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Evidence from Ghana

Emma Whitelaw (University of Cape Town)

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Emma Whitelaw (University of Cape Town)
Nicola Branson (University of Cape Town)

G²LM|LIC
c/o IZA – Institute of Labor Economics
Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-510

Email: glm-contact@iza.org

I Z A Institute of Labor Economics
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ABSTRACT

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Following substantial progress towards universal primary education, governments have increasingly shifted attention to reducing barriers at the secondary school level. Tertiary education participation remains rare across Sub-Saharan Africa, meaning secondary school is increasingly the final stage of education most young people will complete before entering the labour market. In this paper, we study the labour market consequences of Ghana's Free Compulsory Universal Basic Education (1996), a component of which removed fees at the lower secondary level. Using a difference-in-differences design based on treatment intensity and birth-cohort exposure, we estimate the effects of the reform on forms of economic (in)activity, disaggregated by gender. We find that the reform raised educational attainment: scaled by the 10th-90th percentile range of the observed dose, the average causal response implies years of education rose by roughly a quarter of a year, junior high school completion by 2.8 percentage points and literacy by 4.6 percentage points. In the labour market, the reform bolstered employment in the districts where it had most scope to operate, and did so along different margins for men and women. Men's participation in any productive activity was unchanged while employment for pay or profit rose (3.5 percentage points) and production for own final use fell (2.9 percentage points) – a movement across the ICLS-19 production boundary. This occurred alongside a 2.5 percentage point gain in skilled occupations and a 2.4 percentage point gain in public sector employment. Women also left own-use production, by 3.9 percentage points, but only about three quarters of that exit entered employment: the remainder moving out of work and into care/family responsibilities. Women's employment gain was almost wholly informal and vulnerable, with wage employment and skilled work falling. Demographic evidence suggests that part of women's withdrawal reflects the timing of childbearing rather than a permanent change in labour supply.

JEL Classification:
I21, I26, O15, O55

Keywords:
Continuous difference-in-difference; free secondary education; labour force; indicators; gender

Corresponding author:
Emma Whitelaw (University of Cape Town)
Email: emma.whitelaw@uct.ac.za

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1 Introduction

Africa is the only region in the world with a growing youth population, and the share of this population that is transitioning into secondary education is simultaneously rising (Mastercard Foundation, 2020). Off the back of many successful free primary reforms, governments are increasingly shifting policy focus to participation in secondary education to meet SDG 4: ensuring that all girls and boys complete free, equitable and quality primary and secondary education. With high exit from schooling before 12 years completed and low enrolment at the tertiary level, secondary education is increasingly the main platform from which youth enter the labour market. Secondary education has therefore been deemed the 'next frontier' that positions the region to harness its anticipated demographic dividend.

In Ghana, secondary education attainment has expanded markedly over recent decades, alongside successive policy reforms and substantial investment. Nation-wide, the government abolished fees at the lower secondary level (junior high school, JHS) in 1996 and at the upper secondary level (senior high school, SHS) in 2017. The former preceded many other comparable reforms in the region, making the cohorts it reached some of the first in sub-Saharan Africa to be observable at prime working age. Ghana is therefore well placed to address questions about free secondary education and labour market outcomes.

Better labour market outcomes for more educated cohorts are not obvious *ex ante*. Fee removal changes not only the quantity of schooling but the composition of those receiving it: students induced to continue by the removal of fees may differ systematically from inframarginal students in ability, household resources, and outside options, so the average return to the additional schooling need not resemble the return estimated on earlier cohorts. Expansion may also dilute quality where inputs do not scale with enrolment, in which case even inframarginal students may have worse outcomes than they otherwise would. Fee removal can also raise attainment without raising the number of jobs that reward it. Whether educational expansion translates into productive employment is therefore an empirical question. Ultimately, whether the continent stands to benefit from its demographic opportunity will depend on the extent to which gains in educational attainment are converted into meaningful improvements in economic participation.

This paper uses Ghana's 1996 Free Compulsory Universal Basic Education (FCUBE) reform, which abolished fees across basic education. Our margin of interest is its extension of free primary to free and compulsory lower secondary (or junior high school; JHS), and we therefore ask how fee-free JHS affected labour market outcomes 25 years later.[1] We use a continuous difference-in-differences design that combines pre-reform district-level treatment intensity with birth-cohort exposure to estimate effects on educational attainment, labour force participation and employment, separately for men and women. The design recovers the causal effect of the reform rather than the return to an additional year of schooling (which would require an instrumental variables approach). The policy-relevant parameter, however, is arguably the

[1] We do not attempt to isolate the fee channel from the supply-side inputs delivered alongside it, nor the lower secondary component from the wider basic education package; our estimates should be read as the effect of the reform as implemented. We consider this to be the policy-relevant parameter: national fee abolition is, in practice, accompanied by measures to expand capacity, and it is the combined package that governments contemplating free lower secondary must evaluate.

former; it is reforms that governments choose rather than years of schooling.

Evaluations of fee-free schooling reforms in Africa have focused predominantly on demographic outcomes (e.g. Brudevold-Newman, 2021; Kazibwe & Li, 2025; Keats, 2018). Where labour market outcomes of secondary education policies appear in this literature, they are typically secondary: Branson and Whitelaw (2026), Brudevold-Newman (2021), and Kazibwe and Li (2025) report labour market indicators as downstream results rather than as the object of analysis. Other studies of labour market outcomes and education policies in Ghana have been at different margins. The study engages labour market returns to a nation-wide education reform most directly, Adu Boahen et al. (2021), evaluates a policy that altered the duration of schooling rather than its price. The authors find the reform increased the probability of working in the formal sector by 8.2 percentage points and of working as an employee by 7.8 percentage points. At the SHS margin, Duflo et al. (2025) study the labour market impacts of a scholarship programme in rural Ghana. They find that 15 years later female recipients were two times as likely to be employed in the public sector compared to female non-recipients. However, they point to excess queueing as a partial equilibrium effect of the scholarships, as individuals attempt multiple times to qualify for tertiary education and obtain employment in desirable but rationed government jobs. Dinkleman (2026) studies women's work and free primary policies on the continent, including Ghana, using harmonised census data. She shows higher female labour force participation and probability of being in work following free primary education (1961) in Ghana. This is not the case in all countries she studies, however, and she concludes that increases in women's work alongside increases in schooling are not guaranteed.

We identify three key contributions to this literature. First, empirically, we provide estimates of the medium- to long-term labour market effects of fee-free lower secondary education in Ghana, for both men and women. There are 25 years between the first eligible cohorts enrolling in JHS and our data source – the 2021 census – allowing sufficient time for labour market entry and progression. Whether the free secondary reform impacted the productive labour market engagement of the cohorts exposed – a necessary condition for a demographic dividend – has been largely left untested in the fee-abolition literature in Africa.

Second, from a measurement perspective, Ghana's 2021 population and housing census (PHC) permits an analysis of labour market indicators defined according to the 19th International Conference of Labour Statisticians (ICLS 19) standards (International Labour Office, 2013), which narrowed the definition of employment strictly to work done for pay or profit. Where education moves individuals from subsistence work into paid work, as we find in Ghana, previous definitions of employment will return a null result. The questions asked in Ghana's 2021 PHC allow us to apply the narrow definition of employment, and thereby observe a reallocation of work across the production boundary despite no change in the share of the cohort working. The ICLS-19 underutilisation spectrum[2] additionally separates an underemployment margin from a discouragement margin, and we show that the improvement in utilisation operates on the discouragement margin for men and on the hours margin for women – a distinction that

[2]Two of our underutilisation measures cannot be re-constructed exactly per the ILO definition from the census (Section 3.1), but the direction of each departure from the intended concept is known and they are, to our knowledge, the closest approximation to the international standard currently reported for a national fee-abolition reform in sub-Saharan Africa.

would be hidden in a single unemployment rate. In this regard, we also bring a nuanced focus to women's labour market outcomes.

Third, we contribute methodologically. Studies exploiting variation in exposure intensity to schooling reforms have largely relied on two-way fixed effects (TWFE) specifications, including recent work on fee-free topics (e.g. Brudevold-Newman, 2021; Kazibwe & Li, 2025; Stenzel et al., 2024). Callaway et al. (2026) show that with a continuously distributed treatment, linear TWFE regression can fail to deliver an interpretable summary parameter. We therefore estimate and report the average causal response to treatment following the aggregation recommended by Callaway et al. (2026), and present TWFE estimates alongside as a benchmark.

We show that the reform raised educational attainment overall. Moving from the 10th to the 90th percentile of treatment intensity raises years of education by roughly a quarter of a year, completion of at least JHS by 2.8 percentage points, completion of at least primary by 4 percentage points and the share literate by 4.6 percentage points.

Our specification allows us to observe that the education response is not monotone in intensity: among women, it peaks at intermediate exposure and at the highest level of treatment exposure it is indistinguishable from districts with the lowest treatment exposure. This is consistent with supply-side constraints and other attendance-related direct and indirect costs binding over and above tuition expenses. Notably, the gain in literacy is larger than for any of the other attainment margins and is concentrated among women. This suggests that expansion at this margin did not come at the expense of the skills schooling is meant to impart.

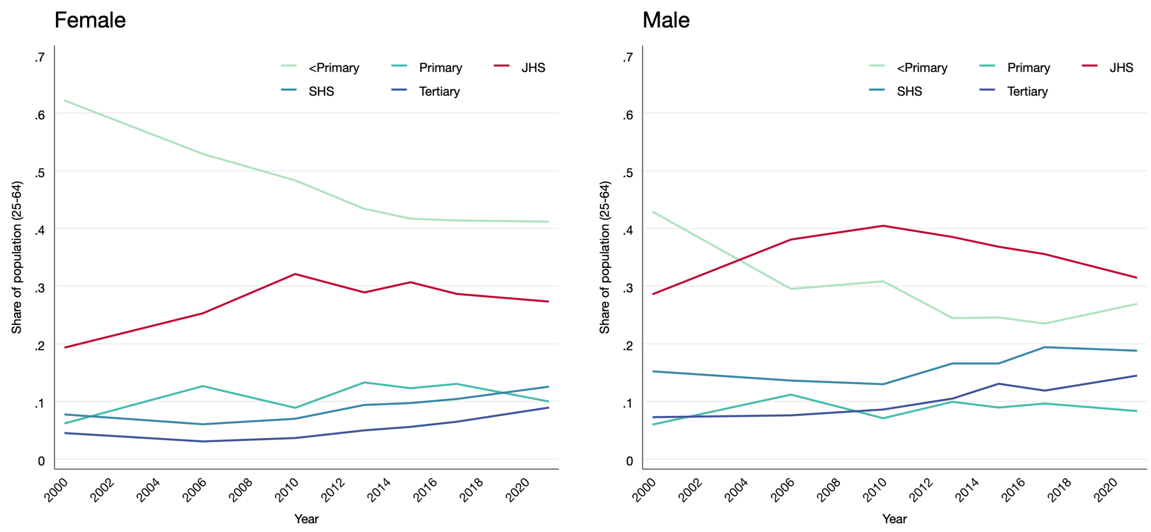
Our central finding is that the reform reallocated work rather than expanding it, along different margins for men and women. Men's participation in any productive activity is unchanged while employment for pay or profit rises (by 3.5 percentage points) and production for own final use falls (by 2.9 percentage points) – a movement across the ICLS-19 production boundary – alongside a 2.5 percentage point gain in skilled occupations and a 2.4 percentage point gain in public sector employment. Women also leave own-use production (by 3.9 percentage points) but only about three quarters of that exit enters employment: their participation in any work activity falls by 0.9 percentage points, the remainder moving into care and family responsibilities. Their additional employment is almost wholly informal and own-account. Demographic evidence suggests that part of women's withdrawal reflects the timing of childbearing rather than a permanent change in labour supply.

The paper proceeds as follows. Section 2 describes Ghana's educational context, locating and detailing the 1996 reform in this institutional setting, and considers existing evidence from studies of education reforms in Ghana. Section 3 introduces the census data and the construction of key ICLS-19-consistent labour market indicators. Section 4 sets out the continuous difference-in-differences framework, outlines the construction of our exposure and treatment measures within this framework, and examines the plausibility of the identifying assumptions. Section 5 presents descriptive statistics, followed by our results on educational attainment and on labour market outcomes by gender in Section 6. Section 7 concludes.

2 Policy context and related studies

2.1 Ghana's education system and the FCUBE reform

Figure 1 shows trends in educational attainment in Ghana by gender, highlighting substantial educational expansion over the period 2000-2021. Most striking is the sharp decline over time in the share of the population with less than primary education. For women, the share completing JHS rose steadily until around 2010, where the share plateaued while completion of upper secondary (SHS) continued to rise. For males, the share whose highest completed level is JHS rose until around 2010 and has declined since – largely because SHS and tertiary completion rose faster than they did for females. Because the figure reports shares by *highest* level completed, a falling JHS share reflects upgrading past JHS, rather than fewer men reaching it.



Source: Own calculations using Ghana Education and Labour Series (GELS) 2000-2021. Cross entropy weighted.

Figure 1: Population shares by gender and highest education level completed (ages 25-64)

These trends follow purposeful education policy reforms and investments. One of the first major reforms was the 1987 New Educational Reform Programme (NERP), which reduced pre-tertiary education from 17 to 12 years: six years of primary school, three years of junior secondary school (from four years of middle school), and three years of senior secondary school (from seven years). Ghana's current education system still follows this 6:3:3 structure. Basic education spans kindergarten, primary and JHS, while secondary education includes SHS and vocational training. Tertiary education comprises universities and other post-secondary institutions.

In addition, Ghana has successively made public basic and secondary education free. Free primary education was first introduced in 1951 under the Accelerated Development Plan (ADP), which aimed to rapidly expand access. Free Compulsory Universal Basic Education (FCUBE) made primary and JHS free and compulsory in 1996, which extended the scope of free education to the lower secondary level.

FCUBE grew out of the 1992 constitution's framing of basic education as a right, and was formally launched in 1996 with the commitment to make the full nine years of basic schooling free and compulsory for every school-age child by 2005, alongside a quality-improvement objective. It was financed in large part by the World Bank, which had been funding the Ghanaian basic education since 1986, as part of the wider 1987 restructuring.

FCUBE covered tuition, textbooks, teaching and learning materials, a subsidy on exercise books, and part of Basic Education Certificate Examination (BECE) fees. Households remained responsible for uniforms, bags, stationery, feeding, and transport. Schools that lost tuition revenue often recovered it, however, through levies and "school improvement" charges. Although FCUBE carried a compulsory component, enforcement was week (see Akyeampong, 2009, for a detailed review of the policy).

FCUBE meant that those already enrolled in primary in 1996 became eligible for free JHS, thereby also raising the return to completing primary. Those too young for primary school in 1996 would subsequently enrol knowing that nine years of schooling would be free and compulsory.

Building on FCUBE, free senior secondary education was introduced in 2017, which eliminated tuition and other schooling-related fees for all students attending public SHS.

2.2 Economic activity in Ghana and the measurement of employment

From 2016, the Ghana Statistical Service adopted the 19th ICLS standards for labour statistics, allowing for self-employment to be disaggregated by its final intended use – own consumption or for market sale. Figure 2 shows that a non-negligible share of both men and women are engaged in own-use production work – 11% and 10% respectively.

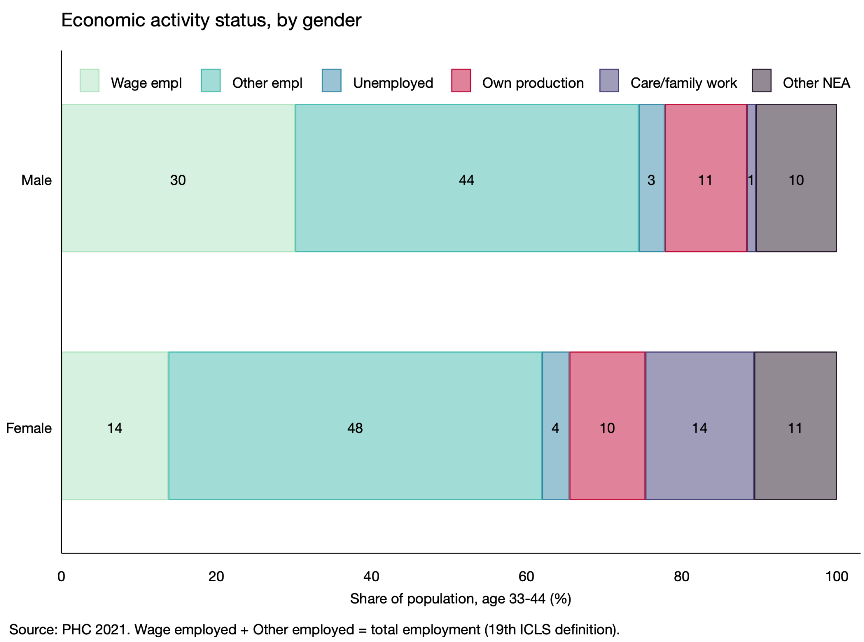


Figure 2: Activity status by gender, sample ages 33-44

The figure also shows the incidence of wage and non-wage (other) employment[3]. Non-wage

[3]97% of which is self-employment; the remaining 3% is contributing family work, domestic work and apprenticeships.

employment dominates for both men and women, although men are roughly twice as likely to be employed in wage employment than women (30% vs. 14%).

