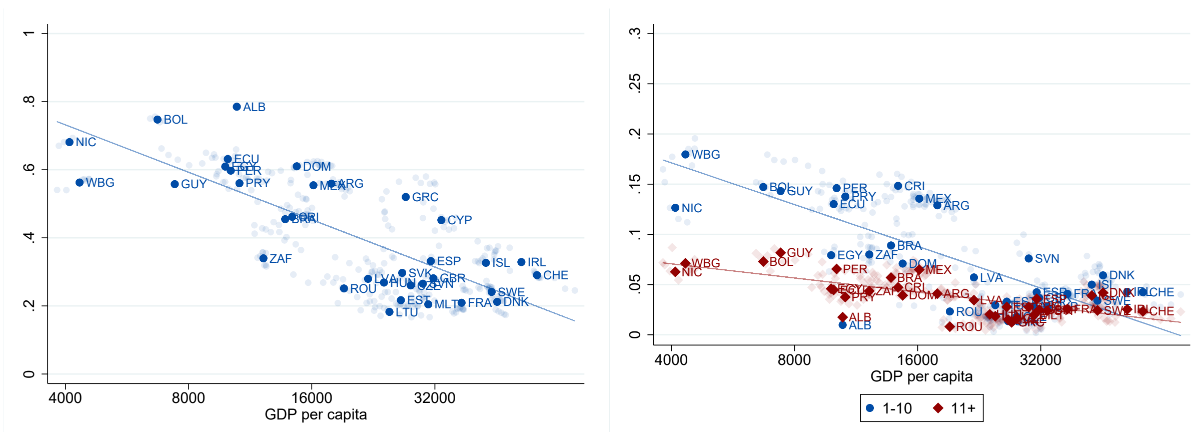
D.6 Detailed Accounting Results: Establishment Size

Figure D5 breaks down exit rates and employment shares by establishment size. Chile explicitly asks about workers in the firm within the entire country, and we drop them from the analysis. As mentioned in the text, countries generally bin establishment size. We use the bin that maximizes sample size, which is a coarse decomposition of 1–10 workers and 11 or more workers. Figure D5b shows that in all countries exit rates are higher for workers in small firms and lowest for workers in large firms. Figure D5a shows that poorer countries have more employment in small firms and less in large firms.

Figure D5: Accounting for Establishment Size

(a) Employment Share by Establishment Size

(b) Exit Rate by Establishment Size

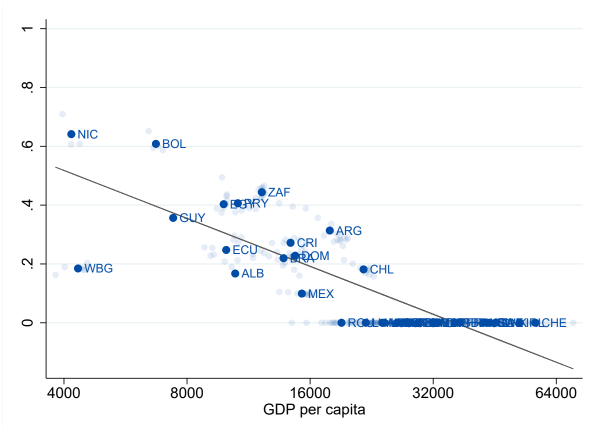


D.7 Detailed Accounting Results: Informality

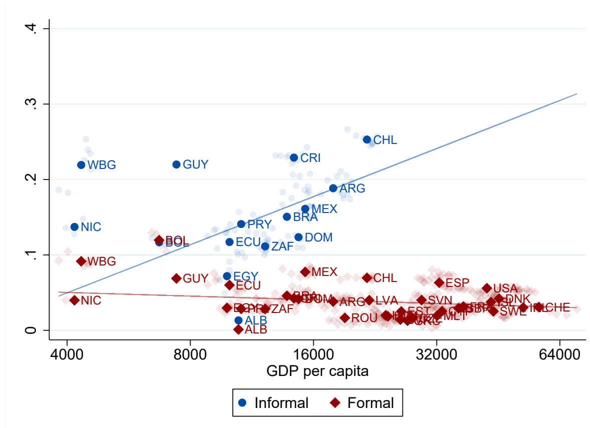
Figure D6 breaks down accounting results for formal and informal wage work. Figure D6b shows exit rates for formal and informal workers. Workers are much more likely to exit from informal work, although the gap is smaller in poorer countries. Figure D6a repeats the share of informal workers by country, which declines from one-half to none in rich countries (the latter, by assumption).

