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THE HUMAN SIDE OF STRUCTURAL TRANSFORMATION

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THE HUMAN SIDE OF STRUCTURAL TRANSFORMATION

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The Human Side of Structural Transformation*

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July 23, 2020

Abstract

We show that the global human capital increase during the 20^{th}-century contributed to structural transformation. We document that almost half of the decline in aggregate agricultural employment was driven by new birth cohorts entering the labor market. We use data on educational attainment and compile a comprehensive list of policy reforms to interpret the differences in agricultural employment across cohorts. We find that the increase in schooling led to a sharp reduction in the agricultural labor supply by equipping younger cohorts with skills more valued out of agriculture. Interpreted through a model of frictional labor reallocation, these facts imply that human capital growth accounts for about 20% of the global decline in agricultural employment.

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1 Introduction

Over the last two centuries, economic development has typically been accompanied by a process of structural transformation: as countries grow richer, workers reallocate from agriculture to manufacturing and services. The literature has emphasized two mechanisms to explain this pattern: a decrease in the relative demand of agricultural goods driven by income effects and an increase in the relative productivity of the agricultural sector (Herrendorf et al., 2014). Both these forces amount to a shift in the relative demand for agricultural labor: keeping fixed their characteristics, workers are progressively more needed in the non-agricultural sector.

However, the labor force itself has been subject to a radical transformation: a global increase in human capital, facilitated by an unprecedented expansion of formal education. In 1950, a 25-year-old man randomly extracted from the world population would have spent, on average, less than four years of his life in a classroom; in 2010, almost nine and a half.[1] If human capital is more useful outside of agriculture, as suggested by the evidence on the sorting of high-skilled workers across sectors, the increase in schooling might have moved the comparative advantage of many workers away from agriculture. In turn, this shift in the relative supply of agricultural labor might have been an important driver of structural transformation. This mechanism, first advanced by Caselli and Coleman II (2001) in the US context, has received little empirical scrutiny in the structural change literature.

This paper formally studies this hypothesis, with the overarching goal of understanding whether increases in human capital have contributed to structural transformation on a global scale. The answer to this question has crucial policy implications. To the extent that schooling is important in this context, educational policy should be considered part of the toolkit of governments wishing to accelerate the process of structural transformation and economic development.

Addressing this question involves a number of challenges. First, several relevant dimensions of human capital are difficult to capture through observable proxies such as years of schooling; for example, schooling quality, educational content and early-childhood human capital accumulation are likely to have changed over time as well. Second, reliable wage data - which could in principle be used to discriminate between demand and supply shifts in the market for agricultural labor - are scarce for developing countries, especially for the agricultural sector. Third, the contribution of human capital growth to structural transformation is likely to be mediated by several general equilibrium effects, which would be missed by a micro-level empirical analysis.

To overcome these difficulties, our analysis combines an empirical cross-cohort analysis of

[1] Authors' calculations using Barro and Lee (2013).

labor reallocation with a two-sector general equilibrium model of structural transformation. Our empirical approach is based on a simple premise: the quantity and quality of education vary across successive cohorts, but they are, for working-age individuals, mostly constant within a cohort over time. This suggests that the extent to which reallocation out of agriculture happens across as opposed to within cohorts is informative on the role of human capital for structural transformation. While changes in the demand for agricultural labor should affect different cohorts similarly, supply shifts due to human capital accumulation should be reflected in differences across cohorts. Building on this insight, we show that the cross-cohort variation in sectoral employment at a given point in time can be used to identify the extent to which changes in human capital have shifted the supply of agricultural labor over time. Through the lens of our model, we then quantify the implications of such shift and the resulting general equilibrium effects for the aggregate rate of structural transformation. We find that the decrease in agricultural labor supply accounts for about 20% of the world decline in agricultural employment.

We start from a simple statistical decomposition of the observed aggregate rate of labor reallocation into changes across and within cohorts. We use repeated cross-sections of micro-level data for 69 countries around the world, covering two thirds of the world population and a large part of the income distribution. We run cohort-level regressions of log agricultural employment on year and cohort dummies and calculate the extent to which changes in the estimated cohort effects for the active cohorts can account for the aggregate rate of reallocation. Naturally, part of the differences in agricultural employment across cohorts might reflect factors associated with age as opposed to fixed cohort-level characteristics; for example, mobility barriers that are likely to be more binding for older workers. To account for this, we also consider a version of our decomposition exercise which separately controls for age and cohort effects.[2]

We find a substantial role for labor reallocation across cohorts. Across countries in our sample, when age effects are controlled for, changes in cohort effects can account on average for about 40% of the overall reallocation out of agriculture. While there is some heterogeneity across countries, the contribution of cohort effects is substantial in the overwhelming majority of them. Overall, our results point towards the importance of cross-cohort changes in workers' characteristics for structural transformation.

We then provide several pieces of evidence to support our interpretation of cohort effects as shifts in human capital. As a starting point, we show that, within each country, faster increases in schooling across cohorts are associated with faster declines in cohort effects. In

[2]The separate identification of year, cohort, and age effects requires at least one linear restriction. We restrict age effects to be zero in the first few years a cohort is active. This restriction is motivated by our model, and captures the idea that mobility costs, as long as they are not too large, affect equally the sectoral decisions of consecutive cohorts at the beginning of their career.

other words, periods with fast improvement in educational attainment have been followed by a large decrease in agricultural employment of the affected cohorts. We then explore, along several dimensions, the determinants of schooling increases to support a causal link between educational attainment and sectoral choices. First, historical data on GDP per capita show that cohorts exposed to economic booms while growing up spend relatively more time in school and have lower estimated cohort effects. Second, we compile a novel dataset on educational reforms and political events (such as independence and democratic transitions) in the countries in our sample. Cohorts exposed to reforms or events that increased their schooling also have lower subsequent agricultural employment. Third, we follow the identification strategy in Duflo (2001), and exploit a school construction program in Indonesia as a shock to educational attainment: the cohorts more affected by the program are less likely to be employed in agriculture. Taken together, the evidence strongly suggests that schooling and educational policy can be important drivers of sectoral reallocation.

We interpret our empirical results through the lens of a general equilibrium model of frictional labor reallocation out of agriculture. The model has three exogenous driving forces: the human capital of new cohorts, the relative sectoral productivity, and a shifter affecting the relative demand for agricultural goods.[3] Workers decide in which sector to work, subject to mobility frictions; firms in both sectors compete for workers. Goods and labor markets clear in equilibrium, determining the relative agricultural price and wage. Human capital is more valued in the non-agricultural sector, which implies that the supply of agricultural labor depends on the average level of human capital of the active cohorts. The demand for agricultural labor, on the other hand, is determined by the relative agricultural revenue productivity. Changes in the relative supply and demand of agricultural labor determine the equilibrium rate of labor reallocation out of agriculture.

The model provides a structural interpretation of our decomposition exercise. First, our theory guides us in the selection of the restriction that we need in order to separately identify cohort, year, and age effects. Mobility costs affect the level of agricultural employment for all cohorts, but - as long as they are not too large - not the rate of reallocation for relatively young cohorts; as a consequence, restricting the age effects to be identical in the first few periods that a cohort is active allows to identify cohort and year effects. Moreover, if the data are generated by our model, cohort effects estimated under this restriction measure the cohort-level average human capital, and the change over time in the average of the estimated cohort effects (for the active cohorts) captures the shift in agricultural labor supply driven by human capital growth. Year dummies absorb changes in the demand for agricultural

[3]In reality, of course, the three forces are jointly determined. For example, higher productivity in human capital intensive sectors might make investment in human capital more valuable. One merit of our methodology is to isolate the contribution of human capital to structural transformation without the need to take a stand on the deep causes of the human capital increase.

labor, while age controls capture the effect of reallocation frictions.

Through the lens of the model, the decomposition results imply a dramatic decline in the agricultural labor supply over time. Keeping prices fixed, this decline explains, on average, almost 40% of aggregate labor reallocation out of agriculture. The general equilibrium impact of this shift is mediated by a combination of the model's parameters - the general equilibrium multiplier - that controls the responsiveness of relative wages and prices. We consider two alternative approaches to quantify the multiplier: calibration and a regression-based exercise exploiting the variation over time in the estimated cohort effects and labor reallocation. In both cases, we conclude that general equilibrium forces attenuate the partial equilibrium impact of human capital growth, which on average accounts for about 20% of the observed rate of reallocation.

Overall, our results show that human capital accumulation dramatically transformed the labor force, shifting labor supply away from agriculture. This shift contributed in a quantitatively important way to the reallocation of employment across sectors. Based on this, we conclude that any credible quantitative analysis of structural transformation cannot fail to consider – as has been mostly done in the literature so far – its "human" side.

Related Literature. We build on the work of Caselli and Coleman II (2001) and Acemoglu and Guerrieri (2008). To our knowledge, Caselli and Coleman II (2001) first argued that the supply of agricultural workers might be relevant to understand structural change. Acemoglu and Guerrieri (2008) build on an insight first proposed by Rybczynski (1955) and formalize the notion that changes in the supply of different inputs may lead to structural transformation if sectors vary in the intensity with which they use them. Our contribution is to develop and apply a methodology to measure changes in the supply of agricultural workers for many countries, link them to changes in schooling, and quantify their aggregate impact. In this sense, we add to a literature studying the quantitative role of changes in the demand for agricultural labor, driven by preferences or technology (Alvarez-Cuadrado and Poschke, 2011; Boppart, 2014; Comin et al., 2015).

More broadly, our work is related to a large literature on the contribution of human capital to growth and development. While most of this literature focuses on the relation between human capital and income per capita (see for example Nelson and Phelps (1966), Barro (1991), Mankiw et al. (1992), and more recently Valencia Caicedo (2018)), we measure the effects of changes in human capital on the supply of agricultural workers and the reallocation of labor out of agriculture. In this context, our cross-cohort analysis quantifies the role of human capital without relying on proxies based on wages or years of schooling, in line with growing evidence that these proxies miss a significant part of the variation in human capital across countries and over time (see Rossi, 2018, for a review).

Our model combines elements and insights already present in Matsuyama (1992b), Lucas (2004), and more recently Herrendorf and Schoellman (2018) and Bryan and Morten (2019). We provide a tractable framework to analytically characterize labor reallocation by cohort in the presence of mobility frictions, which have been shown to affect significantly agricultural workers in developing countries (Ngai et al., 2018). Hsieh et al. (2019) also exploit year and cohort effects to calibrate a model of allocation of talent; compared to their work, we focus on a simpler framework that allows us to analytically consider fixed-cost-type frictions, which turn out to be crucial to correctly identify the role of changes in the supply of agricultural workers. In emphasizing the importance of comparative advantage, our work also relates to Lagakos and Waugh (2013), Young (2013) and Nakamura et al. (2016).

Finally, with respect to the aim of separating the role of labor demand and supply as drivers of sectoral shifts, our paper is closely related to the work of Lee and Wolpin (2006), which devises and structurally estimates a rich model of the process of labor reallocation from manufacturing to services in the United States. We study a conceptually similar question, though in a different context (the transition out of agriculture along the process of development). Moreover, we tackle it from a radically different perspective, imposing the minimal possible structure to interpret patterns of reallocation by cohort. The combination of cohort-level evidence and a model capturing general equilibrium effects makes our work related to a growing literature exploiting micro-level variation to discipline macroeconomic models (Nakamura and Steinsson, 2018).

Structure of the Paper. The paper is organized as follows. Section 2 describes the data, while Section 3 lays out the basic statistical decomposition of aggregate labor reallocation into cohort and year effects. In Section 4 we provide several pieces of evidence on the relationship between schooling and the estimated cohort effects. Section 5 presents the model and Section 6 illustrates the quantitative results. Section 7 concludes.

2 Data

Our main source of data is the Integrated Public Use Microdata Series (IPUMS, see King et al. (2019)). IPUMS data include censuses or large-sample labor force surveys that are representative of the entire population. To improve the coverage of the poorest countries in the world, we supplement IPUMS with the Demographic Health Surveys (DHS, see Boyle et al. (2019)), a collection of small-sample surveys focused on health variables that include information on agricultural employment.

For our benchmark analysis, we include all countries for which we have two or more repeated cross-sections spanning at least ten years, with available information on industry of employment for men aged 25 to 59.[4] We focus on this age range to capture working-age individuals with completed education, and exclude women from the analysis given that their low labor force participation in many countries makes it difficult to properly compute the cohort-level reallocation across sectors.[5] This gives us a sample of 58 countries and 241 cross-sections, covering more than two thirds of the world population, five continents and most of the income distribution. The IPUMS data include 52 countries, of which 9 are high-income, 24 are middle-income and 19 are low-income; all the 12 countries in the DHS data are low-income (some countries are in both IPUMS and DHS).[6] On average, we observe countries over a period of 27 years in the IPUMS data and of 15 years in the DHS data. For robustness, we also consider an extended sample including all countries with cross-sections spanning at least five years and industry information for men aged 25 to 54; this gives us 2 more middle-income countries in the IPUMS data and 13 more low-income countries in the DHS data, for a total of 69 countries and 285 cross-sections.

Our key variable of interest is agricultural employment at the cohort level. We use the variables *indgen* (IPUMS) and *wkcurrjob* (DHS), which are harmonized across countries and time periods, to compute the share (properly weighted) of the male population employed in the industry "Agriculture, fishing and forestry".[7] Figure A.IIIa shows, for each country, the average number of observations at the cohort $\times$ year level. For almost all countries in IPUMS, we have at least 1000 observations per cell. Sample sizes in the DHS data are much smaller. For this reason, we use the 52 countries in IPUMS as our core sample, and report results from the DHS as robustness checks.[8]

We subject our data to three consistency checks. First, we inspect visually, for all countries, the growth rates in aggregate agricultural employment between cross-sections, searching for anomalies. This procedure leads us to exclude one observation from the IPUMS data, and two from the DHS.[9] Second, we inspect visually the cross-sectional relationships between agricultural employment and birth year. We exclude ten cross-sections from the

[4]We exclude cross-sections for which information on industry is missing (which is always the case for the not employed) for more than 25% of men aged 35 to 45. Figure A.I shows that this restriction excludes only very few cross-sections. All the figures and tables labeled A. are included in the Online Appendix.

[5]As Figures A.IIa and A.IIb show, the average employment rate of men aged 25-59 is high and constant.

[6]By high-income (low-income) countries we mean those with GDP per capita greater (smaller) than 45% (10%) of the one of the United States at PPP, in 2000. We use GDP per capita from the Maddison Project Database. Data for Fiji is missing; we assign it to the low-income countries. Puerto Rico is a territory, but we label it a country.

[7]In computing the agricultural employment share, we do not restrict the sample to individuals in the labor force. As we highlight in Figure A.IIc, we do not want to confound entry into the labor force with reallocation out of agriculture. We consider alternatives for robustness.

[8]In several countries, we observe age heaping. We use a standard procedure, illustrated in Figure A.IV, to get a smooth distribution of agricultural employment as a function of age.

[9]We drop the reallocation between 2000 and 2005 for the United States (which corresponds to a change in the sectoral classification) and the reallocation between 2015 and 2016 and between 2016 and 2017 for Senegal. The details are in Figure A.VI and notes.

DHS data that display very large swings across birth cohorts, casting doubts on data reliability.[10] Finally, we verify that the average agricultural employment computed in our final sample is comparable in magnitude with aggregate data from the World Development Indicators (see Figure A.V) – a commonly used data source (Herrendorf et al., 2014).

3 Decomposing Structural Change

We study patterns of labor reallocation out of agriculture by birth cohort. While most of the existing work focuses on aggregate rates of reallocation, we are among the first to systematically document micro-level evidence on the behavior of different cohorts in the process of structural transformation.[11]

3.1 Cohort and Year Components of Labor Reallocation

In each country j, for each cross section t, and for each cohort c, we compute the share of the population in agriculture, $l_{A,t,c,j}$. We normalize c to be equal to the birth year plus 25, so that a cohort first enters into our dataset when $c = t$ and is last in the dataset when $c = t + N$, where $N = 59 - 25 = 34$. The overall share of the population employed in agriculture is given by

$$L_{A,t,j} = \sum_{c=t-N}^{t} n_{t,c,j} l_{A,t,c,j},$$

where $n_{t,c,j}$ is the share of the overall male population aged 25 to 59 belonging to cohort c. Our objective is to decompose changes over time in $L_{A,t,j}$ into a component that captures country-wide trends, and a component that captures changes in the composition of the active labor force.

A Graphical Inspection. As an illustration, we regress, separately for high-, middle-, and low-income countries, $\log l_{A,t,c,j}$ on country fixed effects and dummies that take value one for each decade from 1960 to 2010. Figure Ia plots the resulting decade effects, normalized to the average agricultural employment share in the sample. The figure shows two well-known facts: (i) high-income countries have lower agricultural employment; and (ii) labor has reallocated away from agriculture. It also shows that the share of agricultural employment declined at a log-linear rate, a feature of the data that we leverage in the model.

Next, we run the same specifications, but adding a full set of birth-year dummies. Figure

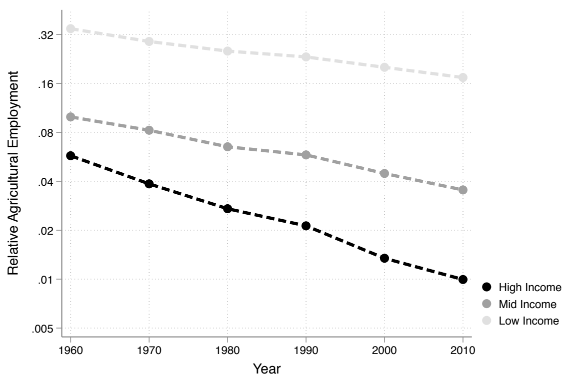
[10] For most countries, the first available cross-section from the DHS data is extremely noisy. Cote d'Ivoire has only two cross-sections, hence excluding the first one leads us to exclude Cote d'Ivoire altogether. The plots of all the omitted cross-sections are in Figure A.VII.

[11] Kim and Topel (1995), Lee and Wolpin (2006), and Perez (2017) document sectorial reallocation by cohort, but limit their focus to, respectively, South Korea, United States and Argentina. In ongoing work, Hobijn et al. (2019) are also using the IPUMS dataset to document patterns on reallocation by cohort.

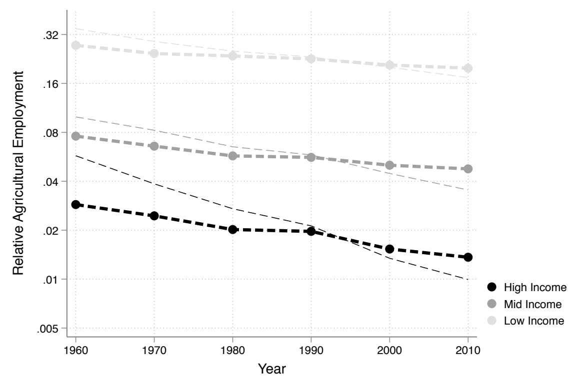
Ib shows that, when controlling for cohort effects, the estimated decade dummies decline at a much slower rate. The decline in agricultural employment obtained by following a given birth cohort over time is approximately half of the aggregate decline. This is because the aggregate decline is partly driven by compositional changes, as showed in Figure Ic: younger birth cohorts have a lower share of agricultural employment in any given year. We also notice that, especially in middle- and low-income countries, the relationship between birth year and agricultural employment is steeper for cohorts born after 1940. We will return to this fact later.

Figure I: Decomposing Labor Reallocation

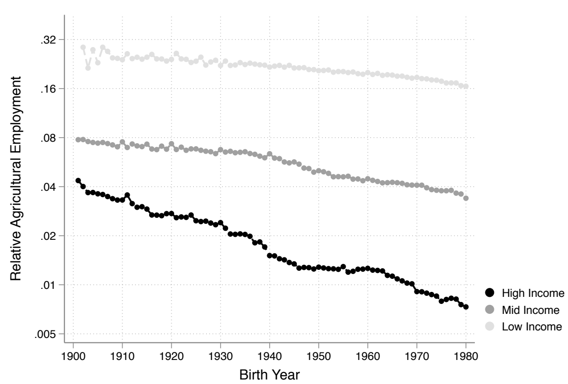
(a) Year Effects Only



(b) Year Effects, Controlling for Cohort Effects



(c) Cohort Effects, Controlling for Year Effects



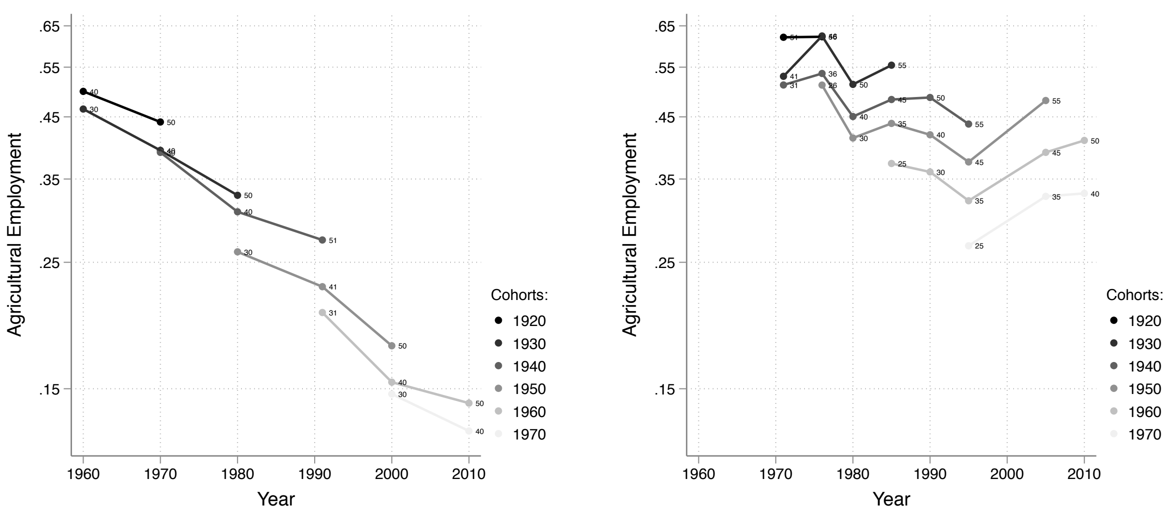
Notes: the Figures show the point estimates for year and cohort effects, renormalized to average to the overall agricultural employment in our samples. Figure Ib includes for comparison purposes the estimates in Figure Ia (lighter lines). The y-axis is on a log scale.

Taken together, Figures Ia and Ib highlight that aggregate structural transformation is the result of two equally important mechanisms: (i) over time, individuals of all birth cohorts move away from agricultural employment – we call this the *year component* of labor reallocation, since it captures country-wide trends; (ii) younger cohorts that enter the labor market are less likely to be employed in agriculture – we call this the *cohort component* of labor reallocation, since it captures changes in the composition of the active labor force.

Two examples. To further illustrate the role of year and cohort components in driving aggregate reallocation, Figures IIa and IIb plot agricultural employment by cohort for two countries. In Brazil, the year component largely drives aggregate reallocation: within each given cohort, a large share of individuals reallocates out of agriculture over time. In Indonesia, the cohort component plays a more important role: there is no systematic within-cohort time trend in agricultural employment, and, in any given year, younger cohorts are less likely to work in agriculture. As younger cohorts enter the labor market and older ones exit, aggregate agricultural employment decreases as a result.

Figure II: Labor Reallocation By Cohort, Two Examples

(a) Brazil (b) Indonesia



Notes: the Figures plot agricultural employment by birth cohort in Brazil and Indonesia. We follow six birth cohorts between the ages of 25 and 59, or as long as we observe them in our data. We highlight with solid dots the years in which we observe agricultural employment. The ages of all cohorts in any observed year are reported.

Formal decomposition. We regress separately for each country the cohort-level agricultural employment on year and cohort effects,

$$\underbrace{\log l_{A,t,c,j}}_{\text{AGR SHARE OF COHORT } c \text{ AT TIME } t} = \underbrace{\mathbb{Y}_{t,j}}_{\text{YEAR EFFECTS}} + \underbrace{\mathbb{C}_{c,j}}_{\text{COHORT EFFECTS}} + \varepsilon_{t,c,j}. \tag{1}$$

and use the resulting estimates to unpack the aggregate rate of labor reallocation into year and cohort components.[12] The average yearly rate of labor reallocation between periods t

[12]We estimate equation (1) in first differences to provide a tight mapping with the model in section 5.

and $t+k_{t,j}$ for country j is

$$\log g_{L_A,t,j} \equiv \frac{1}{k_{t,j}} \log \frac{L_{A,t+k_{t,j}}}{L_{A,t}},$$

where we define $k_{t,j}$ as the number of years between cross-section t and the next cross-section in our data. We can write $\log g_{L_A,t,j}$ as

$$\underbrace{\log g_{L_A,t,j}}_{\text{RATE OF LABOR REALLOCATION}} = \underbrace{\log \psi_{t,j}}_{\text{YEAR COMPONENT}} + \underbrace{\log \chi_{t,j}}_{\text{COHORT COMPONENT}} \tag{2}$$

where

$$\log \psi_{t,j} \equiv \frac{1}{k_{t,j}} \left(\mathbb{Y}_{t+k,j} - \mathbb{Y}_{t,j}\right) \tag{3}$$

$$\log \chi_{t,j} \equiv \frac{1}{k_{t,j}} \log \left(\frac{\sum_{c=t+k_{t,j}-N}^{t+k_{t,j}} n_{t+k,c,j} \exp\left(\mathbb{C}_{c,j}\right)}{\sum_{c=t-N}^{t} n_{t,c,j} \exp\left(\mathbb{C}_{c,j}\right)} \right) = \log g_{L_A,t,j} - \log \psi_{t,j}. \tag{4}$$

The year component $\log \psi_{t,j}$ is the difference between the year effects at time t and $t+k_{t,j}$, while the cohort component $\log \chi_{t,j}$ captures changes in the average cohort effects of the active cohorts. We compute $\log \psi_{t,j}$ and $\log \chi_{t,j}$ for each pair of cross-sections and calculate their average as

$$\log \psi_j = \tfrac{1}{|\mathbb{T}_j|} \textstyle\sum_{t \in \mathbb{T}_j} \log \psi_{t,j}, \qquad \log \chi_j = \tfrac{1}{|\mathbb{T}_j|} \textstyle\sum_{t \in \mathbb{T}_j} \log \chi_{t,j}$$

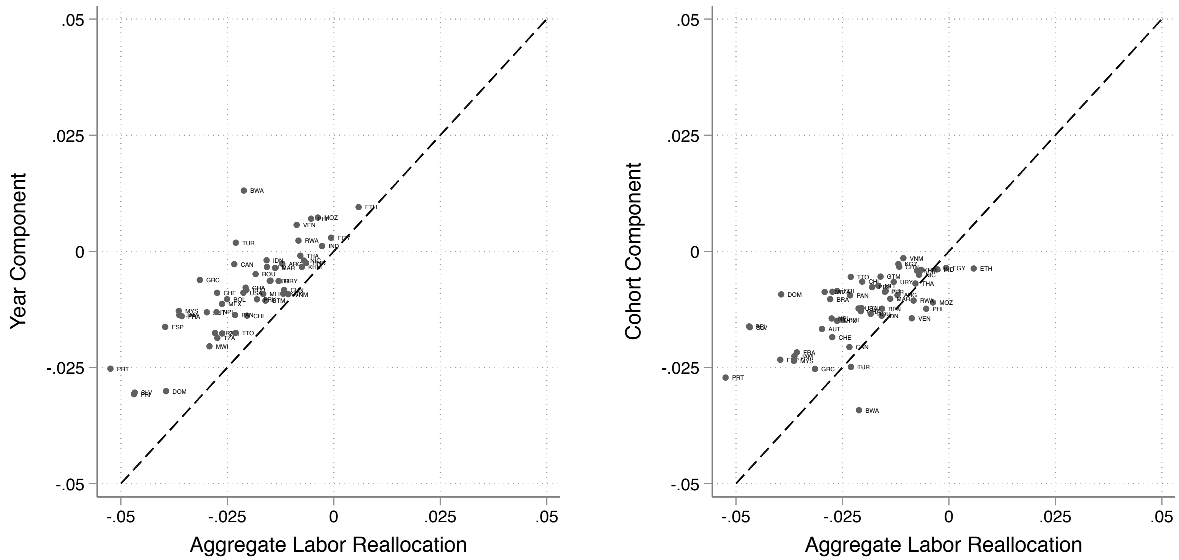
where $\mathbb{T}_j$ is the set of all cross-sections available for country j excluding the most recent one, for which we cannot calculate the reallocation rate. The decomposition of the average reallocation rate between $\log \psi_j$ and $\log \chi_j$ summarizes the patterns of reallocation by cohort shown, for example, in Figure II: the year component $\log \psi_j$ is the average slope of the cohorts' paths, while the cohort component $\log \chi_j$ is the average vertical gaps across cohorts, properly annualized.