Most data sources used to study fee-removal reforms on the continent predate the implementation of ICLS-19, therefore existing literature has largely had to implement the older definition that groups own production with the ‘other employment’ category shown here. If education moves women out of subsistence production and into paid work, this is exactly the shift the older definition cannot register — and it may matter most for women, who more likely combine own-account work with care responsibilities.[4]

2.3 Related literature

Studies of related educations reform in sub-Saharan Africa can be arranged along two dimensions: the margin the reform operated on – i.e. the level of schooling, and whether the reform changed access, duration or price – and the outcome the study measured. Table 1 maps the closest related studies onto these dimensions, highlighting the gap in a nationally assigned, price-based reform at the lower secondary margin evaluated against labour market outcomes.

Table 1: Related studies by reform margin and outcome

Study	Country	Margin	Assignment	Main outcomes
Dinkleman (2026)	Multi-country	Free primary	National	Labour force participation
Keats (2018)	Uganda 1997	Free primary	National	Fertility
Adu Boahen et al. (2021)	Ghana 1987	Duration	National	Labour market
Adu Boahen (2025)	Ghana 1987	Access	National	Fertility
Frimpong and Ayibor (2025)	Ghana 1987	Duration	National	Fertility
Branson and Whitelaw (2026)	Ghana 1996	Free JHS	National	Intergenerational mobility
Brudevold-Newman (2021)	Kenya 2008	Secondary fees	National	Fertility
Kazibwe and Li (2025)	Uganda 2007	Free secondary	National	Fertility, empowerment
Stenzel et al. (2024)	Ghana 2017	Free SHS	National	Completion
Duflo et al. (2025)	Ghana	Secondary fees	Individual	Labour market
Ozier (2018)	Kenya	Secondary access	Individual	Employment type
This paper	Ghana 1996	Free JHS	National	Labour market (19th ICLS)

A first-order question is whether removing fees raises attainment at all. Stenzel et al. (2024) exploits district-level variation in the uptake of Ghana’s 2017 Free SHS policy and estimates a 15 percentage point increase in completion rates, with the largest gains among girls in districts where enrolment expanded most. At the JHS margin, Branson and Whitelaw (2026) study the same 1996 reform we do, using variation in treatment intensity and birth-cohort exposure.[5] Their outcome is intergenerational education mobility, and they find the reform had only a modest effect overall, suggesting that expanded access did not by itself dislodge structural inequalities; the exception is the northern regions, where free JHS strengthened upward mobility and narrowed regional disparities.

Much of the other literature that causally assesses the impact of educational reforms on the African continent, including Ghana, has focussed on fertility outcomes among women (e.g.

[4]The census questions unfortunately cannot speak to the extent of this combination.

[5]We use a similar pre-reform transition rate to construct treatment intensity, and impose the same policy cut-off, but differ in the outcomes focussed on – labour market rather than intergenerational education mobility – as well as estimation strategy. We return to this discussion in Section 6.4.

Brudevold-Newman, 2021; Duflo et al., 2024; Kazibwe & Li, 2025; Keats, 2018; Osili & Long, 2008). In Ghana, two recent papers exploit the 1987 reform, which moved access and duration in opposite directions. Adu Boahen (2025) uses the PHC 2021 and a cohort discontinuity in exposure to study the effects of *access* on fertility: because the shorter cycle lowered the time cost of reaching and completing secondary school, exposed cohorts enrolled at higher rates. Adu Boahen (2025) finds that this resulting rise in secondary schooling reduced fertility and extended age at first birth, attributing the effect to knowledge acquisition, opportunity cost, greater autonomy, and an "incarceration effect" — the mechanical protection afforded by time spent enrolled and therefore away from marriage and childbearing risk. Frimpong and Ayibor (2025), using pooled Ghana Demographic and Health Surveys, instead hold attainment fixed and instrument the change in *duration* conditional on the level completed, a design that isolates the incarceration channel from any change in access. They conclude that duration matters for fertility at the basic level but not at the secondary level.

Attainment and labour market outcomes

A smaller literature follows cohorts exposed to education reforms into work. Adu Boahen et al. (2021) study Ghana's 1987 reform that altered the duration of schooling rather than its price. The authors find improved labour market outcomes for treated cohorts, driven by a quantity rather than a quality effect, with stronger gains for women.

Dinkleman (2026) examines free primary policies and women's work across several African countries, including Ghana, using harmonised census data. She finds higher female labour force participation and employment following Ghana's 1961 free primary reform, but not in every country she studies, and concludes that increases in women's work alongside increases in schooling are not guaranteed. Because the timing of free primary policies forces her to rely on censuses collected before ICLS-19, she is unable to separate own-account work from work for pay or profit.

At the secondary margin, Duflo et al. (2025) find that 15 years after receiving secondary school scholarships, female recipients in Ghana were twice as likely to be in public sector employment as non-recipients, a rise from 6.7% to 13.4%. They also document excess queueing stemming from rationed but highly attractive public sector jobs. Recipients attempt repeatedly to qualify for tertiary education and government employment, and do not appear to commit to an alternative occupation while they wait. Their design assigns scholarships to individuals, so the estimates hold the local economy fixed and capture partial equilibrium effects only. Ozier (2018) uses score cut-offs on Kenya's primary school leaving examination, finding that secondary schooling reduced low-skill self-employment and weakly increased formal employment for older cohorts of young men. The admissions cutoff does not induce a statistically significant change in secondary schooling for women in his sample.

Evidence from panel data in other lower-middle income sheds light on employment transitions of working age women. In India, Sarkar et al. (2019) highlight a number of results relevant to framing the context of women's work and the mechanisms that underpin the results we observe (even though we only observe a point in time). They show an increase in wealth and income of other members of the household leads to lower probabilities of entry into employment and higher probabilities of exit from employment for women, and that a large public workfare program significantly reduces women's exit from the labour force. The find that a new born child induces exit, but having a mother or father in-law co-resident is associated with higher employment probabilities at baseline. Importantly, they show that the correlates of entry and exit/retention do not have the same effect on the likelihood of new participation, and that the same variable may affect entry and exit in different ways. They conclude that encouraging women to take up employment and improving women's retention in the labour market may not be effectively tackled by the same policy instruments.

Duflo et al. (2025), Dinkleman (2026) and Sarkar et al. (2019) additionally all speak to social norms around child rearing and societal norms that stigmatise women's market work as potential channels that affect labour market participation. Dinkleman (2026) additionally considers the scarcity of "clean" (office and white collar) jobs suitable for women as a potential mechanism keeping women out of the labour force. Duflo et al. (2025) emphasise rationing: where desirable jobs are in fixed supply, each new qualified entrant imposes a negative externality on other applicants, producing queues. Bandiera et al. (2022) also show that individuals born in large cohorts are less likely to find a salaried job when they join the labour force.

This paper addresses a gap in the literature: studies of Ghana's fee-removal reforms have concentrated overwhelmingly on demographic outcomes – marriage, fertility timing and teenage motherhood. Where labour market outcomes are examined, they have been examined as supplementary outcomes or at the upper secondary margin, through individually assigned scholarships (partial equilibrium effects hold the local economy fixed), or at the margin of schooling duration rather than of access – and always under employment definitions that count subsistence production as work. Whether national-scale fee-free reforms at lower secondary level shift the productive absorption of the cohorts they educate — a necessary condition for a demographic dividend — needs more rigorous investigation.

3 Data

We use the publicly available ten percent sample of Ghana's 2021 Population and Housing Census (PHC). Three features of this dataset make it well suited to this study. First, the 2021 PHC is the first PHC to record district of birth (n=261). Since our treatment varies at the district level (Section 4.1) and is assigned on the basis of pre-reform schooling conditions, using district of residence would confound exposure with subsequent residential choice: those who acquire more schooling are more likely to migrate, and to migrate toward districts with stronger labour markets. Assigning treatment by district of birth avoids this issue.

Second, 2021 is the first census year at which the cohorts exposed to the reform have reached prime working age. Individuals who entered junior high school after fees were abolished in 1996 were aged 33-37 at enumeration, old enough that their labour market position is less likely to reflect transitory entry dynamics.

Third, the 2021 PHC carried an expanded economic activity module, including questions on hours of work and the presence of a work contract, and is the first large-scale Ghanaian dataset permitting classification consistent with the 19th International Conference of Labour

Statisticians standards (International Labour Office, 2013). This matters because it allows work for pay or profit to be separated from own-use production, and it allows inactivity to be decomposed by its stated reason, distinguishing discouragement from care and family responsibilities. We define these explicitly below.

3.1 Key variables

We measure **educational attainment** in four ways: total years of education, and indicators for completion of at least primary school and of at least JHS. Those who have incomplete JHS are captured as having completed at least primary. We also draw on a measure of self-reported ability to read or write in any language. We include this because attainment and skill need not move together. A reform that raises enrolment without a commensurate increase in teachers, classrooms and materials can raise the number of years completed while lowering the quality of education acquired in each of them. Literacy therefore serves as a check on whether the additional schooling induced by the reform came at the expense of quality. Although self reported, it is the only direct measure of skill the PHC contains.

Our labour market outcomes follow the ICLS-19 framework. First, we define being in **any work** as engaged in any economic activity for at least one hour in the seven days preceding census night. This includes employment for pay or profit as well as own use production.

Employment specifically comprises work for pay or profit, including temporary absence from a job to which the respondent will return. **Production for own final use** falls outside employment, as do unpaid trainee work and volunteer work. **Labour force participation** is reported on a narrow definition, comprising the employed and the **unemployed** (availability to work and active search). Extending beyond the unemployment rate, the ICLS-19 framework defines a spectrum of underutilisation measures:

- **LU1**: the unemployment rate on the strict definition, requiring both availability to work and active search.
- **LU2**: unemployment together with time-related underemployment, *expressed as a share of the labour force*. Time-related underemployment comprises those employed below an hours threshold who are willing and available to work additional hours.
- **LU3**: unemployment together with the potential labour force, *expressed as a share of the extended labour force*, which is the sum of the labour force and the potential labour force. The potential labour force comprises those available for work but not searching (discouraged workers) and those searching but *not currently available*.
- **LU4**: a composite of the above, expressed over extended labour force.

Two components of the underutilisation spectrum cannot be constructed exactly from the PHC. First, the census records hours worked but does not ask about willingness or availability for more hours. This means our LU2 is a proxy for a low-hours share[6] rather than actual

[6]We classify those who report hours worked <35 in the last 7 days as underemployed.

time-related underemployment. Some of those recorded as underemployed will be voluntarily part-time, and our LU2 therefore overstates the ILO concept.

Conversely, our LU3 measure is a lower bound on the ILO concept. The PHC does not contain a category for those searching but not currently available, so that component of the potential labour force is structurally absent. Because LU4 aggregates both LU2 and LU3, its bias is ambiguous, and we place less emphasis on it in our analysis that follows.

Lastly, a limitation arises from a questionnaire skip pattern. The PHC establishes engagement in work through a single question asking whether the respondent undertook any economic activity for at least one hour in the seven days preceding the census night. Those reporting work for pay or for profit are routed to questions on the characteristics of that work. Those reporting economic activity without pay or profit, are routed to a separate question establishing whether that production was market-oriented or for own final use. Only respondents reporting *no* economic activity reach the availability and job-search question. This means that respondents who are engaged in production for own final use (excluded from employment under ICLS-19) are routed past the availability and job search questions entirely, and so cannot be allocated between unemployment and the potential labour force. This means the denominators of LU2 through LU4 exclude a group whose labour force attachment is unobserved, and this should be kept in mind when interpreting results.

We report own-use production as a standalone category rather than a component of our inactivity/not economically active (NEA) measure. Our **NEA** measure therefore only includes those who responded no to engagement in any economic activity and who are not available nor seeking work. We instead create a category for those not in employment nor in education or training (NEET) as a measure of non-employment[7]. We focus on the following two subcategories in our results: NEET but engaged in own production work, and NEET because of care/family responsibilities. Figure 3 illustrates how these various concepts fit together.

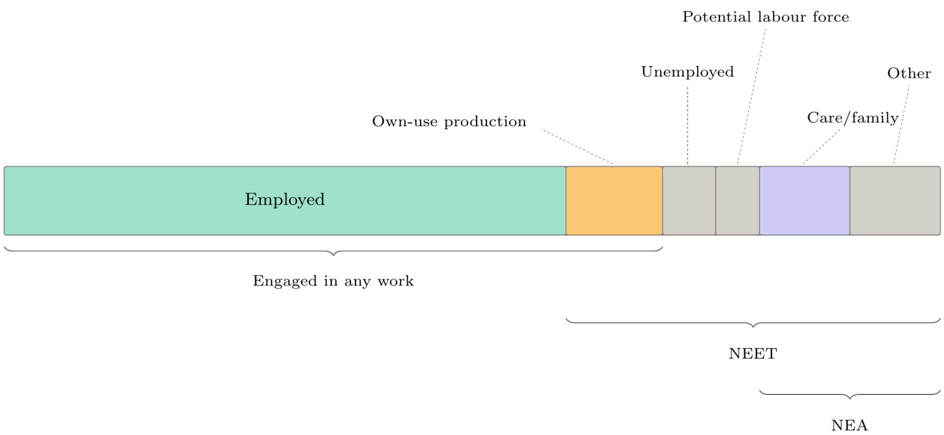


Figure 3: Relationship between measures

Alongside these measures we construct indicators for the type of work performed: employee status, paid work, job informality, occupational skill,[8] and vulnerable employment. We

[7] At the ages we study (33-44) this is, in practice, a measure of non-employment, since participation in education or training is rare beyond the early thirties, but we retain this term given its widespread use.

[8] We do not report effects on agricultural occupation or industry. Movement out of agriculture shows a strong

summarise the definitions of these and other key measures in Table A1.

4 Empirical Strategy

4.1 Research design

The central difficulty in assessing whether a policy reform caused a change in outcomes is that we never observe what would have happened without it. Educational attainment and labour market outcomes were already moving in Ghana before 1996, driven by income growth, urbanisation and earlier reforms, so a simple before-and-after comparison of exposed and unexposed cohorts would attribute all of that movement to the removal of fees. The standard solution is to find a group the policy did not reach and use its trajectory as the counterfactual: this is the canonical difference-in-differences (DiD) design, which compares treated and untreated groups before and after the policy takes effect.

Ghana's 1996 reform offers no such comparison. Fees at the JHS level were abolished nationwide, meaning that the typical treated and control comparison collapses into the before and after one. Intuitively, however, the removal of fees will have had more scope to induce JHS enrolment at the margin where the transition rates between primary and JHS were lower. Additionally, whether a child was exposed to the reform was determined by their age when it was implemented: those young enough to have their whole junior secondary cycle under the new regime would have been exposed, those who had already completed it were not.

These two dimensions play the roles that treatment status and time play in the canonical design. The complement of the pre-reform transition rate stands in for treatment status, except that it is continuous rather than binary, so districts are ordered by how much scope the reform had rather than partitioned into treated and untreated. Birth cohort stands in for time: because we observe a single cross-section, the "before" and "after" periods are cohorts educated under the fee-paying and fee-free regimes respectively, rather than the same individuals observed twice.

Our estimate of interest comes from the interaction of these dimensions: that is, the reform should have raised educational attainment more, in the cohorts exposed, in the districts where JHS fee constraints bound most tightly. Comparing cohorts within a district differences out anything about that district that is fixed across the cohorts we observe, including its level of development, its school infrastructure and its labour market. Comparing districts within a cohort differences out anything common to that cohort nationally, including the secular rise in attainment and the fact that the exposed cohorts are enumerated at younger ages than the comparison cohorts. What remains is the differential movement across cohorts between districts of differing exposure. The identifying assumption is correspondingly that, absent the reform, attainment and labour market outcomes would have evolved in parallel across cohorts in high- and low-transition districts – an assumption we examine in Section 4.4.1.

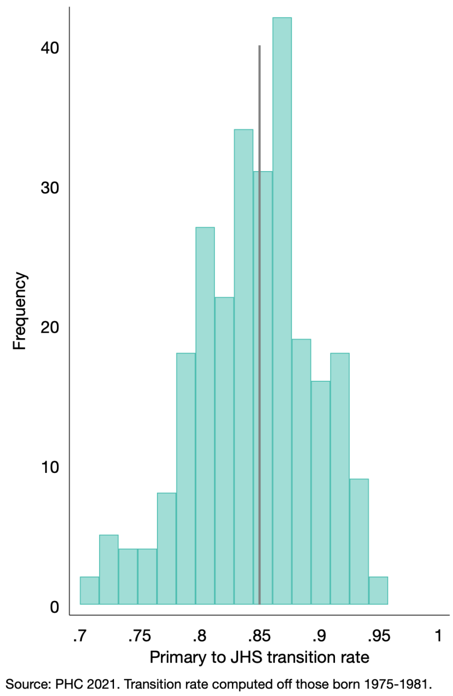
One feature of this design does not carry over from the canonical case. Since exposure is continuous, there is no single untreated group against which effects can be measured, and the familiar interaction coefficient does not correspond to a well-defined average treatment effect.

trend across the cohorts we observe, largely due to broader decline in this industry economy-wide. We therefore cannot separate the effect of the reform from secular structural change on this margin.