Figure D6: Accounting for Informal Employment (Wage Work)

(a) Employment Share, Informal Workers



(b) Exit Rate by Formality



D.8 Additional Results on Tenure-Wage Profiles

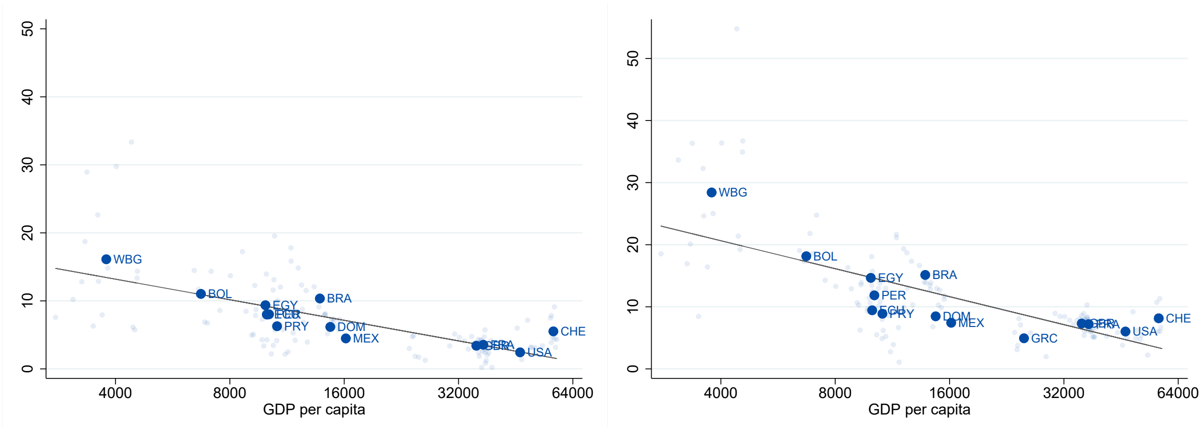
D.8.1 Varying Bins for Tenure

In the main text, we provided results using as our baseline 0–6 months of tenure. Here, we provide robustness by varying the bins. We consider as our baseline 0–12 months of tenure and compute average wage growth for workers with 1–5 years of tenure and 5 or more years of tenure.