Figures IIIa and IIIb plot the year and cohort components against the average reallocation rate. For the overwhelming majority of countries, both the year and the cohort components are negative, hence they contribute to aggregate labor reallocation. Furthermore, countries with faster reallocation have usually larger (in absolute value) year and cohort components, although the year components explain a larger share of cross-country variance.[13]

[13]Figure A.VIII shows that the results are similar if we treat each cross-section as an independent observation – i.e. if we plot $\log \psi_{t,j}$, and $\log \chi_{t,j}$ as a function of $\log g_{L_A,t,j}$.

Figure III: Unpacking Aggregate Labor Reallocation

(a) Year Components (b) Cohort Components



Notes: the left Figure plots, across countries, the year component as a function of the reallocation rate. The right Figure plots the cohort component as a function of the reallocation rate.

Table I summarizes the decomposition results. Across all countries, the agricultural employment declines on average by 2.05% each year, of which 0.83% is due to the year component. Therefore, as showed in column 4, 60% of the aggregate reallocation is due to the cohort component. The contribution of the cohort component to aggregate reallocation is similar for all income groups. Table A.IV shows the results for each country in our sample.

Table I: Unpacking Structural Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Country Type	$\log g_{L_A}$	$\log \psi$	$\log \tilde{\psi}$	$\frac{\log \chi}{\log g_{L_A}}$	$\frac{\log \tilde{\chi}}{\log g_{L_A}}$	$1-\frac{\log \psi}{\log \tilde{\psi}}$	N. Obs
All	-2.05	-0.83	-1.27	0.60	0.38	0.28	52
High Income	-3.42	-1.40	-1.44	0.59	0.58	0.02	9
Middle Income	-2.12	-0.88	-1.55	0.58	0.27	0.32	24
Low Income	-1.30	-0.51	-0.83	0.61	0.36	0.39	19

3.2 Controlling for Age

The statistical decomposition considered so far restricts age to have no effect on the cohort-level agricultural share. However, older workers plausibly face stronger barriers to reallocate across sectors, limiting their labor mobility over time. In the absence of age controls, this will contribute to a large role of cohort effects for the aggregate rate of labor reallocation. Therefore, we next include age controls in the previous decomposition.

It is well known that year, cohort and age are collinear, and can be separately identified only if an additional linear restriction is imposed.[14] Our restriction is that age has no effect in the first few years a cohort is employed. This choice is guided by theory, and will be fully motivated in the context of our model in Section 5. Intuitively, this amounts to assuming that frictions to labor reallocation affect equally consecutive cohorts at the beginning of their working career.

In the implementation of this idea, we face a trade-off between the parametrization of age effects and the sample size for the identification of year effects. At one extreme, a specification including a full set of age dummies, with the coefficients on the first two restricted to be equal to each other, would identify year effects out of the reallocation behavior of one cohort only. To strike a balance between the two sides of this trade-off, we follow Card et al. (2013) and include quadratic and cubic terms for age, centered around a value $\bar{a}$ to be specified below. Separately for each country j, we run

$$\log l_{A,t,c,j} \;=\; \tilde{\mathbb{Y}}_{t,j} + \tilde{\mathbb{C}}_{c,j} + \beta_{1,j}\left(a_{c,t,j} - \bar{a}\right)^2 + \beta_{2,j}\left(a_{c,t,j} - \bar{a}\right)^3 + \varepsilon_{t,c,j}, \tag{5}$$

where $\tilde{\mathbb{Y}}_{t,j}$ and $\tilde{\mathbb{C}}_{c,j}$ denote year and cohort dummies, and $a_{c,t,j}$ is the age of cohort c at time t (for country j). This specification restricts age effects to be 0 at age $\bar{a}$, both in levels and in changes.[15] Since our data come from repeated cross-sections that are on average - across all countries and time periods - 8.8 years apart, we set $\bar{a} = 29.4$, i.e. the average age of the youngest cohort that we observe for at least two successive cross-sections. We explore several alternative specifications for age effects in Table A.VII, including country-specific values for $\bar{a}$, more flexible age dummies, and time-varying age controls; the results are similar to those shown below.

Given the estimates from specification (5), we compute, just as in Section 3.1, the annualized year and cohort components

$$\begin{aligned} \log \widetilde{\psi}_{t,j} &\equiv \frac{1}{k_{t,j}}\left(\widetilde{\mathbb{Y}}_{t+k,j} - \widetilde{\mathbb{Y}}_{t,j}\right) \\ \log \widetilde{\chi}_{t,j} &\equiv \log g_{L_A,t,j} - \log \widetilde{\psi}_{t,j} \end{aligned}$$

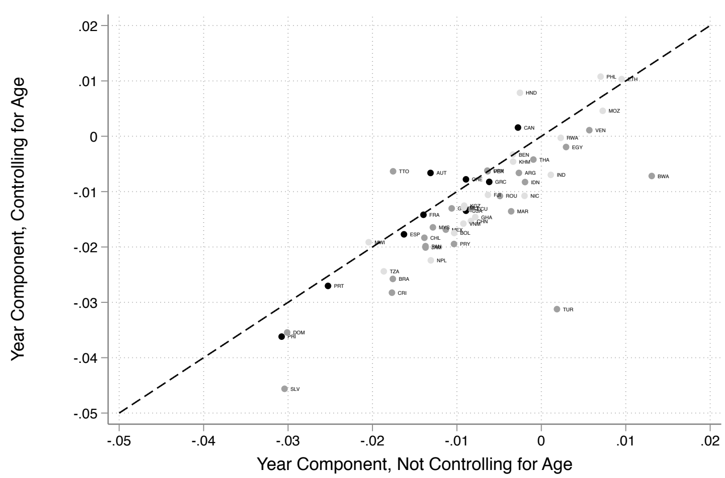
and take their average across all available cross-sections, $\log \widetilde{\psi}_j$ and $\log \widetilde{\chi}_j$.

[14] See Deaton (1997), and more recently Lagakos et al. (2018).

[15] In fact, the omission of a linear term for age is necessary to have the derivative of the age terms to be zero at $\bar{a}$, which is needed for identification of the year trend.

Figure IV plots $\log \widetilde{\psi}_j$ as a function of $\log \psi_j$. Controlling for age matters: almost all countries lie below the 45-degree line, which means that the year components estimated with age controls is larger (in absolute value). Younger birth cohorts, which are less likely to be constrained by mobility frictions, reallocate across sectors at a faster rate. However, even conditional on age, the cohort component of aggregate reallocation is still substantial. Column 3 of Table I shows that the average year component $\log \widetilde{\psi}_j$ is -1.3%, which implies that the cohort component still explains almost 40% of the total reallocation out of agriculture, as shown in column 5.[16] Table I and Figure IV also show that controlling for age effects has a larger impact on the estimated cohort components for middle- and low-income countries. We will return to this result in Section 5, where we show that the ratio between $\log \widetilde{\psi}_j$ and $\log \psi_j$ directly maps into the structural parameters modulating mobility frictions.

Figure IV: Age Controls and Reallocation Frictions



Notes: the Figure plots the year component estimated with age controls – $\log \widetilde{\psi}_j$ – as a function of the year component estimated without age controls – $\log \bar{\psi}_j$. The markers are black for high income countries, gray for middle income countries, and light gray for the low income countries. The 45-degree line shows that in most countries $\log \widetilde{\psi}_j$ is larger than $\log \bar{\psi}_j$ in absolute value.

4 Understanding Cohort Effects: Evidence from Schooling Data

We have shown that cohort effects explain a large share of aggregate labor reallocation. Given the same aggregate conditions, younger birth cohorts are less likely to work in agriculture: the data reveal that they have a comparative advantage for non-agriculture. What determines the shift across cohorts in comparative advantage? This section provides several pieces of evidence to support the interpretation of cohort effects as shifts in human capital.

[16] Table A.VII shows that the results are similar when focusing on the DHS sample.

4.1 Correlation between Schooling and Cohort Effects

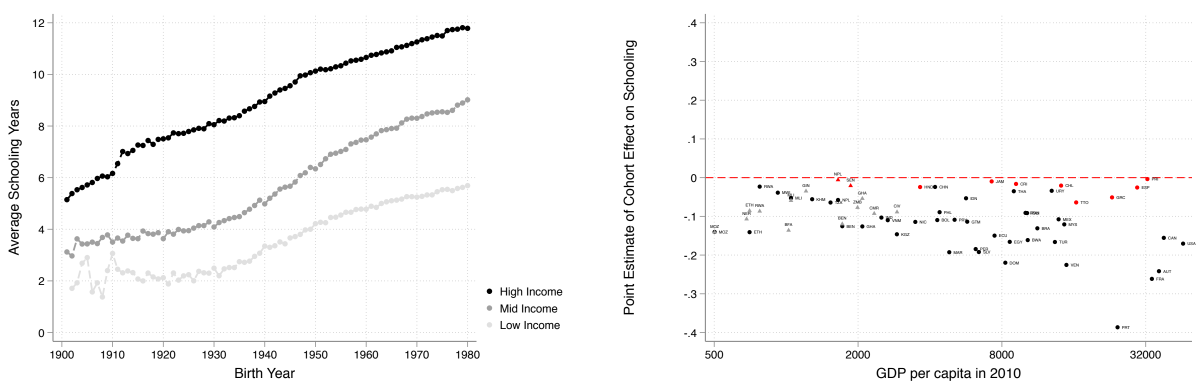
We start by documenting that the cohort effects estimated in Section 3 are correlated with cohort-level educational attainment. We use individual-level educational attainment to compute the average schooling years for each cohort in our dataset. Since we observe cohorts in multiple cross-sections, we extract average schooling by cohort using, separately for each country, a procedure similar to the one used in DeLong et al. (2003) for the United States. More specifically, we project the log of cohort-level average schooling years on a full set of cohort dummies and a cubic polynomial in age, which controls for late enrollment in school (i.e. after 25 years of age) and, especially, mortality and morbidity differences by education groups. We transform the estimated cohort dummies in levels, and denote the resulting schooling level for cohort c in country j as $s_{c,j}$.

As a first step, Figure Va replicates Figure Ic, but using schooling rather than agricultural employment. The relationship between years of education and birth cohorts mirrors the one for agricultural employment. Schooling increased across the world, and, especially in low- and middle-income countries, this increase was faster for the cohorts born after 1940. A visual comparison of the two Figures suggests that the schooling increase might have played a role in shaping the comparative advantage of younger generations. At the same time, the comparison might be confounded by several factors; most obviously, similar time trends in both variables.

Figure V: Educational Attainment and Agricultural Employment

(a) Schooling by Birth Cohort

(b) Regression of Cohort Effects on Schooling



Notes: the left figure replicates Figure Ic using cohort-level schooling rather than agricultural employment. The right figure plots for each country the point estimate of $\hat{\beta}_j$ from specification (6). Black circles and gray triangles are for IPUMS and DHS countries for which $\hat{\beta}_j$ are negative and significant at 5%. Observations in red are not significantly different from 0. We exclude from the figure the highest and lowest observations: $\hat{\beta}_{ARG} = 0.307$ and $\hat{\beta}_{CHE} = -0.462$.

To make progress, we estimate specifications that control for quadratic time trends. We run, separately for each country,

$$\tilde{\mathbb{C}}_{c,j} = \alpha_j + \beta_j s_{c,j} + \delta_{1,j} (c - \bar{c}_j) + \delta_{2,j} (c - \bar{c}_j)^2 + \varepsilon_{c,j}, \tag{6}$$

where $\tilde{\mathbb{C}}_{c,j}$ is the cohort effect estimated in (5), and $\bar{c}_j$ is the first cohort that we observe in each country. The coefficient of interest is β_j; Figure Vb plots it as a function of GDP. For all countries but one, the coefficient is negative: cohorts that are more educated, relative to a country-specific quadratic trend, are less likely to work in agriculture. While the coefficient β_j is negative and significant in almost all countries, there is some heterogeneity; in particular, one extra year of schooling in rich countries appears to have a larger effect on agricultural employment.[17] To focus on one magnitude, we run specification (6) pooling all countries together, allowing for country-specific time trends. The first column of Table II reports the results: one additional year of schooling decreases cohort-level agricultural employment by approximately 10% relative to what it woud have been otherwise. The result is robust to the inclusion of decade of birth dummies interacted by income group (column 2).

Table II: Role of Schooling

	Dependent Variable: Cohort Effect					
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Schooling	-0.104	-0.113	-0.099	-0.087	-0.170	-0.185
	(0.004)	(0.004)	(0.017)	(0.022)	(0.039)	(0.038)
Country Trend	YES	YES	YES	YES	YES	YES
Country FE	NO	YES	YES	YES	YES	YES
Birth-Year Controls	NO	YES	NO	YES	NO	YES
Method	OLS	OLS	IV	IV	IV	IV
Instrument	-	-	GDP Cycle	GDP Cycle	Edu Reforms	Edu Reforms
F Stat First Stage	-	-	14.50	6.20	2.75	2.24
Observations	3238	3238	2778	2778	907	907

Notes: Robust standard errors in parentheses. The country trends include both linear and quadratic terms. Birth-year controls are a full set of decade of birth dummies interacted with income group dummies.

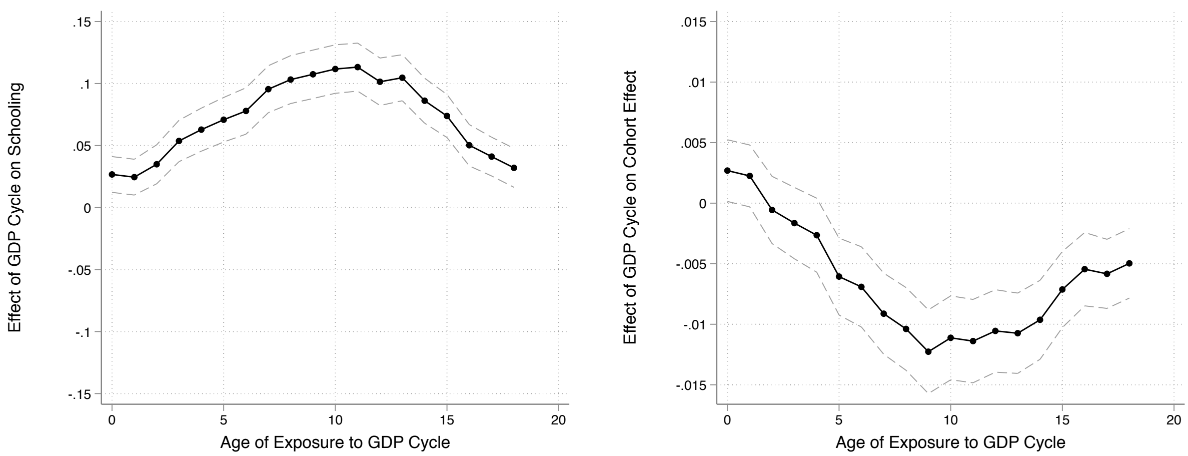
We should be cautious in interpreting this relationship as causal. In particular, cohort-level average schooling might be correlated with other cohort-level characteristics that affect

[17]These patterns hold for DHS countries as well, identified by triangles in Figure Vb: the estimates of β_j are negative and mostly significant, and smaller in magnitude compared to those of richer IPUMS countries.

sectoral choices, such as average early-childhood human capital investments, ability or preferences. Moreover, if educational decisions are forward-looking, changes in schooling might be ultimately driven by the anticipation of higher demand for non-agricultural labor (relative to the quadratic trend).[18] While most of these possibilities are broadly consistent with the core thesis of this paper - i.e. human capital being an important driver of the supply of agricultural labor - establishing whether schooling plays an independent and direct role is important, as formal educational attainment is, at least partially, under the direct influence of educational policy. In the rest of this section, we present several approaches to make progress in this direction.

Figure VI: Effect of GDP at birth on Agricultural Employment and Schooling

(a) Years of School | (b) Cohort Effects



Notes: the left Figure shows the point estimates and 90% confidence intervals for γ_x from the regression $s_{c,j} = \psi_j + \delta_{1,j}(c - \bar{c}_j) + \delta_{2,j}(c - \bar{c}_j)^2 + \sum_{x=0}^{18} \gamma_x y_{x,j} + \varepsilon_{c,j}$, where ψ_j is a country fixed effect and $y_{x,j}$ are the cyclical components of GDP per capita at birth, and at ages 1 to 18 experienced by cohort c in country j. The right Figure shows the corresponding estimates from a regression with the estimated cohort effects $\tilde{\mathbb{C}}_{c,j}$ rather than schooling $s_{c,j}$ as left hand side variable.

4.2 Persistent Effects of Growing up in a Recession

As a first exercise, we isolate the variation in human capital driven by cyclical economic conditions during youth, which should be plausibly orthogonal to the variation in the relative demand for agricultural labor during adulthood. We use historical GDP data from Maddison (2003), which we filter using an HP filter. For each country and birth cohort, we compute

[18]Notice that these concerns are different (and, arguably, less severe) compared to those typically faced by individual-level analyses of returns to education. In particular, individual-level selection in terms of omitted characteristics is not problematic per se, as long as the cohort-level distribution of these characteristics does not vary over time. Moreover, recall that cohort effects are estimated conditional on year effects, therefore controlling for aggregate economic conditions.

19 variables corresponding to the cyclical components of GDP per capita at birth, and at ages 1 to 18. We then run, pooling together all countries, two separate regressions – one for schooling $s_{c,j}$, one for the cohort effects $\tilde{\mathbb{C}}_{c,j}$ – on these 19 variables, controlling for country-specific quadratic trends as in specification (6) and for country fixed effects.

Figures VIa and VIb show the point estimates of the effect of exposure to relatively high GDP on schooling and cohort effects. Cohorts that have been exposed to relatively favorable economic conditions while growing up spend more time in school and have – 15 years later or more – a lower agricultural employment share. Moreover, for both schooling and cohort effects, the estimates are larger at the children's ages when parents need to decide whether to keep them in school. These results are consistent with the hypothesis that children's education is a normal good and that it shapes the comparative advantage for non-agriculture.

While exposure to relatively high GDP during youth might affect human capital formation through mechanisms other than schooling, we run an IV specification to give a sense of the magnitude of the implied causal relationship, if we are willing to assume one. We estimate (6), pooled for all countries, using the cyclical components of GDP per capita during youth as instruments for schooling. The results are reported in columns (3) and (4) of Table II; the point estimates are similar to the OLS ones.[19]

4.3 Educational Reforms and Political Events

Next, we focus on the cross-cohort variation in educational attainment induced by the timing of large country-wide shocks to the educational system. We compile a novel dataset of educational reforms and political events for the countries in our sample. We find a total of 33 policy reforms extending compulsory education, and 80 political events such as independence from colonial powers, transitions to democracy and wars that plausibly impacted (either positively or negatively) the working of the educational system or the costs of acquiring education. We further use historical sources to identify educational reforms that were either not fully implemented due to low state capacity, limited to some regions or phased in slowly over time; 13 out of 33 reforms fall within this category (we refer to them as "weakly-implemented"). Appendix C describes the details of the data construction.

Two examples. Figures VIIa and VIIb use data from two countries, France and Mozambique, to illustrate the type of empirical variation that we use in this section. Both figures show the evolution of the estimated cohort effects and schooling across the cohorts in our sample; the vertical lines highlight the oldest cohort not yet in school when a reform or

[19]The number of observation declines slightly relative to columns (1) and (2), because we don't have available GDP data for all cohorts.

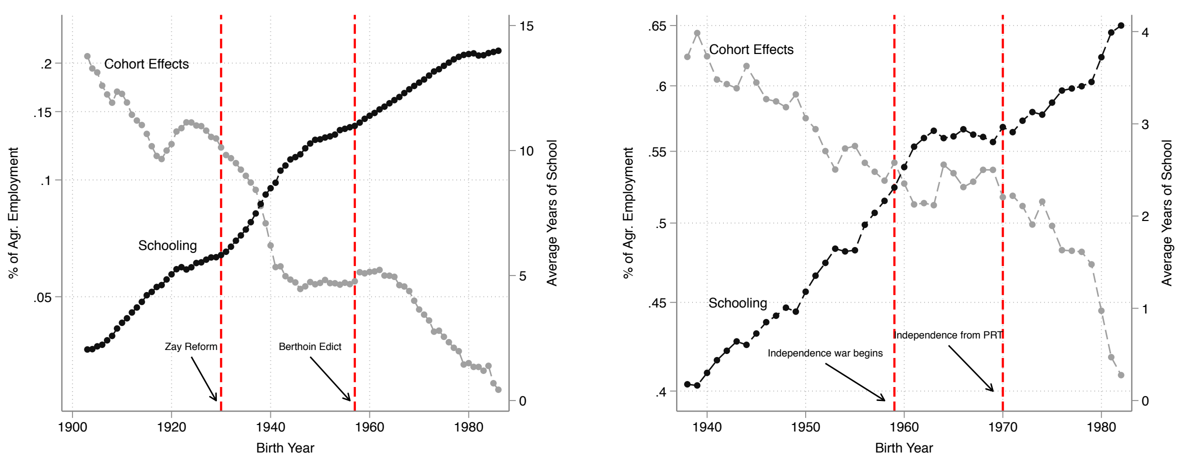
political event takes place.

In France, the Zay Reform of 1936 increased compulsory education for all children to the age of 14, and the Berthoin Edict of 1967 increased it further to the age of 16.[20] Figure VIIa shows trend breaks around the first cohorts affected by the reforms: while there is an overall trend in schooling and agricultural employment across cohorts, individuals that were not yet in school at the time the reform was implemented see a larger increase in schooling and a larger decrease in cohort effects.

Mozambique fought an independence war from Portugal between 1964 and 1975. The war disrupted the educational system, as confirmed by the stagnating educational attainment for the cohorts of schooling age at that time. After independence, the Mozambique Liberation Front led extensive programs for economic development, including free healthcare and education; this is reflected in the faster schooling growth for cohorts born after 1970. Figure VIIb shows that, as for the previous case, the estimated cohort effects mostly mirror the schooling data.

Figure VII: Trend Breaks around Education Reforms and Political Events, Two Countries

(a) Schooling Reforms in France (b) Political Events in Mozambique



Notes: the Figures plot the cohort effects $\tilde{\mathbb{C}}_{c,j}$ estimated from specification (5) (left y-axis) and cohort schooling $s_{c,j}$ for all available birth cohorts (right y-axis). The red vertical lines report the first birth cohorts that are affected by the corresponding policy reform and political event.

All reforms and political events. We now apply a similar graphical analysis to the whole sample. For each policy reform or political event r, we denote as $\bar{c}_r$ the oldest cohort not yet in school at that time. We then compute, for both cohort effects and schooling, the

[20]Of course, due to its timing, the effect of the Zay Reform might be confounded with the effect of World War II. To alleviate this type of concerns, one of the specifications we present below includes decade of birth dummies.

difference between the annualized growth across the cohorts born in 10-year windows before and after $\bar{c}_r$,

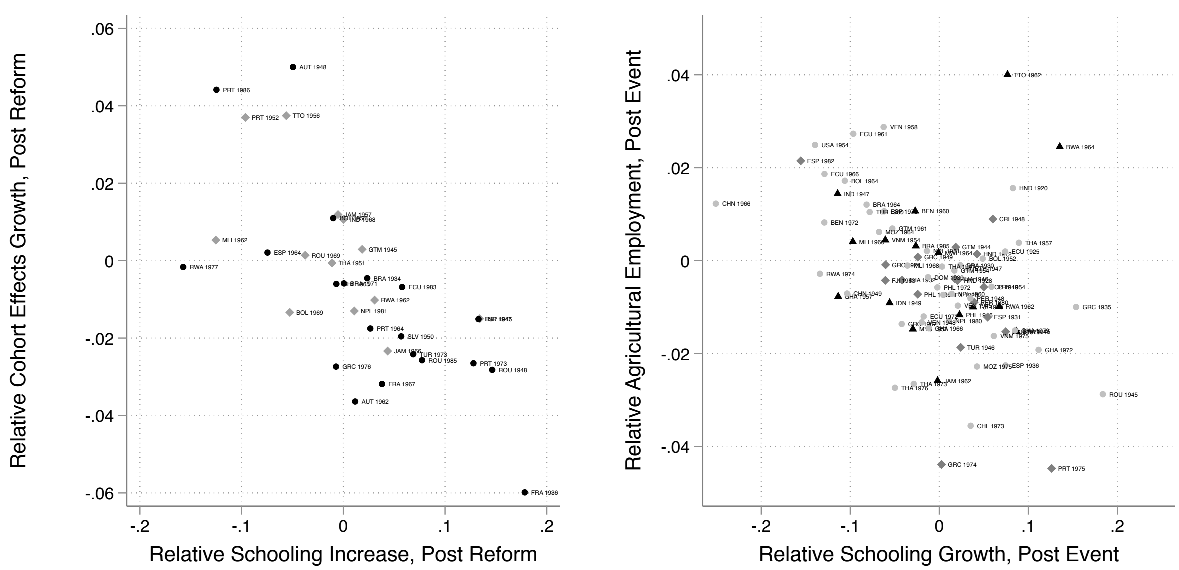
$$\mathbb{A}_r \equiv \frac{1}{10}\left(\tilde{\mathbb{C}}_{\bar{c}_r+10} - \tilde{\mathbb{C}}_{\bar{c}_r}\right) - \frac{1}{10}\left(\tilde{\mathbb{C}}_{\bar{c}_r-1} - \tilde{\mathbb{C}}_{\bar{c}_r-11}\right) \tag{7}$$

$$\mathbb{S}_r \equiv \frac{1}{10}\left(s_{\bar{c}_r+10} - s_{\bar{c}_r}\right) - \frac{1}{10}\left(s_{\bar{c}_r-1} - s_{\bar{c}_r-11}\right) \tag{8}$$

and plot $\mathbb{A}_r$ and $\mathbb{S}_r$ against each other.[21] Figures VIIIa and VIIIb show that when a reform or political event was followed by a positive trend break in schooling – i.e. by a faster increase in schooling for the affected cohorts – it was also followed by a negative trend break in cohort effects. In other words, the negative comovement that we have shown in Figure VII for two countries generalizes to the whole dataset.

Figure VIII: Trend Breaks around Education Reforms and Political Events, All Episodes

(a) Schooling Reforms (b) Political Events



Notes: the Figures plot the changes in the growth rate of cohort effects after a reform or political event, $\mathbb{A}_r$, against the corresponding changes for schooling, $\mathbb{S}_r$, as defined in equations (7) and (8). The left Figure shows the education policy reforms, while the right Figure shows the political events. On the left, the gray diamonds identify the "weakly-implemented" reforms. On the right, the black triangles identify independence from colonial rulers, the dark gray diamonds transitions to democracy, and the light gray circles all other political events.

Figure VIIIa also highlights different patterns between fully-implemented and weakly-implemented educational reforms. Most of the fully-implemented reforms lie on the bottom

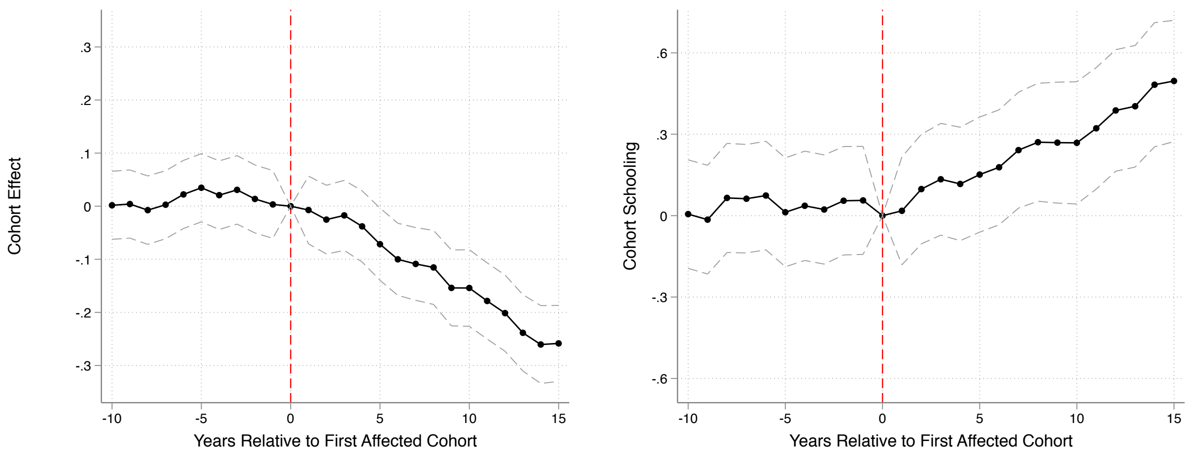
[21] We consider a variant to this exercise in Figure A.IX, where we compute $\mathbb{A}_r$ and $\mathbb{S}_r$ as the differences between the average annualized growth rates across all cohorts within the two 10-year windows; the results are very similar.

right quarter of the graph, meaning that they are associated with positive changes in schooling and negative changes in cohort effects; the pattern for the weakly-implemented reforms (as well as for political events) is less clear: some of them are followed by positive and others by negative trend breaks.[22] In light of this, we next focus on the fully-implemented reforms to quantify the impact of the associated schooling increase on the estimated cohort effects.