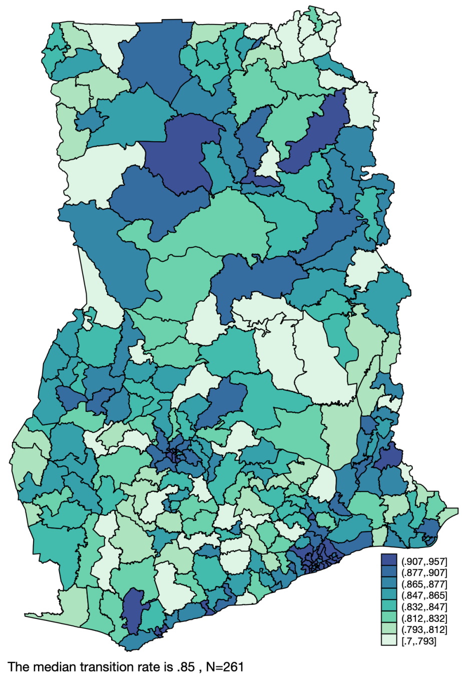
Section 4.3 sets out which parameters this variation does identify and how we estimate them; first we describe the construction of each dimension of exposure.

4.2 Treatment intensity and cohort windows

Treatment intensity, or dose (d), is computed as $1 - transitionrate$, where the transition rate is defined as the share of primary school completers who complete JHS, among those born in the pre-policy period. We take the average transition rate among those born between 1976 and 1981, motivated by the fact that these birth cohorts should strictly be unaffected by the FCUBE policy and should have been educated after the introduction of the NERP. Dose is fixed at the district-of-birth level. Treatment begins at a single date nationwide, so we abstract from staggered adoption. The distribution of transition rates at the district level is illustrated in Figure 4. The median transition rate among the 261 districts is 0.85.[9]



(a) Histogram of transition rates



(b) Map of Ghana (district boundaries): spatial distribution of transition rates

Figure 4: Transition rates (primary to JHS) for defining treatment intensity

Our choice of cohort window is illustrated in Table 2. We define those born between 1984 and 1988 as eligible for the policy based on age of enrolment (six in Ghana). We consider those not exposed to be born between 1977 and 1981. Panel (b) and (c) illustrate these cohorts that make up our sample. The choice of cohort window is motivated by wanting to avoid contamination of a different education regime (pre-NERP, panel (a)) and a potentially different pool of students who would enrol in grade 1 in 1996 under FCUBE (panel (d)). Note that most of the birth cohorts in panel (c) in the table will also have been exposed to some free primary by default. Importantly, because JHS spans roughly ages 12-14[10], cohorts born in 1982 and 1983

[9]Figure A1 in the appendix shows the distribution of transition rates across the sample of individuals.
[10]See Figure A2 in the appendix for the age distribution of pre-FCUBE enrolment.

were still enrolled when fees were abolished and had one and two fee-free years respectively. These cohorts, aged 38 and 39 at enumeration, are therefore partially treated, and we exclude them from the comparison group so that the pre-reform sample comprises only cohorts whose JHS was completed in full under the fee paying regime.

Table 2: Birth cohorts and exposure to Ghana's education reforms

Birth year		Reaches Age 12	Age at	Age in
Aug–Dec	Jan–Jul	by 1 Sept	FJHS (1996)	PHC 2021
(a) *Not fully exposed to NERP*				
1973	1974	1985	23	48
1974	1975	1986	22	47
1975	1976	1987	21	46
(b) *Fully exposed to NERP*				
1976	1977	1988	20	45
1977	1978	1989	19	**44**
1978	1979	1990	18	**43**
1979	1980	1991	17	**42**
1980	1981	1992	16	**41**
1981	1982	1993	15	**40**
1982	1983	1994	14	39
(c) *Exposed to FJHS*				
1983	1984	1995	13	38
1984	1985	1996	12	**37**
1985	1986	1997	11	**36**
1986	1987	1998	10	**35**
1987	1988	1999	9	**34**
1988	1989	2000	8	**33**
1989	1990	2001	7	32
(d) *Exposed to FCUBE full free primary*				
1990	1991	2002	6	31
1991	1992	2003	5	30

Cohorts are defined by the date at which an individual would reach JHS, assuming entry at age 6, on-time progression and a 1 September school-entry cutoff. Note that neither month of birth nor year of birth is not in the public release PHC. We compute year of birth from age, but the absence of month means we cannot perfectly assign exposure per columns 1 and 2, which give the corresponding birth years either side of the September cutoff. This is for illustrative purposes only. NERP denotes the New Educational Reform Programme; FJHS denotes free junior high school, introduced in 1996; FCUBE denotes free compulsory universal basic education.

4.3 Parameters of interest

We follow Callaway, Goodman-Bacon and Sant'Anna (2026) (abbreviated as CGS) on implementing DiD with continuous treatment. Consider potential outcome $Y_{i,c}(d)$, the outcome individual i would realise in cohort c under dose d. For simplicity, assume $c = Post$, that is, c takes a binary value 0 (cohort born pre-FJHS eligibility) or 1 (cohort born post-FJHS eligibility). A *level* treatment effect, $Y_{i,c=1}(d) - Y_{i,c=1}(0)$, is the effect of moving from no exposure to dose d. A *causal response*, $\partial Y_{i,c=1}(d)/\partial d$, is the effect of a marginal increase in the dose. With a binary treatment the two coincide, but with a continuous treatment they do not, and they answer different policy questions (Callaway et al., 2026). For example, reading a large level effect as a marginal one could lead to an incorrect conclusion that raising every district's dose would have large effects.

The average causal response parameter, $ACRT$ answers: *on average across exposed individuals, how much does $Y_{i,j,c}$ change with a marginal increase in dose?* It is the relevant quantity for questions about expanding or contracting exposure at the margin. The average level response, ATT, on the other hand, answers: *how much larger was the effect of the reform, averaged across exposed individuals, than it was in the least-treated districts?* The typical approach in this literature is a TWFE linear regression of the outcome on unit and time effects and the interaction of the dose with a post-reform indicator (e.g Brudevold-Newman, 2021; Kazibwe & Li, 2025; Stenzel et al., 2024). CGS show, however, that TWFE estimates allow inconsistent causal interpretations: expressed in terms of level effects its weights integrate to zero, so it cannot be read as an average treatment effect. Expressed in terms of causal responses, its weights are positive and integrate to one, but do not match the dose distribution among the treated, and depend on the entire dose distribution including the size of the untreated group. In CGS's application, dropping untreated units, which does not change the underlying causal responses, shrinks β^{twfe} by 78%. We report the TWFE coefficient for comparability with prior work, but do not treat it as our headline estimate.

Following CGS, we aggregate the ATT and ACRT parameters using the dose distribution among the treated, rather than letting an estimator choose the weights implicitly (e.g. TWFE).

4.4 Identification

CGS show that conventional parallel trends is not sufficient to compare across doses. Differencing adjacent dose groups yields a causal response plus a selection-bias term arising from the possibility that different dose groups experience different effects of the *same* dose. They show that a strong parallel trends assumption (Assumption SPT) — that the average path of outcomes the entire treated population would have followed at dose d equals the path that dose group d actually followed — eliminates that term. Under SPT the global parameters $ATT(d)$ and $ACRT(d)$ are identified. A limitation is that SPT cannot be tested with pre-treatment data: as CGS observe, before treatment all observed outcomes are untreated potential outcomes, so pre-period tests cannot distinguish the two assumptions.

Identification also requires that outcomes for the comparison cohorts were unaffected by the reform before it took effect. FCUBE was constitutionalised in 1992 but no official implementation was date set. Anticipation is therefore possible in principle: a household expecting fees to be removed might retain a child in primary school who would otherwise have left, raising attainment among cohorts we classify as unexposed. This concern is partly addressed by our donut hole described in Section 4.2. If unexposed cohorts in high-intensity districts still responded to the prospect of fee abolition, their attainment would rise, the comparison group would improve, and the estimated differential would shrink, leading to conservative estimates.

As with parallel trends, no-anticipation cannot be tested against pre-treatment data: before the reform all observed outcomes are untreated potential outcomes, so a flat pre-period slope is consistent with all three assumptions and cannot distinguish between them.

4.4.1 Assessing parallel trends

We follow the event-study approach of CGS: under no-anticipation, both PT and SPT imply that the relationship between outcome changes and the dose should be flat before the reform, so a pre-period slope significantly different from zero is evidence against the design. These tests rule out, for example, the concern that there exist differential cohort trends in labour market outcomes independently of the reform; that is, if less-developed districts were undergoing more rapid structural transformation across the cohorts we observe.

Results for the pre-trend tests (using pre-reform cohorts only) and event-study illustrations (for educational attainment and employment) are shown in Tables A2 and A3 and Figure A3 in the appendix, respectively. Because we are interested in gender differences, and abstracting from the assumptions required to identify a triple DiD (see Caron, 2025), we run separate regressions for men and women and therefore also present pre-trend tests separately by gender. The majority of our outcomes show no statistically significant results during the pre-program period. The one exception among the education outcomes is a negative and weakly significant birth year $\times$ dose coefficient (-0.040, significant at the 10% level) on completed JHS for males, which suggests that males in high-dose districts had fallen behind across pre-reform cohorts (younger cohorts less likely to complete JHS). The coefficient is expressed per unit increase in dose, but treatment intensity ranges from roughly 0.04 to 0.3 across districts, so a unit change lies outside the observed support. When scaled by the observed spread of the dose, it corresponds to roughly half a percentage point and is practically very small.

For labour outcomes in the pooled sample, only employee (conditional on employment) shows a significant but practically small birth year $\times$ dose gradient (-0.034) in the pre-reform sample. The table also shows statistically significant but small birth year $\times$ dose gradient for males who are engaged in care/family work (-0.017) and coefficient on paid worker (conditional on employment), significant at the 10% level (-0.014). Among women in the pre-reform period, younger cohorts were less likely to be in own production work (-0.028) and skilled occupations (-0.026), both significant at the 10% level. When scaled by the dose distribution, all of these coefficients are practically small and round to 0. All other labour market outcomes pass the placebo tests.

For demographic outcomes we find no significant pre-reform gradients with the exception of the number of children ever born alive (-0.331). Children ever born is a cumulative count, so within the pre-reform band younger women have had less time to accumulate births, and because the level of fertility is higher in high-dose districts, the birth-year gradient in the count is steeper there. A negative birth year dose coefficient can therefore arise without any differential change in fertility behaviour. However, if fertility was genuinely falling faster across cohorts in high-dose districts, this would constitute a differential trend. The pattern across the other demographic measures favours the first reading. Ever given birth, births in the preceding twelve months, the number of co-resident children, and age at first birth — all show flat pre-reform gradients. We note, however, that the pre-reform band covers ages 40–44, where age-specific fertility is low, so these measures have limited power to detect a divergence that occurred at younger ages.

4.4.2 Potential threats to identification

Here, we briefly discuss potential threats to identification. The first relates to childhood migration. We assign dose by district of birth rather than district of residence, avoiding the endogenous migration that would contaminate a district-of-residence-based measure. However, individuals born in one district but schooled in another are still assigned the wrong exposure. To the extent that this misclassification is unrelated to potential outcomes, it is classical and attenuates our estimates toward zero, so the reported coefficients are conservative. We cannot, however, rule out that childhood migration was itself selective, which would leave a bias of indeterminate sign.

The second threat relates to differential pandemic exposure. The 2021 PHC was enumerated following the COVID-19 pandemic, and where labour market effects of the pandemic had both age and location gradients, this could bias our DiD estimates. However, pandemic exposure effects will have operated through district of residence, whereas dose is assigned by district of birth, going some way to mitigate this concern.

The next relates to fertility timing. Because exposure is defined by birth cohort and we observe a single cross-section, treated cohorts are necessarily observed at younger ages than comparison cohorts. This concern is most relevant for women: if exposure delays fertility, treated women are observed closer to their peak childcare years than the comparison cohorts, and estimated effects on care-related inactivity may reflect a shift in the timing of withdrawal from the labour force rather than a measured change in lifetime labour supply.

The final two relate to treatment intensity. First, treatment intensity is not randomly assigned. Districts with low pre-reform transition rates were poorer, more rural, and more weakly served by schools, so dose – in part – proxies baseline development. This means fee removal may have bound less tightly where supply constraints rather than direct costs limited enrolment, attenuating the estimated response. We therefore interpret our estimates as the effect of the reform as implemented in less-developed districts, not as the effect of fee abolition in isolation. This is a statement about what we estimate rather than a threat to whether we estimate it consistently.

The second concern arises from the definition of the dose variable. Dose is constructed as one minus the pre-reform transition rate and is therefore, by definition, the distance of a district from the ceiling of universal transition. Any secular convergence in educational attainment – in which districts further from the ceiling improve faster for reasons unrelated to the reform – mechanically generates a positive coefficient on post × dose. The pre-trend tests speak to this directly. Convergence implies that later-born cohorts in high-dose districts gain relative to those in low-dose districts; this would appear as a positive birth year × dose coefficient in the pre-reform sample. We do not find evidence of this (Table A2) .

5The negative and weakly significant coefficient on completed JHS for males (Table A2) is consistent instead with males in high-dose districts having fallen *behind* across these cohorts. We turn to this next.

4.5 Estimation

Following Callaway et al. (2026), we estimate an ordinary least squares (OLS) regression of the form:

$$Y_{i,j,c} = \alpha_j + \theta_c + Post_c \cdot \left(\beta_1 D_j + \beta_2 D_j^2\right) + \varepsilon_{i,j,c}, \tag{1}$$

with district of birth fixed effects, α_j, and cohort fixed effects, θ_c. Y_{ijc} denotes the various education and labour market outcomes (described in Section 3.1) for individual i born in district j in cohort c. Exposure to the reform is indexed by a continuous dose D_j, the intensity of treatment in district j.

In our data, the median of D_j for the sample of respondents is 0.136, with a minimum of 0.042.[11] This means none of our districts have a dose equal to 0[12], only a subset of districts have an intensity *close* to zero. When there are no untreated units the level of the treatment effect is not identified. What remains identified is the level effect relative to the least-treated group, d_L. Since the level effect is continuous in the dose and equal to zero at $d = 0$, $ATT(d_L \mid d_L)$ is unlikely to be zero. Where it is positive, the ATT understates the level effect of the reform[13].

Every estimate we report is therefore a contrast *across doses* – what the reform did at dose d over and above what it did in the least-treated districts. Unless stated otherwise, our results presented as "moving a district from the tenth to the ninetieth percentile of treatment intensity raises Y by ...", should be read as a dose contrast and never as the effect of the reform relative to no reform at all, and bearing in mind the reported magnitudes are likely conservative.

Writing $\hat{f}(d) = \hat{\beta}_1 d + \hat{\beta}_2 d^2$, the ACRT is the plug-in average of $\hat{f}\prime(d)$, i.e. $\frac{1}{n}\sum_i \hat{f}\prime(D_i) = \hat{\beta}_1 + 2\hat{\beta}_2\,\mathbb{E}_n[D]$, over the empirical dose distribution among the treated. Because $\hat{f}$ is quadratic, this coincides with the derivative evaluated at the mean dose. The ATT is the fitted function anchored at the least-treated dose, $\hat{f}(d) - \hat{f}(d_L)$, averaged over the same distribution $\frac{1}{n}\sum_i \left[\hat{f}(D_i) - \hat{f}(d_L)\right] = \hat{\beta}_1\big(\mathbb{E}_n[D] - d_L\big) + \hat{\beta}_2\big(\mathbb{E}_n[D^2] - d_L^2\big)$, and therefore depends on the second moment of the dose, $\mathbb{E}_n[D^2]$, as well as the first. Both moments are taken over exposed individuals in the post-reform cohorts. Under SPT the ATT provides a clean causal contrast between two doses free of selection bias. The causal response function (ACRT) is unaffected by the absence of a zero-dose group.

Note that because we have districts close to 0, as a sense check we also construct an untreated group setting those with dose below 0.05 to untreated status. This means the resulting estimator recovers a biased ATT, where the bias is the average level effect among the districts we have reclassified. Because the level effect is continuous in the dose and equals zero at $d = 0$, this bias vanishes as $\tau \to 0$. We present the ACRT from this approach alongside our main results for comparability.

We present cluster bootstrapped standard errors, clustered at district of birth (261 clusters), obtained by resampling districts of birth with replacement and re-estimating (1) and the aggregation step within each replication. Note, we fix the specification in advance rather than selecting it after inspecting a nonparametric fit; as CGS note, doing otherwise creates a pre-testing problem that invalidates the reported inference. As a robustness check, we also report a cubic specification in Tables A4-A6 in the appendix.

The TWFE that we estimate for comparison, takes the form:

$$Y_{i,j,c} = \alpha_j + \theta_c + \beta_1(Post_c \cdot D_j) + \gamma ethnicity_{ijc} + \varepsilon_{i,j,c}, \tag{2}$$

[11] Dose has a maximum of 0.3, with values of 0.08 at the 10th percentile and 0.205 at the 90th.

[12] When there are completely untreated units in an application, the baseline case of CGS applies.

[13] The same holds symmetrically for outcomes the reform reduces: where $ATT(d_L \mid d_L) < 0$ the anchored contrast is less negative than the level effect.

where α_j, θ_c, Y, D and ε are as defined above; the only addition is the ethnicity controls. Birth-year fixed effects mean the main effect of $Post$ is not identified. The coefficient of interest is β_1, estimated using OLS.