Figure D7: Wage-Tenure Profiles with Occupational Controls

(a) Wage Difference, 1–5 Years of Tenure

(b) Wage Difference, 5+ Years of Tenure

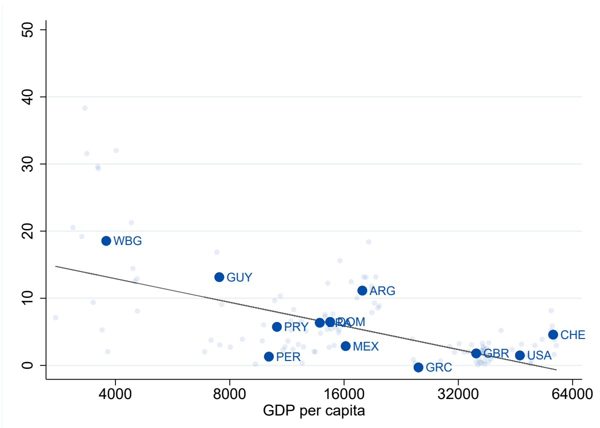


D.8.2 Including Occupational Controls

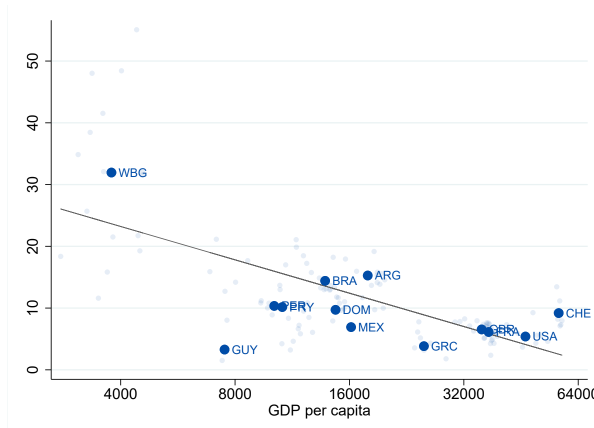
Our baseline results control for education. Here, we also control for occupation and show that the results do not change. We consider various binnings of tenure to show the results are robust. The first two figures are compared with a baseline of 0–6 months of tenure (our baseline in the main text), while the bottom two panels are compared to a baseline of 0–12 months of tenure.

Figure D8: Wage-Tenure Profiles with Occupational Controls

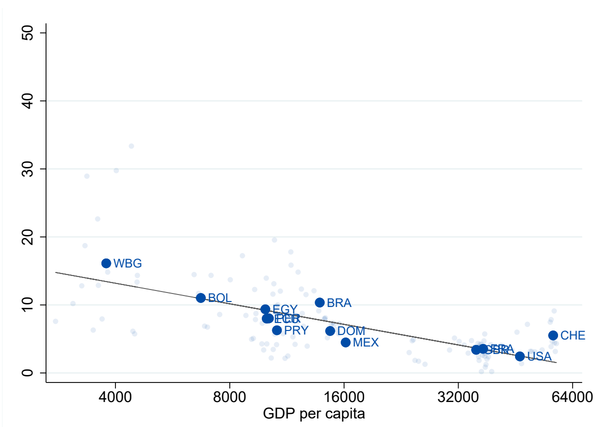
(a) Wage Difference, 6–12 Months of Tenure



(b) Wage Difference, 1–5 Years of Tenure



(c) Wage Difference, 1–5 Years of Tenure



(d) Wage Difference, 5+ Years of Tenure

E Textbook Search and Matching Model

Given that we have constructed the standard flows familiar from search theory, we consider whether the textbook version of this theory provides an obvious explanation for our patterns. We use simplest version of the model from Pissarides (1985). This theory features endogenous match formation but exogenous match destruction, so we focus only on whether it can help us understand cross-country variation in the job-finding rate.

The heart of the theory is the matching function $m(n, v)$ that gives the number of matches formed in a period as a function of the number of non-employed people n and the number of vacancies v. Following standard practice, we assume that this matching function is Cobb-Douglas, $m(n, v) = Mn^{\eta}v^{1-\eta}$. The job-finding rate is then the share of non-employed people who find a job in each period, $m(n, v)/n = M\theta^{1-\eta}$, where $\theta \equiv v/n$ is the market tightness (from the perspective of firms). The parameters M and η are exogenous. The model then has one margin to generate variation in the job-finding rate, which is through market tightness.

All non-employed people are assumed to search for jobs, so variation in market tightness comes from the incentives of firms to post vacancies. The model is set in continuous time, with firms discounting future flows at rate r. Firms pay a flow cost κ to hold a vacancy open and receive flow payoff $x - w$ from a filled position, where x is the value of output, and w is the equilibrium wage. This leads to two value functions for a filled job and a vacancy, J and V respectively:

$$rJ = x - w + \delta(V - J) \tag{6}$$

$$rV = -\kappa + M\theta^{-\eta}(J - V). \tag{7}$$

Firms can enter freely, meaning that the value of posting a vacancy in equilibrium is $V = 0$. This assumption makes it possible to re-arrange the value functions to yield an expression for the job finding rate:

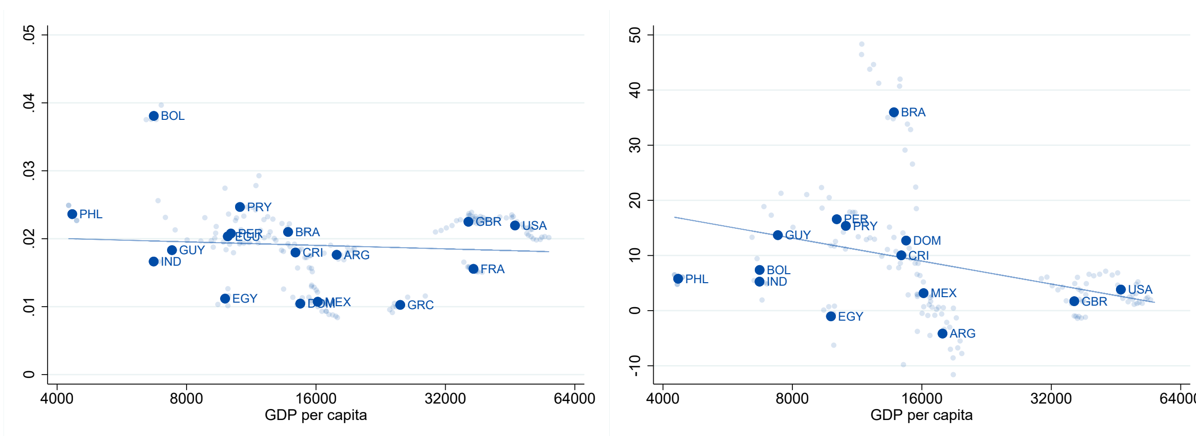
$$\begin{aligned} \text{job finding rate} = M\theta^{1-\eta} &= M^{1/\eta}\kappa^{1-1/\eta}\left[\frac{x-w}{r+\delta}\right]^{(1-\eta)/\eta} \\ = M\theta^{1-\eta} &= M^{1/\eta}\hat{\kappa}^{1-1/\eta}\left[\frac{1-\hat{w}}{r+\delta}\right]^{(1-\eta)/\eta}. \end{aligned} \tag{8}$$

Equation (8) follows after normalizing through by the flow value of output, with $\hat{\kappa} = \kappa/x$ and $\hat{w} = w/x$. It links firms' willingness to post vacancies to match profitability, which depends on wages (relative to output), discount rates, and separation rates.

We explore whether these factors are systematically lower for poor countries, which could explain higher job-finding rates there. In Figure E1a we plot against PPP GDP per capita average wages divided by average GDP per worker, which is our proxy for x. There is no strong trend. The separation rate consists of the employment-exit rate plus the job-to-job transition rate. We have documented that both of these flows are higher in poor countries, which tends to make matches less profitable there. Finally, we assume that firms discount future profits using the interest rate, consistent with standard arbitrage arguments. We plot the real interest rate from World Development Indicators against development in Figure E1b. Interest rates are higher in poor countries, which implies that firms should discount future profits at a higher rate and hence implies matches are less profitable, not more.[27]

Figure E1: Components of Match Profitability

(a) Wages/GDP per worker (b) Interest Rate (Percent)



Altogether, the textbook search and matching theory emphasizes the link between firms' profitability and their willingness to post vacancies, which in turn varies the job-finding rate. Observable indicators suggest matches should be, if anything, less profitable in poor countries; through the lens of the model, this should lead to a lower job finding rate. Thus, the theory is left to appeal to unobservably lower vacancy posting costs $\hat{\kappa}$ or unobservably higher efficiency of the match technology M.[28]

[27]The exact series is FR.INR.RINR. To the extent that firms in poor countries may not have access to credit at these interest rates, they would discount future profits even more, strengthening the result.

[28]Standard logic implies that $\hat{\kappa}$ should be falling with development if it represents physical resources (software for filtering resumes) or constant if it represents worker time (interviewing candidates) - not

rising, as would be required here (Bollard et al., 2016). Martellini and Menzio (2019) provide a theory where the efficiency of the match technology varies endogenously over time.