Figure IX: Event Study of Education Reforms on Agricultural Employment and Schooling

(a) Cohort Effects

(b) Years of School



Notes: the left Figure shows the point estimates and 90% confidence intervals for $\mathbb{I}_x$ from specification (9). The right Figure shows the point estimates for the same specification but using cohort effects on the left hand side. The red line highlights the last cohort not affected by the policy reform.

An event study design. We implement an event study design around the first cohort affected by the increase in compulsory education. For each policy reform r, we keep 10 cohorts older and 15 cohorts younger than $\bar{c}_r$. We detrend schooling and cohort effects using the growth across the cohorts born in a 10-year window before $\bar{c}_r$, and then regress each variable on a full set of dummies around $\bar{c}_r$. In particular, for schooling (and equivalently for the cohort effects), we estimate

$$\hat{s}_{c,r} \;=\; \delta_r + \sum_{x=-10}^{15} \mathbb{I}_{(c=\bar{c}_r+x)} + \varepsilon_{c,r} \tag{9}$$

where δ_r is a reform fixed effect, $\mathbb{I}_{(c=\bar{c}_r+x)}$ is a dummy equal to 1 if cohort c is born x years after $\bar{c}_r$ and $\hat{s}_{c.r}$ is detrended schooling, constructed as

$$\hat{s}_{c,r} \;=\; s_{c,r} - \frac{c-\bar{c}_r+10}{10}\left(s_{\bar{c}_r-1} - s_{\bar{c}_r-11}\right).$$

[22]To formalize these points, Figures A.X and A.XI compare the distributions of trend breaks around the different types of reforms and political events with a placebo distribution of all the possible trend breaks in our data. Only the fully-implemented reforms were followed by larger than average schooling increases.

In Figures IXb and IXa, we report the point estimates for the dummies $\mathbb{I}_x$. Consistently with the previous graphical analysis, we observe an increase in schooling and a decrease in cohort effects for cohorts born after $\bar{c}_r$. To have a sense of the implied magnitudes, we estimate specification (6), pooled across countries for which we have at least one education reform and using the dummies $\mathbb{I}_{(c=\bar{c}_r+x)}$ to instrument for schooling around the policy reforms. The results are shown in columns (5) and (6) of Table II: the event study gives a negative, significant and large relationship between schooling and cohort effects.

4.4 School Construction in Indonesia

Finally, we turn to one specific policy that provides us with quasi-experimental variation in schooling. Following the seminal work of Duflo (2001), we study the effects of the INPRES school construction program in Indonesia, which built 61,000 primary schools between 1974 and 1978. The identification exploits the facts that (i) the intensity of the program - as measured by the number of new schools per pupil - varied across districts, and (ii) only cohorts younger than 6 years old when the program started were fully exposed to it. We run a difference-in-difference exercise, comparing cohorts fully exposed to the treatment to those not exposed to it, in districts with higher and lower treatment intensity. The data – the 1995 intercensal survey of Indonesia –, the identification strategy, and the specification closely follow Duflo (2001). We refer the interested reader to that article for more details.

We restrict the sample to males born between $1950 - 1977$. Consider the following specification

$$y_{icd} \;=\; \alpha_c + \eta_d + \sum_{k=1950}^{1977} \left(T_d \times \mathbb{I}_{ik}\right) \delta_k + \sum_{k=1950}^{1977} \left(\xi_d \times \mathbb{I}_{ik}\right) \varphi_k + \epsilon_{ijd}, \tag{10}$$

where (i, c, d) is an individual i, born in cohort c and currently living in district d, α_c is a cohort fixed effect, η_d is a district fixed effect, T_d is the number of schools built per 1000 children in district d, $\mathbb{I}_c$ is a dummy equal to 1 if individual i is born in cohort c, and ξ_d is the school enrollment in 1972. The coefficients of interest are $\{\delta_c\}_{c=1950}^{1977}$, which capture the effects of program intensity on each cohort. We estimate (10) for three different outcome variables: (i) years of schooling, for our first stage; (ii) a dummy equal to 1 for agricultural employment, for our reduced form specification; (iii) a dummy equal to 1 for non-agricultural employment (non-employment being the residual category with respect to (ii) and (iii)).[23]

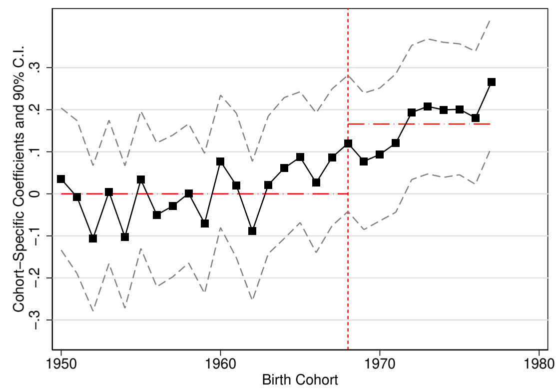
We report the estimated coefficients and associated standard errors in Figures Xa, Xb, and Xc. The coefficients are normalized to average zero for the control cohorts, that should

[23]Karachiwalla and Palloni (2019) run a very similar specification using Indonesian data and include further details on the empirical analysis. While we both reach the same specification independently, the first version of our work was circulated in August 2017 (`https://escholarship.org/uc/item/1ws4x2fg`).

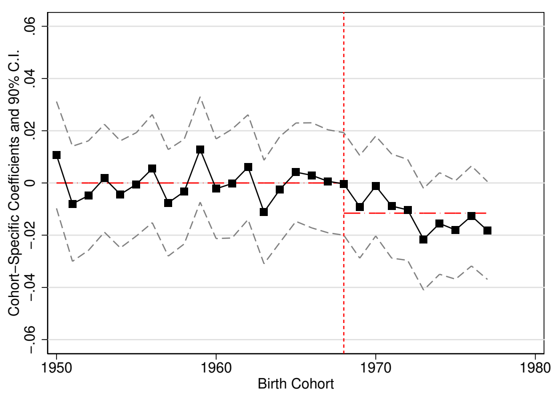
have been at most marginally affected by the treatment. The figures suggest no differential trend prior to the program.[24] As expected, the effect of the program was positive on education, negative on agricultural employment, and positive on non-agricultural employment.

Figure X: INPRES School Construction

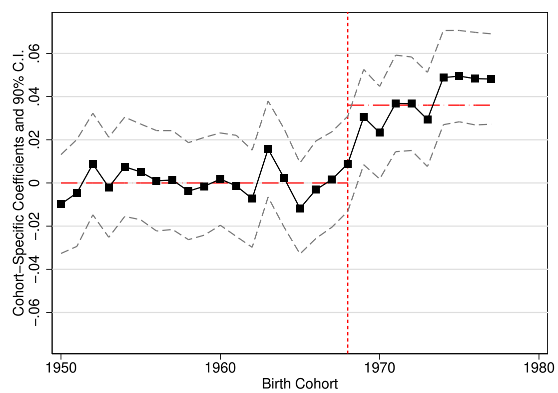
(a) Point Estimates for Education



(b) Point Estimates for Agriculture



(c) Point Estimates for non-Agriculture



Notes: Figure (a) shows the estimates of the cohort dummies from the first stage regression according to specification (10) when the left hand side variable is years of schooling. Figures (b) and (c) show the estimates for the reduce form results – from the same specification (10) – with either agricultural or non-agricultural employment as left hand side variables. The red dotted vertical line separates the treatment from the control cohorts. Data for agricultural employment and schooling are from the 1995 intercensal survey of Indonesia (SUPAS); data for treatment intensity are from Duflo (2001).

As in the original paper, the cohort-specific coefficients are imprecisely estimated and often not statistically different from each other. In order to improve power, we follow Duflo (2001) and focus on the comparison of two cohorts: a treatment cohort of individuals that were between 2 and 6 years old at the time the program was implemented, and a control

[24]When we omit the controls for enrollment in 1972, schooling years show a pre-trend. For this reason, we keep the controls throughout our analysis.

cohort of individuals that were between 12 and 17 years of age. The specification is the same as in (10), but with only one treatment cohort, and thus one coefficient of interest: the interaction between program intensity and the treatment cohort dummy.

Table III displays the results. Columns 1 and 2 show the reduced form specifications: the program is associated with a significant decrease in the probability of agricultural employment and an increase in the probability of non-agricultural employment; the latter is larger than the former, suggesting a significant flow from non-employment to non-agricultural employment as well. Column 3 reports the first stage specification: one extra school per 1000 children increases schooling by ~ 0.14, just as in Duflo (2001). Columns 4 and 5 show the IV results, where years of schooling are instrumented by the interaction between treatment intensity and the treated cohort dummy: one extra year of schooling reduces the probability of agricultural employment by 6.3 percentage points, and increases the probability of non-agricultural employment by 22.3 percentage points. This evidence shows that increases in schooling across cohorts led to lower propensities to work in agriculture.[25]

Table III: School Construction and Sectoral Employment: Evidence from Indonesia

	(1) Employed in Agri	(2) Employed in Non-Agri	(3) Years of Schooling	(4) Employed in Agri	(5) Employed in Non-Agri
Treated Cohort × Intensity	-0.009	0.031	0.137		
	(0.004)	(0.005)	(0.036)		
Years of Schooling				-0.063	0.223
				(0.030)	(0.063)
Cohort Fixed Effects	Y	Y	Y	Y	Y
District Fixed Effects	Y	Y	Y	Y	Y
Method	OLS	OLS	OLS	IV	IV
F Stat First Stage	-	-	-	14.29	14.29
Observations	53154	53154	53154	53154	53154

Notes: Robust standard errors in parentheses.

4.5 Taking Stock

The results in this Section, based on different data sources and empirical approaches, support an interpretation of cohort effects as changes in human capital. We have considered several sources of cohort-level differences in educational attainment, both "demand-driven"

[25]The results of this exercise are not directly comparable to the ones in Table II, since here we are running a linear probability model. In Appendix D, we run a cohort-level regression that allows us to compare the two, and find similar magnitudes.

(Section 4.2) and "policy-driven" (Sections 4.3 and 4.4); in both cases, they are reflected in corresponding differences in agricultural employment, as captured by our cohort effects. The policy results are particular noteworthy, as they suggest that, at the micro level, governments can affect sectoral choices by increasing access to formal education. We now turn to a setup that allows us to evaluate the implications of these results for structural transformation at the aggregate level.

5 Model

This section develops a general equilibrium model of frictional labor reallocation out of agriculture by cohort. The model provides a structural interpretation of the cohort and year effects estimated in Section 3. Moreover, it gives us a framework to compute their aggregate effects in general equilibrium.

5.1 Environment

We start by describing the economic environment. Time is discrete and runs infinitely.

Demographics, Preferences, and Individual Traits. The economy is inhabited by $N+1$ overlapping cohorts, indexed by c, each composed by a continuum of mass one of workers. Individuals of cohort c enter the labor market at time c and then work for a total of $N+1$ periods; therefore, they work each period in $\{c, ..., c+N\}$. They derive an increasing and non-satiated utility from the consumption of an agricultural and a non-agricultural good, and supply labor inelastically.

In each period, workers self-select in one of the two sectors of the economy, agriculture and non-agriculture. In agriculture, all workers have identical productivity. In non-agriculture, workers supply $h(c,\varepsilon)$ efficiency units, where $h(c,\varepsilon)$ depends on the birth cohort c as well as on the individual-level (and time invariant) ability ε. In particular, we assume the Cobb-Douglas aggregator

$$h(c,\varepsilon) = h_c^{\gamma}\varepsilon^{1-\gamma},$$

where h_c captures a non-agricultural productivity shifter specific to cohort c, and $\gamma \geq 0$ is a parameter controlling the relative weight of the cohort- and individual-level components. In what follows, we refer to $h(c,\varepsilon)$ and h_c as, respectively, individual-level and cohort-level human capital; in Section 6.3 we consider an extension where $h(c,\varepsilon)$ reflects a combination of human capital and preferences.[26]

[26]The assumption that non-agriculture is more human capital intensive than agriculture is consistent with widely documented patterns of sorting of high-skilled workers in non-agriculture (e.g. Gollin et al. (2014), Young (2013) Porzio (2017)), larger returns to skills in non-agriculture (see Herrendorf and Schoellman

We assume that ε is distributed according to a Beta$(\nu, 1)$ distribution, where ν is inversely related to the within-cohort variability. This distributional assumption buys tractability by allowing us to reconcile a constant rate of labor reallocation with time-varying sorting on idiosyncratic characteristics, as we show below. Moreover, we assume that h_c grows across cohorts at a constant rate, as summarized by Assumption 1.

Assumption 1. *Cohort-level human capital increases across cohorts at a constant rate*

$$\log \frac{h_{c+1}}{h_c} \quad = \quad \log g_h > 0 \quad \forall \quad c$$

We define the aggregate stock of human capital at time t as

$$H_t = \sum_{c=t-N}^{t} \int h(c, \varepsilon)\, dF(\varepsilon)$$

From the expression for $h(c, \varepsilon)$ and Assumption 1, it follows that H_t grows over time at a constant rate, with $\log \frac{H_{t+1}}{H_t} = \gamma \log g_h > 0$ for any t.

Production. We index the agricultural sector by A and the non-agricultural sector by M. The production of the agricultural good requires land X and the labor input $L_{A,t}$, while the production of the non-agricultural good only requires the labor input $L_{M,t}$. We assume that land is owned collectively by all individuals, who share the profits and use them to finance consumption. Productivity in agriculture, $Z_{A,t}$, may differ from productivity in non-agriculture, $Z_{M,t}$. The relative price of agricultural goods in equilibrium is given by p_t, which we describe below. The revenue functions in agriculture and non-agriculture are

$$\begin{aligned} p_t Y_{A,t} &= p_t Z_{A,t} X^{\alpha} L_{A,t}^{1-\alpha} \\ Y_{M,t} &= Z_{M,t} L_{M,t}. \end{aligned}$$

We assume that individuals of different cohorts are perfect substitutes in both sectors. However, as discussed above, the efficiency units supplied to the non-agricultural sector are heterogeneous both across and within cohorts. Letting $\omega_t(c, \varepsilon)$ be the occupational choice function, taking value 1 if individual (c, ε) at time t works in agriculture and 0 otherwise, the agricultural and non-agricultural labor inputs are given by

$$L_{A,t} \quad = \quad \sum_{c=t-N}^{t} \int \omega_t(c, \varepsilon)\, dF(\varepsilon)$$

(2018)) and skill-specific mobility across sectors (see Hicks et al. (2017)).

and

$$L_{M,t} \quad = \quad \sum_{c=t-N}^{t} \int h\left(c,\varepsilon\right)\left(1-\omega_t\left(c,\varepsilon\right)\right) dF\left(\varepsilon\right),$$

where $F(\varepsilon)$ is the within-cohort distribution of ε.

Firms choose optimally how many workers to hire, and the labor market is competitive. As a result, workers are paid the marginal product of their labor: the individual-level earnings in the two sectors are given by

$$\begin{aligned} y_{A,t} = w_{A,t} = (1-\alpha)\, p_t Z_{A,t} X^{\alpha} L_{A,t}^{-\alpha} \\ y_{M,t}\left(c,\varepsilon\right) = w_{M,t} h\left(c,\varepsilon\right) = Z_{M,t} h\left(c,\varepsilon\right). \end{aligned} \tag{11}$$

where $w_{A,t}$ and $w_{M,t}$ denote wages per efficiency unit in agriculture and non-agriculture.

Sectoral Choice. We now analyze the worker's sectoral choice problem. Given that markets are complete and that there is perfect foresight, we can think of individual (c,ε) choosing at time c a sequence of occupations $\{\omega_t\}_{t=c}^{N+c}$, one for each period in her life. This choice is made taking as given the path of her incomes in agriculture – $\{y_{A,t}\}_{t=c}^{N+c}$ – and non-agriculture – $\{y_{M,t}\left(c,\varepsilon\right)\}_{t=c}^{N+c}$, as defined above. Moreover, sectoral changes are associated with a cost $C\left(\omega_{t-1},\omega_t,y_{A,t},y_{M,t}\left(c,\varepsilon\right)\right)$, discussed in greater detail below. Formally, individual (c,ε) solves

$$\max_{\{\omega_t\}_{t=c}^{c+N}} \sum_{t=c}^{c+N} \beta^{t-c} \quad \left(\omega_t y_{A,t} + (1-\omega_t)\, y_{M,t}\left(c,\varepsilon\right) \quad - \quad C\left(\omega_{t-1},\omega_t,y_{A,t},y_{M,t}\left(c,\varepsilon\right)\right)\right)$$

$$\text{S.T. } \omega_{c-1} = 1;$$

where we are assuming that all individuals are born in agriculture, hence the constraint $\omega_{c-1} = 1$. The mobility friction takes the following form

$$C\left(\omega_{t-1},\omega_t,y_{A,t},y_{M,t}\left(c,\varepsilon\right)\right) = \mathbb{I}\left(\omega_t = 0\right) i y_{M,t}\left(c,\varepsilon\right) + \mathbb{I}\left(\omega_t < \omega_{t-1}\right) f y_{M,t}\left(c,\varepsilon\right) + \mathbb{I}\left(\omega_t > \omega_{t-1}\right) f y_{A,t}$$

and includes (i) an iceberg cost that reduces the non-agricultural wage by a fraction i in each period, and (ii) a fixed cost that reduces the wage in the destination sector by a fraction f in periods when a change of sector takes place. The iceberg cost can be interpreted as an amenity cost – as in Lagakos et al. (2019) – or as any other flow cost associated with leaving the agricultural sector, as, for example, the exclusion from risk-sharing communities (Munshi and Rosenzweig, 2016; Morten, 2019). The fixed cost can be interpreted as a one-time mobility cost, which might be driven by actual moving expenses (if a geographical move is necessary to change sector) or any other associated cost, such as retraining, idle time in between jobs, or one-time emotional costs.[27]

Notice that we have assumed that the mobility costs are constant over time and across cohorts. Moreover, we assume that they are bounded above by $\bar{i}$ and $\bar{f}$, which are explicit functions of the parameters (included in the Appendix). This assumption guarantees that at least some workers reallocate out of agriculture. We discuss further its role for the identification and the interpretation of the results below.

Assumption 2. *Mobility frictions are constant over time, across cohorts, and across individuals within cohorts. Moreover,* $i \in [0, \bar{i}]$ *and* $f \in \left[0, \bar{f}\right]$.

Closing the Model: the Price of Agricultural Goods. To close the model, we would need to describe how the goods' market clears. This would require taking a stand on preferences, the degree of openness of the economy, and the relative world prices of agricultural and non-agricultural goods. As we illustrate below, this is not needed for the purpose of mapping the decomposition exercise of Section 3 into the model. We therefore postulate a log-linear functional form for the relative agricultural price, which can be interpreted as a log-linear approximation of a fully specified model. Specifically, we assume

$$\underbrace{\log p_t}_{\text{Agr Price}} = \underbrace{\eta}_{\text{Elasticity}} \left(\underbrace{\log \theta_t}_{\text{Demand}} - \underbrace{\eta_z \log z_t}_{\text{Supply}} - \underbrace{\eta_L \log L_{A,t}}_{\text{Agr Labor}} + \underbrace{\eta_H \log H_t}_{\text{Human Capital}} \right), \tag{12}$$

where $\log \theta_t$ is a demand shifter that captures the relative demand for agricultural goods, $\log z_t$ is relative agricultural productivity, $z_t \equiv \frac{Z_{A,t}}{Z_{M,t}}$, $L_{A,t}$ is agricultural labor and H_t is the aggregate human capital stock. The parameters η, η_z, η_L, and η_H modulate the role of each variable in determining the agricultural price. In particular, $\eta = 0$ corresponds to the case of a small open economy with no trade frictions – i.e. an economy that takes the prices of agricultural and non-agricultural goods as given (we refer to this case as simply "small open economy"). On the contrary, with $\eta > 0$ an increase in the relative demand increases the relative price, while an increase in the relative supply - either due to an increase in

[27] A small complication arises for the "initial old" born at time 0, which, in absence of any adjustment, would effectively be more impacted by the fixed cost given their shorter life span. To keep the sectoral choice problem symmetric across all cohorts, we assume that the affected "initial old" (i.e. those that would have moved to non-agriculture at a younger age if they had a normal life span) are born in non-agriculture, i.e. their fixed cost is waived (or, equivalently, financed by the other cohorts through lump sum taxes). See Appendix E for more details.

agricultural productivity or employment - should decrease the relative price; that is, η_z and η_L are likely to be positive. An increase in H_t should instead have two opposing effects on the agricultural price: (i) an income effect due to individuals becoming richer, decreasing the relative demand and the relative price of agricultural goods, and (ii) an increase in relative agricultural labor productivity, possibly increasing the relative price. The sign of η_H is thus a priori ambiguous.

This approach preserves tractability while encompassing, in reduced form, the main mechanisms suggested in the literature as possible drivers of structural change. On one hand, a decrease in the demand for agricultural goods over time, as in Kongsamut et al. (2001) and Comin et al. (2015), decreases p_t leading to reallocation of labor out of agriculture. On the other hand, the effect of an increase in relative agricultural productivity depends on openness to trade: if the economy is sufficiently closed – i.e. if η is large enough – an increase in z_t lowers p_t enough to push workers out of agriculture, as in Ngai and Pissarides (2007); if the economy is sufficiently close to a small open economy – i.e. if η is small – a higher z_t pushes workers *into* agriculture, as in Matsuyama (1992a).

We impose the following restrictions on the exogenous determinants of the demand for agricultural labor, θ_t and z_t. First, we assume that they change at constant rates, g_θ and g_z. Second, we require that these rates are not too large, as stated in the following assumption.

Assumption 3. *The demand shifter θ_t and relative productivity z_t change at constant rates g_θ and g_z such that*

$$\log g_{\theta z} \equiv \eta \log g_\theta + (1 - \eta\eta_z) \log g_z \quad \leq \quad \max\{0, -\Psi \log g_h\},$$

where $\Psi \equiv \frac{v\gamma(\alpha+\eta\eta_L)+(1-\gamma)\eta\eta_H}{(1-\gamma)}$.

As we show below, the key role of this assumption is to guarantee that the year component implied by the model is negative, consistently with the empirical evidence in Section 3. This is achieved by ensuring that the decline over time in the relative demand for agricultural labor is large enough, taking into account the general equilibrium effects of human capital growth on relative prices.

5.2 Reallocation by Cohort

We now start the equilibrium characterization by describing the cohort-level reallocation out of agriculture. We focus on a constant reallocation path, i.e. an equilibrium where labor reallocates from agriculture to non-agriculture at a constant rate, as formally defined below.

Definition: Constant Reallocation Path. *A constant reallocation path is given by a series $\{L_{A,t}, w_{A,t}, w_{M,t}(c,\varepsilon), \omega_t(c,\varepsilon)$ for all $c \in [t-N,t]\}_{t=0}^{\infty}$, such that, given paths for agri-*

cultural demand, sectoral productivities, and cohort-level human capital $\{\theta_t, Z_{A,t}, Z_{M,t}, h_t\}_{t=0}^{\infty}$, *firms maximize profits taking wages as given, individuals choose optimally their occupation at each point in time taking wages as given, the labor market clears in both agriculture and non-agriculture, and agricultural employment decreases at a constant rate,* $g_{L_A} \equiv \frac{L_{A,t+1}}{L_{A,t}} < 1$.

Frictionless Reallocation by Cohort. To build intuition, we start from the frictionless case – i.e. $i = 0$ and $f = 0$. If moving is costless, individuals simply choose the sector that gives them the highest income in every period. The occupational choice is given by

$$\omega_t(c, \varepsilon) = \begin{cases} 1 & \text{if } w_{M,t} h(c, \varepsilon) \leq w_{A,t} \\ 0 & \text{otherwise} \end{cases}$$

which generates a cut-off rule such that individual (c, ε) moves out of agriculture at time t if her ability is higher than a threshold $\hat{\varepsilon}_t(c)$, where

$$\hat{\varepsilon}_t(c) = \left[\frac{w_{A,t}}{w_{M,t}} h_c^{-\gamma}\right]^{\frac{1}{1-\gamma}}.$$

Individuals sort into the sector where they have a comparative advantage. Using the expression for $h(c, \varepsilon)$, we can see that there is sorting both within and across cohorts. Within any cohort, individuals with high ε move out of agriculture. Across cohorts, the younger ones, with a higher cohort-level human capital h_c, have a larger share of individuals out of agriculture.

The share of workers from cohort c in agriculture is equal to

$$l_{A,t,c} = F(\hat{\varepsilon}_t(c)) = \left[\frac{w_{A,t}}{w_{M,t}} h_c^{-\gamma}\right]^{\frac{v}{1-\gamma}}, \tag{13}$$

and the reallocation out of agriculture for a given cohort - after substituting for the equilibrium wages and the law of motion of p_t - is given by

$$\log l_{A,t+1,c} - \log l_{A,t,c} = \frac{v}{1-\gamma}\left(\log g_{\theta z} + \eta\eta_H \gamma \log g_h - (\eta\eta_L + \alpha)\log g_{L_A}\right), \tag{14}$$

where $\log g_{L_A} \equiv \log L_{A,t+1} - \log L_{A,t}$. Equation (14) shows that the rate of labor reallocation for a given cohort is constant over time. Last, we notice that the agricultural employment gap between cohort c and cohort $c+1$ at time t,

$$\log l_{A,t,c+1} - \log l_{A,t,c} = \log\left(\frac{h_{c+1}}{h_c}\right)^{-\frac{\gamma v}{1-\gamma}} = -\frac{\gamma v}{1-\gamma}\log g_h,$$

is proportional to the rate of growth in human capital across cohorts: at a given point in time, the larger the human capital gap between young and old workers, the less likely the young are to be in agriculture relatively to the old.

Frictional Reallocation by Cohort. Next, we discuss the role of mobility frictions. The iceberg cost i represents a constant wedge between agricultural and non-agricultural wages; as such, it reduces the level of non-agricultural employment at each point in time, but it does not affect the rate of labor reallocation. The fixed cost f is more consequential, since it prevents *some* cohorts, but not others, from reallocating. Consider relatively young workers employed in agriculture at time t: given that the fixed cost is discounted over the whole working life, those with highest ability among them will still find it worthwhile to switch sector and take advantage of the ever-increasing relative demand for non-agricultural labor. In fact, the rate at which they reallocate is the same as in the frictionless case, even though the frictions do increase the level of agricultural employment. However, workers that are still in agriculture when old may be trapped there by the fixed cost, given that even those with the highest ability among them might not be willing to switch sector with only a few periods left to work. As a result, at a given point in time older workers are more likely to be in agriculture than younger ones, over and above what is implied by the human capital gap between the two generations. We formalize these results in the following Lemma.

Lemma 1: Labor Reallocation by Cohort with Mobility Frictions
Let $a_t(c) = t - c$ be the age of cohort c at time t. There exists a threshold $\hat{a}$, with $1 \leq \hat{a} < N$, such that for any c and t

$$\log l_{A,t+1,c} - \log l_{A,t,c} = \begin{cases} \frac{v}{1-\gamma}\left(\log g_{\theta z} + \eta\eta_H\gamma \log g_h - (\eta\eta_L + \alpha)\log g_{L_A}\right) & \text{if } \ a_{t+1}(c) \leq \hat{a} \\ 0 & \text{if } \ a_{t+1}(c) > \hat{a} \end{cases}$$

$$\log l_{A,t,c+1} - \log l_{A,t,c} = \begin{cases} -\frac{\gamma v}{1-\gamma}\log g_h, & \text{if } \ a_t(c) \leq \hat{a} \\ -\frac{\gamma v}{1-\gamma}\log g_h - [a_t(c) - a_t(c+1)]\,\Lambda & \text{if } \ a_t(c) > \hat{a} \end{cases}$$

where $\Lambda \geq 0$.

Proof. *See Appendix.* □

The fixed cost divides cohorts into two groups, according to a time-invariant age threshold. We refer to cohorts younger than $\hat{a}$ as "unconstrained", and to cohorts older than $\hat{a}$ as "constrained". Notice that the youngest cohort is always unconstrained for the first two periods, as $\hat{a} \geq 1$: this is guaranteed by Assumption 2, which requires the fixed cost f to be no greater than the value $\bar{f}$ that would make the marginal mover of the one-year old cohort indifferent between reallocating out of agriculture or not.[28]

5.3 Aggregate Labor Reallocation

Aggregating up cohort-level agricultural employment, we can write the overall agricultural labor supply at time t as

$$\log L_{A,t} = \lambda_S - \frac{\nu}{1-\gamma}\log H_t + \frac{\nu}{1-\gamma}\log\frac{w_{A,t}}{w_{M,t}} \tag{S_t}$$

where λ_S is a time-invariant term.[29] The supply is upward sloping with respect to the relative wage, as a higher relative wage induces more individuals to stay in agriculture. Moreover, increases in human capital lead to a downward shift of the agricultural labor supply, as human capital is more valued outside of agriculture. It is noteworthy that mobility frictions are subsumed into the λ_S term, and as such do not affect the slope or the magnitude of the shift associated with changes in H_t over time.