5 Descriptive statistics

Table 3: Descriptive statistics by dose group, split by pre/post reform age band

	Pre cohorts				**Post cohorts**			
	Mean: low	Mean: high	Diff	N	Mean: low	Mean: high	Diff	N
Education								
Completed primary+	0.70	0.52	–0.38	157907	0.77	0.61	–0.36	206987
Years of education	8.10	5.61	–0.45	157770	9.28	6.84	–0.44	206798
Completed JHS+	0.63	0.43	–0.41	157907	0.70	0.51	–0.41	206987
Read/write in any language	0.72	0.54	–0.38	157907	0.79	0.63	–0.37	206987
Labor force participation								
Engaged in any work	0.78	0.81	0.07	157907	0.74	0.76	0.06	206987
Employed (19th ICLS)	0.73	0.65	–0.19	157907	0.71	0.64	–0.14	206987
Unemployed/LU1 (narrow)	0.03	0.02	–0.05	157907	0.05	0.04	–0.05	206987
LFP (narrow)	0.76	0.67	–0.21	157907	0.75	0.68	–0.17	206987
Potential labour force (available, not seeking)	0.02	0.03	0.01	157907	0.03	0.03	0.01	206987
NEA	0.15	0.13	–0.05	157907	0.17	0.16	–0.04	206987
LU2 (LU1 incl. underempl)	0.24	0.33	0.20	113240	0.26	0.33	0.17	147968
LU3 (LU1 incl. disc)	0.07	0.07	0.01	117165	0.10	0.09	–0.01	154094
LU4 (LU1+underempl+disc)	0.26	0.35	0.20	117165	0.28	0.36	0.17	154094
NEET-care/family	0.07	0.06	–0.04	157907	0.09	0.08	–0.03	206987
NEET-own production	0.06	0.17	0.34	157907	0.05	0.13	0.31	206987
Employment type								
Employee	0.26	0.14	–0.30	157907	0.33	0.20	–0.29	206987
Employee (emp only)	0.33	0.21	–0.29	109092	0.42	0.29	–0.28	139496
Paid worker	0.73	0.63	–0.20	157907	0.70	0.62	–0.16	206987
Paid worker (emp only)	0.99	0.97	–0.10	109092	0.99	0.97	–0.10	139496
Vulnerable empl	0.48	0.50	0.06	157907	0.39	0.44	0.10	206987
Informal job	0.54	0.54	0.00	157907	0.47	0.49	0.04	206987
Informal sector	0.53	0.54	0.01	157907	0.46	0.49	0.05	206987
Skilled occupation	0.12	0.06	–0.21	157907	0.15	0.09	–0.18	206987
Public sector employment	0.12	0.08	–0.12	109092	0.16	0.13	–0.08	139496
Public sector employment	0.09	0.05	–0.14	157907	0.11	0.08	–0.10	206987
Demographic outcomes								
Age at 1st birth	23.87	22.46	–0.25	70673	23.65	22.21	–0.29	88747
Number children ever born alive	3.42	4.23	0.34	78881	2.55	3.21	0.32	105881
Live births past 12mo	0.04	0.05	0.03	78881	0.11	0.13	0.04	105881
No. children co-resident	2.46	2.71	0.13	78881	2.11	2.48	0.21	105881
Married	0.79	0.81	0.04	157907	0.73	0.77	0.11	206987
Ever given birth	0.88	0.91	0.09	78881	0.81	0.86	0.13	105881
Other / sample composition								
Age	41.73	41.70	–0.02	157907	34.96	34.97	0.01	206987
Akan	0.49	0.44	–0.09	157259	0.50	0.45	–0.11	206087
Coastal region (of birth)	0.54	0.32	–0.44	157907	0.54	0.32	–0.45	206987
Central region (of birth)	0.30	0.44	0.30	157907	0.30	0.44	0.30	206987
Northern region (of birth)	0.17	0.24	0.18	157907	0.16	0.23	0.18	206987
Female	0.49	0.51	0.02	157907	0.51	0.51	–0.01	206987

Sample: 2021 census, post = 1984-1988 birth cohorts, pre = 1977-1981). High/low dose = split at the median dose. Dif = normalised mean difference. Note: Years of education sample sizes are slightly smaller because of missing information. Some variables under employment type are conditional on being employed. Fertility variables only defined for women, and age at first birth only for those who have ever given birth. Ethnicity (Akan shown here) also reflects some missing values.

Table 3 reports means by high vs low dose group within each pre vs post. Districts are split at the median of the treatment intensity distribution in the sample and the reported difference is normalised by the pooled standard deviation. The table highlights that treatment intensity is not randomly assigned. High-dose districts differ substantially from low-dose districts on every educational margin and on region of birth: low dose regions are disproportionately concentrated in the coastal regions and have approximately 2.5 more years of education in the pre-period than high intensity regions. It follows that our estimates describe the reform as experienced in districts that were poorer, more northern and had less education at baseline. Importantly, however, these pre-reform differences are similar across the two age bands: the gender ratio, ethnic composition (Akan), and region-of-birth shares are all balanced across the pre- and post-reform samples.

Notably, high-dose districts display a lower share in employment on the ICLS-19 definition (0.65 against 0.73), the difference being accounted for by own-use production, which is almost three times as prevalent (0.17 against 0.06). Two further features of economic activity are noteworthy before regression estimates are interpreted. First, employment and engagement in any work are lower in the post-reform cohorts compared to the pre-reform cohorts. Other indicators (e.g. unemployment and NEA) move in the opposite direction. This likely reflects the difference in age at which the two groups are observed – the post-reform cohorts are enumerated at 33-37 and the comparison cohorts at 40-44.This is common to districts of all treatment intensities and is therefore absorbed by the cohort effects in our specification.

Second, gaps on these outcomes between high- and low-dose districts narrow modestly in the post-reform band. Since high-dose districts start from lower levels of employment, this narrowing is consistent with a positive DiD treatment effect. These two features have bearing for how every labour market estimate in Section 6 should be read. For example, we do not claim that post-reform cohorts are employed at higher rates than the cohorts before them, who are observed seven years older. The claim is that exposed cohorts in high-dose districts are employed at higher rates, at the ages at which we observe them, than they would have been had the reform not reached their districts. This requires that the age profile of employment does not itself vary systematically with dose, which is the parallel trends assumption we examined in Section 4.4.1.

The demographic margins move in the post-reform band in a way the labour market results discussed below echo: relative to low-dose districts, high-dose districts show a widening gap in marriage (0.01 pre, 0.04 post) and in co-resident children (0.24 pre, 0.37 post), alongside a narrowing gap in completed fertility (0.79 pre, 0.66 post). These are raw comparisons, but they motivate our attention on demographic mechanisms when discussing labour market results.

Lastly, Table 4 provides summary statistics on activity status disaggregated by gender, cohort and dose tercile, providing useful context for the gender disaggregated results in Section 6.2.1. Employment rates are approximately 10 percentage points higher among men than women, with the difference largely being concentrated in care work. There is some descriptive evidence that the reform may have increased participation in education even in the long run - in high tercile districts the share enrolled in education increases to a greater extent than in the lowest tercile.

Table 4: Activity-status composition, by dose tercile, cohort, and gender

	Male						Female					
	T1		T2		T3		T1		T2		T3	
	pre	post	pre	post	pre	post	pre	post	pre	post	pre	post
N	26301	34519	26397	33223	26328	33364	25339	35937	26803	35437	26739	34507
Employed	81.2	78.6	73.6	74.0	69.8	69.7	69.3	65.6	62.7	60.7	58.2	56.4
Own-use production	4.6	3.6	13.6	10.2	17.9	14.8	3.8	2.5	12.9	9.2	17.9	13.7
Unemployed	3.1	4.7	2.5	3.8	2.1	3.4	3.2	4.7	2.6	4.2	2.4	3.6
Potential labour force	1.9	2.6	2.0	2.3	2.2	2.4	2.7	3.3	3.0	3.5	3.1	3.5
NEA - care/family	1.3	1.3	1.0	1.1	1.1	1.1	13.4	16.4	11.7	14.9	11.2	15.3
NEA - other	7.9	8.9	7.1	8.1	6.8	8.1	7.5	7.2	7.0	7.1	7.1	7.0
In education	0.1	0.3	0.1	0.4	0.2	0.5	0.1	0.3	0.2	0.5	0.1	0.5

Employed/Unemployed/PLF/NEA are 19th-ICLS categories; Own-use production is a separate category; NEA-care/NEA-other split the 'not economically active' group, with NEA-care isolating care/family reasons. T1-T3 = dose terciles with T3 being the highest.

6 Results

6.1 Education outcomes

Table 5 presents results on the impact of the reform on educational attainment, for the full sample and separately by gender. Column (1) reports the TWFE estimate; column (2) the ACRT from the CGS-style estimator with an untreated group (districts with dose < 0.05); column (3) the ACRT from the CGS-style estimator with no untreated units; and columns (4) and (5) the scaled ACRT[14] and the ATT, both from the no untreated units specification. Figures 5-7 plot the anchored contrast across the treatment intensity distribution for completed JHS, years of education, and literacy outcomes respectively. The first panel in each figure shows estimates for the full sample, with the next two showing estimates for males and females separately.

Several noteworthy points emerge from the table. The first relates to the TWFE coefficients and concerns raised by Callaway et al. (2026). The TWFE estimates are typically smaller in absolute value than the ACRTs, but Figure 5 shows pronounced concavity in dose, which a specification linear in dose cannot accommodate. Moreover, most of the differences between the TWFE coefficients and the ACRTs are not statistically different at conventional levels, so we read them as suggestive of the functional-form and weighting concerns raised by Callaway et al. (2026) rather than as evidence that those concerns bind decisively in this application.

Next, the results in the table are broadly consistent with free JHS raising educational attainment in exposed cohorts. Scaling the ACRT by the P10–P90 range of the observed dose distribution, the reform raised attainment in years by approximately a quarter of a year. The ATT — the fitted dose-response function averaged over the doses exposed individuals actually faced, anchored at the least-treated district — implies an effect just under half a year (0.44) larger than in the least-treated districts. It is a lower bound on the level effect of the reform if it also raised attainment where intensity was lowest.

[14] The raw ACRT is expressed per unit increase in dose, but treatment intensity ranges only from roughly 0.04 to 0.3 across districts, so a unit change lies far outside the observed support. We therefore also report the ACRT scaled by the P10–P90 range of the dose distribution, which gives the effect of moving a district from the 10th to the 90th percentile of the treatment intensity actually observed.

Table 5: Main results: Education

	Mean	N	Spec 1	Spec 2	Spec 3		
			TWFE	ACRT	ACRT	ACRT (P10-P90)	ATT
All							
Years of education	7.533	364568	1.398***	1.776***	1.956***	0.246***	0.436***
			(0.525)	(0.554)	(0.617)	(0.078)	(0.083)
Completed JHS+	0.570	364894	0.187***	0.219***	0.224***	0.028***	0.040***
			(0.041)	(0.042)	(0.045)	(0.006)	(0.006)
Read/write in any language	0.677	364894	0.295***	0.365***	0.364***	0.046***	0.062***
			(0.047)	(0.046)	(0.052)	(0.007)	(0.006)
Completed primary+	0.654	364894	0.254***	0.317***	0.317***	0.040***	0.057***
			(0.045)	(0.046)	(0.051)	(0.006)	(0.006)
Female							
Years of education	6.508	184631	–0.404	–0.133	0.200	0.025	0.266***
			(0.647)	(0.680)	(0.659)	(0.083)	(0.100)
Completed JHS+	0.489	184762	0.060	0.099	0.122**	0.015**	0.036***
			(0.058)	(0.064)	(0.061)	(0.008)	(0.009)
Read/write in any language	0.604	184762	0.279***	0.378***	0.391***	0.049***	0.076***
			(0.067)	(0.067)	(0.068)	(0.009)	(0.009)
Completed primary+	0.580	184762	0.214***	0.294***	0.306***	0.038***	0.063***
			(0.064)	(0.066)	(0.065)	(0.008)	(0.009)
Male							
Years of education	8.586	179937	2.602***	3.086***	3.140***	0.395***	0.544***
			(0.655)	(0.722)	(0.752)	(0.095)	(0.109)
Completed JHS+	0.653	180132	0.270***	0.295***	0.285***	0.036***	0.039***
			(0.051)	(0.057)	(0.057)	(0.007)	(0.009)
Read/write in any language	0.752	180132	0.273***	0.317***	0.306***	0.039***	0.045***
			(0.056)	(0.057)	(0.060)	(0.008)	(0.009)
Completed primary+	0.729	180132	0.252***	0.299***	0.293***	0.037***	0.046***
			(0.057)	(0.059)	(0.061)	(0.008)	(0.009)

Note: Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.126), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. N and mean reported based on estimation sample from Spec 2 and 3. Spec 1 has slightly lower observations due to missing information in ethnicity. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

The largest attainment effects of the reform appear to be at the primary margin. The scaled ACRT in the full sample for primary completion is 0.04, against 0.028 for at least JHS completion. The younger cohorts in our sample should have already been in primary school in 1996. Fee removal at JHS level plausibly raised the return to completing primary, since primary completion became the gateway to fee-free lower secondary. FCUBE also made the entirety of basic education compulsory, although Akyeampong (2009) shows that far from universal enrolment was achieved. Although we try to minimise the influence of free and compulsory primary on our JHS margin of interest by restricting birth cohorts to below 1989, because of delayed enrolment some of those cohorts may have been induced to enrol in primary by FCUBE. Thus while JHS completion rose, it seems we do pick up effects of the broader FCUBE reform, not just free JHS. We therefore cannot rule out that our estimates of labour market outcomes reflect other components of the FCUBE package that were delivered alongside the abolition of JHS fees.

The patterns by gender are nuanced. The ATT for JHS completion is almost identical for men and women (3.9 and 3.6 percentage points), with the effect on primary completion larger for women (6.3 vs 4.6 percentage points). The male advantage in years of education (0.544 vs 0.266) is consistent with men continuing their schooling beyond the compulsory basic education cycle. Importantly, the apparently null female ACRTs reflect the shape of the dose-response function rather than the absence of an effect: as Figure 6 shows, the female profile rises to a peak near $d \approx 0.16$ before declining, so that the average slope across the dose support is close to zero while the level at typical doses is positive. The female curve falls below zero above $d \approx 0.25$, meaning the reform did less there than in the least-treated districts– not that it did nothing.

Notably, men and women have differently shaped dose-response curves for JHS completion. For men, the estimated response is largely monotone, increasing across the observed support. For women it is inverted-U shaped, and is statistically indistinguishable from zero above ≈ 0.25. In the districts furthest from universal transition (i.e. low transition rates, high intensity) – those that were poorest, most rural and most weakly served by schools – the removal of fees appears to have had no detectable effect on female attainment relative to the least treated districts, consistent with supply constraints, other school-related expenses (exam fees, transport, uniforms), and the opportunity cost of girls' time (e.g. child labour)[15] binding more tightly than price.

Literacy responds more strongly than any of the other attainment margins. Moving a district from the 10th-90th percentile of treatment intensity raises the share able to read and write in any language by 4.6 percentage points, against 4 percentage points for primary completion and 2.8 percentage points for JHS completion. The gain is concentrated among women (see Figure 7): moving from the 10th-90th percentile raises female literacy by 4.9 percentage points on a base of 60.5%, against 3.9 percentage points on a base of 75.2% for men. Averaged over the distribution, the anchored contrasts are 7.6 and 4.5 percentage points for women and men, respectively.

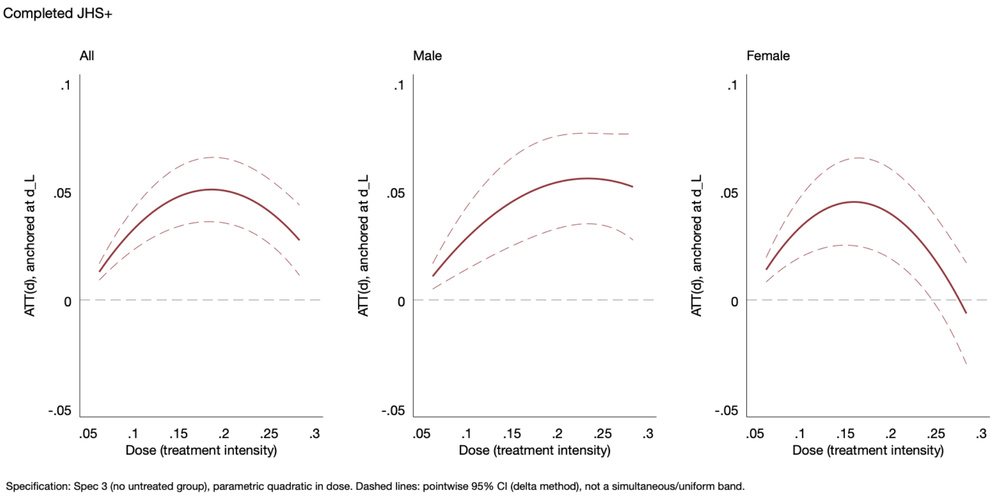


Figure 5: Parametric estimates of $ATT(d \mid d) - ATT(d_L \mid d_L)$; quadratic specification

6.2 Labour market outcomes

We begin with an overview of our results for the pooled sample, before turning to the differences between men and women. Labour market indicators are presented in Table 6 and employment

[15] See findings in this regard Akyeampong's (2009) review of FCUBE.