In equilibrium, agricultural employment is given by the intersection between (S_t) and agricultural labor demand, which, combining (11) and (12), can be written as

$$\log L_{A,t} = \lambda_D + \frac{\eta}{\alpha+\eta\eta_L}\log\theta_t + \frac{(1-\eta\eta_z)}{\alpha+\eta\eta_L}\log z_t + \frac{\eta\eta_H}{\alpha+\eta\eta_L}\log H_t - \frac{1}{\alpha+\eta\eta_L}\log\frac{w_{A,t}}{w_{M,t}} \tag{D_t}$$

Figure XIa plots (S_t) and (D_t) in a supply-demand diagram, and illustrates the forces driving labor reallocation between two generic times t and $t+1$. First, the model features the two main mechanisms behind structural change commonly emphasized in the literature, i.e. a decrease in the relative demand for agricultural goods (θ_t) and an increase in relative agricultural productivity (z_t), which - given Assumption 3 - shift downwards the relative demand for agricultural labor. Second, the increase in H_t over time time leads to a downward shift in the relative supply curve, and possibly - as discussed below - an additional shift of the demand curve through general equilibrium effects. The combination of these demand and supply shifts leads to labor reallocation out of agriculture.

To isolate the role of human capital, Figure XIb displays a counterfactual scenario where the demand forces behind labor reallocation are kept fixed, i.e. $\log g_\theta = \log g_z = 0$. As discussed above, the fact that $\log g_h > 0$ implies that the supply curve shifts downwards. In partial equilibrium, i.e. if the relative wage and price are constant, this shift would result

[28] As we show in the Appendix, $\hat{a}$ is decreasing in f, which implies that the youngest cohort will be unconstrained as long as f is not larger than $\bar{f}$. The role on the upper bound on i imposed by Assumption 2 is instead to ensure that within all cohorts a positive mass of individuals is willing to move out of agriculture.

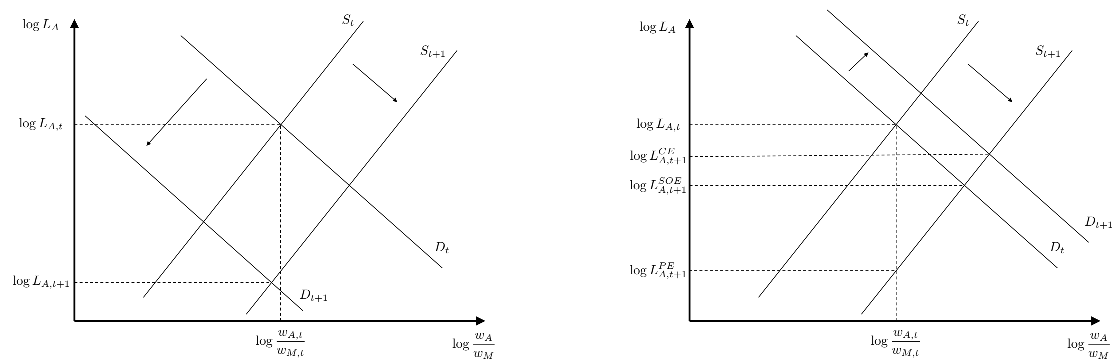
[29] In the frictionless case, this expression can be obtained by summing (13) across cohorts and using the fact that $H_t \propto h_t^\gamma$. We provide the expression for λ_S in the general case with frictions in Appendix E.

at $t+1$ in a level of agricultural employment of $L^{PE}_{A,t+1}$. When wages are allowed to adjust but prices are kept fixed - the case of a small open economy - the resulting agricultural employment is $L^{SOE}_{A,t+1}$, which is larger than $L^{PE}_{A,t+1}$ since the adjustment in relative wages attenuates the employment effect of the supply shift. Finally, if the relative price of the agricultural good adjusts as well (i.e. if $\eta > 0$, as in a closed economy), the increase in H_t leads additionally to a downward or upward shift in the demand curve, depending on the sign of η_H. The resulting agricultural employment $L^{CE}_{A,t+1}$ can be higher or lower than $L^{SOE}_{A,t+1}$, and can in principle even be higher than L_t: if the price elasticity is high enough, a decrease in the supply of agricultural labor could increase the relative price sufficiently to pull workers into agriculture.[30] Figure XIb shows the case where $L^{CE}_{A,t+1}$ is in between $L^{SOE}_{A,t+1}$ and L_t.

Figure XI: Aggregate Labor Reallocation: Graphical Illustration

(a) Overall Reallocation

(b) Counterfactual: $\log g_\theta = \log g_z = 0$



Proposition 1 characterizes the overall rate of labor reallocation on a constant reallocation path. As illustrated above, labor reallocation out of agriculture ($\log g_{L_A} < 0$) can be triggered by demand forces ($\log g_{\theta z} < 0$) and human capital growth ($\log g_h > 0$). The direct effect of each term is mediated by the within-cohort ability distribution - which determines the mass of workers leaving agriculture for a given change in relative wages - and by general equilibrium effects. The impact of demand forces is unambiguous, since $\Theta_D \in \big[0, 1\big)$. The effect of human capital growth can be amplified or attenuated by general equilibrium forces; in a small open economy $\Theta_S = \Theta_D \in \big[0, 1\big)$, while in a closed economy Θ_S can be positive or negative depending on the parameters' values. We refer to $1 - \Theta_S$ as the *general equilibrium*

[30]This result is reminiscent of Matsuyama (1992b), which shows that agricultural productivity growth has opposite implications on agricultural employment in a closed and open economy. The same is potentially true for changes in human capital; however, for those to increase agricultural employment, it needs to be the case that both the economy is sufficiently closed ($\eta > 0$) and the productivity effect of human capital on prices dominates the income effect ($\eta_H > 0$).

multiplier of human capital growth. Mobility frictions are irrelevant for the aggregate *rate* of labor reallocation, even though they do affect the *level* of agricultural employment at each point in time.[31]

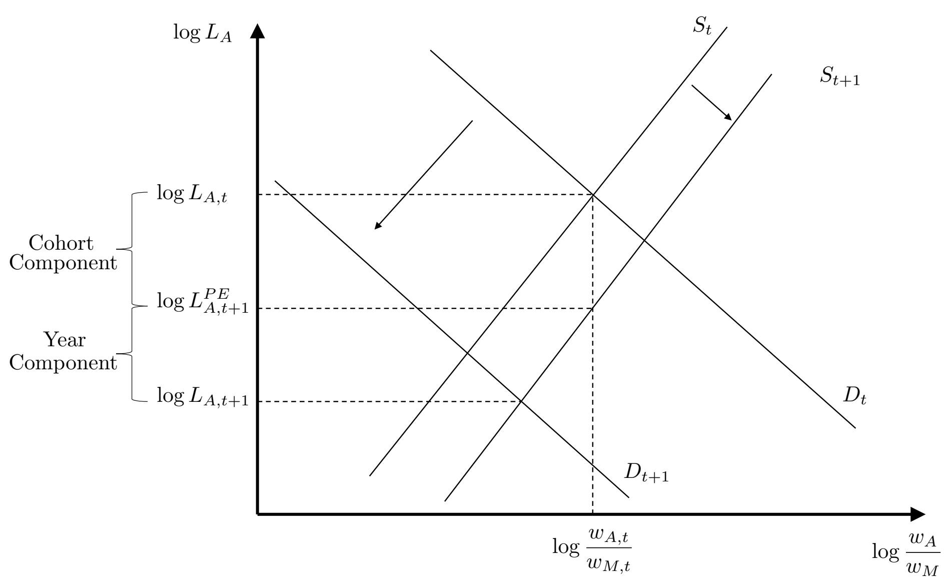
Proposition 1: Aggregate Labor Reallocation
Labor reallocation out of agriculture is given by

$$\log g_{L_A} = \underbrace{\left(\frac{v}{1-\gamma}\right)}_{\text{Skill Distr.}} \left(\left(\underbrace{1-\Theta_D}_{\text{GE}} \right) \underbrace{\log g_{\theta z}}_{\text{Demand}} + \left(\underbrace{1-\Theta_S}_{\text{GE}} \right) \underbrace{-\gamma \log g_h}_{\text{Supply}} \right),$$

where $\Theta_D \equiv \frac{v(\alpha+\eta\eta_L)}{1-\gamma+v(\alpha+\eta\eta_L)}$ *and* $\Theta_S \equiv \frac{v(\alpha+\eta\eta_L)+(1-\gamma)\eta\eta_H}{1-\gamma+v(\alpha+\eta\eta_L)}$.

Proof. *See Appendix.* □

Figure XII: Cohort and Year Components in the Model



5.4 Mapping to the Empirical Decomposition

We now discuss how the model maps into the empirical decomposition of labor reallocation presented in Section 3. We start by considering the frictionless case in order to build intuition, and then turn to the general case.

In absence of mobility frictions (i.e for $i = f = 0$), the log agricultural employment at time t of any cohort c can be written as

[31] Assumption 2 guarantees that frictions are sufficiently small to generate positive reallocation. Trivially, if $f \to \infty$ or $i \to \infty$, there would be no reallocation. Reallocation is either zero, or does not depend on f and i. Our parametric restrictions exclude the case in which reallocation is zero.

$$\log l_{A,t,c} = \hat{\kappa} \ + \underbrace{\frac{\nu}{1-\gamma}\log\left(p_t z_t X^{\alpha} L_{A,t}^{-\alpha}\right)}_{\text{YEAR EFFECTS}} - \underbrace{\frac{\nu\gamma}{1-\gamma}\log h_c}_{\text{COHORT EFFECTS}}$$

where $\hat{\kappa}$ is a cohort- and time-invariant function of parameters. This equation maps into the empirical specification (1). Through the lens of the model, cohort effects are proportional to cohort-level human capital, while year effects are proportional to the relative wage across sectors, which depends on aggregate prices and quantities. The age effects introduced in specification (5) are redundant in this case; once time and cohort effects are accounted for, age does not play any independent role.

Consider the year and cohort components, as defined in (2)-(4), of the rate of labor reallocation between t and $t+1$. The year component captures the difference between the year effects associated to t and $t+1$, which is identified by the average change in agricultural employment for a given cohort. In the frictionless model the rate of reallocation is common across all cohorts, so that - for any c - the year component is given by

$$\log \psi_t = \log l_{A,t+1,c} - \log l_{A,t,c} = \frac{v}{1-\gamma}\left((1-\Theta_D)\log g_{\theta z} + \Theta_S \gamma \log g_h\right)$$

where the second equality follows from plugging the expression for $\log g_{L_A}$ from Proposition 1 into equation (14). The cohort component captures the change over time in the average cohort effects for the active cohorts. Given that in our model cohort effects change across cohorts by a constant amount, this corresponds to the difference between the cohort effects of any two consecutive cohorts, which in turn is given by the cross-cohort agricultural employment gaps averaged across all time periods. In absence of frictions these cross-cohort gaps are constant over time, so that - for any t - the cohort component is

$$\log \chi_t \quad = \quad \mathbb{C}_{c+1} - \mathbb{C}_c = \log l_{A,t,c+1} - \log l_{A,t,c} = -\frac{\nu\gamma}{1-\gamma}\log g_h. \tag{15}$$

These two quantities correspond to different aspects of the process of labor reallocation. Notice from equation (S_t) that (15) represents the magnitude of the shift in the agricultural labor supply driven by human capital growth. As displayed in Figure XII, the cohort component captures the partial equilibrium effect of the change in supply, i.e. the decrease from $\log L_{A,t}$ to $\log L^{PE}_{A,t+1}$. The year component captures the residual part of reallocation, i.e. the difference between $\log L^{PE}_{A,t+1}$and $\log L_{A,t+1}$. Intuitively, gaps in agricultural employment between different cohorts at a given point in time (i.e. the cohort component) identify the extent to which changes in human capital shift the supply curve, keeping wages fixed; on the other hand, changes over time for a given cohort (i.e. the year component) identify the movement along a given supply curve driven by changes in relative wages.

Consider now the general case with mobility frictions. Under specification (1), the structural interpretations of the cohort and year components discussed above would not apply. Lemma 1 shows that the rate of reallocation across t and $t+1$ is cohort-specific, with constrained cohorts not reallocating at all; the year component would therefore pick up the reallocation rate of unconstrained cohorts, scaled down by the share of constrained cohorts. By the same logic, the cohort component would be larger than $-\frac{\nu\gamma}{1-\gamma}\log g_h$, as it would combine the cross-cohort employment gaps for both unconstrained cohorts and constrained cohorts, with the latter being larger than the former. This is where the age controls introduced in specification (5) become important. Under the identification restriction of a zero age effect for any young (unconstrained) cohort, age controls capture the reallocation behavior of old (constrained) cohorts, so that the resulting year and cohort components retain the structural interpretations illustrated in Figure XII. The following proposition formalizes this result.

Proposition 2: Decomposition of Labor Reallocation

Consider the specification

$$\underbrace{\log l_{A,t,c}}_{\text{AGR SHARE OF COHORT } c \text{ AT TIME } t} = \underbrace{\tilde{\mathbb{Y}}_t}_{\text{YEAR EFFECTS}} + \underbrace{\tilde{\mathbb{C}}_c}_{\text{COHORT EFFECTS}} + \underbrace{\tilde{\mathbb{A}}_{t-c}}_{\text{AGE DUMMIES}} + \varepsilon_{t,c}$$

estimated with model-generated data under the restriction that $\tilde{\mathbb{A}}_a = \tilde{\mathbb{A}}_{a-1}$, *where* $a \in [1, \hat{a}]$. *Define the year and cohort components of labor reallocation between* t *and* $t+1$ *as*

$$\begin{aligned} \log \tilde{\psi}_t &\equiv \tilde{\mathbb{Y}}_{t+1} - \tilde{\mathbb{Y}}_t \\ \log \tilde{\chi}_t &= \log L_{A,t+1} - \log L_{A,t} - \log \tilde{\psi}_{t+1}. \end{aligned}$$

Then, for all t,

$$\begin{aligned} \log \tilde{\psi}_t = \log \tilde{\psi} &= \left(\frac{v}{1-\gamma}\right)\left((1-\Theta_D)\log g_{\theta z} + \Theta_S \gamma \log g_h\right) \\ \log \tilde{\chi}_t = \log \tilde{\chi} &= -\left(\frac{v}{1-\gamma}\right)\gamma \log g_h. \end{aligned}$$

Proof. *See Appendix.* $\square$

The following corollary shows how the omission of age controls biases the estimates of the two components. Since mobility frictions limit the reallocation of older workers, not controlling for age results in an overstatement of the cohort component and an understatement of the year component. The difference between the year components estimated with and without age controls is proportional to the share of constrained cohorts, $\lambda(f)$, a natural measure of the severity of reallocation frictions.

Corollary 1: Bias in the Basic Decomposition
Consider specification (1) estimated with model-generated data. The estimated year and cohort components would be

$$\begin{aligned} \log \psi &= \Big(1-\lambda\left(f\right)\Big)\log\tilde{\psi} \\ \log \chi &= \log\tilde{\chi} + \frac{\lambda\left(f\right)}{1-\lambda\left(f\right)}\log\tilde{\psi} \end{aligned}$$

where $\lambda(f) \in \big[0,1\big)$ *is the share of constrained cohorts,*

$$\lambda(f) = \frac{N+1-\hat{a}}{N+1}$$

which is increasing in the fixed cost f *and does not depend on the iceberg cost* i*.*

Proof. *See Appendix.* □

Implications for Wage Data. The mapping between the model and the decomposition developed in this section implies that both human capital growth and reallocation frictions can be quantified without relying on the measurement of wages, which is notoriously difficult for developing countries and the agricultural sector. The model does have predictions on wages that are in line with the limited available evidence; in particular, we show in Appendix E.4 that it is consistent with the observational wage gains for workers moving from agriculture to non-agriculture being smaller than the corresponding cross-sectional gaps (Hicks et al., 2017; Herrendorf and Schoellman, 2018; Alvarez, 2020). However, our analysis in Appendix E.4 also shows that wage data, even if with a panel dimension, would not be enough to infer the magnitude of the frictions; the fixed cost makes the sectoral decision dynamic, and to estimate mobility costs one would need the hypothetical wage paths in agriculture in non-agriculture for both movers and non-movers. Corollary 1 provides an alternative way of quantifying these costs.

6 Quantitative Results

We revisit the empirical results of Section 3 through the lens of our model to quantify the contribution of the global human capital increase to structural transformation.

6.1 Revisiting the Decomposition Results

The model developed in Section 5 provides us with a structural interpretation of the empirical results in Section 3. As shown in Table I, the cohort component is on average -0.78%, corresponding to 38% of the observed rate of labor reallocation. Proposition 2 tells us how to read these figures in the context of the model: human capital growth has induced, on average across countries, a downward shift of the agricultural labor supply at annual rate of -0.78%. This result highlights the key take-away of the paper: changes over time in agricultural labor supply represent a key feature of the process of structural transformation, which is missed by models that abstract from workers' skills and their differential use across sectors.

The model also guides us in the understanding of the aggregate effects of this shift in agricultural labor supply. In a world where wages are kept fixed, the cohort component coincides with the rate of labor reallocation induced by human capital growth, representing 38% of the overall reallocation. In general equilibrium, the impact of the supply shift is mediated by the multiplier $1 - \Theta_s$ (as defined in Proposition 1), which summarizes the adjustment of relative prices. We discuss two approaches to quantify $1 - \Theta_s$ in Section 6.2.

Finally, Corollary 1 shows that the comparison between the decomposition results with and without age controls is directly informative on the severity of reallocation frictions. To illustrate this, we compute for each country j in the sample the value of $\lambda(f_j)$ implied by the model as $\lambda\left(f_j\right) = 1 - \frac{\log \bar{\psi}_j}{\log \tilde{\bar{\psi}}_j}$, and display summary statistics in column 6 of Table I. On average across countries, $\lambda(f_j)$ is approximately 30%, which means individuals' reallocation decision is constrained by the fixed cost in the last 30% of their work-life, or approximately in our sample, after they turn 45 years old. Rows 2-4 report the average $\lambda(f_j)$ separately for low-, middle-, and high-income countries. Frictions are virtually non-existing in high-income countries, and considerably more severe in poorer countries.

6.2 General Equilibrium Effects

The cohort component captures the magnitude of the supply shift associated with human capital growth. How large is the equilibrium impact of such shift? Answering this question requires going beyond the empirical decomposition and taking a stand on the parameters mediating the adjustment of relative prices. Combining Propositions 1 and 2, the overall impact of human capital growth on labor reallocation is the product of the cohort component and the general equilibrium multiplier,

$$\log g_{L_A} = (1 - \Theta_D) \log g_{\theta z} + \underbrace{(1 - \Theta_S)}_{\text{GE MULTIPLIER}} \times \underbrace{\log \tilde{\chi}}_{\text{COHORT COMPONENT}}, \tag{16}$$

where $(1 - \Theta_S) = \frac{1-\eta\eta_H}{1+\left(\frac{v}{1-\gamma}\right)(\alpha+\eta\eta_L)}$.

The multiplier depends on two sets of parameters. First, the parameters modulating general equilibrium adjustments in the labor market: the land share in agricultural production, α, and the distributional parameter $\frac{v}{1-\gamma}$, which represents the elasticity of the agricultural labor supply to the relative wage, as can be seen in equation (13). The multiplier is decreasing in both; intuitively, a higher α implies a larger change in agricultural wages following a given shift in relative labor supply, while a higher $\frac{v}{1-\gamma}$ implies a larger reallocation of labor following a given change in the relative wage. Second, the parameters controlling general equilibrium effects in the goods market: the elasticities of the agricultural price with respect to the human capital stock, $\eta\eta_H$, and agricultural labor, $\eta\eta_L$. The larger these elasticities, the more human capital growth is reflected in higher agricultural prices rather than lower agricultural employment. The GE multiplier is likely to vary across countries, for example as a function of their stage of development or their openness to trade. While a country-specific quantification of the multiplier is beyond the scope of the paper, the next subsections propose two illustrative calculations under different sets of assumptions.

Calibration for Small Open Economies. We consider first a small open economy, for which $\eta = 0$. In this case, the GE multiplier only depends on α and $\frac{v}{1-\gamma}$, which can be mapped into observables as follows. First, α corresponds to the land income share in agriculture, which Herrendorf et al. (2015) estimate to be around 7% in the United States. Land, however, may play a larger role in low-income countries, where agricultural production is less capital intensive; for example, Gollin and Udry (2017) estimate production functions for micro plots in Uganda and Ghana and find land shares of 40%-50%. We therefore consider values of α in the 0.07-0.5 range.

Second, we use information on wage dispersion in non-agriculture to bound $\frac{v}{1-\gamma}$. The within-cohort variance of log non-agricultural wages implied by the model is

$$\operatorname{Var}\left[\log w_{M,t}(c,\varepsilon)\right] = (1-\gamma)^2 \operatorname{Var}\left[\log\varepsilon \mid \log\varepsilon \geq \log\hat{\varepsilon}_t(c)\right] \leq \left(\frac{1-\gamma}{v}\right)^2, \tag{17}$$

where the equality uses the equilibrium wage, and the inequality is due to the properties of the Beta distribution.[32] The within-cohort standard deviation corresponds therefore to an upper bound for $\frac{v}{1-\gamma}$, which we can use to compute a lower bound for the GE multiplier (which, as discussed above, is decreasing in $\frac{v}{1-\gamma}$). While our dataset does not include wages for most countries, Lagakos et al. (2018) provided us with the value of the within-cohort standard deviation for each of the 18 countries in their sample, spanning the income

[32] If $\varepsilon \sim \text{Beta}(v,1)$, then $-\log\varepsilon \sim \text{Exp}(v)$. Also, the variance of a truncated exponential is smaller than the unrestricted variance, which is v^{-2}.

distribution from Bangladesh to the United States.[33] The average standard deviation across these countries is 0.67, with no systematic correlation with GDP per capita. We therefore use $\frac{v}{1-\gamma} = 1/0.67 = 1.5$.

Combining the values for the two parameters, we find a GE multiplier for small open economies ranging between 0.4 and 0.9, with low values in this range more likely to apply to low-income countries. Given a multiplier in the middle of this range, this exercise suggests that the inferred downward shift of the agricultural labor supply can account for about 20-25% of the observed rate of labor reallocation.

Table IV: Estimating the GE Multiplier

	Dependent Variable: $\log g_{L_A,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$\log \chi_t$	0.794	1.161	0.566		0.471	0.814
	(0.150)	(0.470)	(0.183)		(0.264)	(0.367)
$\log \chi_t^S$				0.762		
				(0.299)		
$\log \chi_t$ × Middle Income						-0.376
						(0.445)
$\log \chi_t$ × Low Income						-0.389
						(0.501)
Country FE	NO	YES	NO	NO	NO	NO
Income Group FE	NO	YES	YES	YES	YES	YES
Method	OLS	OLS	OLS	OLS	IV	OLS
F Stat First Stage	-	-	-	-	188.28	-
Observations	145	145	145	145	145	145

Notes: Robust standard errors in parentheses.

The General Case: A Regression Approach. The calibration above only applies to small open economies; given the reduced form nature of the price equation (12), we do not pursue a direct calibration of $\eta\eta_H$ and $\eta\eta_L$. As an alternative approach, we go back to the data and use variation in the estimated cohort component to estimate the GE multiplier directly. The idea follows from equation (16): the larger the GE multiplier, the more the variation in the cohort component should be reflected in corresponding variation in the reallocation rate; at the extreme, if the GE multiplier is equal to 0 (i.e. $\Theta_s = 1$), larger

[33] Refer to Lagakos et al. (2018) for data description and details. Wages are constructed as earnings divided by total hours of work in the period of observation, which is either weekly, monthly, or yearly. We drop the top and bottom 1% of wages to check that the variance estimates are not driven by outliers. For each country, we keep the most recent available cross-section.

cohort components would be compensated exactly by smaller year components, with no impact on the reallocation rate.

To implement this, we consider a stochastic version of the model, where cohort-level human capital h_c, technology z_t and the demand shifter θ_t are subject to temporary shocks that divert them from their constant growth path: $h_c = h_0 g_h^c \xi_{h,c}$, $z_t = z_0 g_z^t \xi_{z,t}$ and $\theta_t = \theta_0 g_\theta^t \xi_{\theta,t}$, with $\mathbb{E}_c[\log \xi_{h,c}] = \mathbb{E}_t[\log \xi_{z,t}] = \mathbb{E}_t[\log \xi_{\theta,t}] = 0$. In Appendix E.5 we show that in the frictionless benchmark the (annualized) cohort component between t and $t+k$ can be written as

$$\log \chi_t = -\frac{\gamma\nu}{1-\gamma}\left[\log g_h + \frac{1}{N+1}\frac{1}{k}\sum_{s=1}^{k}\log\frac{\xi_{h,t+s}}{\xi_{h,t-1-N+s}}\right]$$

and the corresponding reallocation rate as

$$\log g_{L_A,t} = (1-\Theta_D)\frac{\nu}{1-\gamma}\left[\log g_{\theta z} + \frac{1}{k}\log\frac{\xi_{\theta z,t+k}}{\xi_{\theta z,t}}\right] + (1-\Theta_S)\log\chi_t \tag{18}$$

where $\xi_{\theta z,t} \equiv \xi_{\theta,t}^{\eta}\xi_{z,t}^{1-\eta\eta_z}$.[34][35] The variation in $\log \chi_t$ is driven by cross-sectional differences in $\log g_h$ and over-time differences in the average realizations of human capital shocks for cohorts entering and exiting the labor market between t and $t+k$. The variation in $\log g_{L_A,t}$ is driven by the cohort component as well as the unobserved growth rate and fluctuations of technology and demand. We estimate (18) using the country- and year-specific reallocation rates and cohort components computed in Section 3; Table IV displays the results.

Column 1 shows the pooled regression with no additional controls; the implied GE multiplier is around 0.8. This specification relies on $\log \chi_t$ being uncorrelated with $\log g_{\theta z}$ and $\log \frac{\xi_{\theta z,t+k}}{\xi_{\theta z,t}}$, which might not hold if human capital is accumulated faster in anticipation of a faster decline in the demand for agricultural labor. Column 2 introduces country fixed effects, which absorb the cross-sectional variation in $\log g_{\theta z}$ and $\log g_h$; the identifying assumption is that temporary shocks to cohort-level human capital of entering and exiting cohorts are uncorrelated with contemporaneous realizations of technology and demand shocks (while $\log g_h$ can be correlated with $\log g_{\theta z}$).[36] The resulting GE multiplier is higher

[34]For these expressions, we defined $g_{L_A,t}$ as the growth rate of the geometric average of the cohort-level agricultural employment shares; this guarantees that the reallocation rate is log-separable in average human capital growth and cohort-level shocks. In practice, the correlation between the growth rates of the geometric and arithmetic averages of cohort-level agricultural employment is 0.99.

[35]The version of the model with frictions loses analytical tractability when introducing temporary shocks, given that the dynamic sectoral choice problem would need to take into account the possible future realizations of all shocks and their consequences on prices and cohort-level employment shares. Equation (18) applies exactly in the frictionless model, and can be thought of as an approximation for the model with frictions when the variance of the shocks and/or frictions are not too large.

[36]This requires that cohorts entering and exiting at time t do not base their (life-time) human capital accumulation decision on the realization of the $\xi_{\theta z,t}$ shock. This would automatically hold in a model

than in column 1, though more imprecisely estimated. One issue with this specification is that the within-country variation in our dataset is limited, as for most countries we have few repeated cross-sections to work with. Column 3 shows a more parsimonious specification that includes fixed effects for the three income groups used in the rest of the paper, which absorb the variation in $\log g_h$ and $\log g_{\theta z}$ between countries at different levels of development; this gives a more precise estimate of 0.56.

The rest of the Table reports extensions and robustness checks. Given that we use $\log g_{L_A,t}$ in the computation of $\log \chi_t$, one concern is that noise or measurement error might generate spurious correlation between the two.[37] To address this possibility we construct an alternative measure of $\log \chi_t$ based on cohort-level schooling data and the empirical relationship between schooling and cohort effects estimated in Section 4; in particular, we compute $\hat{\mathbb{C}}^S_{c,j} = \hat{\beta} s_{c,j}$, where $\hat{\beta} = 0.113$ is taken from Table II, define

$$\log \chi^S_{t,j} = \frac{1}{k} \log \left(\frac{\sum_{c=t+k-N}^{t+k} n_{t+k,c,j} \exp\left(\hat{\mathbb{C}}^S_{c,j}\right)}{\sum_{c=t-N}^{t} n_{t,c,j} \exp\left(\hat{\mathbb{C}}^S_{c,j}\right)} \right)$$

and use it as a regressor in column 4; the resulting multiplier is marginally higher compared to column 3. In column 5 we use the average estimated cohort effects for exiting cohorts only as an instrument for $\log \chi_t$, based on the idea that it is even more unlikely that individuals base their life-time human capital decisions on the expected realization of shocks at the end of their career; the results are again quantitatively similar to the benchmark in column 3. Finally, column 6 allows the coefficient to vary across income groups; the implied multipliers span the 0.4-0.8 range, with higher values for high-income countries.

Overall, this exercise leads to similar conclusions to the calibration considered above. The GE multiplier is likely to somewhat attenuate the partial equilibrium impact of human capital growth. The attenuation is slightly stronger in low-income countries, consistently with a larger degree of decreasing returns to labor in countries where agriculture is more land-intensive. Based on an average multiplier of around 0.55, we conclude that human capital growth can account for about 20% of the observed reallocation out of agriculture.

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where shocks are serially uncorrelated and human capital decisions are taken before the realization of the technology and demand shocks. As discussed below, we find similar results when focusing on the variation induced by exiting cohorts only.

[37] The direction of the bias is ambiguous ex ante. Noise in the agricultural employment of entering and exiting cohorts generates a spurious positive correlation between overall reallocation and the cohort component, while noise in the agricultural employment of other cohorts generate a spurious positive correlation between overall reallocation and the year component (and, as a consequence, a negative one between overall reallocation and the cohort component). We illustrate this point in Appendix E.5.

6.3 Discussion

To conclude this section, we discuss how variations to some of the model's assumptions might affect the magnitude and interpretation of our quantitative results.

Preferences for Non-Agriculture. Our model maps cohort-level differences in agricultural employment into changes in h_c, a cohort-level attribute that makes individuals more productive in the non-agricultural sector. In practice, the non-monetary value of working in non-agriculture might be growing across cohorts as well, perhaps as a result of changes in the quantity, quality and content of their education. We discuss an augmented setup where h_c reflects a combination of productivity and preferences in Appendix E; within that setting, we show that the cohort component captures the supply shift induced by both factors, irrespective of their (unobservable) relative importance. While changes in preferences and productivity imply different degrees of price adjustments, we show that the two approaches in Section 6.2 still recover the appropriate GE multiplier, again without the need of taking a stand on the relative importance of the two.

Human Capital Accumulation Over the Life-Cycle. Our model abstracts from human capital accumulation over the life-cycle. There could be two types of life-cycle effects: (i) general human capital (i.e h_c increasing as a cohort ages), and (ii) human capital specific to the sector of employment. Recall that we identify year, cohort and age effects under the linear restrictions that age effects are zero in the first years an individual is in the labor market (as implied by the model). If individuals accumulate general human capital while young, leading them to move out of agriculture, we would overestimate the year component – thus underestimating the cohort component and attenuating our results. Sector-specific human capital would work in the opposite direction. As noticed by Lee and Wolpin (2006), sector-specific experience acts as a barrier to mobility. If individuals accumulate in the first years on the job skills which make them more likely to stay in agriculture, then we would underestimate the year component. In practice, whether our results are biased upwards or downwards depends on whether experience human capital is general or sector-specific. Estimates from Altonji et al. (2013), although coming from the United States only, suggest that most experience human capital is general. Similarly, Lee and Wolpin (2006) find that the degree of sectoral specificity of work experience does not appear to be an important determinant of the relative size or growth of sectors.

Endogenous vs Exogenous Human Capital Accumulation. Human capital growth is likely to be driven in practice by a combination of (i) endogenous responses to expected changes in the relative demand for agricultural labor and (ii) other factors, unrelated to sectoral demands, affecting the supply and demand for education. In Section 4 we consider changes in schooling driven by factors plausibly belonging to (ii), and show that they are reflected in changes in agricultural employment. The model on the other hand treats g_h as exogenous, and as such it does not attempt to separately quantify (i) and (ii); the cohort component maps into the overall impact of human capital on agricultural labor supply, irrespective of its drivers.[38] The combination of these approaches tells us that human capital growth contributes to structural change, and educational policies increasing the former are likely to accelerate the latter.

7 Conclusion

This paper explores the hypothesis that the steep increase in human capital during the 20^{th} century contributed to the process of structural transformation, by equipping the new generations of workers with skills more useful outside of the agricultural sector.

We use theory and evidence to support this hypothesis. Drawing on micro data from many countries at different levels of development, we document that a large part of the aggregate rate of labor reallocation out of agriculture was driven by new cohorts entering the labor market, as opposed to movements across sectors for given cohorts. Using information on cohort-specific educational attainment and a newly compiled dataset on educational reforms and other relevant political events, we provide evidence for the fact that the increase in schooling for more recent cohorts led to a sharp reduction in the agricultural labor supply. A model of frictional labor reallocation out of agriculture provides a structural interpretation of our empirical results, suggesting that, taking into account general equilibrium effects, human capital growth explains about 20% of the observed rate of labor reallocation.

We emphasize two important implications of these results. First, while theories of structural change typically focus on factors decreasing the demand for agricultural labor, supply-side changes in the workforce composition and skills - what we call the "human side" of structural transformation - are quantitatively important. Second, to the extent that human capital growth can be promoted by increased access to schooling and educational reforms, these policies should be considered potential tools to accelerate the process of structural transformation.

[38]In Appendix E.7 we perform back of the envelope calculations on the relative role of (i) and (ii) for the historical experience of the countries in our sample, guided by a simple extension of the model where h_c responds endogenously to relative wages, in the same spirit as recent work by Adão et al. (2020). We find that drivers of human capital growth exogenous to sector-specific demands account for 2/3 or more of the estimated cohort component; this conclusion is driven by a low cross-country correlation between the cohort and year components.

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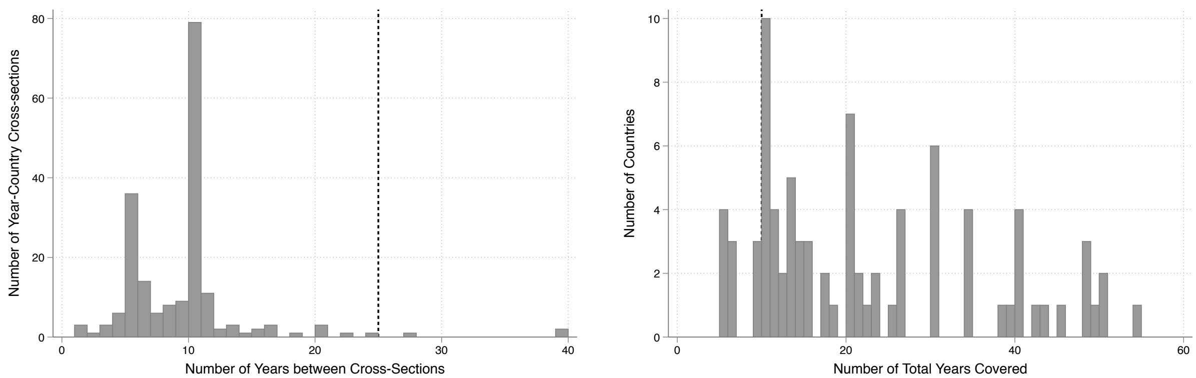
Online Appendix (not for Publication)

A Appendix Figures

Figure A.I: Frequency and Coverage of Cross-sectional Data

(a) Years Gap between Cross-Sections

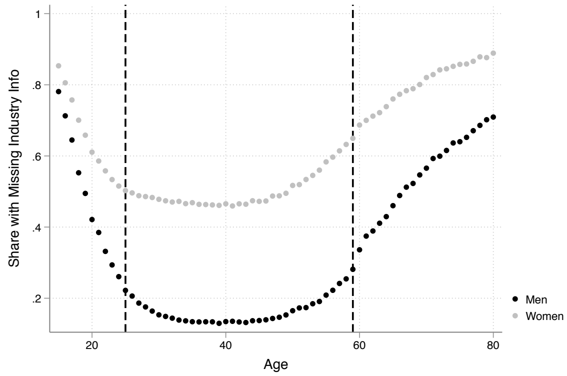
(b) Total Number of Years Covered



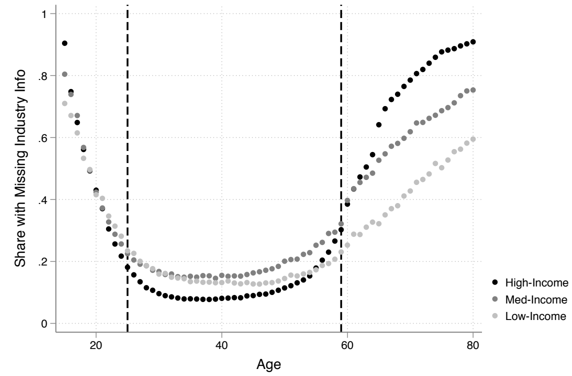
Notes: the left figure shows the histogram of the number of years between observed cross-sections in our data. We drop from the analysis countries/cross-sections to the right of the black dotted line. The right figure shows the histogram of the total numbers of years covered by each country in our data. For our benchmark sample, we exclude countries to the left of the black dotted line. All figures use IPUMS data.

Figure A.II: Missing Industry Information and Labor Force Participation

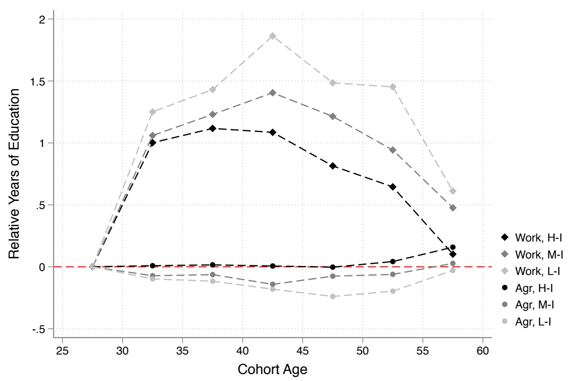
(a) Average Life-Cycle, by Gender



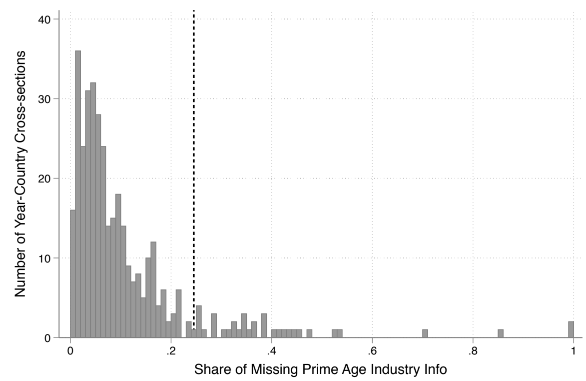
(b) Average Life-Cycle, by Income Level



(c) Selection into Labor Force



(d) Prime Age Males, across countries

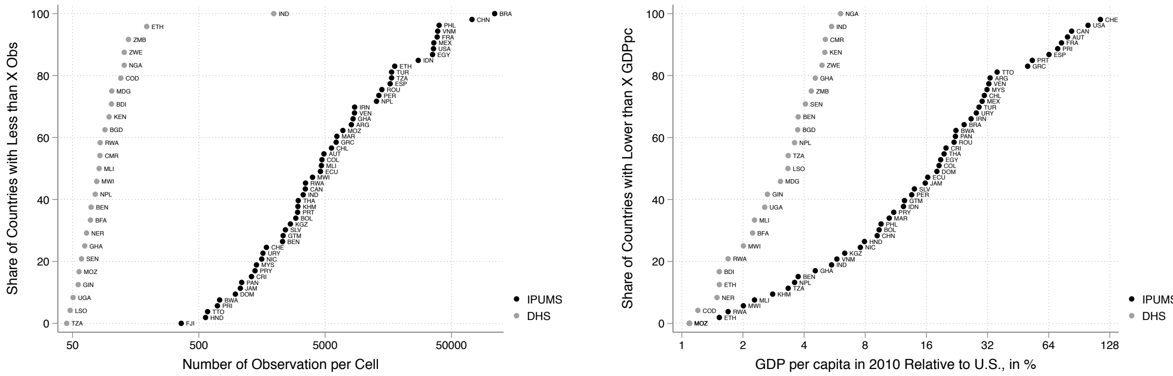


Notes: the two top figures show the share of the population by age reporting non-missing industry information. Information on industry is missing when an individual is not in the labor force. The left figure stratifies by gender and averages across all countries. The right figure keeps only males and stratifies by income group. The bottom left figure shows how the relative average years of schooling vary over the life-cycle for different groups of the population. Specifically, for each cohort-country-year, we calculate the difference between the average education of individuals in and out of labor force (Work) and in and out of agriculture (Agr). We then regress them (separately by income group) on country $\times$ cohort fixed effects and five-year age dummies; the figure reports the point estimates for the latter. We learn that as a cohort ages the average education of the individuals in the labor force initially increases steeply. This is especially true in low income countries and is driven by selection: more educated individuals are relatively more likely to be non-employed when young. We can see that when comparing individuals working in agriculture and the rest of the population there is virtually no selection over the life-cycle. This motivates us to compute agricultural employment as the share of the cohort population employed in agriculture, rather than the share of the population in the labor force employed in agriculture. In this way, we are less concerned that changes in the denominator drive our measured agricultural reallocation. The bottom right figure shows the histogram, across all our cross-sections, of the share of prime-age men that report missing industry information – i.e. that are out of the labor force. We exclude country-years that are to the right of the black dotted line. All the figures use IPUMS data.

Figure A.III: Sample Coverages: DHS and IPUMS

(a) Number of Observations

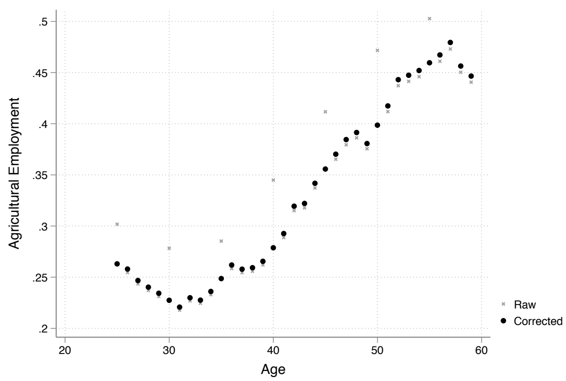
(b) GDP per capita in 2010



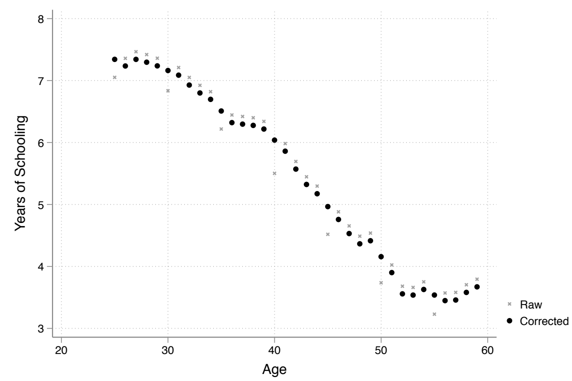
Notes: the two figures compare the coverage of the DHS and IPUMS data in terms of sample sizes (left figure) and GDP per capita of covered countries (right figure). The left figure shows the cumulative density function of countries by the average number of observation for each cohort-year-country cell: DHS data have much smaller samples. The right figure shows the cumulative density function of countries' GDP per capita, relative to the one of the United States in 2010: DHS countries have much lower income. Some countries are in both datasets.

Figure A.IV: Correction for Age Heaping: a Stark Example from Turkey 1985

(a) Agricultural Employment



(b) Years of Schooling

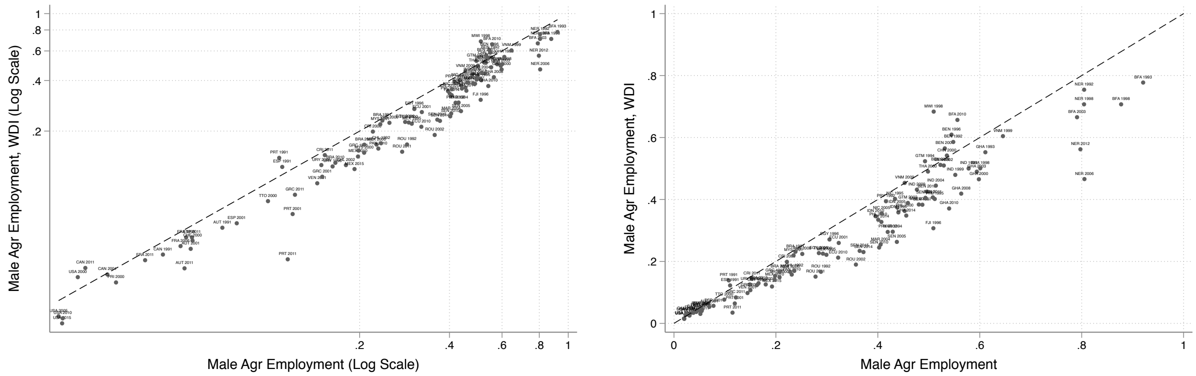


Notes: the two figures illustrate the effect of correcting our data for age heaping for one country affected by this issue. We implement the correction in three steps: i) we compute agricultural employment (or schooling) for each cohort of age in $\{30, 35, 40, 45, 50, 55\}$ as the average between the corresponding variable for one year younger and one year older cohorts; ii) we compute agricultural employment (or schooling) for the 25-year-old cohort as the difference between the original variable and the average gap between the corrected and uncorrected measures for older cohorts; iii) we renormalize so that the average agricultural employment (or schooling) is the same as for the original variable. The figures show that this procedure recovers a relatively smooth distribution over age.

Figure A.V: Comparison with World Development Indicators

(a) Log Scale

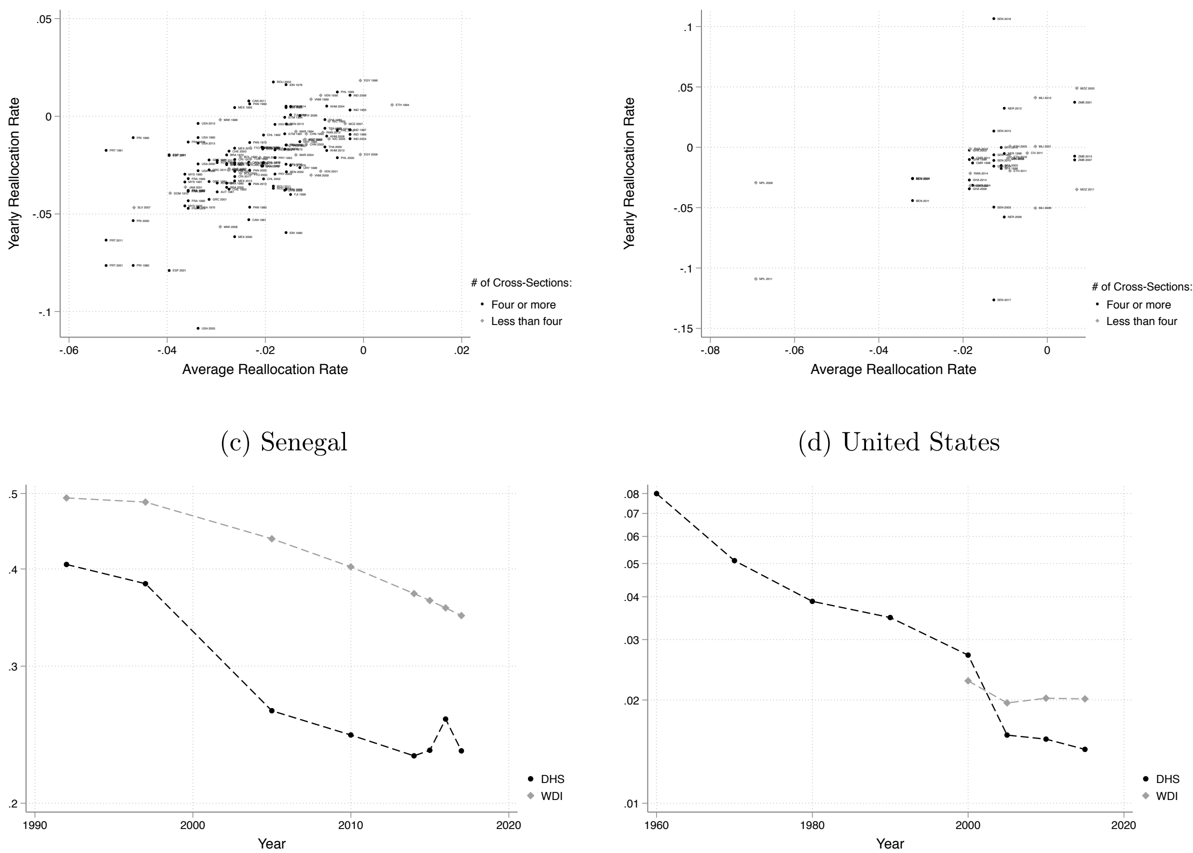
(b) Linear Scale



Notes: the figures compare the share of agricultural employment measured from our main dataset (IPUMS) with aggregate statistics from the World Development Indicators (WDI), based on ILO data. For the WDI, we use the variable: "Employment in agriculture, male (% of male employment)". We keep all country/year pairs for which both IPUMS and WDI data are available. The left figure uses a log scale (to illustrate more clearly countries with high and low agricultural employment), while the right one uses a linear scale. All countries are included in both figures. The WDI data are on average larger, since they refer to the share of the employed population in agriculture, while we consider the share of the working age population in agriculture. Yet, the cross-country patterns are very similar.

Figure A.VI: Data Anomalies Checks

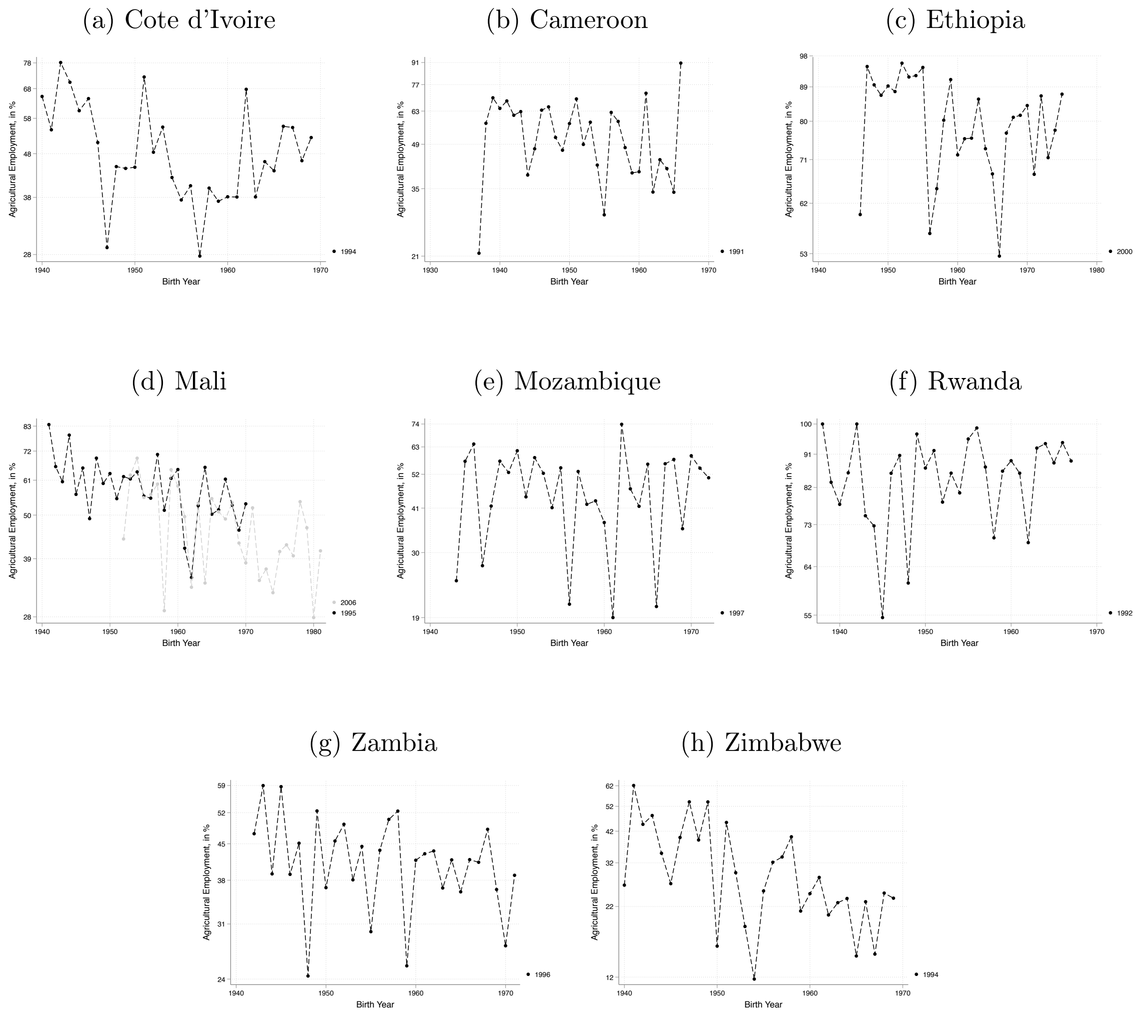
(a) IPUMS (b) DHS



(c) Senegal (d) United States

Notes: the top two figures plot, for IPUMS and DHS countries, the cross-section-specific reallocation rate as a function of the average reallocation rate across all cross-sections. The farther the points are from the 45 degree lines, the more there is within country heterogeneity in reallocation rates over time. We further distinguish between countries for which we have less or more than four cross-sections. For most countries, the within country heterogeneity in reallocation rates is limited. Three observations are clear outliers: USA 2005, and SEN 2017 and 2016. The bottom two figures show the time-series of agricultural employment for these two countries and compare them with data from the WDI. The figures show that there are anomalies in those years, which we thus exclude from the dataset. In the United States, the steep decrease in agricultural employment from 2000 to 2005 corresponded to a change in the underlying sectoral classification. We have not been able to document the reason behind the jump in agricultural employment in Senegal 2016.

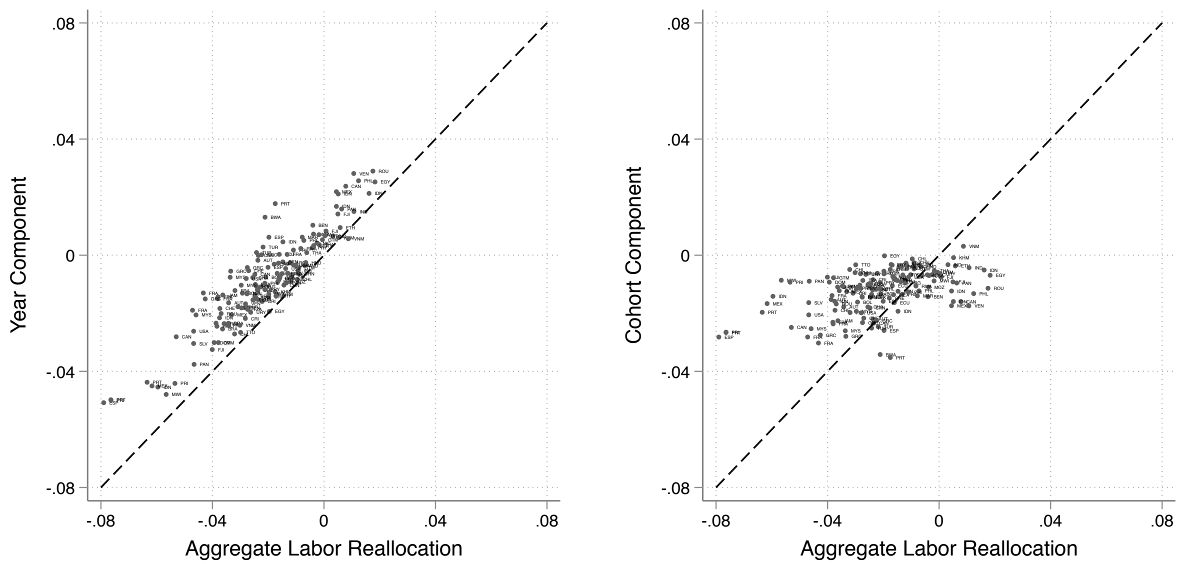
Figure A.VII: Excluded DHS Cross-Sections due to Data Anomalies



Notes: the figures plot average agricultural by birth cohort for all the cross-sections that we have excluded from our analysis. The figures show that the excluded cross-sections are very noisy, making it difficult to interpret the cohort-specific reallocation rate.