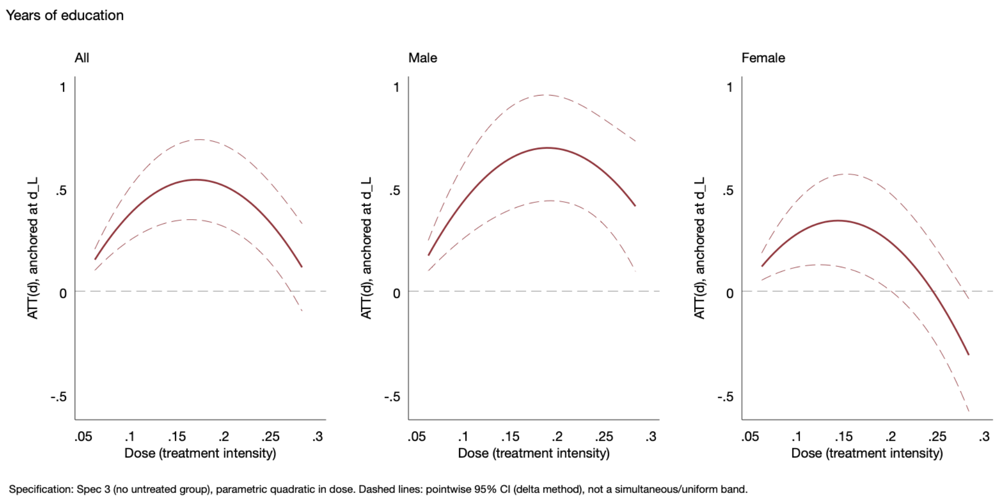


Figure 6: Parametric estimates of $ATT(d \mid d) - ATT(d_L \mid d_L)$; quadratic specification

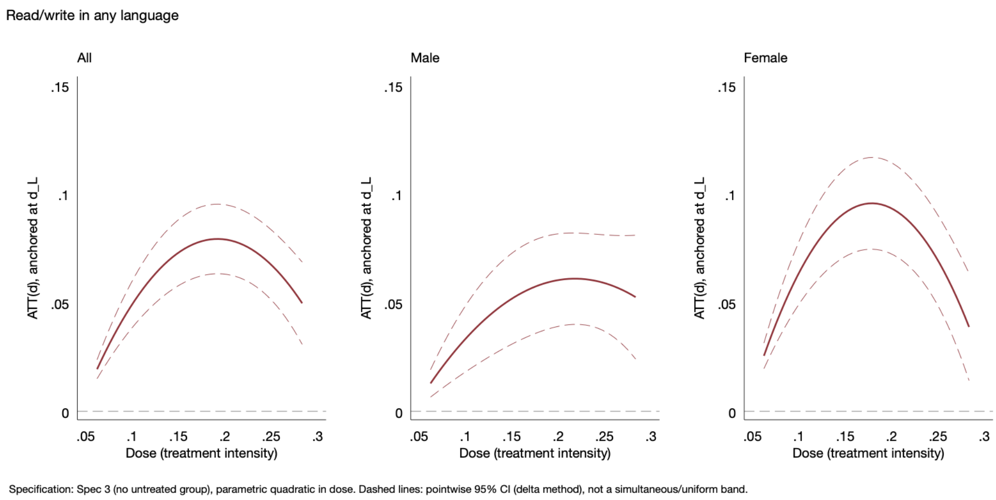


Figure 7: Parametric estimates of $ATT(d \mid d) - ATT(d_L \mid d_L)$; quadratic specification

type indicators in Table 7.

There is a positive employment effect of 3.4 percentage points when moving a district from the 10th to the 90th percentile of the dose distribution. Recall that the post-reform cohorts are enumerated at lower rates of employment, and at younger ages, than the comparison cohorts (Table 3). Our result is therefore not that the exposed cohorts are employed at higher rates than the cohorts before them: it is that they are employed at higher rates, at the ages at which we observe them, than they would have been had the reform not reached their districts. (The same reading applies to every employment, participation and employment-type estimate reported below, and we do not repeat the qualification at each one.)

Narrow labour force participation rises by 3 percentage points, and the 0.4 percentage point decline in unemployment reflects the difference between the employment and participation results. The estimated effects on labour underutilisation are negative throughout: 1.8 percentage points on LU2, 1 percentage point on LU3 and 2.3 percentage points on LU4. These signal an improvement in labour utilisation, with a caveat: as with employment, these are differentials within a common trend rather than reductions in the level of underutilisation. Unemployment and the potential labour force both rise on average between the pre- and post-reform cohorts in districts of both high and low treatment intensity (Table 3). Therefore underutilisation rose by less where the reform had more scope to operate. Part of the decline is also mechanical: LU3 and LU4 are expressed over the extended labour force and LU2 over the labour force, both of which the reform expands.

Table 6: Main results: labour force participation

	Mean	N	**Spec 1** TWFE	**Spec 2** ACRT	**Spec 3** ACRT	**Spec 3** ACRT (P10-P90)	**Spec 3** ATT
Engaged in any work	0.769	364894	0.009	–0.009	–0.003	0.000	–0.003
			(0.032)	(0.038)	(0.031)	(0.004)	(0.005)
Employed (19th ICLS)	0.681	364894	0.247***	0.273***	0.271***	0.034***	0.036***
			(0.044)	(0.049)	(0.044)	(0.006)	(0.007)
NEA	0.152	364894	0.032	0.050*	0.045*	0.006*	0.007
			(0.025)	(0.028)	(0.026)	(0.003)	(0.005)
LFP (narrow)	0.716	364894	0.219***	0.239***	0.240***	0.030***	0.032***
			(0.041)	(0.045)	(0.043)	(0.005)	(0.007)
LU2 (LU1 incl. underempl)	0.288	261208	–0.132***	–0.141***	–0.146***	–0.018***	–0.018***
			(0.037)	(0.046)	(0.039)	(0.005)	(0.006)
LU3 (LU1 incl. disc)	0.084	271259	–0.069***	–0.082***	–0.076***	–0.010***	–0.009**
			(0.026)	(0.031)	(0.025)	(0.003)	(0.004)
LU4 (LU1+underempl+disc)	0.314	271259	–0.166***	–0.174***	–0.182***	–0.023***	–0.023***
			(0.038)	(0.048)	(0.039)	(0.005)	(0.007)
NEET-care/family	0.077	364894	0.019	0.029	0.027	0.003	0.005
			(0.019)	(0.022)	(0.020)	(0.003)	(0.003)
NEET-own production	0.102	364894	–0.237***	–0.274***	–0.269***	–0.034***	–0.037***
			(0.031)	(0.029)	(0.033)	(0.004)	(0.005)
Potential labour force (available, not seeking)	0.028	364894	–0.029**	–0.029**	–0.031**	–0.004**	–0.004*
			(0.012)	(0.013)	(0.013)	(0.002)	(0.002)
Unemployed/LU1 (narrow)	0.035	364894	–0.028**	–0.034**	–0.031**	–0.004**	–0.004*
			(0.012)	(0.015)	(0.013)	(0.002)	(0.002)

Note: Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.126), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. N and mean reported based on estimation sample from Spec 2 and 3. Spec 1 has slightly lower observations due to missing information in ethnicity. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

The central result in Table 6 is a reallocation rather than an expansion of work. The share of the cohort engaged in any productive activity is unchanged – the point estimate is zero to two decimal places and precisely estimated – while employment for pay or profit rises by 3.4 percentage points and participation in own-use production falls by 3.4 percentage points. There is some indication of movement into economic inactivity (0.6 percentage points, significant only at the 10% level), but it is imprecisely measured and we cannot identify the underlying flows.

Table 7 shows where the additional employment was located.[16] Informal jobs rise by 2.9 percentage points, and vulnerable employment by 3.4 percentage points. This suggests that the additional employment is largely informal and vulnerable. It is, however, paid: the share of the cohort in paid work rises by 3.4 points, so the movement across the production boundary is into work for remuneration rather than into unpaid contribution to family enterprise. We

[16]Here again, and throughout, 'additional employment' means employment over and above the least-treated districts, not employment over and above the pre-reform cohorts.

are therefore cautious about the interpretation of 'vulnerable'. Vulnerability is typically used as a marker of precarity, but the transition we observe out of subsistence production and into own-account work may reflect the agency that additional schooling confers. In this context vulnerable employment may still reflect precarity relative to wage employment, but nonetheless be preferable to own account production.

Table 7: Main results: Employment type

			Spec 1	Spec 2	Spec 3		
	Mean	N	TWFE	ACRT	ACRT	ACRT (P10-P90)	ATT
Employee (emp only)	0.321	248588	–0.107*	–0.127**	–0.094	–0.012	–0.002
			(0.056)	(0.064)	(0.060)	(0.008)	(0.009)
Employee	0.219	364894	–0.034	–0.027	–0.006	–0.001	0.010
			(0.042)	(0.052)	(0.048)	(0.006)	(0.008)
Informal job	0.507	364894	0.242***	0.256***	0.228***	0.029***	0.017*
			(0.054)	(0.057)	(0.054)	(0.007)	(0.009)
Skilled occupation	0.106	364894	0.007	0.027	0.052	0.007	0.020***
			(0.035)	(0.039)	(0.041)	(0.005)	(0.007)
Paid worker (emp only)	0.981	248588	0.005	0.007	–0.001	0.000	–0.001
			(0.012)	(0.014)	(0.012)	(0.002)	(0.002)
Paid worker	0.668	364894	0.248***	0.273***	0.269***	0.034***	0.035***
			(0.044)	(0.048)	(0.045)	(0.006)	(0.007)
Public sector employment (emp only)	0.126	248588	0.100**	0.116**	0.149***	0.019***	0.032***
			(0.043)	(0.051)	(0.050)	(0.006)	(0.008)
Public sector employment	0.085	364894	0.063**	0.092**	0.106***	0.013***	0.025***
			(0.030)	(0.038)	(0.036)	(0.005)	(0.006)
Vulnerable empl	0.449	364894	0.277***	0.291***	0.269***	0.034***	0.023**
			(0.060)	(0.068)	(0.060)	(0.008)	(0.010)

Note: Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.126), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. N and mean reported based on estimation sample from Spec 2 and 3. Spec 1 has slightly lower observations due to missing information in ethnicity. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Overall, wage employment does not expand: the coefficient on employee status is negative throughout and indistinguishable from zero in our preferred specification, both unconditionally and among the employed. The additional employment is therefore overwhelmingly own-account. Public sector employment, on the other hand, does rise — by 1.3 percentage points unconditionally and 1.9 percentage points among the employed, when moving a district from the 10th-90th percentile of intensity. On an unconditional base of 8.5% this is a proportional increase of about 15%. Because almost all public sector work is wage work, a rising public sector share alongside a flat overall employee share implies a shift in the composition of wage employment toward the public sector rather than an expansion of wage employment overall. Public sector jobs are typically better remunerated and more secure than the private wage jobs, so this is a substantive gain even though the wage-employment margin as a whole does not move. We show in Section 6.2.1 that it is almost entirely a male phenomenon.

Finally, we note that these labour market responses are large relative to the first stage. An increase in dose that raises JHS completion by 2.8 percentage points raises employment by 3.4 percentage points. We do not interpret this as a return to schooling. Our estimate is the effect of the reform as implemented in a district, which incorporates the supply-side components, cohort-level spillovers operating through siblings and peers whose own schooling may not have changed, and any local labour demand generated by school construction and teacher deployment, in addition to any private return to the schooling induced. Classical measurement error in self-reported educational attainment could additionally be deflating the first stage relative to outcomes recorded on a reference-week basis (as labour market outcomes are).

6.2.1 Gender differences

The gender-disaggregated results in Tables 8 to 11 reveal findings that the pooled-gender estimates hide. For women, participation in any productive activity falls by 0.9 percentage points, while employment rises by 2.9 percentage points and participation in own-use production falls by 3.9 percentage points. Of those leaving own-use production, roughly three quarters enter employment and one quarter move into economic inactivity, with care and family responsibilities accounting for almost the entirety of the latter: a 1.3 percentage point increase in care-related inactivity against a 1.4 percentage point rise in inactivity overall. For men there is no corresponding withdrawal from productive activity (Table 9): participation in any work is statistically indistinguishable from zero, employment rises by 3.5 percentage points and own-use production falls by 2.9 percentage points.

NEET-own production does show a negative pre-trend for women only (Table A3), but the size of the coefficient (-0.028, or -0.003 scaled; weakly significant) is unlikely to explain the observed negative and strongly significant impact on own production work (3.9 percentage points scaled decline). Figure 8 shows the effect for women over the entire dose distribution, a pattern which is unlikely to be overturned by a pre-trend of this size.

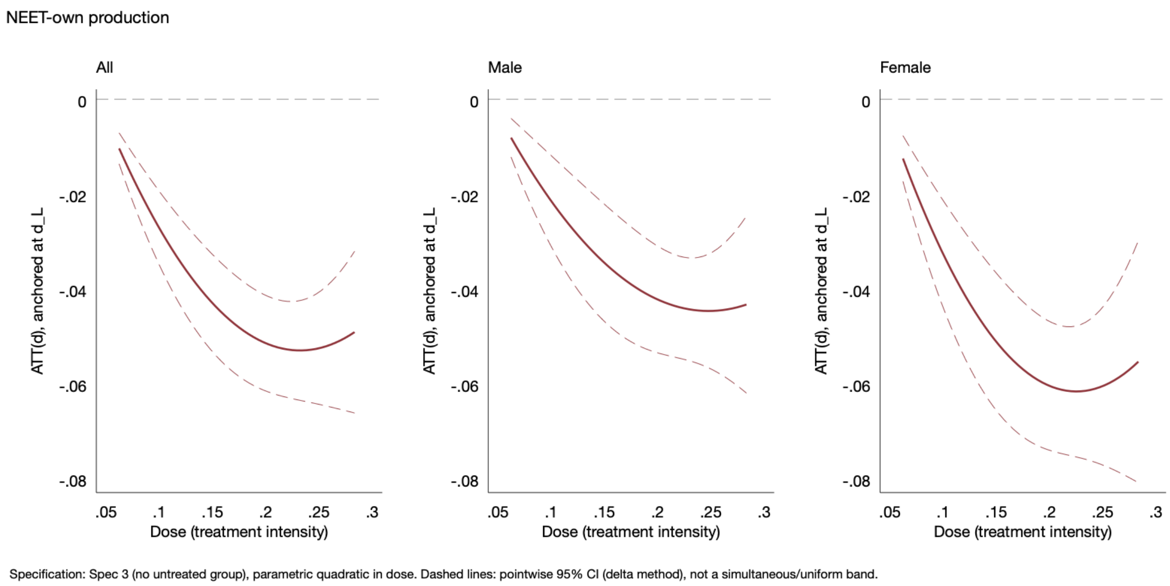


Figure 8: Parametric estimates of $ATT(d \mid d) - ATT(d_L \mid d_L)$; quadratic specification

Whether women's movement into care and family responsibilities represents a choice made possible by improved household circumstances or a constraint on labour market access, we cannot determine. Distinguishing them would require earnings data for the induced self-employed and information on household bargaining, neither of which is available here. Nonetheless, given the large gain in literacy, it is possible that the women induced into

Table 8: Main results: labour force participation female

	Mean	N	Spec 1	Spec 2	Spec 3		
			TWFE	ACRT	ACRT	ACRT (P10-P90)	ATT
Engaged in any work	0.704	184762	–0.041	–0.073	–0.074*	–0.009*	–0.015**
			(0.043)	(0.051)	(0.045)	(0.006)	(0.007)
Employed (19th ICLS)	0.620	184762	0.213***	0.244***	0.228***	0.029***	0.028***
			(0.060)	(0.062)	(0.061)	(0.008)	(0.009)
NEA	0.212	184762	0.082**	0.107**	0.114***	0.014***	0.018***
			(0.039)	(0.046)	(0.040)	(0.005)	(0.007)
LFP (narrow)	0.655	184762	0.185***	0.215***	0.201***	0.025***	0.026***
			(0.059)	(0.064)	(0.062)	(0.008)	(0.009)
LU2 (LU1 incl. underempl)	0.336	121039	–0.152***	–0.197***	–0.174***	–0.022***	–0.021*
			(0.058)	(0.069)	(0.058)	(0.007)	(0.011)
LU3 (LU1 incl. disc)	0.099	126995	–0.055	–0.081**	–0.057	–0.007	–0.006
			(0.035)	(0.039)	(0.035)	(0.004)	(0.006)
LU4 (LU1+underempl+disc)	0.367	126995	–0.176***	–0.221***	–0.200***	–0.025***	–0.024**
			(0.058)	(0.067)	(0.059)	(0.007)	(0.011)
NEET-care/family	0.140	184762	0.079**	0.102***	0.101***	0.013***	0.015**
			(0.034)	(0.039)	(0.033)	(0.004)	(0.006)
NEET-own production	0.098	184762	–0.264***	–0.318***	–0.312***	–0.039***	–0.044***
			(0.046)	(0.040)	(0.043)	(0.005)	(0.006)
Potential labour force (available, not seeking)	0.032	184762	–0.018	–0.019	–0.020	–0.003	–0.002
			(0.017)	(0.019)	(0.018)	(0.002)	(0.003)
Unemployed/LU1 (narrow)	0.035	184762	–0.028	–0.029	–0.027	–0.003	–0.002
			(0.017)	(0.022)	(0.019)	(0.002)	(0.003)

Note: Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.125), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. N and mean reported based on estimation sample from Spec 2 and 3. Spec 1 has slightly lower observations due to missing information in ethnicity. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

employment from own production work were able to transition given the skills that being literate affords. We explore reasons for movement into care work further in Section 6.3.

We note that the improvement in labour utilisation is also composed differently for men and women. Among men the improvement operates on the discouragement margin: LU3 falls by 1.1 percentage points and the potential labour force by 0.5 percentage points, both precisely estimated. Among women both are indistinguishable from zero. The female improvement operates instead on the hours margin: LU2 falls by 2.2 percentage points against 1.3 percentage points for men.

LU4, which aggregates both margins, falls by a similar amount for each — 2.5 percentage points for women and 1.9 for men. Reporting only the composite would suggest that the reform improved utilisation to broadly the same degree for both genders. This is the practical case for the full ICLS-19 spectrum in a setting like this: a composite is uninformative about the margin, and the margin is where the gender difference lies.[17]

Employment type

The gender outcomes diverge even more on the composition of employment. Among women, the employment response is accounted for by informal and own-account work: informal jobs rise by

[17] The direction of the LU2 and LU3 departure from the ILO concept is known and neither depends on gender, so the contrast between men and women is not an artefact of the measures we report.

Table 9: Main results: labour force participation male

			Spec 1	Spec 2	Spec 3		
	Mean	N	TWFE	ACRT	ACRT	ACRT (P10-P90)	ATT
Engaged in any work	0.836	180132	0.025	0.018	0.032	0.004	0.006
			(0.040)	(0.046)	(0.044)	(0.005)	(0.007)
Employed (19th ICLS)	0.744	180132	0.247***	0.264***	0.277***	0.035***	0.039***
			(0.046)	(0.053)	(0.050)	(0.006)	(0.008)
NEA	0.090	180132	0.016	0.029	0.012	0.001	0.000
			(0.030)	(0.028)	(0.033)	(0.004)	(0.006)
LFP (narrow)	0.778	180132	0.218***	0.225***	0.241***	0.030***	0.033***
			(0.044)	(0.045)	(0.046)	(0.006)	(0.008)
LU2 (LU1 incl. underempl)	0.246	140169	–0.099*	–0.078	–0.107*	–0.013*	–0.015
			(0.051)	(0.061)	(0.057)	(0.007)	(0.009)
LU3 (LU1 incl. disc)	0.070	144264	–0.076**	–0.075*	–0.086***	–0.011***	–0.012**
			(0.032)	(0.040)	(0.031)	(0.004)	(0.005)
LU4 (LU1+underempl+disc)	0.267	144264	–0.138**	–0.115*	–0.149**	–0.019**	–0.020**
			(0.054)	(0.066)	(0.060)	(0.008)	(0.010)
NEET-care/family	0.012	180132	–0.006	–0.006	–0.008	–0.001	–0.001
			(0.011)	(0.011)	(0.013)	(0.002)	(0.002)
NEET-own production	0.106	180132	–0.209***	–0.231***	–0.227***	–0.029***	–0.030***
			(0.032)	(0.030)	(0.032)	(0.004)	(0.005)
Potential labour force (available, not seeking)	0.023	180132	–0.037**	–0.036**	–0.039**	–0.005**	–0.005**
			(0.016)	(0.018)	(0.016)	(0.002)	(0.003)
Unemployed/LU1 (narrow)	0.034	180132	–0.029	–0.040*	–0.036**	–0.005**	–0.006*
			(0.018)	(0.021)	(0.018)	(0.002)	(0.003)

Note: Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.126), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. N and mean reported based on estimation sample from Spec 2 and 3. Spec 1 has slightly lower observations due to missing information in ethnicity. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

4.8 percentage points and vulnerable employment by 5.4 percentage points. Wage employment moves in the opposite direction – employee status falls by 2.5 percentage points unconditionally and by 4.2 percentage points among the employed. The share of women in skilled occupations falls by 1.4 percentage points – that is, the gap between high and low intensity districts has widened among the post cohorts. Employment is nonetheless remunerated: paid work rises by 2.7 percentage points unconditionally, essentially the whole of the employment response, while among the employed the paid share does not move at all. Contributing family workers are employed but unpaid, so this constant points to the rise in vulnerable employment being from own-account work rather than unpaid contribution to a family enterprise. We interpret the coefficients on skilled occupation and employee conditional on employment with caution, however, since Table A3 shows a significant birth year × dose gradient in these outcomes for women.

Among men the reverse pattern is observed: the ACRTs on informality are indistinguishable from zero in our preferred specification, while the share in skilled occupations rises by 2.5 percentage points moving from the 10th-90th percentile and by 4.1 percentage points averaged over the dose – a gain of roughly 30% on a base of 13.5%.

Among men, the wage-employment margin does increase, if modestly, and the public sector accounts for all of it. The male employee ACRT is 1.5 percentage points (significant at the 10% level) and the ATT 2.7 percentage points, while public sector employment rises by 2.4

Table 10: Main results: Employment type female

	Mean	N	**Spec 1**	**Spec 2**	**Spec 3**		
			TWFE	ACRT	ACRT	ACRT (P10-P90)	ATT
Employee (emp only)	0.223	114482	–0.338***	–0.380***	–0.338***	–0.042***	–0.030***
			(0.059)	(0.065)	(0.062)	(0.008)	(0.011)
Employee	0.138	184762	–0.207***	–0.238***	–0.199***	–0.025***	–0.015*
			(0.041)	(0.050)	(0.044)	(0.006)	(0.008)
Informal job	0.497	184762	0.386***	0.425***	0.383***	0.048***	0.035***
			(0.067)	(0.073)	(0.069)	(0.009)	(0.011)
Skilled occupation	0.079	184762	–0.132***	–0.138***	–0.115***	–0.014***	–0.004
			(0.035)	(0.043)	(0.040)	(0.005)	(0.007)
Paid worker (emp only)	0.969	114482	0.001	0.005	–0.011	–0.001	–0.004
			(0.023)	(0.029)	(0.022)	(0.003)	(0.004)
Paid worker	0.600	184762	0.210***	0.236***	0.219***	0.027***	0.026***
			(0.059)	(0.060)	(0.059)	(0.007)	(0.009)
Public sector employment (emp only)	0.103	114482	–0.018	–0.012	0.035	0.004	0.022**
			(0.055)	(0.058)	(0.059)	(0.007)	(0.009)
Public sector employment	0.064	184762	–0.031	–0.016	0.008	0.001	0.014**
			(0.035)	(0.043)	(0.039)	(0.005)	(0.006)
Vulnerable empl	0.474	184762	0.426***	0.484***	0.432***	0.054***	0.044***
			(0.072)	(0.077)	(0.075)	(0.009)	(0.012)

Note: Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.125), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. N and mean reported based on estimation sample from Spec 2 and 3. Spec 1 has slightly lower observations due to missing information in ethnicity. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Main results: Employment type male

	Mean	N	**Spec 1**	**Spec 2**	**Spec 3**		
			TWFE	ACRT	ACRT	ACRT (P10-P90)	ATT
Employee (emp only)	0.405	134106	0.029	0.031	0.049	0.006	0.014
			(0.070)	(0.080)	(0.077)	(0.010)	(0.012)
Employee	0.302	180132	0.076	0.112*	0.116*	0.015*	0.027***
			(0.053)	(0.065)	(0.061)	(0.008)	(0.009)
Informal job	0.516	180132	0.108*	0.101	0.087	0.011	0.002
			(0.059)	(0.067)	(0.066)	(0.008)	(0.011)
Skilled occupation	0.135	180132	0.126***	0.170***	0.195***	0.025***	0.041***
			(0.044)	(0.047)	(0.054)	(0.007)	(0.008)
Paid worker (emp only)	0.991	134106	0.004	0.013	0.008	0.001	0.002
			(0.012)	(0.012)	(0.012)	(0.002)	(0.002)
Paid worker	0.738	180132	0.250***	0.274***	0.282***	0.036***	0.040***
			(0.046)	(0.053)	(0.052)	(0.007)	(0.008)
Public sector employment (emp only)	0.145	134106	0.190***	0.211***	0.236***	0.030***	0.039***
			(0.050)	(0.060)	(0.057)	(0.007)	(0.009)
Public sector employment	0.108	180132	0.144***	0.187***	0.190***	0.024***	0.034***
			(0.036)	(0.045)	(0.042)	(0.005)	(0.007)
Vulnerable empl	0.424	180132	0.160**	0.135*	0.141*	0.018*	0.007
			(0.068)	(0.079)	(0.073)	(0.009)	(0.011)

Note: Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.126), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. N and mean reported based on estimation sample from Spec 2 and 3. Spec 1 has slightly lower observations due to missing information in ethnicity. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

and 3.4 percentage points on the same two measures. Since public sector employment is very largely wage employment, this implies that the male gain in wage employment is entirely public sector, with private wage employment flat or falling. FCUBE was delivered alongside classroom construction and teacher deployment, concentrated by construction in the districts furthest from universal transition, and teaching is both a public sector occupation and a skilled one under ISCO-08. Table A8 provides some evidence in this regard: male public employment in the education industry rises by 1.8 percentage points in the highest dose tercile against 0.4 in the lowest, a differential of 1.4 percentage points, close to the whole of the aggregate male gain.

For women, their public sector ATT is positive (2.2 percentage points among the employed and 1.4 percentage points unconditionally) while the corresponding ACRTs are indistinguishable from zero, so the public sector gain is a level shift relative to the least-treated districts rather than something that grows with intensity. Table A8 shows that women's public employment in education rises by 1.4 percentage points in the highest tercile vs 0.6 in the lowest. This is offset by the health idustry, where the increase is largest in the lowest-dose tercile (2 percentage points) and smallest in the highest (1 pp). Public health employment expanded most where treatment intensity was lowest — that is, in the more urban districts where such facilities are concentrated. The null female result therefore partly reflects a composition effect within public sector employment.

6.3 Mechanisms behind women's movement toward care

If exposure delays childbearing (and even if not), exposed women are observed nearer their peak childcare years while controls are past them. The +1.3 percentage points on care/family for women in Table 8 would then be a timing effect that partly unwinds by the time women reach their forties, not a permanent withdrawal. We explore this in Table 12.

Table 12: Main results: Demographic outcomes female

			Spec 1	**Spec 2**	**Spec 3**		
	Mean	N	TWFE	ACRT	ACRT	ACRT (P10-P90)	ATT
Female							
Married	0.776	184762	0.209***	0.215***	0.235***	0.029***	0.030***
			(0.041)	(0.049)	(0.044)	(0.006)	(0.007)
Number children ever born alive	3.288	184762	–2.391***	–2.495***	–2.568***	–0.322***	–0.314***
			(0.249)	(0.287)	(0.250)	(0.031)	(0.042)
Live births past 12mo	0.088	184762	0.090***	0.110***	0.119***	0.015***	0.020***
			(0.024)	(0.027)	(0.023)	(0.003)	(0.004)
Age at 1st birth	23.011	159420	0.079	0.428	0.604	0.076	0.258**
			(0.716)	(0.903)	(0.808)	(0.101)	(0.125)
Male							
Married	0.765	180132	0.549***	0.628***	0.626***	0.079***	0.085***
			(0.063)	(0.071)	(0.070)	(0.009)	(0.011)

Spec 1 = coefficient on post x dose, from a TWFE regression with district-of-birth FE, with age and ethnicity controls. Spec 2 = CGS estimator with a D=0 (untreated) group for districts with dose < 0.05, estimated on the district-level pre/post change - only its ACRT is shown here (raw, unscaled). Spec 3 = CGS estimator with no untreated group, anchored at the minimum-dose district. ACRT (P10-P90) = the raw ACRT coefficient scaled by the P10-P90 range of the observed dose distribution (0.125), i.e. the estimated effect of moving from the 10th to the 90th percentile of observed dose. Standard errors clustered on district of birth (analytic for TWFE, cluster bootstrap with 200 reps for Spec 2/3). Significance levels *** p<0.01, ** p<0.05, * p<0.1.

The number of children ever born is lower among exposed cohorts. We report this for completeness, but do not interpret it as a reduction in family size. Our post-reform cohorts are observed at ages 33-37 and have not completed childbearing, whereas the comparison cohorts have largely done so. The resulting truncation is not uniform across districts: the shortfall is larger where fertility is higher, and treatment intensity is correlated with the overall level of fertility. A negative coefficient therefore arises mechanically. Consistent with this, the means in Table 3 imply a larger absolute decline in high-dose districts.

The probability of having given birth in the preceding twelve months, however, rises by 1.5 percentage points on a mean of 8.8%, an increase of roughly one fifth. Since this is a current rate rather than a cumulative count, it is not subject to the truncation mentioned above, and it passes the pre-trend placebo in a range where age-specific fertility is declining.

This bears directly on the female labour supply results of Section 6.2.1. The increase in recent childbearing, at 1.5 percentage points, is almost exactly the increase in inactivity attributed to care and family responsibilities, at 1.3 percentage points. Exposed women are observed at a point in the childrearing cycle that comparison cohorts have already passed. The withdrawal from work we estimate is therefore, at least in part, a shift in its timing rather than a permanent change in lifetime labour supply, and would be expected to attenuate as these cohorts age. Where self-employment is more flexible than wage employment, this timing may also be reflected in the vulnerable work estimates. Somewhat consistent with delayed fertility is the positive ATT on age at first birth, despite the pre-cohorts having longer to start child bearing.

The probability of being currently married rises by 2.9 percentage points among women and by 7.9 percentage points among men. Since the reform raised attainment across the whole cohort, we interpret the marriage results as likely operating at the marriage market level or through household incomes, rather than as a relationship between a woman's own education and her own labour force participation. Marriage would be close to symmetric within a cohort only if partners were drawn from the same cohort and the same district; in Ghana the spousal age gap is substantial, so treated men marry women who are younger, while treated women marry men who are older and largely untreated. An improvement in men's employment, occupational standing and access to public sector work — which is what we estimate — would raise male marriage rates without a commensurate rise for women, and the asymmetry is therefore not peculiar.

Read alongside the labour market results, this suggests a potential household income channel affecting women's labour force participation. Though we cannot test this explicitly, Table 13 shows care-related inactivity among women by whether a spouse is identified in the household, that spouse's employment status, and the presence of a child under five. Care-related inactivity rises between the pre- and post-reform cohorts in almost every cell, but the differential across dose terciles is strongly graded by the husband's labour market position.

The quantity of interest here is how the pre-to-post change differs across dose terciles. Among those whose husbands hold non-public employment the differential is 0.8 percentage points, and among those whose husbands hold public sector employment the differential is 2.2 percentage points. The largest single differential in the table, 2.8 percentage points, is for women with a public-sector husband and a child under five. This is the configuration a household income channel would produce. The higher-dose districts in which men gained most in employment, occupational standing and public sector work are the districts in which married women withdrew most into care and family responsibilities.

Where husbands are not employed – and we would expect a second earner's participation to matter most – the differential withdrawal disappears and turns modestly negative. Among women whose husbands are not employed and who have a child under five, care-related inactivity rises by 3 percentage points in the lowest tercile (34.1 to 37.1) against 1.3 in the highest (30.6 to 31.9), a differential of -1.7; the corresponding cells without a young child give -0.8. We emphasise, however, that this is descriptive. A husband's employment status is an outcome of the reform, and marriage rates rose differentially with treatment intensity, so the composition of each cell differs across cohorts and terciles in ways this table cannot hold fixed. Nevertheless, we establish that female withdrawal into care work is not distributed uniformly across households, and that its concentration lines up with dimensions where male labour market gains occurred. To the extent that the effect of the reform on women's labour market outcomes are operating through this channel, it does not necessarily suggest a deterioration in women's own labour market opportunities.

Table 13: Female NEET-care by husband's status and child under 5, dose tercile and cohort

	T1 pre		**T1 post**		**T2 pre**		**T2 post**		**T3 pre**		**T3 post**	
	%	N	%	N	%	N	%	N	%	N	%	N
Husband employed (non-public), no child<5	13.0	6380	16.2	5237	10.7	7247	13.9	5238	9.2	7490	12.9	5429
Husband employed (non-public), has child<5	19.7	3832	22.6	9648	16.0	4965	18.8	11062	15.4	5272	19.4	11381
Husband not employed, no child<5	27.0	1261	29.6	939	22.0	1134	28.5	731	24.7	1107	26.5	755
Husband not employed, has child<5	34.1	528	37.1	1210	30.0	600	33.3	1146	30.6	556	31.9	1186
Husband not identified, no child<5	8.5	9943	10.0	12626	8.1	9378	9.9	10768	7.7	9191	10.5	10077
Husband not identified, has child<5	14.1	1769	14.5	3676	11.0	2188	13.1	4269	11.4	2315	13.3	4186
Husband employed (public), no child<5	11.7	1081	11.1	818	9.7	823	7.6	607	9.1	518	10.0	420
Husband employed (public), has child<5	12.3	545	13.5	1783	10.7	468	12.1	1616	9.0	290	13.0	1073

Females only. Husband identified only for women whose own relationship to household head is 'Head' or 'Spouse' - the only pairing that roster can link without ambiguity; all other women fall in the 'husband not identified' rows. Child<5 = any co-resident person coded 'Child' relative to the household head, aged under 5 (will be shared across co-wives in polygamous households).