Figure A.VIII: Unpacking Aggregate Labor Reallocation, All Country-Year Observations

(a) Year Components (b) Cohort Components

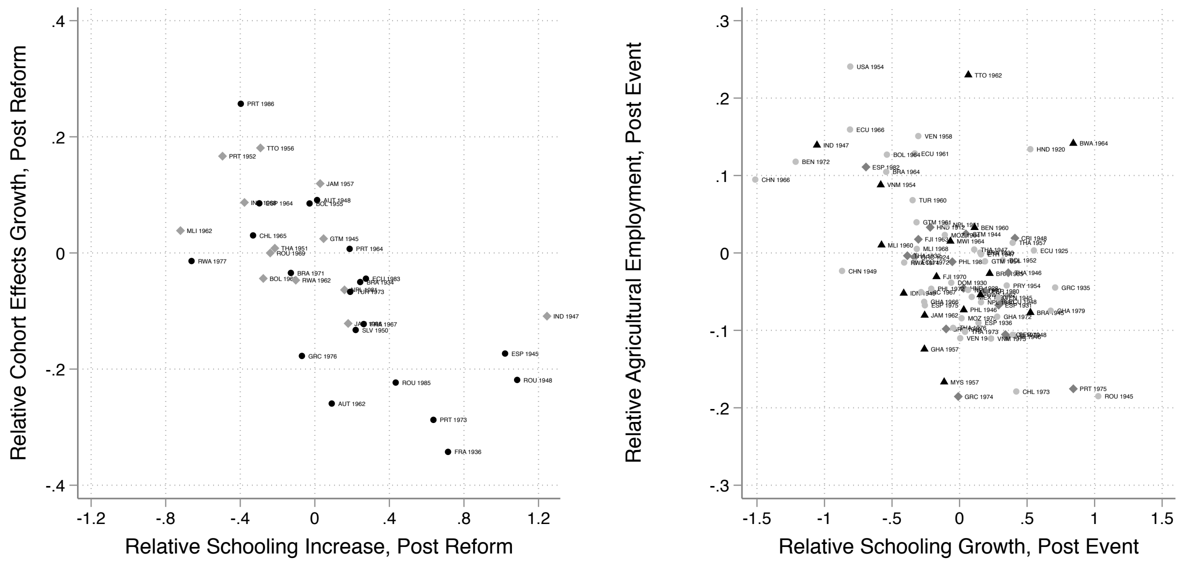


Notes: the left figure plots, for all country-year pairs, the year component as a function of the reallocation rate. The right figure plots the cohort component as a function of the reallocation rate.

Figure A.IX: Trend Breaks around Education Reforms and Political Events, Alternative Specification

(a) Schooling Reforms

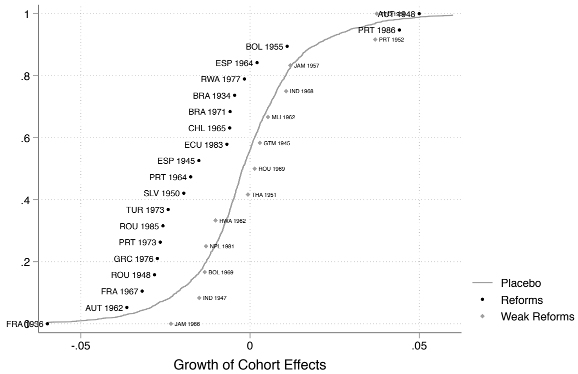
(b) Political Events



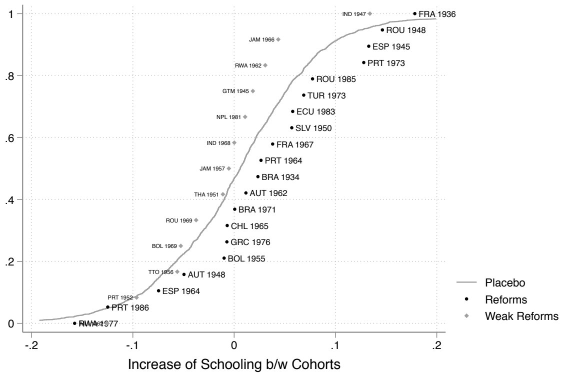
Notes: these figures are identical to Figures VIIIa and VIIIb, except that the trend breaks are computed as the average gaps, over the first 10 affected cohorts, relative to an extrapolated linear trend that starts from the average value for the 10 youngest not affected cohorts and grows at the average annual pace observed across the 10 youngest not affected cohorts.

Figure A.X: Comparison of Policy Reforms Trend Breaks with Placebo

(a) Agricultural Employment



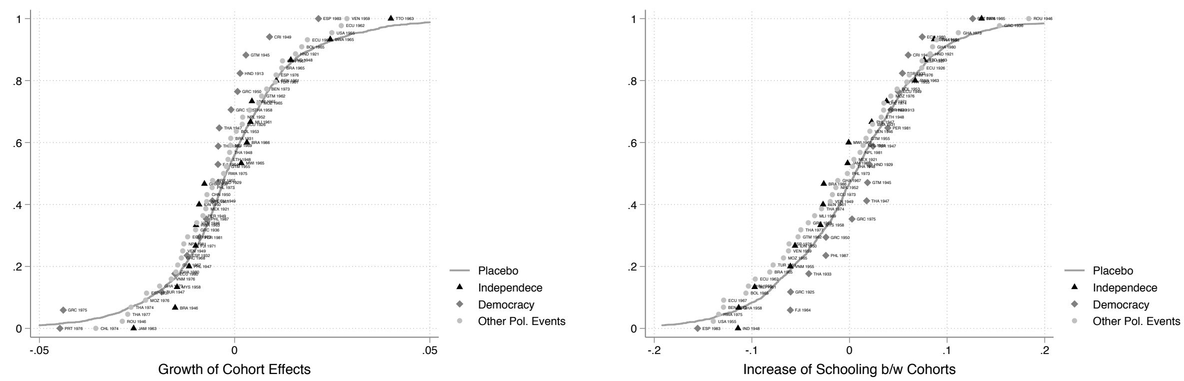
(b) Schooling



Notes: the figures compare the cumulative distribution of trend breaks, for either cohort effects (left) or schooling (right), around the policy reforms with the cumulative distribution of placebo trend breaks. The placebo trend breaks are obtained by applying around all birth cohorts in our data the same procedure used to compute the trend breaks around the policy reforms. The figures show that the trend breaks associated to the fully-implemented reforms have larger increases in schooling and decreases in cohort effects relative to the placebo. The trend breaks associated to the weakly-implemented reforms are distributed similarly to the placebo trend breaks.

Figure A.XI: Comparison of Political Events Trend Breaks with Placebo

(a) Agricultural Employment (b) Schooling



Notes: this figure is identical to Figure A.X, except that it includes political events rather than education reforms.

B Appendix Tables

Table A.I: IPUMS Countries Argentina-Kyrgyz Republic

	Country	(1) GDP pc	(2) Year Range	(3) Min. Agr. Emp.	(4) Max Agr. Emp.	(5) N. Surveys	(6) N. Obs.
(1)	Argentina	33%	1970-1980	13%	16%	2	8073
(2)	Austria	79%	1971-2011	3%	12%	5	4895
(3)	Benin	4%	1979-2013	45%	71%	4	2301
(4)	Bolivia	9%	1976-2001	27%	50%	3	2931
(5)	Botswana	22%	1991-2011	11%	19%	2	731
(6)	Brazil	25%	1960-2010	13%	53%	6	110109
(7)	Cambodia	3%	1998-2013	60%	66%	4	3046
(8)	Canada	83%	1971-2011	3%	8%	5	3486
(9)	Chile	31%	1960-2002	13%	31%	5	5619
(10)	China	9%	1982-2000	55%	66%	3	72838
(11)	Colombia	18%	1964-1973	36%	51%	2	4722
(12)	Costa Rica	20%	1963-2011	15%	51%	5	1306
(13)	Dominican Republic	18%	1960-1970	45%	63%	2	973
(14)	Ecuador	16%	1962-2010	21%	59%	5	4591
(15)	Egypt	19%	1986-2006	22%	27%	3	35479
(16)	El Salvador	14%	1992-2007	19%	39%	2	2426
(17)	Ethiopia	2%	1984-1994	83%	83%	2	17794
(18)	Fiji	-	1966-2014	31%	57%	5	363
(19)	France	74%	1962-2011	4%	19%	8	38596
(20)	Ghana	5%	1984-2010	40%	61%	3	8327
(21)	Greece	50%	1971-2011	9%	29%	5	6118
(22)	Guatemala	12%	1964-2002	39%	68%	5	2326
(23)	Honduras	8%	1961-1974	62%	74%	2	565
(24)	India	5%	1983-2009	44%	53%	6	3354
(25)	Indonesia	12%	1971-2010	35%	59%	8	27341
(26)	Iran	27%	2006-2011	11%	16%	2	8572
(27)	Jamaica	16%	1991-2001	17%	24%	2	1066

Notes: GDP pc is GDP capita as a percent of US GDP in 2000. N. Obs. is the average number of observations within year × cohort cells.

Table A.II: IPUMS Countries Malawi-Vietnam

	Country	(1) GDP pc	(2) Year Range	(3) Min. Agr. Emp.	(4) Max Agr. Emp.	(5) N. Surveys	(6) N. Obs.
(28)	Kyrgyz Republic	6%	1999-2009	40%	44%	2	2657
(29)	Malawi	2%	1987-2008	40%	71%	3	3975
(30)	Malaysia	32%	1970-2000	15%	43%	4	1431
(31)	Mali	2%	1998-2009	59%	70%	2	4674
(32)	Mexico	30%	1970-2015	12%	42%	6	36317
(33)	Morocco	11%	1982-2004	25%	36%	3	6195
(34)	Mozambique	1%	1997-2007	53%	53%	2	6914
(35)	Nepal	4%	2001-2011	50%	65%	2	12767
(36)	Nicaragua	8%	1971-2005	35%	48%	3	1574
(37)	Panama	22%	1960-2010	16%	48%	6	1092
(38)	Paraguay	11%	1962-2002	30%	60%	5	1393
(39)	Peru	14%	1993-2007	22%	27%	2	13325
(40)	Philippines	10%	1990-2010	34%	41%	4	39973
(41)	Portugal	53%	1981-2011	4%	16%	4	3030
(42)	Puerto Rico	71%	1970-2000	3%	10%	4	702
(43)	Romania	22%	1977-2011	15%	29%	4	14083
(44)	Rwanda	2%	2002-2012	61%	67%	2	3500
(45)	Spain	64%	1981-2011	5%	16%	4	16372
(46)	Switzerland	115%	1970-2000	5%	10%	4	1715
(47)	Tanzania	3%	2002-2012	52%	66%	2	16812
(48)	Thailand	20%	1970-2000	50%	73%	4	3065
(49)	Trinidad and Tobago	36%	1980-2000	8%	13%	3	587
(50)	Turkey	29%	1985-2000	22%	32%	3	16815
(51)	United States	100%	1960-2015	1%	8%	8	36093
(52)	Uruguay	28%	1963-2006	12%	22%	4	1610
(53)	Venezuela	32%	1981-2001	10%	13%	3	8549
(54)	Vietnam	6%	1989-2009	47%	61%	3	38960

Notes: GDP pc is GDP capita as a percent of US GDP in 2000. N. Obs. is the average number of observations within year × cohort cells.

Table A.III: DHS Countries

	Country	(1) GDP pc	(2) Year Range	(3) Min. Agr. Emp.	(4) Max Agr. Emp.	(5) N. Surveys	(6) N. Obs.
(1)	Bangladesh	4%	1994-2011	0%	48%	6	100
(2)	Benin	4%	1996-2011	42%	63%	4	70
(3)	Burkina Faso	2%	1993-2010	67%	78%	4	69
(4)	Burundi	2%	2010-2016	66%	68%	2	102
(5)	Cameroon	5%	1998-2011	40%	51%	3	82
(6)	Congo Democratic Republic	1%	2007-2013	46%	52%	2	121
(7)	Ethiopia	2%	2005-2016	74%	83%	3	193
(8)	Ghana	5%	1993-2014	36%	55%	5	62
(9)	Guinea	3%	1999-2012	53%	58%	3	55
(10)	India	5%	2005-2015	34%	36%	2	1961
(11)	Lesotho	3%	2009-2014	21%	32%	2	48
(12)	Kenya	5%	1993-2014	26%	46%	5	97
(13)	Madagascar	3%	2003-2008	64%	73%	2	102
(14)	Malawi	2%	1992-2016	41%	60%	5	78
(15)	Mali	2%	2001-2012	58%	59%	2	81
(16)	Mozambique	1%	2003-2011	44%	60%	2	56
(17)	Nepal	4%	2001-2011	34%	64%	3	76
(18)	Niger	1%	1992-2012	47%	74%	4	65
(19)	Nigeria	6%	1999-2008	36%	41%	3	128
(20)	Rwanda	2%	2000-2014	64%	70%	3	83
(21)	Senegal	4%	1992-2015	22%	40%	6	59
(22)	Tanzania	3%	1991-1999	67%	73%	3	45
(23)	Uganda	3%	1995-2016	48%	73%	5	51
(24)	Zambia	4%	2001-2013	44%	50%	3	139
(25)	Zimbabwe	5%	2005-2015	19%	30%	3	128

Notes: GDP pc is GDP capita as a percent of US GDP in 2000. N. Obs. is the average number of observations within year $\times$ cohort cells.

Table A.IV: Unpacking Structural Change, IPUMS Countries Argentina-Malawi

		(1)	(2)	(3)	(4)
	Country	Year Range	$\log g_{L_A}$	$\log \psi$	$\log \tilde{\psi}$
(1)	Argentina	1970-1980	-1.21	-0.27	-0.66
(2)	Austria	1971-2011	-2.98	-1.31	-0.66
(3)	Benin	1979-2013	-1.57	-0.34	-0.33
(4)	Bolivia	1976-2001	-2.51	-1.03	-1.75
(5)	Botswana	1991-2011	-2.11	1.31	-0.72
(6)	Brazil	1960-2010	-2.79	-1.76	-2.58
(7)	Cambodia	1998-2013	-0.75	-0.33	-0.46
(8)	Canada	1971-2011	-2.34	-0.28	0.16
(9)	Chile	1960-2002	-2.04	-1.38	-1.83
(10)	China	1982-2000	-1.17	-0.83	-1.53
(11)	Costa Rica	1963-2011	-2.62	-1.77	-2.83
(12)	Dominican Republic	1960-1970	-3.94	-3.01	-3.54
(13)	Ecuador	1962-2010	-2.05	-0.83	-1.31
(14)	Egypt	1986-2006	-0.06	0.29	-0.19
(15)	El Salvador	1992-2007	-4.68	-3.04	-4.56
(16)	Ethiopia	1984-1994	0.58	0.95	1.03
(17)	Fiji	1966-2014	-1.49	-0.63	-1.06
(18)	France	1962-2011	-3.57	-1.40	-1.42
(19)	Ghana	1984-2010	-2.07	-0.78	-1.46
(20)	Greece	1971-2011	-3.15	-0.62	-0.82
(21)	Guatemala	1964-2002	-1.61	-1.06	-1.30
(22)	Honduras	1961-1974	-0.65	-0.25	0.78
(23)	India	1983-2009	-0.27	0.11	-0.70
(24)	Indonesia	1971-2010	-1.58	-0.19	-0.83
(25)	Jamaica	1991-2001	-3.63	-1.37	-2.01
(26)	Kyrgyz Republic	1999-2009	-1.19	-0.91	-1.25
(27)	Malawi	1987-2008	-2.92	-2.04	-1.91

Table A.V: Unpacking Structural Change, IPUMS Countries Malaysia-Vietnam

	Country	(1) Year Range	(2) $\log g_{L_A}$	(3) $\log \psi$	(4) $\log \tilde{\psi}$
(28)	Malaysia	1970-2000	-3.64	-1.28	-1.65
(29)	Mali	1998-2009	-1.66	-0.92	-1.29
(30)	Mexico	1970-2015	-2.63	-1.13	-1.69
(31)	Morocco	1982-2004	-1.38	-0.36	-1.36
(32)	Mozambique	1997-2007	-0.37	0.73	0.46
(33)	Nepal	2001-2011	-2.75	-1.31	-2.24
(34)	Nicaragua	1971-2005	-0.70	-0.20	-1.08
(35)	Panama	1960-2010	-2.32	-1.37	-1.99
(36)	Paraguay	1962-2002	-1.80	-1.03	-1.95
(37)	Peru	1993-2007	-1.50	-0.63	-0.63
(38)	Philippines	1990-2010	-0.53	0.70	1.08
(39)	Portugal	1981-2011	-5.24	-2.53	-2.70
(40)	Puerto Rico	1970-2000	-4.69	-3.08	-3.62
(41)	Romania	1977-2011	-1.84	-0.49	-1.08
(42)	Rwanda	2002-2012	-0.83	0.23	-0.03
(43)	Spain	1981-2011	-3.96	-1.63	-1.77
(44)	Switzerland	1970-2000	-2.74	-0.89	-0.78
(45)	Tanzania	2002-2012	-2.74	-1.87	-2.44
(46)	Thailand	1970-2000	-0.79	-0.09	-0.42
(47)	Trinidad and Tobago	1980-2000	-2.31	-1.76	-0.63
(48)	Turkey	1985-2000	-2.30	0.19	-3.12
(49)	United States	1960-2015	-2.12	-0.89	-1.35
(50)	Uruguay	1963-2006	-1.30	-0.64	-0.62
(51)	Venezuela	1981-2001	-0.87	0.57	0.11
(52)	Vietnam	1989-2009	-1.07	-0.92	-1.58

Table A.VI: Unpacking Structural Change, DHS Countries

	Country	(1) Year Range	(2) $\log g_{L_A}$	(3) $\log \psi$	(4) $\log \tilde{\psi}$
(1)	Benin	1996-2011	-3.20	-1.44	-3.11
(2)	Burkina Faso	1993-2010	-1.09	0.30	-0.95
(3)	Cameroon	1998-2011	-2.01	0.27	-0.81
(4)	Ethiopia	2005-2016	-1.37	-0.38	-0.66
(5)	Ghana	1993-2014	-1.85	-0.20	-0.12
(6)	Guinea	1999-2012	-0.94	0.84	1.69
(7)	Mali	2001-2012	-0.05	0.54	0.31
(8)	Nepal	2001-2011	-6.92	-4.78	-6.86
(9)	Niger	1992-2012	-1.02	-0.11	-1.52
(10)	Rwanda	2000-2014	-1.15	0.20	-0.60
(11)	Senegal	1992-2015	-1.38	1.31	0.05
(12)	Zambia	2001-2013	-0.89	0.31	-0.26

Table A.VII: Unpacking Structural Change, Robustness

Panel (a): IPUMS Data

	Exercise	(1) $\log g_{L_A}$	(2) $\log \psi$	(3) $\log \tilde{\psi}$	(4) $\frac{\log \chi}{\log g_{L_A}}$	(5) $\frac{\log \tilde{\chi}}{\log g_{L_A}}$	(6) $1 - \frac{\log \psi}{\log \tilde{\psi}}$	(7) N. Obs
(1)	**Benchmark**	-2.05	-0.83	-1.27	0.60	0.38	0.28	52
(2)	Extended Sample	-2.19	-0.96	-1.41	0.56	0.36	0.25	54
(3)	Five-Years Age Dummies	-2.07	-0.88	-1.21	0.57	0.42	0.24	47
(4)	Country-specific $\bar{a}_j$	-2.07	-0.86	-1.24	0.58	0.40	0.24	52
(5)	One-Year Age Dummies	-2.07	-0.86	-1.20	0.58	0.42	0.23	52
(6)	Four + Cross-Sections	-2.36	-1.02	-1.29	0.57	0.45	0.21	27
(7)	Time-Specific Age Controls	-2.36	-1.02	-1.29	0.57	0.45	0.21	27
(8)	Active Workers Only	-1.99	-0.48	-0.68	0.76	0.66	0.34	54

Panel (b): DHS Data

	Exercise	(1) $\log g_{L_A}$	(2) $\log \psi$	(3) $\log \tilde{\psi}$	(4) $\frac{\log \chi}{\log g_{L_A}}$	(5) $\frac{\log \tilde{\chi}}{\log g_{L_A}}$	(6) $1 - \frac{\log \psi}{\log \tilde{\psi}}$	(7) N. Obs
(1)	**Benchmark**	-1.82	-0.26	-1.07	0.86	0.41	0.75	12
(2)	Extended Sample	-1.86	-0.57	-1.38	0.69	0.26	0.59	25
(3)	Five-Year Age Dummies	-1.98	-0.33	-1.12	0.83	0.43	0.70	11
(4)	Country-specific $\bar{a}_j$	-1.82	-0.26	-1.23	0.86	0.32	0.79	12
(5)	One-Year Age Dummies	-1.82	-0.26	-0.49	0.86	0.73	0.47	12
(6)	Four + Cross-Sections	-1.71	-0.03	-1.31	0.98	0.23	0.98	5
(7)	Time-Specific Age Controls	-1.71	-0.03	-1.36	0.98	0.20	0.98	5
(8)	Active Workers Only	-1.71	-0.24	-1.08	0.86	0.37	0.78	25

Notes: the Table shows results from the decomposition of labor reallocation according to different specifications or sample restrictions. Panel (a) includes data from IPUMS. Panel (b) includes data from DHS. Row (1) includes the specification shown in the main text, as a benchmark. Row (2) considers the extended sample of countries, as described in Section 2. Row (3) uses, as age controls, five year dummies under the linear restriction that the first two are equal. For this specification, we need to include countries whose cross-sections are at most 10 years apart, hence the sample reduces slightly. Row (4) runs the same specification as in the main text, but considers a country-specific $\bar{a}_j$, computed as we did for $\bar{a}$. Row (5) includes a full set of yearly age dummies, under the minimal linear restriction that we can impose in each country to properly identify year and cohort effects. For example, if we observe cross-sections five years apart, we constraint the first six age dummies to be identical. Row (6) only keeps countries for which we have at least four cross-sections. It serves as a comparison for row (7), where we use this restricted set of countries and allow the age effects to vary over time. Specifically, we split, within each country, the time sample in two, and we allow the age effects to be time-period specific. Comparing rows (6) and (7) show that the data are not consistent with frictions changing over time (the two rows are not identical if we include the third decimal point; while within some countries there are larger differences, they cancel out on average). Row (8) computes agricultural employment not as a percentage of the total population, but as a percentage of the population in the labor force. In view of Figure A.IIc, we restrict the sample to individuals between age 30 and 54, which are less likely to be biased by selection in and out of the labor force.

Table A.VIII: List of Education Reforms Austria-Romania

Country	Year	Weakly-Implemented	Description
Austria	1948		Marshall Plan supported education reform. Compulsory education extended by two years.
Austria	1962		Compulsory education extended by one year.
Bolivia	1955		Primary education made compulsory.
Bolivia	1969	1	Compulsory education extended by two years. Limited implementation
Brazil	1934		Four years of primary education made compulsory.
Brazil	1971		Compulsory education extended by four years.
Chile	1965		Compulsory education extended by two years.
Ecuador	1983		Six years of education made compulsory.
France	1936		Zay Reform. Compulsory education extended by one years.
France	1967		Berthoin Edict. Compulsory education extended by two years.
Greece	1976		Compulsory education extended by 3 years.
Guatemala	1945	1	Primary education made compulsory. Weak reform due to low state capacity.
India	1947	1	New constitution established that education will be compulsory to age 14. Weak reform due to low state capacity.
India	1968	1	Changed implementation and enforcement of compulsory education up to age 14. Weak reform due to limited regional coverage and low state capacity.
Jamaica	1957	1	Goal of universal primary education set. Not fully implemented until the 1960s. Weak reform due to slow phase in.
Jamaica	1966	1	Compulsory education extended by three years with assistance of international NGOs. Weak reform due to low state capacity.
Mali	1962	1	Nine years of compulsory education begins to be phased in. Weak reform due to slow phase in.
Nepal	1981	1	Five years of compulsory education established. Limited implementation. Weak reform due to low state capacity.
Portugal	1952	1	Introduction of new enforcement for compulsory education laws. Weak reform due to limited regional coverage.
Portugal	1964		Compulsory education extended by two years.
Portugal	1973		Compulsory education extended by two years.
Portugal	1986		Compulsory education extended by one year.
Romania	1948		Four years of compulsory education established. Previously there was de jure seven years of compulsory education but with little enforcement.
Romania	1969	1	Compulsory education raised to 6 years. Implemented from 1969 to 1977. Weak reform due to slow phase in.
Romania	1985		Compulsory education raised to 8 years.

Table A.IX: List of Education Reforms Rwanda-Turkey

Country	Year	Weakly-Implemented	Description
Rwanda	1962	1	New constitution includes provision for compulsory education. Weak reform due to low state capacity.
Rwanda	1977		Compulsory education extended by two years.
Slovenia	1950		Compulsory education extended by one year.
Spain	1945		Six years of compulsory schooling established.
Spain	1964		Compulsory education extended by two years.
Thailand	1951	1	Compulsory education extended. Weak reform due to low state capacity.
Trididad and Tobago	1956	1	Program for universal primary education begins to be phased in. Weak reform due to low state capacity.
Turkey	1973		Primary education made compulsory.

Table A.X: List of Political Events, Benin-Honduras

Country	Year	Description
Benin	1960	Independence from France.
Benin	1972	Military coup followed by socialist government and single party state.
Bolivia	1952	Socialist revolution.
Bolivia	1964	Military coup.
Brazil	1930	Coup followed by a single democratic election then a shift to autocracy.
Brazil	1945	Autocrat deposed. Democratic elections.
Brazil	1964	Military coup.
Brazil	1985	Democratic elections.
Botswana	1964	Indendence from the United Kingdom.
Chile	1973	Military coup.
China	1949	Establishment of single-party communist regime.
China	1966	Beginning of Cultural Revolution.
Costa Rica	1948	Democratic revolution.
Dominican Republic	1930	Start of Trujillo's autocratic regime.
Ecuador	1925	Military coup. Begins periods of political instability.
Ecuador	1948	Democratic elections.
Ecuador	1961	Military coup, followed in 1963 by another coup.
Ecuador	1966	Democratic elections.
Ecuador	1972	Military coup.
Ecuador	1979	Democratic elections.
Spain	1931	Democratic elections, start of Second Republic.
Spain	1936	Outbreak of civil war.
Spain	1975	Beginning of transition to democracy.
Spain	1982	Democratic elections.
Ethiopia	1947	End of Italian occuption that began in lead up to WWII.
Fiji	1963	Democratic reforms.
Fiji	1970	Independence from the United Kingdom.
Ghana	1957	Independence from the United Kingdom.
Ghana	1966	Military coup.
Ghana	1972	Military coup.
Ghana	1979	Military coup.
Greece	1924	Coup followed by democratic elections. Start of Second Republic.
Greece	1935	Shift to single party rule.
Greece	1949	End of civil war, democratic elections.
Greece	1967	Military coup.
Greece	1974	Democratic elections.
Guatemala	1944	Democratic elections.
Guatemala	1954	Autocratic regime established.
Guatemala	1961	Start of Guatemalan Civil War.
Honduras	1912	Democratic elections.

Table A.XI: List of Political Events, Honduras-Vietnam

Country	Year	Description
Honduras	1920	Manipulated election begins period of political instability and civil conflict.
Honduras	1928	Democratic elections.
Indonesia	1949	Independence from the Netherlands.
India	1947	Independence from the United Kingdom.
Jamaica	1962	Independence from the United Kingdom.
Mexico	1920	Democratic elections.
Mali	1960	Independence from France.
Mali	1968	Military coup.
Mozambique	1964	Outbreak of civil war.
Mozambique	1975	Independence from Portugal.
Malawi	1964	Independence from the United Kingdom.
Malaysia	1957	Independence from the United Kingdom.
Nepal	1951	Shift to limited democracy after popular revolution.
Nepal	1960	Monarchy reasserts more direct control.
Nepal	1980	Reforms reduce power of monarchy.
Peru	1948	Military coup.
Peru	1980	Democratic elections.
Philipinnes	1946	Independence from the United States.
Philipinnes	1972	Declaration of martial law ends Third Republic.
Philipinnes	1986	Democratic elections.
Portugal	1975	Democratic elections following coup in 1974.
Paraguay	1954	Autocratic regime established.
Romania	1945	Establishment of single-party communist state.
Rwanda	1962	Independence from Belgium.
Rwanda	1974	Military coup establishes stable autocracy.
Thailand	1932	Establishment of constitutional monarchy.
Thailand	1946	Democratic elections.
Thailand	1947	Military coup.
Thailand	1957	Military coup.
Thailand	1973	Democratic revolution.
Thailand	1976	Military coup.
Trinidad and Tobago	1962	Independence from United Kingdom.
Turkey	1946	First multi-party elections.
Turkey	1960	Military coup begins period of political instability.
United States	1954	Desegregation of schools begins.
Venezuela	1945	Democratic revolution.
Venezuela	1948	Military coup.
Venezuela	1958	Democratic revolution.
Vietnam	1954	Independence from France and partition into North and South Vietnam.
Vietnam	1975	End of Vietnam War.

C Dataset on Policy Reforms and Political Events

This section describes the procedure used to build the new dataset containing the dates of educational policy reforms and of political events.

C.1 Educational Reforms

We constructed a new dataset of education system reforms that increased the years of compulsory education. The same procedure was used to research all the countries for which we had data on cohort-level education and agricultural employment. First, a general search was done to identify potential reforms to compulsory education. This first search used Wikipedia, Eurydice, reference encyclopedias, Google, and Google Scholar. The identified reforms were then cross-checked against other sources and the following details were recorded: the year the reform went into effect, the age at which individuals ended compulsory education after the reform went into effect, and the total years of compulsory education after the reform went into effect.