6.4 Robustness checks

Tables A4–A6 in the appendix report four variations on our preferred specification: a cubic rather than a quadratic polynomial in the dose; equal weighting of districts in place of population weighting; an alternative construction of the dose; and a wider post-reform age range – including the partially exposed 1983 and 1982 cohorts. We speak to each in turn.

6.4.1 Functional form and equal district weighting

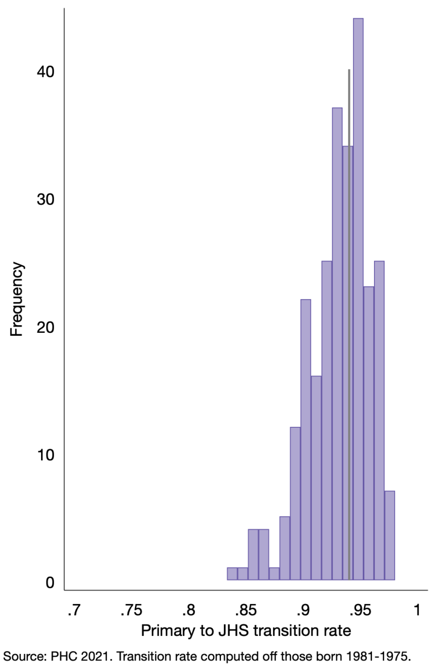
The cubic specification returns ACRTs close to the quadratic throughout, and in no case does the higher-order term change a sign or overturn a conclusion. Years of education move from 1.956 to 2.146, JHS completion from 0.224 to 0.230, employment from 0.271 to 0.291 and own-use production from -0.269 to -0.268.

Weighting districts equally rather than by population attenuates the estimated responses: years of education fall from 1.956 to 1.091, employment from 0.271 to 0.185 and own-use production from -0.269 to -0.177. The two parameters answer different questions. The population-weighted ACRT is the marginal response for the average exposed *person*; the equal-weighted ACRT is the marginal response for the average exposed *district*.

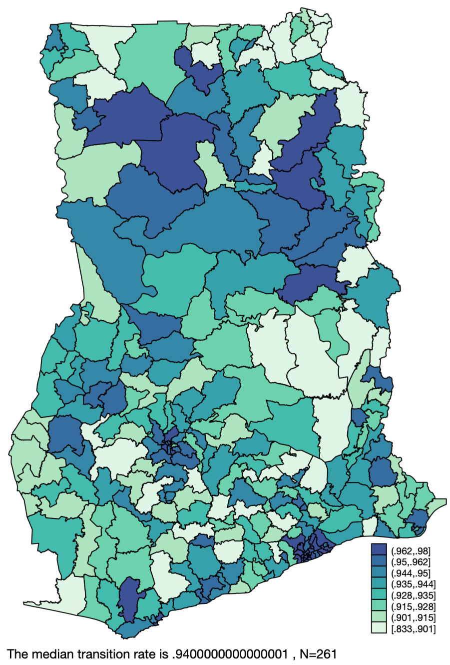
Because the ACRT averages the marginal response across districts, the two differ whenever the dose-response function is nonlinear and the weights place mass on different regions of the dose distribution. High-dose districts are typically smaller, more rural and northern, so they carry little weight under population weighting and considerably more under equal weighting. The population-weighted estimate is our chosen measure because our question is about people rather than districts. The equal-weighted column is the relevant one for the district as the unit of policy.

6.4.2 The construction of the dose

Our dose is one minus the share of primary completers who *complete* junior high school. An alternative is to define it against the share of primary completers who *ever enrol in* junior high school. Figure 9 shows the distribution of this alternative transition rate. It is both higher and more compressed than the completion-based rate: the median rises from 0.85 to 0.94, and the 10th-90th percentile range of the implied dose falls from 0.126 to 0.062, roughly halving.



(a) Histogram of alternative transition rates



(b) Map of Ghana (district boundaries): spatial distribution of alternative transition rates

Figure 9: Alternative transition rates (primary to JHS) for defining treatment intensity

Read off the raw ACRT column in Table A4, the alternative dose appears to produce dramatically larger effects on attainment — 5.396 additional years of education per unit of dose against 1.956 on our preferred measure, a factor of 2.8. However, the ACRT is a slope per unit of dose, so halving the spread of the dose roughly doubles the slope required to trace out the same movement in the outcome. Table 14 therefore scales each estimate by the 10th-90th percentile range of its own dose distribution.

Table 14: Comparing the two treatment intensity measures on a common scale

Outcome	Raw ACRT		Scaled to own P10–P90		Ratio (entry/completion)
	Completion dose	Entry dose	Completion dose	Entry dose	
Education					
Years of education	1.956	5.396	0.246	0.336	1.36
Completed JHS+	0.224	0.541	0.028	0.034	1.19
Completed primary+	0.317	0.746	0.040	0.046	1.16
Read/write	0.364	0.816	0.046	0.051	1.11
Labour market					
Employed (ICLS-19)	0.271	0.383	0.034	0.024	0.70
NEET-own production	−0.269	−0.360	−0.034	−0.022	0.66
Paid worker	0.269	0.372	0.034	0.023	0.68
Informal job	0.228	0.278	0.029	0.017	0.60
Public sector	0.106	0.172	0.013	0.011	0.80
Skilled occupation	0.052	0.109	0.007	0.007	1.04

Raw ACRTs are taken from the *ACRT (main)* and *ACRT (alt. dose)* columns of Tables A4–A6. The completion dose is one minus the share of primary completers who complete JHS (P10–P90 range 0.126); the entry dose is one minus the share who ever enrol in JHS (P10–P90 range 0.062). Scaled entries multiply the raw ACRT by the P10–P90 range of the corresponding dose distribution and are therefore the estimated effect of moving a district from the tenth to the ninetieth percentile of that measure.

On attainment, the entry-based dose delivers effects between 11 and 36 per cent larger than the completion-based dose once scaled. The excess is smallest for literacy and largest for years of education. On labour market outcomes the ranking reverses. The entry-based dose delivers effects that are smaller, by around a third, on employment, paid work and the exit from own-use production, and by two fifths on informality.

The ratio of the employment response to the JHS-completion response is 1.21 under our preferred dose and 0.71 under the alternative. We therefore caution against reading the attainment and labour market responses as scaling together.

A related question is whether the five-year window over which we define transition rates captures structural features of a district or transitory conditions. If capacity at the junior high level binds, the transition rate is determined by the ratio of primary completers to available places, and should be serially correlated absent large changes in either. In the 2021 PHC, the correlation between transition rates computed for cohorts born between 1976 and 1981 and those born between 1969 and 1974 is 0.64. Transition rates are therefore correlated but not perfectly persistent. This is not surprising: the 1969–1974 birth cohorts were educated before the NERP, under a different structure of pre-tertiary schooling, so a shift in measured transition rates across the reform boundary need not indicate that our window fails to capture the structural position of a district under the post-NERP system.

6.4.3 Cohort windows

Widening the post-reform window to include those aged 38 and 39 at enumeration – born in 1982 and 1983 – attenuates the estimates throughout, consistent with these cohorts being partially exposed. ACRTs on years of education fall from 1.956 to 1.563, JHS completion from 0.224 to 0.198, employment from 0.271 to 0.254 and own-use production from -0.269 to -0.231. The uniformity of the attenuation, and the fact that it is proportionally similar across attainment and labour market outcomes, is reassuring. Extending the comparison window to include those aged 45 produces substantively similar results.

Our estimated impact of the reform on years of education, especially among women, is smaller than in Branson and Whitelaw (2026), who also use a continuous DiD approach to study free junior secondary, but take intergenerational education mobility as their outcome and use the Ghana Living Standards Surveys, which capture parental education. Table A7 in the appendix shows how we reconstruct the equivalent of their estimate in our data, using years of education as the outcome and the TWFE specification throughout so that the rungs are comparable. One key difference is the size of their cohort window. Extending the cohort window to mirror their inclusion of younger post-reform cohorts (i.e. the 1989–1992) birth cohorts raises the TWFE coefficient from 1.21 to 3.19, a statistically significant difference. Those cohorts reached grade one at or after 1996 and were therefore exposed to free and compulsory primary education from the start of their schooling, so the estimate they contribute is one for the full FCUBE package rather than for the junior secondary margin we isolate.

For women, widening the cohort window moves the female coefficient from -0.94 to 0.97, and adopting their specification – zone-of-birth interactions in place of district-of-birth fixed effects – moves it further, to 2.77. The two steps contribute in roughly equal measure, so the female difference between the two papers is not principally a cohort-window difference. Nor is the female effect negligible under their specification: it is larger than the corresponding pooled estimate. What the exercise establishes is narrower than a substantive disagreement about women: with their cohorts and their specification, our data return an estimate close to the one they report, and with our cohorts and our specification they do not. That the specification step matters this much for women, and much less for men, is consistent with the shape of the female dose-response function documented in Section 6.1. Figure 6 shows a female profile that rises to a peak near $d \approx 0.16$ and declines thereafter, crossing zero within the observed support. A specification linear in the dose, with region-of-birth rather than district-of-birth controls, cannot represent that shape and will summarise it as a positive average slope.

Two further differences are measurement rather than specification. The first is the assignment of the dose. Branson and Whitelaw (2026) assign treatment intensity by district of residence, whereas we assign it by district of birth — a choice unavailable to them, since the GLSS rounds do not record district of birth. Among individuals born between 1977 and 1988, 38.5% are enumerated in a district other than the one in which they were born, so there is scope for the two measures to diverge. Nevertheless, running their specification on our data with the dose assigned by district of residence produces substantively similar effects on years of education.

The second is the construction of the dose, which differs in two respects. First, their transition rate is defined on movement from primary completion to *any* junior high enrolment, whereas ours rests on completion. Secondly, their dose is computed over 216 districts against our 261, reflecting the repeated subdivision of Ghana's metropolitan, municipal and district assemblies over the intervening period. Neither difference moves the average marginal effect (AME) materially: adopting the 216-district definition returns an AME for women of 2.414

against 2.419 on ours, and substituting the entry-based dose leaves the estimate unchanged at 2.419 (Table A7). However, because the AME is evaluated at the mean dose, and an AME at the mean dose is invariant to any dose rescaling, the stability of the marginal effect should be read as a property of the estimator, not as evidence that the choice between an entry-based and a completion-based dose is irrelevant. In our application, the construction of the dose does matter – attainment responses are larger and labour market responses smaller once each is placed on a common scale – but the TWFE estimate cannot distinguish this. This is an example of the difficulty Callaway et al. (2026) identify.

7 Conclusion

This paper asks whether Ghana's 1996 abolition of fees at the lower secondary level translated into better labour market outcomes for the cohorts it reached, twenty-five years later. Using the 10% sample of the 2021 Population and Housing Census, we combine pre-reform district-level variation in the primary-to-junior-high transition rate with birth-cohort exposure, and estimate the average causal response to treatment intensity following Callaway et al. (2026), in a setting where no district was untreated.

Overall, the reform raised educational attainment. Moving from the 10th to the 90th percentile of treatment intensity raises years of education by a quarter of a year, junior high school completion by 2.8 percentage points and primary completion by 4 percentage points for the pooled male and female sample. The positive literacy effect – 4.6 percentage points moving from the 10th to the 90th percentile, and 4.9 percentage points among women – is evidence that expansion at this margin did not come at the expense of the skills that education intends to impart.

Our central labour market finding is that the reform reallocated work rather than expanding it. The share of the exposed cohort engaged in any productive activity is unchanged, while moving from the 10th to the 90th percentile of treatment intensity raises employment for pay or profit by 3.4 percentage points and reduces participation in production for own final use by 3.4 percentage points. These are contrasts across districts observed at the same ages: exposed cohorts in high-dose districts are employed at higher rates than they would have been absent the reform, not at higher rates than the older cohorts against which they are differenced. This transition would be invisible to a conventional any-work measure. It is, to our knowledge, the first time this margin has been identified for a fee-abolition reform in the region.

The pooled results conceal a divergence by gender. Men gained more years of schooling, moved into skilled occupations and into the public sector, consistent with men continuing their education beyond JHS. Women's additional employment, by contrast, was almost wholly informal and own-account: their wage employment fell by 2.5 percentage points and their share in skilled occupations by 1.4 percentage points. Their participation in productive activity fell overall, as some withdrew into care and family responsibilities. The improvement in labour utilisation was composed differently too, operating on the discouragement margin for men and the hours margin for women, a difference invisible in the composite measure and absent altogether from the strict unemployment rate.

Demographic evidence suggests that part of the female withdrawal reflects timing rather than a permanent change in labour supply: exposed women are 1.5 percentage points more likely to have given birth in the preceding twelve months, closely mirroring the 1.3 percentage point increase in care-related inactivity.

We reiterate that we estimate the effect of the reform *as implemented* in a district, not the return to a year of schooling: the former incorporates the supply-side components of the FCUBE package, cohort-level spillovers, and any local labour demand generated by school construction and teacher deployment. The ratio of our labour market estimates to our first stage exceeds a plausible individual return, and it should not be interpreted as one. FCUBE in Ghana produced slower, steadier growth in attainment than several other reforms on the continent (Akyeampong, 2009), and we cannot extrapolate our findings to situations in which reforms are more successful in raising attainment levels at the outset, or reforms that may raise attainment levels at the expense of quality.

Two implications of our results follow for policy. First, whether a fee-abolition reform is judged a success depends on where the employment/production boundary is drawn: on a conventional any-work measure this reform had a null effect, while on an ICLS-19 basis it shows a 33% reduction in own-use production, moving individuals out of subsistence production and into work for the market. Countries evaluating their own reforms against enrolment and any economic activity alone could miss the margin on which such policies operate in largely agrarian economies.

Second, while removing fees is unarguably a necessary condition for educating Africa's growing youth population for productive employment, Ghana's case indicates that it is not a sufficient one: the constraints that bind after fees have been removed also play a role. Although the transition from subsistence activity toward market exchange expands the productive capacity from which a demographic dividend must ultimately be drawn, fee abolition alone cannot create the wage jobs to absorb a newly qualified workforce (nor the local demand that would reward their skills, nor the childcare provision that would allow women to convert attainment into sustained participation).

Ghana's experience is consistent with education policy and labour market policy operating as complements: schooling shapes the size and capability of the cohort entering work, while the structure of demand shapes what that cohort is able to do with it. Countries across the region now committing to fee-free secondary education are making the first of these investments. Although the constraints that bound in Ghana's highest-intensity districts need not be the ones that bind elsewhere, our results suggest that the labour market outcomes of such reforms may depend in part on the conditions new entrants meet on the demand side.