The search proceeded as follows. First, we checked Wikipedia for articles on each country's education system and its history. Then, we consulted encyclopedias and (for European Union countries) Eurydice. Next, we performed Google and Google Scholar searches of the country's name combined with the following terms: "compulsory education", "compulsory education history", "education history", "education reform", "education reform history". Reforms were found using all these sources, but typically Google Scholar searches were the most informative. If a country's name changed over the sample period, the same searches were done with any other relevant name. For countries that gained independence over the sample period, we did searches covering both pre- and post-independence periods. If no national-level reforms to compulsory education were found for a specific country, we checked the current status of compulsory education, briefly surveyed the country's history, and checked the level of government at which education policy was set to confirm that no reforms to compulsory education should be expected. We did not include any reforms that were introduced solely at sub-national levels, unless the sub-national unit later became an independent country (though some of the included national-level reforms were implemented differentially across regions, as discussed below).

When a reform was found, the details listed above were recorded. When the initial source did not contain all the necessary information, additional sources (primarily on Google Scholar) were consulted. The recorded details for all reforms were then cross-checked against other sources through reform-specific Google Scholar and Google searches. In some cases, especially for low-income countries in the early part of the sample period, it was occasionally difficult to find multiple sources with all the details of the reform. In these cases, efforts were made to at least check that the avaliable sources did not contradict each other.

Finally, reforms were flagged as "weakly-implemented" if the consulted sources highlighted one or more the following: (i) the implementation of the reform was limited due to lack of the necessary state capacity; (ii) the reform was de facto implemented differentially across different regions or areas of the country (including cases where the reform was differentially binding because of pre-exisiting disparities in the enforcement of compulsory education); (iii) the reform was phased in slowly over time (for these cases, the first year of the phase in was recorded as the year of the reform went into effect). An example of weak implementation is Guatemala, where de jure compulsory primary education was established in 1945; however, the consulted sources suggest that there were both issues in the state provision of primary education and limited enforcement of the rules. Another example is Trinidad and Tobago, where universal primary education was introduced in 1956; while the government aimed to increase the provision and uptake of primary education as much as possible, it did not introduce strict rules compelling children to attend school.

C.2 Political Events

We also constructed a new dataset of major political events. The data construction was primarily based on the Wikipedia articles covering each country's history. Whenever the Wikipedia article for a given country was insubstantial, Google and Google Scholar searches were used. We divided political events into three categories: independence, democratization, and other. If an event could fit into multiple categories, we placed it into the independence category first, and the democratization category second.

Some countries have had rapid successions of different governments. In those instances, we tried to include the most important dates and to code them based on the medium-term rather than short-term outcomes. For example, Brazil in the 1930s had a democratic revolution, followed a by single democratic election a few years later, before the declaration of single party rule a few years after that. Since the same politician headed the state from the original revolution through the establishment of single party rule, we only include the initial revolution and classify it in the "other" category rather than democratization.

For what concerns independence, we include independence from a colonial power and/or the formation of a new nation state. Examples are Ghana in 1967 and Poland in 1919. We use the date the country officially became independent. We do not code the end of occupations as independence, based on the reasoning that the transition from a longstanding colonial government to self-rule is likely to bring about a different extent of changes in priorities compared to a return to self-rule after an occupation. For example, the end of German occupation of Czechoslovakia or the end of Italian occupation of Ethiopia are not coded as independence. In both cases, the occupying country was at war for most of the occupation period, and the occupied country had a well established independence before the occupation.

The second category of events is democratization, which we date to the first democratic election. We choose this date based on the reasoning that democratically electing a government should be more relevant to education than the date of a revolution or coup that leads to democratization; moreover, election dates are more easily identified. We do not code as democratizations cases where the first democratic election was immediately followed by an undemocratic transfer of power. Our reasoning here is that if there are benefits to democracy, they are likely to be accumulated because of the constraints and accountability democracy introduces. In the case of a single democratic election followed by the declaration of single party rule, a dictatorship, a coup, or a revolution before the subsequent election, it is not obvious whether the democratically elected government was subject to these constraints.

All other political events are treated as a single category. This includes military coups, the outbreak of violent conflict, democratic revolutions not followed by multiple elections, and the establishment and fall of communist and single-party regimes.

We cross-checked our dataset against the dataset of democratization and democratic reversal in Acemoglu et al. (2019). While our dataset covers a broader range of events and a longer time period, our data on democratization matches their dataset quite closely. Where differences exist, they are accounted for by the methodological choices discussed above.

D Comparing Magnitudes

The magnitude of the estimates in Sections 4.4 and 4.1 are not comparable, due to the different functional forms. This Appendix considers a cohort-level version of the specification in Section 4.4 to fill this gap.

We focus on the two treatment and control cohorts described in the main text. We compute for each cohort $\times$ district pair (c, d) the share of agricultural employment $l_{A,c,d}$ and the average years of schooling $s_{c,d}$, and then estimate

$$\log l_{A,c,d} = \alpha_c + \eta_d + \beta s_{c,d} + \varphi_c \xi_d + \epsilon_{c,d},$$

where $s_{c,d}$ is instrumented by the interaction between the district-level program intensity T_d and a dummy identifying the treated cohorts. The results are reported in Table A.XII. The baseline IV estimate for β is -0.104 (.094), negative but not precisely estimated (column 1). A possible confounding factor is that some cohort $\times$ district pairs display extreme values of the agricultural employment share, close to either 0 or 1. We do not necessarily expect to find an effect of schooling on agricultural participation for cohorts in districts where the agricultural sector is either the only viable employment option or basically non-existent. Consistently with this hypothesis, when we truncate the sample at either the 1^{st} and 99^{th} or at the 2^{nd} and 98^{th} percentiles in terms of agricultural employment, we obtain larger estimates for β (columns 2 and 3). We conclude that the data are mostly consistent with estimates for β in the range between -0.10 and -0.20, similar in magnitude to those reported in Section 4.1.

Table A.XII: School Construction in Indonesia: Cohort-Level Specifications

	(1) Employed in Agri	(2) Employed in Agri	(3) Employed in Agri	(4) Employed in Non-Agri	(5) Employed in Non-Agri	(6) Employed in Non-Agri
Years of Schooling	-0.101	-0.182	-0.216	0.185	0.316	0.293
	(0.114)	(0.126)	(0.134)	(0.084)	(0.136)	(0.131)
Cohort Fixed Effects	Y	Y	Y	Y	Y	Y
District Fixed Effects	Y	Y	Y	Y	Y	Y
Truncation	-	1-99	2-98	-	1-99	2-98
F Stat First Stage	14.44	12.19	11.22	13.93	9.65	9.10
Observations	1868	1830	1784	2160	2019	1998

Notes: Robust Standard Errors in Parentheses.

E Derivations and Extensions

This section includes the proofs of the Propositions of Section 5 and other relevant derivations or extensions. The proofs effectively require to solve the model analytically. As a preliminary step, we first derive in section E.1 the rate of labor reallocation out of agriculture for the frictionless case – i.e. when $i = f = 0$. We then characterize the frictional case in section E.2, providing the proofs for Lemma 1 and Proposition 1. Section E.3 contains the proof of Proposition 2. Section E.4 derives the implications on wages discussed in Section 5. Section E.5 derives the stochastic version of the model discussed in Section 6.2. Sections E.6 and E.7 illustrate the extensions with preferences for non-agriculture and endogenous human capital mentioned in Section 6.3.

E.1 Labor Reallocation in the Frictionless Case: $i = 0$ and $f = 0$

Plugging the equilibrium wages and relative price in equation (13), we get

$$l_{A,t,c} = \left[(1-\alpha)\,\theta_t^{\eta} z_t^{1-\eta\eta_z} X^{\alpha} L_{A,t}^{-\alpha-\eta\eta_L} H_t^{\eta\eta_H} h_c^{-\gamma}\right]^{\frac{v}{1-\gamma}}$$

which we can use to compute the aggregate rate of reallocation as

$$\begin{aligned}
g_{L_A} &= \frac{\sum_{c=t+1-N}^{t+1} l_{A,t+1,c}}{\sum_{c=t-N}^{t} l_{A,t,c}} \\
g_{L_A} &= \frac{\sum_{c=t+1-N}^{t+1}\left[(1-\alpha)\,\theta_{t+1}^{\eta} z_{t+1}^{1-\eta\eta_z} X^{\alpha} L_{A,t+1}^{-\alpha-\eta\eta_L} H_{t+1}^{\eta\eta_H} h_c^{-\gamma}\right]^{\frac{v}{1-\gamma}}}{\sum_{c=t-N}^{t}\left[(1-\alpha)\,\theta_t^{\eta} z_t^{1-\eta\eta_z} X^{\alpha} L_{A,t}^{-\alpha-\eta\eta_L} H_t^{\eta\eta_H} h_c^{-\gamma}\right]^{\frac{v}{1-\gamma}}} \\
g_{L_A} &= \left[g_{\theta z} g_{L_A}^{-\alpha-\eta\eta_L} g_h^{\gamma\eta\eta_H}\right]^{\frac{v}{1-\gamma}} \frac{h_{t+1}^{-\frac{\gamma\nu}{1-\gamma}} \sum_{k=0}^{N} g_h^{\frac{\gamma\nu}{1-\gamma}k}}{h_t^{-\frac{\gamma\nu}{1-\gamma}} \sum_{k=0}^{N} g_h^{\frac{\gamma\nu}{1-\gamma}k}} \\
g_{L_A} &= \left[g_{\theta z} g_{L_A}^{-\alpha-\eta\eta_L} g_h^{-\gamma(1-\eta\eta_H)}\right]^{\frac{v}{1-\gamma}} \\
g_{L_A} &= \left(g_{\theta z}^{\frac{(1-\gamma)}{1-\gamma+v(\alpha+\eta\eta_L)}} g_h^{-\frac{\gamma(1-\eta\eta_H)(1-\gamma)}{1-\gamma+v(\alpha+\eta\eta_L)}}\right)^{\frac{v}{1-\gamma}} \\
g_{L_A} &= \left(g_{\theta z}^{1-\Theta_D} g_h^{-\gamma(1-\Theta_S)}\right)^{\frac{v}{1-\gamma}}
\end{aligned}$$

Taking logs, we obtain the expression in Proposition 1.

E.2 Labor Reallocation in the Frictional Case: $i \geq 0$ and $f \geq= 0$

We assume (and later verify) that, under Assumption 3, individuals never move from non-agriculture to agriculture. As a result, we only need to solve for the optimal timing of a move out of agriculture, if any. Let's consider an individual (c, ε) that is currently in agriculture and at time t has to decide whether to move out of agriculture or not. He would move out if two conditions are satisfied: (i) moving today is better than moving in some future period; (ii) moving today is better than not moving at all.

Consider first condition (i). An individual (c, ε) prefers to move out of agriculture at t rather than at $t+1$ if and only if

$$\begin{aligned}
\sum_{k=t}^{c+N} \beta^{k-t} (1-i)\, y_{M,k}(c,\varepsilon) - f y_{M,t}(c,\varepsilon) &\geq y_{A,t} + \\
+\beta \left(\sum_{k=t+1}^{c+N} \beta^{k-t-1} (1-i)\, y_{M,k}(c,\varepsilon) - f y_{M,t+1}(c,\varepsilon) \right) & \\
\sum_{k=t}^{c+N} \beta^{k-t} (1-i)\, y_{M,k}(c,\varepsilon) - f\left(y_{M,t}(c,\varepsilon) - \beta y_{M,t+1}(c,\varepsilon) \right) &\geq y_{A,t} + \beta \left(\sum_{k=t+1}^{c+N} \beta^{k-t-1} (1-i)\, y_{M,k}(c,\varepsilon) \right) \\
(1-i)\, y_{M,t}(c,\varepsilon) - (1-\beta g_{Z_M}) f y_{M,t}(c,\varepsilon) &\geq y_{A,t} \\
y_{M,t}(c,\varepsilon) &\geq \frac{y_{A,t}}{1-i-(1-\beta g_{Z_M}) f}
\end{aligned}$$

where $g_X \equiv \frac{X_{t+1}}{X_t}$. As we show below, Assumption 2 guarantees that $1-i-(1-\beta g_{Z_M}) f \geq 0$, so that this inequality holds when ε is large enough. Substituting the equilibrium wages, we find that this condition is satisfied when $\varepsilon \geq \hat{\varepsilon}_t(c)$, where

$$\hat{\varepsilon}_t(c) = \left[h_c^{-\gamma} \left(\frac{(1-\alpha)\, p_t Z_{A,t} X^{\alpha} L_{A,t}^{-\alpha}}{Z_{M,t}} \right) \left(\frac{1}{1-i-(1-\beta g_{Z_M}) f} \right) \right]^{\frac{1}{1-\gamma}}. \tag{19}$$

is the ability level of the marginal individual of cohort c at time t. If $g_{Z_M} \geq g_p g_{Z_A} g_{L_A}^{-\alpha}$ (which, as we show below, is implied by Assumption 3), $\hat{\varepsilon}_t(c)$ is decreasing over time: as the wage per efficiency unit grows faster in non-agriculture, individuals with lower and lower ε gradually find it worthwhile to leave agriculture as opposed to spending an extra period there.

We can then verify that if it is better to move at time t rather than at time $t+1$, then it is also better to move at time t than at any time $t+x$. Following the previous derivation, an individual prefers to move at time t rather than at time $t+x$ if and only if

$$y_{M,t}(c,\varepsilon) \geq \left(\frac{1}{1-i-(1-\beta g_{Z_M}) f} \right) \left(\frac{1-\left(\beta g_p g_{Z_A} g_{L_A}^{-\alpha}\right)^x}{1-\left(\beta g_{Z_M}\right)^x} \right) \left(\frac{(1-\beta g_{Z_M})}{1-\beta g_p g_{Z_A} g_{L_A}^{-\alpha}} \right) y_{A,t}(c,\varepsilon).$$

Notice that, given $g_{Z_M} \geq g_p g_{Z_A} g_{L_A}^{-\alpha}$, $\left(\frac{1-\left(\beta g_{pA} g_{L_A}^{-\alpha}\right)^x}{1-\left(\beta g_{Z_M}\right)^x} \right)$ is decreasing in x. Therefore, if it is better to move out of agriculture at time t rather than at time $t+1$, then it must also be better to move at time t rather than at any time $t+x$.

This implies that, conditional on moving at some point, individual (c, ε) prefers to move in the first period t when the condition $\varepsilon \geq \hat{\varepsilon}_t(c)$ is satisfied. Next, we need to verify whether moving at the preferred time is better than not moving at all. This is the case whenever the present discounted value of moving out of agricultural is higher than that of not moving, that is

$$\sum_{k=t}^{c+N} \beta^{k-t} (1-i)\, y_{M,k}(c,\varepsilon) - f y_{M,t}(c,\varepsilon) \;\; \geq \;\; \sum_{k=t}^{c+N} \beta^{k-t} y_{A,k}(c,\varepsilon).$$

$$Z_{M,t} h(c,\varepsilon) \left((1-i) \sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t} - f \right) \geq (1-\alpha)\, p_t Z_{A,t} X^{\alpha} L_{A,t}^{-\alpha} \sum_{k=t}^{c+N} (\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^{k-t}$$

This inequality is satisfied for $\varepsilon \geq \tilde{\varepsilon}_t(c)$, where

$$\tilde{\varepsilon}_t(c) = \begin{cases} \left[h_c^{-\gamma} \left(\frac{(1-\alpha) p_t Z_{A,t} X^{\alpha} L_{A,t}^{-\alpha}}{Z_{M,t}} \right) \left(\frac{\sum_{k=t}^{c+N} (\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^{k-t}}{(1-i)\sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t} - f} \right) \right]^{\frac{1}{1-\gamma}} & \text{if } (1-i)\sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t} - f > 0 \\ \infty & \text{if } (1-i)\sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t} - f \leq 0 \end{cases}$$

While $\tilde{\varepsilon}_t(c)$ could be either decreasing or increasing over time, it definitely decreases at a lower rate than $\hat{\varepsilon}_t(c)$, since as long as $g_{Z_M} \geq g_p g_{Z_A} g_{L_A}^{-\alpha}$ (which, as we show below, is implied by Assumption 3) then $\left(\frac{\sum_{k=t}^{c+N} (\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^{k-t}}{(1-i)\sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t} - f} \right)$ is increasing in t (moreover, as t increases the condition $(1-i)\sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t} - f \leq 0$ is more likely to be satisfied). As a result, there is a time $\hat{t}$ such that $\hat{\varepsilon}_t(c)$ and $\tilde{\varepsilon}_t(c)$ cross. For all individuals with $\varepsilon < \hat{\varepsilon}_{\hat{t}}(c) = \tilde{\varepsilon}_{\hat{t}}(c)$, moving at the preferred time is dominated by not moving at all. The individual with $\varepsilon = \hat{\varepsilon}_{\hat{t}}(c) = \tilde{\varepsilon}_{\hat{t}}(c)$ is the lowest type of cohort c moving out of agriculture, and no other member of cohort c moves out after time $\hat{t}$. We refer to cohort c as "constrained" starting from time $\hat{t}$.

We now show that the parametric restrictions in Assumption 2 ensure that (i) cohort c is not constrained in the first period where it is alive (period c) and (ii) within all cohorts, there are some individuals in non-agriculture. Cohort c is unconstrained at time t if $\hat{\varepsilon}_t(c) \geq \tilde{\varepsilon}_t(c)$, which is satisfied if

$$\left(\frac{\sum_{k=t}^{c+N} (\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^{k-t}}{(1-i)\sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t} - f} \right) (1 - i - (1-\beta g_{Z_M}) f) \;\; \leq 1$$

$$f \leq (1-i) \frac{\sum_{k=a}^{N} (\beta g_{Z_M})^{k-a} - \sum_{k=a}^{N} (\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^{k-a}}{1 - (1-\beta g_{Z_M}) \sum_{k=a}^{N} (\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^{k-a}} \equiv \phi(a),$$

where a is the age of the cohort: $a = t - c$. Notice that $\phi(a) < (1-i)\sum_{k=t}^{c+N} (\beta g_{Z_M})^{k-t}$, which implies that if $f \leq \phi(a)$ then $\tilde{\varepsilon}_t(c) < \infty$. Importantly, $\phi(a)$ does not directly depend on time t or cohort c, but only on age a. In the last period a cohort is alive, i.e. for $t = c + N$ and $a = N$, as long as $f > 0$ the inequality cannot be satisfied, since $\phi(N) = 0$; intuitively, with a positive fixed cost it is never worthwhile to move in the last period. In the first period that a cohort is alive, i.e. for $t = c$ and $a = 0$, the inequality is satisfied if $f \leq \bar{f}$, where

$$\bar{f} \equiv (1-i)\frac{\sum_{k=0}^{N}(\beta g_{Z_M})^k - \sum_{k=0}^{N}(\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^k}{1-(1-\beta g_{Z_M})\sum_{k=0}^{N}(\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^k}$$

As long as $g_{Z_M} \geq g_p g_{Z_A} g_{L_A}^{-\alpha}$ (which, as we show below, is implied by Assumption 3), $\phi'(a) < 0$; therefore, there exists an age $\hat{a}$ such that each cohort is constrained if older than $\hat{a}$ and unconstrained otherwise.[39] From the previous discussion, it follows that $1 \leq \hat{a} \leq N$.

Next, we derive the restriction needed to ensure that the agricultural share of employment is strictly lower than 1 for all cohorts and time periods (which is needed for the rate of reallocation to be constant over time). Given that the agricultural share is decreasing across successive cohorts and over time, this condition is satisfied if the "oldest" cohort active at time 0 ("cohort $-N$") has at least one individual in non-agriculture. This will be the case if the highest ability individual in that cohort is born in non-agriculture (recall that cohorts are always constrained in the last period of their life, so nobody from cohort $-N$ will change sector at time 0), which in turn will be true if that individual would have left agriculture earlier if he had lived a full life (see the discussion in footnote 27), i.e. if

$$\hat{\varepsilon}_{-N+\hat{a}}(-N) = \left[h_{-N}^{\gamma}\frac{w_{A,0}}{w_{M,0}}\left(\frac{g_p g_{Z_A} g_{L_A}^{-\alpha}}{g_{Z_M}}\right)^{-N+\hat{a}}\frac{1}{1-i-(1-\beta g_{Z_M})f}\right]^{\frac{1}{1-\gamma}} \leq 1$$

which is satisfied if

$$i \leq \bar{i} \equiv 1-(1-\beta g_{Z_M})f - h_{-N}^{\gamma}(1-\alpha)\theta_0^{\eta} z_0^{1-\eta\eta_z} H_0^{\eta\eta_H} X^{\alpha} L_{A,0}^{-\alpha-\eta\eta_L}\left(\frac{g_p g_{Z_A} g_{L_A}^{-\alpha}}{g_{Z_M}}\right)^{-N+\hat{a}}$$

where $\frac{w_{A,0}}{w_{M,0}}$ and $L_{A,0}$ jointly solve equations (S_t) and (D_t) for $t = 0$, and the upper bound $\bar{i}$ is assumed to be positive. As stated in Assumption 2, we consider values of $f \in [0, \bar{f}]$ and $i \in [0, \bar{i}]$.

The agricultural share for cohort c at time t is given by

$$l_{A,t,c} = \begin{cases} F(\hat{\varepsilon}_t(c)) & \text{if } a_t(c) \leq \hat{a} \\ F(\hat{\varepsilon}_{c+\hat{a}}(c)) & \text{if } a_t(c) > \hat{a} \end{cases}$$

which, using the expression for $\hat{\varepsilon}_t(c)$ derived above, implies that cohort-level rate of labor reallocation between times t and $t+1$ is

[39] $g_{L_A}^{-\alpha}$ is itself a function of exogenous parameters, as derived below.

$$\log l_{A,t+1,c} - \log l_{A,t,c} = \begin{cases} \frac{v}{1-\gamma}\left(\log g_{\theta z} + \eta\eta_H\gamma \log g_h - (\eta\eta_L + \alpha)\log g_{L_A}\right) & \text{if } a_{t+1}(c) \leq \hat{a} \\ 0 & \text{if } a_{t+1}(c) > \hat{a} \end{cases}$$

as stated in Lemma 1, while the cross-sectional agricultural employment gap between two consecutive cohorts c and $c+1$ is

$$\log l_{A,t,c+1} - \log l_{A,t,c} = \begin{cases} -\frac{\gamma v}{1-\gamma}\log g_h, & \text{if } a_t(c) \leq \hat{a} \\ -\frac{\gamma v}{1-\gamma}\log g_h + \frac{v}{1-\gamma}\left(\log \frac{w_{A,c+1+\hat{a}}}{w_{M,c+1+\hat{a}}} - \log \frac{w_{A,c+\hat{a}}}{w_{M,c+\hat{a}}}\right) & \text{if } a_t(c) > \hat{a} \end{cases} =$$
$$= \begin{cases} -\frac{\gamma v}{1-\gamma}\log g_h, & \text{if } a_t(c) \leq \hat{a} \\ -\frac{\gamma v}{1-\gamma}\log g_h - [a_t(c) - a_t(c+1)]\,\frac{v}{1-\gamma}\log \frac{g_{Z_M}}{g_p g_{Z_A} g_{L_A}^{-\alpha}} & \text{if } a_t(c) > \hat{a} \end{cases}$$

where $\Lambda \equiv \frac{v}{1-\gamma}\log \frac{g_{Z_M}}{g_p g_{Z_A} g_{L_A}^{-\alpha}} \geq 0$ as long as $g_{Z_M} \geq g_p g_{Z_A} g_{L_A}^{-\alpha}$ (which we show below to be the case).[40] Aggregate agricultural employment at time t is given by

$$\begin{aligned} L_{A,t} &= \sum_{c=t-N}^{t} l_{A,t,c} = \\ &= \sum_{c=t-\hat{a}}^{t} F\left(\hat{\varepsilon}_t(c)\right) + \sum_{c=t-N}^{t-\hat{a}-1} F\left(\hat{\varepsilon}_{c+\hat{a}}(c)\right) = \\ &= h_t^{-\frac{\gamma v}{1-\gamma}}\left(\frac{w_{A,t}}{w_{M,t}}\right)^{\frac{v}{1-\gamma}} \times \\ &\times \left[\sum_{c=t-\hat{a}}^{t}\left(\frac{\left(\frac{h_c}{h_t}\right)^{-\gamma}}{1-i-(1-\beta g_{Z_M})f}\right)^{\frac{\nu}{1-\gamma}} + \sum_{c=t-N}^{t-\hat{a}-1}\left(\frac{\frac{w_{A,c+\hat{a}}}{w_{A,t}}}{\frac{w_{M,t}}{w_{M,c+\hat{a}}}}\,\frac{\left(\frac{h_c}{h_t}\right)^{-\gamma}\sum_{k=t}^{c+N}(\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^{k-t}}{(1-i)\sum_{k=t}^{c+N}(\beta g_{Z_M})^{k-t} - f}\right)^{\frac{\nu}{1-\gamma}}\right] = \end{aligned}$$

[40]Notice that here we are implicitly assuming that $\hat{a}$ is an integer. This simplifies the exposition, as it allows us to abstract from the case where some individuals with $\varepsilon > \hat{\varepsilon}_{c+\hat{a}}(c)$ do not move out of agriculture because $\varepsilon < \hat{\varepsilon}_t(c)$ in the last period their age is below $\hat{a}$, and $\varepsilon < \tilde{\varepsilon}_t(c)$ in the first period their age is above $\hat{a}$ (an artifact of the discrete timing of the model). This simplification is without loss of generality, as we can always change the frequency of the model so that $\hat{a}$ perfectly coincides with the age of one of the active cohorts.

$$
\begin{aligned}
&= h_t^{-\frac{\gamma v}{1-\gamma}} \left(\frac{w_{A,t}}{w_{M,t}}\right)^{\frac{v}{1-\gamma}} \times \\
&\times \left[\sum_{k=0}^{\hat{a}} \left(\frac{g_h^{\gamma k}}{1-i-(1-\beta g_{Z_M}) f} \right)^{\frac{\nu}{1-\gamma}} + \sum_{k=\hat{a}+1}^{N} \left(\frac{\left(\frac{g_p g_{Z_A} g_{L_A}^{-\alpha}}{g_{Z_M}}\right)^{\hat{a}-k} g_h^{\gamma k} \sum_{j=0}^{N-k} (\beta g_p g_{Z_A} g_{L_A}^{-\alpha})^j}{(1-i) \sum_{j=0}^{N-k} (\beta g_{Z_M})^j - f} \right)^{\frac{\nu}{1-\gamma}} \right] \\
&= h_t^{-\frac{\gamma v}{1-\gamma}} \left(\frac{w_{A,t}}{w_{M,t}}\right)^{\frac{v}{1-\gamma}} \Omega
\end{aligned}
$$

where the term Ω is constant over time. Using the fact that $h_t^{\gamma} = H_t \left[\left(\int \varepsilon^{1-\gamma} dF(\varepsilon) \right) \sum_{k=0}^{N} g_h^{-k\gamma} \right]^{-1}$ and taking logs, we recover equation (S_t) in the paper, with the constant λ_S given by

$$
\lambda_S = \log \Omega + \frac{v}{1-\gamma} \log \left[\left(\int \varepsilon^{1-\gamma} dF(\varepsilon) \right) \sum_{k=0}^{N} g_h^{-k\gamma} \right]
$$

Given the expression for $L_{A,t}$, the aggregate rate of labor reallocation is

$$
\begin{aligned}
g_{L_A} &= \frac{\sum_{c=t+1-N}^{t+1} l_{A,t+1,c}}{\sum_{c=t-N}^{t} l_{A,t,c}} \\
g_{L_A} &= \frac{\left(h_{t+1}^{-\gamma} p_{t+1} z_{t+1} L_{A,t+1}^{-\alpha}\right)^{\frac{v}{1-\gamma}}}{\left(h_t^{-\gamma} p_t z_t L_{A,t}^{-\alpha}\right)^{\frac{v}{1-\gamma}}} \\
g_{L_A} &= \left(\frac{\theta_{t+1}^{\eta} z_{t+1}^{1-\eta\eta_z} L_{A,t+1}^{-\alpha-\eta\eta_L} g_h^{-\gamma(1-\eta\eta_H)}}{\theta_t^{\eta} z_t^{1-\eta\eta_z} L_{A,t}^{-\alpha-\eta\eta_L}} \right)^{\frac{v}{1-\gamma}} \\
g_{L_A} &= \left(g_{\theta z}^{1-\Theta_D} g_h^{-\gamma(1-\Theta_S)} \right)^{\frac{v}{1-\gamma}}
\end{aligned}
$$

which is the expression given (in logs) in Proposition 1. Even in the presence of frictions, as long as $f \leq \bar{f}$ and $i \leq \bar{i}$, the aggregate rate of labor reallocation is identical to the frictionless case. Using the expression for g_{L_A}, it is easy to verify that under Assumption 3 $g_{Z_M} \geq g_p g_{Z_A} g_{L_A}^{-\alpha}$, which we used throughout the proof.