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Data

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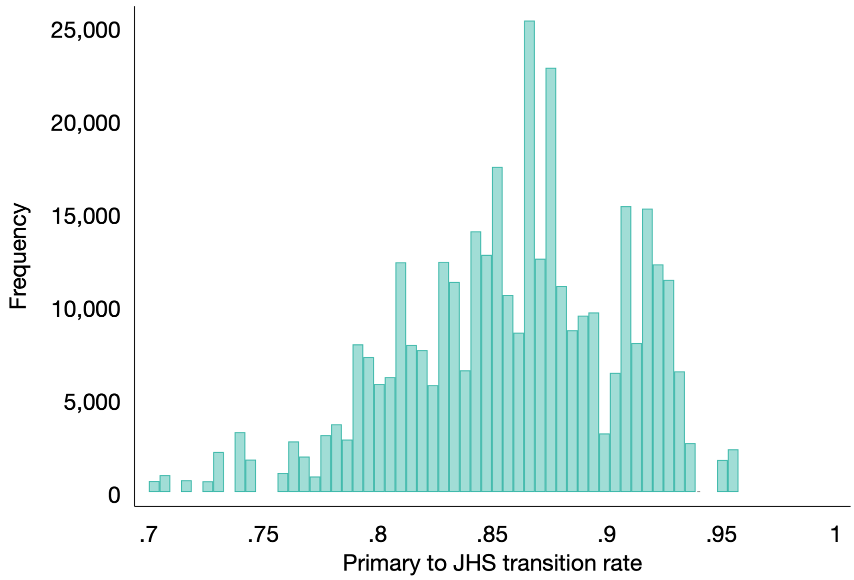
Appendix A

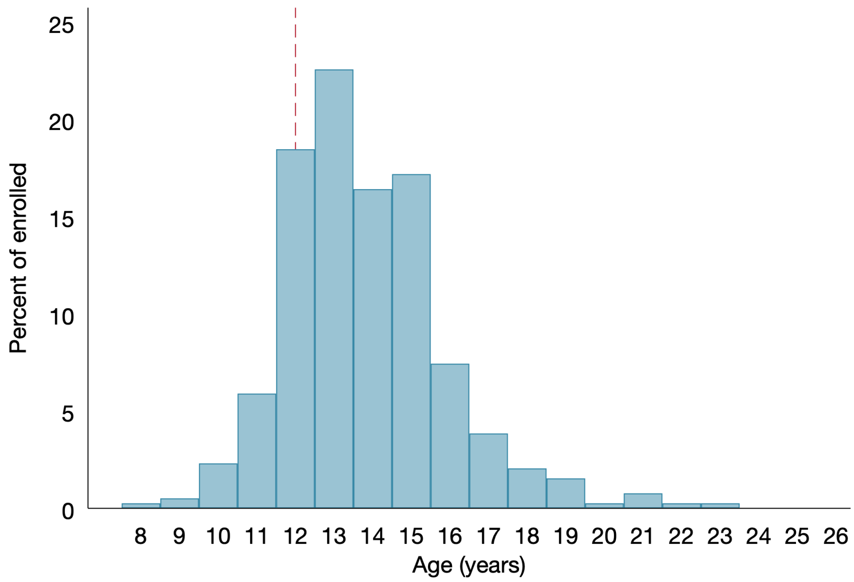
Table A1: Construction of labour market indicators

Indicator	Definition
Panel A: Labour force status (ICLS-19)	
Engaged in any work	Any economic activity for at least one hour in the seven days preceding census night. Filter question that governs all subsequent routing.
Employed	Work for pay or profit. Excludes production for own final use, unpaid trainee work and volunteer work. Temporary absence from a job is defined as employment.
Own-use production	Economic activity without pay or profit, where production is for own final use rather than market-oriented. Falls outside employment.
Unemployed (LU1)	Not employed, available for work, and actively searching.
Potential labour force	Not employed, available for work, not searching.
LFP (narrow)	Employed or unemployed.
NEA	Neither available for work nor searching, among those reporting no economic activity at all. Excludes own-use producers, who are not routed to the availability and search questions.
Panel B: Labour underutilisation	
LU1	Unemployment on the strict definition, over the labour force.
LU2	Unemployment together with time-related underemployment, over the labour force. Underemployment is proxied by fewer than 35 hours worked in the reference week, so LU2 is an upper bound on the ILO concept.
LU3	Unemployment together with the potential labour force, over the extended labour force. The census records no category for those searching but not currently available, so LU3 is a lower bound on the ILO concept.
LU4	Composite of LU2 and LU3, over the extended labour force. Aggregating both, its bias is of ambiguous sign.
Panel C: NEET and its components	
NEET	Not in employment, education or training. At the ages we study this is in practice a measure of non-employment, since participation in education or training is rare beyond the early thirties.
NEET-own production	NEET and engaged in own production work.
NEET-care/family	NEET owing to care or family responsibilities.
Panel D: Employment characteristics	
Employee	Employment status is employee.
Paid worker	Employed and paid. Unpaid family workers and apprentices are employed but not paid.
Vulnerable employment	Self-employed or contributing family worker.
Informal job	Contributing family workers, apprentices and domestic workers, together with those in wage or salaried work who do not hold a contract.
Skilled occupation	ISCO-08 major groups 1–3: managers, professionals, and technicians and associate professionals.
Public sector	Employer is in the public sector.



Source: PHC 2021. Transition rate computed off those born 1981-1975.

Figure A1: Distribution of transition rates across individuals in the sample



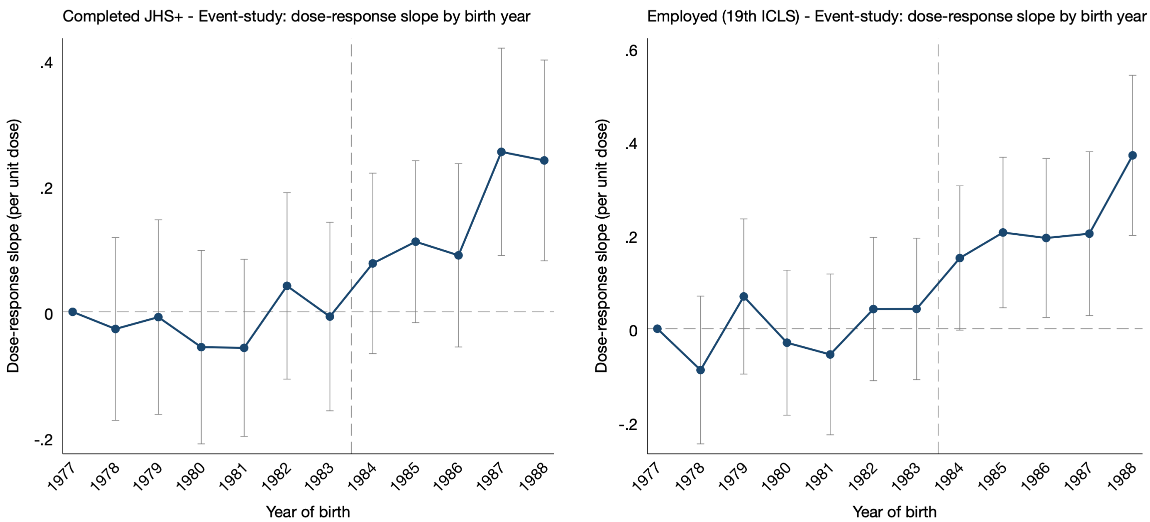
Source: Own compliation using GLSS 3 (N=390).

Figure A2: Age distribution of those who have completed sixth grade and enrolled in the past 12 months

Table A2: Pre-trend placebo (education and demographic outcomes), by gender

	Mean	N	Full sample	Male	Female
Education					
Completed primary+	0.607	157907	–0.015	–0.033	–0.012
			(0.018)	(0.023)	(0.023)
Years of education	6.834	157770	–0.281	–0.406	–0.318
			(0.201)	(0.263)	(0.260)
Completed JHS+	0.526	157907	–0.020	–0.040*	–0.015
			(0.017)	(0.023)	(0.024)
Read/write in any language	0.631	157907	–0.009	–0.032	0.000
			(0.017)	(0.023)	(0.021)
Demographic outcomes					
Age at 1st birth	23.135	70673	–0.067	n/a	–0.067
			(0.333)		(0.333)
Number children ever born alive	3.835	78881	–0.331**	n/a	–0.331**
			(0.143)		(0.143)
Married	0.798	157907	0.006	0.003	0.008
			(0.015)	(0.020)	(0.022)
Live births past 12mo	0.043	78881	0.001	n/a	0.001
			(0.012)		(0.012)
No. children co-resident	2.586	78881	0.135	n/a	0.135
			(0.106)		(0.106)
Ever given birth	0.896	78881	0.012	n/a	0.012
			(0.018)		(0.018)

Placebo sample: pre-reform birth years. Coefficient shown is the birth year × dose interaction, i.e. the slope of the birth year gradient in outcomes with respect to dose. Under parallel trends, this should be flat. District of birth and birth year fixed effects are included. Standard errors clustered on district of birth. Mean/N shown are for the full sample. Significance levels *** p<0.01, ** p<0.05, * p<0.1.



Coefficient = the age x dose interaction coefficient. CIs are analytic, SEs clustered at district of birth. District of birth and age FE are included.

Figure A3: Event study estimates

Table A3: Pre-trend placebo, by gender

	Mean	N	Full sample	Male	Female
Labor force participation					
Engaged in any work	0.794	157907	–0.012	–0.003	–0.020
			(0.014)	(0.018)	(0.023)
Employed (19th ICLS)	0.691	157907	–0.008	–0.027	0.008
			(0.020)	(0.028)	(0.024)
Unemployed/LU1 (narrow)	0.026	157907	0.000	0.003	–0.004
			(0.007)	(0.009)	(0.009)
LFP (narrow)	0.717	157907	–0.008	–0.023	0.004
			(0.020)	(0.027)	(0.024)
Potential labour force (available, not seeking)	0.025	157907	0.003	–0.004	0.010
			(0.006)	(0.007)	(0.009)
LU2 (LU1 incl. underempl)	0.283	113240	–0.018	–0.018	–0.012
			(0.019)	(0.023)	(0.029)
LU3 (LU1 incl. disc)	0.069	117165	0.009	0.005	0.014
			(0.013)	(0.016)	(0.018)
LU4 (LU1+underempl+disc)	0.307	117165	–0.015	–0.020	–0.007
			(0.020)	(0.024)	(0.029)
NEA	0.138	157907	0.009	0.007	0.011
			(0.013)	(0.015)	(0.021)
NEET-care/family	0.066	157907	0.003	–0.017***	0.023
			(0.009)	(0.006)	(0.016)
NEET-own production	0.118	157907	–0.006	0.019	–0.028*
			(0.012)	(0.019)	(0.015)
Employment type					
Employee	0.189	157907	–0.016	–0.025	–0.010
			(0.013)	(0.023)	(0.016)
Paid worker	0.678	157907	–0.006	–0.035	0.019
			(0.019)	(0.027)	(0.025)
Self-employed	0.608	157907	0.004	0.014	–0.003
			(0.015)	(0.025)	(0.024)
Vulnerable empl	0.490	157907	0.007	–0.006	0.021
			(0.017)	(0.028)	(0.024)
Informal job	0.539	157907	–0.001	–0.017	0.016
			(0.020)	(0.031)	(0.027)
Skilled occupation	0.085	157907	–0.014	–0.003	–0.026*
			(0.012)	(0.019)	(0.015)
Emp with hours<35	0.177	157907	–0.020	–0.026	–0.011
			(0.012)	(0.018)	(0.019)
Employee (emp only)	0.274	109092	–0.034**	–0.042	–0.025
			(0.017)	(0.029)	(0.025)
Paid worker (emp only)	0.982	109092	–0.001	–0.014**	0.014
			(0.006)	(0.006)	(0.011)
Public sector employment	0.102	109092	–0.013	–0.014	–0.016
			(0.014)	(0.020)	(0.016)
Public sector employment	0.071	157907	–0.004	–0.003	–0.009
			(0.009)	(0.014)	(0.010)

Placebo sample: pre-reform birth years. The Full/Male/Female columns show the birth year × dose interaction, i.e. the slope of the birth year gradient in outcomes with respect to dose; under parallel trends this should be flat. District of birth and birth year FE are included. Standard errors clustered on district of birth. Mean/N shown are for the full sample. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Education: robustness checks

	Mean	N	ACRT (main)	ACRT (cubic)	ACRT (equal-district wt.)	ACRT (alt. dose)	ACRT (alt. age range)
Years of education	7.533	364568	1.956***	2.146***	1.091**	5.396***	1.563***
			(0.648)	(0.605)	(0.536)	(1.431)	(0.593)
Completed JHS+	0.570	364894	0.224***	0.230***	0.158***	0.541***	0.198***
			(0.049)	(0.045)	(0.041)	(0.107)	(0.043)
Completed primary+	0.654	364894	0.317***	0.334***	0.211***	0.746***	0.255***
			(0.055)	(0.054)	(0.046)	(0.115)	(0.046)
Read/write in any language	0.677	364894	0.364***	0.382***	0.253***	0.816***	0.292***
			(0.054)	(0.055)	(0.046)	(0.115)	(0.046)

All columns = CGS-style estimator (no untreated group, anchored at the minimum-dose district), quadratic in dose unless noted. ACRT = local slope of the dose-response function at the post-period mean dose. Col. 1 = main-results ACRT (population-weighted). Col. 2 = cubic functional form. Col. 3 = same as Col. 1 but with every district weighted equally rather than by population. Col. 4 = same as Col. 1 dose measure robustness. Col. 5 = same as Col. 1, including 1982-83 birth year (38 and 39 year olds). Standard errors clustered on district of birth via cluster bootstrap, 100 reps. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Table A5: labour force participation: robustness checks

	Mean	N	ACRT (main)	ACRT (cubic)	ACRT (equal-district wt.)	ACRT (alt. dose)	ACRT (alt. age range)
Engaged in any work	0.769	364894	–0.003	0.021	0.002	0.000	0.018
			(0.032)	(0.035)	(0.032)	(0.071)	(0.030)
Employed (19th ICLS)	0.681	364894	0.271***	0.291***	0.185***	0.383***	0.254***
			(0.045)	(0.048)	(0.042)	(0.105)	(0.040)
NEET-care/family	0.077	364894	0.027	0.016	0.023	0.029	0.012
			(0.022)	(0.022)	(0.020)	(0.051)	(0.022)
NEET-own production	0.102	364894	–0.269***	–0.268***	–0.177***	–0.360***	–0.231***
			(0.030)	(0.029)	(0.030)	(0.072)	(0.025)
Unemployed/LU1 (narrow)	0.035	364894	–0.031**	–0.029**	–0.036***	–0.038	–0.033**
			(0.013)	(0.014)	(0.014)	(0.027)	(0.013)

All columns = CGS-style estimator (no untreated group, anchored at the minimum-dose district), quadratic in dose unless noted. ACRT = local slope of the dose-response function at the post-period mean dose. Col. 1 = main-results ACRT (population-weighted). Col. 2 = cubic functional form. Col. 3 = same as Col. 1 but with every district weighted equally rather than by population. Col. 4 = same as Col. 1 dose measure robustness. Col. 5 = same as Col. 1, including 1982-83 birth year (38 and 39 year olds). Standard errors clustered on district of birth via cluster bootstrap, 100 reps. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Employment type: robustness checks

	Mean	N	ACRT (main)	ACRT (cubic)	ACRT (equal-district wt.)	ACRT (alt. dose)	ACRT (alt. age range)
Employee	0.219	364894	–0.006	0.008	–0.021	0.064	0.021
			(0.053)	(0.052)	(0.039)	(0.110)	(0.046)
Informal job	0.507	364894	0.228***	0.224***	0.181***	0.278**	0.204***
			(0.055)	(0.056)	(0.051)	(0.129)	(0.046)
Skilled occupation	0.106	364894	0.052	0.075*	0.005	0.109	0.045
			(0.042)	(0.044)	(0.033)	(0.093)	(0.039)
Paid worker	0.668	364894	0.269***	0.292***	0.187***	0.372***	0.253***
			(0.045)	(0.049)	(0.043)	(0.107)	(0.040)
Public sector employment	0.085	364894	0.106***	0.127***	0.054*	0.172**	0.102***
			(0.036)	(0.036)	(0.028)	(0.087)	(0.033)
Vulnerable empl	0.449	364894	0.269***	0.275***	0.204***	0.290**	0.220***
			(0.066)	(0.067)	(0.057)	(0.146)	(0.059)

All columns = CGS-style estimator (no untreated group, anchored at the minimum-dose district), quadratic in dose unless noted. ACRT = local slope of the dose-response function at the post-period mean dose. Col. 1 = main-results ACRT (population-weighted). Col. 2 = cubic functional form. Col. 3 = same as Col. 1 but with every district weighted equally rather than by population. Col. 4 = same as Col. 1 dose measure robustness. Col. 5 = same as Col. 1, including 1982-83 birth year (38 and 39 year olds). Standard errors clustered on district of birth via cluster bootstrap, 100 reps. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Table A7: TWFE coefficients and average marginal effects

Specification	N	Estimate
		Coefficients
(1) TWFE estimate, our cohorts 1977-1988	435571	1.214**
		(0.520)
(2) TWFE estimate, our cohorts 1977-1988, regression on female only	221867	–0.944
		(0.632)
(3) TWFE estimate, cohorts 1977–1992	636001	3.185***
		(0.508)
(4) TWFE estimate, cohorts 1977–1992, regression on female only	325169	0.966
		(0.635)
		AMEs
(5) Interaction with birth zone (cohorts 1977–1992)	636001	2.127***
		(0.055)
(6) Interaction with birth zone, female only (cohorts 1977–1992)	325169	2.766***
		(0.071)
(7) Interaction with birth zone and female (cohorts 1977–1992)	636001	2.434***
		(0.058)
(8) Zone x female heterogeneity (female), current district (216 districts)	636001	2.414***
		(0.071)
(9) Zone x female heterogeneity (female), current district (261 districts)	636001	2.419***
		(0.068)
(10) Zone x female heterogeneity (female), current district (216, entry-based dose)	636001	2.419***
		(0.072)
(11) Branson & Whitelaw TWFE estimate on female (as reported)	11096	2.34***
		(0.27)

Rows 1-4 include district-of-birth fixed effects (FE) as well as birth year and ethnicity controls as per our main TWFE specification. Rows 5-7 show the reported average marginal effect of post at the mean observed dose and row 7 specifically for the female subgroup. When the interaction with zone of birth is included, we do not include district of birth FE, but birth year FE and ethnicity controls are included. Standard errors clustered on district of birth. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Public sector employment by industry, dose tercile, cohort, and gender

	Male						**Female**					
	T1 pre	T1 post	T2 pre	T2 post	T3 pre	T3 post	T1 pre	T1 post	T2 pre	T2 post	T3 pre	T3 post
N	26301	34519	26397	33223	26328	33364	25339	35937	26803	35437	26739	34507
Public & education industry	4.1	4.5	4.8	6.6	3.9	5.7	3.0	3.6	2.3	4.0	1.3	2.7
Public & public admin/defence industry	3.4	3.3	1.9	1.9	1.2	1.2	1.2	1.4	0.4	0.7	0.2	0.4
Public & health industry	1.0	1.5	0.7	1.4	0.4	1.2	1.3	3.3	0.6	2.2	0.4	1.4
Public & other industry	3.5	3.3	2.3	2.5	1.7	1.8	1.5	1.9	0.8	1.1	0.4	0.7
Not public sector	88.0	87.3	90.3	87.7	92.6	90.2	93.0	89.7	95.9	92.0	97.7	94.9

Each column sums to 100, those in the not public sector column include those not working. T1-T3 = dose terciles, with T1 being the lowest doses and T3 the highest. Pre cohorts = 1977-1981, post cohorts = 1984-1988.