Last, we need to verify that workers moving from agriculture into non-agriculture do not ever go back to non-agriculture. To see this, suppose that an individual (c, ε) moves to non-agriculture at time t and then back to agriculture at time $t' > t$; moreover, let $\{\omega_s^*\}_{s=t'+1}^{c+N}$ be the sequence of the worker's occupational choices from time $t'+1$ onwards. Denote by $u_t(\omega_{t-1}, \omega_t, c, \varepsilon)$ the individual's period utility (i.e consumption) at time t, that is

$$
u_t(\omega_{t-1}, \omega_t, c, \varepsilon) = \omega_t y_{A,t} + (1-\omega_t) y_{M,t}(c, \varepsilon) - C(\omega_{t-1}, \omega_t, y_{A,t}, y_{M,t}(c, \varepsilon))
$$

From the perspective of time t, moving to non-agriculture in that period and then back to agriculture in t' must be weakly preferred to staying in agriculture between t and t',

$$\sum_{k=t}^{t'-1} \beta^{k-t}(1-i)\,y_{M,k}(c,\varepsilon) - fy_{M,t}(c,\varepsilon) + \beta^{t'-t}(1-f)y_{A,t} + \beta^{t'+1-t}\left(\sum_{k=t'+1}^{c+N} \beta^{k-t'} u_k(\omega_{k-1},\omega_k,c,\varepsilon)\right) \geq \sum_{k=t}^{t'} \beta^{k-t} y_{A,k} + \beta^{t'+1-t}\left(\sum_{k=t'+1}^{c+N} \beta^{k-t'} u_k(\omega_{k-1},\omega_k,c,\varepsilon)\right)$$

$$\sum_{k=t}^{t'-1} \beta^{k-t}(1-i)\,y_{M,k}(c,\varepsilon) \geq \sum_{k=t}^{t'-1} \beta^{k-t} y_{A,k} + fy_{M,t}(c,\varepsilon) + \beta^{t'-t} f y_{A,t}$$

Given that $fy_{M,t}(c,\varepsilon) + \beta^{t'-t} f y_{A,t} > 0$, this inequality can only be satisfied if there exists a time $\tilde{t} \in [t, t'-1]$ such that $(1-i)\,y_{M,\tilde{t}}(c,\varepsilon) \geq y_{A,\tilde{t}}$. From the perspective of time t', moving back to agriculture in that period must be weakly preferred to staying in non-agriculture for all the remaining periods. Let us consider two mutually exclusive cases: (i) the worker stays forever in agriculture after moving back, i.e. $\omega_s^* = 1$ for all $s \in [t'+1, c+N]$, and (ii) the worker moves again to non-agriculture at some time $t'' > t'$. For case (i), it must be that

$$\sum_{k=t'}^{c+N} \beta^{k-t} y_{A,k} - fy_{A,t} \geq \sum_{k=t'}^{c+N} \beta^{k-t} y_{M,k}(c,\varepsilon)$$

which can only hold if $(1-i)\,y_{M,t'}(c,\varepsilon) \leq y_{A,t'}$. This gives a contradiction, since $t' > \tilde{t}$ and $\frac{\frac{y_{A,t'}(c,\varepsilon)}{y_{M,t'}(c,\varepsilon)}}{\frac{y_{A,\tilde{t}}(c,\varepsilon)}{y_{M,\tilde{t}}(c,\varepsilon)}} = \left(\frac{g_p g_{Z_A} g_{L_A}^{-\alpha}}{g_{Z_M}}\right)^{t'-\tilde{t}} < 1$. For case (ii), it must be that

$$\sum_{k=t'}^{t''-1} \beta^{k-t'} y_{A,k} - fy_{A,t} + \beta^{t''-t'}(1-i-f)y_{M,t}(c,\varepsilon) + \beta^{t''+1-t'}\left(\sum_{k=t''+1}^{c+N} \beta^{k-t''} u_k(\omega_{k-1},\omega_k,c,\varepsilon)\right) \geq \sum_{k=t'}^{t''} \beta^{k-t'}(1-i)y_{M,k}(c,\varepsilon) + \beta^{t''+1-t'}\left(\sum_{k=t''+1}^{c+N} \beta^{k-t''} u_k(\omega_{k-1},\omega_k,c,\varepsilon)\right)$$

$$\sum_{k=t'}^{t''-1} \beta^{k-t'} y_{A,k} \geq \sum_{k=t'}^{t''} \beta^{k-t'}(1-i)y_{M,k}(c,\varepsilon) + fy_{A,t} + \beta^{t''-t'} f y_{M,t}(c,\varepsilon)$$

which again can only hold if $(1-i)\,y_{M,t'}(c,\varepsilon) \leq y_{A,t'}$, leading to a contradiction as discussed above. This proves Lemma 1 and Proposition 1.

E.3 Identification of Year and Cohort Components

Consider first the year component estimated without age controls, as defined in equation (3). In the data, this is identified by the average change across all cohorts in agricultural employment over two years. The change of agricultural employment between time t and $t+1$ for cohort c is given by

$$\begin{aligned}\log l_{A,t+1,c} - \log l_{A,t,c} &= \begin{cases} \log F\left(\hat{\varepsilon}_{t+1}\left(c\right)\right) - \log F\left(\hat{\varepsilon}_{t}\left(c\right)\right) & \text{if } t+1-c \leq \hat{a}\left(f\right) \\ \log F\left(\hat{\varepsilon}_{c+\tau}\left(c\right)\right) - \log F\left(\hat{\varepsilon}_{c+\tau}\left(c\right)\right) & \text{if } t+1-c > \hat{a}\left(f\right) \end{cases} \\ &= \begin{cases} \frac{v}{1-\gamma}\left(\left(1-\Theta_D\right)\log g_{\theta z} + \Theta_S \gamma \log g_h\right) & \text{if } t+1-c \leq \hat{a}\left(f\right) \\ 0 & \text{if } t+1-c > \hat{a}\left(f\right) \end{cases}.\end{aligned}$$

where we used the previously defined $\hat{\varepsilon}_t\left(c\right)$. Running specification (1) using data generated by our model would therefore pin down the following year component

$$\begin{aligned}\log \psi_t &= \left(\frac{\hat{a}\left(f\right)}{N+1}\right)\left(\frac{v}{1-\gamma}\left(\left(1-\Theta_D\right)\log g_{\theta z} + \Theta_S \gamma \log g_h\right)\right) \\ &= \left(1-\lambda\left(f\right)\right)\left(\frac{v}{1-\gamma}\left(\left(1-\Theta_D\right)\log g_{\theta z} + \Theta_S \gamma \log g_h\right)\right)\end{aligned}$$

where we have defined $\left(1-\lambda\left(f\right)\right) \equiv \frac{\hat{a}(f)}{N+1}$. Since $\hat{a}'(f) < 0$, then $\lambda'(f) > 0$. The year component is simply given by the share of unconstrained cohorts, multiplied by the year effect for those cohorts. Despite the heterogeneity across cohorts, the first differences estimator we run in the data correctly identifies the average year effect across cohorts.[41]

To find the cohort effect, we use the fact that by construction $\log g_{L_A} = \log \chi_t + \log \psi_t$, which gives

$$\begin{aligned}\log \chi_t &= \left(\frac{v}{1-\gamma}\right)\left(\left(1-\Theta_D\right)\log g_{\theta z} - \gamma\left(1-\Theta_S\right)\log g_h\right) - \log \bar{\psi}_t \\ &= \left(\frac{v}{1-\gamma}\right)\left[\lambda\left(f\right)\left(\left(1-\Theta_D\right)\log g_{\theta z}\right) - \gamma\left(1-\lambda\left(f\right)\Theta_S\right)\log g_h\right] \\ &= \left(\frac{v}{1-\gamma}\right)\left[\lambda\left(f\right)\left(\left(1-\Theta_D\right)\log g_{\theta z} + \Theta_S\gamma\log g_h\right) - \gamma\log g_h\right].\end{aligned}$$

Next, consider the specification of Proposition 2. The year component is identified out of reallocation of unconstrained cohorts, since the reallocation rate of older cohorts is absorbed by the age

[41] Notice that this is not the case if we run the fixed effect estimator. The reason being that, with a finite number of cross-sections, there would be a correlation between the error term and the year dummies, conditional on cohort effects. This is indeed the case why we use the two steps empirical approach. Empirically, the difference between the two estimators is very small.

dummies. This implies that

$$\log \tilde{\psi}_t = \left(\frac{v}{1-\gamma} \left((1-\Theta_D) \log g_{\theta z} + \Theta_S \gamma \log g_h \right) \right).$$

Finally, using the definition of $\log \tilde{\chi}_t$, we get

$$\begin{aligned} \log \tilde{\chi}_t &= \log g_{L_A} - \log \tilde{\psi}_t \\ & \left(\frac{v}{1-\gamma} \right) \left((1-\Theta_D) \log g_{\theta z} - \gamma (1-\Theta_S) \log g_h \right) - \log \tilde{\psi}_t \\ &= - \left(\frac{v}{1-\gamma} \right) \gamma \log g_h. \end{aligned}$$

which concludes the proof of Proposition 2. Corollary 1 comes directly from the comparison of the shown equations for $\log \tilde{\chi}_t$, $\log \chi_t$, and $\log \tilde{\psi}_t$, and from the definition of $\lambda(f)$.

E.4 Wages

An extensive literature documents the existence of large cross-sectional gaps in average wages between agriculture and non-agriculture, even when conditioning on workers' observable characteristics. However, recent work shows that the observational wage gains for workers moving from agriculture to non-agriculture (or, relatedly, for migrants from rural to urban regions) are an order of magnitude smaller than the corresponding cross-sectional gaps (see Hicks et al. (2017), Alvarez (2020), Herrendorf and Schoellman (2018)). The following Lemma shows that our model is consistent with this evidence.

Lemma 2: Agricultural Wage Gaps

Let $(\hat{c}_t, \hat{\varepsilon}_t)$ be a mover to M at time t and $\bar{w}_{M,t} \equiv \sum_{c=N-t}^{t} \int w_{M,t} h(c, \varepsilon) \, dF(\varepsilon)$ be the average wage in M, then for all periods t

$$\underbrace{\log \bar{w}_{M,t} - \log w_{A,t}}_{\text{Cross-Sectional Wage Gap}} > \underbrace{\log w_{M,t} h(\hat{c}_t, \hat{\varepsilon}_t) - \log w_{A,t}}_{\text{Wage Gain for Movers}},$$

and the wage gain for movers is given by

$$\log w_{M,t} h(\hat{c}_t, \hat{\varepsilon}_t) - \log w_{A,t} = \log \left(\frac{1}{1 - i - (1 - \beta g_{Z_M}) f} \right)$$

Proof. *The wage gap between non-agriculture and agriculture at time t of the marginal individual $(\hat{c}_t, \hat{\varepsilon}_t)$ that moves in that period is given by*

$$\begin{aligned}\log w_{M,t}(\hat{c}_t, \hat{\varepsilon}_t) - \log w_{A,t} &= \log Z_{M,t} h_{\hat{c}_t}^{\gamma} \hat{\varepsilon}_t(\hat{c}_t)^{1-\gamma} - \log(1-\alpha) p_t Z_{A,t} X^{\alpha} L_{A,t}^{-\alpha} \\ &= \log\left(\frac{1}{1-i-(1-\beta g_{Z_M})f}\right),\end{aligned}$$

where we used the equilibrium wage, and the expression for $\hat{\varepsilon}_t(c)$ derived above.

Next, notice that movers from agriculture to non-agriculture have strictly lower human capital than individuals already in non-agriculture. As a result, the fact that agriculture wages are identical for all individuals, while non-agricultural wages are strictly increasing in human capital, implies that the cross-sectional wage gap is larger than the wage gap for movers.

Intuitively, individuals sort across sectors based on their human capital. Movers at time t are indifferent between agriculture and non-agriculture, and thus they are less productive than the average non-agricultural worker. A low wage gain for movers does not necessarily mean that labor mobility across sectors is frictionless, as sometimes inferred in the literature. In fact, conditional on an individual not being constrained, the fixed cost affects her moving decision only through discounting, and a low wage gain might still be consistent with a large fixed cost. This result is driven by two features of our environment: (i) the decision to move out of agricultural is dynamic, hence individuals can choose to postpone it; (ii) relative wages change over time. As a result of these two features, the fixed cost mainly affects the timing of the movement out of agriculture, and it impacts the wage gap only marginally through discounting.

E.5 Model with Shocks

We discuss here the derivation of the stochastic variant of the model referred to in Section 6.2. As explained in the text, we consider the frictionless benchmark and augment it with shocks to cohort-level human capital, technology and demand: $h_c = h_0 g_h^c \xi_{h,c}$, $z_t = z_0 g_z^t \xi_{z,t}$ and $\theta_t = \theta_0 g_\theta^t \xi_{\theta,t}$, with $\mathbb{E}_c[\log \xi_{h,c}] = \mathbb{E}_t[\log \xi_{z,t}] = \mathbb{E}_t[\log \xi_{\theta,t}] = 0$. Individuals choose their sector of employment at each time t knowing their human capital as well as the realizations of $\xi_{z,t}$ and $\xi_{\theta,t}$. As in the baseline model, individuals in cohort c with ability above a threshold $\hat{\varepsilon}_t(c)$ choose non-agriculture; this threshold is stochastic and given by

$$\begin{aligned}\hat{\varepsilon}_t(c) &= \left[h_c^{-\gamma}(1-\alpha) X^{\alpha} p_t z_t L_{A,t}^{-\alpha}\right]^{\frac{1}{1-\gamma}} \\ &= \left[\kappa g_h^{-c\gamma} \xi_{h,c}^{-\gamma} g_{\theta z}^t \xi_{\theta z,t} L_{A,t}^{-\alpha-\eta\eta_L} \prod_{k=t-n}^{t} \left(g_h^k \xi_{h,k}\right)^{\frac{\gamma\eta\eta_h}{N+1}}\right]^{\frac{1}{1-\gamma}}\end{aligned}$$

where κ is a constant; the second equality uses $p_t = \theta_t^{\eta} z_t^{-\eta\eta_z} L_{A,t}^{-\eta\eta_L} H_t^{\eta\eta_H}$, with $H_t \equiv \prod_{c=t-N}^{t} \left(\int h(c,\varepsilon) dF(\varepsilon)\right)^{\frac{1}{N+1}}$. The agricultural share of cohort c at time t is $\hat{\varepsilon}_t(c)^{\nu}$, and the aggregate reallocation rate between t and $t+k$ is

$$\log g_{L_A,t} = \frac{1}{k} \log \frac{\prod_{c=t+k-N}^{t+k} l_{A,t+k,c}^{\frac{1}{N+1}}}{\prod_{c=t-N}^{t} l_{A,t,c}^{\frac{1}{N+1}}} =$$

$$= \frac{1}{1+\frac{\nu}{1-\gamma}(\alpha+\eta\eta_L)} \frac{\nu}{1-\gamma} \left[\log g_{\theta z} + \frac{1}{k} \log \frac{\xi_{\theta z,t+k}}{\xi_{\theta z,t}}\right] -$$

$$- \frac{(1-\eta\eta_H)}{1+\frac{\nu(\alpha+\eta\eta_L)}{1-\gamma}} \frac{\gamma\nu}{1-\gamma} \left[\log g_h + \frac{1}{N+1} \frac{1}{k} \sum_{s=1}^{k} \log \frac{\xi_{h,t+s}}{\xi_{h,t-1-N+s}}\right]$$

The year component between t and $t+k$ is given by the reallocation rate of any cohort active in both years,

$$\log \psi_t = \frac{1}{1+\frac{\nu}{1-\gamma}(\alpha+\eta\eta_L)} \frac{\nu}{1-\gamma} \left[\log g_{\theta z} + \frac{1}{k} \log \frac{\xi_{\theta z,t+k}}{\xi_{\theta z,t}}\right] +$$

$$+ \frac{\eta\eta_H + \frac{\nu(\alpha+\eta\eta_L)}{1-\gamma}}{1+\frac{\nu(\alpha+\eta\eta_L)}{1-\gamma}} \frac{\gamma\nu}{1-\gamma} \left[\log g_h + \frac{1}{N+1} \frac{1}{k} \sum_{s=1}^{k} \log \frac{\xi_{h,t+s}}{\xi_{h,t-1-N+s}}\right]$$

while the cohort component between t and $t+k$ is

$$\log \chi_t = -\frac{\gamma\nu}{1-\gamma} \left[\log g_h + \frac{1}{N+1} \frac{1}{k} \sum_{s=1}^{k} \log \frac{\xi_{h,t+s}}{\xi_{h,t-1-N+s}}\right]$$

Using this expression for $\log \chi_t$, the reallocation rate can be written as

$$\log g_{L_A,t} = \frac{1}{1+\frac{\nu}{1-\gamma}(\alpha+\eta\eta_L)} \frac{\nu}{1-\gamma} \left[\log g_{\theta z} + \frac{1}{k} \log \frac{\xi_{\theta z,t+k}}{\xi_{\theta z,t}}\right] - \frac{(1-\eta\eta_H)}{1+\frac{\nu(\alpha+\eta\eta_L)}{1-\gamma}} \log \chi_t$$

$$= (1-\Theta_D) \frac{\nu}{1-\gamma} \left[\log g_{\theta z} + \frac{1}{k} \log \frac{\xi_{\theta z,t+k}}{\xi_{\theta z,t}}\right] + (1-\Theta_S) \log \chi_t$$

E.5.1 Measurement Error

This Section illustrates the consequences of measurement error or noise in cohort-level agricultural employment. Suppose that in the data we observe $\log \widetilde{l_{A,t,c}} = \log l_{A,t,c} + u_{t,c}$, where $l_{A,t,c}$ is the employment implied by the model and $u_{t,c}$ is a mean-zero serially uncorrelated disturbance that could reflect measurement error or unmodeled shocks at the cohort $\times$ year level. The measured year reallocation rate between t and $t+1$ is given by

$$\log \widetilde{g_{L_A,t}} = \log \frac{\prod_{c=t+1-N}^{t+1} \widetilde{l_{A,t+1,c}}^{\frac{1}{N+1}}}{\prod_{c=t-N}^{t} \widetilde{l_{A,t,c}}^{\frac{1}{N+1}}} =$$

$$= \log g_{L_A,t} + \frac{1}{N+1} \sum_{c=t-N+1}^{t} (u_{t+1,c} - u_{t,c}) + \frac{1}{N+1} (u_{t+1,t+1} - u_{t,t-N})$$

while the measured year and cohort components can be written as

$$\widetilde{\log \psi_t} = \log \psi_t + \frac{1}{N} \sum_{c=t-N+1}^{t} (u_{t+1,c} - u_{t,c})$$

$$\widetilde{\log \chi_t} = \widetilde{\log g_{L_A,t}} - \widetilde{\log \psi_t} = \log \chi_t - \frac{1}{N(N+1)} \sum_{c=t-N+1}^{t} (u_{t+1,c} - u_{t,c}) + \frac{1}{N+1} (u_{t+1,t+1} - u_{t,t-N})$$

from which it follows that a regression of $\log \widetilde{g_{L_A,t}}$ on $\widetilde{\log \chi_t}$ gives the coefficient

$$\begin{aligned}
\frac{Cov\left(\log \widetilde{g_{L_A,t}}, \widetilde{\log \chi_t}\right)}{V\left(\widetilde{\log \chi_t}\right)} &= \frac{Cov\left(\log g_{L_A,t}, \log \chi_t\right)}{V\left(\log \chi_t\right)} - \\
&- \frac{1}{N(N+1)^2} V\left(\sum_{c=t-N+1}^{t} (u_{t+1,c} - u_{t,c})\right) + \\
&+ \frac{1}{(N+1)^2} V\left(u_{t+1,t+1} - u_{t,t-N}\right).
\end{aligned}$$

E.6 Model with Preferences for Non-Agriculture

This Appendix extends the model to allow for non-monetary factors to affect the sectoral choice. We assume that $h(c,\varepsilon)$ is the product of two components: $h(c,\varepsilon)^{\tau}$, the number of efficiency units that individual (c,ε) can supply to the non-agricultural sector, and $h(c,\varepsilon)^{1-\tau}$, the non-monetary value of working in non-agriculture. The exogenous parameter $\tau \in [0,1]$ modulates the relative importance of productivity and preferences; the model presented in the paper corresponds to the case $\tau = 1$ (sorting on productivity only).

The setup is identical to the one in the paper, with the following adjustments. The stock of productive human capital is defined as $H_t = \sum_{c=t-N}^{t} \int h\left(c,\varepsilon\right)^{\tau} dF\left(\varepsilon\right)$, and, given the definition of $h(c,\varepsilon)$ and Assumption 1, grows over time at rate $g_h^{\gamma\tau}$. The non-agricultural labor input is given by $L_{M,t} = \sum_{c=t-N}^{t} \int h\left(c,\varepsilon\right)^{\tau} \left(1 - \omega_t\left(c,\varepsilon\right)\right) dF\left(\varepsilon\right)$. Finally, Assumption 3 is amended as follows

Assumption 3'. *The demand shifter θ_t and relative productivity z_t change at constant rates g_θ and g_z such that*

$$\log g_{\theta z} \equiv \eta \log g_\theta + (1 - \eta\eta_z) \log g_z \quad \leq \quad \max \{0, -\Psi \log g_h\},$$

where $\Psi \equiv \frac{v\gamma(\alpha+\eta\eta_L)+(1-\gamma)\tau\eta\eta_H}{(1-\gamma)}$.

As in the baseline model, this guarantees that the decline in agricultural labor demand is large enough to generate a negative year component.

The overall utility values derived by working in the two sectors are given by $\tilde{y}_{A,t} = y_{A,t} = w_{A,t}$ and $\tilde{y}_{M,t}(c, \varepsilon) = h(c, \varepsilon)^{1-\tau} y_{M,t}(c, \varepsilon) = w_{M,t} h(c, \varepsilon)$. The sectoral choice is based on the comparison of the present values of $\tilde{y}_{A,t}$ and $\tilde{y}_{M,t}(c, \varepsilon)$, taking into account the mobility cost $C(\omega_{t-1}, \omega_t, \tilde{y}_{A,t}, \tilde{y}_{M,t}(c, \varepsilon))$. Following the derivation steps described in Appendix E.2, the aggregate rate of labor reallocation can be written as in Proposition 1, with the exception that Θ_S is given by

$$\Theta_S = \frac{v(\alpha + \eta\eta_L) + (1 - \gamma) \tau\eta\eta_H}{1 - \gamma + v(\alpha + \eta\eta_L)} \tag{20}$$

Intuitively, the more sorting is driven by non-monetary considerations (i.e. the lower τ), the less the growth in h_c across cohorts leads to a price adjustment through H_t. Moreover, the model counterparts of the cohort and year components are as in Proposition 2, again with the only exception that Θ_S is given by (20).

As a consequence, the cohort component still captures the magnitude of the overall shift in agricultural labor supply driven by the growth in h_c across cohorts, which in this version of the model reflects a combination of changes in monetary and non-monetary returns of working in non-agriculture. The equilibrium effects of this shift are mediated by the GE multiplier $1 - \Theta_S = \frac{(1-\gamma)(1-\tau\eta\eta_H)}{1-\gamma+v(\alpha+\eta\eta_L)}$. In a small open economy the multiplier does not depend on τ, and the calibration in Section 6.2 still applies. In the general case with $\eta > 0$, the multiplier does depend on τ; the estimation approach in Section 6.2 recovers the whole multiplier, without the need of taking a stand on the value of τ.

E.7 Endogenous Human Capital Extension

The model treats human capital as exogenous, and as such it does not distinguish between two possible drivers of human capital growth: (i) the endogenous response to increases over time in the relative return from working in the human capital intensive sector, and (ii) factors external to the model, such as policy-driven changes in the supply of education. This Appendix extends the analysis to implement a back of the envelope calculation on the relative importance of these two forces in our sample of countries.

We assume that the human capital of cohort c in country j can be written as a log-linear function of two terms: the endogenous relative return (in present value terms) from supplying one efficiency unit in the two sectors, and an exogenous term $\mu_{c,j}$ capturing all other drivers of human capital accumulation

$$\log h_{c,j} = \theta_w \log \frac{\sum_{k=c}^{c+N} \beta^k w_{M,j}^{k-c}}{\sum_{k=c}^{c+N} \beta^k w_{A,j}^{k-c}} + \log \mu_{c,j}$$

where $\mu_{c,j}$ grows at rate $g_{\mu,j}$ across cohorts. Using the equilibrium growth rates of wages and the definitions of the cohort and year components, the above can be rewritten as

$$\log \chi_j = \theta_w \gamma \log \psi_j - \frac{\upsilon\gamma}{1-\gamma} \log g_{\mu,j}$$

which, substituting for the expression for $\log \psi_j$ given in Proposition 2, is equivalent to

$$\log \chi_j = \underbrace{\frac{\theta_w \gamma}{1+\theta_w\gamma\Theta_S} \frac{\upsilon}{1-\gamma}(1-\Theta_D) \log g_{\theta z,j}}_{\log \chi_j^{\theta z}} + \underbrace{\frac{1}{1+\theta_w\gamma\Theta_S}\left(-\frac{\upsilon\gamma}{1-\gamma} \log g_{\mu,j}\right)}_{\log \chi_j^{\mu}}$$

This expression shows that the cohort component is given by the combination of two terms; the first, $\log \chi_j^{\theta z}$, captures the endogenous response of human capital to shifts in the agricultural labor demand driven by changes in $\theta_{t,j}$ and $z_{t,j}$ over time, while the second, $\log \chi_j^{\mu}$, is proportional to the rate of exogenous human capital growth. If $\theta_w = 0$, i.e. no endogenous link between $\log g_{\theta z,j}$ and $\log g_{h,j}$, then $\log \chi_j = \log \chi_j^{\mu}$; if $\log g_{\mu,j} = 0$, i.e. the endogenous response to $\log g_{\theta z,j}$ is the only source of human capital growth, then $\log \chi_j = \log \chi_j^{\theta z}$. Our objective here is to separately quantify $\log \chi_j^{\mu}$ and $\log \chi_j^{\theta z}$.

We proceed as follows. While $\log g_{\mu,j}$ is obviously unobservable, we can compute $\frac{\upsilon}{1-\gamma}(1-\Theta_D) \log g_{\theta z,j}$ using the estimated year and components and a calibrated value of Θ_S, since

$$\frac{\upsilon}{1-\gamma}(1-\Theta_D) \log g_{\theta z,j} = \log \psi_j + \Theta_S \log \chi_j$$

Under the assumption that $\log g_{\mu,j}$ is uncorrelated with $\log g_{\theta z,j}$, we can estimate the cross-country average of $\log \chi_j^{\mu}$, $\overline{\log \chi^{\mu}}$, as the constant in a cross-country regression of $\log \chi_j$ on $\frac{\upsilon}{1-\gamma}(1-\Theta_D) \log g_{\theta z,j}$; moreover, given the cross-country average of $\log \chi_j$ computed in the paper (-0.78, from columns 2 and 3 of Table I), the cross-country average of $\log \chi_j^{\theta z}$ can be backed out as a residual.

Table A.XIII: Endogenous vs Exogenous Human Capital

	Dependent Variable: $\log \chi_j$		
	(1)	(2)	(3)
$\overline{\log \chi^{\mu}}$ (%)	-0.679	-0.517	-0.879
	(0.181)	(0.190)	(0.165)
$\frac{v}{1-\gamma}(1-\Theta_D)\log g_{\theta z,j}$	0.062	0.151	-0.071
	(0.106)	(0.107)	(0.098)
$1-\Theta_S$	0.556	0.4	0.8
Observations	52	52	52

Notes: Robust standard errors in parentheses. $\overline{\log \chi^{\mu}}$ (%) is the estimated constant times 100.

Table displays the results for alternative calibrations of $1-\Theta_S$. When using our baseline estimate of $1-\Theta_S = 0.556$, the exogenous part of the cohort component (-0.68) accounts for almost 90% of the average cohort component (-0.77). For the range of values for $1-\Theta_S$ estimated or calibrated in the paper, roughly between 0.4 and 0.8, the exogenous part accounts for between 2/3 and more than 100% of $\overline{\log \chi}$ (columns 2 and 3).

This exercise is of course subject to a few caveats. First, some drivers of human capital growth might be correlated with the relative technological or demand growth across sectors, leading to violations of the assumption of no correlation $\log g_{\mu,j}$ between $\log g_{\theta z,j}$; for example, economic growth might decrease θ_t through income effects, and at the same time increase μ_c through an increase in educational spending. It seems plausible that, as in this example, many unobservable drivers of human capital growth will be positively correlated with changes the relative return from working in non-agriculture $\frac{w_{M,t}}{w_{A,t}}$, therefore loading positively on the $\frac{v}{1-\gamma}(1-\Theta_D)\log g_{\theta z,j}$ term in the previous regression and leading us to underestimate the absolute magnitude of $\overline{\log \chi_j^{\mu}}$. On the other hand, our estimate of $\frac{v}{1-\gamma}(1-\Theta_D)\log g_{\theta z,j}$ is likely to be noisy, leading us to underestimate the slope in the previous regression and infer a value of $\overline{\log \chi_j^{\mu}}$ too close to $\overline{\log \chi}$.

Keeping these caveats in mind, this back of the envelope calculation is consistent with a large role for drivers of human capital growth exogenous to the relative return of working in different sectors. This conclusion resounds well with the fact that our sample covers an historical period when many countries embraced broad development strategies leading to large policy-driven expansions of educational attainment (Pritchett, 2001).